Dear Sir:

Sixty-Nine Years of Alfred Russel Wallace Letters to the Editor

Edited by Charles H. Smith & Kelsey Patterson
Preface by Peter Raby

Government Aid to Science

I cannot but feel flattered that my letter on this subject should have been thought so dangerous as to require a leading article in the same number by way of immediate antidote, but I must beg you to allow me to correct one or two errors into which you have fallen as to the views I really hold …

Siri Scientific Press

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ISBN 978-0-9574530-7-4

Published by Siri Scientific Press, Manchester, UK
This and related titles are available directly from the publisher at:

http://www.siriscientificpress.co.uk

From the frontispiece in *Alfred Russel Wallace*, 1891, by E. D. Cope (which is apparently based on an 1877 photograph by W. Usherwood, Dorking, Surrey).

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PREFACE

Wallace and Letter Writing

Peter Raby

"If you had letters almost every day," explained Wallace to his daughter Violet, "about Darwinism, Spiritualism, Vaccination, Socialism, Travelling, Dogs Tails, Cats-whiskers, Glaciers, Orchids, &c – & had books sent to you on all these subjects to acknowledge & read, & requests for information on other subjects, & other subjects, and other subjects – and a book to write, & a garden to attend to, & 4 orchid houses, and chess to play, & visitors to see, & calls to make, and plants to name, – and – **and**, **and**, **and**, &c. &c. &c. &c. &c. perhaps you would be a 'miserable letter-writer' too! Perhaps also, not!"[a] Wallace was seventy-three, and the book he would write the following year was *The Wonderful Century*, a wide-ranging survey of the achievements of the nineteenth century in terms of inventions and scientific discovery, under the heading of "Successes" – a spectrum including "Modes of travelling" as well as "Astronomy and Cosmic Theory" – but with an additional two thirds on "Failures," beginning with some of his long-standing obsessions – "The Neglect of Phrenology," "The Opposition to Hypnotism and Psychical Research," and concluding with powerful jeremiads against "The Demon of Greed" and "The Plunder of the Earth." By this stage of his life Wallace was extremely well-known. His public reputation was based first on his high profile as a scientific naturalist and traveller, kept relatively fresh by a succession of books, although he had published nothing major since *Darwinism* in 1889 and *Natural Selection and Tropical Nature: Essays on Descriptive and Theoretical Biology* in 1891. But he was also seen as someone deeply interested in social issues, in the spiritualist movement, in public controversies and campaigns, as well as in almost every aspect of the natural world, whether or not he had specialist knowledge of a particular area. Lack of long-held knowledge had never bothered Wallace. As a largely self-taught individual, he had great confidence in his capacity – in fairness, in most people's capacity – to study and think through an apparently tricky subject; and if that sometimes entailed lively debate with an established point of view or received wisdom, then that should be seen as part of the collective search for truth. For Wallace, democratic by instinct and, eventually, a socialist, it would be natural to conduct some of these debates in the public forum of journals and newspapers.

Wallace's letter writing began early in his life. As someone who had left home when he was fourteen, to live first with his brother John in London and then as an apprentice to his other older brother William, a travelling surveyor, letters were an important relief from what must have been quite a lonely existence: letters home, letters to his great Hertford friend George Silk, letters, a few years later, to the friend he made at Leicester, Henry Walter Bates. He was seventeen when the universal penny post was introduced in 1840, which, combined with an increasingly swift and reliable postal service, made letter-writing a common good. (In *The Wonderful Century*, he instances the improvements to

[a] Item WP1/2/77 from the Wallace Collection, Natural History Museum (London).

the postal service as a major achievement, to be followed later by the electric telegraph and telephone, in a chapter entitled "The Conveyance of Thought.") To George Silk, he could write with intimacy, sharing jokes, and dreams. To Bates, he would comment on his success as a beetle hunter, or outline his response to his latest reading. When he travelled to the Amazon in 1848, letters took on a more vital role. To his brother-in-law Thomas Sims, he outlined his hopes and plans for the future. To his agent Samuel Stevens, he could note in considerable detail what he was collecting, and where; and Stevens responded by placing extracts of Wallace's (and Bates's) letters in certain natural history publications. This helped to increase the reputations of Wallace and Bates, ensuring that what they were doing reached further than the circles of collectors who were buying their specimens, or the attendees at the specialist meetings of the Entomological Society where Stevens might display some of their discoveries. At some stage Wallace's realisation that he was writing, in this correspondence, potentially for publication must have provided a useful self-discipline; certainly the letters he sent to Stevens from the Malay Archipelago seem more vivid, and more precise in description, than the earlier examples from the Amazon. The letter to the *Zoologist* about the loss of his collections when the *Helen* caught fire on his voyage home, and his trials on the *Jordeson*, presents us with a very different Wallace to the young man who sailed from Liverpool in 1848. This is a man who wants the scientific world to know about his achievements as well as his predicament, and who is already preparing the way for his next expedition.

These "letters from a travelling naturalist" – to which one could add the travel pieces published in the *Literary Gazette*[a] – form a distinct category amongst Wallace's wide-ranging letters to journals and newspapers. But apart from these professionally orientated sequences, what drove Wallace to conduct quite so multi-faceted and extensive a public correspondence? It was not, as his apology to Violet reveals, for lack of occupation. When he returned from the Malay Archipelago in 1862, and lived in London, he enjoyed attending the meetings of the learned societies, the Zoological, the Entomological, the Anthropological and the Ethnological, and the Linnean, just as he took the opportunity of going to the meetings of the British Association for the Advancement of Science. But this period of immersion in public meetings and debates only lasted about fifteen years, and was by no means continuous – for example, he locked himself away in Sussex whilst he was writing *The Malay Archipelago*. Probably the controversy that erupted over his support for William Barrett's paper on thought transference in mesmeric trance at the Glasgow meeting of the British Association in 1876, approved on his casting vote as chairman, brought about the end of this phase. Not surprisingly, he did not succeed with his application the following year for the assistant secretaryship of the British Association, and his failure in 1879 to become the superintendent of Epping Forest – in spite of the testimonials of so many of the great and good in the scientific world – provided a painful knock to his confidence. He moved his family further away from the city, first to Godalming, and later to the south coast, and distanced himself from the London-orientated establishment.

The Epping Forest episode, although it led to personal disappointment, had roused Wallace's passionate involvement in a range of issues: the ownership of land, the tension between private and public interests, the need – right – of the urban poor to have access to

[a] See items S13a, S14a, and S18 in Section 1.

green spaces, the protection of woodland. These all fed into his belief in some form of land nationalisation, as a remedy for the widening gap between the wealthy and the poor in Britain, and as an immediate relief for the human misery he saw around him. Wallace believed in the possibility of rapid social evolution. Having evolved to a certain point by means of natural selection, man could overcome, by the power of moral sense, the worst effects of the struggle for existence in which only the fittest survived. So he felt moved to protest against injustice and cruelty, against the plight of the poor and the dispossessed, wherever he saw evidence. This public commitment to controversial causes led him to the Presidency of the Land Nationalisation Society; and because he never liked to define a problem without suggesting a solution, he would advocate a scheme whereby everyone in the country might be given an acre of land, so lifting a million people from poverty to happiness. It also led him, inexorably, towards socialism, so that in the later stages of his intellectual life he tends towards the ideas of William Morris, or the fiery conviction of George Bernard Shaw, though without the latter's wit. But for forthright condemnation and outrage, Wallace's diatribe in *The Wonderful Century* against the white lead industry, and the terrible toll it exacted on the women who worked in it, loses nothing by comparison with the words Shaw gives Mrs. Warren, in *Mrs. Warren's Profession*. Wallace's views on vaccination were equally forthright: *Vaccination a Delusion; Its Penal Enforcement a Crime, Proved by the Official Evidence in the Reports of the Royal Commission* was the uncompromising title of the pamphlet he circulated to every member of the House of Commons before a debate – and he reprinted the whole thing in *The Wonderful Century*. Whether he and Shaw were or were not correct in their shared distrust of the medical statistics, they were united in indignation at what they saw as a draconian intervention that ignored the terrible insanitary conditions in which so many of the sufferers existed.[a]

Another major area of passionate interest for Wallace was spiritualism. This may seem at first sight an eccentric aberration in someone so driven by the need to establish facts, although an interest in the subject was shared by a surprising range of his contemporaries; and Wallace's own involvement was certainly strengthened by his sister's experiences, and by the comfort he felt he received after the death of his son Bertie. Yet Wallace was adamant that he was arguing from a basis of fact, rather than belief, and was prepared to court disapproval and scorn in pursuing his search for further evidence of the spiritual dimension. If he gave up trying to persuade people such as Huxley to attend séances, he did not shrink from maintaining his position publicly, writing to *The Times*[b] in defence of the American medium Slade, and indeed going into the witness box on his behalf.

The impulse towards public engagement was a permanent trait that Wallace cultivated during the course of his long life. It might seem to be at odds with the natural diffidence and social awkwardness which he identified within himself, but these characteristics were balanced, and overcome, by the drive for truth and clarity, both in scientific and social matters, and the strong sense that the century's ills could be put right in the near, rather than the distant future. New facts needed to be drawn to people's attention; misapprehensions put right. Wallace seized on the expansion of the press avidly. He made use of the journals of the learned and specialist societies, such as the *Zoologist*, *Ibis*, and the

[a] See, for example, items S376a, S551, and S640 in Section 9.
[b] See item S259 in Section 4.

Anthropological Review, and of the more widely read organs such as *The Reader*, and its much more successful successor, *Nature*. But he also wrote to the editors of the newspapers: *The Times*, the radical *Daily News*, the *Pall Mall Gazette*. This might be to answer a criticism, or to clarify or defend his position on a matter, to raise a query, or to ask for information. Occasionally, a note of impatience breaks through: "I have now shown ample reason why further discussion of the matter with Sir Henry Howorth must be unprofitable."[a] But Wallace was an optimist, convinced that the majority of his fellow human beings could be persuaded by reason. And so – "Dear Sir"…

This generous selection of Wallace's letters to the Editor allows us to appreciate his work and interests from a new perspective. It forms a commentary on many of the most significant scientific advances of the nineteenth and early twentieth century, from the standpoint of the co-discoverer of evolution by natural selection; but it also sheds light on the crucial social issues of the age, many of which continue to haunt and perplex us. Wallace remained optimistic, hopeful that the power of reason and a proper appreciation of facts would lead to a better and more equitable life for his fellow human beings. As is broadly true of all his writing, his lucid style makes his letters seem fresh and accessible, and it is hard to think of any contemporary who matches him for range, or for the sustained passion of his arguments.

* * * * *

Peter Raby is an Emeritus Fellow at Homerton College, University of Cambridge, where since 1973 he has lectured on drama and English. Dr. Raby has published widely on Victorian period literary subjects and the history of ideas. His works on Alfred Russel Wallace include a biography, *Alfred Russel Wallace: A Life*, and *Bright Paradise: Victorian Scientific Travellers*.

[a] See item S487 in Section 5.

Introduction

We are gathered here to indulge in something a bit unusual: a collection of letters to the Editor by the great naturalist and social critic Alfred Russel Wallace (1823–1913). It is not uncommon to come across collections of previously *unpublished* letters by prominent figures, but in the present instance the goal is to provide a succinct survey of Wallace's intellectual process, as it was intended for immediate digestion by the public. These, then, are not personal or private letters; instead they were intended either for a range of educated laypersons or specialists, or for the public at large.

The American philosopher Charles Peirce, one of the most perceptive commentators on Wallace's work (in three book reviews *ca.* 1898 to 1906) once said of him that he "never wrote a dull line in his life, and couldn't if he tried";[a] and further opined that he believed "in all he believes down to the very soles of his boots."[b] Surely this is a recipe for some interesting writing, and on the whole the body of Wallace's work measures up to this expectation. The fact is, Wallace was a very good, if sometimes undisciplined (as Peirce also observes[c]) writer, and in every genre he attempted. This has been noticed by just about everyone who has written about him to any extent; still, not enough focus has been put on this, an important legacy.

While the ways he communicated in essays, books, reviews, notes, and technical materials all share an essential similarity – a passion for informative dialogue – the most vital Wallace may be viewed in his published letters to the Editor. In these he summons the energy to respond to criticism, or to explore some new thought, or both. Typically, as with any correspondence destined for publication, the discussions involved ongoing questions, but in this context requiring a quick mind capable of providing both countering arguments and relevant facts. Few people have ever been better than Wallace at this kind of integration, and though Peirce may have recognized this talent, among those who have followed, not so many have.

As I (C. H. S.) have transcribed all of Wallace's known published letters to the Editor for incorporation into my *Alfred Russel Wallace Page* website, it may reasonably be asked, why re-present them in a separate collection? To this question I can give several responses:

(1) The collection is meant to focus on Wallace's powers of argumentation, which are often looked to as one of his primary, if not his greatest, strength as a thinker. It is true that the items here can be found individually at the website, but they are mixed in with all the other publications and don't have the same cumulative effect.

(2) This format provides an interesting survey of many of the important scientific and social issues of the time, eliminating descriptive, highly technical, and long discussions. So, it provides an efficient survey in a "Victorian studies" sense.

(3) The writings are organized by subject, and the best ones chosen. Thus the reader

[a] *The Nation*, 22 February 1906, p. 160.
[b] *ibid.*, p. 160.
[c] *ibid.*, p. 160.

can get "in and out" more easily to get the best idea of how Wallace engaged each subject. There are subject indexes at the website, but their purpose is comprehensiveness, not selectivity.

(4) The website, for all the effort that has been put into it in a bibliographic and reference sense, remains less "user-friendly" in some respects than I would like. An experiment or two has been tried in that direction – for example, by compiling a page dwelling on Wallace as a conservationist – but Wallace had his hand in too many things to attempt mini-surveys of this kind for all of them. This provides such a survey.

(5) Consider… Imagine the average educated layperson, or even a specialist in some field or another, one day hearing David Attenborough speaking of Wallace (Sir David is a big fan) on television or in one of his famous live lectures. He or she says to hi/rself: "Wallace… I've now heard several stories about the interesting ways he thought. How can I find out more about this guy?" Well, this person might buy one of the (very good) biographies available, but sadly, all of these focus on the (admittedly interesting) things he *did*, as opposed to his talent as an engaged commentator. He or she might also visit, in addition to my own, one of the several other Wallace websites now operating,[a] but neither do these parse things out in survey fashion. Another choice: a volume which gets to the essence of the matter.

(6) It's a book. People still like books! Many people like the portability of books, and the still-higher visual quality of a book page than a computer screen. Computer usage (of e-books) is still more about reference than it is reading entire books (various surveys have shown that very few accesses of e-books extend beyond a few minutes in duration, ostensibly because the individual is there looking for some fact or quotation). Admittedly, this undoubtedly is also the main way my *Alfred Russel Wallace Page* website is used.

(7) Finally, and to follow up on the previous point, it is hoped that this collection, more than just being the source for some quote by Wallace that punctuates a given discussion, will give a deeper sense of the man: that he had consistent views, even across subjects, and that the occasional bright quip should not be considered the end of the story.

Another relevant question that could be posed is whether it might be more interesting to make a collection of Wallace's *personal*, largely unpublished, letters. Well, first it must be noted that a website exists that has already collected these materials together under one roof, so to speak.[b] But it will be a while before these are all transcribed, and fully searchable; more importantly, however, I'm not at all convinced that most of Wallace's correspondence (beyond that with Darwin, I admit) will prove of very great interest to the general reader. Personal correspondence is often, of course, very useful to the historian, but most of the examples I have seen of Wallace's are rather mundane compared to his writings designed for publication. Further, a fair number of his private letters (including those to and from Darwin) – the cream of the lot – have actually already been published over the years, in his autobiography *My Life*, in James Marchant's *Alfred Russel Wallace: Letters and Reminiscences* (from 1916), and in a variety of other contemporary and recent

[a] These include *The Wallace Collection* (Natural History Museum London), *The Wallace Correspondence Project* (George Beccaloni and the Natural History Museum London), and *Wallace Online* (John van Wyhe, Charles H. Smith and the National University of Singapore).

[b] This is the just-mentioned *Wallace Correspondence Project*.

publications. Many of the latter have been included online in *The Alfred Russel Wallace Page*.

In fact, the goal here is to better "get into Wallace's head," to use the vernacular. The effort will reward the reader in interesting ways, both historical and psychological. Few will probably sympathize with his thoughts and opinions from beginning to end, but even the most critical of readers will be aware of a tenacity of mind and expression in keeping with the Peirce remarks quoted earlier.

Before turning to some technical remarks on the organization of the book, we must first provide some biographical context.

Wallace's Life

The story of Wallace's life has been told many times, and well,[a] but for the benefit of the reader here we must do so again, if only in brief. He was born Alfred Russel on 8 January 1823 to middle-class but not very well-off parents in Usk, Monmouthshire, in the hinterlands of England and Wales. Wallace's father was not a very good provider for his family, and though having been the beneficiary for many years of a small inheritance, saw it get away in 1837. By that time the family was living in Hertford, having moved there about 1828 to take advantage of inexpensive lodgings. Wallace's entire education had taken place in Hertford, but now he was forced to leave school and go to London, where for a while he worked with his older brother John for a builder. After several months he took up with another older brother, William, who was developing a successful business as a surveyor in the West of England.

For most of the next six years Wallace roamed about with brother William across large sections of western England, and over into South Wales. He began to take an interest in natural history subjects, especially botany, geology, and astronomy, and by the early 1840s was knowledgeable enough to give lectures at local mechanics institutes. But then, in 1843, there was a work slowdown, and William had no choice but to let him go. Undaunted, Alfred found a position in Leicester at a private school, which employed him to teach a variety of elementary subjects.

This was a short but very important period in Wallace's life. It only lasted about fifteen months, from early 1844 to a few months into 1845, but during this time his direction was changed in at least three fundamental ways. First, taking advantage of the good library at Leicester, he read several books that would influence him significantly; these included, among others, Alexander von Humboldt's *Personal Narrative of Travels*, Thomas Malthus's *Principles of Population*, and Robert Chambers's *Vestiges of the Natural History of Creation* (originally published anonymously). He also had the good fortune of meeting another young man, Henry Walter Bates, whose enthusiasm for insect collecting was contagious, turning Wallace toward entomology. And, also importantly, he had his first contacts with the paranormal, witnessing an early demonstration of mesmeric trance. At

[a] See, especially, Peter Raby, *Alfred Russel Wallace, A Life* (Princeton University Press, 2001); Michael Shermer, *In Darwin's Shadow: The Life and Science of Alfred Russel Wallace: A Biographical Study on the Psychology of History* (Oxford University Press, 2002); Martin Fichman, *An Elusive Victorian: The Evolution of Alfred Russel Wallace* (University of Chicago Press, 2004); Ross A. Slotten, *The Heretic in Darwin's Court: The Life of Alfred Russel Wallace* (Columbia University Press, 2004).

that point, trance was looked at rather suspiciously by the intelligentsia, but Wallace soon found that he himself had the knack of inducing such a state on subjects of his choosing. He thus learned "my first great lesson in the inquiry into those obscure fields of knowledge, never to accept the disbelief of great men, or their accusations of imposture or of imbecility, as of any weight when opposed to the repeated observation of facts by other men admittedly sane and honest."[a]

In early 1845 Wallace's brother William died unexpectedly. Wallace returned to Wales to run the business, but soon soured on the effort. Meanwhile, his interest in science and nature continued to strengthen; finally, he suggested to Bates they embark upon a collecting expedition to the tropics. Bates was agreeable, and the two left for the Amazon in early 1848.

Apart from making a living through natural history collecting, Wallace and Bates were interested in looking into possible kinds of evidence bearing on the question of the origin of species. Wallace's reading of *Vestiges* had turned him into a full-blown "transmutationist" (as they referred to "evolutionists" in those days), and this seemed like a good place to take up this agenda. But after four years of collecting specimens, mostly in the lower and middle Amazon and Rio Negro, Wallace still had no handle on the matter. In July of 1852 he left Brazil for England, but more than three weeks into the trip across the Atlantic the vessel on which he was sailing caught fire and sank, taking some two years of his collections with it. He and the crew were lucky to survive at all; fortunately a passing ship saw them in their lifeboats ten days later and picked them up.

Wallace regained British soil on October 1st, 1852. He spent the next seventeen months in various ways – vacationing, delivering lectures to scientific societies, and writing two books[b] – before again setting out on a collecting expedition. He had considered Africa as a possible destination, but in the end settled on the Australasian Archipelago, then known as the Malay Archipelago. Here his four-years of experience in the Amazon served him well; he remained a full eight years and sent home more than 125,000 collected specimens, largely of insects and birds. Here too he emerged as a writer on weighty concepts, including the famous 1858 "Ternate paper" on natural selection that he posted to Darwin, sending the latter into a tizzy over priority. Darwin's friends arranged to have Wallace's essay published, along with some fragments of unpublished materials by Darwin. But most importantly, the whole affair caused Darwin to really get going on his own depiction of the evolutionary process – *On the Origin of Species* – which saw publication in late 1859.

One should contemplate, for a moment, the sheer audacity of Wallace's accomplishments during this period in his life. Although he hired guides and assistants to aid him in both his Amazon and Malay Archipelago travels, most of the time he was largely on his own, with just the odd letter of introduction to help him out among what were, as often as not, largely uncivilized peoples. Later, in places such as his autobiography, he sometimes refers to himself as shy and retiring, but he must have had a real talent for gaining the confidence of his hosts to operate successfully under such conditions for a full twelve years.

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[a] "Notes on the Growth of Opinion as to Obscure Psychical Phenomena During the Last Fifty Years," *The Two Worlds* 15 September 1893, p. 440.

[b] *Palm Trees of the Amazon*, and *A Narrative of Travels on the Amazon and Rio Negro*, both published in 1853.

When Wallace returned from the East in the Spring of 1862, he was no longer an obscure figure serving time in remote places in the name of science. He was immediately thrust into the limelight of the discussions on evolutionary theory, and also soon began to take interest in various social issues. His literary career blossomed as well, especially after his publication in 1869 of *The Malay Archipelago*, a work that has never been out of print, and remains on most critics' lists as one of the top scientific travel works ever written.

Until about 1880 most of Wallace's attention was given over to various issues in the fields of evolutionary biology and biogeography, to which he devoted four great books,[a] and many shorter essays and analyses. After this date he did not "give up" on scientific questions, as has sometimes been claimed, but he did branch out considerably, giving more or less even attention to natural science and social criticism subjects. By the time he died in 1913 he was well known to both intellectuals and the general public, though after his death his name sank into near-obscurity for close to fifty years. The 1958 anniversary celebrations of the emergence of the natural selection concept brought him back some, but more recent trends connected with biodiversity studies and an improved knowledge of the range of his published work have provided a decisive push.

The Present Work

As already stated, the object here is to present a sizable selection of Wallace's letters to the Editor. Not all of Wallace's efforts could be included in this collection without running the project to well over six hundred pages, but the roughly one-third of the items that have been excised arguably represent redundant or less interesting material, or pieces that extend beyond a strict understanding of "letter to the Editor."

The basic approach has been to reproduce the letters as faithful transcriptions of the originals: that is, without additional efforts of dress-up. Thus, as in the original sources, Latin names of species sometimes appear in italics, and sometimes not. Punctuation also maintains the form as given in the original sources. A few liberties have been taken to conserve space, for example by condensing multiple signature lines into single ones, and sometimes excising original editorial introductions if they contain no vital information.

There are ten sections, and the letters within each of these are organized (in most cases) according to date. Each section begins with a short introductory statement to get the reader acclimatized. Sections 2 through 9 are thematic in nature, covering general subjects of Wallace's attention. Section 1, "Early Letters," collects items from before his return to England in 1862, while Section 10, "Miscellaneous Subjects," deals with a variety of less easily categorized topics.

Wallace generally does an excellent job of getting the reader into the core of the matter through references to the pertinent aspects of his subject, whether this be some general remark or comments replying to the opinions of an adversary. As a result, and thankfully, not much editorial input is necessary to set the stage. Each letter is individually introduced (sometimes as a group of related items), but as often as not with not much more than a bibliographic citation. In instances where Wallace does not fully identify the per-

[a] *Contributions to the Theory of Natural Selection* (1870), *The Geographical Distribution of Animals* (1876), *Tropical Nature on Other Essays* (1878), and *Island Life* (1880).

son to whom his remarks are addressed, and this person seems to be of some historical import, a note or inserted name is added. A few additional footnotes provide occasional clarifications. Importantly, it should be mentioned that two numbering systems are used for the footnotes, each on a page by page basis: numbers are used to flag notes that appeared in the original writings, whereas letters are used to identify notes that have been added in the present work.

It should be emphasized that it is not a goal here to try to bring the reader forward in time to the current understandings on each matter. Imagine for a moment what this would take! Two hundred-plus histories would have to be researched, and by editors sometimes largely unfamiliar with the questions Wallace entertained! Perhaps the reader, curious as to the outcome of many of these debates, some still going on today, will take it upon hi/rself to investigate.

The letters in the collection here are identified by the "S" number I have applied at my website (and originally, for most of the items, in the publications list in my first book on Wallace[a]), and a brief-form citation. Where the letter was given a title in its original context, this is given here with capitalized title words; conversely, when a title was absent, a descriptive title is given here, but entirely in lower case. To save space (probably ten pages or more), a separate, full bibliographic, list is not provided for all the items, but such further information can be obtained at my *Alfred Russel Wallace Page* (under "Wallace Writings Bibliography," http://people.wku.edu/charles.smith/wallace/bibintro. htm).

As this collection has been worked up almost entirely through the direct efforts of its co-editors, few acknowledgments of assistance are in order. Continuing moral support by the Department of Library Public Services at Western Kentucky University should be noted, however, as should the excellent materials retrieval services provided by the department's interlibrary loan office: many of the items in the present collection are quite obscure, and originally required considerable persistence to obtain.

Charles H. Smith, Bowling Green, Kentucky, April 2014

[a] *Alfred Russel Wallace: An Anthology of His Shorter Writings*. Oxford University Press, 1991.

Section 1. Early Letters, 1845 to 1862

Introduction

The letters in eight of the ten sections in this collection are grouped according to subject matter, but in this first one it seemed more appropriate to use a strictly chronological, period, approach. During the earliest (pre-1848) period there are only a few Wallace letters known that were directed toward the correspondence columns, and those that are known concerned a variety of subjects only indirectly related to his later activities. He relayed only a few communications during his Amazon expedition that found their way into print; these are all given below. His Malay Archipelago travels yielded a good deal more – some three dozen, and with varying intents – and the total volume of materials involved prevents all of them from being reproduced here. Those left out include redundant reports, some rather technical items, and a few long items that stretch the definition of "letter to the Editor." For a complete accounting (including full-text) of all Wallace materials published during the 1845 to 1862 period, see *The Alfred Russel Wallace Page* online.

What *is* in this section provides an interesting look at Wallace's developing character. The three pre-Amazon communications reveal a probing, nonconformist point of view, one already pushing social agendas. In the Amazon communications Wallace-the-observer emerges into view, and in his account of the fire on the "Helen" we see just how close he came to losing his life for his work. The Malay Archipelago letters are both succinct summaries of his collecting efforts and masterly miniature travel notes, including no small number of quotable passages. From these we hear his observations on the Chinese residents of Singapore, his stay at Sir James Brooke's cottage, the durian, running a-muck, orangutans and birds of paradise, and numerous vignettes on the joys and vicissitudes of collecting.

* * * * *

letter on mesmerism (S1aa)

At the moment this is Wallace's earliest known letter to the Editor: it appeared in the 10 May 1845 issue of The Critic (London), in a regular column titled "Journal of Mesmerism." Wallace had become interested in mesmerism during his stay at Collegiate School in Leicester, where he saw a demonstration by an advocate named Spencer Hall. He later furthered this interest in the Amazon, performing various experiments with native people there.

LEICESTER. – (From a correspondent.) – Seeing in your journal an account from a correspondent, of some mesmeric experiments with metals, I beg to send you a brief

account of a few experiments I have made on the same subject, the results of which, however, *do not agree* with those of your correspondent, and it is for that reason I send them, as I think that it is only by the accumulation of a variety of facts that an explanation will be found. The patient is a boy of about fourteen, of a sanguine temperament, and displays all the usual phrenological and cataleptic phenomena in great perfection. No single metal appears to have any effect upon him, not even gold. *Any two* metals in contact have a most decided and powerful effect. I have tried with gold, silver, copper, zinc, and iron. If any one of these is in the patient's hand, on being touched with any other he immediately drops it, complaining that it hurts and burns him. He can hold any two in different hands, or in the same hand if they are not allowed to come in contact with each other. Wood or stone with a metal produces no effect. From these facts I think it appears a fair conclusion, that the effect is produced by galvanic action to which, in the mesmeric state, the body appears extremely susceptible.

Any two metals in contact with each other and held in the hand do undoubtedly, by means of the moisture of the hand, form a galvanic circuit, and a galvanic circuit cannot be formed without two metals, or two different fluids.

The effects of gold alone, or any single metal, which I have not witnessed, must be owing to a different cause.

The patient who exhibits the phenomena shews the sympathy of feeling very distinctly. If three or four persons take hold of hands, and are connected with him through me, on the last person being pinched he instantly complains and exhibits most unequivocal signs of feeling. – Alfred R. Wallace.

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Corresponding Societies and Lecture Rooms Bill (in Favour) (S1b)

This early letter co-authored by Wallace was printed in the Appendix to the Reports of the Select Committee of the House of Commons on Public Petitions. Session 1846. It came out in favor of a proposed bill that would do away with an archaic position on meeting places.

App. 630. *Mr. John Henry Vivian*. Sig. 16.

6753. The humble Petition of the undersigned Proprietors and Members of the Public Library at Neath, in Glamorganshire.

That the institution of which your Petitioners are proprietors and members was established in the year 1818.

That your Petitioners have learned with much satisfaction that a Bill is now before your honourable House, intituled, "Corresponding Societies and Lecture Rooms Bill," which has for its object the repeal of certain parts of an old statute, 39 Geo. III. *c.* 79, by which heavy pecuniary penalties are imposed upon all persons connected with rooms used for lectures, discussion, news rooms, or libraries, unless licensed in every year by two justices of the peace, and by which statute powers are also given to revoke such licence at any time.

That this Act, which by its title professes to apply only to societies established for

seditious and treasonable purpose, has in recent cases been held to apply to ordinary lecture rooms.

That the only plea for the continuance of such statute could be the dangers to be apprehended from seditious and treasonable meetings, – dangers which from the enlightened and liberal policy of Government happily now no longer exist.

Your Petitioners consider that the formalities required by this statute are a hinderance, vexatious and uncalled for, to societies established for the advancement of knowledge and the progressive improvement of mankind.

And your Petitioners therefore would pray your honourable House to pass the proposed Bill into a law, by which the objectionable statute, as above, may be repealed.

And your Petitioners will ever pray, &c. – John Rowland, President, Willm. Llewellyn, Alfred R. Wallace, &c. &c. &c.

* * * * *

Emigration (S2a)

A letter printed in "The Annals of Progress," a separately paginated serial supplement attached weekly to The People's Journal (London), in this case to the second issue (probably early January 1848) of its Volume 5. An editorial introduction and the letter Wallace refers to are omitted here.

Sir, – I send you the enclosed letter, which I obtained from one of the workmen on the railway here, whose brother is the writer. You may, perhaps, think it worth publishing. It appears to me very important that the working men of this country should be made acquainted with the advantages and capabilities of the southern states of America, as a place for emigration. Thousands of emigrants pour into New York, a part of America which approaches nearer in density of population to our own country than any other, and, of course, every kind of labour being overstocked, many are unable to find employment, or to obtain the means of reaching more thinly populated districts.

A relation of mine[a] has just returned from Georgia and Alabama, and gives a delightful account of the plenty of food and land, and the healthiness of the climate. The southern states are scarcely ever alluded to in our works on emigration, and many of our working-men scarcely know of their existence. The reason of this is, that they are slave states; but this is no reason why they should not be eligible for emigrants. *The example of free labour, and what it can do, before their eyes, would do more for the abolition of slavery, by appealing to the pockets of the planters, than can all the writings of the abolitionists*, which only excite ill feelings, and may perhaps tend to prolong the evil through a spirit of opposition.

If half a dozen of our working-men would go and settle in a southern state, they would be sure to obtain a good living with little labour: and I think all will see that they would be doing much more for the poor slaves than they could effect in any more direct way.

[a] Wallace refers here to his sister Fanny. According to his autobiography *My Life* Fanny had returned to England in September 1847 after a three-year teaching stint at private schools in Georgia and Alabama.

Should these remarks be of any service, you are at liberty to publish them. I remain, yours, &c., Alfred P.[a] Wallace, Neath, Glamorganshire.

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While he was in South America and the Australasian archipelago, Wallace sent back a number of brief reports on his activities to various professional friends. He must have known that many of these would end up in print. Some of the more interesting of these writings follow.

23 October 1848 letter from Parà, Brazil (S3)

The following words from Wallace and his friend Bates were sent to their consignments agent Samuel Stevens on 23 October 1848, and printed in the January 1849 issue of the Annals and Magazine of Natural History.

If any one is curious about our trip up the Tocantins, you may inform them that we ascended to about the 4th parallel of S. lat. near the Rio Tabocas, having reached Arroya, the last abode of civilized people, and passed a little beyond to view the rapids called Guaribas. We hired one of the heavy iron boats with two sails for the voyage, with a crew of four Indians and a black cook. We had the usual difficulties of travellers in this country in the desertion of our crew, which delayed us six or seven days in going up; the voyage took us three weeks to Guaribas and two weeks returning. We reached a point about twenty miles below Arroya, beyond which a large canoe cannot pass in the dry season, from the rapids, falls and whirlpools which here commence and obstruct the navigation of this magnificent river more or less to its source; here we were obliged to leave our vessel and continue in an open boat, in which we were exposed for two days, amply repaid however by the beauty of the scenery, the river (here a mile wide) being studded with rocky and sandy islets of all sizes, and richly clad with vegetation; the shores high and undulating, covered with a dense but picturesque forest; the waters dark and clear as crystal; and the excitement in shooting fearful rapids, &c. acted as a necessary stimulant under the heat of an equatorial sun, and thermometer 95° in the shade. Our collections were chiefly made lower down the river. During the five weeks of our journey we had no rain till the last two days. The weather here is as delightful as ever; the mornings invariably fine, and a shower in the afternoon every third or fourth day, which cools and refreshes everything delightfully. The heat is never oppressive; the nights always cool; there can certainly be no climate in the world superior to this, and few equal. Since sending our last collection, we have had further experience of the rarity of insects in this country. The Lepidoptera are numerous in species, but not in individuals; the Coleoptera are exceedingly scarce, and other orders are generally, like the Lepidoptera, sparing in individuals; we attribute it to the uninterrupted extent of monotonous forest over which animal life is sparingly but widely scattered. However this makes a difference in the commercial value of the subjects. The present collection is the fruits of two months'

[a] The "P." is a transcription error in the original, as several lines of evidence suggest.

devoted and almost exclusive attention to insects. Shells and Orchids continue to be exceedingly scarce.

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12 September 1849 letter from Santarem, Brazil (S4)

Extracts from a letter from Wallace to Samuel Stevens, printed in the February 1850 issue of the Annals and Magazine of Natural History.

I have got thus far up the river, and take the opportunity of sending you a few lines. To come here, though such a short distance, took me a month. I am now waiting here to get to Montalegre, but the difficulties of getting men even for a few days are very great. Here the country is very sandy and dry, with a scrubby, shrubby vegetation; there are however some patches of forest, and in these, Lepidoptera are rather abundant; there are several lovely Erycinidæ new to me, and many common insects, such as Heliconia Melpomone and Agraulis Dido, abundant, which we hardly ever saw at Parà: Coleoptera I am sorry to find as scarce as ever. I hope however to do better at Montalegre, as the hills there are near a thousand feet high, and must I should think produce some. I wish to know what is thought of Cuyaba in the province of Matto Grosso as a locality; it is at the head of the Tapajoz and Paraguay River; there is a communication from here, salt being taken up. I could also from Rio Negro get up the Madeira to Matto Grosso city, or up some branches into Bolivia. Is Bolivia at all known? I see in the Museum Catalogue only five or six Erycinidæ from it, from Mr. Brydges' collections. I see there is a branch of the Andes in it the highest in America, and its capital cities appear higher ground than even Bogota or Quito. Either of the localities can be I think quite as easily reached as the Andes up the Amazon; at all events I should like to know if the ground is open and likely to be good, for some future time, if not just at present. I shall I think get up the Rio Nigro towards the sources of the Orinooko, but I am rather fearful that all N. Brazil is rather poor in Coleoptera.

September 14th. – I believe I shall now start for Montalegre tomorrow, having a canoe lent me; I have however found so many new species of Lepidoptera, that I shall probably stay here a month on my return before going to Rio Nigro, unless indeed I find Montalegre so very good as to induce me to spend till December there. I do not think that you need send me anything till I write again. Pray write whenever you can, and give me all the information you may be able to obtain, both as to what things are wanted in any class or order and as to localities.

The Tapajoz here is clear water with a sandy beach, and the bathing is luxurious; we bathe here in the middle of the day, when dripping with perspiration, and you can have no idea of the excessive luxury of it; the water is so warm that then is the healthiest time. Oranges are about fourpence a bushel here, and are far the best fruit; large pineapples twopence to fourpence, but we seldom eat them. The more I see of the country, the more I want to, and I can see no end of, the species of butterflies when the whole country is well explored. Remember me to all friends.

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letters from Santarem and Barra do Rio Negro (now Manaus), Brazil (S6)

Extracts from letters sent to Samuel Stevens printed in the December 1850 issue of the Annals and Magazine of Natural History.

Santarem, Nov. 15, 1849 (500 miles above Parà).

I spent about three weeks at Montealegre and have now been back here nearly a month, so before I leave for the Rio Negro I send you a small lot of insects; they consist almost entirely of Lepidoptera, the *Beetles* not yet having made their appearance; in the wet season I hear there are plenty both at Montealegre and here, so I shall probably return here, unless I meet with something much better to keep me up above. Of the boxes sent, Nos. 1 and 2 only are for you to dispose of. Your lot, though a small one, I trust will be found a good one; there are a very considerable number of fresh species, one of which (No. 605[1]) is, I think, the *most beautiful thing* I have yet taken; it is very difficult to capture, settling almost invariably high up in trees; two specimens I climbed up after and waited for; I then adopted a long pole which I left at a tree they frequented, and by means of persevering with it every day for near a month have got a good series: the sexes I have no doubt whatever about, though I have not taken them *in copula*; the female flies lower and is easier to take than the male. The allied species (606[2]) was rather abundant at Montealegre; the orange Heliconia-like insect occurred there plentifully. Of all *new species* and others which I know to be good, I have sent plenty; of old things I have sent a few only.

In the *Erycinidæ* there are a great many species fresh to me, and I hope *some new* to Europe: I have now made descriptions of all the species sent, so that should I be obliged again to send home my duplicates or lose any of them, I can still recognize the species. The handsome species I hope will sell well. In box No. 3 I have put a lot of miscellaneous insects, which please take out and dispose of. There is also a small stuffed alligator, a species I think they have not in the Museum; it is the Jacare tinga, of which the tail is *eaten* and is very good; they are an immense deal of trouble in skinning. I have sent also a larger one, which I think is the common species; also a tortoise-shell and a few vertebræ of the large alligator of the Amazon I have put in to fill up; perhaps they may be interesting to geologists to compare with those of fossil Sauria. Shells there are none here. There are two painted calabashes in paper with your name outside; please accept them as a specimen of the Indian girls' work at Montealegre; the varnish, colours, &c., are all made by themselves from the leaves and bark of different trees and herbs; they paint them with bits of stick and feathers, and the patterns are all their own design; they are the usual drinking-vessels here, but less ornamented for common use. I am much in want of some work on the species of butterflies; I think the 'Encyclopédie Méthodique,' vol. ix. by Godart, is the only thing that will do. The leaf in the box is a segment of *Victoria regia*; if any one wants it, you may sell it.

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[1] This beautiful species I find to be the rare *Callithea Sapphira*, Hub., of which hitherto only one example appears to have existed in the collections in this country.

[2] This is Callithea Leprieurii, Feisthamel, also very rare. – S.S.

Barra de Rio Negro (1000 miles above Parà), March 20, 1850.

After sending off the box from Santarem (which I trust you have received safe), I was delayed a fortnight waiting for men to go up the river. After great difficulty I obtained them, but to Obidos only, a distance of about eighty miles (three days); there I was delayed four days, and then got others another stage of four days on to Villa Nova. There I was delayed a week, and was there indebted to the kindness of a trader, who lent me some of his men to get on to Barra. Now however the rains and head winds had set in, so that after rather an unpleasant journey owing to wet and mosquitoes, we arrived at Barra on the 30th of Dec. in thirty-four days from Santarem. I was so anxious to reach here before the wet season had regularly set in, that I never wasted an hour to go on shore but once a day to cook, so that I literally collected nothing on the road except at Villa Nova, where we had tolerably fine weather. After the muddy, monotonous, mosquito-swarming Amazon, it was with great pleasure we found ourselves in the black waters – *black as ink* they are, and well deserve their name; the shores are rugged and picturesque – and greatest luxury of all, mosquitoes are unknown except in the islands. Our voyage, however, was not near so bad as it might have been, for Mr. Spruce, who left Santarem for Obidos exactly a week before us, arrived there only the evening before, having taken *nine* days owing to the want of wind, without which it is impossible to stem the current. We are here staying with Sir Henrique Anthony, in the same house Edwards occupied; he is a most hospitable fellow, and his house is the general receptacle of strangers. I soon found that insects were exceedingly scarce here at this season, it being almost impossible to get half a dozen in a day worth bringing home. Birds too are equally scarce, so I resolved on a short trip up the Rio Negro to where the *Umbrella chatterers* are found. I spent a month there, and being fortunate in finding a good hunter, have got a small but pretty good collection of birds, considering the season.

With regard to living animals, &c., it is quite impossible to send them from here. At Parà they can only be bought at such high prices as not to make it worth the risk. The captains too require half the price for the passage. I had intended, if I could have been now on my voyage up the Rio Negro, to have returned about next Christmas, getting all the live animals I could on the way and coming home myself with them, calculating that I could get sufficient to pay all expenses to England and back; but I do not think now that I shall do so, as I shall probably not be able to start for the frontiers till June or July, and it is nearly a two months' voyage. If therefore sufficient funds arrive by that time, I shall probably stay up in the neighbourhood of the Cassiquiare a year, and then on returning to Barra see about a journey up towards the Andes. I am anxiously waiting also to know about the fish and reptiles, as I do not want to get more if they do not pay.

Besides the umbrella birds, the little bristle-tailed manakin will, I think, be good; also the trumpeter, which is a species different from that at Parà; the muscovy ducks also. Both among the birds and insects there are, I know, many common as well as rare species. There are also two bad specimens of the celebrated "bell bird," which I believe is rare; they frequent the highest trees out of ordinary gunshot; my hunter fired five or six times at each of them, and after several ineffectual shots at another gave it up in despair. Of the curl-crested araçari, I have only at present got a single specimen. The anaçaris I send are two species new to me, and are both much prettier than the curl-crested. I must now not forget to thank you for the prints you sent me, which I only discovered a short time ago,

never having opened the box containing them. Any newspapers or scientific periodicals you can send me will be particularly acceptable.

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letter concerning the fire on the "Helen" (S7)

On his way back to England in 1852, Wallace's ship caught fire and sank. All aboard eventually were rescued, and shortly after regaining English soil Wallace sent an account of his adventure to Zoologist, which carried the story in its November 1852 issue.

As some account of the unfortunate accident that took place on my voyage home from South America may not be unacceptable to your readers, I beg to send you the following brief statement of the facts.

On the 2nd of July of the present year, I arrived in Pará from the river Uaupés, an unexplored branch of the upper Rio Negro. I had with me a considerable collection of birds, insects, reptiles and fishes, and a large quantity of miscellaneous articles, consisting of about twenty cases and packages. Nearly half of these had been left by me at Barra a year before to be sent home; but a new government, arriving there shortly after I left, took it into their heads that I was engaged in a contraband trade, and so I found them still there on my way down, in the present year, and had to bring them all with me.

On the 12th of July I embarked in the "Helen," 235 tons, for London, still suffering from fever and ague, which had nearly killed me ten months before on the upper Rio Negro, and from which I had never since been free.

The cargo of the vessel consisted of India-rubber, cocoa, arnatto, balsam of copaiba, and Piassaba. Almost all my cases were stowed in the hold. On the 6th of August, when in lat. 30° 30′ N., long. 52° W., at 9, A.M., smoke was discovered issuing from the hatch-ways, on opening which, and attempting to ascertain the seat of the fire, the smoke became more dense and suffocating, and soon filled the cabin, so as to render it very difficult to get any necessaries out of it. By great exertions the boats were got out, and bread, water, and other necessaries put into them. By noon the flames had burst into the cabin and on deck, and we were driven to take refuge in the boats, which, being much shrunk by exposure to the sun, required all our exertions to keep them from filling with water. The flames spread most rapidly; and by night the masts had fallen, and the deck and cargo was one fierce mass of flame. We staid near the vessel all night; the next morning we left the ship still burning down at the water's edge, and steered for Bermuda, the nearest point of land, but still 700 miles distant from us. For two days we had a fair easterly wind, but this afterwards changed to N. and N.W., and we could make but little way. We suffered much from the heat by day; and being constantly wet with the spray, and having no place to lie down comfortably, it may be supposed that we did not sleep very soundly at night. For food we did very well, having plenty of biscuit and salt pork, – raw, of course, – which we found very palatable, with a little water to wash it down. After a week, having seen no vessel, we put ourselves on short allowance of water, and then suffered much from thirst; and as we now were in a part celebrated for squalls and hurri-canes, every shift in the wind and change of the sky was most anxiously watched by us. At length, after ten days and nights we heard the joyful cry of "Sail ho!" and by a few

hours' hard rowing got on board the "Jordeson" from Cuba, bound for London, in lat. 32° 48′ N., long. 60° 27′ W., being still about 200 miles from Bermuda.

We now had a very tedious voyage, and soon got to be very short of provisions, the crew being doubled by our arrival: in fact, had not two vessels assisted us with provisions at different times, we should actually have starved; and as it was, for a considerable time we had nothing but biscuit and water. We encountered three very heavy gales, which split and carried away some of the strongest sails in the ship, and made her leak so much that the pumps could with difficulty keep her free. On the 1st of October, however, we were safely landed at Deal, eighty days after we left Pará.

The only things which I saved were my watch, my drawings of fishes, and a portion of my notes and journals. Most of my journals, notes on the habits of animals, and drawings of the transformations of insects, were lost.

My collections were mostly from the country about the sources of the Rio Negro and Orinooko, one of the wildest and least known parts of South America, and their loss is therefore the more to be regretted. I had a fine collection of the river tortoises (Chelydidæ) consisting of ten species, many of which I believe were new. Also upwards of a hundred species of the little known fishes of the Rio Negro: of these last, however, and of many additional species, I have saved my drawings and descriptions. My private collection of Lepidoptera contained illustrations of all the species and varieties I had collected at Santarem, Montalegré, Barra, the Upper Amazons, and the Rio Negro: there must have been at least a hundred new and unique species. I had also a number of curious Coleoptera, several species of ants in all their different states, and complete skeletons and skins of an ant-eater and cow-fish, (*Manatus*); the whole of which, together with a small collection of living monkeys, parrots, macaws, and other birds, are irrecoverably lost.

I may also mention that I had taken same trouble to procure and pack an entire leaf of the magnificent Jupaté palm (*Oredoxia regia*), fifty feet in length, which I had hoped would form a fine object in the botanical room at the British Museum. – Alfred R. Wallace, 43 Upper Albany St., Regent's Park.

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June 1854 communication from Singapore (S13a)

Wallace apparently arranged to do a bit of travel journalism for London's Literary Gazette. The first of these efforts appeared in its 19 August 1854 issue – just two pages from the article by Edward Forbes that prompted Wallace's "Sarawak essay" of early the next year.

Any Encyclopedia or Gazetteer will inform you that the island of Singapore is about twenty-seven miles long and fifteen wide, contains about 65,000 inhabitants, of which more than half are Chinese, was purchased by our Government from a native Rajah, and is now the seat of a great and increasing trade.

I shall therefore leave you to obtain such and any similar information you may require from the abovenamed sources, and confine my present communication to the physical and social peculiarities I have myself been able to observe in the country and the people.

I have been staying for some weeks at a place called Bukit Tima, situated near the

centre of the island, and surrounded by such patches of the virgin forest or jungle as the rapid increase of cultivation has suffered to remain. I am residing with a French Roman-catholic missionary, who has been here several years endeavouring to convert the Chinese from paganism. He has now several hundred converts, and has built a pretty church for their accommodation.

The country round us is pretty equally divided between cleared land and jungle, the latter still covering all the hill-tops, while the valleys are occupied with plantations, either of nutmeg, pepper, or Gambier. The only cultivators are the Chinese, many of whom are very wealthy, owning extensive and valuable estates. Little villages of most wretched and filthy hovels are scattered about, in which the shopkeepers and mechanics reside, while the houses of the landed proprietors are only superior in size.

The Chinese (in Singapore at least) are a most amazingly industrious people. It is almost painful to see how they work, and, except when eating, they are never seen idle. Their general dress is only a short pair of breeches, reaching from the hips to half way down the thighs, and thus almost naked they carry heavy loads of Gambier leaves and pepper, or walk along the dusty road to the town of Singapore, a distance of ten or twelve miles, with a hundred-weight of plantains for sale. Seeing them thus naked at work, I have been much struck by their great resemblance to some of the more athletics tribes of South American Indians. The colour of the skin is almost identical; the colour of the hair, the absence of beard, the muscular development of the limbs, are the same; the countenance is but slightly different, and the peculiar mode of squatting down to eat renders the similarity very remarkable.

My friend, the missionary, said to me the other day, "Singapore is a very strange place; I never did see one like it. It belongs to the English, who bought it from the Malays, but now the Chinese have it quite for themselves. They take what ground they like, and make plantations, and then sell them for a great deal of money, and nobody says anything to them. It is really a very strange place." The Chinese, no doubt, think so, and therefore flock here in great abundance, as places where they can have land for nothing, and are perfectly free to come and go, and to do as they please, are not to be found everywhere. The results of this over-liberal policy have been lately evident in the difficulty there was in putting down the recent insurrection. The Chinese have settled in such a miscellaneous manner, in places which can only be reached by paths scarcely known but to themselves, that they are almost out of reach of all law and police, and can commit murders, when so inclined, almost with impunity. This would not have happened had the lands been regularly settled by purchase at even a nominal rate from the Government, and all squatting in the more remote and uncleared tracts prohibited.

The insurrection was a purely national one, confined entirely to the Chinese of two rival provinces, who have such an hereditary hated for each other, that every two or three years it breaks out in open war to the knife, when the most fearful atrocities are committed, men, women and children murdered in cold blood, houses burnt, and much property destroyed. They might be left to fight it out by themselves, were it not that all the country districts depend for a supply of rice solely on the town of Singapore. Individuals and small parties are afraid to venture there in times of disturbance, and so hundreds and thousands of armed men pour into the town, and all order is at an end. One morning 600 Chinese passed our house in straggling single file, armed, in the most impromptu manner, with guns, matchlocks, pikes, swords, huge three-pronged fishing-spears, knives, hatchets, and long sharpened stakes of hard wood. They were going to buy rice, they said, but they

were stopped on the road by a party of about a dozen Malay police, five of them shot, and the rest turned back. The disturbance lasted a week, and even now men are still occasionally killed, nobody knows why.

When excited the Chinese are very bloodthirsty and cruel, but they are great cowards, and a hundred to one seems about the proper proportion for an equal fight between them and Europeans or Malays. A Chinese village is a strange sight. A row of hovels like ruinous pigstyes, with a receptacle for every description of filth, a cesspool in fact before the front door; pigs, whose excessive fatness we vainly seek to imitate in England, roaming about everywhere, with ducks and fowls in profusion; vegetables, fruit, strange compounds of every description for sale, among which piles of rancid, or, to speak more plainly, stinking fish, force themselves most disagreeably on the attention; half naked long-tailed Chinamen (but no women), some eating rice and the fish just mentioned; others, pea-soup strongly sweetened with course brown sugar – for they sweeten almost everything but their tea – many gambling, seated on mats in the verandahs, a few smoking opium, and the rest gazing, with the ludicrous expression peculiar to them, at the white intruders, and you have a Chinese village in Singapore.

This strange people seem destined to play a great part some day in the world. They are an intruding race, and wherever they settle, the less energetic Malays soon clear out. Even on the peninsula of Malacca there are many places colonized by the Chinese, from which the native inhabitants have retreated, not liking the vicinity of such a go-ahead people.

Singapore is entirely dependent for its supplies of provisions on the neighbouring island. Neither rice, coffee, nor sugar, are grown here; meat and vegetables are brought from Malacca, and other places. Fruits are scarce, and not very good. The oranges are scarcely eatable, the plantains not much better, and the famed mangosteen, though very delicious, is a rarity. There is not, therefore, much to render it a desirable residence, and I shall soon leave it, probably for Borneo, when I have had a few more weeks' entomologizing, the insects being the only class of animals abundant and interesting. – A. R. W., Singapore, June 1854.

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26 September 1854 letter from Singapore (S14a)

Wallace's second communication to Literary Gazette, concerning his trip to Malacca, up the Peninsula, appeared in its issue of 16 December 1854.

In July last I left this town for Malacca, and spent more than two months there.

Few places have populations so varied and distinct living together as are to be found in Malacca. The ubiquitous Chinese are perhaps the most numerous, keeping up their manners, customs, and language; the indigenous Malays are next in point of numbers, and their language is the 'Lingua franca' of the place. Next come the descendants of the Portuguese – a mixed, degraded, and degenerate race, but who still keep up the use of their mother tongue, though ruefully mutilated in grammar; and then there are the English rulers, and the descendants of the Dutch, who all speak English. The Portuguese spoken at Malacca is a useful philological phenomenon. The verbs have mostly lost their inflec-

tions, and one form does for all moods, tenses, numbers, and persons. *En vai*, *nos vai*, does for everything connected with going. Adjectives too have been deprived of their feminine and plural terminations, so that the language is reduced to a marvellous simplicity, and with the admixture of a few Malay words becomes rather puzzling to one who has heard only the pure Lusitanian.

In costume these several peoples are as varied as in their speech. The English preserve the tight fitting coat, waistcoat, and trowsers, and the abominable hat and cravat; the Portuguese patronise a light jacket, or more frequently shirt and trowsers only; the Malays wear their national jacket and sarong, with loose drawers; while the Chinese never depart in the least from their national dress, which, indeed, it is impossible to improve for a tropical climate, whether as regards comfort or appearance. The loosely hanging trowsers, and neat white half-shirt half-jacket, is exactly what a dress should be in this latitude.

The town of Malacca is crowded along the side of the little river, and consists of narrow streets of small houses, some devoted to shops, others to the more fancifully ornamented dwellings of the Chinese. In the suburbs are the houses of the English, and other more civilized inhabitants, embedded in groves of cocoa nut, mangosteen, durian, rambutan, jack, mango, araca nut, and many other fruit trees, the never failing shade of whose varied and beautiful foliage is as agreeable as the fruits themselves, the merits of which I cannot but think have been far too highly rated. Some small hills near the town are entirely occupied as Chinese graveyards, many acres of ground being covered with large horseshoe shaped tombs of solid masonry, generally much and fantastically adorned with painting, gilding, and carving. Further in the interior are extensive marshy flats cultivated as paddy-fields, out of which low isolated hills rise like islands. Further on, again, these flats contract into narrow valleys, winding about amidst low undulations. It is along the sides of these that the Malay villages are situated, only distinguishable by the dense masses of palms and fruit trees in which their houses are buried. Every spot of ground which is not nor has been cultivated is covered with jungle.

In Malacca, as in Singapore, the Chinese do everything. They build houses, they fetch wood and water, they cultivate vegetables, they clear the paddy by laboriously pounding it in a huge mortar, the stamper of which is worked by the feet, they work the tin mines of the interior, and the gold mines of Mount Ophir. They do everything but manage horses. A Chinese groom is an impossibility.

My first excursion was to a place called Gading, thirteen miles from the town, where I had permission to reside in a house occupied by some Chinese Christians who are cultivating a gambir and pepper plantation. The house was a mere huge shed. I lived in it a fortnight, as, strange to relate, the Chinese (I trust because they were Christians) kept it clean. No people in the tropics really cultivate the soil as these do. They do not merely plant and reap. They dig, and trench, and level; they eradicate weeds and stumps; they keep the ground clean, and they manure; the process of manuring, indeed, was the only thing I objected to, as the tank was a large bucket kept standing for convenience in a corner of the house. The rage for liquid manure is such, that in the Chinese villages a bucket often stands near the door for public use. The pigs for the same reason are far better lodged than with us, having a floor of poles with a tank beneath, in which all the manure is collected.

I found the men very quiet and civil, doing anything I required with great willingness. Their food consisted of rice, a little fish, and a few vegetables, with weak tea *ad libitum*. They, however, eat a great deal, and four times a day. The Malays, on the contrary, take

only two meals.

There were several tin mines in the village near us, employing many thousand Chinese. The ore is obtained from beds of a quartzose sand in the flat valley before mentioned. It exists in small black grains (an oxide?), and is separated by washing. This is done generally by hand, in large wooden basins, or sometimes by a stream of water in a large wooden trough. The smelting is done with charcoal, in rude clay furnaces, bound together with poles and rattan; the metal runs into a hole at the bottom, and is ladled into a mould, forming an ingot of about 50lbs. weight, and very pure.

After a fortnight's residence one of my Portuguese servants was seized with fever, and I was obliged to return with him to Malacca, where the other was also taken ill, and then I caught the fever. I recovered by a liberal use of quinine, and went to another locality among the Malays, about whom, and of my visit to Mount Ophir, I will write in my next.

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1854 letter from Sarawak, Borneo (S18)

Wallace's next report to Literary Gazette, focusing on his new base of operations, Sarawak, was printed in its 9 June 1855 issue.

I have been staying some time at a cottage of Sir James Brooke's, about twenty miles inland, on the ridge of a mountain, at an elevation of about one thousand feet. The path up is peculiar, half is over broken rocks, the other half up ladders. These are made of trees about as thick as one's thigh, placed at angles varying from thirty to seventy degrees with the horizon, and having notches cut in them for steps; sometimes they go over chasms between the rocks, or slope over a mass of boulders, or stretch to the edge of a precipice, with a shaky piece of bamboo to hold by, but oftener nothing at all. Over ravines and larger chasms regular bridges are constructed of tall thin poles, crossing each other at the pathway, which consists of a single round and slippery bamboo, and bound together with rattan. There are three paths of a similar character up this mountain to as many Dyak villages, which are situated nearly on the same level a few hundred feet below the summit. These villages are placed in most romantic situations, and might be very pretty were there not such an accumulation of trees, weeds, and rubbish about them. Huge boulders, as big as the houses themselves, rise among them, and hang over them in the most extraordinary manner. Every one is a picturesque object stained with lichens, and on the shady side covered with mosses, while the tops are generally more are less clothed with curious ferns and orchids. All the spaces between are filled up with the cocoa-nut, the gouniti, and the areca palm, with the jack fruit, durian, and mangosteen in smaller quantities. The houses are all elevated on tall poles, on one side perhaps fifteen to twenty feet high, owing to the inequality of the ground, and these posts are generally green with moss and fringed with ferns. The ground between the houses is the general receptacle for all kinds of refuse, part of which is cleared away by the pigs which are constantly roaming about, but the greater part, consisting of the husks of cocoa-nuts and other fruits, remains, and forms a very tan-like mess, soaked as it is with the constant rains, and the dripping from the surrounding trees. Most of the houses are long, and are divided for the occupa-

tion of several families with a common verandah. In each village is one circular house, where the young unmarried men sleep, and where the heads are kept. In these tree villages there are perhaps one hundred skulls, but all very old, none having been procured since the English rajah has governed the country. In many the lower jaw is wanting, and has been supplied by a wooden one with carved teeth, and the eyes are supplied by small white shells. The dress of the men is a long narrow scarf worn round the loins, the end hanging down in front. It is generally bordered with a bright colour, and has a pretty appearance. Beads are occasionally worn round the neck, and rings of brass on the arms, sometimes quite covering them from the elbow to the wrist. The women wear a very scanty petticoat and most extraordinary stays, a cylinder of bamboo and brass wire, quite inflexible, and reaching from the breast to the hips. It is worn when quite young, and seems never to be taken off except to enlarge it as required by the growth of the wearer. The paddy-fields of these people are in the plains below, and they are therefore constantly going up and down the hill, and the women and children carry heavy loads 1000 feet up and down. The result is an enormous development of the leg. The women, especially, have most disproportionate calves, actually thicker than those of the men, and by no means improving their personal appearance. They live on rice and fruits, very little animal food, and sometimes towards the end of the season no rice, when they eat rudely prepared sage instead.

While I was at the cottage a dozen or more of them would come up every day, squat down on the verandah, and watch my proceedings. Before leaving they generally begged for some tobacco, which they prefer to their own betel. Every one, down to boys of six years old, has his little bamboo case, to carry his pinang or areca nut, betel leaves, and a little lime and gambier if he can afford it, these four materials being essential to form a proper betel quid; the test is the bright red colour of the juice, which is freely expectorated, and to a stranger looks very sanguinary.

The mountain itself is of unstratified trappean or porphyritic rock, rising abruptly from the plain. A few miles beyond it a brimstone district commences, and there the gold diggings are situated. They are worked by a company of Chinamen, and by numbers of private miners on a small scale. The gold is not abundant, just paying the miners, and enabling them to live well. At the foot of the hill is a Chinese village of small traders, who deal with and supply the miners. The Chinese villages here are far more pleasing than those of Malacca and Singapore, on account of the number of women and children, which gives them a more domestic and natural appearance. The women are mostly native half-breeds between the Dyak and Chinese, and the mixture has much improved the race. Some of them are really pretty, which can never be said of the Dyaks, except when very young girls. Of course there is also much improvement, morally and politically. These Chinese are a permanent and most valuable part of the population, not mere foreign adventurers; and it is to the increase of the native-born Chinese, and their gradual mixture with the Dyak tribes, under the present good government, that Sarawak may base its hopes of continued prosperity, and may look forward to extending its influence over a great part of Borneo. Men who have wives and families, a house and a country, are also far more easy to govern, because more happy and contented, than the poor bachelor outcasts of Singapore. Here too the Chinese feel they are a portion of the country; they are not merely governed by a set of strangers, but their feelings are consulted and their prejudices respected, as well as their permanent interests attended to, and the result is, that they give

less trouble, and are more amenable to authority here, than in any other part of the East. The wet season is now at its height, and the country is deluged.

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1855 letter from Sarawak, Borneo (S21)

A letter to Wallace's agent Stevens, concerning his insect collecting activities in the Sadong River area of Sarawak. Stevens relayed it to the Zoologist, where it appeared in the August 1855 issue.

Si Munjon Coal Works, Borneo, 8th April, 1855. You will see by the heading of this letter that I have changed my locality. I am now up the river Sadong, about twenty miles N.E. of Sarawak. A small coal-field has been discovered here, and is now being worked. At present the jungle is being cleared, and a road made to carry the coals to the river side, and it is on account of the scarcity of roads in this country that I thought it advisable to come here. Another reason was, that this is the district of the "Mias" or Orang-utan, the natural history of which I am very anxious to investigate, so as to determine definitely whether or no three species exist here, and also to learn something of their habits in a state of Nature. An English mining engineer has the direction of the works here, and has about a hundred Chinese labourers engaged. I am residing with him, at the foot of the hill in which the coal is found. The country all round us is dead level and a perfect swamp, the soil being a vegetable mud, quite soft, and two or three feet deep, or perhaps much more. In such a jungle it is impossible to walk; a temporary path has, however, been made from the river (about a mile and a half) by laying down trunks of trees longitudinally. Along this path is very good collecting-ground, but many fine insects are daily lost, and butterflies can hardly be captured at all, from the impossibility of stepping out of the path, and the necessity of caution in one's movements to preserve balance and prevent slipping, not at all compatible with the capture of active tropical insects. The small clearing in which our houses are situated also furnishes me with many good insects among the trunks and stumps, and other decaying timber. Half a mile further on in the jungle, on the hill side, is another clearing, where coal levels are to be opened; and, lastly, the jungle is being cut down to form a road or railway, and which, as it progresses, I expect will offer me very fine collecting-grounds. Having now been here nearly a month I can offer some opinion on its entomological capabilities. Imagine my delight in again meeting with many of my Singapore friends, – beautiful longicorns of the genera Astathes, Glenea and Clytus, the elegant Anthribidæ, the pretty little Pericallus and Colliuris, and many other interesting insects. But my pleasure was increased as I daily got numbers of species, and many genera which I had not met with before. Longicorns I think are more abundant than at Singapore, and more than half of them are new to me. The species, too, run a size larger. Some of the scarcest there are here the most abundant, while many of the commonest of that locality seem to be altogether absent from this. Curculionidæ are about equal in number, and there is a fair proportion of novelty. Two or three species closely allied to the Mecocerus Gazella are abundant, and many curious Brenthidæ. I am paying much attention to the most minute species, and can safely promise abundance of novelty for Mr. Waterhouse. Carabidæ are hardly so abundant as at Singapore, but I have some beautiful

new Therates, Catascopus and Colliuris, and the curious Thyreoptera also occurs sparingly under Boleti. The Cleridæ seem very similar to those of Singapore, but scarcely so numerous. The Buprestidæ I am happy to say are very fine; not that the species run very large, but they are tolerably abundant. One of the most beautiful I make out to be Belionota sumptuosa, about an inch long, and of the richest golden copper-colour; it flies with the greatest rapidity, making a loud bee-like hum, and settles on timber only in the hottest sunshine. There are also many smaller species of a dark green, variously marked with lighter green or golden spots (Chrysobothris sp.?). Also several slower flying kinds, which when fresh are clothed with a yellow powder, like the Euchroma gigantea? of Brazil, which, however, seldom reaches England in that condition, as it is difficult to capture and kill the insect without injuring its delicate covering. I have also some very curious minute species, making altogether thirty-six species of this interesting family collected in Borneo. I also pay much attention to the Elateridæ, and have many pretty things, especially among the velvety species, with a swollen thorax. Of Heteromera, Erotylidæ, Chrysomelidæ and Trimera, I have hosts of curious things, which are daily increasing in number. The only family in which there is an absolute deficiency, is that of the beautiful Cetoniadæ. I have only at present one or two Tænioderas, a fine green and black Coryphocera, and the handsome Macronota Diardi, which is, I believe, very rare: I scarcely dare hope to increase my collection of this family to any great extent, as they evidently are only abundant in mountainous and rather open shrub-producing districts, while they are scarcely at all represented in the dense and gloomy jungles which are the favourite haunts of all those insects which at any period of their existence feed on fresh or decaying timber, or on the boleti which grow upon it. Among my latest captures are, my first species of Paussus, which I have been long anxiously looking for: I took it in the daytime flying about fallen timber. Two days since I obtained a species of Malacomacrus, a Brazilian genus of Longicorns, described and figured by White in the 'British Museum Catalogue,' and yesterday, while at breakfast, a magnificent black and yellow spotted Lamia flew into the verandah, and was caught in my hand. I have now 135 species of Bornean Longicorns, and I do not despair of getting 200 before I leave this place, which I mean to work thoroughly.

To give English entomologists some idea of the collecting here, I will give a sketch of one good day's work. Till breakfast I am occupied ticketing and noting the captures of the previous day, examining boxes for ants, putting out drying-boxes and setting the insects of any caught by lamp-light. About 10 o'clock I am ready to start. My equipment is, a rug-net, large collecting-box hung by a strap over my shoulder, a pair of pliers for Hymenoptera, two bottles with spirits, one large and wide-mouthed for average Coleoptera, &c., the other very small for minute and active insects, which are often lost by attempting to drop them into a large mouthed bottle. These bottles are carried in pockets in my hunting-shirt, and are attached by strings round my neck; the corks are each secured to the bottle by a short string. The morning is fine, and thus equipped I first walk to some dead trees close to the house frequented by Buprestidæ. As I approach I see the bright golden back of one, as he moves in sideway jerks along a prostrate trunk, – I approach with caution, but before I can reach him, whizz! – he is off, and flies humming round my head. After one or two circuits he settles again in a place rendered impassable by sticks and bushes, and when he leaves it, it is to fly off to some remote spot in the jungle. I then walk off into the swamp along the path of logs and tree-trunks, picking my way cautiously, now glancing right and left on the foliage, and then surveying carefully the surface of

the smooth round log I am walking on. The first insect I catch is a pretty little long-necked Apoderus sitting partly upon a leaf: a few paces further, I come to a place where some Curculionidæ, of the genus Mecopus, are always seated on a dry sunshiny log. A sweep of my net captures one or two, and I go on, as I have already enough specimens of them. The beautiful Papilios, Evemon and Agamemnon, fly by me, but the footing is too uncertain to capture them, and at the same moment a small beetle flies across and settles on a leaf near me – I move cautiously but quickly on – see it is a pretty Glenea, and by a sharp stroke of the net capture it, for they are so active that the slightest hesitation is sure to lose the specimen. I now come to a bridge of logs across a little stream; this is another favourite station of the Buprestidæ, particularly of the elegant Belionota sumptuosa. One of these is now on the bridge, – he rises as I approach, – flies with the rapidity of lightning around me, and settles on the handle of my net! I watch him with quiet admiration, – to attempt to catch him then is absurd; in a moment he is off again, and then settles within a yard of me; I strike with all my force, he rises at the same moment, and is now buzzing in my net, and in another instant is transferred in safety to my bottle: I wait a few minutes here in hopes that another may be heard or seen, and then go on; I pass some fallen trees, under which are always found some Curculionidæ, species of Alcides and Otops, – these I sweep carefully with my net and get two or three specimens, one new to me. I now come to a large Boletus growing on a stump, – I push my net under it, two Thyreopteræ run on to the top, I knock one with my hand into my net, while the other has instantly escaped into a crack in the stump and is safe for this day, but his time will come. In some distance now I walk on, looking out carefully for whatever may appear; for near half-a-mile I see not an insect worth capturing; then suddenly flies across the path a fine Longicorn, new to me, and settles on a trunk a few yards off. I survey the soft brown mud between us, look anxiously for some root to set my foot on, and then cautiously advance towards him: one more step and I have him, but alas! my foot slips off the root, down I go into the bog and the treasure escapes, perhaps a species I may never obtain again. Returning to the path, another hum salutes my ear, and the fine Cetonia, Macronota Diardi, settles on a leaf near me, and is immediately secured: a little further, a yellow-powdered Buprestis is caught in the same manner. Having reached the usual limits of my walk in this direction, I turn back and am soon rewarded by what appears a Colliuris sitting on a leaf, but which is discovered, on capturing it, to be of the equally acceptable Longicorn genus Sclethrus: a little further and a true Colliuris is caught. These insects I have named, from their elegant form, lady-beetles, English names being necessary for the use of my boy Charley,[a] who is now a rather expert collector. During the rest of the walk back, the principal insects I get are two velvety Elaters crawling on the logs, and two or three curious Heteromera in the same situation. Returning by the Chinamen's houses, I find, at an odoriferous puddle, the fine Papilio Iswara, which I capture, as well as a P. Evemon and P. Sarpedon. I then walk to the other clearing, where, among the fallen timber and branches, I get several small Buprestidæ; numbers of the handsome red Eurycephalus maxillosus are here constantly flying about and crawling on the timber. On one tree I find running about with ants, which they much resemble, the curious little short-elytra'd Longicorns, Hesthesis sp. Here also, I get two or three pretty species of Clytus and a Callichroma. Between whiles I have picked up a few flies, wasps and bugs, and have got tolerably filled bottles. Returning home, I find Charley has also had a fair day. We empty our bottles into boiling water,

[a] Wallace's field assistant, Charles Allen (1839–1892).

and on pinning and setting our captures, find we have got between us 94 beetles, 51 different species, 23 of which are new to my collection: I have 5 new Longicorns, 2 new Buprestidæ, and 5 new Curculionidæ. I have been out five hours, and consider this a very good day's work. It will be seen that a far larger number of insects can be collected in a day in England, but perhaps hardly such a large proportion of species.

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On the Bamboo and Durian of Borneo (S27)

Wallace's treatments of these two botanicals are much-quoted classics. They were recorded in a letter to Sir William Jackson Hooker, who printed them in Volume 8 of his Journal of Botany in 1856.

Two vegetable productions particularly attracted my attention in Borneo, – the Bamboo, most useful of plants, and the Durian, king of fruits.

Different species of Bamboo abound in all tropical countries, and wherever they are found the natives apply them to a great variety of uses. Their strength, lightness, smoothness, straightness, roundness, and hollowness, – the facility and regularity with which they can be split, – their different sizes, the varied distance of their joints, the ease with which they can be cut, and with which holes can be made in them, – their hardness outside, their freedom from any taste or smell, their great abundance, and the facility with which they are propagated, – all make them fitted for a hundred different purposes, to serve which other materials would require much labour and preparation. They are at once the most wonderful and the most beautiful production of the tropics, and the best gift of Nature to uncivilized man.

I shall briefly mention the uses to which they are applied by the native tribes of Borneo, which have fallen under my notice, and which have struck me the more forcibly, because in the parts of South America I have visited, Bamboos are comparatively scarce, and where found, but little used, their place being taken, as to one class of uses, by the great variety of Palms, and as to another, by Calabashes and Gourds.

The Dyak houses are all raised on posts, and are often two or three hundred feet long, and forty or fifty wide. The floor is always formed of large bamboos, which are split into four or five strips, so that each may be nearly flat, and these are firmly tied down with rattan to the rafters beneath. This, when well made, is a delightful floor to walk upon barefooted, the rounded surfaces of the bamboo being very smooth and agreeable to the feet, while at the same time affording a firm hold. But what is more important, they form, with a mat over them, an excellent bed, – the elasticity of the bamboo, and the undulating nature of the surface, being far superior to a more rigid or flatter floor. Here at once we have a use which cannot be supplied so well by another material without a vast amount of labour, all Palm stems and other substitutes requiring much cutting and smoothing, and not being equal to bamboo when finished. Some tribes however prefer a flat and close floor, and they make bamboo-boards for the purpose, by splitting open a large bamboo on one side only, and flattening it out, so as to form beautiful slabs, eighteen inches wide and six feet long, with which they floor their houses. These, with constant rubbing and daily smoke, become dark and polished, so that their material can at first sight be hardly recog-

nized. What labour is here saved, to a savage with only his axe, who, if he wanted boards, must hew them out of the solid tree, and, with all his labour, could never produce a surface so smooth and beautiful as the bamboo, thus treated, affords him. Again, if a temporary house or shed is wanted, either by the traveller in the jungle or by the native in his paddy-fields, nothing is so convenient as the bamboo, with which a house can be constructed with half the labour and in half the time, than if any other material is used.

The hill Dyaks in the interior of Sarawak make paths for great distances, to their cultivated grounds, in the course of which they have to cross rivers and numerous gullies and ravines, or sometimes to avoid a long circuit, to carry the path along the face of a precipice. In all these cases the bridges they construct are of bamboo, and so admirably adapted is the material to the purpose, that it seems doubtful whether they would ever have made them had they not possessed it. The Dyak bridge is simple but well designed. It consists merely of bamboo poles, crossing each other at the roadway like the letter X, and rising, sometimes on one side, sometimes on both, three or four feet above it. At the crossing they are firmly bound together, and to a horizontal bamboo, which forms the only footpath, with another higher up, serving as a hand-rail. When a river is to be crossed, an overhanging tree is chosen, from which the bridge is partly suspended, and partly supported by diagonal struts from the banks, so as to avoid placing posts in the stream itself, when liable to floods. In carrying a path along the face of a precipice, trees and roots are made use of for suspension, from every little notch and crevice struts arise, while immense bamboos, of fifty or sixty feet long, are fixed on some bank or tree below. These bridges are traversed daily by men and women carrying heavy loads, so that any insecurity is soon discovered, and, as the materials are close at hand, immediately repaired. When the path goes over very steep and slippery ground, the bamboo is used to form steps. Pieces are cut, about a yard long, and opposite notches being made at each end, holes are formed, through which pegs are driven, and a ladder or staircase is produced with the greatest celerity. It is true that much of this will decay in one or two seasons, but it is so quickly replaced, as to make it more economical than using a more durable wood.

One of the most striking uses to which Bamboo is applied by the Dyaks, is in climbing the loftiest forest-trees, either to gather fruit or to obtain wax. The honey-bee of Borneo very generally makes its nest on the branches of the "Tappang," a tree which towers above all others in the forest, and whose smooth cylindrical trunk rises a hundred feet or more without a branch. Bees'-wax is one of the most valuable products of the forest, and the Dyaks climb these lofty trees at night to obtain it, by means of bamboo pegs driven into the wood. These pegs are formed of thick, old bamboo, split to about two inches wide. Each is cut above a joint, which forms a solid head to bear the blows of the mallet, and the point is flat and broad, cut away carefully to the siliceous outer coating. To the head of each is strongly tied a strip of the tough rind of a water-plant. The climber carries forty or fifty of these pegs in a basket by his side, and has a wooden mallet suspended round his neck; he has also prepared a number of strong, but slender bamboos, each from twenty to thirty feet long. One of these he sticks firmly in the ground at the foot of the tree, and close to it; he then drives in a peg as high as he can reach, and ties it firmly by the head to the bamboo; climbing up upon this, he drives in and ties two other pegs, each about three feet from the one below it, passing his arm between the tree and the bamboo, to hold the peg which he is driving in. He soon reaches the top of his pole, when another one is handed up to him, and being bound to the one below, he ascends in the same way another twenty feet. When his pegs are exhausted, a boy brings a fresh basket-

ful up to him, and a long cord enables him to pull up the bamboos as he requires them. This mode of ascent looks perilous, but is in reality perfectly secure. Each peg holds as tightly as a spike-nail, besides which the weight is always distributed over a great number of them by means of the vertical bamboos. Trees which branch at forty feet or less, are often ascended by pegs alone, which, besides being dangerous, requires much skill and activity in the climber, as he must grasp the middle peg firmly with his hand to hold himself up, and has but one hand at liberty to drive in the pegs. I have seen trees ascended by both methods, and admired the excellent qualities of bamboo, as well as the ingenuity of the Dyaks in taking advantage of them.

Split and shaved thin, bamboo is the strongest material for baskets; conical fish-traps, hencoops, and birdcages are made by splitting a piece up to the joint which forms the top, gradually-increasing circles of rattan being inserted below; rough fruit-baskets are also rapidly made in this manner. Aqueducts are formed by large bamboos split in half, supported on crossed poles of various heights. They are the Dyaks' only water-vessels, and are in fact superior to earthen vessels, being clean, light, and easily carried. A dozen water-bamboos stand in the corner of every Dyak house. They also make excellent cooking utensils; vegetables and rice are often boiled in them. They are used to preserve sugar, vinegar, honey, salted fruit or fish, – in fact, they answer every purpose for which jars and bottles are used by us. In a small bamboo case, prettily carved and ornamented, the Dyak carries his *sirih* and lime for betel-chewing, and his little long-bladed knife has a bamboo sheath. His favourite pipe is a huge hubble-bubble, which he will construct in a few minutes by inserting a small piece of bamboo for a bowl, at an acute angle, into a large cylinder, about six inches from the bottom, which contains water through which the smoke passes. In many other small matters the bamboo is of daily use, but enough has been here mentioned to show its value, as a substitute in many cases for iron, and in enabling the natives to dispense with a variety of tools and utensils.

The second object of my especial admiration is the Durian, a fruit of which we hear little in England, where all praise is given to the Mangosteen, while the Durian is generally mentioned as a fruit much liked by natives, but whose offensive smell renders it disagreeable to Europeans. There is however no comparison between them; the Mangosteen resembles a peach or a grape, and can hardly be said to be superior, if equal, to either; the Durian, on the other hand, is a fruit of a perfectly unique character; we have nothing with which it can be compared, and it is therefore the more difficult to judge whether it is or is not superior to all other fruits.

The Durian grows on a large and lofty forest-tree, something resembling an Elm in character, but with a more smooth and scaly bark. The fruit is round or slightly oval, about the size of a small melon, of a green colour, and covered with strong spines, the bases of which touch each other, and are consequently somewhat hexagonal, while the points are very strong and sharp. It is so completely armed that if the stalk is broken off it is a difficult matter to lift one from the ground. The outer rind is so thick and tough that from whatever height it may fall it is never broken. From the base to the apex five very faint lines may be traced, over which the spines somewhat curve and approximate; these are the sutures of the carpels, and show where the fruit may be opened with a heavy knife and a strong hand. The five cells are silky-white within, and are filled with a mass of firm, cream-coloured pulp, containing about three seeds each. This pulp is the eatable part, and its consistence and flavour are indescribable. A rich custard highly flavoured with almonds gives the best general idea of it, but there are occasional wafts of flavour

that call to mind cream-cheese, onion-sauce, sherry-wine, and other incongruous dishes. Then there is a rich glutinous smoothness in the pulp which nothing else possesses, but which adds to its delicacy. It is neither acid nor sweet nor juicy; yet it wants neither of these qualities, for it is in itself perfect. It produces no nausea or other bad effect, and the more you eat of it the less you feel inclined to stop. In fact, to eat Durians is a new sensation worth a voyage to the East to experience.

The smell of the ripe fruit is certainly at first disagreeable, though less so when it has newly fallen from the tree; for the moment it is ripe it falls of itself, and the only way to eat Durians in perfection is to get them as they fall. It would perhaps not be correct to say that the Durian is the best of all fruits, because it cannot supply the place of subacid juicy fruits such as the orange, grape, mango, and mangosteen, whose refreshing and cooling qualities are so grateful; but as producing a food of the most exquisite flavour it is unsurpassed. If I had to fix on two only as representing the perfection of the two classes, I should certainly choose the Durian and the Orange as the king and queen of fruits.

The Durian is however (in another way) dangerous. As a tree ripens the fruit falls daily and almost hourly, and accidents not unfrequently happen to persons walking or working under them. When a Durian strikes a man in its fall it produces a fearful wound, the strong spines tearing open the flesh, while the blow itself is very heavy; but from this very circumstance death rarely ensues, the copious effusion of blood preventing the inflammation which might otherwise take place. A Dyak chief informed me that he had been struck down by a Durian falling on his head, which he thought would certainly have caused his death, yet he recovered in a very short time.

Poets and moralists, judging from our English trees and fruits, have thought that there existed an inverse proportion between the size of the one and the other, so that their fall should be harmless to man. Two of the most formidable fruits known, however, the Brazil Nut (*Bertholletia*) and the Durian, grow on lofty trees, from which they both fall as soon as they are ripe, and often wound or kill those who seek to obtain them. From this we may learn two things: – first, not to draw conclusions from a very partial view of Nature; and secondly, that trees and fruits and all the varied productions of the animal and vegetable kingdoms, have not been created solely for the use and convenience of man.

The unripe Durian makes a very good vegetable, and it is also eaten raw. In a good fruit season the Dyaks preserve quantities of the pulp salted in jars and bamboos, in which state it will keep the year round, and is much esteemed as a relish with their rice. They seem hardly to appreciate the ripe fruit in its perfection, from the quantities they gather unripe, and from the small value they place upon it, as compared with the Jack and some other fruits. In Borneo great numbers of Durian trees have been planted on the mountains occupied by the Dyaks, and on the rivers' banks in the interior. In the jungle are found two varieties with much smaller fruits, one of them of an orange-colour inside; and these are probably the originals of the large and fine Durians which seem never to be produced in a wild state. In the tropics as well as in our colder climates, fruits always seem to be improved by cultivation.

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August 1856 letter from Ampanam, Lombok (S31)

Another letter from Wallace to his agent Stevens, printed in the January 1857 issue of Zoologist. This communication is notable for its first record of Wallace's realization that a great faunal discontinuity exists between the Indonesian islands of Bali and Lombok (here referred to by Wallace as "Baly" and "Lombock").

Another month has passed since I wrote to you, and there is still no chance of a passage to Macassar; having missed one opportunity by being away from the village, I am afraid to go out in the country any more, and here there are nothing but dusty roads and paddy fields for miles around, producing no insects or birds worth collecting: it is really astonishing, and will be almost incredible to many persons at home, that a tropical country when cultivated should produce so little for the collector: the worst collecting-ground in England would produce ten times as many species of beetles as can be found here, and even our common English butterflies are finer and more numerous than those of Ampanam in the present dry season; a walk of several hours with my net will produce perhaps two or three species of Chrysomela and Coccinella, and a Cicindela, and two or three Hemiptera and flies; and every day the same species will occur. In an uncultivated district which I have visited, in the south part of the island, I did indeed find insects rather more numerous, but two months' assiduous collecting have only produced me eighty species of Coleoptera! why there is not a spot in England where the same number could not be obtained in a few days in spring. Butterflies were rather better, for I obtained thirty-eight species, the majority, however, being Pieridæ; of the others, Papilio Peranthus is the most beautiful.

The birds have, however, interested me much more than the insects, as they are proportionably much more numerous, and throw great light on the laws of geographical distribution of animals in the East. The Islands of Baly and Lombock, for instance, though of nearly the same size, of the same soil, aspect, elevation and climate, and within sight of each other, yet differ considerably in their productions, and, in fact, belong to two quite distinct zoological provinces, of which they form the extreme limits. As an instance, I may mention the cockatoos, a group of birds confined to Australia and the Moluccas, but quite unknown in Java, Borneo, Sumatra and Malacca; one species, however (Plyctolophus sulphureus), is abundant in Lombock, but is unknown in Baly, the island of Lombock forming the extreme western limit of its range and that of the whole family. Many other species illustrate the same fact, and I am preparing a short account of them for publication. My collection here consists of sixty-eight species of birds, about twenty of which are probably not found west of the island, being species either found in Timor and Sumbawa or hitherto undescribed. I have here, for the first time, met with many interesting birds, whose structure and habits it has been a great pleasure to study, such as the Artamidæ and the genera Ptilotis, Tropidorhynchus, Plyctolophus and Megapodius.

The islands of Baly and Lombock are inhabited by Malayan races, closely allied to the Javanese. Baly has several rajahs, who are under the protection of the Dutch; Lombock has one rajah, who governs the whole, and is quite independent. These two islands are wonderfully cultivated, – in fact, they are probably among the best cultivated in the world: I was perfectly astonished when, on riding thirty miles into the interior, I beheld

the country cultivated like a garden, the whole being cut into terraces, and every patch surrounded by channels, so that any part can be flooded at pleasure; sometimes a hollow has the appearance of a vast amphitheatre, or a hill-side of a gigantic staircase, and hundreds of square miles of an undulated country have been thus rendered capable of irrigation, to effect which almost every stream has been diverted from its channel and its waters distributed over the country. The soil is a fine volcanic mould of the richest description, and the result of such a mode of cultivation is an astonishing fertility; the ground is scarcely ever unoccupied; crops of tobacco, Indian corn, sugar cane, beans and cucumbers, alternate with the rice, and give at every season a green and smiling appearance to the island: it is only on the summits of the hills and on the tops of the undulations, where water cannot be brought, that the ground is left uncultivated, but in these places a short turf gives food to the cattle and horses, which are very abundant, and clumps of bamboos with forest and fruit trees have all the appearance of an extensive park, and a pleasing contrast to the more regularly cultivated districts. I have been informed by parties capable of forming a judgment that in the best cultivated parts of Java so much labour has not been expended on the soil, and even the industrious Chinese can show nothing to surpass it: more than half the Island of Lombock consists of rugged volcanic mountains, which are quite incapable of cultivation, yet it exports more than 20,000 tons of rice annually, besides great quantities of tobacco, coffee, cotton and hides. Our manufacturers and capitalists are on the look-out for a new cotton-producing district: here is one to their hands. The islands of Baly, Lombock and Sumbawa can produce from ten to twenty thousand tons of cotton annually; it costs here uncleaned about 1½ cent a-pound; the qualities are various, – some, I believe, very good, so it can easily be calculated whether, after cleaning, it would pay.

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27 September 1856 letter from Macassar, Celebes (S32)

A letter printed in the Zoologist issue of April 1857, probably communicated by Samuel Stevens.

At length I am in Celebes! I have been here about three weeks, and as yet have not done much, except explored the nakedness of the land, – and it is indeed naked, – I have never seen a more uninteresting country than the neighbourhood of Macassar: for miles around there is nothing but flat land, which, for half the year, is covered with water, and the other half is an expanse of baked mud (its present state), with scarcely an apology for vegetation; scattered about it are numerous villages, which, from their being imbedded in fruit trees, have the appearance of woods and forests, but which, in fact, are little more productive to the insect collector than the paddy-fields themselves. Insects, in fact, in all this district there are absolutely none. I have got a bamboo-house near one of these villages, about two miles from the town, which does very well for my head-quarters: to get into the country is difficult, as it belongs to native princes, and there is no accommodation whatever for Europeans: there is, however, a patch or two of forest about six or eight miles off, and to it I have made several excursions, and got some birds and butterflies, but no beetles, which, at this season, seem altogether absent. I cannot help

comparing the facilities of the collector on the Amazon with the difficulties here: whether at Parà, Santarem, Barra, Obidos or Ega, or any other town or village, you may always find good forest collecting-ground within a few minutes' or half-an-hour's walk of the place, – you can live in the town, and collect in the country round. In no place in the East that I have yet seen can this be done: miles of cultivated ground absolutely barren for the naturalist extend round every town and village, and to get into the country with any amount of necessary luggage is most difficult and expensive: then, too, the necessaries of life, have all to be brought from the town, which renders living very dear; the only way of moving is by means of porters or small carriages, the cost of which is about ten times that of boat hire, and in many cases you must expose yourself to the risk of life and property, being beyond the sphere of any civilized government. However, I hope soon to make arrangements for a small house near the forest I have spoken of, where I can stay a week at a time, and then bring home and store my collections at my house near Macassar: already I can see that I shall get a pretty good collection of birds. Raptorial birds are abundant (the first place I have seen them so in the Archipelago); I have already seven species, one or two of which I have no doubt are new: of the forty species of birds I have already collected none are handsome, but several, I think, are new, among them a Cinnyris and a pigeon; the rare parrot, Prionitus platurus, is not uncommon here, though I have only obtained as yet only one specimen. Among my few butterflies are two Pieridæ, handsome and quite new, and two or three Danaidæ which I do not remember to have seen: I have as yet got no Papilios, but do not despair of soon obtaining some fine ones. The place where I hope to do best is Bontyne, about sixty miles from here: there is a road or path overland, but it would be very difficult to take all the luggage I require by that route, and by the sea, at the present time, owing to the wind being contrary, often takes from a fortnight to a month. In about January, however, the wind will be fair, and the trip is then only twenty-four hours, when I shall probably go there, as I am informed there is plenty of forest, and the highest mountains in the island are close by.

The people here have some peculiar practices. 'Amok,' or, as we say, 'running a-muck,' is common here; there was one last week: a debt of a few dollars was claimed by a man of one who could not pay it, so he murdered his creditor, and then, knowing he would be found out and punished, he 'run a-muck,' killed four persons and wounded four more, and died what the natives consider an honourable death! A friend here, seeing I had my mattrass on the floor of a bamboo-house, which is open beneath, told me it was very dangerous, as there were many bad people about, who might come at night and push their spears up through me from below, so he kindly lent me a sofa to sleep on, which, however, I never used, as it is too hot in this country.

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1857 letter from Dobbo, Aru Islands (S35)

Samuel Stevens read this letter and postscript to the Entomological Society of London meeting of 5 October 1857; later that year it was printed in their Proceedings series.

Dobbo, Arru Islands, March 10, 1857. – Here I am, alive, well, and hard at work. I

have been here just two months, and as I am going into the interior I leave this note to be sent by a vessel which returns to Macassar in April. The country is all forest, flat and lofty, very like the Amazonian forest. Insects, on the whole, are tolerably plentiful in specimens, but very scarce in species. There are, however, some fine things, and I am getting good series of several, including Ornithoptera sp., near Priamus, perhaps O. Poseidon, or close to it, a glorious thing but hard to get perfect; four or five other rare or new Papilios, but all are scarce; Cocytia d'Urvillei? rather scarce, a lovely creature; also Hestia d'Urvillei. For six weeks I have almost daily seen Papilio Ulysses? or a new closely-allied species, but never a chance of him; he flies high and strong, only swooping down now and then, and off again to the tree-tops: fancy my agony and disgust; I fear I shall never get him. There is a fine Drusilla or Hyades abundant, with numerous varieties; but the Lycænidæ and Erycinidæ are the gems; I only wish there were more of them; there are about half-a-dozen species equal to the very finest of the little Amazonians. The Coleoptera are far too few in species to please me: in two months' hard work I can only muster fifty Longicornes, a number I reached in ten days in Singapore; but Lamellicornes are the most extraordinarily scarce; I have only nine species, and four of them single specimens; there are, however, two fine Lomaptera among them, I hope new. All other groups are the same; Geodephaga, scarcely a dozen species, and nothing remarkable; not one Cicindela; only one Tricondyla (T. aptera?) and one Theretes (T. labiata), with not a single Colliuris; two or three fine Buprestes, however, and some remarkable Curculionidæ, with the beautiful Tmesisternus mirabilis, make a pretty good show.

On my way here we stayed six days at Ké Island, and I got there some very fine beetles, two fine Cetonias, and a Buprestis the most beautiful I have seen. Of the few insects I got there the greater part were different from any I have seen here, though the distance is only sixty miles, the mountains of Ké being visible from Arru in fine weather. This makes me think I shall get different things at every island in this part of the Archipelago. Arru is zoologically a part of New Guinea. Of the birds here half are New Guinea species; in the small island where we live many of the birds of Arru never come, such as the two species of the birds of Paradise, the black cassowary, &c. I am going now to the mainland, or great Island of Arru, in search of these birds, but have had the usual difficulty about men and boats.

I have learnt here all about New Guinea; parts are dangerous, parts not; and next year, if I live and have health, I am determined to go. I must go either to Banda or Ternate first, I have not yet decided which, and shall try and go to the large Island of Wargion,[a] at the north-east of New Guinea, where are found the Epimachus magnificus, three rare species of the Paradise birds, and the glorious Ornithoptera d'Urvilliana? The weather here is very changeable; storm, wind and sunshine alternately. I think nine-tenths of the things I am getting will be new to the English collections; with which comfort for our entomological friends, I remain yours sincerely, Alfred R. Wallace.

Postscript. – Dobbo, May 15. – I have returned from my visit to the interior, and the brig is not gone yet; so I add a postscript. Rejoice with me, for I have found what I sought; one grand hope in my visit to Arru is realized: I have got the birds of Paradise (that announcement deserves a line of itself); one is the common species of commerce, the Paradisea apoda; all the native specimens I have seen are miserable, and cannot possibly be properly mounted; mine are magnificent. I have discovered their true attitude when

[a] Likely a transcripton error for "Waigiou."

displaying their plumes, which I believe is quite new information; they are then so beautiful and grand that, when mounted to represent it, they will make glorious specimens for show-cases, and I am sure will be in demand by stuffers. I shall describe them in a paper for the 'Annals.' The other species is the king bird (*Paradisea regia*, Linn.), the smallest of the paradisians, but a perfect gem for beauty; of this I doubt if any really fine specimens are known, for I think Lesson only got them from the natives; I have a few specimens absolutely perfect. I have, besides, a number of rare and curious birds, – the great black cockatoo, racquet-tailed kingfisher, magnificent pigeons, &c., – and a fair addition to my insects and shells. On the whole I am so much pleased with Arru that my plans are somewhat altered: on returning to Macassar I shall probably not stay more than two or three months, but get as soon as I can to Ternate, and then to the north coast of New Guinea, where all the remaining species of Paradise birds are found? I believe I am the only Englishman who has ever shot and skinned (and ate) birds of Paradise, and the first European who has done so alive, and at his own risk and expense; and I deserve to reap the reward, if any reward is ever to be reaped by the exploring collector. I think there is good work for three years in N.E. Celebes, Gilolo Ceram, north coast of New Guinea, and intermediate islands, of all of which Ternate is near the centre, and it is certainly one of the least-explored districts in the world, and one which contains some of the finest birds and insects in the world. On the whole I have had much better health here than at Macassar, but I am now, and have been a whole month, confined to the house, owing to inflammation and sores on the legs, produced by hosts of insect bites. Confinement has brought on an attack of fever, which I am now getting over. My insect collecting has suffered dreadfully by this loss of time. – A. W.

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20 December 1857 letter from Amboyna, Moluccas (S44)

A portion of a letter to Samuel Stevens that was printed in a mid-1858 issue of Zoologist.

. . . To persons impressed with the idea of the prevalence of large insects in the tropics, my Macassar collections will appear most extraordinary; the average size is certainly less than that of our British species, and the colours not at all more brilliant. Of the Carabidæ (more than one hundred species), the greater part are under 4 lines and a very large number under 2 lines, whilst several under 1 line are perhaps the smallest of the family: the Brachelytra (eighty or ninety species) are, with the exception of about a dozen, very minute and obscure: the Rhynchophora are all small, and there are about one hundred species of minute Necrophaga, Xylophaga, &c., and about eighteen species of the elegant little Anthici, whilst the Longicornes, Buprestidæ and Cetoniæ, usually so abundant, are very scarce: if we were to take away some dozen purely tropical forms, the collection would have all the appearance of one from an extratropical and even northern locality, owing to the large proportion of Carabidæ, Staphylinidæ and Necrophaga, the small average size of the species and the obscurity of their colours.

Amboyna, where I am staying a month only, on my way to Ternate, offers a striking contrast to the country I have just quitted: it is eminently tropical; the number of large and handsome species in all orders of insects is perhaps greater than in any other place I have

visited, and the forms far more closely resemble those of Aru than of Borneo or Macassar; a number of the common species of the surrounding island are represented at Amboyna by others very closely allied or by varieties, but in almost every instance they are of larger size and more brilliant colours, – Papilio Severus and Ulysses are larger here than at Aru, whilst Deiphobus is larger than the closely allied Memnon of the Sanda Island or Ascalaphus of Macassar. In the Hymenoptera, the species of Vesipidæ and Pompilidæ are gayer than the allied species I have found in other countries; a Laphria and an Anthrax are larger than any Diptera I have yet found of the same genera; while the Coleoptera include the gigantic Eucheirus longimanus and a number of large and handsome Longicornes, Buprestidæ and Anthribidæ: it may be easily imagined, therefore, that Amboyna is a tempting place, well worth a thorough exploration, and I shall probably return to it unless I shall be able to visit Ceram, which I expect will contain almost all the Amboyna species, and probably many more, as is known to be the case with the birds. Though everybody says this is the dry and hot season, yet the weather has been terribly wet and windy, and during the twelve days I have now resided in a little hut in the jungle I have not had a single hot sunny day; here, as everywhere in the East, there is no forest left for many miles round the town, and there was the usual difficulty in finding a locality and a home, and in conveying my baggage. In the town I reside with Dr. Mohinke, the chief physician of the Moluccas, a German, an entomologist, and a very learned and hospitable man; he has lived in Japan, made a voyage to Jeddo, ascended volcanoes, and made collections: my pleasure may be imagined in looking over his superb collection of Japanese Coleoptera, large and handsome Longicornes and Lucani, tropical Buprestidæ and northern Carabi: he has also an extensive collection of Coleoptera made during many years' residence in Sumatra, Java, Borneo, and the Moluccas – a collection that makes me despair; such series of huge Prioni, Lamiæ and Lucani, Dynastidæ and Eucheirus! It is such collections that give, and have always given, such an erroneous idea of Tropical Entomology: these collections are made entirely by natives. Dr. Mohinke has resided here in Amboyna, for example, *two* years, and every native in the island knows that large and handsome beetles will be purchased by him; he has, therefore, hundreds of eyes spread over hundreds of square miles, and thus species which in ten years might never once occur to a single collector, are inevitably obtained by him in greater or less abundance, whilst the smaller, more active, and much more common species are never brought at all. The Eucheirus is evidently rare, yet Dr. Mohinke has a fine series, obtained at intervals from different localities; he also sends bottles and casks of arrack to the Dutch officers resident in different islands, and though he sometimes has them returned crammed full of a single species of common Calandra or Passalus, yet he occasionally gets some magnificent insects. I believe myself that, as a general rule, beetles are rare exactly in proportion to their size, rare both in species and in individuals; in four years' almost daily search in the Eastern forests I have never found a large Prionus myself, and I have collected nearly four thousand species of Coleoptera: such collections as those of Mr. Bates and myself, made in such distant countries (both generally considered among the richest in large species), are what show the true nature of tropical insects, and I believe that a careful examination of these will lead to the conclusion that there is no superiority whatever in the average size of tropical Coleoptera over those of temperate climates, and that in many groups the latter have the decided advantage. – A. R. Wallace.

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29 October 1858 letter from Batchian, Moluccas (S48)

This extract from a letter to Stevens was read at the Zoological Society of London meeting of 22 March 1859, and reported in the pages of their Proceedings series later that year.

Here I have been as yet only five days; but from the nature of the country, and what I have already done, I am inclined to think it may prove one of the best localities I have yet visited. Birds are as yet very scarce; but I still hope to get a fine collection, though I believe I have already the *finest and most wonderful* bird in the island. I had a good mind to keep it a secret, but I cannot resist telling you. I have a new *Bird of Paradise*! of a new genus!! quite unlike anything yet known, very curious and very handsome!!! When I can get a couple of pairs, I will send them overland, to see what a new Bird of Paradise will really fetch. Had I seen the bird in Ternate, I should never have believed it came from *here*, so far out of the hitherto supposed region of the *Paradiseidæ*. I consider it the *greatest* discovery I have yet made; and it gives me hopes of getting other species in Gilolo and Ceram. There is also here a species of Monkey – much further eastwards than in any other island; so you see this is a most curious locality, combining forms of the East and West of the Archipelago, yet with species peculiar to itself. It also differs from all the other Moluccas in its geological formation, containing iron, coal, copper, and gold, with a glorious forest vegetation and fine large mountain streams: it is a continent in miniature. The Dutch are working the coals; and there is a good road to the mines, which gives one easy access to the interior forests.

I can do nothing at drawing birds, but send you a horrible sketch of my discovery, that you may not die of curiosity. I am told the wet season here is terrible, and it begins in December; so I shall probably have to leave then.

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28 January 1859 letter from Batchian, Moluccas (S50)

Mr. Stevens communicated the following extract from a Wallace letter to the Entomological Society of London meeting of 6 June 1859. It was later printed in their Proceedings series.

I had determined to leave here about this time, but two circumstances decided me to prolong my stay – *first*, I succeeded at last in taking the magnificent new Ornithoptera, and, *secondly*, I obtained positive information of the existence here of a second species of Paradisea, apparently more beautiful and curious than the one I have obtained. You may perhaps imagine my excitement when, after seeing it only two or three times in three months, I at length took a male Ornithoptera. When I took it out of my net, and opened its gorgeous wings, I was nearer fainting with delight and excitement than I have ever been in my life; my heart beat violently, and the blood rushed to my head, leaving a headache for the rest of the day. The insect surpassed my expectations, being, though allied to Priamus, perfectly new, distinct, and of a most gorgeous and unique colour; it is a fiery golden orange, changing, when viewed obliquely, to opaline-yellow and green. It is, I think, the

finest of the Ornithoptera, and consequently the *finest butterfly in the world?* Besides the colour, it differs much in markings from all of the Priamus group. Soon after I first took it I set one of my men to search for it daily, giving him a premium on every specimen, good or bad, he takes; he consequently works hard from early morn to dewy eve, and occasionally brings home one; unfortunately several of them are in bad condition. I also occasionally take the lovely Papilio Telemachus, *n.s*.

I have sent off a small box containing four males, one female, and one young bird of the new Batchian Paradisea, besides one red-ticketed private specimen; six males and five females of the new Ornithoptera, and seven Papilio Telemachus.

Tell Mr. Gray and Mr. Gould that the Paradisea had better not be described yet, as I am making great exertions to get the second species, evidently of the same genus, which will enable a generic character to be more accurately given. The butterflies, I trust, will be both figured, male and female, either in Mr. Hewitson's book or in Ent. Soc. Trans. For the Ornithoptera I propose Crœsus as a good name. Butterflies are scarce; good beetles turn up occasionally, but nothing very grand. I have now a handsome series of Buprestidæ, and a remarkably pretty lot of Longicorns; one of my last acquisitions is a grand bronzy Tmesisternus, 1½ inch long, a single specimen only. In almost all orders, and in birds, there is a deficiency of species; yet there are so many pretty and brilliant things, and a few so grand and new, that on the whole I am inclined to think my Batchian collection will be the best I have made anywhere.

Another reason which may induce me to stay perhaps two or three months longer at Batchian is that I have had no fever here, which I have never been free from two months at a time for the last two years before; and I may therefore hope to get my health well established for my next journey to New Guinea.

The butterflies will make a show-box which will, I think, be admired almost as much as the birds of Paradise.

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22 October 1859 letter from Amboyna (S58)

Extracts from letter, probably to P. L. Sclater, printed in the second volume of his journal The Ibis.

. . . I have just packed up a large collection of Gilolo and Ternate birds, as well as those from Menado. The former are a much gayer lot, comprising a fine series of *Pitta maxima*, a new *Megapodius*, I think, handsomely banded on the back, and a *Semioptera*, which differs a little from the Batchian specimens in the much greater length of the breast plumes and other details. Is the *Calœnas* the true *nicobarica*? If so, it is a unique case of a true land-bird ranging through the whole Archipelago, and beyond its limits from the Andamans to New Guinea. I do not know where Bonaparte got his information about its being arboreal. Here it is truly terrestrial, perching only to rest and sleep.

It is astonishing how little care even professed naturalists have given to determining localities. The localities of species given by the 'Dutch Scientific Commission' are full of errors. *Ptilonopus monachus* and *P. hyogaster* are given to Celebes, whereas they are unknown there, but are abundant in Gilolo and Batchian; and exactly the same error is

made with *Macropygia reinwardti*, which you will see in my collections, but not from Celebes. *Todiramphus funebris* is also unknown in Celebes, but common in Gilolo, so that the Dutch naturalists seem to have placed all their species of unknown locality in Celebes, acting as the French have done in giving to the little island of Vanikoro hundreds of insects which were never found there.

Among the other interesting species from Gilolo are a *Ptilonopus* and a *Platycercus* – both, I think, new; the beautiful *Ianthœnas halmaheira*, Bp., and several fine aquatic birds and Waders.

In a few days I commence work in Ceram, where I hope to make a very fine collection, especially of *Psittacidæ*, the Lories of Ceram surpassing even those of New Guinea in variety and beauty. I live in hopes too of a new *Semioptera*, or some equally interesting form.

I take every opportunity of purchasing live specimens of Parrots from the islands I may probably not visit, and hope to get most valuable materials for elucidating their distribution in the East, which is in the highest degree interesting. Between the *Lorius garrulus* of Gilolo and that of Batchian there is a constant difference in the size of the dorsal yellow patch: are they considered distinct species?

The species of Ceram birds mentioned in Bonaparte's 'Conspectus' are very few: how is it, then, that it has such a name for fine birds? I know nothing fine from it, but the Lories, which are superb. However, I hope and believe it will produce some very fine things – new Pigeons, perhaps. The Cassowary is said to be abundant in Ceram, and to be the same as the New Guinea species. The *Tanysipteræ* are very puzzling: which is the true *T. dea*, Linn.? The Dorey and Ternate specimens seem almost identical, and in G. R. Gray's list, New Guinea specimens are put as *T. dea*. If so, then the larger white-tailed species found in Amboyna and Ceram is undescribed, and is perhaps the same as the white-tailed specimens from the Kaisa Islands, sent with my Batchian collections. The *Carpophaga perspicillata* of Amboyna differs also from those of Gilolo and Batchian in the much lighter colour of the head. Now, I believe in all these cases, where the difference is *constant*, we must call them *distinct species*. A 'permanent local variety' is an absurdity and a contradiction; and, if we once admit it, we make species a matter of pure opinion, and shut the door to all uniformity of nomenclature."

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7 December 1860 letter from Ternate (S61)

A letter to Samuel Stevens printed in The Ibis issue of April 1861.

I returned to Ternate a few days after the last mail had left here, having had a most hazardous voyage from Ceram and Waigiou. My collections are immense, but very poor, when it is considered that they are the result of nine months' collecting by two persons in East and North Ceram, Mysol, and Waigiou. Ceram is a wretched country; and the Papuan Islands, now that the cream is taken off by Aru and Dorey, are really not worth visiting, except for the Birds of Paradise.

My beetles, I am sorry to say, are most miserable – smaller and more obscure species than at Dorey, and only a few of the good ones found there, and none in any quantity.

In birds there is absolutely nothing good but the *Paradisea rubra*, which is the only species that inhabits Waigiou, and is peculiar to that island.

I have been so busy with my mass of specimens (all wanting sorting and cleaning), and with my numerous letters and books (a whole year), that my mind has been too much unsettled to write. Next mail I shall write to all my entomological and ornithological friends who have been kind enough to send me communications.

I do not like the figure of *Semioptera wallacii* copied in 'The Ibis' from Gould's: the neck-shields are not shown to advantage; and the white plumes should be raised much higher or laid down lower – they are neither one thing nor the other.

C. Allen starts in a week or two for N. Guinea – to the true locality for the rarer Birds of Paradise, and I trust he may be successful. The last voyage, with all its dangers and disappointments, has nearly sickened me, and I think in *one year* I shall return.

I seem to have all your letters but one (April 16, 1860).

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Section 2. Natural Selection, Adaptation and Darwinism

Introduction

Wallace is, of course, best known for his contributions to the theory of evolution by natural selection. These came most visibly in the form of numerous technical and popular articles and several books, but he was also well represented on the subject in the correspondence columns of the day. Some of these discussions continued on through several issues, back-and-forths resembling a dual, or perhaps combat between well-armed gladiators. Here, only Wallace's sides of the questions are reproduced, but generally speaking he was sensitive to readers' needs, and kept his adversaries' points well within view.

With respect to economy of expression and clearness of argumentation, many of these items represent Wallace at his best as a writer. His letters on perception and instinct in lower animals are classics; so too, those on the inheritance of acquired characters, and Lamarckism. Many of his letters on evolution-related subjects appeared in the widely-read journal *Nature*, and must have produced a considerable influence on the thinkers of the time.

It is not possible in the space allowed to reproduce *all* of Wallace's letters to the Editor on these subjects, but those that do appear here tackle all the areas with which Wallace is usually associated, and a few with which he is not. Included are discussions of mimicry and protective resemblance, instinctive behavior, perception, animal locomotion, George Romanes's theory of physiological selection, Lamarckism, the inheritance of acquired characters, character variation, sexual selection, recognition marks, panmixia, the utility argument, etc., etc.

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Wallace's first in-print defense of Darwinian principles came as late as November 1863, in a prickly analysis of the reasons for the shape of bees' cells. In a pair of early (21 January and 11 February, respectively) 1865 letters to the London review The Reader, things got even more personal.

The 'British Quarterly' and Darwin (S107)

Allow me to call the attention of your readers to a very gross attempt, in the last number of the *British Quarterly Review*, to mislead the unscientific public. At p. 143, in an article on the 'Supernatural,' after disposing of Hume, Strauss, Baden Powell, and such small fry, in a few lines each, the reviewer claims physical science as his ally, and calls into the witness-box 'the geologist' (one of a type now happily almost extinct), who, he says, will tell us that again and again the special interference of a Creator has been re-

quired, and who finally 'will tell you that this same "development" or "origin of species by natural selection" is an unblushing intruder into the domain of science, unlicensed and unrecognized.' This, however, is not strong enough. The model 'geologist' is sent down, and a new witness is specially called in a *note*, which is so 'unblushing' that I give it entire.

> Let us hear a word on the subject of development from one who has won scientific laurels by a life of study and thought: – "All the great living and recently deceased masters of physical science reject it. Does it appeal to anatomy and physiology? Cuvier, Owen, and Carpenter cry out against it. Does it evoke the aid of chemistry? Berzelius, Turner, and Liebig see its shallowness. Does it call on zoology for aid? Agassiz and Ehrenberg can refute its claims. Does it search the archives of geology for support? Sedgwick, Miller, Lyell and D'Orbigny can show how certainly it will fail. Or, finally, does it appeal to botany? Hooker and Lindley, Torrey and Gray, know that it will certainly glean nothing to sustain it in that flowery field. The fact is that it is only here and there a second-rate naturalist will sympathize at all with such dreamy views." (Dr. E. Hitchcock, in "Bibliotheca Sacra," vol. xi. p. 789.) We do not think *anything in this extract unwarranted*, even though Mr. Darwin has added his name to the roll of non-theistic theorists; for though he is distinguished as a naturalist in the department of observation, his book exhibits philosophic abilities of the lowest order. Nothing can be more significant than his entire abandonment of geology; nothing more foolish than the supposition that some strata are so lost that no trace of them can be found; and nothing more unscientific than to help his theory to take its absurd shape out of the barely possible but utterly unknown.

Either the writer of this article knew that at least four of the persons here mentioned – Carpenter, Lyell, J. Hooker, and A. Gray – so far from *rejecting* or *crying out against* 'development' and 'the origin of species by natural selection' are its strongest supporters, or he did not know it. He is, therefore, either imposing a deliberate and wilful misstatement on the public, or he is incredibly ignorant of the subject he is writing upon. Again, when he talks of Darwin's 'entire abandonment of geology,' does he know that almost all the great modern geologists are converts to his views? and when he stigmatizes Darwin's work as 'foolish' and 'absurd,' does he know that John Stuart Mill has adduced it as one of the most wonderful examples of logical reasoning extant?

It is hardly worth while to break such a fly upon the wheel, but it is well to make known as widely as possible to what weak subterfuges those who attempt to stem the flood of modern thought with the worn-out theological mop are at last driven. – A. R. W.

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The British Quarterly Reviewer and Darwin (S108)

It is related that in a certain 'case' where the defendant had not a leg to stand upon, his counsel's brief contained instructions 'to abuse the plaintiff's attorney.' In like manner the 'British Quarterly Reviewer' finds it convenient to make remarks on my supposed 'youth' and 'warmth of temper' (referring to both *twice* in the course of his reply), while he does not attempt to disprove my main charge, viz. that his statements would give to any reader, previously unacquainted with the subject, a totally false idea of the nature of the Darwinian hypothesis and of the reception it has met with among men of science.

That the extract from Dr. Hitchcock's work *did* not originally refer to Darwin was

perfectly clear to every one; but the very point of my objection was, that the Reviewer himself *applied it* to Darwin – first, by putting it as a note to a statement that 'development' or 'origin of species by natural selection' (using the very title of Darwin's work) 'was unlicensed and unrecognized in the domain of science;' and then adding himself: 'We do not think *anything* in this extract *unwarranted*, even though Mr. Darwin has added his name,' &c. If this is not making Dr. Hitchcock's words refer to Darwin's doctrines as well as to Lamarck's, there is no meaning in language.

Again, the Reviewer's own extracts from Mill's Logic (the meaning of which I think I fairly gave from memory in a few words) answer my purpose completely, for the man who, in the opinion of one of the first thinkers of the age, has produced 'an unimpeachable example of a legitimate hypothesis,' and has performed 'a wonderful feat of scientific knowledge and ingenuity,' must certainly have reasoned logically and well, and cannot honestly be stigmatized as 'foolish,' 'unscientific,' and 'absurd,' words which the Reviewer has applied to Darwin or his work.

The continued opposition of a few of our *oldest* geologists cannot be held to prove Darwin's 'entire abandonment of geology,' still less that 'nothing can be more foolish than the supposition that *some* strata are so lost that no trace of them can be found.' The Reviewer may find something that he will perhaps consider far 'more foolish' in the annual addresses for 1863 and 1864 of the late President of the Geological Society, Professor Ramsay, in which he adduces an immense mass of facts to prove that in the Palæozoic series alone there are *ten distinct breaks* in the succession of strata, each probably indicating a longer time than that occupied by a single formation; and then states his opinion that 'each of these breaks implies a *lost epoch*.' In the same address Professor Ramsay speaks of 'Mr. Darwin's more philosophical argument of descent with modification.'

I have purposely avoided introducing new subjects of discussion, it being simply my object to show that, however hard the 'British Quarterly Reviewer' may have succeeded in hitting me, his own reply has confirmed rather than disproved the charges in my first letter. – A. R. W.

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Natural Selection (S123)

The concept of mimetic resemblance had been introduced by Wallace's old friend Henry Walter Bates in 1862, but it was by no means a fully worked-out – or accepted – theory as of 1866. This letter appeared in the 1 December 1866 issue of The Athenaeum.

At the last meeting of the Entomological Society, the subject of "Mimicry," or protective resemblances, was brought forward by Prof. [J. O.] Westwood and myself, and during the discussion that ensued some objections were made to the explanation of the phenomena first given by Mr. Bates, and adopted by me as the only sufficient one. Time did not permit of an answer being made to those objections, and as they are at first sight very plausible, and were brought forward not by one of Mr. Darwin's opponents, but by a gentleman who fully admits the great principles of evolution and development in organic nature, they may probably have weight with some persons. Believing, however, that they

are entirely unsound, may I beg a little of your space to give my reasons for rejecting them.

Mr. [David] Sharp stated that four different causes might be sufficient to produce the phenomena of "mimicry" more or less completely, viz., first, accidental resemblances; secondly, similar conditions of life; thirdly, heredity, or reversion to a common ancestral type; and, fourthly, the preservation of useful variations.

To the first, or accidental resemblances, it was admitted that very few, if any, of the cases adduced by Mr. Bates or myself could be due. The last is the one we adopt. The second and third only remain, and these, Mr. Sharp argued, would account for most, or perhaps all, of our cases without the agency of natural selection at all. Now, all I can admit is, that in some cases of very closely allied species of the same or of closely allied genera, an accurate external resemblance, such as we term "mimicry," might possibly be produced either by "heredity," or by the action of like conditions. But in all the cases in which the insects resembling each other belong to distinct *orders*, or distinct *families*, or to *genera* not intimately allied, or even to well-marked *sections* of extensive genera, I entirely deny that either or both of these causes could have produced the whole series of phenomena presented by *mimicking* insects, and for the following reasons, which appear to me sufficiently conclusive: –

1. In all cases of mimicry, the resemblance of the one species to another in a different group is entirely superficial, and is always strictly confined to those characters which cause the one to *look like the other*. The structure, the habits, the form of inconspicuous parts, the colour of inconspicuous parts, the nature of the food, or the character of the larva and pupa, are not, as far as we know, ever modified in a similar manner. But if such general causes as "heredity" or "similar conditions" produced resemblances, these resemblances should affect various parts of the organization, not those conspicuous to the eye only. The effect being limited with strict reference to external resemblance, seems to me a fatal objection to referring it to any cause or causes of a general nature.

2. There are no grounds for believing that minute details of colouration and marking are due to climatal conditions at all, still less that they can be produced so identically alike in species of groups widely differing in organization; neither is there any evidence that such details are ever continued by heredity to one species only in each of two distinct family groups which contain hundreds of other variously-coloured species.

3. It is only a very few groups of insects which are the subjects of imitation by many other groups. But "heredity" should affect nearly all groups not too remotely allied; and "common conditions" should affect all species inhabiting the same forests with some approach to an average frequency. The fact that there is no such miscellaneous character in the resemblances (the group of Danaioid butterflies being the mimicked in the great majority of cases) tells us plainly that no causes affecting all insects alike can be at the bottom of this curious phenomenon.

4. Protective resemblance to a species of a distinct order sometimes occurs, as in the curious *Orthopterous* insect adduced by Prof. Westwood, which had been always taken for a *Coleopterous* insect that inhabits the same country (*Tricondyla* sp.). Neither "heredity" nor "like conditions" can be called in here; yet the phenomenon is so similar to that of the mimicking butterflies, that the idea of a similar cause in both instances is irresistibly forced upon us.

5. Resemblances of the most perfect kind occur between insects and inanimate objects. Phasmidæ imitate sticks, leaves, or moss most wonderfully. The larvæ of Ge-

ometræ also imitate sticks. Thousands of tropical Coleoptera imitate bark (and it is always those that cling to bark); others that sit motionless on leaves cannot be distinguished from the dung of birds dropped on a leaf. These are most clearly *protective imitations*, and they can none of them possibly be produced by "heredity" or "similarity of conditions," but, if produced at all by natural causes, seem clearly due to the continued preservation of useful variations. The mimicry of other insects is equally protective, and there is every probability that both were produced in a similar manner.

6. This is rendered still more certain by the fact that in both classes of resemblance it is the female only that is most frequently protected, for reasons which I have already explained, but cannot now enter into. It is only the female "leaf-insect" that is so wonderfully like a leaf; and in many species of Pieris and Diadema it is the females only that mimic Heliconias and Euplæas. This fact alone renders it in the highest degree improbable that the two groups of phenomena should have been due to distinct causes, even if the preceding arguments had not shown us how impossible it was to explain any of the main features of "mimicry" by such causes as "heredity" or "the action of like external conditions."

For these reasons it appears to me indisputable that "natural selection," or the continued survival of variations useful to the possessor, is the only theory yet before us which is capable of explaining the whole of the facts presented by "mimicking insects."

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Caterpillars and Birds (S130)

A letter printed in the 23 March 1867 issue of The Field. Wallace and Darwin had discussed this matter in private correspondence the month before, with Darwin exclaiming "Bates was quite right, you are the man to apply to in a difficulty. I never heard anything more ingenious than your suggestion . . ."

Sir, – May I be permitted to ask the co-operation of your readers in making some observations during the coming spring and summer, which are of great interest to Mr Darwin and myself. I will first state what observations are wanted, and then explain briefly why they are wanted. A number of our smaller birds devour quantities of caterpillars, but there is reason to suspect that they do not eat all alike. Now we want direct evidence as to which species they eat and which they reject. This may be obtained in two ways. Those who keep insectivorous birds, such as thrushes, robins, or any of the warblers (or any other that will eat caterpillars), may offer them all the kinds they can obtain, and carefully note (1) which they eat, (2) which they refuse to touch, and (3) which they seize but reject. If the name of the caterpillar cannot be ascertained, a short description of its more prominent characters will do very well, such as whether it is hairy or smooth, and what are its chief colours, especially distinguishing such as are green or brown from such as are of bright and conspicuous colours, as yellow, red, or black. The food plant of the caterpillar should also be stated when known. Those who do not keep birds, but have a garden much frequented by birds, may put all the caterpillars they can find in a soup plate or other vessel, which must be placed in a larger vessel of water, so that the creatures cannot escape, and then after a few hours note which have been taken and which left. If the vessel could be placed where it might be watched from a window, so that the kind of birds which took

them could also be noted, the experiment would be still more complete. A third set of observations might be made on young fowls, turkeys, guineafowls, pheasants, &c., in exactly the same manner.

Now the purport of these observations is to ascertain the law which has determined the colouration of caterpillars. The analogy of many other insects leads us to believe that all those which are green or brown, or of such speckled or mottled tints as to resemble closely the leaf or bark of the plant on which they feed, or the substance on which they usually repose, are thus to some degree protected from the attacks of birds and other enemies. We should expect, therefore, that all which are thus protected would be greedily eaten by birds whenever they can find them. But there are other caterpillars which seem coloured on purpose to be conspicuous, and it is very important to know whether they have another kind of protection, altogether independent of disguise, such as a disagreeable odour and taste. If they are thus protected, so that the majority of birds will never eat them, we can understand that to get the full benefit of this protection they should be easily recognised, should have some outward character by which birds would soon learn to know them and thus let them alone; because if birds could not tell the eatable from the uneatable till they had seized and tasted them, the protection would be of no avail, a growing caterpillar being so delicate that a wound is certain death. If, therefore, the eatable caterpillars derive a partial protection from their obscure and imitative colouring, then we can understand that it would be an advantage to the uneatable kinds to be well distinguished from them by bright and conspicuous colours.

I may add that this question has an important bearing on the whole theory of the origin of the colours of animals, and especially of insects. I hope many of your readers may be thereby induced to make such observations as I have indicated, and if they will kindly send me their notes at the end of the summer, or earlier, I will undertake to compare and tabulate the whole, and to make known the results, whether they confirm or refute the theory here indicated. – Alfred R. Wallace, 9 St. Mark's-crescent, Regent's Park, N.W.

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The Philosophy of Bird's Nests (S167aa)

Wallace sent this letter to The Echo, a liberal London newspaper, where it was printed in its issue of 10 June 1870. In an 1867 paper reprinted in his collection Contributions to the Theory of Natural Selection in 1870, Wallace had expressed the opinion that nest-building in birds was a learned – not instinctual – behavior. Not everyone agreed with him.

Sir, – Your correspondent, Mr. George Rooper, who severely criticises what he supposes to be my views in your paper of the 8th inst., is so evidently well acquainted with the habit of birds that I regret he did not take the trouble to read my book before he took up his pen to write on the subject. A criticism at second-hand is hardly likely to be a just one.

I am, however, always pleased to be criticised by one who has practical knowledge, however much he may misunderstand me, and as Mr. Rooper adduces some interesting

facts which seem to me strongly to corroborate my theory, I will, with your permission, make a few remarks on his letter.

Mr. R. asserts that "my first axiom that birds build with the materials readiest at hand, is absolutely contrary to fact; birds invariably seek the materials of their nest at a distance." At a distance from what? I would ask. Not surely at a distance from the places they frequent daily in their search for food, though it may be at a distance from the nest; and the former, not the latter, is my axiom. Take the case of the rooks. They are great wanderers, going miles away during the day, but returning to the rookery at night. They spread themselves over the country to seek food, for the simple reason that the whole population of a rookery could not live a week if confined to the immediately adjacent fields, and neither could all find materials for their nests so quickly and so easily in the trees they inhabit as in the wide extent of country they daily frequent.

The fact of the grebe bringing up bits of weed from the bottom to make its nest is new to me, and is very interesting, since it exactly accords with my theory. For is not the grebe pre-eminently a diver? Does it not live by diving, and bring all its food out of the water; and is not the fact that it also brings weed for its nest out of the water instead of from its surface, strikingly accordant with the view, that birds use those materials for their nests that come most directly in their way during their daily search for food.

As for the water-ouzel bringing dry oak leaves home from "incredible distances," the fact, of which Mr. R. gives no proof, seems to me incredible; because, in Wales and Devonshire, where the bird abounds, oaks are one of the commonest trees, and grow, more or less plentifully, on the banks of most streams.

Mr. R. denies that the kingfisher makes a nest at all, and says that the supposed nest is merely the dirty bird's dunghill. But Mr. Gould[a] has himself obtained a perfect hollow nest, formed of fish-bones, so delicately white and so beautifully put together, as to be quite an ornamental object. If Mr. R. will call and see this nest, I think he will withdraw his accusation against the poor kingfisher.

Mr. R. also denies that birds alter and improve their nests as occasion requires, but I have stated many facts which prove the contrary; and quite recently M. Pouchet[b] has shown that the common swallow has, during the last forty years, materially altered its mode of nest-building at Rouen, and that *the alterations are decided improvements*. An account of M. Pouchet's observations is given in *Nature* for April 7th last.

The close similarity of nests of the same species of bird at the present day is admitted, but is nothing to the purpose, since my statement is, that changes occur slowly in relation to changed conditions of the bird itself, or its surroundings. Unfortunately we have no nests of prehistoric or fossil birds, and can therefore only determine the question by reason and analogy.

Though the nests of the wren, the robin, or the nightingale may be rough externally, or of apparently loose and flimsy texture, yet they are smooth and regular within, and are so well constructed that the materials cling together and form a compact and tolerably strong abode for the young birds. It is certain, therefore, that they cannot be put together in the "careless, unmethodical manner" Mr. R. thinks they are, but must be, to some extent, formed like a woven fabric, implying both activity and delicacy in the bills and feet

[a] John Gould (1804–1881), English ornithologist.

[b] Félix-Archimède Pouchet (1800–1872), French naturalist, and a leading proponent of the theory of spontaneous generation.

of the builders.

These, and many more of Mr. Rooper's objections, are sufficiently answered in my volume of Essays, where he will see that the fact of the female redbreast being conspicuously coloured, is perfectly consistent with my theory, *since the colour is hidden while she is sitting on the nest*, while the hen goldfinch is certainly less vividly coloured and less conspicuous than the cock birds. – Alfred R. Wallace, Holly House, Barking, E.

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The 17 November, 1 December, and 8 December 1870 issues of Nature contain Wallace's side of a multi-directioned exchange with Alfred W. Bennett, a mathematically-inclined botanist.

Natural Selection – Mr. Wallace's Reply to Mr. Bennett (S175)

Mr. A. W. Bennett's article entitled "The Theory of Natural Selection from a Mathematical Point of View," contains several criticisms on my own writings, and touches on some points which have not yet been fully discussed. I propose, therefore, to reply to such of these as appear to be of sufficient importance.

The first objection brought forward (and which had been already advanced by the Duke of Argyll) is, that the very title of Mr. Darwin's celebrated work is a misnomer, and that the real "origin of species" is that spontaneous tendency to variation which has not yet been accounted for. Mr. Bennett further remarks, that throughout my volume of "Essays"[a] I appear to be unconscious that the theory I advocate does not go to the root of the matter; and this unconsciousness is not apparent only, for I maintain, and am prepared to prove, that the theory, if true, does go to the root of the question of the origin of species. The objection, which, from its being so often quoted and now again brought forward, is evidently thought to be an important one, is founded on a misapprehension of the right meaning of words. It ignores the fact that the word "species" denotes something more than "variety" or "individual." A species is an organic form which, for periods of great and indefinite length as compared with the duration of human life, fluctuates only within narrow limits. But the "spontaneous tendency to variation" is altogether antagonistic to such comparative stability, and would, if unchecked, entirely destroy all "species." Abolish, if possible, selection and survival of the fittest, so that every spontaneous variation should survive in equal proportion with all others, and the result must inevitably be an endless variety of *unstable forms*, no one of which would answer to what we mean by the word "species." No other cause but selection, has yet been discovered capable of perpetuating and giving stability to some forms and causing the disappearance of hosts of others, and therefore Mr. Darwin's book, if there is any truth in it at all, has a logical claim to its title. It shows how "species," or stable forms, are produced out of unstable spontaneous variations; which is certainly to trace their "origin." The distinction of "species" and "individuals" is equally important. A horse or a number of horses, as such, do not constitute a species. It is the comparative *permanence* of the form as distinguished from the ass, quagga, zebra, tapir, camel, &c., that makes them one. Were there a mass of intermediate

[a] *I.e.*, *Contributions to the Theory of Natural Selection* (Macmillan, 1870).

forms connecting all these animals by fine gradations, and hardly a dozen individuals alike – as would probably be the case had selection not acted – there might be a few horses, but there would be no such thing as a species of horse. That could only be produced by some power capable of eliminating intermediate forms as they arise, and preserving all of the true horse type, and such a power was first shown to exist by Mr. Darwin. The origin of varieties and of individuals is one thing, the origin of species another.

Mr. Bennett next discusses the phenomena of "mimicry," and proposes to show, by mathematical calculations, that the effects could not be produced by natural selection. But, at the very outset, he makes an important error, which seriously affects his subsequent reasonings; for he leads his readers to understand that there is only one completely mimicking species of *Leptalis*, while the majority are of the normal white-butterfly type. The fact is, however, that but few species of *Leptalis* retain the simple colouring of their allies the Pieridæ, while the great majority are either coloured like the Heliconidæ, or show a considerable amount of colour or marking in that direction. He is also apparently unaware that some Heliconidæ (*Ithomia eurimediæ,* for example) approximate in colour to the normal white and yellow species of *Leptalis*, and thus renders it much less difficult to understand how a sufficient amount of variation in colour might occur at a first step, to produce a resemblance which, viewed at some considerable distance, would be deceptive, and therefore useful.

We next come to the demonstration by means of figures, and we here find still more serious errors. Mr. Bennett says, that supposing a *Leptalis* may vary in twenty different ways, one only being the direction required, – "the chance of any individual producing a descendant which will take its place in the succeeding generation varying in the required direction, is 1/20; the chance of this operation being repeated in the second generation is $1/20^2 = 1/400$; the chance of this occurring for ten successive generations is $1/20^{10}$, or about one in ten billions;" whence it is concluded that there are overwhelming chances against any progressive variation in the right direction ever taking place. But first, I do not admit the assumption that only one variation out of twenty would be in the right direction; when it is remembered how great is the variety of the Heliconidæ, both in colour and marking. It seems more likely that one-fourth or one-third at least would help to approximate to some of them, and thus be useful. Taking, however, Mr. Bennett's own figures, there are three great oversights in this one short sentence. The first is, that each *Leptalis* produces, not one only, but perhaps twenty or fifty offspring; the second is, that the right variation has, by the hypothesis, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful, causing the chance of the right variation being reproduced to become greater and greater.

Now with these three modifications the weight of the argument is entirely destroyed; for, allowing the *Leptalis* to produce only twenty offspring (a small number for a butterfly), the chances become even that one out of the twenty varies in the right direction. But nineteen out of the twenty, on the average, are soon killed off by the various causes that keep down the population of the species, and the chances are very much in favour of that one surviving which, by the hypothesis, has varied in the right direction. It is not pretended that this one would survive always, or even on the average, but in a large number of cases it would certainly do so; and taking Mr. Bennett's own estimate of a million individuals as the population of a rare species, we may fairly estimate that in a quarter, or say even in a tenth part of these, the surviving offspring would possess the favourable varia-

tion. But now a new factor enters into the problem, of which Mr. Bennett takes no account. Those that have already varied tend to leave offspring varying in the same direction as themselves; and as these will all have advantage, the offspring of the one-tenth will increase at the expense of those of the nine-tenths; and this tendency being still more powerful in the third generation, with the additional advantage as the numbers increase of the chance of both parents being favourable varieties, we may fairly expect the favourable to have completely exterminated the unfavourable variations, and to have firmly established themselves as a well-marked race. The enormous possible rapidity of multiplication, enabling a pair of individuals to produce millions in a few generations; the survival of the fittest, giving to favourable variations – not their bare numerical chance, as Mr. Bennett supposes, but – a certainly in the long run of living at the expense of the rest; and the powerful influence of heredity, which actually increases the *tendency* to produce the favourable variations with each succeeding generation, – are three of the main foundation-stones of the theory of natural selection, yet all three are ignored in this attempted mathematical demonstration of its insufficiency.

There is one other point in the theory of the origin of "mimicry" that deserves notice. It is, that the modifications leading to it are much more easy to explain than those leading to new genera and families, because the changes effected are wholly superficial and are almost entirely confined to colour. Now colour is both more variable than any other character, and is less intimately correlated with structure, so that great changes of colour may rapidly occur without in any other way affecting the individual, as we see in almost all our domestic animals. Experiments in breeding show that very large spontaneous variations of colour are frequent in insects; and thus the number of steps to produce a required amount of change may be much fewer than in cases of structural modification, in which every other part of the organism has to be coordinated to work harmoniously with the modified organ.

I may here take the opportunity of denying that I have argued, as Mr. Bennett says I have, that "an infinitesimal and inappreciable distinction may make the difference of a slightly longer span of life being allowed to the butterfly to lay its eggs in safety;" and I cannot imagine how he could have imputed to me anything so absurd. What I have maintained is, that for natural selection to act, either in producing "mimicry," or structural changes, no large or special variations are required, because the usual amount of *variability* which occurs in every part of every organism is sufficient. ("Contributions," pp. 287–291.) But so far from supporting this to be "infinitesimal" or "inappreciable," I show that it is so palpable and so readily appreciated by horticulturists and breeders as to have enabled them to produce all the wonderful variety in our domestic animals and cultivated plants. And every entomologist knows that similar variability exists in insects, and that the constantly occurring variations of colour are especially great.

Mr. Bennett next returns to the laws of variation, and, because Mr. Darwin says that we are profoundly ignorant of these (although he himself has done so much to elucidate them), maintains that we cannot really know anything of the origin of species. As well might it be said that, because we are ignorant of the laws by which metals are produced and trees developed, we cannot know anything of the origin of steamships and railways. Spontaneous "variations" are but the materials out of which "species" are formed, and we do not require to know how the former are produced in order to learn the origin of the latter. But though we may not know the laws which determine each variation in detail, the

general causes which lead to variation are not difficult to perceive. We do not know all the laws and causes that have given their peculiar form to each mountain or each valley, but we know a good deal of the general causes which have produced them, and we can perceive that the reason no two are exactly alike is, the number and complexity of the causes and the endless variety of conditions under which these causes have acted. In the far more complex operations of the development and growth of organisms, affected as we know they are by almost infinitely numerous and ever varying external and internal causes, it would be a much greater mystery if there were no variations, and if absolutely identical forms were produced by constant diversity of conditions. Even the successive offspring of the same parents are developed under very different conditions. At each succeeding year, and at every different period of each year, the parents have changed in age, in size, in vigour, health, and constitution; they may be living in a different locality, have different food, and be subjected to every different physical and mental influences. Add to this the effect of cross unions of distinct individuals, each with its own characteristic peculiarities, which are in varying degrees transmitted to the offspring; and further, that these modified offspring are submitted to a somewhat different set of conditions from the parents, and intercross perhaps with a distinct set of individuals; and add the effects of atavism in bringing up long ancestral characters, and it can hardly be said that the almost universal fact of "spontaneous variation" is quite unaccounted for. But as I have already remarked, this variability could never by itself produce *species*, but must absolutely prevent their production without the eliminating, accumulating, and fixing powers of selection, multiplication, and heredity.

In Mr. Bennett's concluding passages he advances a theory of his own on the subject of "mimicry," to the effect that it is connected with intelligence or instinct, "and runs almost *pari passu* with the development of the nervous system." In support of this view he asserts that it is "strongly developed in birds." This is erroneous. In birds it is very rare, only two or three cases being known, and these not nearly so remarkable as hundreds that occur in insects; and in mammalia, with the exception of one doubtful case, it is absolutely unknown. This view, therefore, is directly opposed by the facts.

I have only one more point to notice, a charge of inconsistency against myself. Mr. Bennett quotes me to the effect that man's chief peculiarities of form and structure were developed before his intellect had raised him above the condition of the brutes, and also *imputes* to me the belief that certain peculiarities in his structure (the absence of hair on his body, for example) "must have been in some way connected with *his* reasoning powers." But this is Dr. Laycock's view, which I have expressly repudiated, and I have never used a word to show that I believed that man has modified his own structure in any important degree, by the conscious or unconscious exercise of his reasoning powers. I have, it is true, declared my belief that "some intelligence" has acted on him, but I have also, I think, made it quite clear that I did not believe it to be his own intelligence. The inconsistency, therefore, is of Mr. Bennett's making.

I think I have now noticed the chief points in this last assault on the theory of Natural Selection, which has failed, like all preceding ones. Its author also exhibits the usual inability to keep steadily before him the great fundamental principles of the theory he is discussing, so that his arguments continually break down owing to his taking a partial and wholly inadequate view of its mode of operation. In the case of "mimicry" he is not sufficiently careful in his statement of the facts, and this, combined with his imperfect grasp of

the theory, entirely neutralises the elaborate numerical proofs which at first sight appear so overwhelming.

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The Difficulties of Natural Selection (S176)

As Mr. Bennett complains that I have charged him with errors he has not committed (which I should much regret to have done), I must ask permission to justify my statements by a reference to his own words.

1. Mr. Bennett says that he is unable to discover where he has led his readers to understand that there is only one completely mimicking species of *Leptalis*. I will therefore show him where he has done so. In the third column of his article (p. 31) he says: "Another South American genus of Lepidoptera, the *Leptalis*, belongs structurally to an entirely different class, the *Pieridæ*, and the majority of its species differ correspondingly from the *Heliconidæ* in their size, shape, colour, and manner of flying, being nearly pure white. There is, however, *one particular species* of *Leptalis*, which departs widely in external facies from *all its allies*, and so closely resembles a species of *Ithomia* as to deceive," &c. &c. Then comes the argument and the mathematical calculations always referring to "the *Leptalis*," and it is at the end of this, at the bottom of the next column, that we have the following passage (of which Mr. Bennett in his reply has only quoted a line and a half): "For supposing the chance is reduced from one in ten million to one in ten thousand, and it is said that the world has existed quite long enough to give a fair chance of this having occurred once, it is not a solitary instance that we have. Mr. Bates states that in a comparatively small area several distinct instances of such perfect mimicry occur, Mr. Wallace has a store in the Malay Archipelago, Mr. Trimen records several of wonderful completeness in South Africa," &c. Now, as there is not a word here about other species of *Leptalis*, but only about other cases of mimicry, as *Leptalis* is unknown in Africa or the East, as mimicry occurs in other genera and families of Lepidoptera, and other orders of insects, and as Mr. Bennett has himself stated, that the "*one particular species of Leptalis* departs widely in external facies from *all* its allies," I think it will be admitted that I was justified in asserting that Mr. Bennett's readers would be "led to understand," that there was only one species of completely mimicking *Leptalis*. If I was not so justified I confess my ignorance of the English language, and beg Mr. Bennett's pardon.

2. I leave your readers to judge for themselves whether the fact of a *Leptalis* having twenty offspring does or does not affect the mathematical argument as set forth by Mr. Bennett; but when, in answer to my statement, that the right variation has, by the hypothesis, a greater chance of surviving than the rest, he asks: "By what hypothesis? The hypothesis that these small variations are useful to the individual, the very hypothesis against which I am contending as unproved," – I must protest against his denying his own words. For, at p. 31, col. 1, he says: "The next step in *my argument* is, that the smallest change in the direction of the *Ithomia* which we can conceive, on any hypothesis, to be beneficial to the *Leptalis* is, at the very lowest, one-fiftieth of the change required to produce perfect resemblance;" and six lines farther on, "For the sake of argument, however, *I will suppose* that a change to the extent of one-fiftieth is *beneficial*," and then comes the calculation. Again, I must acknowledge my ignorance of the meaning of words if Mr. Bennett does not here directly contradict himself. I never said the hypothesis was proved, but only that

Mr. Bennett's argument, founded on it, was unsound, and for the sake of the argument he had admitted the hypothesis.

Mr. Bennett goes on to say: "The new factor, of which I take no account, is, again, entirely dependent on the admission of the natural selectionist premiss." This new factor is the principle of *heredity*. As he acknowledges that he takes no account of it, we must presume that he denies its existence; and as the whole of Mr. Darwin's theories and my own fall to the ground without it, he might have spared himself the trouble of his "mathematical demonstration."

3. I do not consider, as Mr. Bennett seems to do, that the distinction between "protective resemblance" and "mimicry" is a subtle one. Anyone who reads his paragraph on this subject (p. 32, col. 2) will, I think, be under the impression, as I was, that he alluded to mimicry, or mimetism, properly so called, as being strongly developed in birds. It seems, however, that he means only protective resemblance; but this, I believe, to be equally common among the very lowest forms of life. Transparency, for example, is a great protection to aquatic animals, and it is very prevalent in low organisms. Fishes are all, or almost all, protectively coloured, by the back being dark and the belly light, so that, whether looked at from above on the dark background, or from below on the light one, they are equally difficult to see. In many fishes, too, we have a specific protective resemblance as perfect as in any birds (see "Contributions to the Theory of Natural Selection," p. 55), and this is as much opposed to Mr. Bennett's theory as the absence of true mimicry in birds and mammals.

4. Mr. Bennett says, I have "brought no evidence to show that *extremely small variations* afford any immunity from the attacks of enemies," – but this was quite unnecessary, because I show that the variations which continually occur in insects are by no means "extremely small." He also says that I "give no explanation of the tendency of the *Leptalis*, referred to by Mr. Bates, to produce naturally varieties of a nature to resemble *Ithomiæ*." But Mr. Bates introduces this remark with – "It would seem as if;" and though I think that the fact may be so, and that it is not difficult to explain, yet I do not feel bound to explain every supposed fact as if it were a well-established one. As to the "parallelism of the development of protective resemblance and of instinct in the animal world," which I am also asked to explain, I deny that it has been proved to exist.

In conclusion, I will observe that the theory of Natural Selection, and its subordinate theory, Mimicry – have now been so fully developed by Mr. Darwin, Mr. Bates, Mr. Trimen, and myself, that I conceive it to be a full and sufficient answer to any opponent if we can show that his particular objections are unsound. This, I believe, I have done in the case of Mr. Bennett, although I am sorry to find that he cannot see it, and it is therefore unnecessary to go fully into the collateral points on which he has touched, and which have already been sufficiently explained by Mr. Darwin or myself.

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The Difficulties of Natural Selection (S177)

I find, on looking again at Mr. Bennett's article, that I have misrepresented him on one point, for which I beg to apologise. On his supposition, that the first twenty possible steps on the road to mimicry are absolutely useless, his argument will have some weight. This supposition, however, is entirely unsupported by facts. Very large variations of col-

our are exceedingly common in butterflies; and when such variations are in the right direction, they must in some cases be useful. I believe myself that far less than fifty, or even twenty, steps of variation would in some cases produce very good mimicry. – Alfred R. Wallace.

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Mimicry versus Hybridity (S179)

A letter printed in the 29 December 1870 number of Nature. Wallace was a founder of the title, and over a period of more than forty years contributed some one hundred fifty letters, articles, book reviews, and other notices to its pages.

I am rather surprised that Mr. Andrew Murray should have advanced his theory of mimicry being due to hybridisation, without adducing one solitary fact to prove that hybridisation between distinct *families* of insects ever occurs, or that, if it do occur, the offspring are fertile *inter se*. Mimicry is most frequent between very distinct *families* or higher groups, and often between different *orders* of insects. We may fairly consider that the "natural orders" of plants, as being the next well-marked groups above genera, are about equivalent to the *families* of insects, so that the analogy furnished by hybridisation among plants, on which alone Mr. Murray's theory is founded, wholly breaks down, unless he can show (which he has not done) that such hybridisation occurs between species of different "natural orders," or of well-marked groups higher than genera. It would be mere waste of time to discuss the details of a theory whose fundamental assumption is not only quite unsupported by fact, but is diametrically opposed to the almost, if not quite, universal fact that hybrids do not occur between species of different families or higher groups.

Mr. Scudder's[a] letter contains some interesting and suggestive facts, and opens up a new field of investigation as to the immunity of certain species, in their egg or larva state, from the attacks of hymenopterous and dipterous parasites. It is, I believe, now stated for the first time, that the peculiar secretions which render the Danaidæ distasteful to birds not only extend to their larva and egg state, but act as a safeguard from the attacks of parasites. The objection that it would have been more advantageous for the larva than for the imago of the *Limenitis misippus* to mimic the *Danais archippus*, appears to me to have no weight. We do not know, for instance, if such mimicry would be any defence against parasites who may be guided by smell rather than sight; and from the frequent limitation of certain odours and secretions to whole genera or families, the variations necessary to produce them may be of rare occurrence.

The fact that *Limenitis misippus* and *L. ursula* are about equally plentiful is not at all remarkable, since there are species of all degrees of rarity in every extensive group; but in this case it happens that both insects are mimickers, *Limenitis ursula* resembling the common N. American *Papilio philenor*, especially on the under side, which is exposed when the insects are at rest. This case of mimicry is not so perfect or so striking as the other, but that it is one is pretty certain, and there are several other instances in various

[a] Samuel H. Scudder (1837–1911), American entomologist.

parts of the world in which *Papilios* of certain groups are the objects of mimicry. Although Mr. Scudder has never seen a bird capture a butterfly, others have been more fortunate, and that they are thus captured very largely in the tropics is certain. It is not improbable, from the rarity of mimicry in the temperate zone, that the few cases which exist may have been produced under the more favourable climatal and organic conditions of the semi-tropical epochs anterior to the glacial period.

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In a pair of letters printed in the 6 July 1871 and 20 July 1871 issues of Nature, Wallace complained rather sharply about Mr. Henry Howorth's objections to some of the terminology of Darwinism.

A New View of Darwinism (S197)

The very ingenious manner in which Mr. Howorth first misrepresents Darwinism, and then uses an argument which is not even founded on his own misrepresentation, but on a quite distinct fallacy, may puzzle some of your readers. I therefore ask space for a few lines of criticism.

Mr. Howorth first "takes it" that the struggle for existence "means, in five words, the persistence of the stronger." This is a pure misrepresentation. Darwin says nothing of the kind. "Strength" is only one out of the many and varied powers and faculties that lead to success in the battle for life. Minute size, obscure colours, swiftness, armour, cunning, prolificness, nauseousness, or bad odour, have any one of them as much right to be put forward as the cause of "persistence." The error is so gross that it seems wonderful that any reader of Darwin could have made it, or, having made it, could put it forward deliberately as a fair foundation for a criticism. He says, moreover, that the theory of Natural Selection "has been expressively epitomised" as "the persistence of the stronger," "the survival of the stronger." By whom? I should like to know. I never saw the terms so applied in print by any Darwinian. The most curious and even ludicrous thing, however, is that, having thus laid down his premisses, Mr. Howorth makes no more use of them, but runs off to something quite different, namely, that *fatness* is prejudicial to fertility. "Fat hens won't lay," "overgrown melons have few seeds," "overfed men have small families," – these are the *facts* by which he seeks to prove that the *strongest* will not survive and leave offspring! But what does nature tell us? That the strongest and most vigorous plants *do* produce the most flowers and seed, not the weak and sickly. That the strongest and most healthy and best fed wild animals *do* propagate more rapidly than the starved and sickly. That the strong and thoroughly well-fed backwoodsmen of America increase more rapidly than any half-starved race of Indians upon earth. No *fact*, therefore, has been adduced to show that even "the persistence of the stronger" is not true; although, if this had been done, it would not touch Natural Selection, which is the "survival of the fittest."

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Mr. Howorth on Darwinism (S198)

Mr. Howorth sneers at "Survival of the Fittest" as an "identical expression" which "might have suggested itself even to a child," an axiom, in short, of which the truth cannot be disputed. This is satisfactory; but it is strange that he did not apply this axiom to his own theory, and see how they agreed together. He would probably admit, as another discovery "that might have suggested itself to a child," that *as a rule* the entire offspring of each animal or plant, except the one or two necessary to replace the parents, die before they produce offspring (this has never been denied since I put it prominently forward thirteen years ago). He would further admit, I have little doubt, that a great majority of animals and plants produce during their lifetime from ten to a thousand offspring, so that fifty will be a low average, but the exact number is of no importance. Forty-nine, therefore, of every fifty individuals born, die before reaching maturity; the fiftieth survives because it is "best fitted to survive," because it has conquered in the struggle for existence. Will Mr. Howorth also admit as self-evident, that this one survivor in fifty is healthy, vigorous, and well nourished, not sickly, weak, or half-starved? If he maintains that it is the latter, I shall ask him to prove it; if the former, then what becomes of his theory as an argument against Natural Selection? For, admitting as a possibility that his theory of the greater fecundity of the weak, &c., is true, how are these weak or sickly parents to provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves? The one in fifty who survives to leave descendants will inevitably be the strong and healthy offspring of strong and healthy parents; the forty-nine who die will comprise the weaker and less healthy offspring of weak and sickly parents; so that, as Mr. Darwin and myself have long ago shown, the number of offspring produced is, *in most cases*, the least important of the factors in determining the continuance of a species.

I have thought it better to go thus into the heart of the question, rather than defend myself from the charge of dogmatism, for stating as a fact that the most vigorous plants and animals are the most fertile. I repeat the statement, however, referring to Mr. Darwin's observations, and especially to those in which he *demonstrates by experiment* that cross-breeding produces the most vigorous and luxuriant plants, which again produce by far the largest quantity of seed. The facts that wild animals and plants are, as a rule, healthy and vigorous, that the head of the herd is the strongest bull, and that weak and sickly carnivora are rarely found because they must inevitably starve to death, sufficiently refute Mr. Howorth's theory as against Natural Selection. If he can point to any district upon the earth where the animals and plants are in a state of chronic debility, disease, and starvation, I may admit that there his theory holds good; but such a district has not yet come under my observation, or, as far as I am aware of, been recorded by any traveller.

I still maintain (Prof. Jowett's authority notwithstanding) that the phrase "Persistence of the Stronger" does not truly represent "Natural Selection" or the "struggle for existence;" and, though it may often be true, is not the whole truth. The arguments of Mr. Howorth from the history of savages will, I think, not have much weight, if we may take as an example his putting together as cause and effect the extinction of the Hottentots and their now obtaining enough to eat.

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Instinct (S216)

Instinct was undoubtedly a "hot button" subject for Wallace: he was definitely on the side of the fence favoring demonstrated proofs of the phenomenon, as opposed to a priori assumptions. This letter appeared, somewhat surprisingly, in the 15 September 1872 issue of The Spiritualist (London).

Sir, – I am surprised that so accurate a man as my friend Mr. H. G. Atkinson should have so misunderstood my meaning as to say that I "deny to the lower animals their instincts," and "strive to make facts square with my theories," – and further that my "denial of the existence of such powers is as futile as it is in utter defiance of fact." Now will it be believed that I have never denied the existence of instinct; I have, it is true, denied that it has been proved that birds build their nests by instinct, and have maintained that a consideration of all the facts is, in this case, entirely opposed to that view; and I suppose Mr. Atkinson himself would admit that each case of supposed instinct is to be judged by itself, according to the facts of that case. I have also expressed my belief that much of the supposed instinct of the lower animals can be explained by initiation and observation, and the peculiar organisation which necessitates certain movements, and renders certain actions pleasurable. In my short essay on "Instinct in Men and Animals," published in my "Contributions to the Theory of Natural Selection," I maintain that the senses and mental powers of the lower animals are probably so different from ours, and are so little known, that we cannot safely arrive at conclusions drawn from a comparison of their actions with ours; but that among the higher animals, where there is a closer resemblance in senses and mental powers to ourselves, the facts which I throughout appeal to, do not prove instinct. I maintain that experiments on instinct have not been sufficiently carried on, and I conclude, not that there is no such thing as instinct, but that it should not be accepted as proved in any particular case "until all other possible modes of explanation have been exhausted." Having thus expressed myself, I do not like being accused of the positive and dogmatic denials of instinct, which Mr. Atkinson imputes to me. I am open to conviction by facts, and I may remark that at the British Association meeting at Brighton, a valuable paper was read by Mr. Spalding[a] relating a series of experiments on newly-hatched chickens, which go to show that many simple actions, involving appreciation of form and distance, are well performed without experience; but we have as yet no experiments to show that the exceedingly complex actions involved in the higher instincts can be so performed.

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letters on instinct

In 1873 a long discussion unfolded in the pages of Nature on the relation of perception to instinct in the non-human animal world. Wallace's contributions appeared in the 20 February, 22 May, and 14 August issues, respectively.

[a] Douglas Spalding (1841–1877), English biologist, discoverer of behavioral imprinting.

Inherited Feeling (S222)

The remarkable case of an inherited feeling of dislike for a special class of persons, communicated by Mr. Darwin, appears to me to support a view I have long held (but not yet published) as to the explanation of another class of so-called instincts. The three separate instances given in which the dogs showed a violent antipathy to butchers, either without seeing them or when they were dressed as gentlemen, clearly indicates that it was through the sense of smell that the painful sensation was experienced; and this is quite in accordance with the wonderful delicacy and importance of this sense in most animals, and especially in dogs. It is natural to suppose that some ancestor of these dogs was systematically and cruelly ill-treated by several butchers, perhaps from some thievish propensity or other bad habit which required frequent punishment, so that the smell of a butcher came to be invariably associated with pain and a desire for revenge. But the most important fact to observe is, that there must be some peculiar odour developed in human beings by constant contact with flesh, which a dog can recognise apart from individual peculiarities and in spite of perfect disguise. Now the power many animals possess to find their way back over a road they have travelled blindfolded (shut up in a basket inside a coach for example) has generally been considered to be an undoubted case of true instinct. But it seems to me that an animal so circumstanced will have its attention necessarily active, owing to its desire to get out of its confinement, and that by means of its most acute and only available sense it will take note of the successive odours of the way, which will leave on its mind a series of images as distinct and prominent as those we should receive by the sense of sight. The recurrence of these odours in their proper inverse order – every house, ditch, field, and village having its own well-marked individuality – would make it an easy matter for the animal in question to follow the identical route back, however many turnings and cross-roads it may have followed. This explanation appears to me to cover almost all the well-authenticated cases of this kind.

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Perception and Instinct in the Lower Animals (S227)

The suggestion made by me in your issue of February 20, that animals which had been deprived of the use of their eyes during a journey might retrace their way by means of smell, had the effect of letting loose a flood of illustration, fact, and argument bearing more or less directly on the question; and as the stream now seems to have run nearly dry, I ask permission briefly to review the evidence adduced, so far as it affects the particular issue I brought forward. Several of the writers argue as if I had maintained that in all cases dogs, &c., find their way, wholly or mainly, by smell; whereas I strictly limited it to the case in which their other senses could not be used. The cases of this kind adduced by your correspondents are but few. The first, and perhaps the most curious, is that of Mr. Darwin's horse; but, unfortunately, the whole of the facts are not known. As Mr. Darwin himself pointed out, the horse may have lived in the Isle of Wight, and been accustomed to go home along that very road. I would suggest also that the country might resemble some tract in the neighbourhood of his own home; or that the horse, having been brought from home by a route and to a distance of which it had no means of judging, thought its master was riding home on the occasion in question, and therefore objected to turning

back. Anyhow, the case is too imperfect to be of much value as evidence in so difficult a matter. "J. T." (March 26) quotes the case of the hound sent "from Newbridge, county Dublin, to Moynalty, county Meath," thence long afterwards to Dublin, where it broke loose, and the same morning made its way back to its old kennel at Newbridge. I can find no "Newbridge, county Dublin," although there is a Newbridge, county Kildare, which is 26 miles from Dublin, on a pretty direct high road. That the dog never attempted to return during its "long stay" at Moynalty seems to show that some special facilities existed for the return from Newbridge. What they may have been we cannot guess at in the total absence of information as to the antecedents of the dog, the route by which he returned, and the manner in which he conducted himself on first escaping in Dublin.

The next case, of the two dogs returning from Liverpool to near Derby, is vague, and also without necessary details. It happened 50 years ago, and the only evidence offered as to the mode of the dogs' return is that "*it is said* they were seen swimming the Mersey." "N. Y.'s" case (April 24) of the dog who "did not make haste back," and therefore could not have returned by smell, is also most inconclusive. The distance was only 20 miles, and we know nothing of the route the dog followed, or the time it took. How do we know the dog did not wait the three weeks till it saw someone it knew living at or near its former house, and followed that person? This appears to me to be an exceedingly probable way of accounting for many of these returns where the distance is not very great. This brings me to the case of Mr. Geo. R. Jebb, who seems to have gone to the trouble of making an experiment which, with a little more trouble, might have been very complete and satisfactory. The dog was taken by rail very circuitously from Chester to a place 10 miles from Chester. It "hung about the station for about an hour and a half," and in three hours more arrived at its home. But we are still left totally in the dark, both as to the route it took or the process by which it decided on that route. What is required in such experiments is, that a person not known to the dog should be ready to watch and follow it (on horseback), noting carefully on the spot its every action. We should then perhaps know why it "hung about the station" an hour and a half before commencing its journey home, and afterwards, whether it showed any hesitation as to its route, and whether it followed the road or went straight across country. A few experiments carefully made in this way, at distances varying from 10 to 30 miles, and with a thorough knowledge in each case of the animal's antecedents, would, I venture to say, throw more light on this interesting question than all the facts that have been yet recorded. The only experiment of this kind I have met with is in the work of Houzeau ("Etudes sir les Facultés Mentales des Animaux"), and it is so curious that I give the passage literally. He says (vol. i. p. 156): "I have succeeded in making young dogs of five or six months lose themselves on first going out with me. They would begin by seeking for my trace by smell; but not succeeding in this, they would decide to return home. If there was a path, they followed the route by which they had come. If it was an untrodden virgin country, they shortened the circuits they had made in coming, but did not altogether depart from them. One would say that memory furnished a certain number of points which divided the route, and they went towards these by memory of directions. Thus inscribing chords to the curve by which they had come, they returned to the house." M. Houzeau's general conclusion from a considerable body of observations made with this point in view is, that animals find their way by exactly the same means as man does under similar circumstances, that is, by the use of all their faculties in observation of locality, but especially by a memory of directions and by a ready recognition of places once visited, which serve as guide-posts when they are again met

with. This seems to me a very sound theory, and quite in accordance with all that is known of the manner in which savages find their way.

The more general objections to my little theory which are made in your leading article appear to depend on the denial, to such animals as dogs and horses, of that amount of common sense and reasoning power which I believe them to possess, and also to the assumption that in the case supposed they would recollect merely the odours, not the objects the presence of which these odours had indicated. I imagine that animals know, just as well as we do, that some sights, sounds, and smells are caused by permanent, others by evanescent or changeable causes. The smell or sound of a flock of sheep would indicate to a dog the presence of an actual flock of sheep, just as surely as the sight of them would do, and he would no more lose his way because those sheep were not in the same place the next day or the next week, than he would had he travelled the road on foot with his eyes open. The smell of a wood, of a farmyard, of a ditch, a village, or a blacksmith's shop, with the more or less characteristic sounds accompanying these, would tell the dog that corresponding objects were there just as surely as the sight of them would do. On his return he would recognise the objects, not the smells and sounds only, and he would be no more puzzled by the absence of certain moveable objects he had recognised by smell than he would be had he seen them. I quite believe that mistakes would often be made owing to the discontinuousness of sufficiently characteristic odours; but the process of "trial and error," suggested by F. R. S., would be constantly used, and this is in accordance with the length of time usually taken in these journeys, often very much longer than would be required for a return by the shortest route and at moderate speed.

A friend has communicated to me a most remarkable fact, of a different character from any which have been referred to during the course of this discussion; and as I have it at first hand and took the exact particulars down as narrated to me, I think it will be of value. Many years ago, my friend lost a favourite little dog. He was then living in Long Acre. Three months after, he removed to a house in another street about half a mile off, a place he had not contemplated going to or even seen before the loss of the dog. Two months after this (five months after the dog was lost) a scratching was one day heard at the door, and on opening it the lost dog rushed in, having found out its master in the new house. My friend was so astonished that he went next day to Long Acre to an acquaintance who lived nearly opposite the old house (then empty) and told him his little dog had come back. "Oh," said this person, "I saw the dog myself yesterday. He scratched at your door, barked a good deal, then went to the middle of the street, turned round several times, and started off towards where you now live." My friend cannot tell, unfortunately, what time elapsed between the dog's leaving the old and arriving at the new house. If every movement of this dog could have been watched from one door to the other, much might have been learnt. Could it have obtained information from other dogs (and that dogs can communicate information is well shown by Mr. A. P. Smith's anecdote in your issue of three weeks back)? Could the odour of persons and furniture linger two months in the streets? These are almost the only conceivable sources of information, for the most thorough-going advocates for a "sense of direction" will hardly maintain that it could enable a dog to go straight to its master, wherever he might happen to be.

Not to trespass further on your space, I would venture to hope that some persons, having means and leisure, would experiment on this subject in the same careful and thorough way that Mr. Spalding experimented on his fowls. The animals' previous history must be known and recorded; a sufficient number of experiments, at various distances and under

different conditions, must be made, and a person of intelligence and activity must keep the animal in sight, and note down its every action till it arrives home. If this is done I feel sure that a satisfactory theory will soon be arrived at, and much, if not all the mystery that now attaches to this class of facts be removed.

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Perception and Instinct in the Lower Animals (S228)

In answer to Mr. George J. Romanes (*Nature*, August 7) I beg to say that I particularly inquired of my friend whether he had been to or near his old house on the day the dog returned, or shortly before, and he assured me that "he had never been near it since he left." I ought to have stated this in my account of the circumstance.

I shall make no further remarks on the subject, because I believe that nothing satisfactory can be arrived at till experiments of the nature indicated in my last letter have been systematically carried out. – Alfred R. Wallace.

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Wallace sent a pair of letters on the mechanics of bird flight to Nature in 1874; they appeared in the 19 February and 26 March issues.

Animal Locomotion (S240)

While admitting that Dr. [James Bell] Pettigrew appears to have made mistakes in his figures, and that he has not explained his views in the clearest manner, nevertheless it appears to me that, on the very important question of whether a bird's wing during onward flight moves downward and forward or downward and backward, he is right in asserting the former to be the fact.

The arguments of Mr. Garrod and Mr. Ward against this view seem to be founded on two assumptions – that the wing during its down-stroke is an inflexible plane, and that during its upward motion the quills open so perfectly that there is neither vertical nor horizontal resistance. But every feather of a wing is highly flexible towards its extremity, so that during the down-stroke the whole posterior margin of the wing must be curved up by the pressure of the air, thus forming a highly effective propelling surface owing to the rapid motion of this part of the wing. During the upward stroke the feathers open freely so as greatly to diminish, though not wholly to prevent, downward reaction; but the broad soft web of each quill will be bent down by the rapid escape of air between the quills, and this will necessarily give a forward motion, probably equal to that attained during the down-stroke, in which the small curved surface has a greater resistance and more rapid motion. If then the up- and the down-stroke both produce onward motion, the resultant of this motion will be in the direction of the mean position of the wings, which we may take to be about that of the body of the bird; but if the down-stroke were directed *backward* and the up-stroke *forward*, the resultant onward motion would be obliquely downward, and this downward angle of motion would tend to be so much increased by the continual gravitation of the body that the surplus vertical reaction of the down-stroke over the up-

stroke would not be able to overcome it. A slight upward angle of the mean position of the wing-plane seems therefore to be essential to secure horizontal forward motion as a general resultant of the upward and downward action of the wings under the influence of gravitation; and to Dr. Pettigrew belongs the merit of showing that this is one of the most important characteristics of the flight of birds, and, probably in a still greater degree, of that of insects. A bird's wing is a highly complex apparatus, subject to a variety of flexures and motions in every feather; and it is only by a careful consideration of the action of the resisting medium on these variously curved elastic surfaces, both during the upward and downward motion of the wings, that we can arrive at any definite notion of their supporting and propelling effect. The experiments of Prof. Marey do not seem to contradict the theory of Dr. Pettigrew, as far as I can make out from an abstract of these given in the "Ibis" for 1870, p. 267; though, as his apparatus only gave the motion of the wing relatively to the body of the bird, they are not of very much value in determining the absolute angular position of the wings, which is what we want to arrive at. The highly-inclined position of a hovering bird is more to the point, as any less degree of inclination would lead to onward motion.

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Animal Locomotion (S241)

My former letter on this subject was merely to show that, mechanically, Dr. Pettigrew's view of the forward motion or inclination of a bird's wing during the down stroke was less absurd than had been supposed, and even seemed necessary to flight. I did not profess to have made accurate observation or experiment on the point. I accept, therefore, the observation of the Duke of Argyll[a] as to the vertical motion of the heron's wing; but as he expressly refers to its great concavity, that would give a vertical down stroke the effect of a somewhat forward stroke of a flatter wing. The proper inference would therefore seem to be, that in birds with less concave wings the stroke is slightly directed forwards. As to the last two paragraphs of his Grace's letter, he will see, if he refers again to mine, that he has quoted words I never used. I impute to Dr. Pettigrew the "merit of showing" that the "*slight upward angle* of the mean position of the wing plane is essential to secure horizontal forward motion as a general resultant," &c., and this is exactly what the Duke denies.

Mr. James Ward's elaborate analysis of the down stroke of a bird's wing simply shows (if correct) that in the position he ascribes to it (moving downward and backward) it would send the bird horizontally forward. Of course it would. But then what becomes of the bird during the up stroke in an opposite direction? The bird is then falling, and by the downward reaction of all the solid surface of the anterior margin of the wing, and of all the feathers, however, obliquely turned, it is driven farther downwards; and as this takes place between every two down strokes, and approximately during an equal space of time, how is a horizontal average motion to be produced unless the down stroke alone produces, not a horizontal, but a highly-inclined upward motion? Mr. Ward's whole argument appears to me to ignore the great downward reaction, added to gravitation, during every up stroke, which requires that the down stroke should not merely support the bird,

[a] The Duke of Argyll (1823–1900), Scottish statesman and writer.

but raise it up vertically just as much as during the up-stoke it has fallen vertically. The matter, however, is not to be settled by discussing theoretically, but by observation and experiment. I simply maintain that the results of Dr. Pettigrew's observations and experiments are not, as supposed, inconsistent with mechanical principles; and nothing in your correspondent's letter induces me to alter that opinion.

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Migration of Birds (S244)

This short (one-column) letter to the Editor, printed in the Nature issue of 8 October 1874, is, perhaps unexpectedly, one of Wallace's most reprinted works. In it he comments on the relation of natural selection to the evolution of bird migration patterns.

The subject to which Prof. Newton[a] has called attention is one of great interest to all naturalists, and requires to be studied systematically; for I can hardly think that the solution is so "simple in the extreme" as Mr. Newton thinks it may be.

It appears to me probable that here, as in so many other cases, "survival of the fittest" will be found to have had a powerful influence. Let us suppose that in any species of migratory bird, breeding can as a rule be only safely accomplished in a given area; and further, that during a great part of the rest of the year sufficient food cannot be obtained in that area. It will follow that those birds which do not leave the breeding area at the proper season will suffer, and ultimately become extinct; which will also be the fate of those which do not leave the feeding area at the proper time. Now, if we suppose that the two areas were (for some remote ancestor of the existing species) coincident, but by geological and climatic changes gradually diverged from each other, we can easily understand how the habit of incipient and partial migration at the proper seasons would at last become hereditary, and so fixed as to be what we term an instinct. It will probably be found, that every gradation still exists in various parts of the world, from a complete coincidence to a complete separation of the breeding and the subsistence areas; and when the natural history of a sufficient number of species in all parts of the world is thoroughly worked out, we may find every link between species which never leave a restricted area in which they breed and live the whole year round, to those other cases in which the two areas are absolutely separated. The actual causes that determine the exact time, year by year, at which certain species migrate, will of course be difficult to ascertain. I would suggest, however, that they will be found to depend on those climatal changes which most affect the particular species. The change of colour, or the fall, of certain leaves; the change to the pupa state of certain insects; prevalent winds or rains; or even the decreased temperature of the earth and water, may all have their influence. Ample materials must exist, in the case of European birds, for an instructive work on this subject. The two areas should be carefully determined for a number of migratory birds; the times of their movements should be compared with a variety of natural phenomena likely to influence them; the past changes of surface, of climate, and of vegetation should be taken account of; and there seems no rea-

[a] Alfred Newton, English ornithologist (1829–1907).

son to doubt that such a mode of research would throw much light on, if it did not completely solve, the problem.

This is an appropriate opportunity for making a suggestion which has long been in my mind. It is, that it would be a valuable and interesting addition to *Nature*, if we were supplied with a weekly (or monthly) "Calendar of Periodical Phenomena in Natural History," such as the average dates of appearance and departure of migratory birds, of the opening and fall of the leaf of our forest trees and common cultivated trees and shrubs; of the flowering of our common field and garden plants; and also the mean *highest* and *lowest* temperature of each *day*, the direction of the wind and amount of rainfall for each *week*, according to the Greenwich averages. None of this information is given in the usual almanacks or periodicals, and it is by no means easy to find it when wanted. Yet it is surely of much value to everyone who lives in the country, and would be the means of exciting an intelligent interest in such observations and inquiries as those to which Prof. Newton has called our attention in his interesting article.

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Automatism of Animals (S245)

A letter to the Editor, on the subject of reflex actions, printed in the 22 October 1874 issue of Nature.

Your correspondent, Mr. Wetterhan, has, I think, misunderstood Prof. Huxley's argument; which is, not that the adjusted motions he refers to never were the result of conscious and voluntary motion, but that they are not so now. His letter has, however, induced me to call attention to what has always seemed to me a real difficulty. As I understand automatic or reflex actions, they are those which have been so constantly repeated and which are so essential to the well-being of the individual, that the various nerves implicated have become so perfectly co-ordinated that the appropriate stimulus sets the whole machinery in motion without any conscious or voluntary action on the part of the individual. Thus we can quite understand how a paralysed limb would be drawn up when the sole of the foot is tickled or the toe pricked. If, however, any such irritation continues to be felt in the normal state, a man would stoop down and remove the irritating substance with his hand, or would place his foot upon the opposite knee, and, stooping down, endeavor to *see* the object which caused the irritation. But these are *conscious*, not *reflex*, acts. They are not repeated often enough, and are not sufficiently identical in form, to become automatic; and we are not told that a wholly paralysed human body does actually go through these various motions, as it certainly would do if not paralysed.

Now, in the case of the frog I can quite understand the jumping, swallowing, swimming, and even the balancing; for all these are actions so essential to the animal's existence, and so often repeated during life, as to have become automatic. So, also, I can understand the drawing up of the foot to remove an irritation on the side of the body, for with the short-necked frog this too is an essential, and must have been an oft-repeated action. But we are further told that "if you hold down the limb so that the frog cannot use it, he will, by and by, take the limb of the other side and turn it across the body, and use it for the same rubbing process." Now, this seems to me not to be explicable by automatic

or reflex action, because it cannot have been an action frequently if ever performed during the life of every frog. It is true that from the co-ordination of the movements of the opposite limbs, we might expect, if the irritation were continued, and the leg on the same side kept for some time in motion, that the other leg would begin to move *in the same way*. But what causes it to move in a quite different and unusual way, *across* the body to the opposite side; and this, as related, at once and without first trying its own side? The most usual motion of both legs is directly up and down, each on its own side. What is it that causes one of these legs, when it begins to move, not to move in the usual way (that which *is* automatic during life), but in an unusual manner, which must have been very rarely, if at all, used during life, and when used must have been purely conscious and voluntary? I think I cannot be mistaken in considering this to require some explanation. It may be that the frog is constantly, during life, crossing one foot over to rub the opposite side of the body; but we cannot accept this as an explanation unless it has been observed to be a fact. What puzzles me is, that Prof. Huxley, Dr. Carpenter, and Mr. Darwin, all refer to this case as an example of reflex action, and none of them see any difficulty in it, or seem to think that it requires any more explanation than the remaining quite intelligible cases. As others may, like myself, feel the difficulty I have endeavoured to point out, I hope some of your physiological correspondents will enlighten us if they can. – Alfred R. Wallace.

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Did Flowers Exist during the Carboniferous Epoch? (S309)

A letter to the Editor of Nature on a paleontological subject, printed in its issue of 24 April 1879.

I cannot accept Mr. McLachlan's[a] reference of the interesting *Breyeria borinensis* to the Ephemeridæ, even though he has "examined the fossil," and "has no doubt" about it. The photograph which I possess is so beautifully sharp that it brings out the minutest details, and a careful examination and comparison of it with specimens and drawings leads me to the conclusion, that in the general character of the wing-neuration it is strictly lepidopterous and of the Bombycine type, having the costal, subcostal, and median nervures, with their branches and bifurcations, arranged precisely as in that group, but differing in the much greater length of the wing and the increased number of the branches of the subcostal vein – seven instead of four. In some of the Chalcosiidæ, however, there are often six branches to this vein, but crowded together and sometimes anastomosing, owing to the much shorter apical portion of the wing. In this family also we often have an intermediate false vein, which is distinctly visible in the fossil. Until, therefore, I am referred to some group of insects with which it more nearly agrees, I must believe it to be an ancestral moth, even though, according to Prof. Haeckel and Mr. Scudder, moths ought not to have existed in the carboniferous epoch.

After a careful comparison of the photograph with specimens and figures of Ephemeridæ, I can see *no resemblance whatever* to the neuration of the family with which Mr. McLachlan so confidently associates it; while the "dense transverse reticulation" to

[a] Robert McLachlan (1837–1904), English entomologist.

which he refers seems to me to be merely due to crumpling of the membrane, and certainly bears no close resemblance to the strong reticulation of the veining of the Ephemeridæ, and it is, moreover, only visible at all at the base of the wing. The general form of the wing and arrangement of the veins are, however, so different, as, to me, to be conclusive against this view.

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Difficult Cases of Mimicry (S359)

Wallace had already performed a useful service by earlier introducing Müller's work on mimicry to a larger audience, but he was prompted to comment further in a communication printed in the Nature issue of 22 March 1883.

I have received from Mr. Thos. Blakiston, of Tokio, Japan, a communication to the *Japan Mail* by himself and Prof. Alexander, commenting on my article in *Nature*, vol. xxvi. p. 86, and pointing out some errors as to the estimated advantage derived by the mimicking butterflies. On referring to my article, I find that I have, by an oversight, misstated the mathematical solution of the problem as given by Dr. Fritz Müller and confirmed by Mr. [Raphael] Meldola, and have thus given rise to some confusion to persons who have not the original article in the *Proceedings of the Entomological Society* to refer to. Your readers will remember that the question at issue was the advantage gained by a distasteful, and therefore protected, species of butterfly, which resembled another distasteful species, owing to a certain number being annually destroyed by young insectivorous birds in gaining experience of their distastefulness. Dr. Müller says:

> If both species are equally common, then both will derive the same benefit from their resemblance – each will save half the number of victims which it has to furnish to the inexperience of its foes. But if one species is commoner than the other, then the benefit is unequally divided, and the *proportional advantage* for each of the two species which arises from their resemblance is *as the square* of their relative numbers.

This is undoubtedly correct, but in my article I stated it in other words, and incorrectly, thus:

> If two species, both equally distasteful, resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (*a*) is twice as numerous as another (*b*), then (*b*) will lose only one-fourth as many individuals as it would do if it were quite unlike (*a*); and if it is only one-tenth as numerous, then it will benefit in the proportion of 100 to 1.

This statement is shown by Messrs. Blakiston and Alexander to be untrue; but as some of your readers may not quite see how, if so, Dr. Müller's statement can be correct, it will be well to give some illustrative cases. Using small and easy figures, let us first suppose one species to be twice as numerous as the other, *a* having 2000 and *b* 1000 individuals, while the number required to be sacrificed to the birds is 30. Then, if *b* were unlike *a* it would lose 30 out of 1000, but when they become so like each other as to be mistaken, they would lose only 30 between them, *a* losing 20, and *b* 10. Thus *b* would be 20 better off than before, and *a* only 10 better off; but the 20 gained by *b* is a gain on 1000,

equal to a gain of 40 on 2000, or four times as much *in proportion* as the gain of *a*. In another case let us suppose *c* to consist of 10,000 individuals, *d* of 1000 only, and the number required to be sacrificed in order to teach the young birds to be 110 for each species. Then, when both became alike, they would lose 110 between them, *c* losing 100, *d* only 10. Thus *c* will gain only 10 on its total of 10,000, while *d* will gain 100 on its total of 1000, equal to 1000 on 10,000, or 100 times as much *proportional gain* as *c*. Thus, while the gain in actual numbers is inversely proportional to the numbers of the two species, the *proportional* gain of each is inversely as the *square* of the two numbers.

I am, however, not quite sure that this way of estimating the *proportionate* gain has any bearing on the problem. When the numbers are very unequal, the species having the smaller number of individuals will presumably be less flourishing, and perhaps on the road to extinction. By coming to be mistaken for a flourishing species it will gain an amount of advantage which may long preserve it as a species; but the advantage will be measured solely by the fraction of *its own numbers* saved from destruction, not by the proportion this saving bears to that of the other species. I am inclined to think, therefore, that the benefit derived by a species resembling another more numerous in individuals is really in inverse proportion to their respective numbers, and that the proportion of the squares adduced by Dr. Müller, although it undoubtedly exists, has no bearing on the difficulty to be explained.

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The Colours of Arctic Animals (S378)

Wallace was to the study of protective coloration as his friend Bates was to mimicry. In a letter to Nature printed in its issue of 16 April 1885 he discussed one related question.

I am sorry that I cannot agree with my friend Mr. Meldola as to the insufficiency of the explanation of the white coloration of Arctic mammals and birds as due to protective adaptation, since it appears to me that there is no important group of facts in natural history of which the explanation is more complete; while on the other hand I venture (though with some hesitation) to question the basis of his counter explanation, as I am not aware of any sufficient proof that colour, *per se*, affects the radiation of low grade heat. At all events I feel tolerably certain that this cause, if it exists, has had no perceptible influence in determining the white colours of Arctic animals.

I am not myself aware of there being "many species" possessing the white coloration as to which there is any difficulty in seeing the advantage they may derive from it, and there is certainly a large body of facts showing that *colour* is, in almost all animals and in every part of the world, more or less protective or adaptive. If the white coloration of Arctic animals stood alone, it might be thought necessary to supplement the protective theory by any available physical explanation, but we have to take account of the parallel cases of the sand-coloured desert animals and the green-coloured denizens of the ever-verdant tropical forests; and though in both these regions there are numerous exceptional cases, we can almost always see the reason of these, either in the absence of the need of protection or in the greater importance of conspicuous colouring. In the Arctic regions

these exceptions are particularly instructive because in almost every case the reason of them is obvious. Let me call attention to a few which now occur to me.

In the Arctic zone the wolf does not turn white like the fox, the reason evidently being that he hunts in packs, and concealment from his prey is not needed. So the musk-sheep and the yak, though both exposed to the extremest cold, are not white, because they are both swift and strong and need no concealment from their enemies. For the same reason neither the moose, the caribou, nor the reindeer are wholly white. Again, the glutton and the sable are dark-coloured, though inhabiting the coldest regions, and this is clearly because they are arboreal, and are better concealed from their prey by a dark than a light colour. If any useful protection from cold were to be obtained by a white coat, we should expect it to appear in such a case as the Esquimaux dogs, exposed for countless generations to the severest climate. But they gained the required warmth by a thickening of the woolly undercoat in winter, as do many other animals; and this suggests the general proposition that it will be always easier and safer to gain warmth in this way than by a modification of colour, which could certainly have but a very small effect, and might often interfere with adaptations of far greater importance. Exactly analogous cases occur among birds. The raven is, perhaps, the extremest Arctic species, but, feeding on carrion, it has no need of concealment in approaching its prey, and thus it keeps its jet black coat in the depths of the Polar winter.

The physical explanation of melanism in butterflies and some other insects, on the other hand, seems to me to be probably a sound one; but even that requires more evidence and a fuller knowledge of the habits of the species before we can admit it as proved. It may be that the dark colouring is protective, assimilating with the surroundings of the insect when at rest, and this can only be decided by observations specially directed to the point in question.

But even if, in this case, the dark colour has been produced in order to favour the absorption of the direct rays of the northern sun, it affords no support whatever to the totally different case in which the radiation of the obscure heat from an animal body has to be checked. I may, perhaps, be ignorant on the point, as it is rather out of my line, but I am not aware of any good experiments to determine the influence of colour *per se*, as distinct from the structure and surface-texture of coloured substances, on the radiation or absorption of heat of a low grade of temperature, and from a dark source. The only authority I have at hand (Ganot's "Physics," eighth edition) seems rather to imply that colour has no effect in such cases, for I find it stated, at p. 338, that the radiating power of *lampblack* and *whitelead* are identical, both being given as 100, while *Indian ink* is only 88. Again, at p. 352, the absorptive power of these two substances is given as 100, the source of heat being copper at 100° C., while that of Indian ink is given as 85. This seems to show that surface-texture or molecular structure is the important point, while colour has no effect whatever.

In order to determine experimentally whether white fur or feathers are inferior to black as radiators of animal heat, it would not do to employ stained or dyed materials, because the pigments employed might affect the texture of the surface, and produce an effect not at all due to the colour. A fair test would be afforded by two samples of cloth or flannel woven from white and black natural wool respectively, the wool to be obtained from the same breed of sheep, and, if possible, from the same district, while the material must be as nearly as possible identical in weight and texture. I shall be glad to learn from Mr. Meldola, or any other of your readers, whether any experiment of this kind has been

made, or whether there is any valid reason for believing that the radiation of animal heat is at all affected by colour alone.

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letters to *Nature* regarding George Romanes's theory of physiological selection

Wallace and the prominent biologist George Romanes (1848–1894) had a long squabble over the validity of the latter's theory of physiological selection. In addition to full-scale articles appearing in Fortnightly Review and Journal of the Linnean Society: Zoology, the discussion enlivened the pages of Nature four times over a five year period.

Physiological Selection and the Origin of Species (S390: 16 September 1886)

As Mr. Romanes has referred to my article in the current number of the *Fortnightly Review*, and stated that he is prepared to answer what he terms "the very obvious exceptions" which I have taken to his theory, I shall be glad to be allowed to state, very briefly, what those exceptions are, and to give an illustration of one of the more important of them.

(1) Mr. Romanes makes a great deal of the alleged "inutility of specific characters," and founds upon it his extraordinary statement that, during his whole life, Darwin was mistaken in supposing his theory to be "a theory of the origin of species," and that all Darwinians who have believed it to be so have blindly fallen into the same error. I allege, on the contrary, that there is no proof worthy of the name that specific characters are frequently useless, and I adduce a considerable series of facts tending to prove their general utility.

(2) In support of his view as to the swamping effects of intercrossing, Mr. Romanes objects to the assumption of Darwin, "that the same variation occurs simultaneously in a number of individuals," adding: "Of course, if this assumption were granted, there would be an end of the present difficulty"; and his whole argument on this branch of the question rests on the assumption being false. I adduce evidence – copious evidence – that the supposed assumption represents a fact, which is now one of the best-established facts of natural history.

(3) Mr. Romanes states, as the special feature of his physiological varieties, that "they cannot escape the preserving agency of physiological selection." He gives no particle of proof of this, while I show that, on the contrary, it is hardly possible for them to survive to a second or third generation. It is on this point that I wish to give an illustration. Mr. Romanes speaks of his supposed variations as "showing some degree of sterility with the parent form," while continuing to be fertile "within the limits of the varietal form"; but I hold that any such variety (beyond single individuals) can hardly exist, while he has adduced no proof whatever of their existence. To show the improbability of their existence, let us suppose a definite case.

In a given species there is born an individual, A, which is infertile with the bulk of the species, but fertile with some few individuals of the opposite sex, *a*, *b*, *c*. Let there be a

second individual, E, born from other parents in another part of the area occupied by the species, and fertile only with *e*, *f*, *g*. Other individuals, K, P, R, &c., may have similar relations, each infertile with the bulk of the species, fertile only with a few individuals which may be termed their physiological complements. Now each of these, separately, is a physiological variety, but the whole set, A, E, K, P, R, do not form one, but five distinct varieties. To form one variety all of them must be fertile with the same identical set of individuals of the opposite sex, and this seems to me to be so highly improbable that it must not be assumed till rigidly proved. Yet there is not one passage in Mr. Romanes' paper to show that he recognised this difficulty; on the contrary, he always speaks as if any number of separate physiological variations within one species must necessarily form one variety. It will easily be seen that the chances against any single variety of this nature being preserved are overwhelmingly great. For, first, at least two of the complementary individuals must survive to the breeding-season, and the chances against this are measured by the fertility of the species. If it produces ten young each year, the chances are between nine and ten to one against any one of them surviving. The chances against the *two* complements surviving will be about ninety to one; and then there remains the chances against the two meeting at the breeding-season, for, by the assumption, there is nothing whatever to bring them together but chance, and this may be any number of thousands to one.

There are, no doubt, other possible cases in which the physiological variety might be continued, but, as I have shown in my paper, the chances against it are always very great. Here, then, are three objections to Mr. Romanes' theory which seem to me to be weighty and fundamental; yet he says, in effect, that he anticipated, and is prepared to answer, them. This, I must say, puzzles me; because in the whole of his lengthy paper, occupying seventy-five pages, I cannot find any adequate recognition of their existence, or any attempt whatever to answer them.

My apology for writing this is that I am shortly leaving England, and wish the readers of *Nature,* who may not have seen the *Fortnightly*, to be aware of the character of the objections which Mr. Romanes declares that he anticipated, but apparently thought of too little importance to require any discussion in his paper.

* * *

Mr. Romanes on Physiological Selection (S395: 17 February 1887)

I have just seen Mr. Romanes's article in the *Nineteenth Century*, and his letter specially replying to myself in your issue of January 13 (p. 247). I do not propose to continue the discussion, but ask leave to make a few observations on some features of his reply in both the article and the letter.

On the question of the "inutility of specific characters," he appeals to authority against me, and especially to Darwin's very cautious remarks, which seem to me to support my view much more than they do those of Mr. Romanes; but in any case this is a matter in which I decline to accept authority as an infallible guide. The impossibility of proving a negative is proverbial, but my opponent declares that his negative – the uselessness of specific characters – wants no proving, but must be accepted till in every case the affirmative is proved. Here, again, is a canon of criticism the validity of which I wholly deny.

As to the swamping effects of intercrossing, there is again an appeal to authority, and

Mr. Romanes now explains away (in the *Nineteenth Century*) what he had said about "simultaneous variations," by asking me to show such variations as the occurrence of an incipient spur on a duck's foot or horn on the head of a racehorse, in the belief, apparently, that these are the class of characters which are distinctive of closely-allied species! Such a demand, seriously made, appears to me so preposterous as to render further discussion of the matter with such an adversary out of the question.

The argument to show that the supposed physiological variations would be perpetuated, seems to me as weak and unsatisfactory as ever. The question is really not worth further discussion till the required variations are proved to exist in the requisite abundance and possessing the peculiar relations to each other and to the rest of the species which would alone give them any chance of survival.

I now leave the question, as between myself and Mr. Romanes, to the consideration of those naturalists who may be able to bestow upon it the requisite time and attention.

* * *

Dr. Romanes on Physiological Selection (S428: 27 November 1890)

In his two latest articles dealing with this subject, Dr. Romanes has made certain statements as to my position in regard to it which call for a brief notice on my part.

In his original paper, and in the summary of it published in *Nature*, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection. A few quotations will show this. He says: "It becomes almost impossible to doubt that the primary specific distinction (meaning sterility) is, as a general rule, the primordial distinction" (*Nature*, vol. xxxiv. p. 339). Again, he enforces this as against Darwin's view that sterility was a consequence or concomitant of other differences, as follows: "My theory, on the other hand, inverts this order, and supposes the primary distinction to be likewise (in most cases) the primordial distinction" (*l.c.*, p. 363). This is very clear, but to show that he limited the term "physiological selection" to the results supposed to arise from this phenomenon, we have his reply to Mr. Galton, who urged a fact also dwelt upon by Darwin – the psychological disinclination to mate between many varieties – as an important factor in the differentiation of species: "Now I have fully recognized this principle as one amongst several others which is accessory to, although independent of, physiological selection" (*l.c.*, p. 407). A little further on he again states his fundamental fact thus: "If my theory is true, it must follow, as Mr. Galton says, that such unions would be more or less sterile, and, as this sterility is itself the only variation which my theory supposes to have arisen *in the first instance, ex hypothesi* we can have no means of observing whether or not the individuals which present this variation 'consort with outsiders,' or with those individuals which do not present it" (*l.c.*, p. 407). As if to leave no possible doubt as to the special point of his new theory, he again enforces it in the following passage: "And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in all other respects resemble its parent stock, and so be able to compete with it on equal terms" (*l.c.*, p. 408).

Now surely all this makes it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, was founded on the supposition of sterility-

variation alone, arising in an otherwise undifferentiated species; and he claimed that such variations "cannot escape the preserving agency of physiological selection," and that "physiological selection must be quite as vigilant as natural selection, and it seizes upon the comparatively unuseful variation of sterility with even more certainty than natural selection can seize upon any useful variations" (*l.c.*, p. 364).

These last statements, by the truth of which alone the use of the term "selection" can be justified, I showed by two carefully considered cases to be absolutely unfounded, and the exact opposite of what must really occur (*l.c.*, p. 467; and "Darwinism," p. 182). Having thus proved that "physiological *selection*," in the only form claimed by Dr. Romanes as original, does not exist, and that the only modes by which degrees of sterility between distinct species can arise are those discussed or suggested by Darwin himself, with the addition of the possible action of natural selection in increasing incipient sterility between slightly differentiated forms, will it be believed that I am accused of having appropriated the theory of physiological selection without acknowledgment! In the *Nineteenth Century* (May 1890, p. 831), Dr. Romanes says of me: "He presents an alternative theory to explain the same class of facts. Yet this theory is, purely and simply, without any modification whatsoever, a restatement of the first principles of physiological selection, as these were originally stated by myself." And now, in the October issue of an American magazine, *The Monist*, he has an article entitled "Mr. A. R. Wallace on Physiological Selection," in which the original main point, of sterility-variations alone leading to and constituting "physiological selection," is almost entirely ignored, and the various modes by which isolation is produced between incipient species or in which infertility arises in correlation with other divergent characters, are all claimed as forming part of the theory of physiological selection. He quotes from "Darwinism" my exposition of the effects of partial infertility arising between "two varieties in process of adaptation to somewhat different modes of life within the same area," to show "how unequivocal and complete is Mr. Wallace's adoption of our theory" *(The Monist*, No. I, p. II). "Our" refers to Mr. Gulick, who is taken into partnership by Dr. Romanes. And again he speaks of "the peculiar position to which he has eventually gravitated with reference to my views – professing hostility on the one hand, while reproducing them as original on the other" (*l.c.*, p. 19).

I have here confined myself to showing, by Dr. Romanes's own repeated and emphatic statements, what was the essential and original theory to which he gave the name of "physiological selection." The whole of this special doctrine I have argued against as unsound, because, on close examination, it proves to be quite inadequate to produce any such effects as are claimed for it. Whether I was right or wrong in doing so, I did, as a matter of fact, and do still, wholly reject this fundamental and essential part of the theory – the only part which had even a *primâ facie* claim to originality. I also totally reject the two subsidiary doctrines on which Dr. Romanes lays great stress as adjuncts of his theory – that of the inutility of a large proportion of specific characters, and that of the power of isolation alone "without the aid of natural selection" to produce new species; while, so far as I know, the only points in which I agree with him are those in which we both make use of Darwin's facts and adopt Darwin's explanation of them. Yet, notwithstanding this rejection of all that is special in his teachings, Dr. Romanes has the hardihood to assert that I claim them as my own; that I merely restate his theory "purely and simply, without any modification whatsoever"; and that my adoption of his theory "is unequivocal and complete."

I leave it to others to characterize these extraordinary statements in the terms that fitly apply to them.

* * *

Dr. Romanes on Physiological Selection (S429: 18 December 1890)

As Dr. Romanes now declares that the essence of his theory of physiological selection is "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species," and that the occurrence of infertility among the members of an undifferentiated species is secondary and comparatively unimportant, I ask leave to quote one or two more of his original statements, in addition to the four emphatic passages quoted in my communication of November 27.

(1) "When accidental variations of a non-useful kind occur in any of the other systems or parts of organisms, they are, as a rule, immediately extinguished by intercrossing. But whenever they happen to arise in the reproductive system in the way here suggested, they must inevitably tend to be preserved as new natural varieties, or incipient species. *At first the difference would only be in respect of the reproductive system; but eventually, on account of independent variation, other differences would supervene, and the new variety would take rank as a new species*" (*Nature*, vol. xxxiv. p. 316).

The words I have italicized show clearly that variation in fertility only was what Dr. Romanes then claimed as essential to his theory. Again, after referring to variations in the season of flowering as a "well-known and frequently observed cause" of isolation, he adds: –

(2) "But it is on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection" (*l.c.*, p. 337).

The meaning of this is still further enforced by other passages. After discussing the supposed causes of infertility, he says: –

(3) "Why should we suppose that, unlike all other such variations, it can never be independent, but must always be superinduced as a secondary result of changes taking place elsewhere? It appears to me that the only reason why evolutionists suppose this is because the particular variation in question happens to have as its result the origination of species" (*l.c.*, p. 339).

And again: –

(4) "It appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that *the cases where it has been superinduced by the secondary distinctions are comparatively few in number*" (*l.c.*).

Notwithstanding the passages I have now quoted, emphasizing eight times over, in different ways, that the theory is essentially one of variations as regards fertility and sterility alone, Dr. Romanes now says that, even if all this is wrong, "the principle of physiological selection, as I have stated it, is not thereby affected." If this is not an absolute change of front, words have no meaning; and it is further shown to be so by the fact that Dr. Romanes acknowledged that Mr. Catchpool had "very clearly put forward the theory of physiological selection." But Mr. Catchpool clearly distinguished between the old theory that species arise *first* by variation in form and structure, and only *gradually* become mutually infertile, and the new theory that they arise "by spontaneous variations in the generative elements, and are in this case *originally* mutually infertile, but only *gradually*

become otherwise divergent" (*l.c.*, vol. xxxi. p. 4).

That this was the essential and original "physiological selection," that was claimed as supplying the missing link required to make the origin of species by natural selection a reality, is yet further shown by the repeated statements that physiological "selection" is a powerful preservative agent. Besides the statement already quoted, that variations in fertility "cannot escape the preserving agency of physiological selection," we have the assertion, quoted above, that such variations "must inevitably tend to be preserved as new natural varieties or incipient species," and the following still more emphatic assertion: – "Neither are we concerned with the degrees of sterility which the variation in question may in any particular case supply. For whether the degree of sterility with the parent form be originally great or small, the result of it will in the long run be the same: the only difference will be that in the latter case a greater number of generations would be required in order to separate the varietal from the parent form."

Now my contention has always been, and still is, that there is no principle at work which can accumulate or even preserve the variations of infertility occurring in an otherwise undifferentiated species, and that the term physiological "selection" is therefore a misnomer, and altogether misleading. If Dr. Romanes will carefully work out numerically (as I have attempted to do) a few cases showing the preservative and accumulative agency of pure physiological selection within an otherwise undifferentiated species, he will do more for his theory than volumes of general disquisition or any number of assertions that it *does* possess this power.

My next contention is, that this is the only new part of his theory – as he himself shows by his reference to the ordinary view, of sterility following other changes, as that which "evolutionists suppose." All the rest is to be found more or less fully discussed in Darwin's works; and I myself claim only to have carefully studied Darwin's facts, and his brief but most suggestive discussion of them in his chapter on "Hybridism" (vol. ii. of "Animals and Plants under Domestication"), and by arranging them more systematically to have shown that they do really give a fairly consistent and sufficient solution of the problem. The only part of my work I claim as a distinct addition to the theory is the proof that, under certain conditions that appear to me probable, natural selection *is* capable of increasing incipient infertility between distinct races or varieties; and the same view was submitted to Darwin twenty years ago.

Lastly, I totally and emphatically deny that any portion of my facts or conclusions on the subject were derived from Dr. Romanes's writings on "physiological selection." The only two sentences he has quoted from my book to prove that I have done so merely express what he himself has declared to be the common opinion of evolutionists, and which is also the direct outcome of the facts collected by Darwin. If this is "the whole essence of physiological selection," then physiological selection is but a re-statement and amplification of Darwin's own views, since he certainly assumed, and proved, that "some amount of infertility" characterized "some varieties" of animals and plants, and that this infertility, when it occurs, is of some use in preventing the swamping effects of intercrossing. I feel sure that if *this* had been stated, at the outset, to be what was termed "physiological selection," no discussion would have arisen as to the principle involved, but only as to its novelty and as to the appropriateness of the name given to it.

If now, notwithstanding his repeated and emphatic statements that variation as regards fertility in otherwise undifferentiated species was what constituted the basis of his theory of physiological selection, Dr. Romanes continues to assert that I have adopted that

theory "purely and simply, without any modification whatever," it will show that our respective standards of scientific reasoning and literary consistency are so entirely different as to render any further discussion of the subject on my part unnecessary as regards myself and useless as regards Dr. Romanes.

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Mr. Gulick on Divergent Evolution (S410)

John Gulick (1832–1923), an ally of George Romanes, came up with a theory of "cumulative divergence" that Wallace found hard to accept. His letter on the matter appeared in the Nature issue of 20 September 1888.

Mr. Gulick's paper on this subject appears in the last number of the Journal of the Linnean Society as having been "communicated by Alfred Russel Wallace, F.L.S." It may therefore be supposed that I recommended its publication, or that I agree with its main argument; and as this is not the case, I ask permission to say a few words on the subject in the columns of *Nature*.

In 1872, Mr. Gulick sent me his paper on "Diversity of Evolution under One Set of External Conditions," requesting me, if I thought fit, to communicate it to the Linnean Society. As the paper contained a body of very interesting facts observed by the author, I had no hesitation in recommending its acceptance by the Society, although I did not agree with the conclusions Mr. Gulick drew from his facts.

Last year Mr. Gulick send me the manuscript of his present paper, informing me that it was the result of long-continued study of the subject, and asking me to forward it to the Linnean Society. I did so, writing to the Secretary that I had not read the paper through, and did not undertake the responsibility of recommending it for acceptance.

Having now read the paper in print, I find very little in it that I can agree with. I can discover in it no additional facts beyond those which were set before us in the former paper sixteen years ago, while there is an enormous body of theoretical statements, many of which seem to me erroneous, and a highly complex classification of the conditions under which the separation or isolation of individuals of a species takes place, with a new and cumbrous terminology, neither of which, in my opinion, adds to our knowledge or comprehension of the matter at issue.

As in almost every page of this long paper I find statements which seem to me to be either disputable or positively erroneous, any extended criticism of it is out of the question; but I wish to call attention to one or two points of vital importance. Mr. Gulick's alleged discovery is, "the law of cumulative divergence through cumulative segregation" (p. 212). He maintains that any initial variation, if isolated by any of the causes he has enumerated, but remaining under identically the same environment, will increase till it becomes in time a specific or even a generic divergence, and this without any action whatever of natural selection. Now if this is *a fact* it is a most important and fundamental fact, equal in its far-reaching significance to natural selection itself. I accordingly read the paper with continual expectation of finding some evidence of this momentous principle, but in vain. There is a most elaborate discussion and endless refined subdivisions of the varied modes in which the individuals constituting a species may be kept apart and pre-

vented from intercrossing, but no attempt whatever to prove that the result of such complete or partial isolation is "cumulative divergence." The only passage which may perhaps be considered such an attempt at proof is that on p. 219, where he supposes an experiment to be made, and then gives us what he thinks "experienced breeders" will assure us would be the result. In this experiment, however, there is to be constant selection and reassortment of each brood, yet he asserts that "there is no selection in the sense in which natural selection is selection"; by which he appears to mean that the selection is by "separation" not by "extermination." This, however, seems to me to be a distinction without a difference.

Again, in the various illustrations of how "cumulative segregation" is brought about, natural selection must always come into play – as in the case of a change in digestive powers, and consequent adoption of a different food (p. 223), leading to partial isolation; and such cases are exactly what is contemplated by Darwin in his brief statement of the effects of "divergence of character" ("Origin," pp. 86–90), while the concurrence of "isolation" as a factor is fully recognized at pp. 81–83 of the same work (6th edition).

It appears to me that throughout his paper Mr. Gulick omits the consideration of the inevitable agency of natural selection, arising from the fact of only a very small proportion of the offspring produced each year possibly surviving. Thus when, at p. 214, he states that "the fact of divergence in any case is not a sufficient ground for assuming that the diverging form has an advantage over the type from which it diverges," he omits from all consideration the fact that at each step of the divergence there was necessarily selection of the fit and the less fit to survive; and that if, as a fact, the two extremes have survived, and not the intermediate steps which led to one or both of them, it is a proof that *both* had an advantage over the original less specialized form. Darwin explains this in his section on "Extinction caused by Natural Selection" (p. 85). On the whole, I fail to see that Mr. Gulick has established any new principle, either as a substitute for, or in addition to, natural selection as set forth by Darwin. Others, however, may think differently; and I shall be glad if any naturalists who have studied Darwin's works will point out, definitely, in what way this paper extends our knowledge of the mode in which species have originated.

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Lamarck *versus* Weismann (S415)

Here Wallace defends himself against assertions he is adopting a Lamarckian stance in his new book, Darwinism. The letter appeared in the 24 October 1889 issue of Nature.

I had not intended to reply to Mr. Cunningham's criticism of a passage in my book which he thinks is pure Lamarckism (see *Nature*, July 25, p. 297); but now that Prof. Ray Lankester adopts the same view, I will make a few remarks upon the case Mr. Cunningham italicizes the words, "the constant repetition of this effort causes the eye gradually to move round the head till it comes to the upper side," and claims this as a Lamarckian explanation. But if we italicize the following words, which occur three lines further on, "*those usually surviving whose eyes retained more and more of the position into which the young fish tried to twist them*," we shall see that the survival of favourable variations is,

even here, the real cause at work. For the transference of the eye to the upper side was a useful change – perhaps, under the peculiar conditions of existence and development – an absolutely essential one. The amount to which the eye could be twisted and retained in its new position was variable, as all other such characters are variable. Those individuals who had this faculty in the greatest degree were among those that survived, and it is not at all necessary to assume that any portion of the change *due solely to the effort* was inherited, but only that those individuals which were the most favourably constituted in this respect transmitted their peculiar constitution to their offspring, and thus the twisting would take place earlier and earlier in the development of the individual. Even Darwin himself, who believed in the heredity of acquired variations, says that "the *tendency to distortion* would no doubt be increased through the principle of inheritance"; and this is really all that is necessary. In most of the higher animals symmetrical development of the two sides of the body is of vital importance, and is strictly preserved by natural selection; but more or less defect of symmetry often occurs as a variation or monstrosity, and in cases where such asymmetry was useful these variations would be preserved and increased by selection and heredity. An altogether erroneous view is taken of the fact of effort being used in this case, as if it were something unusual. But in all cases selection produces changes which are useful and whose use is often indicated by effort. The giraffe uses effort in stretching its neck to obtain food during a drought; the antelope exerts itself to the utmost to escape from the leopard; but it is now recognized that it is not the individual change produced by this *effort* that is inherited, but the favourable constitution which renders extreme effort unnecessary, and causes its possessors to survive while those less favourably constituted, and who therefore have to use greater effort, succumb. In the case of the developing flat-fish also, the effort indicated the direction of the useful modification, and any variations tending either to the right kind of asymmetry or to a mobility of the eye, admitting its being twisted and retained in its new position, during the growth of the individual, would be certainly preserved.

I wish to take this opportunity of thanking Prof. Ray Lankester for his careful and appreciative review of my book. I am too well aware of my own deficiency in training as a naturalist not to admit all the shortcomings which he so tenderly refers to. It is quite refreshing to me to read at last a real criticism from a thoroughly competent writer, after the more or less ignorant praise which the book has hitherto received. I admit also that the term "laboratory naturalist," to which he demurs, was not well chosen. I meant it as the opposite, not so much to "field naturalist" as to "systematic naturalist"; and it still seems to me that the gentlemen he refers to as not being "laboratory naturalists" are still less "systematic naturalists," in the sense of having specially devoted themselves to the observation, description, and classification of more or less extensive groups of species of living organisms.

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Birds and Flowers (S425)

A short but interesting letter printed in the Nature issue of 24 July 1890.

In your note on Mr. G. F. Scott-Elliot's paper on this subject (*Nature*, July 17, p. 279) you remark: "In accordance with the view of Darwin, but opposed to that of Wallace, Mr.

Scott-Elliot believes that the identity of colour (an unusual shade of red) in the majority of ornithophilous flowers and on the breasts of species of *Cinnyris* is an important element in pollination by birds." There must be, I think, some misapprehension here. I am not aware that Darwin has anywhere referred to the colours of birds as being generally similar to those of the flowers they frequent. Mr. Grant Allen has done so in his work on "The Colour-Sense," and I have opposed his views in *Nature* (vol. xix. p. 501), because he founds the resemblance on the theory of sexual selection, and because the facts do not support any such general relation. That such a relation does sometimes occur I have shown, by quoting Mrs. Barber in my "Darwinism" (p. 201) as to the scarlet and purple colours of a sun-bird being highly protective when feeding among the similarly coloured blossoms of the *Erythrina caffra*, which, at the time, has no foliage. I have also called attention (in the same work, p. 319) to the numerous flowers now known to be fertilized by birds, and to the numerous large tubular flowers of a red and orange colour in Chile and the Andes, which are apparently adapted to be fertilized by humming-birds. The general uniformity of colour would be advantageous as an indication of bird-flowers as distinguished from insect-flowers; but there is no similarity to the colours of the birds. Curiously enough, the common Chilian Eustephanus is green-coloured in both sexes, while its close ally in Juan Fernandez is red in the male. Yet the flowers it frequents in the island are not red, but mostly white and yellow (see "Tropical Nature," p. 272). It is evident, therefore, that the prevalent colours of the flowers do not determine the colours of the birds which frequent them, unless those colours are so predominant that a similar colour becomes protective, as is more generally the case in the scantily-wooded plains of South Africa than anywhere else.

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The Inheritance of Acquired Characters (S439ac)

Wallace made more contributions to The Garden, a London weekly, than to any other serial publication, save Nature. Many of these items were minor gardening-related notes, but some touched on more scientific questions. This one appeared in the 15 August 1891 issue.

Sir, – The reviewer of Mr. [George] Henslow's book on "The Making of Flowers" in a late issue says that Weismann's theory, that acquired characters are not inherited, "has many facts against it, as observed by farmers and gardeners every day of their lives." As one of those who accept, provisionally, Weismann's theory, and also as, in a humble way, a gardener, I should be very glad to have precisely stated some of the very numerous facts of constant occurrence said to be against this theory.

In order to avoid needless trouble and misunderstanding of the real point at issue, I ask to be allowed to state the problem as it appears to me.

In the first place, every farmer and gardener knows that plants raised from seed vary immensely, and that it is by the selection of variations of this kind in the desired direction that all the improved varieties of our flowers and vegetables have been produced, "because such variations are more or less hereditary." These are "congenital" or "germ" characters, and of their transmission to the offspring there is no question. But many, per-

haps most, of these characters only appear in the adult individual as variations of the flower and fruit, and thus variations of this kind may appear to be due to external conditions without being really so. Again, it is so constantly the practice of gardeners, &c., to select from among such variations those that suit their purpose, while in other cases the forces of Nature bring about a corresponding selection, that this is apt to be overlooked, and thus the effects of selection among spontaneous or congenital variations get mixed up with the effects of changed conditions acting on individual plants or animals, producing changes of form or structure, which changes may conceivably be either confined to these individuals or be transmitted to their offspring. Every gardener and farmer modifies the plants he grows by supplying them with special conditions which produce results favourable to his special purpose. Such are the various manures, the particular soils, the supply of water, of shelter from winds, of shade or sunshine, the time of sowing, &c.; and the question is whether changes thus produced in the individual are transmitted, as a rule, to the offspring. To determine this is not easy, because, as I have said, the cultivator always selects also, and perhaps no experiment has ever yet been made in which selection has been rigidly excluded. In order to do so, some such course as the following must be tried:

Let seed of some easily grown plant be taken and divided fairly into two equal portions. Let each set of plants may be submitted to a diverse set of conditions during its whole life, but always so regulated as to allow both sets to grow healthily and to produce flowers and seeds. Next year let the whole of the seeds from each lot be again sown and subjected to the same conditions, or if the seed is more than required, let the same quantity of each be taken by some process which will not select the best seeds in any way, but will take a true average sample. In the second year a very considerable difference will, no doubt, be seen in the two lots grown under diverse conditions, and to determine if this difference acquired by the individuals is in any degree hereditary, a fair sample of the seed of each must now be sown side by side, under as near as possible identical conditions. If now the difference which had been produced in the stem, foliage, flowers, or fruits of the two lots grown under different conditions is maintained when the seed is grown under the *same conditions* – selection at any stage having been carefully avoided – then there would be some evidence that individually acquired characters may be, and are actually inherited.

With regard to Mr. Henslow's main theory that irritations set up by insects have modified structures, and that these modifications have been inherited, I would ask whether plants that have for many generations produced galls under the irritation of insects have ever been known to produce galls when the visits of the gall-producing insect have been absolutely prevented? If the results of insect irritation are in any degree inherited, we ought surely to find some plants which constantly produce growths resembling those of the special galls of the species, but without any insect agency having intervened. – Alfred R. Wallace.

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Variation and Natural Selection (S440)

Wallace and C. Lloyd Morgan (1852–1936), a founder of the field of comparative psychology, were mutual admirers. This didn't mean they agreed

on everything, as this letter, printed in the 1 October 1894 issue of Nature, demonstrates.

In Prof. C. Lloyd Morgan's Presidential address to the Bristol Naturalists' Society, on "The Nature and Origin of Variations" (of which he has kindly sent me a reprint from the Society's Proceedings), there are one or two points on which there seems to me to be a slight misconception; and as the difficulties suggested have probably occurred to other naturalists, I wish to make a few observations in the hope of throwing a little light on this obscure subject.

After referring to the proofs of the variability of species in a state of nature which I have adduced in my "Darwinism" (to which proofs Prof. Lloyd Morgan has made some important additions in his recent work on "Animal Life and Intelligence") he remarks: – "We have been apt to suppose that a species is so nicely adjusted to its surrounding conditions that all variations from the type, unless of a very insignificant character, would be rapidly and inevitably weeded out. This, it is clear, is not true at any rate for some species." And a little further on, after discussing the question whether variations in all directions occur in equal proportions – an equality which does not appear to me to be at all necessary, or to have been ever suggested as occurring – he says: "And the candid biologist must, I think, admit that the evidence in Mr. Wallace's third chapter, while conclusive as to the occurrence of variations, gives on analysis little or no evidence of any selective agency at work."

The difficulties here stated appear to me to depend, chiefly, on not taking account of some important facts in nature. The first fact is, that the struggle for existence is intermittent in character, and only reaches a maximum at considerable intervals, which may be measured by tens of years or by centuries. The average number of the individuals of any species which reach maturity may be able to survive for some years in ordinary seasons or under ordinary attacks of enemies, but when exceptional periods of cold or drought or wet occur, with a corresponding scarcity of certain kinds of food, or greater persecution from certain enemies, then a rigid selection comes into play, and all those individuals which vary too far from the mean standard of efficiency are destroyed.

Another important consideration is that these epochs of severe struggle will not be all of a like nature, and thus only one particular kind of unbalanced or injurious variation may be eliminated by each of them. Hence it may be that for considerable periods almost all the individuals that reach maturity may be able to survive, even though they exhibit large variations in many directions from the central type of the species. During such quiescent periods, the chief elimination will be among the young and immature. Thus, with birds probably nine-tenths of the destruction occurs among the eggs and half-fledged young, or among those which have just escaped from parental care; while those which have survived to breeding age only suffer a slight destruction in ordinary years, and this may occur partly among the less experienced, partly among those which are old and somewhat feeble.

The severe elimination that occurs in the earlier stages may be thought to be accidental, but I doubt if it is really so except in a very small degree. The protection and concealment of the eggs and young in the nest will depend chiefly on the mental qualities or instincts of the parents, and these will have been always subject to a rigid selection owing to the fact that those with deficient instincts will leave fewer offspring to inherit their deficiency. And with young birds of the first year there will be an equally rigid selection of the incautious, and of those who are deficient in any of the sense-perceptions, or are less

strong and active than their fellows.

The proof that there *is* a selective agency at work is, I think, to be found in the general stability of species during the period of human observation, notwithstanding the large amount of variability that has been proved to exist. If there were no selection constantly going on, why should it happen that the *kind* of variations that occur so frequently under domestication never maintain themselves in a state of nature? Examples of this class are white blackbirds or pigeons, black sheep, and unsymmetrically marked animals generally. These occur not unfrequently, as well as such sports as six-toed or stump-tailed cats, and they all persist and even increase under domestication, but never in a state of nature; and there seems no reason for this but that in the latter case they are quickly eliminated through the struggle for existence – that is, by natural selection.

One more point I will advert to is Prof. Lloyd Morgan's doubt, in opposition to Mr. Ball, "whether a thicker or thinner sole to the foot is a character of elimination value, whether it would determine survival or elimination, and make all the difference between passing or being plucked in life's great competitive examination." This seems to me to be a rather unfortunate objection, since, in constantly recurring circumstances during the life of a savage, this very character must be of vital importance. Whether on the war-path, or in pursuit of game, or when escaping from a human enemy or from a dangerous animal, the thickness of the sole, its insensibility to pain, and its resistance to wear and tear must have often determined life or death. A man who became sore-footed after a long day's tramp, or one whose thin sole was easily cut or torn by stones or stumps, could never compete with his thicker soled companions, other things being equal; and it seems to me that it would be difficult to choose a single physical character whose variations would be more clearly subject to the law of selection.

With the greater portion of Prof. Lloyd Morgan's very interesting address I am in perfect accord, and it is because his remarks and suggestions are usually so acute and so well founded that I have thought it advisable to point out where I think that his objections have a less stable foundation.

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Topical Selection and Mimicry (S442)

Writer David Syme's anti-Darwinism book On the Modification of Organisms elicited a rather savage review from Wallace in the pages of Nature. Syme responded with a long reply, but Wallace showed he was still one of Darwin's champions with countering remarks published in the 12 November 1891 issue.

Mr. Syme now says: "The references to Darwin in my book are absolutely correct," and – "In every reference to Darwin's views I gave the page and the edition from which the quotation was taken." Assertions, however, are not proofs; but if Mr. Syme will point out where Darwin defines natural selection as "the struggle for existence," and where Darwin "insists that variations are created by natural selection," statements which occur at p. 8 and p. 15 of Mr. Syme's book, I will acknowledge that I have misrepresented him. Otherwise I see nothing that requires modification in my article. But as Mr. Syme claims to have taken "the utmost pains" to quote Darwin's exact words, I will refer to other cases.

At p. 12 he says, "The second assumption is that favourably modified individuals should be few in number, 'two or more';" and for this he refers to "Plants and Animals under Domestication," vol. ii. p. 7. The true reference is to vol. i. p. 7, where Darwin says: "Now, if we suppose a species to produce two or more varieties, and these in course of time to produce other varieties, &c." Here we see that Mr. Syme puts "individuals" in the place of "varieties," and thus makes Darwin appear to say the exact reverse of his main contention, which is, that ordinary variability occurring in large numbers of individuals, not single sports, are the effective agents in the modification of species.

Again, at p. 102, Mr. Syme says, when discussing cross-fertilization and variability: "No doubt self-fertilization is a great factor in producing uniformity of colour. That this uniformity is not due to the plants having been 'subjected to somewhat diversified conditions,' as Darwin intimates, is shown by the fact, &c." But Darwin, as every student knows, said exactly the reverse of this – that the somewhat diversified conditions produced *variability*; and Mr. Syme's great efforts to understand him and to quote him correctly again fail of success.

One more example is to be found at p. 110, where he says: "Darwin has distinctly laid down the principle that if it can be proved, by a single instance, that one organism exists for the benefit of another organism, his whole system would fall to the ground." But the statement made by Darwin was, that if any part of the structure of one species could be proved to have been formed for the *exclusive* good of another species it would annihilate his theory ("Origin," 6th edition, p. 162). Mr. Syme omits the essential word "exclusively," and thus appears to have a strong case against the theory.

As an example of general misrepresentation, I will refer to p. 86, where Mr. Syme states that "the Darwinist" "carefully ignores the facts which point in the opposite direction" (of the necessity for insect fertilization of flowers); and on the next page, after referring to cleistogamic and other self-fertilized flowers, he asks: "Why does the Darwinist omit mention of such structures as these?" But he does not refer us to the Darwinists in question who, while discussing insect fertilization, "carefully ignore" self-fertilization; and as his statement will be taken to include all, or at least the majority of Darwinists, it must be held, by those who are acquainted with the facts, to be a very absurd misrepresentation.

Other examples might be given, but these are sufficient to support my statement that Mr. Syme has both misquoted and misrepresented Darwin.

The exposition of his theory of "topical selection" to explain the phenomena of mimicry, as given above, may be left to the judgment of the readers of *Nature*. – Alfred R. Wallace.

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Note on Sexual Selection (S459)

Wallace and Darwin disagreed on several elements of the latter's theory of sexual selection. Wallace continued to criticize the theory after Darwin's death in 1882, as in this letter printed in Natural Science in December 1892.

In Mr. Cunningham's review of Mr. Romanes' new work, "Darwin, and after Darwin," the writer gives an independent and thoughtful criticism of my views on that subject, and I should like to be permitted to make a few observations in reply thereto. Mr. Cunningham says: – "Mr. Wallace argues as though the superior male, *facile princeps*, in the competition for a living, could found a line of descendants inheriting his own health and vigour, without female assistance." And again: "A male that excels in the struggle for existence is a complete failure, so far as the species is concerned, unless he can succeed also in finding mates." These passages seem to me to involve suppositions against all probability and all evidence. They imply that the better organised male, in all respects, except in ornament, is rejected by the females in favour of the worse organised in every respect, except in ornament. There are here two improbable assumptions – the first, that the most ornament is usually, or frequently, dissociated from the best general organisation; and the second, that any such less perfectly organised male would be preferred by the female on account of his slightly superior ornament. For the difference, it must be always remembered, *is* slight. Out of a hundred male pheasants or peacocks of the same age, the difference in length of plumes or shade of colour is rarely very conspicuous or even perceptible, except by close comparison; and if whatever difference there is were *not* usually associated with vigour and health, then the two forms of sexual selection – by combat and by display of ornament – would lead to different results; and, as males with ornamental appendages usually *do* fight for the females, the most ornamented would *not*, in their case, be the parents of the next generation.

We are, therefore, forced to conclude that the two qualities – general vigour and ornament – *are* not independent of each other, but are developed *pari passu*, and the problem then becomes, does the female determine her choice by the latter rather than the former? I quite agree with Mr. Cunningham when he says: "The sexual desire of the female has a hereditary association with certain sensory stimuli, and the means of furnishing these stimuli are constantly reproduced and improved by inheritance in the males." I further admit that the display of ornament by the male is one of the means of exciting this desire; but mainly because it is an indication of sex, of sexual maturity, and of sexual vigour, probably not at all on account of details of colour or pattern.

There is, however, another consideration which Mr. Cunningham appears to have overlooked, and that is, the necessary weakness, comparatively, of female selection, owing to the very limited range of her choice. The law of survival of the fittest has such enormous selecting power because of the overwhelming odds against the less fit. A species which has two or three broods a year, or one large brood, and which lives, say, ten or twenty years, as do many of the vertebrata, produces from 50 to 100 successors of each pair, from which one or two only are selected to take the place of their parents. But in the case of sexual selection, it is a question of probably not more than two or three to one in most species, and in many even less, for there is no evidence and little probability that the number of healthy and competent males that fail to find mates bears any large proportion to those that do find them. Much of the success of particular males must depend on early chance encounters with a mate, while the competition can only be among small groups in each locality. If we add to this the consideration that in almost every case combat, or agility, or bodily vigour must have great influence, the part that remains to be played by ornament alone will be very small, even if it were proved, which it is not, that a slight superiority in ornament alone usually determines the choice of a mate.

This, however, is a matter that admits of experiment, and I would suggest that either

some Zoological Society or any person having the means, should try such experiments. A dozen male birds of the same age – domestic fowls, common pheasants, or gold pheasants, for instance – should be chosen, all known to be acceptable to the hen birds. Half of these should have one or two tail plumes cut off, or the neck plumes a little shortened, just enough to produce such a difference as occurs by variation in nature, but not enough to disfigure the bird, and then observe whether the hens take any notice of the deficiency, and whether they uniformly reject the less ornamented males. Such experiments, carefully made and judiciously varied for a few seasons, would give most valuable information on this interesting question. Till this is done, suppositions as to what determines the choice of the female can have but little value.

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The Non-Inheritance of Acquired Characters (S473)

Wallace continued his lifelong attack on Lamarckism in a letter printed in the Nature issue of 20 July 1893.

I wish to call the attention and elicit the opinion of naturalists as to the interpretation of certain facts bearing upon this question.

In my article in the *Fortnightly Review* of May last, p. 664, I give what appears to be a new interpretation of facts which have been often quoted, as to the change in the external characters of a Texan species of Saturnia when the larvæ were fed upon *Juglans regia*, its native food-plant being *Juglans nigra*; and the somewhat analogous facts as to *Artemia salina* being changed into *A. Milhausenii* (the former living in brackish, the latter in salt water) when the water became gradually more salt; the change in this case being progressive, year by year, and proportionate to the change in the saltness of the water. The reverse change was also effected by gradually reducing the salinity of the water inhabited by *A. Milhausenii*.

As regards the former case I remarked in my article as follows:

> Prof. Lloyd Morgan (in his 'Animal Life and Intelligence,' pp. 163–166) clearly sees that this and other cases do not prove more than a modification of the individual; but it seems to me to go further than this. For here we have a species the larvæ of which for thousands, perhaps millions, of generations have fed upon one species of plant, and the perfect insect has a definite set of characters. But when the larvæ are fed on a distinct but allied species of plant, the resulting perfect insect differs both in colouration and form. We may conclude from this fact that some portion of the characters of the species are dependent on the native food-plant, *Juglans nigra*, and that this portion changed under the influence of the new food-plant. Yet the influence of the native food-plant had been acting uninterruptedly for unknown ages. Why then had the resulting characters not become fixed and hereditary? The obvious conclusion is, that being a change produced in the body only by the environment, it is not hereditary, no matter for how many generations the agent continues at work; in Weismann's phraseology it is a somatic variation, not a germ variation.

I then referred to the marked difference between somatic and germ variations in plants, the former disappearing at once, the latter persisting, when cultivated under ab-

normal conditions; and also to the cases of many closely allied species of animals and of the races of mankind, which preserve their distinctive characteristics when living and breeding under very different conditions.

The above seems to me a perfectly valid and logical argument, and I was interested to see how it would be met by Lamarckians, who have frequently referred to the same facts as being obviously in their favour, though without any attempt to show how and why they are in their favour. I was therefore rather surprised to read, in the July issue of the *Contemporary Review*, a paper by Prof. Marcus Hartog, in which he characterises my argument as a very bad kind of special pleading, and adds that it amounts to this: "Any change in the offspring produced by altered conditions in the parent is limited to characters that are 'not fixed and inherited'; for fixed and inherited characters cannot be altered by changed conditions in the parent; therefore no experimental proof can be given of the transmission of acquired characters."

The above is of course simple reasoning in a circle, and I cannot recognise it as my reasoning. I have made no general proposition that "fixed and inherited characters cannot be altered by changed conditions in the parent," or that "no experimental proof can be given of the transmission of acquired characters." But I argue that when a decided character is immediately changed by changed conditions of the individual, as in Saturnia, it is not "fixed and inherited." The experiment itself shows that it is not a fixed character, and there can be no proof that it is inherited so long as it only appears under the very same changed conditions that produced it in the parent.

As to experimental proof I believe it to be quite possible. There is one case, which I do not remember having seen referred to, in which nature has tried an experiment for us. I was informed by the President of the Deaf-Mute College at Washington that the male and female students frequently marry after leaving the college, and that their children are rarely deaf-mutes. But the point to which I wish to call attention is the admitted fact that there is usually no disease or malformation of the vocal organs in a deaf-mute. Now, before deaf-mutes were taught to talk as they are now, they passed their whole lives without using the complex muscles and motor-nerves by the accurate coordination of which speech is effected. Here is a case of complete disuse, and there must have been some consequent atrophy. Yet it has, I believe, never been alleged that the children of deaf-mutes exhibited any unusual difficulty in learning to speak, as they should do if the effects of disuse of the organs of speech in their parents were inherited. Here is at all events the material of an experiment ready to our hands. An experiment to show whether the effects of use and disuse were inherited might also be tried by bringing up a number of dove-cot pigeons in a large area covered in with wire netting so low as to prevent flight, at the same time encouraging running by placing food always at the two extremities of the enclosure only, or in some other way ensuring the greatest amount of use of the legs. After two or three generations had been brought up in this way, the latest might be turned out among other dove-cot pigeons, at the age when they would normally begin to fly, and it would then be seen if the diminished wing-power and increased leg-power of the parents were inherited.

No doubt many better experiments might be suggested; but these are sufficient to indicate the character of such as do not require that the offspring be submitted to the same conditions as those which produced the change in the parents, and which thus enable us to discriminate between effects due to inheritance and those due to a direct effect of the conditions upon the individual. The cases of the Saturnia and the shrimps are of the latter

kind, and in their very nature can afford no proof of heredity.

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Recognition Marks (S485)

This short note, printed in the Nature issue of 16 November 1893, responded to an enquiry by a Mr. MacGillivray on rabbits' use of their tails as danger signals.

Mr. MacGillivray has failed to grasp the principle of natural selection when he thinks that it cannot produce a character useful to other animals of the same species. The action of natural selection is to preserve the *species*, as well as each individual separately; and, consequently, every character useful to the species as a whole would be preserved. This is obvious when we consider such characters as nest-building in birds, and milk-secretion in mammals, which do not benefit the individual possessors, but their offspring; and the same principle applies to every character which is mutually useful to individuals of the same species, as are what I have termed "recognition characters." Neither can I admit that the habits of the hare render the white upturned tail "quite unnecessary." The hare is a nocturnal feeder, and a mark which readily distinguishes a friend from an enemy, and enables the young during their short period of infancy to keep within sight of the mother, must be of considerable importance.

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Panmixia and Natural Selection (S499)

The OED defines "panmixia" as [August] "Weismann's term for a supposed promiscuous reproduction of all manner of ancestral qualities or tendencies, consequent on the cessation of natural selection in relation to organs which have become useless or little used, and tending to the degeneration of these organs." Wallace provided an analysis of the subject in the 28 June 1894 issue of Nature.

Mr. Weldon's letter on this subject, in *Nature* of May 3, calls, I think, for a few further observations. He first criticises the statement that "the survival-mean must, on cessation of selection, fall to the birth-mean," by showing that there are probably cases in nature in which the survival and birth-means may coincide, owing to the removal by selection of all individuals above and below the mean, they being approximately equal in number. This is, no doubt, the case with certain characters of a species, but probably never with all or even with most characters. Darwin states that in France and Germany white pigeons are killed off by kites, and that on the coast of Ireland black fowls are also killed off by sea-eagles. These and other analogous facts render it probable that in many species of animals colour is kept to the inconspicuous and protective mean tint by the elimination of all individuals which vary much on either side of it, and thus, as regards colour, the birth-mean and the survival-mean may be almost identical. But with many other characters this is not the case. In sheep, cattle, and horses it has been observed that when the

larger lowland breeds are taken to bleak mountain regions they gradually dwindle in size, only the smaller and hardier of each generation surviving the severe winter and spring climate and the comparatively innutritious food. Here the elimination is clearly in one main direction; and the absence of this selection due to the transference of the whole body of such reduced individuals to a milder climate and better pastures, would no doubt lead to a slight increase of average size, indicating that the birth-mean had been above the survival-mean. So also in the case of the half-wild horses of Circassia, which are greatly exposed to attacks of wolves and to extreme vicissitudes of climate, swiftness, strength, wariness, and a hardy constitution must be kept at a high level of efficiency by the elimination of the less gifted in these qualities; so that here again the birth-mean must be below the survival-mean. In such cases as these there seems no difficulty in the fact that the mean characters do not change for many generations; for this is in accordance with Darwin's principle that natural selection "cannot produce absolute perfection, but only relative perfection." When the average characters of a species have reached a point such that it can permanently maintain itself in a given area, then no further change will occur; but, the less efficient being constantly weeded out, the survival-mean will be necessarily a little above the birth-mean. Both means will, however, be sensibly permanent as long as the environment remains unchanged.

Mr. Weldon says that it has not been shown that, in some given case, Panmixia does in fact occur; and further, that in the only case which has been experimentally investigated – that of the stature of civilised Englishmen – the consequences said to result from it do not, in fact, occur. To obtain absolute evidence of Panmixia, or of the action of Natural Selection, is extremely difficult, because we cannot first compare and measure minutely a large number of individuals in a state of nature, and then follow those same individuals throughout their lives and see how nature deals with them. We can, however, observe what happens in the case of semi-wild animals, and the examples already cited show that natural selection must, and actually does, act on the character of colour, weeding out those which diverge on both sides towards whiteness or blackness, and in the case of physical and mental activities destroying those which fall below the standard of excellence requisite for the preservation and continuance of life.

In our domesticated animals, on the other hand, we find what are probably examples of the effects of Panmixia. The wing-bones of our pigeons, fowls, and ducks, as compared with wild individuals, were found by Darwin to be decidedly reduced in size in proportion to the leg-bones; but a part of this may be due to disuse in the individual, and to determine the share of the two causes seems impossible. There are, however, a few characters in which we see Panmixia alone at work in our domesticated animals. Such are, for example, the constant appearance and increase among them of prominent *unsymmetrical* markings, as in dogs, cats, cattle, and horses. Such markings never occur in wild races, or if they occur in individual cases they never increase; and I have given reasons for thinking that symmetrical colour and marking is kept up in nature for facility of recognition, a factor essential to preservation, and to the formation of new species. In this case, there can be no question of disuse, while as we know that white and unsymmetrical individuals do occasionally occur in wild species, but never increase, the fact of their increase under domestication must be due to the absence of whatever form of natural selection eliminates them in nature; that is, to Panmixia. Another illustration may perhaps be found in the fact of curled tails appearing in domestic pigs and some races of dogs, while no wild animal is known which has a curled tail. We can hardly doubt that the special form of tail in each

animal is of use to it, and that any abnormality, like a curled tail, would be eliminated under nature. Its appearance and perpetuation under domestication is therefore a fair example of Panmixia.

The slow increase of the stature of civilised Englishmen, which Mr. Galton is said to have proved, may, it seems to me, be partly a result of Panmixia, and partly due to more healthy conditions of life acting on the individual. It is, I presume, a fact, as generally stated, that old armour shows that the knights of the middle ages were rather short men. This may have been a result of natural selection, because, as a rule, the strongest and most active men are rather under than over middle height; while tall men would certainly be more exposed to danger, would have to carry a greater weight of armour, and by thus overloading their horses would be under a disadvantage in battle. Tall men would thus be killed off rather faster than short men; and the same might be the case even after the disuse of armour, so long as rapine and civil war prevailed over a large part of the country. But during the last two centuries of comparative peace tall men have been under no such disadvantage, and their survival may have aided in bringing about the slight increase of average stature which has been observed.

One other point in Mr. Weldon's communication requires notice. He considers that the frequent occurrence of abnormalities and the wide range of variation in many species, show that "natural selection is in most cases an imperfect agent in the adjustment of organisms." This conclusion does not appear to me to be a logical one, since it ignores the admitted fact of the exceedingly intermittent character of selection and its constantly varied *locus* of action. Each species of animal is subject to a number of quite distinct dangers – hunger, cold, wet, disease, and varied enemies – and all these are separately intermittent in their action. Some affect the species at one time of the year only, some at another; but most of them only reach their maximum of intensity at long intervals – once or twice, perhaps, in a century. Whether cold winters or hot summers, excessive drought or excessive wet, deep snow or phenomenal hail or wind-storms, all are intermittent and occur with extreme severity only at long intervals. These intermittent waves of meteorological phenomena have their corresponding "waves of life," as Mr. [William H.] Hudson well terms them, such as phenomenal swarms of locusts or of wasps, of caterpillars, mice, or lemmings, and to a less conspicuous degree of almost every living thing. It follows, that during a succession of favourable seasons variation can go on almost unchecked, and even hurtful abnormalities and imperfections may survive for a few years, but soon there comes a check to the increase, and the most abnormal forms die out; while after a greater or less interval either adverse seasons or an increase of living enemies weed out all the extreme disadvantageous variations, leaving only the pick of the typical form to continue the race. This may occur again and again, each special period of stress affecting different organs or faculties – now abnormal colour, now deficient agility, now again incaution or a weak digestion – till in turn every departure from the best adapted mean form is eliminated, to again arise and again be extinguished as favourable or unfavourable conditions prevail. Thus, I am fully in agreement with Mr. Thiselton Dyer when he said: "I feel more and more that natural selection is a very hard taskmaster, and that it is down very sharply on structural details that cannot give an account of themselves." (*Nature*, vol. xxxix. p. 9.) The appearance of imperfect adjustment is thus only a temporary phenomenon, while that there is an underlying permanent adjustment is indicated by the long-continued identity of specific characters to which Mr. Weldon refers.

As it is very important to obtain some direct evidence of the action of natural selec-

tion, I wish to suggest a mode of doing so which might probably be successful. There is much evidence to show that the migrating birds which visit us in early summer are very largely old birds which have lived through two or more migrations; and, consequently, that of the large number of young birds which migrate in autumn for the first time a very small proportion return to our shores. If this is so, then the extreme severity of the selection during migration would afford us the opportunity of determining some of the physical characters which influence it, combined no doubt with mental characteristics which we have no means of gauging. I would suggest, therefore, that two or three common species of migrants should be chosen, of which the young birds of the year can be distinguished with certainty. Of these birds a number of observers should collect specimens just before their autumnal migration, and should carefully record the characters fixed upon in the case of the young and old birds separately. Probably the weight, the total length, and the length of the wing, would be sufficient, since heavy birds with comparatively short wings would hardly be adapted for a long-continued flight. By laying down the dimensions of some hundreds of specimens in curves of variation, whatever difference existed between the young and old birds would be easily detected; and this difference would presumably be the difference between the birth-mean and the survival-mean, so far as the selective influence of migration is concerned. In the following spring another set of specimens of the same species should be collected and measured; and we should then perhaps be able to determine the characters which had led to the selection of the young birds which had survived the double migration.

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On the Colour and Colour-patterns of Moths and Butterflies (S535)

Mimicry, instinct, and experiment are all featured subjects in this letter printed in the 29 April 1897 issue of Nature.

A paper by Mr. Alfred Goldsborough Mayer, on "The Colour and Colour-Patterns of Moths and Butterflies" (*Proceedings* of the Boston Society of Natural History, vol. xxvii. No. 14, pp. 243–330, March 1897), is a rather elaborate discussion of a subject which has lately attracted much attention; but though Mr. Mayer has made some interesting experiments and observations, his results are neither so novel nor so important as he claims them to be. One of the most interesting parts of the paper is the account of the development of wing-colours during the pupal state, a summary being given of previous researches, supplemented by a series of new observations on common species of American moths and butterflies. The result arrived at is, that the wings are at first transparent, then white, then drab or dusky yellow, while all the purer and brighter colours arise later on. This is what might be expected from the general distribution of colour in lepidopterous insects, and has been indicated by Dr. Dixie and other writers as probable.

Some ingenious experiments were made for the purpose of ascertaining whether the wing-scales were of any use in giving a greater hold on the air. The wings, with and without scales, were attached to a delicate pendulum, but no measurable difference in air-friction was found. Neither do the scales perceptibly strengthen the wings, hence it was

concluded that they have been developed solely as colour-producing organs of use to the various species.

A considerable space is devoted to the development of the colour-patterns of the Danaoid and Arcræoid Heliconidæ and the phenomena of mimicry. These are illustrated by four coloured plates intended to show the markings of a large number of species. These plates do not represent the insects themselves, but are "projected by Keeler's method" on rectangles of uniform size, which are supposed to afford more accurate means of comparison. This will seem to most naturalists to be a great mistake. It not only renders the patterns of the most familiar species almost unrecognisable, but it introduces many possibilities of error in the process of projection which even a comparison with the species represented may not enable us to detect. In the case of mimicking species it has the further disadvantage of obscuring differences of outline, and by irregular distortion giving undue prominence to what may be very slight differences in the actual species. In many mimicking species there is a wonderful similarity of general effect combined with considerable differences of detail, and by the process of "projection" these differences of detail may be exaggerated while the general similarity is obscured.

While accepting Fritz Müller's explanation of the mimicry of protected species by each other, and as also affording the only intelligible reason for there only being two types of colour-pattern in the whole 400 species of the Danaoid Heliconidæ, he says that "unfortunately no *direct* experiments have been made on the feeding habits of young South American birds." But in view of the careful experiments of Prof. Lloyd Morgan on a variety of young birds this is hardly necessary, as it is proved that they have in no case any instinctive knowledge of what is edible or distasteful, while they acquire the knowledge by experience with extreme rapidity. Like many other writers on the subject who have recently criticised and rejected the theory of warning colours as indicating inedibility, Mr. Mayer does not distinguish between the *habitual* and the only *occasional* enemies of protected insects. Thus he refers to the experiments of Beddard, showing that toads will eat any insects whatever; but it is quite certain that toads are not very dangerous enemies to either butterflies or their larvæ, nor probably are marmosets, which are also general feeders. There is quite sufficient evidence to show that insects with warning colours *are* rejected by most insectivorous birds and lizards, which are certainly the most general and most dangerous devourers of insects both in the larva and winged state, and these facts, taken in conjunction with the experiments of Prof. Lloyd Morgan, afford a firm foundation for the whole theory of warning colours and mimicry. – A. R. W.

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The Utility of Specific Characters (S558)

The idea that specific characters must have utilitarian value is a critical element of the Darwin–Wallace natural selection model. In the 12 January 1899 issue of Nature *Wallace continued to defend this point.*

In a recent issue of the *Journal of the Linnean Society* (Zoology, No. 172) there is a short paper by my friend Dr. St. George Mivart, in which he gives numerous cases of species of Lories peculiar to various Papuan or Pacific Islands, which differ in some details of

coloration from allied species in other islands, while they are usually altogether unlike the other birds inhabiting the same island. He then argues, as Captain Hutton had done with regard to similar phenomena among the fruit pigeons of the genus Ptilopus, that these various specific markings cannot be useful, and especially that they cannot be needed as "recognition-marks," because the whole coloration of the genus is so distinct that they cannot possibly be confounded with any other birds now inhabiting the same islands. He therefore concludes that these facts "are fatal to a utilitarian explanation of the origin of all specific characters." At the same time he accepts evolution and the natural biological origin of these and all other characters. These conclusions appear to me to be wholly illogical and to be reached by omitting to take account of the fundamental idea of organic evolution itself, namely, that each species has been, somehow, developed from an allied but distinct species, living or extinct. I therefore ask leave to point out how this omission affects the problem.

It is quite clear then that each distinct species of lory or fruit pigeon now found isolated from their allies in so many of the Pacific Islands *must* (if evolution is admitted) have originated by modification from some other parent species. The modification may have occurred in another island (or continent) or in the island in which the modified species now exists; but, in either case during the process of differentiation, recognition-marks would be of vital importance by checking intercrossing, so much so that it is doubtful whether in many cases the required structural or physiological modifications could be brought about without them. I do not remember that this proposition has been seriously denied, and it is the omission to take account of it that invalidates the argument of Dr. Mivart and Captain Hutton, founded upon the existing distribution of the species in question.

Perhaps these gentlemen will reply that they hold the views of Romanes and Gulick, that the specific differences in question are the direct result of the action of changed conditions on the progeny of the individuals which first reached the islands; but this theory is a pure assumption in support of which I am not aware that any adequate facts or observations have been adduced, while such changes in *all* the individuals exposed to the influence of the new conditions is entirely opposed to the known facts of variation. Supposing, however, that the existing species originated in the islands where they now occur by modification of some two or more original immigrants, let us consider *how* the change would be effected in accordance with the known facts of variation and natural selection.

The first thing that happens on the introduction of a new form into an island well-suited to it, and with no other enemies than those to which it is already adapted, is to increase rapidly till the island is fully stocked – witness the rabbit in Australia, New Zealand, and Porto Santo, the sparrow in America, and numerous other cases. But as soon as the island is fully stocked and all future increase dies off annually, natural selection begins its work, and the least adapted to survive, in every stage from the egg to the parent birds, get destroyed by some means or other. Now, if this process of elimination is identical in character with that to which the species was subjected in its former home no specific change will take place, because the whole structure and habits which constituted "adaptation to conditions" in its former habitat are equally effective in its new abode. But if there is any difference in the environment which requires a new adaptation, whether as regards food, seasons, diseases, or enemies of other kinds, then natural selection will certainly tend to bring about that new adaptation, and as in such a limited area local segregation will be ineffective, some external indication, marking off the new and better adapted from

the old less adapted type, will be of the first importance in the prevention of inter-crossing and thus hastening the process of complete adaptation; and these external indications are what I have termed "recognition-marks." When the new type is fully established and the old parent-form has died out, the work of these recognition-marks will have been done; but having been established by a severe process of selection they have become fixed and continue to form the "specific character" distinguishing the new from the old species. The repeated statement of Dr. Mivart, that in this or that case the peculiar marking cannot be a recognition-mark, or that such "recognition-marks" are quite needless, is therefore beside the question, since the very existence of the new species during the process of differentiation may have depended upon them.

I have here confined myself strictly to the one point raised by Dr. Mivart and Captain Hutton, having already dealt with the general question of "utility" elsewhere.

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Is the Swedish Red Water Lily a Variety of *Nymphæa alba*? (S568a)

A discussion printed in the 12 August 1899 issue of The Garden.

Having had the above Water Lilies growing side by side for five or six years, the differences appear to be so great and so constant that it is very difficult to believe the former to be a natural variety or sport from the other. The differences consist in the size and shape of the leaf, the size, shape, and number of the petals, and the colour of both leaves and flowers. These differences are as great as, if not greater than, those between most of the recognised species of the genus Nymphæa.

I will briefly enumerate these differences. The leaves of N. alba are somewhat longer than broad; those of N. rosea are broader than long. A full-sized leaf of each chosen at random measured as follows: N. alba, 10¼ inches long, 8¾ inches broad; N. rosea, 6 inches long, 6⅝ inches broad. Of course, both vary somewhat in proportions, but this difference seems fairly constant and gives a distinct character to the two groups of leaves. Accompanying this difference of proportion is a marked difference of outline, especially of the base of the leaf. In the white species the lobes usually overlap for more than half their length, and the lower termination of each lobe is a very slightly pointed oval. In the red form the lobes open at a very wide angle, and the termination is an oblique unequal-sided blunt point. Out of more than 100 leaves on my red Lily I can see none that approach the overlapping form of the white species, of which it is a specific character, Babington indicating it by the words "notch in the leaves with parallel sides." A Siberian species, N. nitida, is described in Hemsley's "Hardy Plants" as having "deeply lobed leaves with spreading lobes," which exactly describes those of the red-flowered form. Equally distinct is the colouring of the two leaves, which differ as follows: N. alba, above, clear bright green, below, yellowish green; N. rosea, above, an olive-green, very distinct; below, a full dull red, faintly tinged with green.

Coming to the flowers, the differences are equally great. The botanical books give from sixteen to twenty-four as the number of petals in N. alba, but in N. rosea they are much more numerous – usually about forty. They are also broader and blunter; hence the flowers of N. rosea are smaller, with a more regular rosette of petals, often as perfect and

compact as in a well-formed double Dahlia. Now it appears to me that these various and well-marked differences in form, size, proportions, and coloration of the leaves, as well as in the size, shape, and number of the petals and the coloration of the complete flower – differences which in their entirety are as great as between any two species of the genus inhabiting the same continent – do in themselves constitute *primâ facie* evidence that the two forms are distinct species. Of course such evidence must give way if there is direct proof that the one form is produced from the other by seed *without the possibility of any crossing having taken place.* The words in italics indicate the essential point needed to constitute such a proof, and I send these notes in the hope of eliciting from some of your readers what is the nature of the evidence that has led botanists, apparently without any hesitation, to class two such very different plants as belonging to the same species.

I may add that I am now growing and flowering in a small tank out of doors, but warmed by a lamp, two blue Water Lilies – Nymphæa scutata, from Port Elizabeth, and what appears to be Nymphæa stellata, the roots of which were obtained from Diep River, about ten miles from Capetown, where they were probably introduced, though now apparently growing wild. – Alfred R. Wallace, Dorset.

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Jenner and the Cuckoo (S578)

This interesting letter appeared in the 2 July 1900 issue of The Vaccination Inquirer and Health Review. To anti-vaccinationists like Wallace, William Jenner was a villain, yet in this instance, in which Wallace felt he had been done an injustice, he came to his defense.

Dr Montague R. Leverson, of Fort Hamilton, New York, having written me about the facts as to the young Cuckoo throwing the other young birds out of the nest of its foster-parents, which account he, like so many other persons, wholly disbelieves, I have recently obtained for him Mrs Blackburn's book, "Birds of Moidart," (1895) in which she describes her careful and repeated observation of the fact, and gives a drawing of the bird in the very act of performing the operation. The same description was first published in *Nature*, vol. v., p. 383, signed J. B., but referring to a versified tale, "The Pipits," illustrated by Mrs Hugh Blackburn, (1872, Maclehose, Glasgow). The same letter was printed in *The Lancet* in 1892 (July 2nd).

As the observation now made is most precise and direct, and as it curiously agrees in *details* with the observations of Jenner in his letter to John Hunter, printed in the *Philosophical Transactions* (vol. lvii-viii, pp. 225, 226) it is, I think, due to themselves, and to our cause, that those writers who have adduced Jenner's statements on this point as a proof of his unreliability should acknowledge their error, in order that this accusation, unsupported by observation or by any well established facts, should not continue to be brought forward as an argument by anti-vaccinators.

This is the more important as the facts had already been several times confirmed by independent testimony, so as to satisfy some of our most careful and accurate naturalists. In the 4th edition of *Yarrell's British Birds*, edited by Prof. Alfred Newton, he says, after quoting Jenner's account, – "This remarkable habit of the young Cuckoo has been so

abundantly confirmed by the testimony of unimpeachable eye-witnesses in many countries, and in England among others by Montague and Mr Blackwall, whose names are a sufficient guarantee for the accuracy of their observations, that the unbelief in Jenner's statements, hinted or openly expressed by some zoologists, is hardly to be justified by the most ardent supporter of absolute proof." (vol. ii., p. 396.)

Prof. Newton also tells us that a French writer, Lottinger, in 1782, "himself had personal proof of the expulsion of an egg from the nest by a young Cuckow, (Hist. du Coucou d'Europe, p. 18.)" And in the *English Cyclopædia* (Natural History), vol. ii., p. 246, there is an account of an observation earlier than that of Jenner giving almost exactly the same facts.

And all we have against these repeated and concordant observations is – not observation to the contrary, but more or less positive denial, disbelief, or mere ridicule. Among these unbelievers Waterton has been quoted, as if his opinion should outweigh other observers' facts. But there was probably no more prejudiced or irrational writer in the English language when dealing with the observations of others. To give two examples; he denied the *possibility* of the Dipper walking under water, and tried to prove it by reasoning and ridicule. Yet no *fact* in nature is more certain or more universally admitted by ornithologists. In the same way he tried to prove that Vultures found their food by smell and not by sight, ridiculing the direct observations and experiments of Audubon and others which were opposed to his views. Yet here again the unanimous verdict of naturalists is against him, and, as regards the very same species which he observed in Guiana, I, myself, proved that it does not detect food by smell. (See my *Travels on the Amazon*, cheap edition, p. 125).

I should have written to make this correction and appeal in favour of Jenner long ago, but, till recently, I had no knowledge of Mrs Blackburn's work, and could give no reference to it. Having now read her account and examined her drawing, I need only say, in conclusion, that I am completely satisfied of the accuracy of Jenner's observation thus fully and repeatedly confirmed. – Alfred R. Wallace, *Parkstone, Dorset.*

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Are Plant Diseases Hereditary? (S596)

A discussion printed in The Garden issue of 17 May 1902.

"The hereditary transference of diseases to succeeding generations is unknown in the vegetable world. The seed of plants afflicted with all possible sorts of diseases may be utilised without the slightest concern for the formation of new crops." – Professor Hartig, quoted by Nisbet in "Studies in Forestry," page 172.

The above quoted passage will seem to many readers very extraordinary if not positively erroneous. They will say: "Many diseases of man and animals are hereditary; why should not the same rule apply in plants?" And, indeed, the statement of Professor Hartig being expressly limited to the vegetable world would seem to acknowledge that it does apply in the case of animals. But although the belief that it does so apply has been till very recently almost universally held by biologists, of late years great doubt has been thrown upon the fact, due mainly to the researches of Galton and Weisman, leading to the

belief that "acquired characters" are not transmissible to offspring, and that diseases are certainly in most cases acquired by the parent, not born with him.

In a very interesting and original work, "The Present Evolution of Man," by Dr. G. Archdall Reid, this subject is very fully discussed, and it is shown that, with very few exceptions, there is no proof whatever of the inheritance of disease in man, but only of the inheritance of a *tendency* to the special disease of the parent, so that under similar unhealthy conditions of life or of exposure to infection, the child is likely to contract the same disease, which will thus appear to be hereditary without being really so. This is clearly the case with gout and consumption, which have both been held to be hereditary, but in no case has an infant been born suffering from these diseases.

The only diseases which appear to be really transmitted are those in which a mother suffers from one of the zymotic diseases previous to the birth of her child, and the disease germs through her blood obtain access to the blood of her unborn offspring. Thus children are sometimes born apparently suffering from syphilis and even from small-pox, when the mother is, or has recently been, actually suffering from those diseases; but Dr. Reid urges that this cannot be held to prove actual heredity of the disease, but merely that the otherwise healthy child has been infected through the mother before birth, just as it might be after birth through the milk of a wet-nurse suffering from the same disease. In this latter case no one could possibly say that the infection proved the hereditary transmission of the disease, but only an infection as purely extraneous as if the poor child had been inoculated with it, or had been in close contact with another child suffering from it. It seems therefore highly probable that the statement made by Professor Hartig as regards plants is really true as regards the higher animals also; but there is a special reason why it should apply more rigidly in the case of plants which it may be as well to mention. It is very doubtful whether any of the diseases to which domesticated animals are so subject really exist among fully adult animals in a state of nature – that is, in regions where they are not in contact with domesticated animals of their own species, or where their natural conditions of life have not been injuriously modified by human agency. The cause of this immunity is the severity of the action of natural selection or the "survival of the fittest," which in this special case may be best expressed as "the extinction of the unfittest." If we consider that a wild animal can only maintain its existence day by day through being able both to obtain food and to escape from its enemies, and that any serious illness would certainly endanger its existence by rendering it unable to do either, we see that all liability to disease has been so constantly eliminated generation after generation during the whole course of the development of the species, that almost perfect health under the normal conditions of existence has long since been attained. But however rigid this selecting process is in the case of the animals, it is much more rigid in the case of most plants, because the actual or potential rate of increase is so much greater. However numerous may be the offspring of the higher animal, those of plants are far more numerous, often in the proportion of a hundred to one. Every year millions of seeds germinate which never grow into mature plants, and as the slightest tendency to disease or constitutional weakness in any seedling would certainly give that individual a special cause of extinction in addition to the general causes which affect those which are healthy, it follows that all tendency to injurious disease would be eradicated among plants even more early and more completely than in the case of animals.

Although I have no acquaintance with the detailed facts on which the statement at the

head of this article was founded, I am disposed to accept it as an accurate one from its accordance with the general principles of evolution and the now generally accepted laws of heredity.

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Genius and the Struggle for Existence (S600)

Comments printed in the 29 January 1903 issue of Nature.

Will you allow me to supplement the excellent reply of Sir Oliver Lodge to your correspondent Mr. G. W. Bulman by a few remarks dealing more specifically with that gentleman's difficulty, which is one very widely felt, but is, I believe, founded on a misconception?

The words "useful" and "advantage" have two distinct meanings, the one referring to material the other to intellectual and moral results; and it is in the former sense only that they can be properly used in relation to natural selection or survival of the fittest. In that relation, physical results only are of value – those that tend to the preservation of life on occasions of stress and danger. In deciding whether any quality, physical or mental, is of *value* in this sense, Lloyd Morgan's admirable test should be applied – "Is it of survival-value?" If not, then it is *not* useful in the struggle for existence either to the individual or the race, unless it happens to be combined with other qualities which are, in an exceptional degree, of survival value. Now genius in all its varying manifestations is a quality which has hardly any relation to survival except an adverse one, and only in exceptional cases is of any *material* advantage to the race. The genius of the poet, of the writer, of the artist, even of the inventor, only occasionally benefits the race in its material struggle with other races, while it very rarely gives long life and an ample progeny to the possessor. Its *use* to him is solely the enjoyment of the exercise of his faculty of creating. Too frequently it is of no material use whatever to him, and he dies in poverty and neglect. The two races that have exhibited the highest manifestations of genius were the ancient Greeks and the Jews. But this genius did not advantage their respective races in the struggle for existence. Both of them became permanently subject races, and that they have survived at all is not due to their genius, but to their exceptionally fine physical qualities, their courage and their endurance.

As a matter of fact, the law of the survival of the fittest has almost entirely ceased to apply to civilised man, and the more civilised he is the less it applies. I have already shown (in the chapter on "Human Selection" in my "Studies"), how, under a higher civilisation and a truer social system, it will be superseded by another law, which may be termed "the perpetuation of the fittest," and which will operate as automatically and as beneficially in improving the human race as natural selection has acted in improving the lower animals. At present, as Darwin himself fully recognised, it is not the best or the highest that survive, but a comparatively low type morally and intellectually, though in relation to our present very imperfect civilisation they may be held to be the fittest. It is, however, fitness to "succeed in life," as it is termed, not necessarily to survive; and this is indicated by the comparatively short lives of millionaires and of the inhabitants of cities,

who are continually replaced by the sons of the less successful but more virile inhabitants of the rural districts.

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The Future Problems and Aims of Ornithology (S620a)

Wallace provided this short reply to a letter of enquiry on this subject sent out by the Editor of the journal The Condor; it was printed in its issue of May–June 1905.

Dear Sir:

The chief department of Zoology that I take much interest in now, is the carrying out of experimental observations on the various alleged *instincts* of the higher animals (as the alleged instinct of direction) and also of experiments to prove or disprove the alleged *heredity* of acquired *characters*, and similar problems. With such a large endowment as the Leland Stanford University has, I wonder some experimental farm for these purposes has not been founded. Almost every other department of biology seems now to be overdone – except also the accurate observation of animal life in *the tropics*, for the purpose of detecting the *utility* of all the *special* characters of the various groups of land animals.

I trust these hints may induce some students with independent means to take up some of these studies. – Yours very truly, Alfred R. Wallace. Broadstone, Wimborne, England**.**

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Fertilisation of Flowers by Insects (S633)

This letter, a response to an enquiry, was printed in the 31 January 1907 issue of Nature. A Mr. Swanton had asked whether the prediction made by both Darwin and Wallace that a hawk-moth with a very long proboscis would be discovered that would account for the surprisingly long nectary found in a species of Madagascan orchid had ever borne out (it eventually was).

In reply to Mr. Swanton's letter, I have not heard of any moth from Madagascar with an exceptionally long proboscis. I think, however, I did hear of one from East Africa with a proboscis nearly the length required; but as entomologists do not usually open out and measure the length of proboscis of all the large Sphingidæ they receive, some of the required length may exist unnoticed in our public or private collections. An inquiry at the insect departments of the Natural History Museum, and also of that of the Jardin des Plantes, would perhaps afford Mr. Swanton the required information. – Alfred R. Wallace.

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The First Paper on Natural Selection (S657)

A letter to the Editor of The Times (London) printed in its 3 July 1908 issue.

Sir, – My attention has been called to the article under the above heading in your issue of Tuesday last, in which I find a misconception or oversight as regards the argument I put forth 50 years ago, which I ask your permission to correct.

After describing (in his fifth paragraph) my views as to the survival of favourable variations (but not in my own words), the writer adds this criticism: – "There is a logical flaw here resulting from the use of such indefinite words as 'stronger' and 'inferior' instead of the phrase 'adapted to the environment.'" But the word "stronger" (printed with quotation marks) I cannot find once used in my paper in the sense implied, and the word "inferior" only once, and then immediately after a reference to the greater or less power of survival under adverse conditions.

But even if they had been more frequently used, they should be always interpreted by reference to a preceding section headed "The Abundance or Rarity of a Species dependent upon its more or less perfect Adaptation to the Conditions of Existence," an idea which forms the keynote of my whole paper, and recurs in such phrases as "tending to increase the facilities for preserving existence," "the most capable of supporting existence," both being used in the very paragraph from which the word "inferior" is quoted.

As what Professor Lloyd Morgan has tersely named "survival value" is the idea pervading my whole original article, and it is the first time the charge of being "illogical" has been made against it – and that in a journal which will spread the charge over the whole civilized world – I may be excused for the wish to show that this charge is not justified. – Yours, Alfred R. Wallace.

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Section 3. Anthropology/Human Evolution

Introduction

It is not uncommon to see the words "the anthropologist Alfred Russel Wallace…", despite the fact that Wallace is best known as an evolutionist and biogeographer. This is only fair, actually, as in Wallace's world the nature and fate of the human race held a special place. His mind was flexible enough to embrace digressions into biology, physical geography and geology, and economics, but at all times it was not long before he found his way back to human evolution, broadly characterized.

Wallace's anthropological writings began in his earliest adult years and continued on through to his death in 1913. Around 1843 he wrote an unpublished essay titled "The South-Wales Farmer" which some might view as an ethnological study; it anticipates many of his later publications, relaying observations on land use, cultural habits, character, religion and language. A few years later he began to consider the notion of human evolution. This was a complex question, and he wasn't sure how to reconcile our animal characteristics with our higher attributes: morality, mathematics, etc. Even by 1858 and the Ternate essay on natural selection he was unsure how to approach the subject, as this milestone work contains no comments even on "human animal" evolution, much less on the origins and modification of higher consciousness. The solution would only become apparent when he was introduced to spiritualism.

Meanwhile, and thereafter, Wallace was pursuing many more immediate questions concerning anthropological subjects, including the origins of civilization, the evolution of morality, various ethnographic matters, and the evolution of language. The selections presented here feature some very nice writing on these subjects, back in a time when lively discussions on them did not have the benefit of as much empirical evidence as might have been wished for.

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How to Civilize Savages (S113)

This long, essay-like commentary appeared in The Reader issue of 17 June 1865. It was written at an important point in Wallace's intellectual evolution: just before his investigation of spiritualism, while he was contemplating ways to effect a "general improvement of savage races," as he muses here. The interesting question, however, is whether he is really talking about possible means of improving all races.

Do our missionaries really produce on savages an effect proportionate to the time, money, and energy expended? Are the dogmas of our Church adapted to people in every

degree of barbarism, and in all stages of mental development? Does the fact of a particular form of religion taking root, and maintaining itself among a people, depend in any way upon race – upon those deep-seated mental and moral peculiarities which distinguish the European or Aryan races from the negro or the Australian savage? Can the savage be mentally, morally, and physically improved, without the inculcation of the tenets of a dogmatic theology? These are a few of the interesting questions that were discussed, however imperfectly, at a meeting of the Anthropological Society in 1865, when the Bishop of Natal read his paper, "On the Efforts of Missionaries among Savages;" and on some of these questions we propose to make a few observations.

If the history of mankind teaches us one thing more clearly than another, it is this – that all true civilizations and all great religions are alike the slow growth of ages, and both are inextricably connected with the struggles and development of the human mind. They have ever in their infancy been watered with tears and blood – they have had to suffer the rude prunings of wars and persecutions – they have withstood the wintry blasts of anarchy, of despotism, and of neglect – they have been able to survive all the vicissitudes of human affairs, and have proved their suitability to their age and country by successfully resisting every attack, and by flourishing under the most unfavourable conditions.

A form of religion which is to maintain itself and to be useful to a people, must be especially adapted to their mental constitution, and must respond in an intelligible manner to the better sentiments and the higher capacities of their nature. It would, therefore, almost appear self-evident that those special forms of faith and doctrine which have been slowly elaborated by eighteen centuries of struggle and of mental growth, and by the action and reaction of the varied nationalities of Europe on each other, cannot be exactly adapted to the wants and capacities of every savage race alike. Our form of Christianity, wherever it has maintained itself, has done so by being in harmony with the spirit of the age, and by its adaptability to the mental and moral wants of the people among whom it has taken root. As Macaulay justly observed in the first chapter of his history: "It is a most significant circumstance that no large society of which the tongue is not Teutonic has ever turned Protestant, and that, wherever a language derived from that of ancient Rome is spoken, the religion of modern Rome to this day prevails."

In the early Christian Church, the many uncanonical gospels that were written, and the countless heresies that arose, were but the necessary results of the process of adaptation of the Christian religion to the wants and capacities of many and various peoples. This was an essential feature in the growth of Christianity. This shows that it took root in the hearts and feelings of men, and became a part of their very nature. Thenceforth it grew with their growth, and became the expression of their deepest feelings and of their highest aspirations; and required no external aid from a superior race to keep it from dying out. It was remarked by one of the speakers at the Anthropological Society's meeting, that the absence of this modifying and assimilating power among modern converts – of this absorption of the new religion into their own nature – of this colouring given by the national mind – is a bad sign for the ultimate success of our form of Christianity among savages. When once a mission has been established, a fair number of converts made, and the first generation of children educated, the missionary's work should properly have ceased. A native church, with native teachers, should by that time have been established, and should be left to work out its own national form of Christianity. In many places we have now had missions for more than the period of one generation. Have any self-supporting, free, and national Christian churches arisen among savages? If not – if the

new religion can only be kept alive by fresh relays of priests sent from a far distant land – priests educated and paid by foreigners, and who are, and ever must be, widely separated from their flocks in mind and character – is it not the strongest proof of the failure of the missionary scheme? Are these new Christians to be for ever kept in tutelage, and to be for ever taught the peculiar doctrines which have, perhaps, just become fashionable among us? Are they never to become men, and to form their own opinions, and develop their own minds, under national and local influences? If, as we hold, Christianity is good for all races and for all nations alike, it is thus alone that its goodness can be tested; and they who fear the results of such a test can have but small confidence in the doctrines they preach.

But we are told to look at the results of missions. We are told that the converted savages are wiser, better, and happier than they were before – that they have improved in morality and advanced in civilization – and that such results can only be shown where missionaries have been at work. No doubt, a great deal of this is true; but certain laymen and philosophers believe that a considerable portion of this effect is due to the example and precept of civilized and educated men – the example of decency, cleanliness, and comfort set by them – their teaching of the arts and customs of civilization, and the natural influence of the superiority of race. And it may fairly be doubted whether most of those advantages might not be given to savages without the accompanying inculcation of particular religious tenets. True, the experiment has not been fairly tried, and the missionaries have almost all the facts to appeal to on their own side; for it is undoubtedly the case that the wide sympathy and self-denying charity which gives up so much to benefit the savage, is almost always accompanied and often strengthened by strong religious convictions. Yet there are not wanting facts to show that much may be done without the influence of religion. It cannot be doubted, for example, that the Roman occupation laid the foundation of civilization in Britain, and produced a considerable amelioration in the condition and habits of the people, which was not in any way due to religious teaching. The Turkish and Egyptian Governments have been, in modern times, much improved, and the condition of their people ameliorated, by the influence of Western civilization, unaccompanied by any change in the national religion. In Java, where the natives are Mohammedans, and scarcely a Christian convert exists, the good order established by the Dutch Government and their pure administration of justice, together with the example of civilized Europeans widely scattered over the country, have greatly improved the physical and moral condition of the people. In all these cases, however, the personal influence of kindly, moral, and intelligent men, devoted wholly to the work of civilization, has been wanting; and this form of influence, in the case of missionaries, is very great. A missionary who is really earnest, and has the art (and the heart) to gain the affections of his flock, may do much in eradicating barbarous customs, and in raising the standard of morality and happiness. But he may do all this quite independently of any form of sectarian theological teaching, and it is a mistake too often made to impute all to the particular doctrines inculcated, and little or nothing to the other influences we have mentioned. We believe that the purest morality, the most perfect justice, the highest civilization, and the qualities that tend to render men good, and wise, and happy, may be inculcated quite independently of fixed forms or dogmas, and perhaps even better for the want of them. The savage may be certainly made amenable to the influence of the affections, and will probably submit the more readily to the teaching of one who does not, at the very outset, attack his rude superstitions. These will assuredly die out of themselves, when knowledge

and morality and civilization have gained some influence over him; and he will then be in a condition to receive and assimilate whatever there is of goodness and truth in the religion of his teacher.

Unfortunately, the practices of European settlers are too often so diametrically opposed to the precepts of Christianity, and so deficient in humanity, justice, and charity, that the poor savage must be sorely puzzled to understand why this new faith, which is to do him so much good, should have had so little effect on his teacher's own countrymen. The white men in our Colonies are too frequently the true savages, and require to be taught and Christianized quite as much as the natives. We have heard, on good authority, that in Australia a man has been known to prove the goodness of a rifle he wanted to sell, by shooting a child from the back of a native woman who was passing at some distance; while another, when the policy of shooting all natives who came near a station was discussed, advocated his own plan of putting poisoned food in their way, as much less troublesome and more effectual. Incredible though such things seem, we can believe that they not unfrequently occur wherever the European comes in contact with the savage man, for human nature changes little with times and places; and I have myself heard a Brazilian friar boast, with much complacency, of having saved the Government the expense of a war with a hostile tribe of Indians, by the simple expedient of placing in their way clothing infected with the smallpox, which disease soon nearly exterminated them. Facts, perhaps less horrible, but equally indicative of lawlessness and inhumanity, may be heard of in all our Colonies; and recent events in Japan and in New Zealand show a determination to pursue our own ends, with very little regard for the rights, or desire for the improvement, of the natives. The savage may well wonder at our inconsistency in pressing upon him a religion which has so signally failed to improve our own moral character, as he too acutely feels in the treatment he receives from Christians. It seems desirable, therefore, that our Missionary Societies should endeavour to exhibit to their proposed converts some more favourable specimens of the effect of their teaching. It might be well to devote a portion of the funds of such societies to the establishment of model communities, adapted to show the benefits of the civilization we wish to introduce, and to serve as a visible illustration of the effects of Christianity on its professors. The general practice of Christian virtues by the Europeans around them would, we feel assured, be a most powerful instrument for the general improvement of savage races, and is, perhaps, the only mode of teaching that would produce a real and lasting effect.

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Mr. Wallace on Natural Selection Applied to Anthropology (S125)

A contribution to the Anthropological Review of January 1867. In this letter Wallace discusses some issues connected with his classic paper "The Origin of Human Races and the Antiquity of Man Deduced from the Theory of 'Natural Selection'," delivered nearly three years earlier at a meeting of the Anthropological Society of London.

Sir, – In the last number of your periodical, Dr. Hunt's[a] paper "On the Application of the Principle of Natural Selection to Anthropology," which was read at Nottingham, is printed at length. I beg, therefore, a little of your space to reply to the charge of being entirely "illogical," in the application which I have elsewhere made of the principle of natural selection to the question of the origin of man.

Dr. Hunt selects from my paper "On the Origin of Human Races, etc., deduced from the Theory of Natural Section," published in the *Review* of May, 1864, two statements of opinion which are not, perhaps, very important parts of that paper, and maintains that they are not fairly deducible from Mr. Darwin's principles. The first is, "Man may have been, indeed I believe must have been, once a homogeneous race." The second is that, "Man may and probably will in the future again become a single homogeneous race." I am only now concerned to show, that admitting the application of "Natural Selection" to man, these are fair and logical inferences.

In Chapter IV. of *The Origin of Species*, Mr. Darwin maintains, that just as all varieties of a species are descended from one homogeneous species, so all the species of a genus are descended from one parent species, and all the genera of a family from one parent genus; and generally any group of animals, whether large or small, has descended from a more or less remote single species. It matters not, therefore, whether man be a species with many varieties, or a genus with many species, in either case he has, on Mr. Darwin's principles, descended from *one species*, and if that one species was sub-divided into varieties, then by going a little further back we arrive at their common ancestor in a single homogeneous species, or one in which there are no well marked and permanent varieties. Such species among animals, when we now find them, are almost always confined to a limited area, cosmopolitan species being as a rule variable. I think, therefore, that when I state that I believe man was once "a single homogeneous race," I am merely stating a truism to those who admit the application to him of the principle of "natural selection."

It is, however, my second statement, that man may in the future again become a single homogeneous race, that seems so paradoxical to Dr. Hunt; and no doubt it is very like blowing hot and cold with the same mouth to make "natural selection" answerable for such opposite results. But the whole scope and purport of my paper was to show, that since that early period at which the ancestors of mankind formed a single homogeneous race, all the very distinct forms now existing (and perhaps others now extinct) were produced by "natural selection," till the process was checked by the development of the *mind* of man, causing changes of his external form to be less important than advances in his intellectual and moral nature. Had this check not occurred it seems to me probable that the world would now be inhabited by many quite distinct *species*, and, perhaps, even distinct *genera* of the animal *man*. As it is, the forms of man on the earth are steadily decreasing in number, owing to the more rapid mental, moral, and physical development of a few superior races. It is a bare fact that man *is* becoming more homogeneous. The most extreme forms, the native American, the New Zealander, the Australian, and the Polynesian races, are all doomed. It is a mere question of time as to when these will become extinct. And when I see how Europeans have spread over tropical South America, when I contemplate the rapid increase of that energetic race which in three centuries has changed the vast continent of North America from a waste of forest to a hive of

[a] James Hunt (1833–1869), English speech therapist and anthropologist.

industry and high civilisation, when I think of the possible advances of science in making the forces of nature subserve the wants and supplement the energies of this dominant race, I cannot believe that the resistance of lower races and lower civilisations will permanently avail them, or that climatal influences will for ever prevent the tropics from being the home of the civilised man armed with ever-increasing insight into nature and nature's laws.

This is the "struggle for existence" on the grandest scale; and I believe the next few centuries will see it go on at such a rate that even the great races hitherto dominant in their own areas – the Negroes, the Hindoos, and the Mongols – will begin to suffer from it. I cannot believe that the progress of civilisation and science will stop, and I can see no end to such progress, but the absorption and displacement of lower races by higher, till the world again become inhabited by a "homogeneous race," whose command over nature and whose powers of intercommunication will be such as to prevent local conditions affecting, to more than a very slight degree, its external characteristics. The globe is or very soon will be to civilised man, an area over which he can roam at will, with as much ease as any animal over the single island or continental area that it inhabits; and it will then become impossible for an inferior race long to maintain itself against him. If then we admit that the mental are more important than the more corporeal forces in the great human "struggle for existence," it is a necessary result that the higher will continue to displace or absorb the lower races of man. It is for those who deny this to show why and when this process will cease.

I believe that I have now shown that the principles of Mr. Darwin's *Origin of Species*, if applied to man with such modifications as are required by the great development and vast importance of his intellectual and moral rather than his mere animal nature, leads to the apparently paradoxical result that he is tending to become again as his progenitors once undoubtedly must have been, "a single homogeneous race."

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The Origin of Civilization (S152)

A third-person summary of remarks Wallace made following the presentation of a paper by Sir John Lubbock at the annual British Association for the Advancement of Science meetings was printed in the October 1869 issue of Anthropological Review. Subsequent comments on these remarks in The Spectator caused Wallace to send this letter to its Editor; it appeared in the 11 September 1869 issue of that periodical.

Sir, – In your issue of August 28th you do me the honour to notice some remarks which I offered on Sir John Lubbock's paper at the recent meeting of the British Association; but, owing to the imperfect manner in which the proceedings were reported, you have entirely misunderstood what I really said. I beg, therefore, that you will allow me to state what are the opinions I hold on this point, and which I then endeavoured to express.

You represent me as saying: – "Suppose that a European colony were entirely isolated from their race, then I believe that there is almost a moral certainty that in the course of centuries they would suffer a considerable amount of degradation, and hardly be recog-

nized as the descendants of a civilized people;" and you then go on to argue (and, I think, very justly) that under such circumstances progress is, at least, as probable as degeneration. But the supposition which I made was a different one. It was the isolation of a *very small* European community in a country *very ill-adapted* for civilization and progress, – a country, like Australia, with no indigenous animals capable of domestication, and without cereals, or roots, or fruits adapted for cultivation, – a country without native iron, and with such an unpropitious climate as to necessitate frequent migrations and a perpetual struggle to support life. Under such conditions I maintained that degradation to comparative savagery would be inevitable, just as under analogous circumstances would be the reversion of cultivated plants or domestic animals to a state approaching that of their wild allies. I argued, therefore, for degradation under extremely unfavourable conditions, not as the result of mere isolation; and as, during the long period that man has existed upon the earth, such unfavourable conditions must frequently have occurred, it appears to me more philosophical to admit that some of the lower races may owe their present state of barbarism to a partial degradation, than to maintain that they necessarily represent an original low condition, above which they can at no time have arisen.

Again, you quote me as having found among savages "a most delicate sense of right and wrong," and as deducing from this fact a theory – "that they are degenerate persons, who have retained amidst their degeneracy a primeval idea of morals." Allow me to say that I neither expressed nor do I hold any such theory. My object was simply to show that, treating the question as a scientific one, to be determined solely by facts, and not by feelings, there is really no such clear evidence of progress in morals as there is of progress in intellect. Children, modern savages, and prehistoric man alike exhibit deficiency of intellectual power, but we do not find an equally constant deficiency in moral feeling. Intellect, no doubt, reacts upon morals by determining the more remote effects of our actions, and by logically extending the sphere of our sympathies; but a moral sense certainly exists in savages, which, within a limited sphere of action, seems as powerful an incentive to regulate conduct as it is among the most civilized races. Morality is an essential part of man's nature, which can only be fully developed by that true civilization towards which we have as yet hardly taken the first steps. The great mass of the people in civilized countries derive benefit from modern science and its marvellous practical applications, just in the same way as do the savages who receive the products of Manchester looms and Birmingham workshops. Owing to their geographical position, the former derive rather more benefit, but as to knowing and understanding anything of this wonderful "science," the creator of the civilization which surrounds them, they are as absolutely ignorant as the Malay or the negro. Exposed as they are to the enormously increased temptations to vice with which civilization surrounds them, how can we wonder if their moral nature often remains as imperfect and undeveloped as it does in savages?

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The Origin of Moral Intuitions (S153)

A letter on the origins of morality, printed in the Scientific Opinion issue of 15 September 1869.

Sir, – I do not think your correspondent "S. L." is justified in speaking so contemptuously of Mr. Hutton's original and well-reasoned article "A Questionable Parentage for Morals;" and I am inclined to think that Mr. Herbert Spencer himself would not claim for his views on this subject that demonstrable certainty which "S. L." seems to think attaches to them. The question really depends upon the more fundamental one, of whether man's entire mental and moral nature is the product and outcome of that material organization whose laws of growth and development Mr. Spencer has so well elucidated. If mind with all its powers is simply a function of organized matter, then Mr. Spencer's theory of the origin of morals is the only one which can be held by a student of science. If, however, there is any thing in man more than his physical organization, then it becomes a subject of strict scientific and philosophical inquiry to determine from a study of the phenomena of his mind in various stages of growth and under various conditions, what is the mental substratum required to account for the development of the faculties we actually find in him. Mr. Spencer maintains that an appreciation of utilities is all that is required to develop the moral sense; Mr. Hutton argues that this is insufficient, and that the moral sense itself, the appreciation of right and wrong, with a mental impulse towards the first and away from the second, is an essential part of the mental substratum of our nature.

To go into the question at all fully would be quite out of place here; I will therefore only adduce one group of facts which seem to me inexplicable on the utilitarian hypothesis. The utilitarian sanction for truthfulness is by no means very powerful or universal. Few laws enforce it. No very severe reprobation follows untruthfulness. In all ages and countries falsehood has been held permissible in love, and laudable in war; while, even to the present day, it is held venial by the majority of mankind in trade and commerce. A certain amount of untruthfulness is a necessary part of politeness in the east and west alike, while even severe moralists have held a lie justifiable to elude an enemy or prevent a crime. Such being the difficulties with which this virtue has had to struggle, with so many exceptions to its practice, with so many instances in which it brought ruin or death to its too ardent devotee, how can we believe that considerations of utility could ever invest it with the mysterious sanctity of the highest virtue, – could ever induce men to value truth for its own sake and practice it regardless of consequences?

Yet it is a fact that such a mystical sense of wrong does attach to untruthfulness, not only among the higher classes of civilized people, but among whole tribes of utter savages. Sir Walter Elliott tells us (in his paper "On the Characteristics of the Population of Central and Southern India," published in the *Journal* of the Ethnological Society of London, vol. I., p. 107) that the Kurubars and Santals, barbarous hill-tribes of Central India, are noted for veracity. It is a common saying that "a Kurubar *always* speaks the truth;" and Major Jervis says, "the Santals are the most truthful men I ever met with." As a remarkable instance of this quality the following fact is given. A number of prisoners, taken during the insurrection, were allowed to go free on parole, to work at a certain spot for wages. After some time cholera attacked them and they were obliged to leave, but every man of them returned and gave up his earnings to the guard. Two hundred savages with money in their girdles, walked thirty miles back to prison rather than break their word! My own experience among savages has furnished me with similar, although less severely tested, instances, and we cannot avoid asking, how is it that in these few cases "experiences of utility" have left such an overwhelming impression, while in so many others they have left none? The experiences of savage men as regards the utility of truth must, in the long run, be pretty nearly equal. How is it then that in some cases the result is

a sanctity which overrides all considerations of personal advantage, while in others there is hardly a rudiment of such a feeling?

The intuitional theory explains this by the supposition that there is a feeling – a sense of right and wrong – in our nature antecedent to and independent of experiences of utility. Where free play is allowed to the relations between man and man, this feeling attaches itself to those acts of universal utility or self-sacrifice which are the products of our affections and sympathies, and which we term moral, while it may be, and often is, perverted to give the same sanction to acts of narrow and conventional utility which are really immoral, – as when the Hindoo will tell a lie but will sooner starve than eat unclean food, and looks upon the marriage of adult females as gross immorality.

The strength of the moral feeling will depend upon individual or racial constitution, – the acts to which its sanctions are applied will depend upon how far the simple feelings and affections of our nature have been modified by custom, by law, or by religion.

The question to be considered is, first, whether such an intense and mystical feeling of right and wrong (so intense as to overcome all ideas of personal advantage or utility) could have been developed out of accumulated ancestral experiences of utility; and, in the second place, whether feelings so developed by one set of utilities, could be transferred to acts of which the utility was partial, imaginary, or altogether absent.

Although myself an enthusiastic admirer of Mr. Spencer's writings, and a follower of his philosophy, I am decidedly of opinion that there is a limit to the sphere which that philosophy embraces, and that the limit is to be found in the doctrine of the origin of morals.

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Man and Natural Selection (S173)

In this long response printed in the 3 November 1870 issue of Nature, Wallace reacts to criticism of his Contributions to the Theory of Natural Selection.

The following reply to M. Claparède's "Remarques à propos de l'ouvrage de M. Alfred Russel Wallace sur la Théorie de la Sélection Naturelle," was written some months ago, and was intended as an appendix to a French translation of my "Essays" by M. Lucien De Candolle, to be published by Reinwald, of Paris. As it is now very uncertain when the translation will appear, and as M. Claparède's critique has been highly spoken of in several English periodicals, I think it advisable that my answer to it should be no longer delayed.

In the "Archives des Sciences de la Bibliothèque Universelle," for June, 1870, M. Edouard Claparède has done me the honour to make my "Contributions to the Theory of Natural Selection" the subject of some critical remarks. To these I now propose briefly to reply.

I must premise that I do not intend to discuss here any of those difficulties which my critic finds in the theory of sexual selection, and which apply as much to Mr. Darwin's views as to my own, because, in his new work now announced, that theory will, I have no doubt, be fully developed, and be supported by a mass of facts and observations, in the absence of which further argument is useless. I proceed therefore to the objections that

apply more especially to my own views.

At p. 15 of his "Remarques" M. Claparède says,

> Son étude est consacrée à la coloration des oiseaux et, absorbé dans son sujet, l'auteur oublie que d'autres facteurs peuvent, aussi bien que la couleur, attirer l'attention des ennemis sur la gent ailée. Un nid couvert d'un dôme volumineux échappera tout aussi peu, grâce à ses dimensions, à l'œil d'un animal en quête de proie, que quelques plumes brillament colorées. Les gamins de nos villages en savent quelque chose, comme l'a remarqué M. le Duc d'Argyll, et ils ne réussissent que trop, à la présence d'un gros nid, à deviner l'oiseau caché et sa couvée.

This objection does not seem to me very serious, because in the first place, nests, however large, generally harmonise in colour with surrounding objects, and are not so easily seen at a little distance as a bright patch of colour; and, secondly, because "gamins" are not the chief natural enemies of the feathered tribes, while hawks and falcons do not break open nests, although they do seize and devour birds.

After giving (p. 23-25) what I must allow to be a very fair abstract of my reasons for believing that Natural Selection is not the only power that has operated in the development of man, M. Claparède intimates that I have so completely abandoned my own Darwinist principles that the reader will easily refute my arguments. He therefore confines himself to certain "reflections." I regret that he did not think it necessary to do more than this, because I have as yet in vain sought from my reviewers for any other than general objections to my arguments on this subject, and am at a loss to know how they can be so easily refuted. M. Claparède's "Reflections," however, do, fortunately, take the form of arguments. He says (p. 25),

> M. Wallace n'a pas reculé devant l'explication de la formation graduelle du chant de la fauvette et du rossignol par voie de selection naturelle. La chose est toute simple, bien fou serait celui qui voudrait recourir ici à l'intervention d'une Force supérieure, ami du Beau! Les fauvettes femelles et les rossignols de même sexe ont toujours accordé de préférence leur faveurs aux mâles bons chanteurs. C'était la conséquence de leur goûts musicaux et des aptitudes harmoniques de leur oreille. Malheur aux pauvres mâles à registre peu étendu ou à timbre fêlé! les douceurs de la paternité leur ont été impitoyablement refusées; ils sont morts de jalousie dans la tristesse et l'isolement. Ainsi s'ést formée la race des bons chanteurs qui peuplent nos bocages. Pourquoi n'y a-t-il pas des chanteuses? Sans doute que les oiseaux mâles ne se sont jamais souciés de la voix de leurs épouses, soit parcequ'ils n'avaient pas l'oreille juste, soit plûtot, car cela sera contradictoire, parceque leurs gouts musicaux étaient suffisament satisfaits par leurs concerts personels. Peut-être aussi les femelles n'avaient-elles point d'aptitude virtuelle au perfectionnement de la voix; peut être avaient-elles atteint l'extrême limite de développement vocal compatible avec l'organisation d'un oiseau du sexe féminin; ou bien enfin la sélection naturelle produite sous l'influence des poursuites exercées par des ennemis de toutes sortes contre les belles couveuses, sélection favorable, selon M. Wallace, à la production de couleurs sombres, a-t-elle mystérieusement éteint même l'éclat de sa voix? Quoiqu'il en soit, il est évident pour M. Wallace que la sélection sexuelle, en d'autres termes le goût des dames fauvettes pour la musique, a amené le grand perfectionnement de la voix des virtuoses de l'autre sexe. Mais dans l'espèce humaine, la chose aurait-elle pu se passer ainsi? Le chant harmonieux et enchanteur d'une *prima donna* aurait-il pu naître et se perfectionner par voie de sélection? Le goût musicale des auditeurs pourrait-il avoir eu une influence selectrice sur ce phénomène? Jamais, au grand jamais! Seule l'intervention d'une Force supérieure a pu amener un résultat pareil, car jamais homme primitif n'a eu de goût pour la musique. M. Wallace le sait bien: il a vécu si longtemps parmi les sauvages qui ont pu le lui dire!

> Au contraire, les femelles fauvettes primitives et les femelles rossignols primitives, avaient déjà le goût musical longtemps avant que leurs époux eussent appris à chanter. Comment M. Wallace le sait-il? Le lui ont-elles dit? N'importe, il le sait.

It is a pleasure to read anything so brilliant as this, but it hardly seems to touch the point of my argument. Male birds *do* sing at pairing time to the females. Mr. Darwin says in his "Origin of Species," "All those who have attended to the subject believe that there is the severest rivalry between the males of many species to attract, by singing, the females." Female birds do *not* sing. These are facts, and they perfectly accord with the theory of the perfection of song having been developed, in the *males*, by sexual selection. In man the facts are all different. Savage women have generally no *choice* as to their husbands, as has been so fully shown by Sir John Lubbock; and in the few cases where a choice is open to them, there is not a particle of evidence to show that a musical voice ever determines that choice. Still less reason is there to think that this quality determines the male savage in choosing his wife. Yet a wonderful musical organ has been developed in both sexes, of which the use to man in his struggle for existence has not yet been shown. Surely here is a difficulty which required facts and arguments for its elucidation rather than a brilliant display of wit.

Again, in reply to my arguments as to the total absence of hair from the back of man, we are told that it should be no difficulty to a person who believes that *hairy* mammals and *feathery* birds have been derived from *scaly* reptiles ("Remarques," pp. 27, 28). But surely this is not the argument of a Darwinian. For the hair and the feathers are *useful* to their several possessors, just as the scales were to their ancestral reptiles; whereas the very essence of my difficulty is, that the nudity has *not* been shown to be *useful* to man. M. Claparède thus concludes his remarks on this subject: – "Que M. Wallace soit au moins conséquent dans la question de la chute des poils. Si l'intervention d'une Force supérieure lui semble nécessaire pour épiler le dos de l'homme, qu'il sache se résoudre à la faire agir de même sue l'échine de l'éléphant, du rhinocéros, de l'hippopotame ou du cachalot." But the four mammals here mentioned are *thick-skinned* animals, one *aquatic*, one *amphibious*, the other two inhabitants of *hot* countries, lovers of *shade* and of *marshes*. Can anything be more clear than that, in all these cases, the hair was little or not at all wanted, and, owing to their habits, was very probably even injurious, and has therefore partially disappeared by means of natural selection? while the extinct mammoth and woolly rhinoceros are instances which prove that it always re-appeared when the needs of the animal required it. If the hair disappeared from the back of tropical man by the action of the same law which caused it partially to disappear from the tropical elephant, we must ask why it did not re-appear in the arctic Finns and Esquimaux, as it re-appeared in the arctic mammoth? It is rather for me to say – "Que M. Claparède soit au moins conséquent dans la question de la chute des poils."

The last point on which my critic remarks is my argument, that the brain of savage man is in advance of his needs, and therefore could not have been acquired by natural selection; and he asks, why I do not apply the same reasoning to many other cases, especially to that of the great group of birds with a complex larynx, comprising all the singing birds, yet having many species which do not sing. He says (p. 29),

> Ces oiseaux possèdent dans leur larynx un organe beaucoup trop bien conformé pour l'usage qu'ils en font. Il est donc nécessaire d'admettre l'intervention d'une Force supérieure pour façonner cet appareil, inutile aux oiseaux qui le possèdent, mais calculé en vue de générations nouvelles qui, dans un avenir plus ou moins éloigné et dans des conditions

> déterminées apprendront à chanter. Que M. Wallace aurait-il à répondre à une semblable argumentation?

My answer is, that the cases are not parallel or similar; if they were so, I should certainly adopt the same conclusion in both. To make them logically comparable, it would be necessary to prove that all the earlier forms of the group had the vocal organs fully developed, but did not sing; or what might be held to indicate this, that at present only a few species sing, while the great mass do not. But so far from this being the case, the majority of the species of the group have musical or sonorous voices, and there is no evidence to show that the vocal apparatus was fully developed before the power of singing began to be exercised. Man, on the contrary, stands alone in the development of his brain, and M. Claparède does not rebut the evidence I have adduced to show that the brain in savage and prehistoric man was in advance of his requirements.

In concluding his remarks, M. Claparède endeavours to impale me neatly on the horns of a dilemma, as follows:

> Ou bien M. Wallace a eu raison de faire intervenir une Force supérieure pour expliquer la formation des races humaines et guider l'homme dans la voie de la civilisation, et alors il a eu tort de ne pas faire agir cette même force pour produire toutes les autres races et espèces animales ou végétales; ou bien il a eu raison d'expliquer la formation des espèces végétales et animales par la seule voie de la sélection naturelle, et alors il a eu tort de recourir à l'intervention d'une Force supérieure pour rendre compte de la formation des races humaines.

These are his last words, and they seem to me to be the weakest in the whole paper, being a pure begging of the question. They assume that man presents no phenomena which differ in kind from those presented by other animals, whereas I have adduced a number of such phenomena which my critic has neither disproved nor dented. My whole argument is founded on certain facts, and on these facts only. My critic admits the facts, does not refute my arguments, yet maintains that I should give up my conclusion, because the theory of Natural Selection *must* apply equally to man and the rest of Nature, or to neither. But why must it do so? Darwin himself claims no such universality for it. He admits that even the common origin of animals and plants rests only on analogy, and that "it is immaterial whether it is accepted or not." But M. Claparède is more Darwinian than Darwin himself, and would, I presume, say that, either all animals or plants must be descended from one common ancestor or, that no two species are thus descended. I maintain, however, that man is descended from a lower animal form, but I adduce facts which go to prove that some other law or power than Natural Selection has specially modified him. If Darwin is not anti-Darwinian in admitting, as he does, the possibility that animals and plants may not have had a common ancestor, I may surely deny that I am anti-Darwinian when I show that there are certain phenomena in the case of man that cannot be wholly explained by the law of Natural Selection.

I must not conclude without thanking M. Claparède for the very flattering terms in which he has spoken of the larger portion of my work, and also for the general accuracy and fairness with which he has condensed my views and arguments in the last essay, to which he especially takes objection.

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Meyer's Exploration of New Guinea (S235)

Wallace kept up on the exploration activities of others around the world. Wallace also knew Meyer as the translator of his book The Malay Archipelago into German. This letter was printed in the 11 December 1873 issue of Nature.

Few persons can have read Dr. [Adolf Bernhard] Meyer's account of his recent adventurous and very successful journey with more interest than myself; but I confess I was surprised to find that the translator of my book should have misunderstood what I had stated, and so create a difference between us where none exists. He says (speaking of Dorey) that I "have not given a correct impression of the natives of the surrounding hills and mountains, separating them in some way from the inhabitants of the coast, as smaller, uglier, not mop-headed," &c.; and that he finds on other hand, that "there is no generic difference at all between the Papooas of the mountain and the Papooas of the coast, except such differences as we find everywhere between the highlanders and coast inhabitants of the same race." Now I say exactly the same thing: "From these (sketches) and the captain's description, it appeared that the people of Arfak were similar to those of Dorey." ("Malay Archipelago," 3rd Ed. p. 505.) Dr. Meyer however, probably refers to what I say of the people of one hill village, close to Dorey: "The inhabitants seemed rather uglier than those at Dorey village. They are, no doubt, the true indigenes of this part of New Guinea, living in the interior, and subsisting by cultivation and hunting. The Dorey-men, on the other hand, are shore dwellers, fishers, and traders in a small way, and have thus the character of a colony who have migrated from another district. These hillmen, or Arfaks, differed much in physical features. They were generally black, but some were brown like Malays. Their hair, though always more or less frizzly, was sometimes short and matted," &c. (p. 499). I can only suppose that the word "differed" in the above passage was taken to mean "differed from the Dorey people," whereas the context shows that it means "differed among themselves," or varied, which would have been a better word. In the preceding page I have stated of the inhabitants of Dorey: "The majority have short woolly hair;" so that there is no difference from them in that respect. In all I have written about the Papuans I have maintained that the people of New Guinea and of all the immediately surrounding islands are of one race, with very unimportant local differences; and I do not think my remark, that the people of one village were "*rather* uglier" than those of another, three miles off, justifies the idea that I supposed there was any "difference," in an ethnological sense, between them. I cannot find that I have said a word about difference of stature.

The great success of both Messrs. D'Albertis and Meyer in penetrating inland in New Guinea will, it is to be hoped, induce other travellers to attempt the exploration of the far larger and less known southern portion. Two Europeans, with a small steam launch and a Malay crew, would, no doubt, be able to penetrate a long way up some of the larger rivers, and establish a station from which exploration of the central mountains might be effected. There is now no portion of the globe so completely unknown as this, or which promises such great results for every branch of Natural History. – Alfred R. Wallace.

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Man and Evolution (S368ac)

In this letter printed in the The Daily News (London) issue of 22 December 1883, Wallace objects to an interpretation of his views on human evolution.

Sir, – In your article on Mr. Romanes' "Mental Evolution in Animals" my opinions on the above question are referred to, and as they are not accurately represented I trust you will allow me to make a few explanatory remarks. The writer of the article says: "Mr. Darwin held that to man's mind the general laws of evolution apply. Mr. Wallace holds that they do not apply, but that 'a distinct exception must be made in the case of the human organism, or at all events in the case of the human mind.'" I cannot find these words (which are given as a quotation) in the last chapter of my "Contributions to Natural Selection," where I have treated the question in some detail; and the whole gist of my argument is, not that natural selection "does not apply," but that it does not exclusively apply, being supplemented by some unknown higher law. To show that I do actually recognise the action of natural selection in producing some of the higher human faculties, allow me to quote one passage. I say (p. 351): "Turning to the mind of man, we meet with many difficulties in attempting to understand how those mental faculties which are especially human could have been acquired by the preservation of useful variations. At first sight it would seem that such feelings as those of abstract justice and benevolence could never have been so acquired, because they are incompatible with the law of the strongest, which is the essence of natural selection. But this is, I think, an erroneous view, because we must look not to individuals but to societies; and justice and benevolence, exercised towards members of the same tribe, would certainly tend to strengthen that tribe, and give it a superiority over another in which the right of the strongest prevailed, and where consequently the weak and the sickly were left to perish and the few strong ruthlessly destroyed the many who were weaker." Here, then, I fully recognise the power of natural selection to develop some mental faculties; but I go on to show that there are others, as well as some physical characters, which could not have been so developed, and I thence conclude that man was not developed exclusively by natural selection even if animals were so developed, but that in his case "some higher law" has intervened. This is very different from "barring" evolution in the case of man, as your reviewer says I do. Mr. Darwin himself admits that natural selection "has been the main, but not the exclusive means of the modification of organisms," and I have given reasons why this is still more emphatically true in the case of man; and these reasons have, so far as I know, never been satisfactorily confuted. As to the hypothetical mode by which I suggested that the difficulty might be got over, it remains a mere suggestion, the correctness of which I am by no means anxious to maintain; but that the difficulties I have stated are real difficulties, and as regards natural selection alone insuperable ones, I am as much convinced as ever. Evolution, however, is a very different thing, and I can hardly imagine any mode or origin of man or his faculties which would not be in accordance with that great principle, which is, essentially, the principle of gradual modification under the action of laws, however complex or obscure those laws may be. – I remain your obedient servant, Alfred R. Wallace.

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Remarkable Ancient Sculptures from North-west America (S433)

Remarks printed in the Nature issue of 26 February 1891. Wallace's comments here are straightforward enough, but it is noteworthy that Terry's report represents one of the earliest studies cited by the Sasquatch investigation community of the late twentieth century.

Mr. James Terry has just published descriptions and photographs of some of the most remarkable works of prehistoric man yet discovered on the American continent. The title of his paper[1] is sufficiently startling, but it is fully borne out by the beautiful full-size and half-size photographic prints with which it is illustrated. They represent three rude, yet bold, characteristic, and even life-like sculptures of simian heads, executed in basalt. One of these belongs to the author, one to Mr. T. Condon, and the third to Prof. O. C. Marsh, who referred to it, in his address "On Vertebrate Life in America," in the following terms: – "On the Columbia River I have found evidence of the former existence of inhabitants much superior to the Indians at present there, and of which no tradition remains. Among many stone carvings which I saw, there were a number of heads which so strongly resembled those of apes that the likeness at once suggests itself. Whence came these sculptures and by whom were they made?" Unfortunately we have no detailed information as to the conditions under which these specimens were found, except that "they would be classed as 'surface finds,' from the fact that the shifting sand-dunes, which were largely utilized for burial purposes, are continually bringing them to the surface and exposing them." This gives no indication of their antiquity, but is quite compatible with any age which their other characteristics may suggest. The size of the heads varies from eight to ten inches in total height, and from five and three-quarters to six and a half inches in width. The three are so different from each other that they appear to represent three distinct animals; and, so far as I can judge, they all differ considerably from the heads of any known anthropoid apes. In particular, the nostrils are much farther from the eyes and much nearer to the mouth than in any of the apes. In this respect they are more human; yet the general form of the head and face, the low and strongly-ridged forehead, and the ridges on the head and cheeks seem to point to a very low type of anthropoid. In a letter to Mr. Terry, Mr. Condon suggests "that they were copied from the figure-head of some Malay proa that may have been wrecked on the coast;" but such a supposition is quite inadmissible, since nothing at all resembling these heads is ever carved on Malay proas, and there is no reason to believe that if such a carving did come into the possession of the natives they would ever think of copying it in stone; while these sculptures were found two hundred miles from the coast on the east side of the Cascade Mountains.

Taking into consideration the enormous antiquity of the stone mortars and human remains found in the auriferous gravels of California buried under ancient lava streams and associated with a flora and fauna altogether different from that of any part of America at the present time, Mr. Terry's own conclusion appears the more probable. It is, "either that the animals which these carvings represent once existed in the Columbia valley, or that, in the remote past, a migration of natives from some region containing these monkeys reached this valley, and left one of the vivid impressions of their former

[1] "Sculptured Anthropoid Ape Heads found in or near the Valley of the John Day River, a tributary of the Columbia River, Oregon." By James Terry. (New York, 1891.)

surroundings in these imperishable sculptures." The latter alternative appears to me, for many reasons, to be highly improbable; and though the former will seem to many persons to be still more improbable, I am inclined provisionally to accept it.

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Prenatal Influences on Character (S476)

Although Wallace felt there was no evidence for the transmission of acquired physical characters, he was not so sure about the notion of acquired mental characters. This letter, printed in the Nature issue of 24 August 1893, addresses the latter issue.

The popular belief that prenatal influences on the mother affect the offspring physically, producing moles and other birth-marks, and even malformations of a more or less serious character, is said to be entirely unsupported by any trustworthy facts, and is also rejected by physiologists on theoretical grounds. But I am not aware that the question of purely mental effects arising from prenatal mental influences on the mother has been separately studied. Our ignorance of the causes, or at least of the whole series of causes, that determine individual character is so great, that such transmission of mental influences will hardly be held to be impossible or even very improbable. It is one of those questions on which our minds should remain open, and on which we should be ready to receive and discuss whatever evidence is available; and should a *primâ facie* case be made out, seek for confirmation by some form of experiment or observation, which is perhaps less difficult than at first sight it may appear to be.

In one of the works of George or Andrew Combe, I remember a reference to a case in which the character of a child appeared to have been modified by the prenatal reading of its mother, and the author, if I mistake not, accepted the result as probable, if not demonstrated. I think, therefore, that it will be advisable to make public some interesting cases of such modification of character which have been sent me by an Australian lady in consequence of reading my recent articles on the question whether acquired characters are inherited. The value of these cases depends on their differential character. Two mothers state that in each of their children (three in one case and four in the other) the character of the child very distinctly indicated the prenatal occupations and mental interests of the mother, though at the time they were manifested in the child they had ceased to occupy the parent, so that the result cannot be explained by imitation. The second mother referred to by my correspondent only gives cases observed in other families which do not go beyond ordinary heredity.

> "I can trace in the character of my first child, a girl now twenty-two years of age, a special aptitude for sewing, economical contriving, and cutting out, which came to me as a new experience when living in the country amongst new surroundings, and, strict economy being necessary, I began to try and sew for the coming baby and for myself. I also trace her great love of history to my study of Froude during that period, and to the breathless interest with which my husband and I followed the incidents of the Franco-German war. Yet her other tastes for art and literature are distinctly hereditary. In the case of my second child, also a daughter (I having interested myself prior to her birth in literary pur-

suits) the result has been a much acuter form of intelligence, which at six years old enabled her to read and enjoy the ballads which Tennyson was then giving to the world, and which at the age of barely twenty years allowed her to take her degree as B.A. of the Sydney University.

"Before the third child, a boy, was born, the current of our life had changed a little. Visits to my own family and a change of residence to a distant colony, which involved a long journey, as well as the work which such changes involve, together with the care of my two older children, absorbed all my time and thoughts, and left little or no leisure for studious pursuits. My occupations were more mechanical than at any other time previous. This boy does not inherit the studious tastes of his sisters at all. He is intelligent and possesses most of the qualifications which will probably conduce to success in life, but he prefers any kind of outdoor work or handicraft to study. Had I been as alive then as I am now to the importance of these theories, I should have endeavoured to guard against this possibility; as it is, I always feel that it is perhaps my fault that one of the greatest pleasures of life has been debarred to him.

"But I must not weary you by so many personal details, and I trust you will not suspect me of vanity in thus bringing my own children under your notice. Suffice it to say that in every instance I can and do constantly trace what others might term coincidences, but which to me appear nothing but cause and effect in their several developments.

"I will pass on to quote a few passages from letters written to me by two highly intelligent mothers, whom I asked to give me their experiences on this subject, if they had any.

"Mrs. B___ says: 'I can trace, nay, have traced (in secret amusement often), something in every child of mine. Before the birth of my eldest girl I took to ornithology, for work and amusement, and did a great deal in taxidermy too. At the age of three years I find this youngster taking such insects and little animals as she could find, and puzzling me with hard questions as to what was inside them. Later on she used to be seen with a small knife, working and dissecting cleverly and with much care and skill at their *insides*. One day she brought me the tiniest heart of the tiniest lizard you could imagine, so small that I had to examine it through a glass, though she saw it without any artificial aid. By some means she got a young wallaby and made an apron with a pocket inside which she used to call her "pouch." This study of natural history is still of interest to her, though she lacks time and opportunities. Still, she always does a little dissecting when she gets a chance.'

"I never noticed anything about P___ for some years. Three months before he was born a friend, whom I will call Smith, was badly hurt, and was brought to my house to be nursed. I turned out the nursery and he lay there for three months. I nursed him until I could do so no longer, and then took lodgings in town for my confinement. Now after all these years I have discovered how this surgical nursing has left its mark. This boy is in his element when he can be of use in cases of accident, &c. He said to me quite lately, 'How I wish you had made a surgeon of me.' Then all at once the light flashed in upon me, but, alas! it was too late to remedy the mistake.

"Before the birth of the third child I passed ten of the happiest months of my life. We had a nice house, one side of which was covered with cloth of gold roses and bougainvillea, a garden with plenty of flowers, and a vineyard. Here we led an idyllic life, and did nothing but fish, catch butterflies, and paint them. At least, my husband painted them after I had caught them and mixed his colours. At the end of this time L___ was born. This child excels in artistic talent of many kinds, nothing comes amiss to her, and she draws remarkably well. She is of a bright, gay disposition, finding much happiness in life, even though not always placed in the most fortunate surroundings. Before the birth of my next child, N___, a daughter, I had a bad time. My husband fell ill of fever, and I had to nurse him without help or assistance of any kind. We had also losses by floods. I don't know how I got through that year, but I had no time for reading. N___ is the most

prudent, economical girl I know. She is a splendid housekeeper and a good cook, and will work till she drops, but has no taste for reading, but seems to gain knowledge by suction."

If the preceding cases are fully and accurately stated they seem to afford grounds for further investigation. Changes in mode of life and in intellectual occupation are so frequent among all classes, that materials must exist for determining whether such changes during the prenatal period have any influence on the character of the offspring. The present communication may perhaps induce ladies who have undergone such changes, and who have large families, to state whether they can trace any corresponding effect on the character of their children.

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Woman: Her Brain, Mental Capacity, and Character (S624a)

Some brief comments printed in the April 1906 issue of Ethological Journal responding to an address of this title by Bernard Hollander delivered to the Ethological Society on 10 January 1906.

The subject is one on which I have no special knowledge, and the only suggestion I can make is, that perhaps it might be shown how the special mental peculiarities of women have arisen through selection of those who were in every way best fitted for the production and rearing of healthy children. Many, perhaps all of the characteristics mentioned, have almost certainly been thus evolved; and it is, in my opinion, one of the condemnations of our present social system, that it has led to the deterioration of a large proportion of the women both of the highest and the lowest social grades, in these most vital qualities.

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Dr. A. R. Wallace & Sir W. M. Ramsay's Theory (S645)

Wallace's letter to the Editor is the featured element in this story, printed in the 13 September 1907 issue of Public Opinion (London).

Did Man Reach His Highest Development in the Past?

Dr. A. R. Wallace, the distinguished scientist, writes to the Editor of *Public Opinion* respecting Sir W. M. Ramsay's article, summarised in this paper last week.

Sir W. M. Ramsay contended that "degeneration was the outstanding fact in religious history, and that the modern theory often takes the last product of degeneracy as the facts of primitive religion. Having attained this view I recognise that it was the basis of the Pauline philosophy." It is of this theory that Dr. Wallace writes thus:

"I have been much interested in the account you have given of Sir William Ramsay's article in the *Contemporary Review* on the Philosophy of Religion, and so far as your extracts and remarks go I am largely in sympathy with it. So far back as 1876 I expressed

very similar views as to the early civilisation and intellectual development of mankind as Sir W. Ramsay holds in regard to his religious development. I was led to give attention to this subject by reading an address to the Literary and Philosophical Society of Liverpool in 1873 by a very acute and philosophical thinker, Mr. Albert Mott, in which he maintained that 'our most distant glimpses of the past are still of a world peopled, as now, with men both civilised and savage'; and, further, 'that we have often entirely misread the past by supposing that the outward signs of civilisation must always be the same, and must be such as are found among ourselves.'

Man at His Highest

"It was in my address to the Biological Section of the British Association at Glasgow that I somewhat developed these ideas, passing in review the sculptures of Easter Island, the North American earth mounds, and the Great Pyramid, as well as 'the elevation, at once intellectual and moral, displayed in the writings of Confucius, Zoroaster, and the Vedas,' and reaching the conclusion that 'man's intellectual and moral development reached almost its highest level in a very remote past.' My final conclusion was thus expressed: 'If the views now advanced are correct, many, perhaps most, of our existing savages are the descendants of higher races; and their arts, often showing a wonderful similarity in distant continents, may have been derived from a common source among more civilised peoples.'

"These views I still hold, and they enabled me, in 1892, when studying the Australian type, for the purpose of a new edition of 'Stanford's Compendium of Geography,' to reach the conclusion that the Australian aborigines are really a degraded outlier of the great Caucasian type of man – that they are closely allied to ourselves, and are known, by all who have sympathetically studied them, to have many good qualities, both moral and intellectual. This view of their affinities is now generally accepted by anthropologists, and is adopted by Prof. J. W. Gregory in the last edition of the same work just published, and the result of a personal study of the natives in the interior of the country. This conclusion is especially interesting as at once raising what had previously been almost always classed among the very lowest of human races to a place in close affinity with the very highest.

Decadence of the Australian Aborigines

"My British Association Address is republished in my 'Natural Selection and Tropical Nature'; while an extended chapter on the 'Affinities and Origin of the Australian and Polynesian Races' is contained in the first volume of my 'Studies Scientific and Social.' In this chapter I have stated somewhat fully the reasons for my conclusion, illustrated by photographs, both of Australians and of the various other low types of unmistakably Caucasian origin. Some of these photographs will, I think, surprise those who have been accustomed to look upon the aborigines of Australia as very little higher than the anthropoid apes."

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Section 4. Spiritualism and Related Subjects

Introduction

Wallace's involvement in spiritualism has sometimes been viewed as his greatest intellectual weakness, but we should be careful not to oversimplify. Wallace was in fact a committed spiritualist for a bit more than half his life, from late 1866 on, but one should understand that his allegiance to its message was the logical outcome of his search for a coherent understanding of humankind's "higher attributes," and not a simple fascination with disembodied spirits (entertaining though they may sometimes have been…). Wallace's view of the "spirit realm" was hyper-naturalistic (or perhaps scientistic): that is, he (and many other spiritualists) believed it to be just another part of the natural world. To be sure, it was supposed to be nonphysical, but in contrast with Christian visions of heaven and hell, for example, it was thought to operate under natural law.

It was Wallace's reading of the literature of spiritualism that convinced him of this as much as it was his experiences at séances. Its philosophy promoted a view not unlike Spencerian logic: that the progression of the human soul after physical existence began with its experiences during that existence, and its willingness to improve – to receive only what was deserved. Spiritualists believed that communication between the spirit world and living humans was ongoing, through the emotions stirred up by dreams and other subliminal processes. This could be productive – evolutionary – if the recipients of these nudges made appropriate use of them.

Now, whether any of this holds water or not, it must be understood that this is what Wallace thought, and any attempt to understand his world view after 1865 must allow him this anchor. This means his other evolutionary views as well, because without spiritualism Wallacean natural selection is incomplete, unable to reconcile the existence of characters which appear to have no purely utilitarian value.

Wallace's various defenses of spiritualism – and there were many – dwell both on its logic, and its apparent manifestations. As to the latter, most observers would say he was on shaky ground, yet their non-existence cannot be proved outright at this point. This uncomfortable position will be maintained until someone comes along with a better way of looking at the matter.

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Dr. Carpenter and Psychic Force (S206)

A letter printed in the 15 February 1872 issue of The Spiritualist (London).

Mr. Alfred R. Wallace, President of the Entomological Society, has favoured us with the following copy of a letter sent to the *Daily Telegraph*, but not published by that journal:

Sir, – In the report of Dr. [William B.] Carpenter's lecture at Chelsea (given in your issue of Saturday last), there occurs a passage so extraordinary and so entirely misleading, that I must beg you, in the interests of truth, to allow me to make a few remarks upon it. Dr. Carpenter is stated to have said that he would grapple with Mr. [William] Crookes' "Psychic Force;" and, in attempting to do so, exhibited an experiment intended to show (and which his audience must have believed really did show) that Mr. Crookes was ignorant of the merest rudiments of mechanics, and was deluded by an experiment, the fallacy of which an intelligent schoolboy could have pointed out. Dr. Carpenter, it is said, exhibited a glass of water poised against an equal weight upon a balance, and showed, that by dipping a finger in the water – that is, by *pressing* with a force exactly equal to the weight of the water displaced by the immersed finger – you increased the weight on that side of the balance. Now, unless the audience were intended to believe that Mr. Crookes was ignorant of this childishly simple fact; and further, that it completely accounted for the result of his experiment, for what purpose was this experiment shown? Yet if this *is* what it was intended to prove, then it becomes absolutely certain that Dr. Carpenter could never have read Mr. Crookes's account of his experiments given in October last in the *Quarterly Journal of Science* (for he would certainly not wilfully misrepresent the experiment), and was therefore in complete ignorance of what he was attempting to disprove. For, will it be believed, Mr. Crookes expressly states that, "*dipping the hand to the fullest extent into the water does not produce the least appreciable action on the balance*," the reason of which is sufficiently clear, for his woodcut shows, and his description tells us, that the vessel of water was not placed on the scale of a balance at all, but on a board *exactly over its fulcrum* or point of support at one end, while the distant end was suspended from a balance. Yet this balance showed a force of more than *one pound* exerted on it, when Mr. Home merely dipped the tips of the fingers of one hand in the water! Dr. Carpenter is an "eminent man of science" and a fellow of the Royal Society; yet if your reporter has correctly stated his mode of criticising the experiments of another F.R.S., we may be excused for not placing implicit confidence in the "two eminent men of science," who are said by Dr. Carpenter to have reported Mr. Crookes's facts "good for nothing." – Alfred R. Wallace.

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Ethnology and Spiritualism (S208)

An oft-cited letter to the Editor that appeared in the 7 March 1872 issue of Nature. In this note Wallace replies to Edward B. Tylor's own reply to Wallace's earlier review of Tylor's book Primitive Culture.

There is only one point in Mr. Tylor's communication (*Nature*, Feb. 29, p. 343) on which it seems desirable that I should say a few words, in order that I may not be supposed to assent to what I conceive to be a most erroneous view. Mr. Tylor suggests that the phenomena that occur in the presence of what are called mediums, are or may be of the same nature as the subjective impressions of persons under the influence of a powerful mesmeriser. Five and twenty years ago I was myself a practised mesmeriser, and was able to produce on my own patients almost the whole range of phenomena which are

exhibited in public as illustrative of "mesmerism" or "electro-biology." I carried on numerous experiments in private, and paid especial attention to the conditions under which the phenomena occur. During the last seven years I have had repeated opportunities of examining the phenomena that occur in the presence of so-called "mediums," often under such favourable conditions as to render trick or imposture simply impossible. I believe, therefore, I may lay claim to some qualifications for comparing the mesmeric with the mediumistic phenomena with especial reference to Mr. Tylor's suggestion, and I find that there are two great characteristics that broadly distinguish the one from the other.

1. The mesmerised patient never has *doubts* of the reality of what he sees or hears. He is like a dreamer to whom the most incongruous circumstances suggest no idea of incongruity, and he never inquires if what he thinks he perceives harmonises with his actual surroundings. He has, moreover, lost his memory of what and where he was a few moments before, and can give no account, for instance, of how he has managed to get out of a lecture-room in London to which he came as a spectator half an hour before, on to an Atlantic steamer in a hurricane, or into the recesses of a tropical forest.

The assistants at the *séances* of Mr. Home or Mrs. Guppy are not in this state, as I can personally testify, and as the almost invariable *suspicion* with which the phenomena are at first regarded clearly demonstrates. They do not lose memory of the immediately preceding events; they criticise, they examine, they take notes, they suggest tests – none of which the mesmerised patient ever does.

2. The mesmeriser has the power of acting on "certain sensitive individuals" (not on "assemblies" of people, as Mr. Tylor suggests), and all experience shows that those who are thus sensitive to any one operator are but a small proportion of the population, and these almost always require previous manipulation with passive submission to the operator. The number who can be acted upon without such previous manipulation is very small, probably much less than one per cent. But there is no such limitation to the number of persons who simultaneously see the mediumistic phenomena. The visitors to Mr. Home or Mrs. Guppy all see whatever occurs of a physical nature, as the records of hundreds of sittings demonstrate.

The two classes of phenomena, therefore, differ fundamentally; and it is a most convincing proof of Mr. Tylor's very slender acquaintance with either of them, that he should even suggest their identity. The real connection between them is quite in an opposite direction. It is the mediums, not the assistants, who are "sensitives." They are almost always subject to the mesmeric influence, and they often exhibit all the characteristic phenomena of coma, trance, rigidity, and abnormal sense-power. Conversely, the most sensitive mesmeric patients are almost invariably mediums. The idea that it is necessary for me to inform "spiritualists" that I believe in the power of mesmerisers to make their patient believe what they please, and that this "information" might "bring about investigations leading to valuable results," is really amusing, considering that such investigations took place twenty years ago, and led to this important result – that almost all the most experienced mesmerists (Prof. Gregory, Dr. Elliotson, Dr. Reichenbach, and many others) became spiritualists! If Mr. Tylor's suggestion had any value, these are the very men who ought to have demonstrated the subjective nature of mediumistic phenomena; but, on the contrary, as soon as they had the opportunity of personally investigating them, they all of them saw and admitted their objective reality.

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Spiritualism and Science (S219)

This letter to The Times (London), printed in its 4 January 1873 issue, was later reprinted several times in other venues.

Having been named by several of your correspondents as one of the scientific men who believe in spiritualism, you will perhaps allow me to state briefly what amount of evidence has forced the belief upon me. I began the investigation about eight years ago, and I esteem it a fortunate thing that at that time the more marvellous phenomena were far less common and less accessible than they are now, because I was led to experiment largely at my own house, and among friends whom I could trust, and was able to establish to my own satisfaction, by means of a great variety of tests, the occurrence of sounds and movements not traceable to any known or conceivable physical cause. Having thus become thoroughly familiar with these undoubtedly genuine phenomena, I was able to compare them with the more powerful manifestations of several public mediums, and to recognize an identity of cause in both by means of a number of minute but highly characteristic resemblances. I was also able, by patient observation, to obtain tests of the reality of some of the more curious phenomena which appeared at the time, and still appear to me, to be conclusive. To go into details as to those experiences would require a volume, but I may, perhaps, be permitted briefly to describe one, from notes kept at the time, because it serves as an example of the complete security against deception which often occurs to the patient observer without seeking for it.

A lady who had seen nothing of the phenomena asked me and my sister to accompany her to a well-known public medium. We went, and had a sitting alone in the bright light of a summer's day. After a number of the usual raps and movements our lady friend asked if the name of the deceased person she was desirous of communicating with could be spelt out. On receiving an answer in the affirmative, the lady pointed successively to the letters of a printed alphabet while I wrote down those at which three affirmative raps occurred. Neither I nor my sister knew the name the lady wished for, nor even the names of any of her deceased relatives; her own name had not been mentioned, and she had never been near the medium before. The following is exactly what happened, except that I alter the surname, which was a very unusual one, having no authority to publish it. The letters I wrote down were of the following kind: – y n r e h n o s p m o h t. After the first three – y n r – had been taken down, my friend said, "This is nonsense, we had better begin again." Just then her pencil was at e, and raps came, when a thought struck me (having read of, but never witnessed a similar occurrence) and I said "Please go on, I think I see what is meant." When the spelling was finished I handed the paper to her, but she could see no meaning in it till I divided it at the first h, and asked her to read each portion backwards, when to her intense astonishment the name "Henry Thompson" came out, that of a deceased son of whom she had wished to hear, correct in every letter. Just about that time I had been hearing *ad nauseam* of the superhuman acuteness of mediums who detect the letters of the name the deluded visitors expect, notwithstanding all their care to pass the pencil over the letters with perfect regularity. This experience, however (for the substantial accuracy of which as above narrated I vouch), was and is, to my mind, a complete disproof of every explanation yet given of the means by which the names of deceased persons are rapped out. Of course, I do not expect any sceptic, whether scien-

tific or unscientific, to accept such facts, of which I could give many, on my testimony, but neither must they expect me, nor the thousands of intelligent men to whom equally conclusive tests have occurred, to accept their short and easy methods of explaining them.

If I am not occupying too much of your valuable space I should like to make a few remarks on the misconceptions of many scientific men as to the nature of this inquiry, taking the letters of your correspondent Mr. Dircks as an example. In the first place, he seems to think that it is an argument against the facts being genuine that they cannot all be produced and exhibited at will; and another argument against them, that they cannot be explained by any known laws. But neither can catalepsy, the fall of meteoric stones, nor hydrophobia be produced at will; yet these are all facts, and none the less so that the first is sometimes imitated, the second was once denied, and the symptoms of the third are often greatly exaggerated, while none of them are yet brought under the domain of strict science; yet no one would make this an argument for refusing to investigate these subjects. Again, I should not have expected a scientific man to state, as a reason for not examining it, that spiritualism "is opposed to every known natural law, especially the law of gravity," and that it "sets chymistry, human physiology, and mechanics at open defiance;" when the facts simply are that the phenomena, if true, depend upon a cause or causes which can overcome or counteract the action of these several forces, just as some of these forces often counteract or overcome others; and this should surely be a strong inducement to a man of science to investigate the subject.

While not laying any claim myself to the title of "a really scientific man," there are some who deserve that epithet who have not yet been mentioned by your correspondents as at the same time spiritualists. Such I consider the late Dr. Robert Chambers, as well as Dr. Elliotson, Professor William Gregory, of Edinburgh; and Professor Hare, of Philadelphia – all unfortunately deceased; while Dr. Gully, of Malvern, as a scientific physician, and Judge Edmonds, one of the best American lawyers, have had the most ample means of investigation; yet all these not only were convinced of the reality of the most marvellous facts, but also accepted the theory of modern spiritualism as the only one which would embrace and account for the facts. I am also acquainted with a living physiologist of high rank as an original investigator, who is an equally firm believer.

In conclusion I may say that, although I have heard a great many accusations of imposture, I have never detected it myself; and, although a large proportion of the more extraordinary phenomena are such, that, if impostures, they could only be performed by means of ingenious apparatus or machinery, none has ever been discovered. I consider it no exaggeration to say, that the main facts are now as well established and as easily verifiable as any of the more exceptional phenomena of nature which are not yet reduced to law. They have a most important bearing on the interpretation of history, which is full of narratives of similar facts, and on the nature of life and intellect, on which physical science throws a very feeble and uncertain light; and it is my firm and deliberate belief that every branch of philosophy must suffer till they are honestly and seriously investigated, and dealt with as constituting an essential portion of the phenomena of human nature.

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A Sitting With Dr. Slade (S255)

A letter to the Editor printed in the 25 August 1876 issue of The Spiritualist (London). Henry Slade, a visiting American, had been accused of slate-writing fraud and Wallace, who believed in his genuineness, was here coming to his defense.

My *séance* with Dr. Slade, on August 9th, was very similar in its details to that so admirably and fully described by Serjeant Cox, in the pages of *The Spiritualist*. Little is needed, therefore, but for me to confirm the accuracy of that description.

Writing came upon the upper part of the slate, when I myself held it pressed close up to the under-side of the table, both Dr. Slade's hands being upon the table in contact with my other hand. The writing was *audible* while in progress. This one phenomenon is absolutely conclusive. It admits of no explanation or imitation by conjuring.

Writing also came on the under-side of the slate while laid flat upon the table, Dr. Slade's hand being laid flat on it, immediately under my eyes.

A chair was moved, and held for several seconds with the seat up to the table at the furthest corner from Dr. Slade, while both his hands were clasped on mine, and his body was quiescent.

I was repeatedly touched and my clothes pulled on the side turned away from Dr. Slade; my chair was rapped on the back, and sharp taps came under the cane seat of my chair.

While Dr. Slade was holding the slate in one hand, the other being clasped on mine, a distinct hand rose rapidly up and down between the table and my body; and, finally, while Dr. Slade's hands and mine were both on the centre of the table, the further side rose up till it was nearly vertical, when the whole table rose and turned over on to my head.

These phenomena occurred in broad daylight, with the sun shining into the room, and with no one present but Dr. Slade and myself. They may be witnessed with slight variations by any of our men of science, and it is to be hoped that those who do not take the trouble to see them will, at all events, cease to speak disparagingly of the intellectual and perceptive powers of those who, having seen, declare them to be realities.

It is also not too much to ask that men who have previously denied the possibility of such phenomena, and have accused others of prepossession and self-delusion, should, after having seen Dr. Slade, make some public acknowledgment of their error.

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A Spirit Medium (S259)

Wallace infuriated many of his colleagues when, as President of the Section of Biology at the annual British Association for the Advancement of Science meetings in 1876, he allowed a paper on spiritualism to be read. Complaints followed; in this letter, printed in the 19 September 1876 issue of The Times (London), he responded to these.

Sir, – In *The Times* of the 16th inst. Professor E. Ray Lankester states that I am per-

sonally responsible for the reading of Professor [William] Barrett's paper before the Anthropological Department of the British Association, and that my supposed conduct is "more than questionable." May I be allowed to show that this accusation (for such it amounts to) is wholly without foundation?

The paper in question was brought before the Committee of Section D by the secretary, before which time I had never seen it. A member proposed that it should be reported on, but after a full discussion this was negatived. The paper then passed to the Departmental Committee, where it was again discussed, and, on division, was left to be read in due course. Professor Lankester is evidently ignorant of the fact that the reading of this paper was decided after a vote taken in two Committees, and he was, therefore, not justified in making the unqualified statement that, "in consequence of the more than questionable action of Mr. Alfred Wallace, the discussions of the British Association have been degraded by the introduction of the subject of spiritualism." As to Professor Lankester's opinion as to what branches of inquiry are to be tabooed as "degrading," we have, on the other side, the practical evidence of such men as Lord Rayleigh, Mr. Crookes, Dr. Carpenter, and Colonel Lane Fox – none of them inferior in scientific eminence to Professor Lankester, yet all taking part in the discussion, and all maintaining that discussion and inquiry were necessary; while the close attention of a late President of the Association and of a crowded audience showed the great interest the subject excited.

As I have now shown that Professor Lankester commenced his letter with an erroneous statement of fact, and a "more than questionable" statement of opinion, it is not to be wondered at that I find the remainder of his communication equally unsatisfactory. His account of what happened during his visit to Dr. Slade is so completely unlike what happened during my own visit, as well as the recorded experiences of Serjeant Cox, Mr. Carter Blake, and many others, that I can only look upon it as a striking example of Dr. Carpenter's theory of preconceived ideas. Professor Lankester went with the firm conviction that all he was going to see would be imposture, and he believes he saw imposture accordingly. The "fumbling," the "manœuvres," the "considerable interval of time" between cleaning the slate and holding it under the table, and the writing occurring on the opposite side of the slate to that on which the piece of pencil was placed, were all absent when I witnessed the experiment; while the fact that legible writing occurred on the clean slate when held entirely in my own hand while Dr. Slade's hands were both upon the table and held by my other hand, such writing being distinctly audible while in progress, and the further fact that Dr. Slade's knees were always in sight, and that the slate was never rested upon them at all, render it quite impossible for me to accept the explanation of Professor Lankester and Dr. Donkin as applicable to any portion of the phenomena witnessed by me. – Yours faithfully, Alfred R. Wallace, Glasgow.

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Dr. Carpenter on Mesmerism, &c. (S263)

Wallace's nemesis on the subject of spiritualism was the physiologist William B. Carpenter (1813–1885). Carpenter had plenty of bad things to say about spiritualism, and sometimes didn't worry about getting his facts straight. This

response by Wallace to some remarks by Carpenter appeared in The Daily News (London) issue of 11 December 1876.

In your article on this subject, you appear to have been led into an error by Dr. Carpenter's constant habit of giving only one side of the question, and completely ignoring all facts which tell against his theory. You say, speaking of [Baron von] Reichenbach, 'But he did not try secretly removing the magnets, and then asking the sensitives whether they still saw the flames.' Now, every one who has read Reichenbach's book must know that tests of this kind were applied by him again and again, in an endless variety of ways. The magnets were continually changed in number, size, and position, in the totally dark chamber, and more than this, the magnet was sometimes completely hidden by a screen, but a lens was so placed as to throw the image of it (had there been light) on the wall. In every case, the sensitives described the flames from the magnet as small or large, single, double, or treble, high or low, to the right or to the left, just as the magnets were changed; and when the lens was used they described the flame on the wall, and were then asked to place their finger on it, when Reichenbach marked the place with a pencil, and found afterwards that the mark was exactly where the image would be thrown by the lens. Now, the negative fact, that in some cases mesmeric patients can be made to see anything by 'expectation,' does not disprove these cases in which all expectation was carefully excluded. Again, as to mesmerism at a distance unknown to the patient, Dr. Carpenter gives cases in which this failed, and in which the patient was mesmerised merely by the expectation of being so. This is one side – the negative side – of the question. But Dr. Carpenter knows that there is a positive side, which he ignores, of cases in which mesmerism has been produced when the patient did not and could not know he was being mesmerised. One of these is given by Professor Gregory's "Letters on Animal Magnetism," page 107, which happened in his own house to a member of his own family; and, to show the kind of evidence that Dr. Carpenter carefully omits to allude to, I hope you will allow me space briefly to state the facts. In Professor Gregory's house one evening this lady was mesmerised by a Mr. Lewis. Next morning at breakfast the lady complained of a headache. After his lecture Professor Gregory met Mr. Lewis, and told him that the lady he had mesmerised had a bad headache, to which Mr. Lewis replied that he would think of it some time during the day and mesmerise her so as to remove the headache. Professor Gregory did not return home till 5 o'clock, when, without being asked, the lady at once said to him that she had been mesmerised while nobody was present and while playing the piano, at half-past three o'clock. Her arms lost their power, and she was obliged to lie down and go into a short mesmeric sleep, and when she awoke her headache was gone. In the evening Professor Gregory met Mr. Lewis again; and was told that he had mesmerised the lady, as promised, as soon as he could get home to his lodgings, which was about half-past 3. The distance between the two houses was about a third of a mile; and the whole of the circumstances were such as to render 'expectation' out of the question. Dr. Carpenter continually compares the evidence for the facts of clairvoyance and spiritualism with that for the facts of science, to the disadvantage of the former. May I ask if it is the 'scientific' method of inquiry, to ignore facts which tell against you, and, while making much of negative evidence in your favour, to lead the public to suppose that there is no positive evidence on the other side? In the matter of the divining rod, I could adduce equally good and positive testimony against the lecturer's negative facts, but the

above is sufficient to prove that whoever wishes to know the whole truth on this matter must not rely on the statements of Dr. Carpenter.

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Dr. Carpenter on Spiritualism (S264)

Printed in The Daily News (London) issue of 19 December 1876.

Mr. Alfred R. Wallace writes to us: – "I am very sorry to find that in his second lecture, reported in your columns on Saturday, Dr. Carpenter has made statements which, as a matter of fact, are untrue, and which (as I shall show) he had the means of knowing to be untrue. Leaving it to Mr. Crookes and to the friends of the 'two American girls' to reply to the allegations made about them, I beg to be allowed to point out an important misstatement which concerns myself. Dr. Carpenter is reported to have said that 'a friend who believed' once told him of a small table rising up bodily when the medium and two or three other persons placed their hands on the top, and that it was suggested that a cylindrical cage of paper and hoops should be placed round the table to prevent the possibility of its being raised by the medium's toe under cover of her dress. His friend asked – 'If it is done without disturbing the paper, what will you say?' 'I will say, then,' replied Dr. Carpenter, 'it is a thing deserving further investigation.' The paper cage was put round the feet of the table and Dr. Carpenter assured his audience that he never after heard of the table dancing up and down, and that 'that test was sufficient to his mind to dispose of that particular case.' Now, I should like to ask Dr. Carpenter whether he would be surprised to hear that I myself was the 'believing friend' who told him about the small table and the paper cage, and further that I told him that it had been tried and had perfectly succeeded? And even if he had quite forgotten this – which I admit he may have done – the experiment is fully described, along with another even more conclusive test, in the notes of personal evidence given in my little book on 'Miracles and Modern Spiritualism' (pp. 133, 134), which I may assume one who comes forward to enlighten the public has taken the trouble to read. In the same book (p. 128) he will find an account of another table rising, while a sceptical friend who accompanied me was looking on and could see the whole lower part of the table with the feet freely suspended above the floor. It is hardly too much to say that every one of the more important statements which Dr. Carpenter puts before the public as evidence in his favour may be shown to be equally opposed to the actual facts of the case; but I will continue myself to one of these. Dr. Carpenter asserts that the two French clairvoyants, Alexis and Adolphe, were broken down by a test case as related by Sir J. Forbes, and he describes their method of procedure as 'guessing the number of letters in a word.' If they said 'six,' some one was sure to say 'very near,' and so they gathered indications, and they also knew what sort of words were likely to be given. And this is actually set before the public as an adequate account of the clairvoyance of these remarkable young men. As regards Adolphe, I can from personal observation declare that it bears not the most remote relation to what he did; and Dr. Edwin Lee, a well-known physician, in his book on 'Animal Magnetism,' has given, from personal observation, a minute account of the clairvoyance of Alexis at Brighton, which occupies twenty-five pages. Among a great variety of most remarkable tests, he frequent-

ly read passages in books brought at random a number of pages in advance of the page opened, but at the level of a line indicated. Numbers of these tests are recorded, the words read always being found at the level indicated, but not always at the exact number of pages in advance asked for. The evidence for this as well as for many other forms of clairvoyance is overwhelming, and the tests applied of the most varied and stringent character. It has lately been asserted that professed conjurors are the proper persons to test the alleged powers of mediums and clairvoyants. Now, if there is one thing more than another which conjurors know all about, it is tricks with cards; and Robert Houdin is acknowledged to have been one of the greatest of modern professors of the art of leger-demain. Yet when he took his own new cards and dealt them himself on the table Alexis named every card before it was turned up, and in subsequent games he told Houdin every card he held, and even what would be trumps before it was turned up. At the end of two sittings, which are found recorded in Dr. Lee's book (pp. 231–233), Houdin wrote to his friend M. de Mirville: 'I came away from this seance as astonished as any one can be, and fully convinced that it would be quite impossible for any one to produce such surprising effects by mere skill.' Dr. Carpenter quotes Robert Houdin for his own purpose; why did he not tell his audience of the great conjuror's testimony as to the possession by Alexis of some power other than 'mere skill'? But besides this remarkable testimony of an expert of the highest rank, we have the evidence of many physicians of eminence to the reality of clairvoyance. In the 'Dictionnaire de Médecine,' article 'Magnetisme,' the Parisian Professor of Medicine, Dr. Rostan, says, 'There are few facts better demonstrated than clairvoyance. I placed my watch at a distance of three or four inches from the occiput of the somnambulist, and asked her if she saw anything. "Certainly," she replied, "it is a watch, ten minutes to eight." M. Ferrus repeated the experiment with the same successful result. He turned the hands of his watch several times, and we presented it to her without looking at it; she was not once mistaken. Dr. Herbert Mayo, a physiologist who, in his lifetime, had a scientific reputation as high as that of Dr. Carpenter himself, testified to the reality of clairvoyance; and Dr. Lee quotes the testimony of other physicians, demonstrating that Dr. Carpenter's views set forth in the *Quarterly Review* article to which he so often refers are entirely opposed to facts. But as such evidence is systematically ignored, apparently because it cannot be answered, and would render the opinions of 25 years ago as untenable as from the advance of knowledge of this subject we might expect them to be. The most charitable view we can take of Dr. Carpenter's persistently ignoring or misstating all facts opposed to his own stereotyped theories is, that he is the slave of a 'dominant idea' – the idea that all such facts as we have adduced (and they could be multiplied a hundred-fold), whether witnessed by conjurors, physicians, or men of science, and however carefully investigated, are to be got rid of by the cry of 'delusion or imposture.' Dr. Carpenter himself assures us, however, that 'the subjection to a tyrannically dominant idea is monomania or insanity.'

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Mr. G. H. Lewes's Exposure of Mrs. Hayden (S265)

A letter printed in the Spectator issue of 23 December 1876.

Sir, – In your comment on Mr. Lewes's letter you seem to imply that the experiment described may prove imposture, but that Professor De Morgan's experiment was equally decisive against imposture. Will you allow me very briefly to point out that the alleged exposure proves nothing without assuming the very fact at issue – that Mrs. Hayden herself caused the raps following the indications given by the person who pointed to the letters of the alphabet? For let us assume, on the other hand, that the raps were, as alleged, caused by invisible beings, perhaps not superior in intelligence to Mrs. Hayden, and equally liable to be affected by insult or impulse, and that these beings could read, *more or less imperfectly*, the questioner's mind. Nonsense questions were asked these intelligences, and absurd or contradictory answers were *sought to be obtained* by dwelling on certain letters. These absurd answers were obtained. This is consistent with the supposition on two theories. Either the intelligence could read only the questioner's active desire for a certain answer while pointing to the letters, and accordingly gave that answer; or, if it were able also to perceive the question (though less vivid in the questioner's mind at the moment), it might well adopt the human principles of answering what would be impertinent questions in the only way they deserved an answer. It is a fact within my own knowledge, and is well known to all spiritualists, that both kinds of answers are obtained in private circles where any imposture is out of the question. Professor De Morgan's experiment on the other hand absolutely precluded imposture on Mrs. Hayden's part, since not only were the letters and pointer carefully concealed from her, but the answer, though correct, was in words which the Professor was not expecting. The one experiment was purely negative and inconclusive, the other positive; and I cannot understand how so logical a mind as that of Mr. G. H. Lewes can put the two results even in the same category, much less allow the negative evidence to prevail. – I am, Sir, &c., Alfred R. Wallace.

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Spiritualism (S266aa)

A letter printed in the 28 December 1876 issue of The Daily News (London). The people Wallace is discussing here are: Dr. William B. Carpenter; Henry Slade, a slate-writing medium; Alexis Didier, a professional clairvoyant; and Sir John Forbes, a physician who investigated Didier's claims.

I venture to hope you will allow me a few words of personal explanation. Dr. Carpenter's account of his séance at my house is substantially correct, but I most positively assert that I invited him as earnestly as I could both at the time and subsequently, to renew his visit, under the firm belief that had he done so two or three times, he would have witnessed the rising up of the protected table, which he admits would have been a remarkable phenomenon. My objection was, and is, that he conveyed the impression to his audience that the experiment referred to had not succeeded at all, when he knew of my positive statement that it had succeeded with me and many of my friends. Dr. Carpenter's failure to obtain this and similar tests does not arise from his 'atmosphere of incredulity,' but simply from his want of perseverance. He appears never to have continued a series of four or five sittings with any one medium and the same circle of friends, a condition which all inquirers know to be most essential to success. Would he do so even now (say,

for example, with Dr. Slade), I feel convinced that the negative results of his whole life, so far as this inquiry is concerned, would give way before positive facts; and he would then perhaps admit that the conclusive tests as regards Alexis, quoted by me, cannot be discredited by the failure of Dr. Forbes to obtain similar results.

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letter clarifying Wallace's position on Darwinism and spiritualism (S268a)

A letter printed in the Inangahua Times (Reefton, New Zealand) issue of 16 March 1877, in their "Miscellaneous News" column.

My attention having been called to a discussion in your paper in October last, in which both Captain Hutton[a] and 'Veritas' appears to misunderstand the points on which I differ from Mr. Darwin, may I be allowed briefly to explain my views on the special matters referred to. Firstly, then, I truly accept the doctrine of Evolution and the theory of descent as applied to the development of all organic forms, including man, while my objections refer solely to the assumption that no other agencies than 'spontaneous variation' and 'natural selection' have caused such development. Even Mr. Darwin now admits that there are such unknown laws or agencies at work, and those who deny this are more Darwinian than he is himself. As regards man, I hold that his descent from a lower animal is almost demonstrated; but I maintain that in his case there are plain indications that other causes have been at work in addition to those which have operated in the case of the lower animals. I also hold that there is much reason to believe in a radical change of nature having occurred in man in correlation with the development of the human form. This is a very different thing from 'not including man in the theory of descent,' imposed on me by Captain Hutton. As to my belief in the phenomena of Spiritualism proving that I am a bad logician, I would remark that if belief in facts or phenomena, after careful personal investigation, implies bad logic – merely because these facts are unpopular, and are disbelieved by those who have not investigated them – then all the founders of science have been illogical. I maintain, on the contrary, that the 'bad logic' is theirs who decide a priori what is and what is not possible, and ridicule the careful researches of men who, like Mr. Crookes, the late Professor De Morgan, Mr. C. F. Varley, and many others, have fully considered the sources of possible error or delusion, and yet, after long observation and repeated tests, have arrived at the conclusion that these phenomena are realities."

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Spiritualism and Conjurors (S271)

A letter printed in The Spiritualist (London) issue of 17 August 1877.

[a] Captain Frederick W. Hutton (1836–1905), New Zealand naturalist.

A short time since there was a note in *The Spiritualist* to the effect that Dr. Lynn was exhibiting "burlesques of spiritual phenomena at the Aquarium." From Dr. Lynn's antecedents it seemed probable that this should be so, but probabilities are not facts. From what a friend informed me I had my doubts, and I therefore visited the Aquarium a few days back, and, with four other gentlemen, went on the stage to take part in the *séance*. Your readers must be told that Dr. Lynn is not the performer, but a gentleman who is introduced as "a medium – a real medium;" and I must say I believe him to be one. We first sat at a table – a very common and undoubtedly genuine table, which I turned over and examined, and after about four minutes' sitting this table rose up full two feet from the floor, and floated horizontally round the stage twice, resisting my efforts to stop it. All our hands were *on* the table; it moved about rapidly but somewhat irregularly; and no wires or machinery had anything to do with it. To me the motion was exactly such as I have experienced, with a genuine medium, but more powerful. Then followed the cabinet *séance*, the "cabinet" being a baize curtain supported by four poles on a carpet-covered platform, raised about a foot above the stage, and having no connection with it. We examined it thoroughly, and it was absolutely above suspicion. The medium was tied, hands and feet, in the usual way in a chair, by two gentlemen, and almost all the phenomena which characterised the Davenports' performance were here reproduced, but with even *greater rapidity*. At the very same instant that the curtain was drawn, hands appeared over the top, and the moment they descended the curtain was drawn back, and we found the medium with feet and hands tied exactly as before. This was repeated in various ways half a dozen times. Then, for an instant, three figures appeared in the cabinet robed in white from head to foot, and the next instant they disappeared, the medium being found tied as before, with no possibility of concealing the white robes, to say nothing of the figures which were there. The medium's coat was also removed and afterwards put on again, his hands remaining tied, and one of the spectators who entered the cabinet had his coat-sleeves turned inside out, and the coat put on him again without his being able to give any account of how it was done.

Now, it seems to me that a very bad effect will be produced by telling the public that this is all imposture; for they will naturally say, "We see no difference between this performance and those which you tell us are real: if this is imposture, then all your alleged spiritual manifestations are imposture also." I trust that you, Mr. Editor, or some of your readers, who have had more experience of mediums than I have had, will visit the Aquarium theatre and tell us your impressions after going on the stage; and if you think it is all juggling, point out exactly where the difference lies between it and mediumistic phenomena. I must also add, that when I was there Dr. Lynn said nothing against Spiritualists or Spiritualism. Of course, he made his usual fun, and referred to the risk of prosecution if he *said* it was all done by spirits – a remark which his audience took as an excellent joke, but which might have another meaning.

There have been many cases in which genuine clairvoyance has been brought before the public as conjuring; and now that the exhibition of anything claimed to be spiritual manifestations is punished by the strong arm of the law, it is to be expected that some physical mediums will engage themselves to professors of legerdemain in order to secure peace and safety. If the phenomena I have described are produced by conjuring, it is clear that Dr. Lynn, who is a master of the art, could do it himself and thereby add to his reputation; but he does not, and, as I venture to think, *cannot* do so, and until some one up to the

tricks of conjurors really shows "how it is done," I shall continue to hold the opinion that we have here a case of genuine mediumship.

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Slate-Writing Extraordinary (S273)

A letter to the Editor placed in the 6 October 1877 issue of The Spectator. In this much-reprinted communication Wallace refers to two well-known spiritualist mediums of the period, Francis W. Monck and Henry Slade.

Sir, – I trust you may consider the following experiment worthy of record in your paper, because it differs from cases of abnormal slate-writing of which evidence was adduced at the trial of Slade, and because it affords a demonstration of the reality of the phenomenon and the absence of imposture from which there seems no escape. I confine myself to this one experiment, and narrate the essential facts only.

The sitting was at a private house in Richmond, on the 21st of last month. Two ladies and three gentlemen were present, besides myself and the medium, Dr. Monck. A shaded candle was in the room, giving light sufficient to see every object on the table round which we sat. Four small and common slates were on the table. Of these I chose two, and after carefully cleaning and placing a small fragment of pencil between them, I tied them together with a strong cord, passed around them both lengthways and crosswise, so as effectually to prevent the slates from moving on each other. I then laid them flat on the table, without losing sight of them for an instant. Dr. Monck placed the fingers of both hands on them, while I and a lady sitting opposite me placed our hands on the corners of the slates. *From this position our hands were never moved, till I untied them to ascertain the result.* After waiting a minute or two, Dr. Monck asked me to name any short word I wished to be written on the slate. I named the word "God." He then asked me to say how I wished it written. I replied, "lengthways of the slate;" then if I wished it written with a large or a small "g," and I chose a capital "G." In a very short time, writing was heard on the slate. The medium's hands were convulsively withdrawn, and I then myself untied the cord (which was a strong silk watch-guard, lent by one of the visitors), and on opening the slates, found on the lower one the word I had asked for, written in the manner I had requested, the writing being somewhat faint and laboured, but perfectly legible. The slate with the writing on it is now in my possession.

The essential features of this experiment are, – that I myself cleaned and tied up the slates, that I kept my hand on them all the time, that they never went out of my sight for a moment, and that I named the word to be written and the manner of writing it after they were thus secured and held by me. I ask, how are these facts to be explained, and what interpretation is to be placed upon them? – I am, Sir, &c., Alfred R. Wallace.

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Mr. Wallace and Reichenbach's Odyle (S276)

Complaints about Dr. Carpenter feature in this letter printed in the Nature issue of 1 November 1877.

I am amazed that Dr. Carpenter should think it necessary to make public, with such haste, Prof. Hoffmann's statement that Baron Reichenbach's facts and theories are not accepted by the body of scientific men in Germany. Of course they are not. But how this affects their intrinsic accuracy I fail to see. Less than twenty years ago the scientific men of all Europe utterly disbelieved in the co-existence of man with extinct animals; yet the facts adduced by Freere, Boué, McEnery, Godwin Austen, Vivian, and Boucher de Perthes, are now admitted to have been trustworthy and deserving of the most careful examination. The whole history of scientific discovery from Galvani and Harvey to Jenner and Franklin, teaches us, that every great advance in science has been rejected by the scientific men of the period, with an amount of scepticism and bitterness directly proportioned to the novelty and importance of the new ideas suggested and the extent to which they run counter to received and cherished theories. Rejection is one thing, disproof is another; and I have in vain searched for anything like disproof, or even rational explanation, of Reichenbach's facts: his theory, or "Odyle-doctrine," I have never "attempted to rehabilitate," as Dr. Carpenter, with his usual misconception, says I have done. In my review of Dr. Carpenter's lectures (*Quarterly Journal of Science*, July, 1877, p. 396), I adduce five tests employed by Reichenbach, and also the independent and simultaneous confirmation of Dr. Charpignon in France; and the only reply I get is: "All men of science disbelieve them." With the facts of history above alluded to in my mind, and believing that human nature is very much the same in the nineteenth century as it was in the eighteenth, I can only say, "so much the worse for the men of science."

Dr. Carpenter's reference to the believers in a flat earth, as a parallel case, is unfortunate, because the two cases are really of a totally different nature. Those who maintain the earth to be flat do not deny the main facts which we rely on as proving it to be round, but they attempt to give other explanations of them. The dispute is on a question of reason and inference; and every intelligent and fairly educated man is able to decide it for himself. But in Reichenbach's case it is the *facts* that are rejected without disproof or adequate explanation. The two cases are therefore quite distinct, and Dr. Carpenter's attempted parallel, as well as his setting up of scientific disbelief as a conclusive reply to evidence, is in conformity with his whole treatment of this subject.

I trust that such of the readers of *Nature* as may feel any interest in the questions at issue between Dr. Carpenter and myself will read my article above referred to, and not allow themselves to be influenced by Dr. C.'s repeated appeals to authority and to prejudice. – Alfred R. Wallace.

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Mr. Crookes and Eva Fay (S281: 1877)

Wallace's comments are again directed against Dr. William Carpenter in this 6 December 1877 letter printed in Nature. "Mr. Crookes" is the famous physicist

and chemist William Crookes, discoverer of the element thallium, later-to-be President of the Royal Society (1913–1915) – and also an early investigator of spiritualistic phenomena.

In Dr. Carpenter's eagerness to show that his statements about Mr. Crookes and Eva Fay had some basis of fact, he seems entirely to have forgotten the real issue which he has himself raised, and which is of great importance to all engaged in the study of these tabooed subjects. The question simply is, whether *any* investigation of the alleged abnormal powers of individuals, however painstaking and complete it may be, and however decisive its results, is to be branded opprobrious epithets, without any proof of error or fallacy, but merely on the dicta of newspaper writers and alleged "exposers."

In the case before us Mr. Crookes made certain experiments in his own laboratory, in which the greatest refinements of modern electrical science were employed; and of these he published a detailed account. That is the sum total of his acts and deeds in regard to Eva Fay. Yet because these experiments have been referred to in America as indorsing Eva Fay's remarkable powers, and because some persons charge her with being an impostor, and go through an alleged imitation of her performances, Dr. Carpenter accuses Mr. Crookes of encouraging "disgraceful frauds" and indorsing a "notorious impostor." Now it is clear that, to support this accusation, Dr. Carpenter must *prove* that Eva Fay was an impostor in respect to what happened *in Mr. Crookes's house*, and that, to use Dr. Carpenter's own words, she evaded his "scientific tests" by a "simple dodge." He must prove that Mr. Crookes exhibited culpable carelessness or incapacity in accepting, as conclusive, tests which were really fallacious; for, otherwise, how can Mr. Crookes be held responsible for anything which happened afterwards in America? Dr. Carpenter has promised to do this in the forthcoming new edition of his lectures; but as the accusation against Mr. Crookes has been made in the pages of *Nature*, and the question is a purely scientific one – that of the absolute completeness of the test of "electrical resistance" – I call upon Dr. Carpenter to explain fully to the readers of *Nature* the exact particulars of that "simple dodge" which is to destroy Mr. Crookes's reputation as a physical experimenter, and to sustain the reputation of his accuser. Unless the explanation is so clear and conclusive as to satisfy *all* the witnesses of the experiments that Eva Fay *did* evade the scientific tests, and that what they saw was simple conjuring, then Dr. Carpenter is bound to find a conjuror who will submit to the same tests as Eva Fay did, and produce the same phenomena before the eyes of the witnesses, so as to show "how it is done." Mr. Maskelyne, who professes to have exposed Eva Fay, will of course be ready to do this for an adequate remuneration, which I feel sure will be forthcoming if Dr. Carpenter is proved to be right and Eva Fay's "simple dodge" is clearly explained.

I have already shown (in this month's *Fraser*) that the supposed *exposure* of Eva Fay in America was no exposure at all, but a clumsy imitation, as will be manifest when it is stated that the exposer, Mr. Bishop, performed all his tricks by *stretching the cord* with which his hands were secured to the iron ring behind his back! There is hardly a greater exhibition of credulity on record than Dr. Carpenter's believing that *such a performer* proved Eva Fay to be an impostor and Mr. Crookes's experiments valueless. But what can we expect when we find a *Daily Telegraph* report quoted as an authority in a matter of scientific inquiry?

I venture to think that, whatever may be their opinions as to the amount of *fact* in the phenomena called "spiritualistic" (by Dr. Carpenter, but never by Mr. Crookes), all men

of science will agree with me that Dr. Carpenter is bound to *prove by direct experiment* that Mr. Crookes and his coadjutors were the victims of imposture on the particular occasion referred to; or if he fails to do this, that he should in common fairness publicly withdraw the injurious accusations he has made against Mr. Crookes and all who are engaged in similar investigations. If this is not done it is equivalent to deciding that no *possible* proof of such phenomena is admissible – a position which is not that of Dr. Carpenter, or, as far as I am aware, of the scientific world generally.

I beg to take this opportunity of apologising for my involuntary appearance under false colours in this month's *Fraser*. The letters "F.R.S." were added to my name after the corrected proofs left my hands and wholly without my knowledge. I have desired the editor to make a statement to this effect in his next issue, but in the meantime wish to set myself right with the readers of *Nature*.

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The Curiosities of Credulity (S285)

A letter printed in the 12 January 1878 number of the Athenæum.

Owing to absence from home I have only just seen Dr. Carpenter's letter in the *Athenæum* of December 22nd, to which I now beg leave very briefly to reply.

I must first remark on the extreme inconvenience of Dr. Carpenter's erratic mode of carrying on a discussion. As soon as his lectures on 'Mesmerism, Spiritualism, &c.,' were published, I wrote a review of them in the *Quarterly Journal of Science* of July last. To this Dr. Carpenter replied in *Fraser's Magazine* of November, promising a fuller reply to certain points in the new edition of his 'Lectures,' then in the press. As the article in *Fraser* was of a very personal character, I issued a rejoinder in the same periodical the following month. A discussion has also been carried on in *Nature*, and the scene of the contest is now removed to the *Athenæum*, many of whose readers are probably ignorant of its previous phases.

Dr. Carpenter comes before a fresh audience in order to reply to a specific charge of mis-statement which I made against him in the *Quarterly Journal of Science* (July, 1877, p. 398), which charge, as I will proceed to show, he endeavours to evade by a wordy defence, which really amounts to an admission of it. In his 'Lectures' (p. 71) is the following passage:

> It was in France that the pretensions of mesmeric clairvoyance were first advanced; and it was by the French Academy of Medicine, in which the mesmeric state had been previously discussed with reference to the performance of surgical operations, that this new and more extraordinary claim was first carefully sifted, in consequence of an offer made in 1837 by M. Burdin (himself a member of that Academy) of a prize of 3,000 francs to any one who should be found capable of reading through opaque substances. The money was deposited in the hands of a notary for a period of two years, afterwards extended to three; the announcement was extensively published; numerous cases were offered for examination; every imaginable concession was made to the competitors that was compatible with a thorough testing of the asserted power; and not one was found to stand the trial.

My readers will observe that this is deliberately stated to be the *first* time that clairvoyance was carefully sifted in France; yet it now appears that Dr. Carpenter perfectly well knew of the Commission of the same Academy about ten years earlier, which, after five years of most careful and elaborate experiments, gave a unanimous Report positively in favour of the reality of clairvoyance.

But Dr. Carpenter would have us believe that he studiously avoided all mention of this Report because it had been proved to be wholly founded on imposture or error; and he endeavours to establish this by giving a single hearsay case of a confession of imposture *on another person not even a member of the Commission!* I feel sure that the impression conveyed to the readers of Dr. Carpenter's letters would be that the case of alleged imposture by one of the mesmeric patients of MM. Georget and Rostan occurred to members of the Commission, and that the case had been examined by them and reported on as genuine. But this impression would be entirely erroneous. The members of the Commission, whose names are appended to the Report, are as follows: 1, Bourdois de la Motte (President); 2, Fouquier; 3, Gueneau de Mussy; 4, Guersent; 5, Itard; 6, Leroux; 7, Marc; 8, Thillaye; 9, Husson (Reporter). Against the voluminous and interesting details of this Report, its carefully repeated experiments, its cautious deductions, its amazing facts, not one particle of rebutting evidence is adduced. Yet Dr. Carpenter thought himself justified not only in ignoring its existence, but in giving his readers to understand, by an express form of words, that no such inquiry was ever made! This was the accusation I made against him, and the readers of the *Athenæum* can now judge as to the candour and sufficiency of the reply.

I must add a few words on the way in which Dr. Carpenter treats M. Rostan, "one of the ablest medical psychologists of his day." Dr. Carpenter states, as a fact, that, "when a second edition of the 'Dictionnaire de Médecine' came out in 1838, he (M. Rostan) withdrew the article he had contributed to the first"; and then, further on, it is stated that "M. Rostan, by his own confession," had been led away by cunning cheats in the matter of clairvoyance. Now I have always understood that M. Rostan was much annoyed at his article being superseded in the second edition of the Dictionnaire; and, as this is *à priori* probable, I require some direct evidence of Dr. Carpenter's assertion that he voluntarily withdrew it. This is the more necessary because the still more important and damaging statement – that M. Rostan made a "confession" that he had been led away by cunning cheats – is also given as a hearsay report without any reference or authority; and it looks very much as if Dr. Carpenter's logic had deduced the "confession" as an inference from the "withdrawal," no evidence whatever being offered for either of them. If this should really be the case, then the severest things I have said as to Dr. Carpenter's mode of carrying on this discussion will be more than justified.

Throughout my discussion of this subject with Dr. Carpenter I have strictly confined myself to questions of *fact* and of *evidence*, and have maintained that these are of more value than *opinions*, however numerous or weighty. My criticisms have, for the most part, been directed to misrepresentations of facts and suppressions of evidence on the part of my opponent. The readers of the *Athenæum* will now be able to judge, as regards one case, whether that criticism is sound; and for numerous other cases I refer them to my articles in the *Quarterly Journal of Science* and in *Fraser's Magazine*. If they read these, they will, I think, agree with me that the cause of truth will not be advanced by the further continuance of a discussion in which one of the parties perpetually evades or obscures the most important points at issue, and at every step introduces fresh mis-statements to be

corrected and fresh insinuations to be rebutted, as I have shown that Dr. Carpenter has done in his numerous writings on this subject.

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A Substitute for the Reincarnation Theory (S285a)

Wallace never became a supporter of the theory of reincarnation. This note was printed in The Spiritualist (London) issue of 25 January 1878.

While having a sitting a few days since with that excellent trance medium, Mr. Fletcher, I was informed that in most cases the connection between a person and his guardian spirit is one of mutual advantage. With very few exceptions those who quit this world have not obtained from it all that it is intended to supply of intellectual and moral training, and until they have done this they are unable to quit the earth and set out on the path of purely spiritual advancement. In order to obtain such training it is necessary that they should attach themselves to some person with whom they are both physically and mentally in harmony, and through him obtain the new ideas and ever growing knowledge of the earth-life. To this person they impart, by mental impression, ideas and impulses connected with matters of which they may have a more extended knowledge. These impressions can be best communicated while the recipients are in a somewhat passive mental state, and I was much struck by the remark that while our *first* ideas and impulses are usually our own (as no impression can be given while our minds are actively engaged), our *second* thoughts on the matter are often those of our spirit guides, such thoughts appearing to come to us, we know not how, at a time when we had ceased to think actively on the subject. The well-known saying, "Second thoughts are best," may thus have a deep spiritual meaning. Of course, it is not to be supposed that all second thoughts have this origin. The more thought we give to a subject the more likely we are to arrive at correct conclusions; but how often, after having as we think settled a question satisfactorily, an idea occurs which we describe by saying, "It has just struck me that I had better do so-and-so," and we do it and find it for the best; though while ourselves earnestly thinking of the matter this particular course of action had quite escaped us.

Now, if this intimate communion between a spirit and a man is as necessary to the former as it is beneficial to the latter, it may be described as a *spiritual* reincarnation by which the spirit is enabled to complete that preparation for a higher life which is, on the spiritual hypothesis, the sole explanation and justification of the existence of a material universe. And, further, if this *spiritual* reincarnation, the reality of which such ample proofs are given in the writings of every class of Spiritualists, does really serve the purpose of carrying on the earthly education of spirits, there can be no need for that actual *material* reincarnation of which fully one half the spirits who communicate with us profess their entire ignorance.

I have little doubt that these ideas have been already better expressed by others, but they certainly struck me as being both suggestive and novel, and coming as they professed to do directly from a spirit-guide, they were very impressive.

I may here add that my first sitting with Mr. Fletcher a few weeks ago carried to my

mind a fuller conviction of the reality of spirit life than all the physical phenomena I have ever witnessed. There was no *absolute* test, but I obtained information and advice which I value greatly, and the whole of the communications were given in a *form* which could not have been obtained from my own mind, even though most of the actual facts were within my knowledge.

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Psychological Curiosities of Credulity (S287)

In this letter printed in the 2 February 1878 issue of the Athenæum, Wallace responds to some comments of Dr. William B. Carpenter on mesmerism and related subjects.

Dr. Carpenter's letter in your last issue so entirely fails to meet the allegations in my communication to the *Athenæum* of January the 12th, that I should be content to let the matter rest. But Dr. Carpenter now makes fresh statements, which I maintain to be both inaccurate and misleading, and I therefore ask permission to point them out.

In the first place, it is untrue that I have ever "required" Dr. Carpenter "and every one else" to have "full faith" in the Academic Report of 1830. My sole objection was to Dr. Carpenter's ignoring its very existence. It is also untrue that the French Academy of Medicine "deliberately reversed the judgment" of its first Commission, "as having been obtained by fraud and chicanery." The second and third Commissions were of more limited scope than the first; their conclusions were mostly negative; and neither they nor the Academy itself in any way pronounced judgment on the first Commission, as Dr. Carpenter's words imply that they did. The subject having thus been again brought to the notice of your readers, I beg leave to offer a few remarks on the nature of the investigation by the first Commission of the French Academy of Medicine, and on the value of the evidence it affords.

We may certainly assume that the members of this Commission fairly represented the best medical talent of France at that time, for they were chosen by a representative society of doctors, who would carefully select men of exceptional acuteness, caution, and judgment, to investigate a subject about which there was so much dispute as mesmerism, or, as it was then termed, magnetism. That the members of the Commission were quite aware of the chief sources of error, and were not led away by any contagious enthusiasm, is evident from their Report itself. Thus, they declare that many of the effects ascribed to magnetism are produced by "expectation," "ennui," and by "the imagination"; that the effects are "very varied in different individuals"; and that some of the phenomena may be simulated and "furnish charlatanism with the means of deception." It is interesting to note that the patient first submitted to examination by M. Foissac (on whose proposition the Commission was appointed) turned out a complete failure; and had the Commission been hasty in arriving at conclusions, its report might have been as adverse to mesmerism as those which succeeded it. But the inquiry was continued, and other patients were found who submitted to every possible test. Finally, the members who attended the experiments, nine in number, *unanimously reported, after five years of inquiry*, "that magnetization without the knowledge of the patient; pre-vision of organic phenomena; knowledge of the internal

condition of other persons; and true clairvoyance, had been demonstrated to them." One of the somnambulists determined correctly the symptoms of M. Marc, a Commissioner; and also the disease of another person, the accuracy of the diagnosis being confirmed by *post mortem* examination. Clairvoyance was proved by one of the patients repeatedly reading and naming cards, while four of the Commissioners successively held his eyes closed with their fingers, – a test, the absolute conclusiveness of which every one may satisfy himself of. These are mere illustrations from among dozens of similar cases.

Now are we to believe that nine eminent medical men, investigating this subject at great length, and reporting on it with a full sense of responsibility, could possibly have been the victims of imposture and delusion throughout the whole inquiry, without any one of the nine so much as suspecting its possibility? That they had full confidence in their facts is shown by the following extract from the concluding portion of their Report:

> Certainly we dare not flatter ourselves that we shall make you share entirely our conviction of the reality of the phenomena which we have observed, and which you have neither seen, nor followed, nor studied with or in opposition to us. We do not, therefore, exact from you a blind belief in all that we have reported. We conceive that a great part of the facts are so extraordinary that you cannot grant it to us; perhaps we ourselves should have refused you our belief, if, changing places, you had come to announce them before this tribunal to us, who, like you at present, had seen nothing, observed nothing, studied nothing, followed nothing of them.

Dr. Carpenter wholly disbelieves the facts and conclusions of the Commission, as well as the whole mass of subsequent confirmatory testimony. This he is quite free to do, without any objection on my part. I only object to his first denying (by implication) the very existence of the Report, and then giving illogical and inadequate reasons for so doing. These so-called reasons are, that the Academy did not formally adopt the Report; that two subsequent Commissions *failed* to obtain confirmatory evidence, and *did* detect some attempts at imposture; and that a woman made a death-bed confession that she had, in some unexplained way, tricked two doctors who were not members of either of the Commissions.

But the first Commission also had "failures"; it also recognized "imposture" or attempts at imposture; but besides these it obtained absolutely conclusive facts, which have subsequently been often confirmed, but have never been satisfactorily explained away. To take the ground that clairvoyance is altogether impossible, and therefore incredible on any evidence, would be an intelligible position; but to admit that it is a question of evidence, and then to reject such direct, positive, and weighty evidence as that of the Commission of 1825–31 on the mere negative grounds above referred to, is utterly unintelligible; for to do so is to place ignorance above knowledge, and to estimate negative as superior to positive results.

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Materialisation and Exposures (S356)

A letter printed in the 7 October 1882 issue of the spiritualist magazine Light.

Sir, – Allow me to say a few words on an aspect of this question which none of your numerous correspondents have dwelt upon sufficiently, but which appears to me to lie at the very root of any sound judgment on the matter.

It seems to be admitted by Spiritualists generally that there is such a thing as genuine materialisation, by which is meant the appearance of visible and tangible human forms, in the presence of a medium, which are yet not ordinary human beings; and that this is the case when such forms are witnessed by several persons at once, the medium being at the same time visible, or when the body of the medium can be felt, or, still more clearly, when these forms are seen to come into being and to vanish away again in proximity to the medium. Hundreds of Spiritualists have witnessed some of these phenomena, as I have myself. They have occurred with a considerable number of different mediums, under the most varied and rigid test conditions; and with none, perhaps, more frequently, for a longer period, and testified by more trustworthy witnesses, than with Miss Wood.

But with most of these mediums, similar forms in all respects to those just referred to also appear under less perfect test conditions, that is, when the medium is concealed from view, and usually fastened in some way supposed to prevent the possibility of voluntary or involuntary personation. In some of these cases what appears a good test is obtained by the form being palpably larger or smaller than the medium, sometimes confirmed by exact measurement; or by the medium being found in a state of trance and in his or her usual dress a few seconds after the white-robed and, perhaps, bare-footed form had disappeared in the cabinet. Yet in none of these cases, usually deemed quite satisfactory, have we any proof that the form was separate from the medium, because it is known to all investigators that mediums can be loosed from any bonds or from clothing, and re-introduced into them, and that the medium's body can be *transfigured* and made to assume a totally distinct aspect, and much larger or smaller dimensions, than those natural to it. The elongation of Mr. Home, and the American medium (Mrs. __), whose body was found to be absent from the cabinet in which her clothes remained, while forms of men, women, and children successively came out of it, will occur to the memory of all acquainted with Spiritual literature.

Now in all these phenomena conditions determine the nature and character of the manifestations; when the conditions are highly favourable forms can be produced apart from the medium; when less favourable they can only be produced by releasing and transfiguring the medium; and the latter is in many cases only one degree less marvellous than the former. Of this latter class I have seen very many examples with several different mediums, tested either by close examination of the countenance, by seeing the medium a few seconds after the form had retired, by accurate measurement of the form and of the medium, or by a rigid search of the medium's body and surroundings immediately after the séance, when no particle of the copious draperies and headdresses of the forms which had appeared could be found; yet I think it not only possible, but highly probable, that in some or all of these cases the form *was* the medium, transfigured, under conditions which rendered it a marvellous exhibition of super-human agency.

Now if in any of these latter cases the form had been violently seized and found to be the medium, free from bonds and divested of a portion of his or her clothing, that fact would not in any degree have affected the genuineness of the phenomenon as regards the medium; but if this had been done before the *tests* above enumerated had been applied the opportunity of getting such tests would have been lost, and an undeserved stigma thrown upon an innocent person. But we may go further than this, and maintain on substantial

grounds the extreme probability that if a form is seized which is really distinct from the medium, yet the result may be that the form and the medium will be forcibly brought together, and a false impression conveyed that the form *was* the medium.

For what do we know of this most stupendous phenomenon of a more or less solid, visible form being produced, except the constant conditions under which it appears and the uniform statements of the agencies who produce it? Whenever the form is seen in process of formation it grows, or seems to grow, out of the body of the medium or in close proximity to it. To depart, it goes back to the medium. A common phenomenon is the temporary return of the form to the medium "to gain strength." A well attested phenomenon is, that marks made on the form or injuries done to it appear on the body of the medium. In the case of Dr. Monck, food taken by the form reappeared in the mouth of the medium, visibly to several witnesses altogether above suspicion. The weight of these forms fluctuates greatly, and there is some evidence to shew that the weight of the medium is simultaneously diminished. These facts all prove a most intimate and real connection between the form and the medium, a connection utterly beyond the powers of our material science to understand; and what applies to the *matter* of the forms seems to apply equally to the *matter* of the drapery in which the forms usually appear. We are *told*, also, that the forms are produced mainly from the body or aura of the medium, and that they must return to it or injury will result. And this is so likely in itself, and so accordant with all the facts we can observe, that we are bound to accept it, at least till we know more of the matter. Yet those who think the reality of materialisation can be tested by seizing the form and keeping it away from the medium, must ignore all these considerations, and believe, apparently, in the teeth of all the evidence, that the forms are real independent entities, who come from some other world, who have no connection whatever with the medium (except as somehow helping them to come to us), and who can be kept by force away from the medium for our examination and amusement! And, what is to me more strange still, a considerable number of advanced Spiritualists, including many who are looked up to as teachers of the unenlightened, adopt this improbable and unfounded hypothesis, and cry out with horror at the immoral imposture, either of the medium or of the beings who produce the phenomena.

It is my deliberate conviction – accepting the reality of these phenomena as witnessed personally and as narrated by a crowd of competent observers, and deducing from them the obvious and close material and spiritual connection of the form and medium – that the fact of a person seizing the form and finding the medium, is not and cannot be a proof of fraud, but is, in all probability, *the natural and inevitable effect of trying to keep apart two beings who are really one, and who cannot be rudely interfered with without danger to the human body, whose abnormal organisation supplies the material for the temporary materialisation of these marvellous existences.* If this were only one out of many alternative deductions from the facts and teachings before us it would be both cruel and unwise to choose always the least favourable interpretation, and as a consequence of doing so to attach the stigma of imposture on persons who have for many years borne a good character, and who are known to be true and powerful mediums. But when, as I maintain, this hypothesis is the only one which the facts directly support, it follows, that the only "exposure" made is of the incapacity of experimenters who deal with the greatest mysteries of an unknown universe as if they were mere questions of terrestrial mechanics.

So far as I remember, every medium for materialisation, however perfectly their powers may have been tested, however good their character, has been subjected to accusations

of fraud on somewhat similar grounds to those now alleged against Miss [C. E.] Wood. The fact that whenever a form has been seized it has been found, *after a struggle of some moments' duration*, to merge into the medium, lends additional support to the view here advanced, since on the mere doctrine of chances there ought to have been, by this time, at least, one genuine form seized, if such a capture is possible. I have shewn reasons for thinking it *not possible*, and I believe that, even where the medium and form have been seen separate, a forcible seizure would result in their speedy amalgamation.

I feel sure that many who have studied these phenomena most closely will be satisfied that my views are in harmony with the facts taken in their entirety; and I have thought it now opportune to explain my reasons for them (however briefly and inadequately), because, if accepted, they will afford the best antidote to the crude notion that "seizing" a materialised form can ever test its genuineness. – Alfred R. Wallace.

P.S. – I will add one word more of practical suggestion. It has been asserted by one of your most eminent and esteemed correspondents, that "not one person in fifty can tie a woman so that she cannot get out of the bonds by normal means, if she is a practised trickster." This I entirely dispute. There are three portions of the body from which a close band cannot possibly be slipped – the neck, the wrist, and the ankle. Tie a tape firmly and closely round a woman's waist, knot it hard behind over a piece of card, and seal it to the card, then carry the ends either to a chair, or any other piece of furniture, and tie and seal again, and though the person will be not the least inconvenienced and the hands free, no trickster in the world can escape from the tape, *and then replace herself in it again*, even if she were in the light instead of being in darkness. If, under these conditions, a form appears, and afterwards the medium is found secured as at first, we have positive proof of superhuman power. We have no proof that the form was not the medium; *that* we can only get by seeing or feeling the two simultaneously; but the above simple mode of tying is absolute security against the bugbear of trickery by genuine mediums. – A.R.W.

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The "Journal of Science" on Spiritualism (S382)

Printed in the 11 July 1885 issue of Light (London).

My article on the "Harmony of Spiritualism and Science," written for an American newspaper, and republished in "Light" of May 30th, has been honoured by a notice in the *Journal of Science,* and I have been requested to make a few remarks in reply to the same. I cannot say that I myself think the criticism worth answering, because it is founded on assumptions which will, I am sure, not be granted by men of science in general; still, as they may present difficulties to some readers, it is perhaps as well to show their weakness.

The writer's main and fundamental objection is stated as follows: –

> Science is based upon what we, for want of a better name, term *law*. Spiritualism rests upon *will*. Science, and not merely our present science, but any possible science, so far as I can conceive it – takes its stand upon the causal nexus, upon the regular sequence

> of cause and effect. Iron always sinks in mercury, and always dissolves in hydrochloric acid, &c., &c. . . .

In this passage and in what follows, the term "science" is completely misused. It is taken as synonymous with a limited branch of science, namely – physics. There are, however, whole regions of science in which there is no such regular sequence of cause and effect and no power of prediction. Even within the domain of physics we have the science of meteorology in which there is no precise sequence of effects; and when we came to the more complex phenomena of life we can rarely predict results and are continually face to face with insoluble problems; yet no one maintains that meteorology and biology are not sciences – still less that they are out of harmony with or opposed to science. The absence of uniformity, and the impossibility of predicting what will happen under all circumstances are not, therefore, confined to Spiritualistic phenomena alone. Assuming that they are so, however, the writer thus continues:

> With the advent of Spiritualism all this beautiful simplicity has been swept away. If Spiritualists are not mistaken there are around us numbers of finite invisible beings, of unknown powers, and of unknown intentions capable of interfering with the order of nature. They can raise bodies in the air against the force of gravitation. They can kindle fires at pleasure, or deprive fire of the power of destroying organised beings or of occasioning pain. . . . To me it seems that, if these contentions are true, if there exist beings around us capable of exerting such powers, there are introduced, so to speak, into every equation a number of unknown quantities, rendering it for ever insoluble. We can only say 'such results will follow under such conditions, if no spirits think proper to interfere.' It seems to me that before any harmony can be shown between Spiritualism and science it must be ascertained what are the limits of the powers of these 'spirits' and under what conditions can they be exerted. In that manner only can a basis for science be saved.

In this passage there are both misstatements of fact and illogical conclusions. There is little or no proof that the "spirits" around us can of *themselves* do any of the things alleged. They require in almost every case, perhaps in every case, the assistance of human beings, and not only so, but of particular human beings with special organisations – those we term mediums. Here at once is a limitation to their power, and so great a limitation that the cases in which they can interfere with the ordinary effects of natural law are but very rare exceptions. Unless specially sought after, not one person in a thousand ever comes in contact with these phenomena, and even when sought for the general complaint is that they are exceedingly hard to find. To maintain that all science is impossible because once or twice in the lives of one person in a thousand some interference with the ordinary course of nature may occur, is about as sensible as to maintain that agriculture is impossible because phenomenal hailstorms may destroy, or exceptional whirlwinds may carry away, crops, or to give up all quantitative astronomical observation because earthquakes or terrestrial tremors, which cannot be predicted, may alter the level or the orientation of the instruments. And when we come to vital, and mental, and moral phenomena, we are still more subject to "unknown quantities in our equations." The apparently healthy man dies suddenly, while one who has always been weak and ailing lives to a good old age. The sober, moral, and religious citizen suddenly commits a horrible crime. The man of commanding genius becomes hopelessly insane. Yet these terribly real "unknown quantities" do not render either vital, or mental, or moral science impossible, still less do they place these studies altogether outside of science and in antagonism to it.

Again, as regards the impossibility of any science, as the critic alleges, where *will* intervenes, we have the human will as a constant factor in sociology, in anthropology, in ethical science, in history, in psychology, yet no one maintains that all these studies are opposed to science even if they have, as yet, no claim to rank among established or exact sciences.

Now, so far as we know, the *will* of spirits is no more erratic in its manifestations than the will of living men. It appears to be equally subject to general laws and influences, and, on the average, no more affects the orderly sequence of Spiritualistic phenomena than do the individual wills of human beings affect the orderly sequence of mental, social, or moral phenomena. It is a great mistake to impute all the uncertainty of phenomena with mediums to the erratic *will* of the spirits concerned. Very little is probably due to this cause, while the greater part is certainly owing to what may strictly be termed terrestrial conditions. We know something of these conditions already, and when we know more we have every reason to believe that much of the uncertainty will cease. Not less unsatisfactory is the remark with which our critic concludes this part of the subject: –

> To harmonise science with Spiritualism it will then be, in the first place, necessary to discover the limits of the power of spirits, under what conditions it is exerted, and how it may be combated when and where it is desirable.

But in all these respects Spiritualism is fully as advanced as is science itself. We know, practically, the limits of the power of spirits on this earth at the present day, and under ordinary conditions, quite as well as we know the limits of the power of earthquakes and volcanoes, of disease, of insanity, and of human intellect, and we know how to combat their evil effects quite as well in our domain of observation as do men of science in theirs.

Then we have the bugbear of the "creation or destruction of energy" in Spiritualistic phenomena brought forward, and we are told that scientific men will seek for "precise answers" to the question where the power comes from "before they can accept the Spiritualistic theories." But nobody asks them to accept the Spiritualist theories before they have investigated the Spiritualist facts.

It has usually been the boast of science that it accepts, and co-ordinates, and studies *all* the facts of nature in order to explain them; but with respect to our facts it applies a different rule and asks for a complete theory – a "precise explanation," before it will even begin to study them. We are informed that, in order – "To establish a harmony between Spiritualism and science it will be necessary, I submit, to show the origin of the energy which is at the disposal of spirits." But science itself does not yet know the "origin of the energy" of gravitation, yet the theory of gravitation is its proudest boast. Science only guesses at the "origin of the energy" of the magnet; and in tracing all terrestrial energy to the sun it only removes the difficulty one step, and cannot do more than make more or less probable guesses as to where the energy of the sun comes form. It is surely not scientific to demand of a new and very difficult science the complete solution of its most fundamental problems as a preliminary to recognising its existence, yet this is how the writer in the *Journal of Science* proposes to treat the students of Spiritualism.

The last passage I shall refer to is that in which the critic considers that Swedenborg was the victim "of delusion or imposture," because, while describing Jupiter and Saturn he said nothing about Uranus or Neptune. The assumption underlying this argument is, that if spirits exist and communicate with men they *must necessarily* know more of the

material universe than men do, and *must* communicate their superior knowledge to us. This extraordinary misconception well illustrates the tone of mind of the writer, who has evidently given very little attention to the theories and conclusions of the more advanced of modern Spiritualists. He has yet to learn that the facts of Spiritualism are one thing, the value of the information obtained from Spiritualistic sources quite another thing. It is marvellous that so many people who deny that we have any evidence whatever of the existence of spirits, yet claim to know *à priori* exactly what spirits ought to know and ought to tell us, if they do exist!

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The Harmony of Spiritualism and Science (S383)

A letter printed in the 25 July 1885 issue of Light (London), responding to criticism of Wallace's essay of the same title.

I find some difficulty in comprehending the exact position of Mr. Frederick F. Cook in his elaborate "Rejoinder" to my article, but with your permission I will briefly notice his direct criticisms of my views, because they have a certain amount of plausibility owing to the extremely condensed form in which I was compelled to express myself in the space that was allowed me.

Mr. Cook first objects to my proposition that – "man consists essentially of a spiritual nature or mind intimately associated with a spiritual body or soul, both of which are developed in, and by means of, a material organism." This, he says, is a case of spiritual suicide, and is directly opposed to my previous statement that – "mind is the cause of organism and perhaps even of matter itself." But surely, it is clear that in the last quoted passage I am speaking of mind in the abstract or as a fundamental principle, while in the former I am dealing with mind as individualised in the human form. There is, I conceive, no contradiction in believing that mind is at once the cause of matter and of the development of individualised human minds through the agency of matter. And when, further on, he asks, "Does mortality give consciousness to spirit, or does spirit give consciousness for a limited period to mortality?" I would reply, "Neither the one nor the other; but, mortality is the means by which a permanent individuality is given to spirit."

His next serious objection is to my supposition that, "it may well be that evolution is a fundamental law of the universe of mind as well as that of matter." This, he says, is a purely materialistic thought. But here again it is clear by the context that I am referring solely to the development of individualised human minds, of which alone we know, or can know, anything, not to mind in the abstract, of which we know absolutely nothing; and I see no materialism in the supposition that such finite individualised minds can only be produced under some law of evolution.

The last special criticism refers to my belief that "progress towards a nobler and happier existence in the spiritual world" is dependent on the cultivation of our higher moral feelings here. My critic says that this is an utter denial of justice or equality, because our moral nature, as well as our environment, is imposed upon us; but he does not say whether he accepts the alternative position, that all are to be at once good and happy in the future state, and that the most selfish, vicious, and sensual are to make equal progress with the

benevolent, self-sacrificing, and virtuous. It seems to me that this latter condition of things would be the most opposed to justice, and even to possibility, and would render the present world, with all its trials, a hopeless and insoluble mystery, while it is certainly opposed to the whole body of information and teaching which we receive from spiritual sources.

It seems to me that my critic, throughout, confuses together the general with the special, the universal with the individual, in discussing the relations of spirit and matter, while he equally confounds proximate with ultimate results in his remarks on the spiritual world. My observations and reasonings have been confined throughout to the nature and relations of individualised human minds and their proximate condition in the spirit world. Speculations on the nature or origin of mind in general as well as those on the ultimate states to which human minds may attain in the infinite future, I look upon as altogether beyond the range of our faculties, and to be, therefore, utterly untrustworthy and profitless.

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Letter from Dr. Alfred R. Wallace, In Re Mrs. Ross (S396)

A famous letter printed in the 5 March 1887 issue of the Boston spiritualist newspaper The Banner of Light. In it Wallace discusses two séances he attended with the philosopher William James.

In Prof. James's letter, published by you last week, he refers to myself as having been present with him at two séances at Mrs. Ross's when he believes there was "certainly roguery." In order that my silence may not be interpreted as implying that I accept this view, I ask leave to make a few remarks.

Prof. James adduces a certain number of circumstances which seemed to him suspicious. My own experience of materializations extends to about twenty séances with five different mediums, under the most varied conditions and tests, and I am satisfied that such suspicions as Prof. James adduces are absolutely worthless as evidence. When from such "suspicions" – which are very different from proofs – he arrives at the conclusion that there "*certainly* was roguery," he seems to me to exhibit such an unphilosophical frame of mind as to deprive his opinion of the value it might otherwise possess.

With respect to the two séances at which I was present with Prof. James, I will adduce a few *facts* as opposed to his *suspicions*. The usual, and I believe almost universal practice at Mrs. Ross's séances, is to have the sliding doors between the front and back rooms closed, and, if desired, sealed. If, therefore, confederates get into the room, they *must* enter by some secret opening into the cabinet. At our first séance the doors were left open, *at Prof. James's special request,* in order to render it impossible for confederates to enter from the back room, and I was invited to sit in the opening. This departure from the usual course, *at request of a visitor*, after the rooms had been well searched by a party of sixteen persons, and just before the séance began, would alone satisfy most persons that confederates were not employed, since, their supposed ordinary mode of ingress being rendered useless, they could not take part in the performance. Prof. James thinks, however, that they could have entered the back room noiselessly, and could have slipped close

past me into the cabinet, unperceived by myself or by any other person. I myself am positive this could not have been done; and I am also sure that the female figure in white, which, as Prof. James says, came out to me "the moment the séance began," was not Mrs. Ross (unless completely transformed in size and figure), as I held her hand and looked closely into her face. But, on Prof. James's theory, it must have been Mrs. Ross, since no other person was at that time in the cabinet.

At the second séance the doors were shut and sealed, and the confederates, if any, must have entered the cabinet itself by some secret opening. Seven distinct figures appeared, varying in size from a tall man down to a baby. Now, in order to account for the presence of these figures, Prof. James makes two statements, which I invite him to prove experimentally. First, he says "good carpentry can make a secret door in any wall." Many persons, thinking of secret doors in cabinets and in wainscotted rooms, will hastily assent to this proposition; but the wall in question is papered down to the mopboard eight inches above the carpet, and on the opposite side it is smoothly plastered down to a four-inch board. I ask Prof. James to produce anywhere a secret door *in such a wall* which some one of six intelligent men, having access to both sides of the wall, shall not discover in five minutes, and I submit that unless he has *seen* such a secret door that cannot be detected, his statement is unfounded and misleading, and ought not to have been made.

His second statement is, that such secret door can be unmade in forty-eight hours – of course so that the unmaking cannot be detected. Here again I invite him to produce new woodwork, new paint, new putty over nailholes, and new plaster and paper, which cannot be detected as being new work by some one of six men of average intelligence after five minutes' examination.

It is by such thoughtless statements as these that most of the accusations against mediums are supported; but when they are made by an investigator, who claims to be both unprejudiced and scientific, they should be either upheld by an appeal to facts, or unreservedly withdrawn.

These remarks apply equally to the mythical mopboard door, the only means of ingress and egress for confederates alleged by the exposers. Even if it were not demonstrated by the careful examination of Dr. Moore and others, as given in your paper of the 19th inst., that there has not recently been any secret opening in the place referred to, no person of common sense could believe that a slit eight inches wide on one side, and four inches on the other, could allow of the noiseless and rapid ingress and egress of full-grown men and women, besides children, night after night, without hitch or detection.

I am, myself, as anxious as Prof. James to have the whole truth of this matter brought to light; but I am not, as he seems to be, satisfied with evidence which would be valueless in a court of justice. Of course, if the whole thing is held to be incredible, *because impossible*, there is no need for any evidence or for any exposure. But this is not Prof. James's point of view. He claims to be an unprejudiced investigator, who, by the very fact of being an investigator, admits the *possibility* that the phenomena of materialization may be produced otherwise than by imposture. What, then, is the evidence on which he founds his accusation against the Ross family of being a "gang" whose fraud *has been exposed*?

Twelve gentlemen go together on purpose to expose, and by their superior force are able to do what they please; yet, up to this date, we have no statement by them, or on their behalf, which is not either disproved by facts or quite consistent with the forms seized being what they profess to be. None of the alleged confederates were secured, or any steps taken to identify them. They have all vanished into space, and the "ghostly muslin"

with which they were said to be draped has vanished likewise. The "frightened children" said to have been found in the cabinet were not asked for their names, or the addresses of their parents, so as to secure their appearance in a court of justice, if required. The tall Indian, the two young men and the two children, are all as if they had no existence! If it is asserted that they *must* have been human beings from the mere fact of their visible and tangible appearance, then no other evidence was needed; but if imposture is to be *proved* – not merely *asserted* – then we have a right to ask for some material and producible evidence of the existence to-day of the five alleged confederates; and not a particle of such evidence is given us! Again, we are entitled to ask, Was the mopboard story an observed fact or a mere inference? Will any one of the twelve gentlemen give us the exact particulars of this marvelous secret door, which twenty-one other gentlemen declare, after careful examination, to have left no traces of its existence? Will they tell us how wide and how long it was? Did it open with a hinge or by sliding? and if the latter, did it slide up, or down, or sideways? What was the character and size of the corresponding secret door into the cupboard in the back room, of which there in now no trace? Did any one of the twelve exposers themselves pass through this opening, or even put their heads or their arms through it, so as to prove that it really existed? – that it was, in theatrical language, "a practicable passage" for men, women and children? All this is of the essence of the question, whether the forms seized temporarily, but none of them retained or identified, were actual human confederates, since it is admitted that only by some such opening could confederates have entered the room. Yet up to the present time we have no single fact of this kind clearly alleged by eye-witnesses; and we are accordingly forced to conclude that these twelve gentlemen, who went specially to expose an imposture, came away without any careful examination of the one thing which would confirm their story!

Under these circumstances, I wait for fuller and more precise statements of what occurred at this now celebrated séance, before I can accept Prof. James's *dictum* that Mr. and Mrs. Ross, with at least five confederates, form a gang of unconvicted impostors.

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Nellie Morris (S411)

One of Wallace's contributions to a private correspondence among Wallace, E. Vaughan Jenkins, and Eleanor M. Sidgwick (Mrs. Henry Sidgwick) on the "spirit entity" Nellie Morris; multiple letters from all three parties were later printed in the October 1888 issue of the Journal of the Society for Psychical Research.

Dear Sir [Mr. E. Vaughan Jenkins], – You have evidently formed erroneous ideas of what "materialisation" is. No Spiritualist believes it to be "the real body" of the individual, or even "a real body" in one sense of the term. It is *something* temporarily material for purposes of identification; but what exactly no one can tell. All the information we can get shows that it is formed partly (often chiefly) from the body of the medium, partly from the bodies of the persons present, or from their "atmosphere" or emanations and that the likeness to any individual is produced by an *effort* which is not always successful, since, during the same evening, the same spirit-form sometimes appears in very different degrees of likeness to his mortal body; sometimes more like the medium, hence many of the

accusations of imposture. A little book called *Materialised Apparitions*, by Mr. E. Brackett, published at Boston, and which you can probably get from Burns, Southampton-row, will give you much information on the nature and peculiarities of these forms and the conditions under which they appear. I met Mr. Brackett in Boston, and can testify to the honesty, ability, and earnestness of the man and of his book. The *permanent materialisation* of hair and portions of garment is very extraordinary. Sometimes such things *do* vanish away, either rapidly or gradually, but in other cases both remain. The *hair* I had here, but have now returned it to General Lippitt. All we can at present do is to make sure of *the facts*. The laws of the phenomena we may never know till we are spirits ourselves, and not, perhaps, even then. Can we tell, *really*, how we move our hands and fingers to write and express our thoughts? Spirits do not appear to be able to tell us *how* they materialise. It is a faculty exercised by the will-power of *some* spirits, and is probably quite as rare and remarkable and inexplicable among them as physical mediumship is among us. – Believe me, yours faithfully, Alfred R. Wallace, Firth Hill, Godalming.

P.S. – The appearance of the double of any living person, sometimes to two or more witnesses, seems analogous to materialisation, and the person whose double appears has no conception *how it is done*. Neither have the spirits who materialise, except that it seems to be more directly a matter of *will* with them. See *Phantasms of the Living*. – A. R. W.

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Mr. Angelo Lewis and Dr. Monck (S416)

This letter to the Editor concerning the medium Francis W. Monck was printed in the October 1889 issue of the Journal of the Society for Psychical Research.

Sir, – In the last number of the *Journal* you print a letter from Mr. Angelo Lewis in which occurs the following passage:

> One of Dr. Monck's stock feats was to place a musical-box on the table, and cover it with a cigar-box, after which it played or stopped playing at command. Suggested explanation, 'Spirits.' Real explanation, that the box in question did nothing at all, the sound being produced by a second box strapped to Monck's leg above the knee (inside the trouser), and set in motion by pressure against the under surface of the table.

Now, as you refuse to accept any evidence of spiritual phenomena on vague hearsay, I think you should equally refuse to accept or print such vague accusations as this. Does Mr. Lewis mean by "real explanation" that he himself actually discovered a box tied to Dr. Monck's leg in the way described? Or, merely, that in his own imitations of the phenomenon he uses one so tied? If the latter, I submit that he had no right whatever to use the term "real explanation" or to treat Dr. Monck as an impostor in this matter. I happen to have witnessed the phenomenon myself, and I can declare positively that Mr. Lewis' account of it is incorrect. The following is an extract from my notes made at the time: "September 21st, 1877. Séance at Mr. C. Reimers', 6, Manor-villas, Richmond. Present – Dr. Monck, Mr. and Mrs. Bennett, Dr. Malcolm, Mrs. Firmin and sister, Mr. Reimers, Mr. A. R. Wallace." (The last of an extraordinary series of phenomena is de-

scribed as follows.) "A musical box, or rather the working part taken out of its box, was placed on the table laid on a sheet of white paper. It then played and stopped when requested, and this took place when I placed my hand lightly on it. Under these conditions it played and stopped just as I desired, Dr. Monck's hands being at a considerable distance on the table. Each person in succession placed his hands on the box and *felt* it play or stop when desired, thus proving that it was not *another box* under the table which played."

On this occasion, then, there was no cigar-box to hide the musical-box and muffle its sound, while the hands of the spectators assisted their eyes and ears in declaring that the actual box before them played and stopped at command.

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Is Pre-existence a Necessary Corollary of Future Existence? (S422a)

A letter printed in the 14 June 1890 issue of Light (London).

Sir, – Mr. Paice says that to him and others "previous existence is as certain as the after existence which we hold to be proved"; and other writers in "*Light*" have expressed the same opinion, and have urged that an infinite future implies, logically, an infinite past. Like yourself, Mr. Editor, I am quite unable to see this; while the difficulties in the way of accepting any such universal law appear to me to be insuperable.

I should like to state a few of these difficulties. If a personal existence, which persists after physical death, implies necessarily a personal existence before physical birth, it can only be on the metaphysical ground that a life which is to have no end cannot have had any beginning, and this is the ground on which it has been urged. But, if so, we are met with this difficulty – either the life we now possess has grown or developed to its present condition, or it has not. If it has so grown or developed, then, however slow that growth may have been, yet in an infinite past it must have reached infinite development. With progress or growth of *any* kind whatever, a finite and measurable development necessarily implies a finite period of growth – that is, a beginning; and if so, that beginning may just as logically be at each person's birth into this world as at any far removed period of past time.

If, on the other hand, we are not growing or developing beings, then our existence, in our present grade of imperfection, throughout a past and for a future eternity, is as near an approach to a logical and unthinkable absurdity as anything can be.

Development, however, will probably be granted by Mr. Paice and those who think with him; and, therefore, I submit, eternal pre-existence is a contradiction. If we are to progress in the future and have progressed in the past, then we certainly *had* a beginning. But this by no means implies that we must necessarily have an end. It seems to be merely the word "infinite," with its unthinkable implications, that has led to this idea. With an infinite and eternal universe (and we cannot possibly think of it as finite either in time or space) infinite developments of being both in number and gradation are possible, of beings ever beginning but never ending.

Of course, the whole question relates to *personal* existences with permanent (if partially discontinuous) identities. As to the rudiments or soul-stuff out of which such

personality first arises it is not worth while arguing, as we have no facts to go upon. But the difficulty of the diverse natures with which we are born, and which to many seems to need re-incarnation or "karma" to explain, appears to me to be no difficulty at all. I look on these diverse natures as the necessary result of the mode of increase of all but the lowest organisms, through the agency of male and female. This mode of increase has been the means of supplying the variations which have led to the continuous development of the organic world with all its myriad forms of use and beauty; in mankind they lead to that infinite diversity of intellectual and moral nature, of tastes, habits, faculties, and inspirations, which are, in all probability, by their action and reaction on each other, equally essential for the full development of the highest nature of man. Any theory like Re-incarnation suggests, to my mind, a crude and clumsy mode of development, as unnecessary as it is unsupported by any facts or analogies either in the material or the spiritual universe. – Alfred R. Wallace.

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Pre-Existence (S425a)

More on the matter of reincarnation, in a letter printed in the 26 July 1890 issue of Light (London).

Sir, – I did not intend to enter into any controversy on this subject, and will now only say a very few words, because Mr. Paice has quoted an argument of mine which he seems to think is inconsistent with a belief in each person's individuality originating at his birth. My argument in "Darwinism" was to show that there were peculiarities in our mental nature that could not be explained by a development through the law of "natural selection"; and the conclusion I arrived at was, that the *development* of man's spiritual nature was determined by other unknown laws, though its *derivation*, like that of the body, was by hereditary descent through the complex lines of diverging ancestors. I can conceive, for example, either that the human spirit has an inherent power of progression under favourable conditions, or that it is subject to influences from the spirit-world which, without destroying its individuality, greatly foster its growth and advancement. The fact, proved by Mr. Galton in his *Hereditary Genius*, and a matter of common observation to most of us, that mental and moral tendencies *are* often hereditary though subject to greater divergencies than physical characteristics, is, I think, a clear indication that both *originate* through the same law of ancestral derivation, though their progressive *development* seems to be subject to different laws. The fact of the hereditary transmission of mental and moral qualities seems to me fatal to the theory of Re-incarnation as being the general law of spiritual development.

The argument or illustration from a supposed eternal progression is not worth pursuing, since it leads to so many insoluble and even unthinkable problems. The illustration from the hyperbola does not seem to me to render the position at all more intelligible, of personal identities progressing from a past eternity to result in all the weakness and imperfection of existing human nature. I limited my argument strictly to the origin of our personal individualities or identities. If these have, for all of us, existed from eternity, then we are all uncreated independent beings – gods, in fact – and our present state of

weakness, ignorance, and impotence, in relation to the universe around us, becomes still more inexplicable and contradictory. – Alfred Russel Wallace.

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Mr. S. J. Davey's Experiments (S436)

A letter printed in the March 1891 issue of the Journal of the Society for Psychical Research (London).

Sir, – In the January number of the *Journal* the death of Mr. S. J. Davey is announced, with a complimentary reference to his "experiments," recorded in Vol. IV. of the *Proceedings*. I, and many other Spiritualists, thought at the time that to publish those experiments without any elucidation of them other than Mr. Davey's assertion, that they were all "tricks," was an unscientific and unfair proceeding, since it accepted as evidence in his case a mere personal statement which it has always refused to consider of the slightest value when made by Spiritualists.

Now, however, that further secrecy is unnecessary, I trust that Mrs. Sidgwick, Mr. Hodgson, and any other persons to whom (as stated at p. 485 of *Proceedings*, Vol. IV.) Mr. Davey communicated "the details of his methods," will give a full account of them, in order that we who believe that there are genuine phenomena of which Mr. Davey purported to give "trick" imitations, may be able to judge how far this claim is supported by the actual facts of the case.

If such experiments as those recorded at Sittings 11 and 12, and at the materialisation séance, are clearly and fully explained as mechanical or sleight-of-hand tricks, available under the conditions usually adopted by professed mediums, it will do more to weaken the evidence for Spiritualistic phenomena than anything that has yet been adduced by disbelievers. As one of the witnesses says: "I believe that a full explanation of his methods would 'fire a shot heard round the world' in almost every civilised community where the phenomena of so-called 'Spritualism' are perplexing, and often madden true and good people." (*l.c.*, p. 435.) But to have this effect it will not do to explain *some* of the phenomena by trick, leaving the more mysterious unsolved. They are claimed to be *all* trick, and unless *all* can be so explained many of us will be confirmed in our belief that Mr. Davey was really a medium as well as a conjurer, and that in imputing all his performances to "trick" he was deceiving the Society and the public. – Alfred R. Wallace.

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Dr. Carl du Prel and Spiritualism (S446a)

A letter printed in the 9 April 1892 issue of Light (London).

Sir, – I am very much pleased to find that Dr. Carl du Prel now avows himself almost as much a Spiritualist as any of us. I have thought that some of his recent writings indicate this; but as I have read none of his works but Mr. Massey's translation of his

"Philosophy of Mysticism" I could not be sure how far his opinions had changed. That work is a storehouse of valuable facts for the Spiritualist; but all are set forth as illustrative of the theory of "double consciousness," or "the transcendental Ego." The standpoint of the whole book may be illustrated by the author's remark as to the alleged "guides" and "guardian spirits" of mediums: "The third possibility, that the guides are actual third persons, that is, other subjects, must remain excluded until they exhibit characteristics not to be explained by even the double nature of man. But as we do not know the faculties of our second Ego, nor, therefore, how much they will explain, this is a case which cannot easily occur." (Vol. I., p. 137.) Nowhere in the entire work is it alleged that there are any phenomena which the theory of the second Ego will not explain. Now, if I understand Dr. du Prel's letter aright, he does admit that there are phenomena which imply the agency of "other subjects" – that is, of what we term 'spirits.'" It is to be wished that the later works, to which Dr. du Prel refers in his letter, could be translated into English for the benefit of those Spiritualists who, like myself, are unable to read them in the original. – Alfred R. Wallace.

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letter to Annie Besant on theosophy and spiritualism (S447)

This letter to Annie Besant, Editor of Lucifer and prominent theosophist, responded to her survey of opinion regarding matters of common interest to theosophists and spiritualists. It was printed in the 15 April 1892 issue of that title.

Dear Mrs. Besant, I could very easily answer the questions you send, but I do not see what purpose it would serve. *Opinions* are of no value without stating the *evidence* on which they are founded. Again, the questions seem altogether one-sided, as none of them touch on the special teaching of Theosophy. Yet further, so few Spiritualists see *Lucifer* that the discussion would not reach them. It seems to me that to serve any useful purpose the questions should be so put as to bring out the crucial *differences* between Spiritualists and Theosophists. The *evidence* for each special doctrine should be summarized in the answers. And, lastly, arrangements should be made to have the whole discussion published in *Light* as well as in *Lucifer*. – Yours very faithfully, Alfred R. Wallace, Parkstone, Dorset.

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Psychography in the Presence of Mr. Keeler (S452)

A note printed in the July 1892 issue of Psychical Review.

On January 19th, 1887, while in Washington, I accompanied some friends, two of whom were complete skeptics, to one of Mr. Keeler's seances. Before the seance commenced, it was suggested that the paper block on which messages were usually written

and which was lying on a table, should be privately marked. Accordingly one of the skeptics loosened the edges of the block and marked about a dozen sheets with his initials – L. O. H. At the seance, the medium sits in front of a calico screen about five feet high hung across the corner of the room, behind which is a small table, a tambourine, stick, bell, etc. A lady from among the visitors sits beside the medium, who places both his hands upon her arm, and another calico screen tied across at the level of their necks hides the lower portion of their bodies. From behind the calico screen, above the head of the medium, a hand appears which takes a pencil and the paper block from the hand of a gentleman sitting near. The sound of writing is then heard, a sheet of paper seems to be torn off, and is immediately thrown over the screen and falls between the medium and the spectators. It is found to contain either some remark pertinent to what has been occurring at the moment or a message for some of the audience; and frequently a dozen or more such messages are given in the course of the evening, most of which are said by the recipients to contain names or facts which they recognize as correct. Sometimes a hand holding the pencil, appears to come bodily through the calico screen and writes on the paper block held by a person indicated. On this evening, I was asked to hold the block, and it was written on by a hand which appeared to come through a slit in the screen just above the medium's shoulder. The writing was rapid and partly unintelligible, but the words appear to be – "Friends were here to write, but only this one could this time. Come when they can." Later on a paper was thrown out to me containing these words – "I am here. William Wallace." Both the sheets are initialed L. O. H., showing that they could not have been prepared beforehand. No aperture could be found in the calico screen when it was examined after the seance, and no means could be discovered by which any person could have entered the corner of the room cut off by the screen. There was sufficient light to see everything and to read the writing, and full examination of the room was permitted both before and after the seance.

At another seance on February 21st, a paper was thrown out to me on which was the following message, in a different handwriting from the previous one – "I write for Mr. William Wallace, my old friend, to say that he is desirous of giving you an important message and will do so on a clear night when he can write himself. William Martin."

Two days afterward, I had another message in the same writing, beginning – "I am William Martin, and I come for Mr. William Wallace, who could not write this time after all" – and then the message goes on to refer to a matter on which I had written a letter to a newspaper that very morning. These two communications are important on account of the person from whom they purport to come. My eldest brother, William, had been educated as an architect and surveyor, and after leaving the gentleman with whom he had been articled, he went to London and engaged himself with a large London builder, to obtain a practical knowledge of materials and construction. This builder was named Martin, and he had a son about my brother's age. This was in the year 1830 or thereabouts, and when I was living with my brother some ten years later, he used often to refer to his friend Martin, but I do not remember hearing him spoken of in any other way, and therefore did not know his Christian name. Since my brother died, in 1845, I have heard nothing of these Martins, and no one in America, besides my brother John, who resides in California, and myself, could possibly know anything of the relations existing sixty years ago between them and my brother. I do not think I have ever heard their names mentioned since my brother's death, and it was therefore most startling and altogether unexpected to have the name brought before me in this manner in connection with that of my brother. I may

add that on enquiring of my sister, who being nearer my brother's age, knew more of his early life, she informs me that the Christian name of both the elder Martin and of his son was William.

At a subsequent seance on February 26th, I received a message in quite a distinct handwriting, claiming to be from the elder Martin and stating that he was a friend of my father's. Whether this was so I do not know, but as my father lived much in London in his early life, it is very probable, and will account for my brother's business connection with the Martins. The essential point, however, is, that after more than forty years of silence and forgetfulness, the names of these Martins and my brother should be brought before me at the place and in the manner here described.

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The Late Mr. S. J. Davey's Experiments (S467)

A letter printed in the March 1893 issue of the Journal of the Society for Psychical Research.

Sir, – In reference to the explanations of Mr. Davey's methods of conjuring, given by Mr. R. Hodgson in the *Proceedings* of July last, I wish to make a few remarks. It is, of course, quite clear, from the facts now given, that Mr. Davey was a very clever conjurer; that he possessed the somewhat rare natural faculties of the high-class prestidigitateur; and that he had cultivated these faculties in the special department of imitating *slate-writing* and other spiritualistic phenomena. It also seems clear to me that he possessed the faculty of thought-reading and thought-impressing in a high degree; and, further, it seems probable that he also possessed mediumistic power, which occasionally took part in his performances.

In regard to some of the sittings, Mr. Hodgson's explanations are so full as to show that all was performed by clever trick, but in others there is no explanation given, except a general reference to the "methods" used in other cases. I have neither time nor inclination to go into these cases in detail, nor would there be any use in doing so. The important question is, whether the methods which Mr. Davey used in his trick-performances are such as will serve to explain most, or all, of the slate-writing of professional mediums. I shall, therefore, accept Mr. Hodgson's challenge (in *Proceedings*, Vol. IV., Part XI., p. 404) to point out exactly where the difference lies between Mr. Davey's performances and those of mediums.

I have not myself had so much experience of slate-writing as many other Spiritualists, nor have I received such marvellous and demonstrative tests as have occurred to others. But I have witnessed successful slate-writing with four different mediums; and their proceedings, in the three best cases, afforded none of the opportunities for trickery on which Mr. Davey appears to have mainly relied. I will now point out some of the conditions and arrangements on which Mr. Davey's performances depended, and which were not present on the occasion of either of my sittings.

1. Mr. Davey on several occasions asked his visitors to bring *three* slates with them, which afforded him the opportunity of writing on one of them, and substituting this for one of the others after they have been cleaned for the experiment.

On no occasion were any slates asked for previous to my sittings, but a single slate was once taken, and writing obtained on it without its leaving my sight or that of my brother.

2. During the greater number of Mr. Davey's sittings he left the room, either before or in the midst of the performance, often more than once, giving him the opportunity required for some of his best tricks.

At none of my sittings did the medium leave the room.

3. Many of Mr. Davey's tricks depended on the slate being held *under* the table.

At none of my sittings, except the first (with Slade), were the slates ever put under the table.

4. Mr. Davey had a duster and blotting-paper on his table, used ostensibly for cleaning and drying the slates, but which were of great use to him in concealing and transposing them.

At none of my sittings were either of these articles on the table, a small piece of sponge being the only thing used for cleaning the slates.

5. Long waiting, during the sittings, to relax the attention of the sitters, was used by Mr. Davey.

At none of my sittings did I have to wait more than five or ten minutes, in several much less. At my best sitting, with Fred Evans in San Francisco, seven slates filled with writing or portraits, including letters signed with the correct Christian name or initials of several long deceased members of my family, together with six portraits produced on paper touched by no one but myself, were obtained in a sitting of about half an hour.

6. At some of Mr. Davey's sittings the visitors were in the room an hour before the séance began, during which time some of their slates were taken away and written on.

7. During some of the slate experiments, other tricks, with glasses, coins, etc., were sometimes interpolated, at other times three or more sitters were intermixed in one experiment, thus causing confusion and affording the opportunity for writing on or transposition.

Nothing of this kind occurred during either of my sittings.

8. Mr. Davey asked his sitters to change places, sometimes more than once during the same sitting; thus offering other opportunities for manipulation of the slates.

No such change was made during my sittings for slate-writing.

9. Apparently all Mr. Davey's visitors sat at the table, or if there were any onlookers they were, like Mr. Hodgson, in the secret.

At one of my best sittings (with Keeler at Washington) a friend sat about a yard back so as to see all that passed at the table where I and the medium sat. On this occasion I examined two slates, tied them together, placed my hand on them, on the table, the medium placing his hand on mine, and in a minute or two I opened the slates and found several lines of writing inside. Nothing else whatever happened, and any substitution was simply impossible.

At the sitting with Evans, I and my brother sat with the medium at the table, and two friends, who had had numerous sittings before, sat about three yards off, so as to see all that passed, while leaving the séance wholly to us. These conditions are, I submit, the most unfavourable to any trickery by the medium.

I have now pointed out nine distinct features which differentiate Mr. Davey's performances from those of the slate-writing mediums I have had the opportunity of observing. These features, either singly or in combination, constitute the essential conditions of most

of Mr. Davey's conjuring performances; and they correspond so closely with those used by all conjurers, and are so different from those adopted by most mediums, that the difference would have been at once noticed had those familiar with the slate-writing of a number of different mediums been allowed to witness Mr. Davey's experiments.

There are a few points in connection with Mr. Davey's power of thought-reading and willing, and perhaps of mediumship, which call for notice. In *Proceedings*, Part XI., p. 406, Mr. Davey tells us that he was affected a good deal during his first experiments with "involuntary movements." Such movements are almost universal in the earlier stages of mediumship. Many of his sitters report that he was "violently agitated," that electric shocks seemed to pass through him; that he exhibited great nervous strain with beads of perspiration. These are usual phenomena with some mediums, but Mr. Davey does not tell us whether or no they were all simulated by him.

At p. 412 of same *Proceedings*, Mr. Davey describes how he asked a gentleman to *think* of a number, apparently with no limitation as to number of digits. He then writes on a slate what he *thinks* is the number, and it proves to be correct, namely, 98. He adds that he has had "several somewhat similar experiences." This faculty, with the corresponding one of impressing his thoughts on others, he evidently calculates on; for Mr. Hodgson tells us (*Proceedings*, Part XXII., p. 275) that he draws a figure or number that *he thinks* the sitter is most likely to choose. Also, in his book experiments, he calculates on *forcing* a sitter to choose the book he requires. The record of his sittings shows that he tried this experiment with ten different sitters; with four it failed or was inconclusive, but with the other six it succeeded more or less completely. Now it is certain that with sitters whose choice was uninfluenced, not more than, if so many as, one in ten would choose the one book, out of a hundred or more, placed conspicuously in order to be chosen. Most people would avoid such a book. Not only the book has to be forced, but the line or page, chosen by chance and sometimes neither spoken nor written down, has to be accounted for. In several cases the words found on the slate were correct for either line or page, and sometimes for both, as in Sitting VII., of which no explanation is given. Then we have Mr. Dodds' case, where he chooses *Taine on Intelligence*, because he had been reading another work of Taine's that morning, and only *thought* of a page and line; yet words were written from that line of another page of the book. Again, in Sitting XVI., Miss Symonds has three book experiments, choosing a different book each time, she says "at random." Yet passages or words from two of these books were written, in one case from the page and line chosen; and in the third case it was written that there was no such page, which was correct. Of these three experiments, also, no explanation or suggestion of any kind is offered.

In what is called the "Sitting for Materialisation," a confederate entered the room, and was enabled to do so by the complete darkness. I have witnessed numerous far better materialisation-phenomena in private houses, under circumstances which rendered the presence of a confederate impossible; and on none of these occasions was there darkness, but always light enough to allow all the persons in the room to be seen. The accounts of the sitters with Mr. Davey as to the locking and sealing of the door were so grossly inaccurate that I was led to suppose the phenomena were genuine.

I think that I have now shown that there are *very great differences* between Mr. Davey's performances and those of mediums. This would have been of little importance had not his séances been given undue prominence by publication in the Society's *Proceedings*, and been thenceforth appealed to as proving that the slate-writing of mediums

was also trickery. Mr. Davey's repeated refusal to exhibit his performances to those Spiritualists who had had a large experience of slate-writing in the presence of mediums, should, in my opinion, have been sufficient to exclude his paper from the pages of a scientific journal, since he thereby proved that to elicit the truth in the matter was not so much his object as to keep up his reputation as an exposer of the tricks of mediums. The *comparative* experiments, which could alone have given any scientific value to his performances, being absent, nothing remains to interest Spiritualists beyond clever conjuring intermingled with more or less of psychical or mediumistic power.

I venture to hope that other Spiritualists may now make known their experiences of slate-writing phenomena, under conditions very different from those present during Mr. Davey's performances, and such as to exclude the agencies he mainly employed. – Alfred R. Wallace.

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The Response to the Appeal (S474)

On the commencement of the spiritualist magazine Borderland in 1893 its editor (W. T. Stead) solicited evaluations of the publication's stated purpose from a number of notable figures, including Wallace. His reply, given below, was printed in the first issue of the magazine in July 1893.

I have no doubt your new periodical will be the means of bringing together much valuable information that would otherwise be lost.

The only point on which I have a few remarks to make is that of your suggested classification. It seems to me that your divisions are not, or may not be, real ones. In (2) and (3) it seems to me necessary to insert the words "appear to" before "depend," because I believe that many which thus "appear" do not really so depend. It involves a theory which may not be a true one. The one thing I object to in your recent writings on the subject is your assumption of *theories* as if they were *facts*. Thus the statement that certain persons at a distance write through your hand, though they are totally unconscious of doing so, seems to me a pure *theory*, and an unfounded one, not a *fact*, as you appear to think. So I object to Mr. Myers speaking of the "second self," the "subconscious ego," &c., &c., as if they were *facts*, instead of very doubtful theories; and I think it a great pity that you also adopt his terms as representing proved facts.

There seems to me to be a much more valid basis of classification of the phenomena, determined by their subjective or objective nature as indicated in the following, rough outline:

A. *Subjective*. – Phenomena which affect the senses of one person only, while others present are not thus affected. These may be *veridical* and therefore *real*, but *subjective*.

B. *Perhaps subjective but with an objective basis*. – Phenomena which affect the senses of all present in the same way but which have no material indication of objecting, *e.g.*, apparitions, noises, opening doors which are yet locked and remain locked, &c., &c.

C. *Objective*. – Movements of matter not caused by physical agency of any person present, as direct writing, and drawing spirit photographs, flower bringing, levitation, &c., &c.

These seem to me fundamental classes, while yours are subdivisions of each of them.

Wishing you every success.

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letter concerning Eusapia Palladino (S518a)

A letter to the Editor of The Daily Chronicle, printed in its 1 November 1895 issue, discussing the illusionist Maskelyne's condemnation of the Italian medium Eusapia Palladino.

Sir, – Will you kindly allow me space for a few critical remarks on Mr. Maskelyne's communication in your paper of Tuesday? Mr. M. declares that Eusapia is a clever trickster, and he tells your readers how, in his opinion, everything that he witnessed during the séance he attended at Cambridge might have been done. He does not, however, give any *proof* that things were so done, and it is to show that, in one case at least, this proof might have been and therefore ought to have been given; and also to point out what seem to me to be insuperable difficulties in his explanation of this one most striking and critical phenomenon, that I now ask the attention of your readers. I allude to the lifting of the small wicker table from the floor on to the table in front of the medium while both the medium's hands (as well as her feet) were securely controlled.

It is this fact – that both hands of the medium were secured – which renders this one phenomenon far more important than any others reported as having occurred during the Cambridge sittings, since Mr. Maskelyne, Dr. Hodgson, and Mr. Myers agree in their belief that all the rest of the phenomena were mere tricks effected by the aid of one of the medium's hands which she managed to get free in the way they have explained. But in the case of the wicker table, we have Mr. Maskelyne's own admission that her hands were securely held by himself and Professor Lodge, adding emphatically, "There was no mistake about it." He asks therefore – "How then did she lift the table?" and he thus answers his own question: "The simple fact is that she leant away from me, seemingly as far as she could, and threw her head back. Then with her teeth she seized the wicker table, and at the cost of some exertion, extending her legs as a counterpoise, she lifted it up and dropped it on the table in front of her. Naturally, in its progress it described a semi-revolution, and came down bottom upwards." That, according to Mr. Maskelyne, is "how it was done," simply and naturally!

Now, taking the conditions exactly as stated by Mr. Maskelyne himself, I venture to think that it could not possibly have been done as he describes, and that the attempt to do it would have been instantly detected. He tells us that the small wicker table was placed "on the right" of the medium's chair, not behind her; and this is shown to have been so by his statement that she leant away from him as far as she could, and therefore towards the wicker table, and also towards Professor Lodge, who was holding her right hand. But if a person leans on one side and brings her head down to the level of a table her mouth will be more or less vertical, and in order to seize the edge of a table with her teeth she must

turn her head either upward or downward, it being in either case extremely difficult if not impossible for her to grasp the thick edge of a wicker table between her teeth. Admitting, however, the possibility of her thus grasping it, her hold could not be square to the edge of the table but more or less oblique, and when the table was lifted up the table-legs would necessarily project horizontally sideways, and as Professor Lodge is a large man they would almost inevitably have struck against his head, body, or arm.

Mr. Maskelyne's simple explanation of this alleged trick seems, then, to be a mere verbal explanation not corresponding to the actual conditions under which the phenomenon occurred. He had, however, a really simple means of testing the accuracy of his explanation which, as he says nothing about it, he appears to have overlooked. However light such a table may be (and neither its size nor weight are given), the strain on the jaws when lifting by one edge only must be very great, and the grip needed proportionally powerful. Such a grip would necessarily leave permanent tooth-marks on the soft wicker-work of the table, and the presence or absence of such marks would be an absolute test of the truth or falsehood of Mr. Maskelyne's theory. Yet no examination of the table for such marks appears to have been made.

I hope that Professor Lodge will inform your readers whether, in his opinion, the table could have been lifted by the medium over his arm and close to his head in the way Mr. Maskelyne describes; and also that Mr. Myers will kindly state whether his wicker table bears the marks of Eusapia's teeth on its edge. We shall then be in a better position to estimate the value of Mr. Maskelyne's very confident explanations, which the public, of course, accept as actual demonstrations.

It appears to me that accusations of wilful fraud, even against a medium, should be supported by all available facts, and by fair inference from them; and that this is not less incumbent on us when the accuser is a professional conjuror and exposer of mediums, and the accused is only an ignorant foreigner, and a woman. – Alfred R. Wallace, Parkstone, Dorset.

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The Theory of the Double (S522)

Being in two places at once is the subject of this letter, printed in the 22 February 1896 issue of the spiritualist magazine Light.

In 'Borderland' for January Mr. Stead gives an account, with full details and proofs, of the appearance of the double of Mrs. A. at a church in a suburb of London, seven or eight miles distant from her own house, where she was at the time ill in bed. She was seen at the church by Mr. Stead himself and by several of his family, who knew her perfectly, as well as by the clergyman, the deacons, and other persons. She remained there from about 7.5 to 8.30 p.m, was offered a hymn-book, which she held as given to her, but did not otherwise use; and when leaving, a few minutes before the congregation, pushed open the swing door and was not seen again. At her own house she was very ill with spasms, in the afternoon. The doctor came to see her between five and six, and ordered her to go to bed; her servants and a relation saw her in bed asleep between six and

seven, and again saw her asleep about nine, when she awoke, and finished writing a letter in bed to Mr. Stead, telling him of her illness. The whole testimony is, as Mr. Stead says, absolutely conclusive that she could not possibly, in any normal way, have gone from her own house to this distant part of London, and returned to her house and bed at the times when she was seen there. Hence he concludes that this is, perhaps, the most perfectly attested case of a 'double' on record.

Now, if we take these facts as perfectly established, it is interesting to ask what they really prove, and by what theory they may best be explained. Mr. Stead gives no theory, except what is implied by the use of the term 'double,' and by his preliminary statement that it is a problem relating to the 'personality,' and that 'there is no chasm to be bridged in its case between the living and the dead.' He apparently believes, therefore, as do most 'Psychical Researchers,' that the double is really some portion of the 'personality' of the individual whose image appears, and is in some unknown way produced by that individual alone.

Now the misfortune of holding so fast by this theory, and treating 'doubles' as quite distinct from, and much more easily investigated phenomena than, 'ghosts,' is that the many distinct ways in which the phenomena *may* have been produced are entirely overlooked, or not thought worthy of careful consideration. In this case of Mrs. A., for example, the appearance may conceivably, and in strict analogy with known facts, have been produced in four distinct ways, which may be thus briefly stated: –

(1) A true 'double,' or ghost, of Mrs. A. produced by the agency of her own spirit.

(2) An apparent 'double,' or lifelike image of her, produced by spirits, as in materialisations.

(3) A real person, who is a medium, transfigured and impressed to act as the double of Mrs. A.

(4) Mrs. A., herself *in trance*, conveyed by her guides to and from the church where she appeared, and impressed to act as she did act.

I myself have not yet met with any sufficient evidence to prove that the first theory is the true one in this or any other case. The second seems to me to be the most frequent and most probable explanation of 'doubles.' The third is a possible method, as there are numerous cases of mediums being so 'transfigured' as to resemble other persons. The fourth may not be very common, but seems to me to accord best with the phenomena that actually occurred in the case of Mrs. A. My reasons for this belief are as follows: –

(1) It is a most suggestive fact that during the whole period she was in the church – 7.5 to 8.30 p.m. – she was seen by no one in her own house, but was believed by all to be asleep in bed from about 6.30 to 9.0. This offered ample time for her trance to be deepened, for dressing herself unconsciously, for her conveyance almost instantaneously (as Mrs. Guppy, Mr. Williams, and Mr. Henderson were conveyed), across a considerable part of London, to and from the church.

(2) She behaved in the church as if in a trance. She did not see or recognise Mr. Stead, although he looked straight at her as she walked out. She sat still during all the service, taking a hymn-book when offered her, but making no use of it, and not noticing the collection box when held before her.

(3) She entered the church late, and left it before any of the congregation. This would imply that time was limited, it being necessary that her going and returning should be unnoticed. Such a deep trance as was needed for this journey may have been actually

remedial, and have enabled her, when she awoke at nine o'clock, to finish her letter to Mr. Stead, and thus lead to the remarkable body of proof he was able to collect.

(4) She had been seized, on the previous Sunday, with 'an almost uncontrollable desire' to attend the service in that particular church. She was, however, very ill, and Mr. Stead made her promise not to attempt to go until quite strong. Such an intense desire to go to a particular church by a lady, who, we are told, is very sceptical, was evidently not normal, and may have been induced in her for the purpose of preparing for, and calling attention to, the remarkable test phenomenon that was to be produced on the following Sunday.

These four considerations seem to me to point to the explanation that it was Mrs. A. *herself* who appeared at Mr. Stead's church on Sunday evening, October 13th, of last year. If it is objected, as it probably will be by Mr. Stead and the Psychical Researchers, that this explanation is absurdly improbable and incapable of proof, I reply, that it is not antecedently more improbable than any of the other explanations, and that it is in harmony with well-attested facts. The case of the conveyance of Mrs. Guppy from her own house at Holloway to a room in the centre of London where a séance was being held is, I venture to say, quite as well attested as is the appearance of Mrs. A. at church when she could not have been there by normal means. For the information of Mr. Stead and of the younger Spiritualists, I will briefly recapitulate the facts. Mrs. Guppy and her lady companion were together making up the week's accounts, Mrs. Guppy standing before the fire with a pen and paper, putting down items of expenditure which her companion was giving her. Suddenly there was silence, and the lady, looking up, found Mrs. Guppy gone. She was surprised, and after some little time went to look for her, but she was not in the house. About an hour later she was brought home by two friends in a cab. They stated that, holding a séance in a dark room, with the doors locked, they heard a slight noise, and, on lighting up, Mrs. Guppy was found standing on the middle of the table, in her slippers, bareheaded, with a pen in one hand and notebook or paper in the other, *the ink of the last entry being still wet*. The door was locked. Mrs. Guppy was somewhat dazed and frightened. She stated that while engaged as above described she suddenly, without any sense of motion, found herself in darkness and heard strange voices. I knew several of the parties concerned in this strange operation, and had their statements direct. The whole details were at once published in the 'Spiritualist' newspaper, and the correspondence of the time at both ends was such that only a few minutes could have elapsed between Mrs. Guppy's disappearance from Holloway and re-appearance in the locked séance room. Now, as Mr. Stead says with regard to Mrs. A.'s double, the only alternative to a real supernormal phenomenon in this case is that there was an elaborate conspiracy of some dozen people, almost all honest, and even enthusiastic inquirers into Spiritualism, to deceive their fellow workers and the public. The *evidence* for a supernormal transference was here about as complete as it possibly could be; and in the case of Mr. Henderson it was, if I remember, equally complete. Yet such astounding phenomena were then so new that few, even among Spiritualists, believed in them. But many things have happened since that period (I have no reference at hand to the year), and now I presume all advanced Spiritualists who have read the records accept the phenomenon as a genuine one. At all events, my contention is, that the evidence for it is fully as complete as for the appearance of Mrs. A.'s material, force-exerting supposed double; and, this being so, it affords the best and least-difficult means of explaining that appearance. Everything points to its

having been a real person in a trance, impelled to act by some outside power, and conveyed to and from the church by some abnormal agency.

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Miracles and Modern Spiritualism (S527a)

This letter to the Editor, concerning a new edition of Wallace's book Miracles and Modern Spiritualism, was printed in the 20 June 1896 issue of Light.

Sir, – Your correspondent, 'Scriba,' has overlooked the fact that both the passages he quotes from the latest editions of my books appeared in the first editions, about twenty years ago. 'Natural Selection' was published in 1870, and the chapter from which he quotes is reprinted without alteration, as shown in the preface, where all additions or alterations are referred to. My 'Miracles and Modern Spiritualism' was published in 1874, and no alteration has been made in the chapter quoted. I do not myself see any inconsistency in the passages quoted by your correspondent. I believe that the individual human spirit is developed *in* and *by means of* the body, and that the mental powers and faculties of the spirit are developed along with, and by means of, the brain. When it leaves the body it possesses the exact grade of development and amount of knowledge it had acquired in the body, the spirit of a child possessing the mind of a child, and that of a philosopher the mind of a philosopher. The statement that 'size of brain is one of the most important elements which determine mental power or capacity,' is in perfect harmony with the other statement that it is 'spirit alone that feels, and perceives, and thinks'; though, so long as the spirit is in the body, it does so by means of the brain and nervous system which formed an essential condition of its development. If this were not so, if the spirit were mentally independent of the organism it is here bound up with, there would be no close relation between the mental powers and characters of the spirits of infants and adults, or those of fools and wise men, who, the moment they got rid of the body, would be alike in mental power and knowledge. But all the facts and all the teaching of spirit phenomena show us that this is not so, but that the spirit is exactly what it was here, and starts on its further development from the exact point it had reached here. . . . – Alfred R. Wallace.

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Spiritualism (S530)

A letter concerning the nature of spiritualism, printed in the Echo (London) issue of 12 September 1896.

Sir, – I depart from my rule as to intervention in any newspaper controversy on this subject because your correspondent H. B. Samuels appeals to me to say something; and if I am silent he and others equally ignorant of the facts and literature of Spiritualism may think that his statement has some value. The discussion having been started by a notice of

my book, one would have thought that anyone, before taking part in it, would at least have read that book, or some others of recognised authority on the matter. What would be thought of a person who had never witnessed the simplest experiments in electricity, or read any book upon it, venturing to give his opinion in public on its nature and on the theories respecting it? It is only on the question of Spiritualism that ignorance seems to be considered a qualification for discussing a subject.

Mr. Samuels tells us that, although a strong Materialist, he takes an interest in the question, and that his reading and conversation forces him to the conclusion that Spiritualism is humbug. That is a very common conclusion with those whose reading on or against the subject is limited, but when Materialists extend their inquiry beyond reading and conversation into the phenomena themselves they very often give up their Materialism and become converts to Spiritualism. Robert Dale Owen, Dr. George Sexton, and Annie Besant were certainly three of the most able and intellectual of the teachers of Secularism in this country, yet they were all converted to Spiritualism by *facts* so clear, so cogent, so often repeated under varied conditions, and so completely inexplicable by any other theory than that of the agency of disembodied intelligences, that to such honest and truth-seeking minds no other conclusion was possible. I myself went through exactly the same process of conviction; while many others, such as Robert Chambers, S. C. Hall, Rev. Stainton Moses, and William Crookes, F.R.S., were converted by equally cogent facts from various forms of orthodox Christianity to that broad and humanising religious belief which results from the best Spiritualistic teaching.

Now Spiritualists do not ask Mr. Samuels and his fellow-disbelievers to accept the facts and conclusions of these eminent persons on their testimony; on the contrary, they think better of those who disbelieve until they get satisfactory personal evidence. What they do ask is that disbelievers should suspend their judgment, and not accept second or third hand statements to the prejudice of Spiritualism while they reject even first-hand testimony in its favour. There is probably no subject on which so much misrepresentation and positive falsehood have been put before the public as has been used against Spiritualism, and the paragraph quoted by Mr. Samuels as to the Seybert Commission is a comparatively mild example of these. After a very brief investigation, mainly with one medium, Mr. Keeler, they issued a "Preliminary Report," in which they expressed their belief that the phenomena they witnessed were produced by fraud. But in no one case do they claim to have *detected* fraud, founding their belief solely on the assertion that everything that happened *might* have been produced by the medium himself. They do not even profess to *prove*, by any measurements or by independent experiments, that the medium, under the special conditions, could possibly have produced everything that happened, so that their statement that he did so has no value whatever as a scientific investigation, and certainly not as a thorough and impartial one. This "Preliminary Report" was published nearly ten years ago; it was at once answered by General F. J. Lippitt of Washington, who pointed out errors, illogical statements, and concealment of important facts, and from that day to this no explanation has been given and no further report issued, or is apparently likely to be issued.

In conclusion, I may add, as the result of 30 years' inquiry into the subject, that the phenomena and theories of modern Spiritualism are fully as varied and as complex as those of modern electricity; that to gain any adequate knowledge of them requires long-continued and patient experiment and study, and that secondhand statements as to doubts, difficulties or errors are as utterly valueless and unimportant in the one case as the other.

To avoid misconception, I must add that I do not allege that there is any true parallel between electricity and Spiritualism as objects of study. The one is a physical, the other a psychical, science. The phenomena in the one case depend only on physical conditions, and can therefore, when those conditions are learnt, be repeated at will; the other depends also on psychical and at present only partially understood conditions, and on the capacities and wills of unembodied intelligences over whom we have no control, but whose powers in producing phenomena are affected both by our physical and mental idiosyncrasies, and also by the meteorological and their physical surroundings. The phenomena themselves are, therefore, not under our control, although under favourable circumstances they are produced with such abundance and under such absolute test conditions as to satisfy every inquirer who witnesses them. – Alfred R. Wallace.

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Mr. Podmore on Clairvoyance and Poltergeists (S561)

A long letter to the Editor of the Journal of the Society for Psychical Research, printed in its February 1899 issue.

Sir, – Will you allow me to make a few remarks on the treatment of these questions by Mr. Podmore in the *Proceedings* for December, 1898? In my opinion the credit of the Society as a body for the scientific investigation of a variety of very remarkable and little known phenomena is seriously injured by the method he has adopted – that of omitting to refer to the best evidence in the matters he discusses, giving prominence to every possible supposition of imposture on the part of the agents and of incompetence on the part of the observers, and then stating his adverse conclusions with a confidence and authority which should only be displayed after a full presentation and unprejudiced discussion of the whole evidence available. I will only refer to a few examples to justify this statement, as I have neither time nor inclination, nor is it here necessary, to do more.

Mr. Podmore devotes more than five pages to the case of Alexis Didier, who, he endeavours to show, might have been, and, therefore, probably was, an impostor. He first describes the mode of bandaging the eyes "generally," which was not the more effective mode usually adopted as described by Dr. Lee and others. He then states that, from the detailed descriptions of many observers, he concludes that "the power exercised by Alexis was perfectly normal" – that is, that he saw with his eyes in the ordinary way, and that his reading sealed letters, describing the contents of closed boxes, and playing ecarté rapidly and often telling his opponents' cards as well as his own, were, or might have been, all clever trickery. Every difficult case quoted is explained on this assumption, though acknowledging that this explanation was not necessarily correct. But he continually dwells on the possibility of fraud, on the agents having highly-trained confederates, on the simplicity of the numerous witnesses, and on the fact that "the reports which we possess are mostly at second-hand."

But in a very well known work, Dr. Edwin Lee's *Animal Magnetism*, that physician reports, from personal observation, fourteen séances in Brighton and Hastings at which a large number of experiments were made, far the greater number of which were entirely successful, and many very remarkable. Especially so was the description of a tin box and

its contents in the coroner's office at Norwich, Dr. Lee having put into the medium's hands a letter from the coroner in which this box was referred to as a test (p. 257). The reading of passages in books several pages in advance is what Mr. Podmore considers to be "most strongly suggestive of trickery"; but Dr. Lee gives numerous cases where no opportunity for trickery existed. The books were often brought by visitors as being old or uncommon, they were opened at any page and Alexis marked a line and was then asked to read the same line 10 or 20 pages in advance. The line given by him was usually found at the same level but not at the same number of pages from the open page. Many sceptics applied this test with books of their own, and in some cases the sentences read were quite unexpected and unusual. Mr. Podmore states that in the accounts he has read, when sealed packages were given him "the seal must be broken and the contents shown to a sympathetic witness"; but in the long series of experiments of this nature reported by Dr. Lee, I find that this condition was required only in one or two cases, while many sealed packets are stated to have been described correctly while unopened.

The card-playing, which Mr. Podmore considers to be "most probably deliberate fraud," happens to be that as to which the evidence that it was *not* fraud is most conclusive. It occurred at almost every séance, and in a number of cases cards were named correctly as they lay upon the table backs upwards, while on one occasion a large folio volume was placed upright between the two players without preventing Alexis from naming the cards in his opponent's hand. But the absolute proof of the reality of the clairvoyance while card-playing is the evidence of Robert Houdin, who has been called the prince of conjurers and to whom everything that could be done with cards was perfectly familiar.

At the request of the Marquis de Mirville he had two séances with Alexis, and certified in writing that he found it "impossible" to class the phenomena "among the tricks which are the objects of my art." And after the second séance he wrote – "I therefore came away from this *séance* as astonished as any one can be, and fully convinced that it would be quite *impossible* for any one to produce such surprising effects by mere skill."[1] With such testimony as this, and that of Dr. Lee himself, what is the value of Mr. Podmore's suggestions of "deliberate fraud," or at the best of "unconscious jugglery" when in the trance state, together with his final suggestion of an elaborate "intelligence department," and of "highly-trained confederates" as an explanation "not to be summarily dismissed"?

And such explanations as this, given as the result of an examination of the best evidence, are the more futile when we consider the mass of first-class personal testimony to the reality of clairvoyance of the same nature as that of Alexis that is accessible to every enquirer. Such are those by the late Professor Gregory in his *Letters on Animal Magnetism* (pp. 395–408) forming absolutely conclusive tests through an ignorant girl who

[1] Dr. Lee, in his *Animal Magnetism*, pp. 162–3, gives the essential part of Houdin's two letters; but in order to understand the full weight of this testimony it is essential to read De Mirville's detailed account of his interviews with Houdin, and of the séances with Alexis, to which Houdin went with the full belief that he could expose him. This most interesting account occupies the first chapter of De Mirville's great work, *Des Esprits et de leurs Manifestations Fluidiques*, which is in the Society's library. Houdin also tested the reading of closed books; and Alexis informed the great expert of a fact relating to one of his most intimate friends, which Houdin declared at the time could not possibly be true, but which he afterwards acknowledged to be correct. (See *Des Esprits*, I., p. 26, footnote.)

could not read or write, and of a character more marvellous than any of the clairvoyance of Alexis. Other cases with the same medium are recorded by Dr. Joseph Haddock, M.D., of Bolton, who had her in charge as a patient, in the Appendix to his work on *Somnolism and Psycheism*. In three separate cases this girl recovered lost property when all other means had failed; besides successfully describing distant persons and events unknown to any of the persons near her. Dr. Herbert Mayo, in his *Letters on the Truths contained in Popular Superstitions*, gives a successful test experiment with a Parisian clairvoyante, he being at the time in Prussia. And besides these we have the Report of the Commission of the *Académie Royale de Médecine* strongly affirming the reality of clairvoyance. But all this evidence of men of the highest character and ability, after careful and often long-continued personal observation and test, is wholly ignored by Mr. Podmore in his attempt to show that Alexis might have been, and probably was, an impostor. I submit that such a mode of treating this important subject is utterly unscientific, is opposed to the rules of evidence and of common sense, and is unworthy of the prominent place it occupies in the *Proceedings* of the Society.

The same defects in an even more exaggerated form are found in his conclusions as to "Poltergeists" given in his review of Mr. Andrew Lang's *Making of Religion* in the same number of the *Proceedings*. He says that he formerly thought it "not improbable that there was something inexplicable in these Poltergeist manifestations." Now, having taken the eleven cases investigated by the Society, and, presumably, given due weight to all other well known records, he concludes: – "I cannot find any evidence that would justify such a supposition (that is, that there is anything inexplicable in them) even as a working hypothesis." Then, after nearly two pages of reply to Mr. Lang's criticisms he thus concludes: – "For myself, I am grieved to think that the Poltergeist should go. He was a more picturesque figure than the naughty little girl who takes his place. There are too many naughty little girls on this planet already."

If this judgment is given on the eleven cases alone, the evidence for which he has adversely criticised, then he should not state in such positive terms a conclusion founded upon such utterly inadequate evidence. If, on the other hand, his words – "I cannot find any evidence" – imply that he has considered the best of the existing testimony, then so positive a conclusion should not be stated without at least pointing out the grounds on which he rejects it. For it is the case that no class of psychical phenomena rests on such an extensive basis of well attested facts – facts which were at the time, and have ever since remained, inexplicable by other than a supernormal cause. I will, therefore, briefly enumerate a few of the best attested of these cases for the benefit of such readers as are not acquainted with them; seven which occurred during the present century and two earlier ones.

1. The Drummer of Tedworth, as the disturbances at the house of Mr. Mompesson in 1662 are usually termed, deserves attention, both because it presents the main features of all these cases, and especially because it was recorded by a contemporary and eye-witness of the highest character and of exceptional ability, the Rev. Joseph Glanvil, a fellow of the Royal Society and a writer on the Baconian philosophy. In this case "the naughty little girl" was 10 years old, and the disturbances continued for two years, to the great distress of Mr. Mompesson who would have been delighted to have had the cause of it discovered. The disturbances consisted of various noises, knockings, scratchings, and drums heard as if over the house; shaking of the floor and of the whole house; the children's clothes and other articles thrown about the room, and chairs and stools moving about by themselves in

the presence of numerous witnesses. The noises were sometimes so loud that they were heard in the fields near and even awakened people in the village at a considerable distance. Mr. Glanvil himself heard the knocks and scratchings continuing for half an hour while the children on whose bed it occurred were lying quite still with their hands outside. He also heard loud pantings as of a large dog, which was so violent as to cause the windows and the whole room to shake. The account is given in considerable detail in Glanvil's *Sadducismus Triumphatus*, and I cannot understand how anyone admitting, as Mr. Podmore does, that "it is solely a question of evidence," can come to the conclusion that we have here no evidence of anything inexplicable, "even as a working hypothesis."

2. Half a century later, in 1716, we have the remarkable disturbances at Epworth Parsonage, Lincolnshire, where the Rev. Samuel Wesley was rector. Here again we find exceptionally good contemporary records by various members of the Wesley family, all far above the average in intelligence and freedom from superstition. Samuel Wesley himself kept a journal in which all the chief occurrences were described, and there are also numerous letters from various members of the family to their friends and to John Wesley, describing the various events as they occurred. It is interesting to note that the manservant who first heard the noises and witnessed the movements of various articles, had no fear whatever, and that each member of the family in turn, when told of what had happened, entirely disbelieved that there was anything that could not be soon explained, till he or she had witnessed the phenomena, when it was perceived to be wholly beyond their experience and utterly inexplicable by any known causes. At length the whole household – nine or ten persons – witnessed the disturbances, Mr. Samuel Wesley being the last and most incredulous, and he too was forced to admit that they were wholly abnormal.

The noises were of various kinds, knockings, footsteps, and creaking or drumming noises, which moved about to various parts of the house while being followed, but no cause for which could ever be detected. Often there were tremendous bangings and clashings as if heavy lumps of coal were rolling down the stairs, or all the glass and china in a cupboard smashed to pieces, yet nothing could be found. Movements were also varied. Hand-mills were whirled round, windows rattled, door-latches moved up and down making a great clatter. On one occasion, when Mr. Wesley went to his study, of which he always kept the key, the door was pushed back against him as if by a person inside, but there was nobody. Then began a knocking in various parts of the room, and he was pushed against his desk as by an invisible person. Often the noises were so loud and varied that for the greater part of the night no one could sleep. The disturbances lasted with more or less violence for two months and then wholly ceased. Many of the sounds were of a nature that no one could imitate, and were often such that no person could produce without instant detection. The letters and journal were preserved and were published by Priestly in 1791, and by Dr. Adam Clarke in his *Memoirs of the Wesley Family*; while John Wesley himself, in 1720, collected the evidence of all the witnesses and published his account in the *Arminian Magazine*.

Here surely is another case in which the evidence of "something inexplicable" is both good in itself and demonstrative of inexplicability. It is widely known and easily accessible. Yet Mr. Podmore says: – "I cannot find any evidence" to justify the supposition of "inexplicability."

3. Coming to the present century we have first the case of the castle of Slawensik, in Silesia, in 1807. These disturbances were witnessed by Councillor Hahn and Cornet Kern, both young men of good education and in perfect health, and free from all superstí-

tious ideas. For more than two months they witnessed almost daily and nightly the most extraordinary phenomena. Pieces of lime appeared to fall from the ceiling and flew about the room to such an extent that the whole floor and tables were often covered, yet the closest examination could not detect any sign of its having come from the ceiling. Noises were heard like hammering on boards or the sounds of distant artillery. But most extraordinary were the movements of almost every loose article in the room, such as knives, forks, brushes, slippers, soap, candlesticks. Sometimes these things would rise from the table before the eyes of both of them and then fall to the floor. Many other persons, officers, inspectors, tradesmen, and visitors saw the same things, and no witness of them could ever suggest a natural explanation. Hahn soon became greatly interested in these strange occurrences, applied many tests and kept a careful record of them. And he especially notes, in reply to the objection of delusion, that whenever several persons were present, after each abnormal event he asked each person what he saw or heard, and in every case all witnessed the same thing; while many of the phenomena happened while he was entirely alone.

He gave his narrative of these events to Dr. Justinus Kerner, who has published it in his book on the Seeress of Prevorst (pp. 274–289 of Mrs. Crowe's translation), and a good abstract is given in Dale Owen's *Footfalls*. Here again we have absolutely inexplicable occurrences, and the evidence for them must certainly be classed as exceptionally good.

4. We now come to the remarkable bell-ringing at Major Moor's house, Great Bealings, near Woodbridge, Suffolk, in 1834. It began on February 2nd, and continued almost daily till March 27th. The most careful examination and observation by the Major and his friends failed to discover any natural cause. All the bells rang either together or separately, except the front door bell, which would be the most easy to play tricks with. They rang just the same when all the servants were brought together by Major Moor; and also in the presence of reporters and others. The violence of the peals and the rapidity of the moving bells could not be imitated. Major Moor wrote an account of the disturbance in a letter to the *Ipswich Journal*, and besides many inadequate or foolish attempts at explanation he received letters from all parts of the kingdom describing similar occurrences in various houses. A clergyman, who wrote from a rectory in Norfolk, described various loud and disturbing noises resembling those at Epworth, which had been heard by himself and family for nearly nine years, and which could be traced for sixty years back. Lieutenant Rivers had equally mysterious bell-ringing with those at Bealings in his rooms at Greenwich Hospital. Constant watching by himself, by friends, by the official surveyor and bell-hanger, failed to discover any cause whatever. This ringing lasted four days.

In a little book called *Bealings Bells* Major Moor gives an account of his own case and those of the various other persons who had communicated with him; and the whole constitutes a body of facts attested on the best possible evidence, which is alone sufficient to demonstrate that "something inexplicable" of which Mr. Podmore declares he cannot find any good evidence at all![1]

5. In 1838 a violent outbreak of stone throwing and other disturbances occurred at the farmhouse of Banchory, in Aberdeenshire. On the 5th of December and for five days after, great numbers of sticks, stones, and earth-clods flew about the yard and struck the house. Hundreds of persons came from far and near to see the marvel and none could find

[1] This book is in the library of the Society, and a good summary of the facts is given in Owen's *Debatable Land*, pp. 239–245.

any cause. Then for two weeks the disturbances occurred inside the house, where knives, plates, mustard pots, flat irons, and many other articles flew about the room or came down the chimney. Sometimes they flew from room to room; and there were also tremendous knockings on the doors and roof, while sticks and stones flew against the windows and broke them. People for 20 miles round came to see the phenomena, including farmers, gentry and clergymen, but could find no explanation. At length the two servant-girls were "strictly examined" and sent to prison, and as the disturbances then ceased the conclusion seemed to be that they *must* have done it all, although of the hundreds who had been present no one ever saw them do a single thing. The phenomena were closely like those at the castle of Slawensik, and suggest a common cause. The case is reported by Mackay in his *Popular Delusions* and is summarised in Owen's *Footfalls*, p. 183.

6. The case of Mary Jobson of Sunderland, in 1839, is especially interesting because she was attended by Dr. Reid Clanny, F.R.S., who published an account of the extraordinary things witnessed by himself and also by three other medical men and other persons, sixteen in all. The phenomena consisted of violent knocking, footsteps, doors opened and shut, voices, music, water thrown on the floor, and beautiful designs appearing on the ceiling, all without any discoverable cause; and all in presence of a sick girl of thirteen who had been long treated for a mysterious disease by bleeding, blistering, and purging which almost killed her. A short abstract of Dr. Clanny's publication is given by Howitt in his *History of the Supernatural*, Vol. II., p. 450. Dr. Clanny was ridiculed and persecuted, but always maintained his firm conviction of the reality of these inexplicable phenomena.

7. The disturbances in a burial-vault beneath a chapel in the public cemetery of Arensburg in the island of Oesel, in 1844, are noteworthy, because they were officially inquired into by a commission consisting of Baron de Goldenstubbé, the Bishop of the province, a physician, the Burgomaster of the town, and two members of the Consistory. The disturbances consisted in the coffins which had been placed side by side in the vault, being found, on the occasion of a funeral, to have been displaced so as to lie on each other in a confused heap. They were put back in their places and the doors securely locked, but when privately inspected shortly afterwards by the Baron who was president of the Consistory, they were found in the same disorder as before. After satisfying themselves that the foundations and floor of the vault were untouched, and that there was no secret entrance, the Commission had the coffins replaced, and fine wood ashes were strewn over the pavement of the vault, the stairs, and the floor of the chapel. All the doors were locked and doubly sealed with official seals, and a guard of soldiers watched the building for three days and nights. Then the members of the Commission returned, found the seals intact, the ashes throughout the chapel, stairs, and vault, wholly undisturbed, and with no marks of footsteps; yet all the coffins but three (as before) were scattered about in confusion, the lid of one had been forced open, and several others, though very heavy, had been set up on end. An official report was drawn up stating these facts, and was signed by all the members of the Commission; it is preserved with the archives of the Consistory, and may be seen by any respectable visitors. The disturbances are said to have continued for some months longer, when it was determined to cover the coffins thickly with earth so as completely to bury them, after which no further disturbances of any kind took place. The facts are stated by R. D. Owen in his *Footfalls*, p. 186, he having obtained them, in 1859, from the daughter and son of Baron Goldenstubbé, who were living near at the time and

heard of all the occurrences when they happened. Here, again, we have the best evidence as to occurrences which were, and are, wholly inexplicable.

8. *Stone-throwing in Paris*. This is remarkable as having been watched by the police for three weeks continuously without detecting the cause. A small house in a populous quarter, but isolated by the removal of other houses, was, as stated in the police report, assailed "every evening and through the whole night by a hail of projectiles which, from their bulk and the violence with which they have been thrown, have done such destruction that it has been laid open to the day, and the woodwork of the doors and windows reduced to shivers, as if it had sustained a siege, aided by a catapult or grape shot." The stones, etc., appeared to come from a great height in the air, and all the powers of the police, employed day and night on the spot, were never able to discover the cause.

This case is referred to in Owen's *Footfalls*, but a fuller account is given by De Mirville in his work *Des Esprits*. I have given a full account, translated from *La Gazette des Tribunaux* (the official organ of the French police), in my *Miracles and Modern Spiritualism*, p. 284, which was verified by a literary friend at the British Museum as an exact translation. A later notice declared that "the phenomena remain inexplicable," and De Mirville tells us that nearly a year afterwards he enquired of the police, of the *Gazette*, and of the owner of the house, who had suffered serious loss both in house and furniture, but nothing whatever had been discovered. (*Des Esprits*, Vol. I., p. 384.) Yet Mr. Podmore tells us that *he* can find no evidence of any such inexplicable occurrences!

9. The next, and in some respects the most remarkable case to be cited, is that of the disturbances in the house of the parish priest of Cideville, Seine Inferieure, in 1851, which lasted two months and a-half, and was the subject of a law-suit for defamation of character, during which all the main facts were legally established and duly recorded. The story is a long and interesting one, and is given in full detail in Dale Owen's *Footfalls*, pp. 195–203, and, briefly, in my *Miracles and Modern Spiritualism*, p. 79.

The phenomena were much the same as in the other more violent disturbances already described. Tremendous knockings, scratchings, and shakings of the house occurred, sometimes as if everyone in the house were simultaneously beating the floors with mallets; fire-irons, hammers, tables, desks, and other articles moved about the rooms in the presence of many witnesses, without any apparent cause. The Marquis de Mirville, who owned property in the neighbourhood, the Mayor of Cideville, and many of the gentlemen, ladies, and clergy of the country round, witnessed these phenomena and gave evidence before the court, which sat while the disturbances were still going on. A full summary of this case is given by De Mirville; with the detailed judgment of the Court and the more important parts of the evidence (*Des Esprits*, Vol. I., Chap. XI.) and every reader of this narrative must agree with Dale Owen's concluding remark, "I doubt if it be possible to find a case more explicit or better authenticated than the foregoing."

In conclusion, I maintain strongly that the nine cases I have here briefly summarised rest upon emphatically good evidence, and are of such a nature as to be quite inexplicable on any supposition of delusion or imposture. And further, I maintain that they are quite as worthy of attention and of equal weight, as if they had been observed and described by Mr. Podmore himself or by any of the most trusted members of the Society for Psychical Research; while they rest on better evidence, and have every one of them greater importance whether on account of their duration, the nature of the phenomena observed, or the character and ability of the witnesses than even the best of the eleven cases by criticising which Mr. Podmore founds his general conclusion, that he can find no evidence

whatever of any of these phenomena being genuine or even "inexplicable," and that the only "Poltergeists" are "naughty little girls." I therefore urge that his mode of treatment as regards this wide-spread and important class of psychical phenomena, is utterly inadequate and unscientific, and therefore unworthy of a place in the *Proceedings* of the Society. – Alfred R. Wallace.

Note. – Besides the above, every enquirer should examine the cases of "Stone-throwing" given by William Howitt in three articles in Vol. VI. of the *Spiritual Magazine*. Several of these are as marvellous and as well attested as those here given, especially that of another French parsonage in 1835 (p. 51), and one in Ceylon in 1863 (p. 66). Numerous cases are also to be found in the later volumes of this magazine.

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Clairvoyance and Poltergeists (S565)

A reply to comments on the preceding selection was printed in the April 1899 issue of the Journal of the Society for Psychical Research (London).

Sir, – To follow Mr. Podmore in his reply to a portion of my contentions is needless, as I am content to leave the question to the judgment of any earnest enquirer who will read the evidence at length in the works I have referred to. A great deal of his minute criticism tending to discredit the witnesses seems to me to be of exactly the same character as the well-known *Historic Doubts concerning Napoleon Buonaparte* of Archbishop Whateley, or the still cleverer *jeux d'esprit* on the first Chinese war, which I have not seen since I was a youth, and a reference to which I shall be glad if any of your readers can give me.

I will make one or two brief observations only, on Mr. Podmore's "historic doubts." He says that Councillor Hahn's evidence is not "exceptionally good," because written 18 months after the events. But what events! Things going on for two months, almost daily and hourly, of the most marvellous and antecedently incredible character, witnessed by his friend and by many other persons none of whom could even suggest *any* explanation of them. His detailed account shows *to my mind* that he *did* keep full notes at the time, but even if he did not, the facts were such as were never to be forgotten. And his giving this account to Dr. Kerner for publication in after life, when he was a person of some official standing, is a guarantee of his earnestness that we should not overlook.

I also protest against what seems to me an interpretation of part of Mrs. [John] Wesley's evidence that is wholly unjustified by the facts. She narrates how, going down the stairs with her husband, two sounds were heard, "on my side" like a bag of money emptied, "and on his" as if a quantity of bottles were smashed. Mr. Podmore says this means that there was only one sound differently interpreted by the two people! And because in another account she says that these sounds were *not* simultaneous, that therefore she is not to be believed, and, generally, that nothing at all occurred but what *could have* been, and therefore in all probability *was* produced by one of the daughters, Hetty, who did *not* give her own account of what happened in addition to the accounts of the eight other members of the family!

This is quite in the style of "historic doubts," and as such I leave it to your readers. – Alfred R. Wallace.

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A Message to My Fellow Spiritualists for the New Century (S591)

A short note printed in the 28 December 1900 issue of the spiritualist journal The Two Worlds.

Spiritualism is not an end in itself, but a means of advancing humanity both morally and materially. How much this advance is needed has never been more forcibly shown than at the present time. Plague and Famine, the most terrible famine of the century, raging almost unheeded in India; England engaged in crushing out two nationalities with fire and sword in South Africa; America doing the same in the Philippines; and all the great powers uniting in the plunder and massacre of the helpless Chinese – all this by the professed followers of the Prince of Peace, and under the lying pretence of civilisation and Christianity. Oh! the mockery! the pity of it!

The first duty of Spiritualists is, not only to protest with voice and pen against these national crimes, but to render them impossible in the future by the regeneration of our social system, which, in its awful contrasts of luxury and starvation, of vicious idleness and the grinding toil of millions, is the real cause of them. We must claim for *all* that perfect equality of opportunity, which is the only safe foundation of a really civilised society.

If you will continually keep *this duty* before you, asking yourselves *how* you can best further this great Cause, your spirit-guides will, I feel sure, impress you how you should act so that the New Century may witness the birth, and perhaps even the maturity of a truly moral and spiritual civilisation. – Your friend and well-wisher, Alfred R. Wallace.

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Life After Life. "Star-Shine and Immortal Tears." (S685)

A letter printed in The Clarion (London) issue of 20 January 1911.

Sir, – I have read the letters on "Life after Life" in this week's *Clarion*, and that of Mr. Rose last week, but as, so far, the correspondence rests wholly on the individual *likes* and *dislikes* of the writers, with a few dogmatic assertions, that a future life is "wholly opposed to evolution and to science," etc., I see no use whatever in taking part in it. Let us suppose that a century ago a number of well-educated and enlightened men discussed the possibilities or probabilities of communicating across the great oceans in a few minutes or seconds, and each one expressed his anticipation or *hopes*, his *likes* or *dislikes* of such a possibility – would such opinions or feelings be of any value whatever? I think not; and chiefly because they were not acquainted with the slender but very suggestive

amount of *facts* in electrical science even then known. And as to the dogmatic assertions as to what is "scientific" or "rational" or "probable," or opposed to science and to evolution, what is the value of such opinions as opposed to those of men like Sir William Crookes, Sir Oliver Lodge, the Earl of Crawford, Professor W. F. Barrett, of Dublin, the late Robert Chambers, and Professor Lombroso, besides a hundred others who have been convinced by *facts*, carefully and thoroughly investigated, that there *is* life, after this life, that the "bourne from which no traveller returns" is a figment of the imagination – a fallacy due to the ignorance of an unscientific age.

Your correspondents, so far, seem to be totally ignorant of the fact that in every civilised country in the world there are *millions* of educated men and women who, against all their prepossessions and beliefs, have become convinced by *direct evidence* that the so-called dead *do* return; that every civilised country possesses numerous *societies* and many *periodicals* devoted to this inquiry and dealing with a vast mass of well-sifted evidence; and that there exists a literature of several thousand volumes devoted to it, for the most part as well written and as closely founded on observed facts as those dealing with any of the recognised sciences.

What should we think of people who discussed the future possibilities of electricity without even a rudimentary knowledge of it?

My only contribution to this discussion will be to name a few of the easily accessible books which set forth some of the *evidence* to which I have alluded, and without a knowledge of which no one can have the slightest pretensions to express an opinion upon the subject:

(1) "Report on Spiritualism," by the Committee of the Dialectical Society (cheap edition, 1873).

(2) "Automatic Speaking and Writing – A Study," by Edward T. Bennett (for twenty years Assistant Secretary to the Society for Psychical Research).

(3) "Psychic Philosophy as the Foundation of a Religion of Natural Law," by V. C. Desertis.

(4) "Footfalls on the Boundary of another World," by Robert Dale Owen.

(5) "The Debateable Land Between This World and the Next," by Robert Dale Owen.

(6) "Miracles and Modern Spiritualism," by Alfred R. Wallace.

I mention this last book because to my own knowledge many persons have, by its perusal, been led to study the subject practically, and have obtained through such study a complete assurance of the reality of "Life After Life." – Yours very truly, Alfred R. Wallace.

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Mr. Blatchford's Dogmatism (S699)

A letter in the 11 September 1912 issue of The Christian Commonwealth (London).

Dr. Alfred Russel Wallace, F.R.S., O.M., writes:

I have read the discussion between yourself and Mr. Blatchford with great interest, and I am sorry I cannot undertake to do more than express my general opinion, and refer to my own books for the facts and reasonings on which that opinion is based.

I am in a position now to sympathise with both of your conclusions ("meaning and purpose alone justify the existence of the universe and faith that it has meaning and purpose alone makes life livable." – *Christian Commonwealth*, August 28.) because, for the first half of my life I was as thorough an Agnostic as Mr. Blatchford himself, and I am afraid almost as dogmatic and one-sided in my opinions. Now I take your view of the whole problem, but I still, and more than ever, feel that all attempts to state or define the nature, capacities, or possibilities of that power above us, which is the source of all power and all life, which we speak of as God or Deity, is beyond our conceptions, as is all that is infinite. We cannot go further than Pope in his fine line:

"Thou great first Cause, least understood."

Mr. Blatchford is too dogmatic when he says, as he has repeatedly done in the "Clarion," "I do not know: nobody does know." Unfortunately, he is totally ignorant of two vast accumulations of facts which I have examined and tested to the best of my ability for between forty and fifty years, and which afford

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for any clear and valuable opinions on the matter. These are the facts revealed by what is termed Psychical Research on the one hand and the inner powers and capacities of living things on the other.

I have summarised the evidence for the former in my book on "Miracles and Modern Spiritualism," and for the latter in my recent volume on "The World of Life" and my previous work on "Man's Place in the Universe." The enormous body of evidence adduced in these three works, each confirming and strengthening the conclusions of the other, will, I feel sure, satisfy any careful reader that to imply, as Mr. Blatchford does, that nobody knows anything on these vast subjects – the nature and inner causes of Matter and Energy, Life and Mind – than is expressed in his favourite formula, "I don't know; no other man knows," is quite unjustifiable.

My friends Sir William Crookes and Sir W. F. Barrett have been students of psychical phenomena about as long as I have, and, like myself, the longer they live and the more they know of it the more convinced they are that "the survival of man after death" is as well established as a large portion of what we term "science." Yet this is one of the things which our friend Blatchford declares that "nobody knows."

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Section 5. Geology and Physical Geography

Introduction

Among the least familiar elements of Wallace's career is his work within the realms of geology and physical geography, and especially the latter. In geology he was influenced early on by his roamings as a surveyor, and absorption of the uniformitarian writings of Charles Lyell. But in physical geography he was more the student of Alexander von Humboldt. Humboldt's ideas on "surface physics," expressed in books such as *Aspects of Nature* and *Cosmos*, helped Wallace zero in on the more immediately dynamic aspects of surface processes, especially as these related to problems of animal and plant distribution.

Humboldt, though not a transmutationist, was one of the first biogeographers, and preached a scientific approach to its problems. Lyell himself was one of Humboldt's chief disciples, especially in his attention to the careful collection and study of facts. But other of Humboldt's disciples (perhaps most notably Franz Julius Ferdinand Meyen 1804–1840) were more fervent holists than was Lyell, and their "systems" perspective was adopted by Wallace rather early on, well before he came to natural selection.

Wallace wrote relatively little on geology *per se*, though he made plentiful use of the facts of earth history to support his views on evolutionary process. He was a more committed physical geographer, especially on the subjects of "geological climates" and glaciology, and even developed a synergistic theory of Ice Age development. Later, he would apply his knowledge of physical geography to his discussion of the possibility of life on other worlds. Although most of these efforts seemed to be linked to a desire to create more perfect evolutionary and biogeographical models, their allure to him on their own terms is evident in the passion he put into the letters to the Editor reproduced below.

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Is the Earth an Oblate or a Prolate Spheroid? (S115)

Wallace's experience as a surveyor served him well in an exchange with a man with some rather strange geodetical views. The first of two letters on the title subject appeared in <u>The Reader</u> issue of 19 May 1866.

In [Archdeacon] Dr. Pratt's letter in your journal of April 28, he seems to argue that modern astronomers and geometers are in error as to the true form of the earth. His words are: "In conformity with the assumed oblate figure of the earth, arcs of the meridian should progressively *diminish* from the equator to the poles. *In fact*, these arcs become *longer* with advance in this direction." And he goes on to advance a theory of some polar attraction in space which has drawn out the earth at the poles instead of flattening it, as is

commonly, but he thinks erroneously, assumed. [Johannes] Von Gumpach has been long asserting the very same thing, and has importunately called the attention of our Government to the fact, that numbers of vessels are annually lost owing to the impossibility of calculating their true position, so long as navigators mistake the very figure of the globe they are travelling over. But his warnings have been all in vain. The Admiralty persist in refusing to alter the Nautical Almanack, and the philosopher thinks he has just cause of complaint because the Astronomer Royal will neither accept his conclusions nor point out the flaw in his argument.

Now that a mathematician and astronomer like Dr. Pratt takes up the very same ground as Von Gumpach, it seems time that the matter should be clearly explained; and, with your permission, though neither an astronomer nor mathematician, I will endeavour to do so; and I have the more hope of succeeding because I once felt a difficulty as to the very same point myself.

The fact (universally stated in works on astronomy and geodesy) that degrees of the meridian *increase* in length towards the poles, on account of the earth's *compression* at the poles, is, indeed, one well calculated to mystify a mere mathematician, though it is clear enough to anyone who reflects on the various conditions involved in the problem. If we look at the diagram of a sphere, and the space from the equator to the pole be divided into equal parts subtending angles of one degree each at the centre, and we then flatten the poles by cutting off a portion with a curve of greater radius, it is evident that the distance from the pole to the centre of the sphere will be shorter than before, and therefore, that degrees of latitude, *measured angularly from that centre*, would really diminish in length from the equator towards the poles.

But in our actual rotating globe, the unequally curved surface is one of equilibrium, owing to the varying centrifugal force at different latitudes; and, as degrees of a meridian can only be measured upon the surface by *tangents* or *perpendiculars to it* (obtained by the *spirit-level* or the *plumb-line*), it follows that a degree at the pole, measured by an angular instrument from the earth's centre, would not represent a degree of latitude, because the curvature of the polar regions has its centre much further off than the earth's centre of gravity, and a degree measured on the surface would therefore be longer. The *centre of curvature* of the earth's surface rarely coincides with the centre of gravity, and a plumb-line will therefore not always point directly to that centre. It will do so only at the equator and the pole. Everywhere else adjacent plumb-lines will meet at points within or beyond the centre, according as the curvature of the surface is less or greater than the mean curvature of the globe. The flattened polar regions are, for the geometer, portions of a larger sphere; the protuberant equator (as far as latitude is concerned) is part of a smaller one; and degrees of the meridian measured on these parts must be respectively longer and shorter than what would be due to the mean curvature of the globe.

These considerations seem so very obvious, that I am almost afraid I have mistaken Dr. Pratt's meaning. I hope, however, that the explanation here given may be useful to some young astronomers, as I do not recollect seeing it in any popular work.

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The Figure of the Earth (S116)

Wallace's first letter drew a response from Dr. Pratt, but a week later, in The Reader issue of 2 June 1866, Wallace ended the discussion with the following unanswerable remarks.

Sir, – Dr. Pratt maintains two propositions, which are incompatible with each other:

1. That a plumb-line everywhere points to the centre of the earth.
2. That the earth, at the sea level, is not a sphere.

If both are true, it follows that there are places where the plumb-line is not at right angles to the ocean surface, so that the water must there stand permanently out of level. In other words, the forces that determine the direction of the plumb-line, and those that determine the level of fluids, are not the same at the same points on the earth's surface. Will he explain this little difficulty in the way of his peculiar view?

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Ocean Circulation (S214)

Wallace was an admirer of the theories of Scottish astronomer James Croll (1821–1890). In this letter, printed in the 22 August 1872 issue of Nature, he attempted to expand on some of his ideas.

Although no mathematician, and only an amateur in physics, it appears to me that the difficulties and objections of Mr. Croll on this subject may be obviated, and the whole question elucidated by a reference to the admitted facts, and a common sense interpretation of them. And first, as to the fact that the surface water of the Atlantic Ocean, in moving northwards from the equator to 60° lat., has almost wholly lost the easterly motion of rotation it should have brought with it. This loss is imputed by Mr. Croll to friction only, and he argues that the much lower velocity of the northward current must, therefore, be wholly neutralised by friction. This is his main argument, which he has repeatedly adduced, and to which he has hitherto received no reply. But, although his reasoning might be admitted if the conditions affecting the two motions were the same, it seems to me to be quite inapplicable to the present case. If, in the temperate zone, the ocean extended uninterruptedly in an east and west direction round the globe, it would no doubt retain a considerable portion of the equatorial eastern motion, and whatever deficiency existed might fairly be imputed to friction. But the Atlantic is actually like a huge lake, with continuous eastern and western shores, and the water which flows northwards along the eastern shore is prevented from moving eastwards, not by friction against water or even against the shore, but by having to perform work in lifting or heaping up the water against the shore, just as the water of a pond or lake is heaped up on the leeward side by a strong wind. As the direction of the motion of the water will, however, by the hypothesis, be oblique or somewhat north of east, some of the motion will be diverted northwards along the eastern shore, and thus tend to increase the northerly flow. The 9,925 pounds of energy (according to Mr. Croll) are not therefore consumed in overcoming the frictional

resistance to eastward motion, but for the most part in doing the actual work of overcoming gravitation and holding up the waters at a higher level, and the theoretical amount of this rise can, no doubt, be easily calculated for us by Mr. Croll.

The case of the water moving northward is very different. There is a clear passage into the polar area, and probably up to and beyond the pole; and within this area there is a continual diminution of bulk of the entering water as it becomes cooled, as well as a continual subsidence of the surface water, producing a partial depression to be constantly filled by water from the south. Experiment proves that if at one end of a vessel of warm water ice is applied at the surface, the cooled water instantly sinks, and its place is taken, not by water rising upwards from below, but by a horizontal movement of the surface gradually propagated to the other end of the vessel, while the descending cold water creeps along the bottom, and gradually acquiring a higher temperature, rises and completes the circuit. It is somewhat difficult to conceive, theoretically, how such a circulation can commence, because the cooled atoms of water must displace others before they can descend, and these again must displace others, and so on over the whole mass. The amount of energy due to the superior weight of the first-cooled atoms may appear inadequate to perform so much work, but nevertheless circulation does commence and indefinitely continues so long as a difference of temperature of the two ends of the vessel is kept up. The extreme mobility of the particles of water, and the almost total absence of friction between them, seems to be influential in producing this result; and it is not probable that any minute difference of level that may be caused on the surface of the water by difference of temperature has anything to do with the motion; and I cannot help thinking that the supposed six-feet incline from the equator to lat. 60° is, if it exists, by no means an effective cause of the oceanic circulation. – Alfred R. Wallace

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A Twenty Years' Error in the Geography of Australia (S291)

A letter to the Editor printed in the Nature issue of 20 June 1878.

In almost every detailed map of Australia, including some of the latest, we find, at the head of the Alligator River, in about S. lat. 13½°, and E. long. 133°, some such note as this: – "Steep walls, 3,800 ft." This is copied from the map illustrating "Leichardt's Journal," published in London in 1847. This map was (as stated in the preface) drawn by S. A. Perry, Esq., Deputy Surveyor-General of New South Wales, from materials furnished by Leichardt, and was engraved in London by Arrowsmith. As Leichardt only returned from his first expedition at the end of 1845 or beginning of 1846 he could have had no opportunity of correcting or revising this map. Mr. James Wilson, the geologist to the North Australian Expedition under Mr. A. C. Gregory, having passed over much of the same country, and finding the plateau nowhere more than 1,600 feet above the sea, came to the conclusion that Leichardt's supposed statement was an engraver's or printer's error which had escaped correction, and gave his reasons for this view in the *Proceedings* of the Royal Geographical Society, vol. i. p. 230, and subsequently in the same society's *Journal*, vol. xxviii. p. 137 (1858). Notwithstanding the extreme improbability – almost amounting to absurdity – of there being precipices of the enormous height of 3,800 feet, in a country

where there were no important mountains, and where Gregory, who had passed within eighty miles, and M'Douall Stuart, who had passed within forty miles of the place, found nothing but a moderately-elevated plateau, with ravines never exceeding 600 feet deep, no notice appears to have been taken of Mr. Wilson's correction, but the "3,800 ft." has been copied again and again in works of reputation and authority. We find it even in the new edition of the "Encyclopædia Britannica," art. "Australia," given as an established fact in the following words: – "On the north side of the continent, except around the Gulf of Carpentaria, the edge of the sandstone table-land has a great elevation; it is cut by the Alligator River into gorges 3,800 ft. deep."

The curious thing is, however, that this marvellous phenomenon, which, if it existed, would be unapproached in Australia and equalled nowhere but among the mountains of the great continents, is not even alluded to in the published journal of the traveller who is supposed to have discovered it! On Leichardt's *map* the "steep walls" are noted between the stations of November 10 and 11, but in his "Journal" we find no reference to anything remarkable till November 17, when he comes to the head of a magnificent valley, into which he was obliged to descend, and which caused him much delay and circuitous explorations on account of its steep rocky walls estimated by him to be "1,800 feet high." It is pretty clear, then, that the "3,800 feet" is a map error, and that even the 1,800 feet is merely an estimate, and probably an over estimate; for we must take into consideration the evidence of other explorers in the same region, and the appalling effects of coming, in a nearly level plateau, to the brink of such a precipitous rocky barrier.

I am making a similar correction to the above by means of a note in a work I am now engaged upon (on Australian Geography), but as the error has obtained such wide circulation and seems so hard to kill, it becomes advisable to call attention to it as soon as possible, and in a way that will be likely to attract attention.

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The Formation of Mountains

A discussion under this title went on for several weeks in Nature in late 1878 and early 1879. Wallace's three contributions follow:

S295 (12 December 1878)

In the account of M. Favre's[a] experiments in *Nature*, vol. xix. p. 103, I find the following passage: – "It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, 'the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more.' It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others."

The whole theory of these dislocations, &c., thus depends on the assumption that the interior of the globe is cooling more rapidly than the crust. This has always seemed to me an impossibility, and even an absurdity, and I shall be very glad if any of your corre-

[a] Jean Alphonse Favre (1815–1890), Swiss geologist.

spondents will explain how it is possible. I have always understood that the surface of the earth does not now derive any appreciable portion of its heat from the interior; but if the interior is cooling rapidly, to what can it part with its heat but to the crust? Volcanoes and hot springs no doubt allow a certain portion of heat to escape, but it must be an infinitesimal part of the heat of the entire mass. If the meaning of the statement is, that the heat received from the sun now keeps the surface at a permanent mean temperature, quite irrespective of central heat or cold, and that therefore the loss of heat by volcanoes, &c., causes the centre to cool while the crust does not – this may be admitted, but it is doubtful whether it can have any bearing on the effects observed. For, on this theory, all the compression would take place in that shallow superficial layer which is kept above its normal temperature by the sun's radiation; and as we go back into past time this superficial layer would be thinner and thinner. But all geological evidence goes to show that folded and contorted rocks were subject to compression at considerable depths; and further, that such contortion was greater in comparatively early than in very late geological times – both facts directly opposed to the theory in question. Will any one of our great physicists enlighten us? – Alfred R. Wallace.

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S298 (16 January 1879)

I have deferred replying to Mr. Fisher's[a] letter (*Nature*, vol. xix. p. 172) till I had an opportunity of looking at Maxwell's "Theory of Heat;" but, having done so, I am no wiser, for I do not find the point in dispute anywhere referred to. In the "English Cyclopædia," art. "Heat," I find, however, the following statement: "If we suppose the mass of the earth to have been at any remote period at a very high temperature, the effect of the radiation of its heat through the colder surrounding space would be, to cool first the superficial strata, and successively, *though in a less degree*, the internal strata." This slower cooling of the internal parts of a heated mass seems a necessary result of the "law of exchanges," to which the supposed "more rapid cooling of the interior of the globe than the crust" seems as decidedly opposed.

Mr. Fisher's illustration certainly shows how the centre *might* cool more rapidly than the outside, if heat were not subject to laws, and could set the law of exchanges at defiance. He says: "As the people disperse they move off the more quickly the further they get from the dense mass." This would be true for heat, and exactly corresponds to the quotation given above from the "English Cyclopædia;" but it is inconsistent with Mr. Fisher's statement a little further on, that the numbers in an outer belt "may continue about the same, while those in the central crowd become fewer and fewer." The two things are contradictory; and I still fail to see how the "more rapid cooling of the interior of the earth," limited as it must be to that superficial layer within which the effects of solar heat are confined, can be held to furnish a *vera causa* for the compression and contortion of deeply seated rocks and their upheaval into mountain chains. – Alfred R. Wallace.

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[a] Revd. Osmond Fisher (1817–1914), English geophysicist.

S300 (30 January 1879)

The letter of the Rev. O. Fisher in *Nature*, vol. xix. p. 266, is conclusive as to the more rapid cooling of the interior than the outer crust of a heated globe under the conditions of our earth, and I thank him for clearing up the point. But the question remains, whether the amount of contraction of the interior, and consequent crumpling of the crust, thereby produced in a definite time, is sufficient to account for the elevation of our mountains. It is necessary to take account of the following facts: –

1. That the greater part of the elevation of all our chief mountain ranges occurred during the eocene and miocene periods.

2. The warmer climates of those periods (certainly due to external and not to internal heat) would have tended to diminish the rate of cooling and consequent contraction of the earth.

3. The Rev. O. Fisher appears to have demonstrated that, even allowing for the total shrinkage due to the earth's cooling for the last hundred million years (from a mean temperature of 7,000° F., as calculated by Sir William Thomson), the amount of elevation thereby caused would be *very much* less than that of existing lands and mountains. But we know that these have been lowered by denudation, and again elevated many times over during that period.

The inadequacy of the alleged cause for the production of our existing mountains would therefore seem to be conclusively established. – Alfred R. Wallace.

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Wallace's "Australasia" (S317)

In a letter printed in the 30 October 1879 issue of Nature Wallace responded to some criticisms of his book by a reviewer.

Allow me to thank the writer of the review in *Nature*, vol. xx. p. 597, for some valuable criticisms of my book. It is quite refreshing after the common-place praises of most reviews to have one's errors pointed out and omissions noticed, and I hope to make use of such corrections in a forthcoming new edition. At the same time there are a few points on which I wish to say a word. In the first place the book is not a scientific work, but one of a series intended, as expressly stated, "for general reading." This is, of course, no excuse for errors, but it is a sufficient reason for giving *general* rather than detailed descriptions of weapons, canoes, &c., and for occasionally stating roughly the *size* of an article even when it varies greatly, in order to give definite ideas to readers who may be complete strangers to the whole subject.

I quite agree with my reviewer, that too much is included to be properly treated in one volume, but that was a matter dependent on the arrangement of the series, over which I had no control; and as I had in the earlier portion of the work overrun the space allotted me, I was obliged to restrict my notices of many parts of Polynesia, which is no doubt the most imperfect portion of the volume. It is here that the original work is most utilised, and it will be found that most of the passages criticised (including that in which I am charged with "becoming quite poetical") are [Friedrich von] Hellwald's. Of course, I

should have corrected all his small inaccuracies, but it was almost impossible to do so without rewriting his work altogether. No doubt a very interesting volume could be written on Polynesia alone by the aid of the German authorities referred to by the reviewer; but when I state that the time allowed me for the composition of the entire work was six months, and that I actually completed it in eight, it will be seen that I was compelled to limit myself in the study of authorities as well as in the space I could devote to particular islands.

I think my reviewer forgets the character of the book as essentially geographical, when he objects to my treating New Zealand apart from Polynesia; hence I cannot admit the soundness of his criticism on the comparison of the characters of the Fijians and Polynesians, a comparison which, if I remember rightly, is that of an author who knew them both thoroughly – the Rev. G. Turner. I must also demur to the implication that land can never have extended where there is now a sea 2,000 fathoms deep. I suggest (p. 564) an extension of New Zealand as far as the Kermadec Islands as having possibly occurred "at some remote epoch," and I certainly fail to see its impossibility; yet this is what is suggested by my reviewer's remark, that unfortunately there is a depth of 2,000 fathoms between them, and that such an extension "cannot therefore have existed." Moreover, the beautiful map of ocean depths with which the volume is illustrated shows a somewhat less depth than 2,000 fathoms on a slightly curved line between the islands, and I believe about the same depth exists between Madagascar and Africa, which have certainly at one time been joined.

There are some other matters touched upon on which I still venture to differ from my reviewer, especially as to the marvellous character of the Easter Island and other remains, and as to the value of the substitution of more for less liberal sectarian teaching in the Sandwich Islands; but on these points I have quoted authorities of considerable weight, and I leave my readers to form their own opinion. As to all matters of fact, I gladly accept correction from one who evidently writes with the advantage of a personal acquaintance with most of the countries referred to in his article. – Alfred R. Wallace.

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letters to the Editor on the causes of the Ice Age

In six letters to the Editor (the first sent to Geological Magazine, the rest to Nature) printed over a period of fifteen years, Wallace discussed various objections raised to his ideas on the causes of the Ice Age.

Dr. Croll's Excentricity Theory (S325: June 1880)

Sir, – In your last number Mr. Searles V. Wood advances what he considers to be "the conclusive objection" to Dr. Croll's theory of excentricity as a cause of the glacial epoch, viz. that North America was glaciated further south than Europe, in proportion to its *present* difference of winter climate, while Dr. Croll admits his theory "to be baseless unless there was a *complete diversion* of the warm ocean currents from the hemisphere glaciated."

I do not myself remember that Dr. Croll ever made such an admission, and it is

certainly not necessary for the application of his theory. But whether there was a *partial* or a *complete* diversion of the Gulf-stream from the coasts of Europe, the result anticipated by Mr. S. V. Wood – a complete *similarity* in the extension of ice over the two continents – was not to be expected, *because they are subject to very different conditions*, independently of the action of ocean currents.

Europe is interpenetrated by seas having a southward opening, while the mass of land in Western Europe is trifling compared to that of North America. Transfer the Mediterranean to America and you have a sea entering south of Cape Hatteras, and extending quite across the continent to the Sierra Nevada of California, with northward branches reaching to Lake Huron! The influence of such a sea receiving the waters of one of the largest tropical rivers (the Nile), together with the broken form of the western coast of Europe and the narrowness of the land, must be alone sufficient to give Western Europe an insular climate as compared with Eastern America. But at the same time we have on the American side conditions tending in the very reverse direction. The enormous ice-bearing masses of Greenland and Grinnell's Land[a] immediately to the north and north-east, and the Highlands of Labrador in the latitude of the Germanic plain, combined with the great *cul-de-sac* of Hudson's Bay, to receive icebergs from the north, and pile them up in its southern inlet, almost in the latitude of London, must have tended to lower the climate of North America during the Glacial epoch as much as the Mediterranean and the Bay of Biscay must have ameliorated that of Europe.

These causes of difference of climate depend on broad geographical facts, which we have every reason to believe existed during the Glacial epoch as they do now, and they appear to me amply sufficient to account for the 10° or 12° further southward extension of the ice in America than in Europe, even if the Gulf-stream were "completely diverted." But I do not believe it was completely, but only partially diverted and also diminished in intensity, and it therefore still exerted *some* differential action on the climates of the opposite coasts of the Atlantic. I would also point out that the difference between the latitudes of points with the same *winter* isothermals in West Europe and East America averages about 20°, which is much greater than the difference of the limit of glaciation in the countries, and this would show that some equalizing effect *was* produced by the diminished and partial diverted Gulf-stream, as Dr. Croll's theory requires.

Having recently been subjecting the whole of the evidence on the subject of "geological climates" to a careful examination, I may state, that I have arrived at an important modification of Dr. Croll's theory, which will, I believe, obviate the chief objections that have hitherto been made to it. The subject will be fully discussed in a volume I am now engaged in printing.

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Geological Climates (S331: 9 December 1880)

It was with great surprise I read Prof. [Samuel] Haughton's unqualified statement in last week's *Nature*, that – "It is *impossible* to suggest any rearrangement of land and water which shall sensibly *raise the temperature of the West of* Europe," – since I had, as I thought, in my recently-published volume – "Island Life" – not only "suggested" such a

[a] Grinnell Land forms the central part of Ellesmere Island in arctic Canada.

rearrangement, but also adduced much evidence to show that it had actually occurred throughout the periods when both the West of Europe and the Arctic regions enjoyed a much higher temperature than they do now. I will now briefly re-state my "suggestion," and will also make a few remarks on the general causes of difference of temperature, which may serve to render the subject more intelligible.

It is now well known that places in the temperate zones owe their temperature at different seasons only partially to the amount of direct sun-heat they receive, but very largely to the amounts of heat brought to them by currents of air. Thus we explain, not only the mild winter climate of our islands as due to the prevalence of westerly and south-westerly winds which have become warmed by passing over the Atlantic, but also the wonderful inequality of temperature at different seasons of the year. When we have warm spring-like days in mid-winter, it is because these warm currents of air are passing steadily over our islands; while continued hard frosts are as clearly due to masses of cold air from the north or north-east which drift down to us, often with no perceptible wind. Again, when in April and May we have days as cold as those of December and January, they can always be traced to northerly or easterly currents of air, and are probably often connected with the southern drift of the icebergs at that season. It is clear then, that if south-westerly winds were to continue throughout the winter, the severity of that season would be entirely abolished; and the same effect would be produced if by any means the winds from the north and east lost their severity.

Now the source of the constant warmth of our westerly winds is admitted to be the influx of warm water into the North Atlantic – chiefly by the Gulf Stream; and this warm northward flow of tropical water, being primarily due to the trade-winds, is not confined to the Atlantic, but is equally present in the other great oceans, and similar effects are produced in them, though nowhere to so great a degree as in our islands, owing to our insular position and the great extent to which Europe to the east of us is permeated by water as compared with North America or Asia. The North Pacific, with its great Japan current, is probably quite as warm as the North Atlantic; but Vancouver's Island, though further south than London, has not so mild a climate; and this can be clearly traced to the great mass of land to the east and north of it, the lofty snow-clad mountains, and the absence of those deep gulfs and inland seas which do so much to ameliorate the climate of Europe.

Prof. Haughton states, in his "Lectures on Physical Geography," that the Kuro Siwo, or great Pacific current, is two and a half times as large as the Gulf Stream, while the Mozambique current, which forms the outflow of the warm waters of the Indian Ocean, is one and a half times as much, so that these two currents have together four times the bulk and heating power of the Gulf Stream. If therefore these two currents at any time obtained an entrance into the Arctic Ocean, it is difficult to over-estimate their effect on its climate. The Gulf Stream, of which probably not half passes northwards of our islands, gives to Iceland the same winter temperature as Philadelphia, and keeps the North Cape (far within the Arctic circle) permanently free from ice, and this, notwithstanding the powerful counteracting influences of the lofty Scandinavian mountains on the one side, and the huge ice-clad plateau of Greenland on the other. Suppose that only an equal proportion of the Kuro Siwo entered the Arctic Ocean, is it not probable that no sea-ice at all would form there? While, if Greenland were less elevated and thus ceased to be an accumulator of ice, the combined effect might be to render the whole Polar area free of icebergs. This would at once do away with the chief source of winter cold to all north

temperate lands, and ameliorate the climate of America as much, proportionately, as that of Europe. But we have yet to consider a still more powerful agent in ameliorating the climate of Western Europe in Secondary and early Tertiary times. The heated waters of the Indian Ocean have now no northern outlet, and only penetrate the continent in the sub-tropical Red Sea and Persian Gulf. Now if we suppose the waters of the Bay of Bengal and the Arabian Sea to have had northward outlets through the heart of the Euro-Asiatic continent, penetrating in two or more directions into the then much more extensive Arctic Ocean, we should have an agency at work which would render the presence of any permanent ice in the North Polar area as impossible as it is now in Scotland. The cooling agency of ice being once abolished, the comparatively small area of the Polar as compared with the Tropical seas (about one-tenth) would facilitate the raising of the temperature of the former to perhaps 15° or 20° F. above the freezing point, and this would not only give the Arctic lowlands a climate quite sufficient for the vegetation which we know they supported, but, by doing away with the only source of our winter cold, would give our islands a perfect immunity from frosts and render them capable of supporting the vegetation now characteristic of sub-tropical lands.

That the modifications of land and sea here indicated *did* exist throughout a considerable portion of past geological ages, and that the existing consolidation of the great northern continents, to which the possibility of our present Arctic climates is mainly due, is a comparatively recent and abnormal phenomenon, I have endeavoured to prove in the work already referred to. At present I have only undertaken to show, that a "suggested" re-arrangement of land and water adequate to raise the temperature of Western Europe to a very sensible, or even to a very large extent, *is* "possible."

* * *

Geological Climates (S335: 6 January 1881)

I should not say more on this subject, but that the last paragraph of Mr. [John] Starkie Gardner's letter seems to imply that I have adopted some of his views without acknowledgment. Now I certainly read his article in *Nature* of December 12, 1878, with much interest and profit; but, as regards the special question of the cause of the mild climates of Eocene and Miocene times, I entirely disagreed with his views, as is sufficiently shown by my recent letter in *Nature*. I quite admit that the closing up of the North Atlantic between Europe and North America *might* have considerably raised the temperature of Britain, but it would just as certainly have rendered the Arctic regions even colder than they are now, by shutting out the Gulf Stream, whereas all the evidence points to *continuous* mild Arctic climates through Cretaceous, Eocene, and Miocene times. Again, though I admit that there has probably, on more than one occasion during the Tertiary period, been a land connection between North-West Europe and North-East America, yet the peculiar distribution of the Tertiary mammalia of Europe and North America indicates that such connection was exceptional, and only endured for very short periods, the rule being a separation like that which now exists. I could therefore only have quoted Mr. Gardner's view to disagree with it; and I did not think it advisable to encumber the exposition of my own theory with more references of this kind than were absolutely necessary. I may add, that the extension of the Miocene Arctic flora to Grinnell Land since Mr. Gardner's article appeared, renders his views still more untenable. Of course I here refer to my chapter on

"Mild Arctic Climates" in "Island Life." In my letter to *Nature* I confined myself strictly to the point raised by Prof. Haughton, which I did not consider had been adequately met by Mr. Gardner's hypothesis.

* * *

Geological Climates (S336: 20 January 1881)

The letter of Prof. Haughton in last week's *Nature* so bristles with figures and calculations that some of your readers may feel a little puzzled and may be unable to detect the fallacies that lurk among them. The question is far too large a one to be fully discussed in your columns. I shall therefore confine myself to pointing out the erroneous assumptions and false inferences which vitiate all the learned Professor's calculations, having done which my own theory will remain, so far, intact.

The whole argument against me is based upon an "ideal ice-cap," extending from the Pole to lat. 60°. A considerable but unknown thickness is given to this imaginary field of ice, and it is then calculated that the three great ocean streams, even if admitted to the Arctic area in the manner I suggest, would not get rid of this mass of ice. There are however several important misconceptions and illogical deductions underlying the whole argument, and when these are exposed the results, however accurately worked out, become completely valueless.

We first have it stated that if heat and cold were uniformly distributed over the Polar regions the whole would be permanently frozen over, and an ice-cap be formed of great but varying thickness, diminishing from the Pole to about lat. 60°. But even this preliminary statement is open to serious doubt; for ice cannot be formed without an adequate supply of water, and over a large part of the Polar area no more snow falls than is annually melted by the sun and by warm southerly winds blowing over the heated land-surfaces of Asia and America. Admitting however that any such ice-cap could be formed, it would certainly not form in *one year* but by the accumulations of a long series of years; and any estimate of the *total* heat required to melt it has no bearing whatever on the *annual* amount that would be sufficient, since this depends solely on the average thickness of the ice *annually* formed, of which Prof. Haughton says nothing whatever.

The amount of rainfall in the Arctic regions (mostly in the form of snow) is certainly very small. It is estimated by Dr. Rink[a] to be only twelve inches in Greenland, and this is probably far above the average. All that falls on the inland plains of Asia, Europe, and America is however melted or evaporated by the action of the sun and air far from the influence of the Gulf Stream. The thickness of ice formed annually over the whole area of the Arctic Ocean I have no means of estimating. In open water in very high latitudes it may be considerable, but perennial ice-fields can only increase very slowly. I should therefore very much doubt if the thickness of ice now formed annually over the whole Arctic area averages nearly so much as five feet; and Prof. Haughton himself calculates that our own Gulf Stream is now capable of melting this quantity.

The first assumption, therefore – that the amount of heat required to be introduced into the Arctic regions in order to raise their mean temperature above the freezing-point is "accurately measured" by the amount required to melt an "ice-cap" covering the whole

[a] Hinrich Johannes Rink (1819–1893), Danish geologist and paleontologist.

area to a thickness of several hundred feet – is grossly erroneous; and it is so because it takes the hypothetical *accumulated* effects of *many years* Arctic cold under altogether impossible conditions, and then estimates the amount of heat required to melt this whole accumulation in *one year*!

But we find a second and equally important error, in the assumption (involved in all Prof. Haughton's arguments and figures) that all the ice of the alleged "ideal ice-cap" must be melted by that portion of the Gulf Stream which actually enters the Polar area, where its temperature is taken to be 35° F. or only 3° above the melting point of ice. A large quantity of the Arctic ice, however, even now floats southward to beyond lat. 50° in both the Atlantic and Pacific, and is melted by the warmer water and atmosphere and the hotter sun of these lower latitudes. Now, as it is an essential part of my theory that much of Northern Asia and North America were under water at those early periods when warm climates prevailed in the Arctic regions, it is clear that whatever Arctic ice was then formed would have a freer passage southwards, and as the south-flowing return currents would then have been more powerful and more extensive than at present, a much larger proportion of the ice would have been melted by the heat of temperate instead of by that of Arctic seas.

Prof. Haughton admits that the Kuro Siwo and the Mozambique currents together, if they entered the Polar seas, would be equal to the melting of a layer of ice more than thirteen feet thick over the whole area down to lat. 70°. But if our own Gulf Stream is sufficient to get rid of the whole of the ice that now forms annually – as Prof. Haughton's figures show that it would probably be, and as it would be still more certainly were Greenland depressed, thus ceasing to be the great Arctic refrigerator and ice-accumulator – then the heat of the other two currents would be employed in raising the temperature of the Arctic seas above the freezing-point; and if we take the area of the water as about equal to that of the land, we shall have heat enough to raise the whole Arctic ocean to a depth of full 180 feet more than 20° F., or to a mean temperature of 52° F., and as this would imply a still higher surface temperature it is considerably more than I require.

Unless therefore Prof. Haughton can prove that the amount of ice now forming *annually* in the Polar regions is *very much more* than an average of five feet thick over the whole area, his own figures demonstrate my case for me, since they prove that the rearrangement of land and sea which I have suggested would produce a permanent mild climate within the Arctic circle and proportionally raise the mean temperature of all north-temperate lands.

Briefly to summarise my present argument: – Prof. Haughton's fundamental error consists in assuming that the true way of estimating the amount of heat required in order to raise the temperature of the Polar area a certain number of degrees is, – first, to suppose an accumulation of ice indefinitely *greater* than actually exists, and then to demand heat enough to melt this accumulation *annually*. The utmost *possible* accumulations of ice in the Arctic area, during an indefinite *number of years*, and under the most *adverse physical conditions* imaginable, are to be all melted in *one year*; and the heat required to do this is said to be the "accurate measure" of that required to raise the temperature of the same area about 20°, at a time when there were no such great accumulations of ice and when all the physical conditions *adverse* to its *accumulation* and *favourable* to its *dispersal* were immensely more powerful than at present!

When this fundamental error is corrected, it will be seen that Prof. Haughton's

calculations are not only quite compatible with my views, but actually lend them a strong support.

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The Cause of an Ice Age (S520: 9 January 1896)

The letter of Prof. G. H. Darwin in your last issue states very clearly the argument on which Mr. Culverwell[a] and himself rely as affording a demonstration of the inadequacy of the astronomical theory. It now seems opportune, therefore, to lay before your readers the general considerations which lead me to the conclusion that the whole argument they rest upon is unsound; and, further, that Sir Robert Ball's ratio of 63 to 37, representing the ratios of sun-heat received by each hemisphere in summer and winter respectively, is (contrary to Prof. Darwin's view) an important factor in any adequate discussion of the problem.

Accepting Prof. Darwin's estimate that the difference in the amount of sun-heat received in our latitudes during high and low eccentricity, would only give to Yorkshire the amount received by London or *vice versa*, I entirely demur to his statement that this would be also a measure of the amount of change in the climates of these places. To do so is to assume that the climate of a place, as regards the amount and distribution of its temperature, is determined by one factor only – the amount of sun-heat it receives.

How very erroneous is this assumption, may be shown by the contrasted climates of places on the east and west sides of the Atlantic, due to the influence of both ocean-currents and prevalent winds; but even more strikingly by a comparison (which I made in my "Tropical Nature") between certain tropical and temperate climates. In Java, about 8° south of the equator, the altitude of the noonday sun in June is about 58½ degrees, while at London during the same month it is 62°, the length of the day at the same time being 5½ hours greater with us. The sun-heat received in London must therefore be *considerably greater* than that received in Java, and, according to the rule that the *amount* of sun-heat determines temperature, London should then have the warmest climate. The fact, however, is that our mean temperature in June is more than 20° lower than that of Java and our mean highest temperature about 18° lower, a result due, as I have shown, to a variety of causes, of which the temperature of the atmosphere in all surrounding areas, the action of aqueous vapour in reducing the loss by radiation, and the accumulation of heat in the soil, are probably the most important. These facts prove, I think, that the amount of heat received by the whole hemisphere, through its influence on both oceanic and aerial currents, must be taken account of in estimating temperatures under different phases of eccentricity; and that any determination of the amounts of sun-heat received at particular latitudes, considered by themselves, are necessarily misleading and must usually indicate a difference of climate far below the truth.

But there is another consideration of even more importance which entirely invalidates the arguments of those who, like Mr. Culverwell and Prof. Darwin, treat the problem as one to be determined by a simple mathematical calculation of amounts of sun-heat re-

[a] George H. Darwin (1845–1912), English astronomer (and Charles Darwin's son); Edward P. Culverwell (1855–1931), English physicist and educator.

ceived on the same area at different times. This is, the remarkable difference in the behaviour of air and liquid water on the one hand and snow and ice on the other, as regards climate; the former from their great mobility tending to the diffusion of heat, the latter by its comparative immobility to the accumulation and perpetuation of cold. Without this power of accumulation perpetual snow on tropical and temperate mountains, and glaciers in hot sub-alpine valleys and at only 705 feet above the sea-level in latitude 43° 35′ south in New Zealand, would be impossible. In either of these cases, if an elevation of about a thousand feet should double the area of the snow fields, which might easily be the case, the outflowing glaciers would be greatly increased in magnitude and might either descend to much lower levels or spread out over large areas of the lowlands – and all this without any change whatever in the total amount of sun-heat received by the countries in which they occur.[1]

For some years past there has been a persistent attack by astronomers and physicists on the explanation of the glacial epoch put forth by Croll and adopted with some modifications by many students of glacial phenomena. But as these writers have all treated the problem as a question of the direct effect of the amount of sun-heat received at different epochs in corresponding latitudes, completely ignoring the great distributing and accumulating agencies which are always and everywhere in action, their theoretical conclusions appear to us to be entirely beside the question. We have to deal with a highly complicated problem in physical meteorology, which cannot be solved by an appeal to the well-known facts of the amounts of sun-heat received, any more than can the June climates of London and Batavia or the general climates of Ireland and Manitoba or Terra-del-Fuego (in about the same latitude) be explained from similar data. The great merit of Croll was, that he fully realised the complexity of the problem; that he took account of the various relations and reactions of the oceanic and aerial currents, and the physical characteristics of air and water, snow and ice; and that he showed how these causes reacted on each other so that the winds and ocean currents of one hemisphere might have an influence on the accumulation of snow and ice in the other. Whatever errors he may have made in matters of detail, his method was undoubtedly a sound one, and it is because so many recent writers on the subject have wholly ignored his method without even attempting to prove that it is erroneous, that their views appear to us to be both retrograde and scientifically unsound.

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The Astronomical Theory of a Glacial Period (S521: 6 February 1896)

Mr. Culverwell has pointed out to me that I am in error when I include him among those writers who think that the problem of glacial periods is to be solved by considering only the varying amounts of sun-heat at different epochs. On referring to his paper, which I had not at hand when I wrote, I find that this is the case, and that he is careful to limit his calculations as giving only the variations of temperature due to *direct* sun-heat. He also discusses, though very briefly and inadequately, the effects due to *transference* of heat from one area to another. Although willingly making this correction at his request, I am

[1] This remarkable property and its effects are explained in some detail in my "Island Life," p. 131 (second edition), under the heading "Properties of Air and Water, Snow and Ice, in Relation to Climate," and in the four following sections.

still, after another perusal of his paper, quite unable to see that it finally disposes of Croll's theory, much less of that modification of it which I have myself set forth.

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Climates of Vancouver Island and Bournemouth (S333)

A letter to the Editor, also concerning glaciology, printed in the Nature issue of 23 December 1880.

I think it very probable that your correspondent Capt. Verney is right about the climate of Vancouver's Island [*see* S331, above]. My only sources of information were maps of isothermals in Keith Johnston's and Phillips' Atlases, which show the *mean* temperature about the same as that of the south of England, while the *winter* temperature is shown as being decidedly colder, and it was to this I more especially referred. The mainland of British Columbia is undoubtedly colder than that of Western Europe, but Vancouver's Island itself and the adjacent sea may be really milder; and if so it is another proof of the great power of the returning Japan current.

I shall be very glad of Prof. Haughton's criticisms on my hypothesis; and in the mean time will only say: 1. That unless Bournemouth is *never* cooled by north and north-east winds, any amelioration of the climate of the Polar regions would certainly benefit it. 2. That as by my hypothesis the entrance of two new gulf-streams into the Arctic Ocean would entirely prevent the formation of ice; the return currents that would undoubtedly be produced would *not be cold currents* in the sense in which they are now, as they would probably be always considerably above the freezing point. – Alfred R. Wallace.

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Mr. Wallace's Reply to Mr. T. Mellard Reade on the Age of the Earth (S367)

When William Thomson (later Lord Kelvin) used a pre-nuclear physics understanding to suggest in 1864 that our Sun might not be old enough to permit a Darwinian interpretation of evolution, Darwin and Wallace reacted rather differently to this challenge. Darwin hoped there was something wrong with Thomson's reasoning (there was), and let the matter go. Wallace attempted a reconciliation: by trying to estimate the age of the earth with a calculation of how long it would take to produce the observed sedimentary rock record. His first efforts were published in an 1870 paper in Nature, but the discussion, as we will see, went on for decades. This letter was printed in the October 1883 issue of Geological Magazine.

I have just received from Mr. T. Mellard Reade, F.G.S., a copy of his paper on the "Age of the Earth" (which appeared in your *Magazine* of July last), in which I am asked to put that gentleman right as regards what he calls his "analysis" of some figures and

estimates given in my "Island Life"; and I gladly seize the first opportunity of doing so. To avoid the necessity of repeating my own statements as well as those of Mr. Reade, I must ask the reader who is interested in this matter to refer back to the above-mentioned article.

The first statement of Mr. Reade's which I have to "put right" is the following: – "It is evident, if the figures mean anything at all, that three millions of square miles 177,200 feet thick represent the whole of the rock removed by denudation in all forms since the geological history of the earth began. Spread this over 57 million square miles of land and we get a deposit 9326 feet thick deposited in all geological time." This is not quite an accurate representation of my statements. The figures quoted represent, not the whole matter denuded, but only that portion of which a record still exists in the rocks; and this matter has been deposited, not "in all geological time," but only in that portion of geological time indicated by the known series of stratified rocks; unconformities and other breaks representing unknown intervals of which we have no record. With these corrections the figures used by me do imply what Mr. Reade says they do; which is, in other words, – that the average thickness of that portion of the earth's crust formed by the known stratified rocks does not probably exceed nine or ten thousand feet.

Mr. Reade, however, without directly impugning these figures, attempts to show that they lead to absurd or incredible results, and he does this by manipulating them in a way which is altogether beyond my comprehension. He first says, that these rocks have been made and destroyed over and over again; and then argues that, *because* the exposed igneous rocks cover about 1/12 of the land surface, *therefore* "each particle of rock, on the average, has been denuded and laid down at least twelve times." I have in vain tried to see any connection between these two statements, but what follows is still more unintelligible. Mr. Reade adds: – "From this it follows that the actual thickness of the sedimentary crust of the earth, if there were no sedimentary rocks except on the site of the present land areas, *would be* 12/9326 = 777 feet." Correcting the clerical error of 12/9326 instead of 9326/12, this means that, because the stratified rocks have been successively formed from the denudation of older rocks (stratified and igneous), therefore their *actual* thickness would be many times less than by estimates founded on direct measurement it is known they *actually* are! It is, I think, evident that, from Mr. Reade's point of view, he should have here multiplied instead of divided by 12. For if the older rocks have been reduced in thickness by denudation, and their débris has gone to form newer rocks in each successive epoch, it is clear that when first deposited all the rocks *would have been* thicker than now, though there is no definite relation between the number of successive formations and their greater thickness, as Mr. Reade seems to suppose. For example, if half the original Palæozoic rocks have been denuded to form the Mesozoic and parts of the later rocks, and half of the original Mesozoic to form the Tertiary, and half these again to form glacial and recent deposits, each would have been at first about twice as thick as it is now, – not one-fourth the thickness, as Mr. Reade's mode of calculation would make them; and as the whole problem is one of the *time* taken to produce these various deposits, the greater original thickness would have to be used in the calculation.

But, even if Mr. Reade's figures are thus corrected, his whole criticism is radically unsound; for, as I have explained in my original discussion of the subject, denudation is so unequal in its action and occurs so generally on the edges of uplifted strata not over their surfaces of deposit, that it would be quite possible for 9/10 or even 99/100 of a formation to be destroyed by denudation, and yet for the remaining 1/10 or 1/100 to give a fairly

accurate measure of the *average* thickness or even sometimes of a *maximum* thickness of the original deposit. Our measures of the thickness of the sedimentary rocks will, therefore, not be seriously affected by the fact that by far the larger portion of all of them have been destroyed by denudation, and again and again laid down to form newer rocks; and as I have used measures of the *maximum* thicknesses, I have considered that these would in all probability not differ much from the original *average* thicknesses of the same rocks before they had suffered denudation. No doubt some rocks may have been wholly destroyed by denudation, or are so covered up by later deposits as to be beyond our reach, and to allow for these I am willing to admit that my estimate of the whole thickness of the rocks, and therefore of the time taken to produce them, may have to be considerably increased; but this would bring my figures nearer to those usually arrived at, not enormously further from them as Mr. Reade endeavours to prove.

Yet again, Mr. Reade points out that continents have fluctuated, and have sometimes been larger than now. To allow for this he doubles the land surface and reduces the corresponding thickness of the strata to one-half! But, surely, if the continents have been sometimes larger, they have also been sometimes smaller, and I see no reason to think we can take any fairer *average* than that of the present area; and even if the average *had* been double, then the denudation and the deposit would presumably have been double also, not half as Mr. Reade suggests.

With regard to my fundamental position – that the areas of deposition are (and always have been) very much smaller than the areas of denudation, and that, in making any estimate of geological time founded on the thickness of the sedimentary rocks and the known rate of denudation, this fact must be taken account of, Mr. Reade makes no objection; and, whatever "confusion of ideas" may have pervaded my estimate, the subject has certainly not been rendered clearer by his criticism.

Finally, as regards the general theory of the "Permanence of Oceans and Continents" (or, more properly, of Oceanic and Continental areas), which Mr. Reade somewhat sneeringly remarks "is now becoming fashionable," – it is time that its opponents should give up petty criticism of unimportant details or collateral issues, which have little bearing on the main question, and attempt to grapple with the whole body of facts and arguments adduced in its support by some of the first geologists of the day, and which I have endeavoured to set forth in a connected form in the pages of "Island Life." Any such general examination of the question from an adverse point of view, I have hitherto failed to meet with.

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The Permanence of Ocean Basins (S457)

Part and parcel of Wallace's model of biogeographic regionalization was the understanding that the continents and oceanic basins were fundamentally permanent structures relative to one another (a model that was overturned by the plate tectonics theory that emerged in the early 1960s). From time to time Wallace defended his position, as in this letter printed in the November 1892 issue of Natural Science.

It seems desirable that I should say a few words in reply to Dr. Blanford's letter in the last issue (p. 639), and to Mr. Jukes-Browne's[a] article in the preceding number of *Natural Science* (pp. 508–513).

Both these writers lay stress upon a supposed considerable modification of my views in my late paper. I reply that there is no real alteration; because the 1,000-fathom line was never adduced by me as an absolute and rigid boundary between the oceanic and continental areas, but as a general indication of their respective limits according to the best obtainable evidence. In proof of this, I may point out that even in the first edition of "Island Life" (p. 444) I refer to a submarine plateau at a depth of between 1,000 and 2,000 fathoms, which stretches southward from New Zealand towards the Antarctic continent, and suggest an ancient connection "with the Great Southern Continent by means of intervening lands and islands"; and in my "Darwinism" (p. 346) I define the permanence of oceanic and continental areas as meaning that, "while all of them have been undergoing changes of outline and extent from age to age, they have yet maintained substantially the same positions, and have never actually changed places with each other"; and again, at p. 347, after stating that the 1,000-fathom line "marks out, approximately, the continental area," I add: "There may, of course, have been some extensions of land beyond this limit, while some areas within it may always have been ocean; but so far as we have any direct evidence, this line may be taken to mark out, approximately, the most probable boundary between the continental areas and the great oceanic basins."

In my recent paper I have merely repeated and enforced these statements by showing how little real difference is made by carrying the possibilities of Continental extension, in rare cases, as far as the 1,500- or 2,000-fathom line. There is, therefore, no ground for alleging any departure from supposed "extreme views" which I formerly held, since the fresh arguments I have adduced show any great extension beyond the 1,000-fathom line to be in the highest degree improbable.

Dr. Blanford objects to my statement that the theory of the permanence of the ocean basins was "attacked" by him. Perhaps the word was not well chosen, and I should have said "criticised," but it was held by Mr. Jukes-Browne to be such a damaging criticism that (in private correspondence) he expressed surprise that I had not replied to its arguments in the new edition of "Island Life." But though the passage quoted by Dr. Blanford is not very antagonistic, there is much in the "Address" itself that is altogether opposed to my views. For instance, the writer argues in favour of a former land-connection between South America and Africa, which he says is "chiefly shown by tropical forms," though adding, "but these may have migrated far southward during warm periods;" and, later on, he speaks of a possible "girdle of land, chiefly in low latitudes, round nearly three-quarters of the globe, from Peru to New Zealand, and the Fiji Islands." But any such land-extension as this is so wholly inconsistent with the permanence of the great ocean basin of the Pacific, that to argue in favour of it is certainly to attack the theory of permanence. Again, a direct land-connection between South Africa, Madagascar, and the Peninsula of India is very strongly advocated, but the 1,000-fathom line shows us an actual though slightly circuitous connection by means of existing continental areas between Africa and India, and to hold that this would have been insufficient – why, I cannot possibly understand – and to prefer to bridge across an ocean between 2,000 and 3,000 fathoms deep, in

[a] William Thomas Blanford (1832–1905), English naturalist; Alfred John Jukes-Browne (1851–1905), English geologist.

order to reduce the distance to about three-quarters of the other route, is again to hold views totally inconsistent with the theory of ocean-permanence.

Even in the passage from his "Address" quoted by Dr. Blanford, there is more antagonism than agreement; for I cannot admit that "there is no evidence whatever . . . that every ocean-bed now more than 1,000 fathoms deep, has always been ocean." If by "every ocean-bed" we mean the great oceanic basins as distinguished from deep seas within continental areas, and if we admit those very rare and limited encroachments of old continents on the margins of these oceans, which, as I have shown, I have always admitted, then I maintain that there is very strong evidence indeed of the permanence of all the great ocean-basins, and that the 1,000-fathom line still gives us the best indication of the general limits of the old continental areas. This is so, because the facts adduced in my last paper show that any extension of the continents into the oceanic areas much beyond the 1,000-fathom line would necessitate the submergence to great depths of many times their area of existing land.

Before concluding, I must briefly notice a very extraordinary claim of Mr. Jukes-Browne. He says ("Evolution of Oceans and Continents," p. 510): – "Those who oppose the doctrine of permanence say that the present continents are the outcome of a long series of geographical mutations," each phase being "an episode in a long process of geographical evolution."

But this is exactly what those who *uphold* permanence have always said. Dana taught the evolution of the American continent nearly forty years ago; Sir Archibald Geikie has discussed "Geographical Evolution" with permanence of continental areas; and I myself, following these great masters, have endeavoured to sketch out the process of this evolution and its results. This "process of evolution" necessarily implies permanence of position of the continental as regards the adjacent oceanic areas – the only permanence that has ever been postulated by myself or others, and it is simply amazing to find this very process now claimed as if it were the discovery and the distinctive teaching of those who oppose permanence!

In conclusion, I may say that, while admitting with pleasure the growing approximation of views on this subject, I cannot forget that it has been, and still is with many writers, the practice to assume former continental extensions across the great oceans in order to explain difficulties in the distribution of single genera or families; that geologists of repute have claimed the Dolphin bank in the Atlantic trough as the relic of a chain of mountains comparable with the Andes; that oceanic islands have been recently claimed to be merely the tops of submerged mountains, which can only be properly compared with the highest points of continents, and that a geological critic so late as 1879 considered the idea that the oceans had always been in their present positions "a funny one." If such extreme views are now less common than they were, I hope that I may, without presumption, claim to have had some share in bringing about the change in scientific opinion now in progress.

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letters to *Nature* on the age of the Earth

Before radiometric dating, workers could only guess at the absolute age of the

earth. As mentioned earlier, Wallace's main approach to the problem involved calculating rates of surface erosion, and what that meant in terms of the thicknesses of the rock units that had built up. In three letters to Nature in the 1890s Wallace tried to clarify his views on the matter.

The Earth's Age (S458: 22 December 1892)

I am glad that Mr. Hobson[a] has formulated his difficulty as to the measurement of geological time by the comparative rates of denudation and deposition, because it shows that I cannot have explained my views as clearly as I thought I had done; yet on again reading over pp. 217–223 of "Island Life," I can hardly understand how he has missed the essential point of the argument. Fortunately, there is no dispute as to the data, only as to the conclusions to be logically drawn from them.

Mr. Hobson says that I account for a deposit of 177,200 feet (the supposed thickness of all the stratified rocks) over an area of 3,000,000 square miles (the estimated area over which at any one epoch stratified rocks are being deposited) in 28,000,000 years (the deduced estimate of known geological time); and then adds: "Whereas, what has to be accounted for is an area of 57,000,000 square miles *of the same thickness*" (my italics). This seems to me a most amazing misconception; for it means that every single formation and every stratum or member of each formation, was deposited to the same average thickness over the whole land surface of the globe (area 57,000,000 square miles)! And this implies that at every successive period, from the Laurentian to the Pliocene, the conditions of denudation and deposition were totally different from what they are now, since at the present time it is demonstrable that the area of deposition of continental debris is only a fraction of the whole continental area. It implies further, that during each geological period the whole of the existing land area must have been, either at once or in rapid succession, sunk beneath the sea in order to allow of its being all covered with each successive formation – an amount of repeated upheaval and depression which hardly the most extreme convulsionist of the old school would have postulated. I cannot make the matter clearer, and trust that on further consideration Mr. Hobson will admit that his objection is invalid.

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The Earth's Age (S460: 5 January 1893)

The first part of Mr. Hobson's letter alone requires notice from me, as the latter part characterizes as absurd the views of those eminent geologists who have estimated the total thickness of the sedimentary rocks, and seems to assume that such writers as the late Dr. Croll and Sir Andrew Ramsay overlooked the very obvious considerations he sets forth.

As regards myself, he reiterates the statement that when geologists have estimated the total thickness of the sedimentary rocks at 177,200 feet, they mean that this amount of sediment has covered the whole land surface of the globe; that, for example, the coal measures, the lias, the chalk, the greensand, the London clay, &c., &c., were each deposited over the whole of the continents, since it is by adding together the thicknesses of

[a] Bernard Hobson (1860–1932), English geologist.

these and all other strata that the figure 177,200 feet (equal to 33 miles) has been obtained.

Mr. Hobson concludes with what he seems to think is a *reductio ad absurdum*: – "Dr. Wallace's calculation leads to the absurd result that continents are growing nineteen times as fast as materials are produced to supply their growth."

But the apparent absurdity arises from the absence of any definition of the "growth of continents," and also from supposing that the growth of continents is the problem under discussion. The question is, as to the growth in thickness, of sedimentary deposits such as those which form the geological series. These deposits are each laid down on an area very much smaller than the whole surface of the continent from the denudation of which they are formed. They are therefore necessarily very much thicker than the average thickness of the denuded layer, and the ratio of the area of denudation to the area of deposition, which I have estimated at 19 to 1, gives their proportionate thickness. If Mr. Hobson still maintains that he is right, he can only prove it by adducing *evidence* that every component of the series of sedimentary rocks has once covered the whole land-surface of the globe; not by assuming that it has done so, and characterizing the teaching of all geologists to the contrary as absurd.

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The Age of the Earth (S513: 25 April 1895)

In Dr. Hobson's letter on this subject, he confuses the argument by the introduction of a new factor (never alluded to in the former discussion, or in my theory as stated in "Island Life"), the *bulk* or *volume* of the matter deposited. This has nothing whatever to do with the practical problem, because it is admittedly impossible to form *any* estimate of the total bulk of all the stratified rocks of the earth during all geological time; while it is equally impossible to form any estimate of the total bulk of the denuded matter, since we have no clue whatever to the number of times the same areas have been again and again denuded. But the maximum *thickness* of the same rocks, compared with the average *rate* of denudation, and the coincident maximum *rate* of deposition, *do* furnish materials for an estimate, since they can all be approximately determined from actual observation; and the result is what I have given. If Dr. Hobson had referred to the former discussion he would have avoided imputing to me "fallacies" which I never made. I never said a word about "equal bulks" of material being deposited in less time than they were denuded. But, as the only available data are those of *thickness,* not bulk, then it is clear that, if the area of deposition is one-nineteenth of the area of denudation, the *rate* of deposition of a known *thickness* of rocks will be nineteen times as great as the known *rate* of denudation. It was necessary for me to point this out when first discussing the subject, because one eminent writer had made the rate of deposition *less* than the rate of denudation, because the water-area is greater than the land-area of the globe; while an eminent geologist has quite recently taken the rates of denudation and deposition as being *equal*. If, however, the area of deposition is very much *less* than the area of denudation, which is now admitted to be the fact, then the *rate* of deposition *per foot of thickness* will be many times *greater* than the *rate* of denudation.

I should not have thought it necessary again to state this very obvious conclusion, had not Prof. Sollas, while so clearly pointing out Dr. Hobson's misconception as to the area

over which the maximum thickness of the strata extended, omitted to refer to the confusion he has now for the first time introduced into the problem, by references to the *bulk* or *volume* of the sedimentary rocks, a factor which all previous writers have seen to be wholly beyond even an approximate determination.

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letters to *Nature* on the glacial origin of alpine lakes

Wallace was a supporter of Sir Andrew Ramsay's (1814–1891) theory of the glacial origin of alpine lakes, which some thought might be caused by other forces. Remarks offered in Nature led Wallace to make a reply, which later led to further replies in the same journal, as well as a long two-part paper published in the Fortnightly Review.

The Glacier Theory of Alpine Lakes (S462: 8 March 1893)

The letter of the Duke of Argyll against the theory of the formation of alpine lakes by glacial action shows such an amount of misconception of the theory itself, and so completely ignores the great weight of evidence in its favour, that a few words on the other side seem desirable.

The Duke says that glaciers "do not dig out," do not "act like a ploughshare," but, when moving down a slight incline do "scoop," as well as rub down and abrade. No observer of glaciers has ever stated, so far as I know, that they do "dig out," and it is equally erroneous to say that they "scoop," for that implies that it is the end of the glacier that acts. But the end is its weakest point, where it is melting above and below, and where consequently it can do practically nothing. The whole action of a glacier is a grinding action, and its grinding power is greatest where it is thickest, and where, consequently, it presses on the rocks with the greatest weight. The result of this grinding is seen in the muddy stream issuing from all existing glaciers; while the well-known "till" is the product of the rock grinding mill of ancient glaciers and ice-sheets.

Notwithstanding the Duke's disbelief in ice-sheets I venture to think that their former existence has been demonstrated both in Scotland and Ireland; but leaving this point, I wish to make a few remarks on the extreme inadequacy of the earth-movement theory to account for the facts. In the first place it is certain that no alpine lake can possibly have a long life, geologically speaking. In the course of a few thousands of years, certainly in less than a hundred thousand, all alpine lakes would be filled up by the sediment brought into them. It follows that all the existing lakes must have been formed about the same period, and that, geologically, a very recent one, and corresponding approximately with that of the well-known glacial epoch. But if these lakes were all formed by earth movements, either just before the glacial epoch came on, or during its continuance, or afterwards we have to explain the remarkable fact that such movements only occurred within the limits of glaciation, never beyond those limits. In Wales, Cumberland, and Scotland, in the Alps, in Scandinavia, in Finland, in the northern United States and Canada, in Mongolia and Thibet, in Tasmania and New Zealand, we have thousands of rock-basin lakes, amid palpable signs of glaciation. But the moment we pass beyond the

glaciated districts, mountain lakes abruptly cease. There are hardly any in Spain, none in the Great Atlas, none in Sardinia or southern Italy, except in the volcanic areas and away from the mountains, none in any of the West Indian islands with their fine mountain-ranges, none in the peninsula of India or in Brazil. And there is exactly the same distribution of fiords. We have them in Norway, in West Scotland, in Alaska, in South-West America, and in New Zealand, all characterised by deeper water within than at their outlets, and all in glaciated countries, but nowhere else in the world.

Now it is simply impossible to believe that at a very recent period there should have been earth-movements of such a character as to produce lakes, but always in glaciated districts and never beyond them, unless the movements were a result of the glaciation. This has not, I believe, been yet suggested; but, in view of the modern theory that any considerable loading of the surface produces subsidence, it is at least a possible explanation. But there are some important facts that seem more in favour of the grinding out of the lake-basins by the enormous weight of ice accumulated over their sites during the height of the ice-age. Looking at a geological map of the Alps it will be seen that most of the lakes are more or less bordered by tertiary or secondary rocks. Lakes Annecy and Bourget are in miocene and eocene; the lake of Geneva on the north side is miocene or jurassic; the lake of Neuchatel, miocene; lakes Thun and Brienz, eocene or jurassic; lake Lucerne, eocene and miocene; lakes Zug and Zurich in miocene; lake Constance miocene; lake Maggiore is mostly in gneiss, but it is very suggestive that it is here comparatively shallow, but becomes suddenly deeper and reaches its maximum depth in its lower portion where it is bordered on the east by the jurassic beds; lake Como also has its greatest depth in triassic rocks, the upper portion, where gneiss prevails, deepening gradually southward as in a submerged valley. Equally suggestive is the fact that in the eastern Alps of Tyrol and Carinthia, where gneiss, porphyry, and the older stratified rocks prevail, and where glaciers are not now so extensive, there are hardly any lakes, except on the northern borders, where a considerable number occur in eocene, cretaceous, jurassic, or triassic formations.

These various facts as to the distribution of alpine lakes – their almost total absence in all parts of the world outside of glaciated districts, and within glaciated districts their prevalence in the newer and more easily denuded rocks – are what have to be explained by the advocates of the theory of earth-movements, and this, so far as I am aware, they have never attempted to do. Equally important, and equally difficult to explain on the earth-movement theory, is the fact that alpine lakes are almost always situated just at those spots where, by means of converging valleys, the glaciers would become heaped up and attain their maximum thickness, or where there is good evidence that they have been very thick; and it is the grinding power of this enormous weight of ice, acting differentially as regards the softer and harder rocks, that has worn out hollows in pre-existing valleys now occupied by lakes. In almost every case, too, it will be seen that there is a constriction or narrowing of the valley towards or beyond the lower end of the lake, which, by preventing the free escape of the ice, has increased its thickness and grinding power.

In the presence of such important series of facts as those here referred to, mere opinions, or even small and detailed cases of difficulty, can have no weight; but there is yet another consideration, which most geologists will admit is antagonistic to the earth-movement theory. The whole tendency of geological observation is in favour of the usually very slow rate of earth-movements, while it is equally in favour of the comparatively rapid action of denudation by running water. But in order that earth-movement

could form a lake, it would be necessary that the rate of elevation or depression should be so great that the river could not keep pace with it by cutting down its channel; and, considering that all the rivers in question are rapid mountain streams carrying great quantities of sediment, this will be admitted to be a very improbable supposition. But when we add to this the still greater improbability that such rapid earth movements have occurred in scores and hundreds of cases, all at about the same time, geologically speaking, and all just in those spots where it can be shown that during the glacial period ice must have accumulated, and where the rocks were of such a character as to admit of being ground away; and yet further, that no similar earth movements producing similar results have recently occurred in any part of the globe beyond the limits of glaciation, the whole assumption becomes so hugely improbable as to render the theory of lake-formation by ice-grinding easy in comparison.

Sir Charles Lyell considered that the gravest objection to the glacial-erosion theory was the entire absence of lakes where they ought apparently to exist; and he instanced the valley of Aosta and the Dora Baltea, the glacier of which produced the enormous moraines of Ivrea.[a] The valley of the Rhone above Martigny may be adduced as another example of the absence of lakes where they might be expected. But this kind of difficulty will apply to many other valleys, and can only be answered by general considerations. In both these cases the valleys are comparatively broad and open, and have a rather rapid descent. It is probable, therefore, that the ancient glacier in both was of a nearly uniform thickness, so that its wearing action on the floor of the valley would be tolerably uniform. To produce a lake we require essentially a differential action. There must be much more rapid degradation in one part than in another, due either to greater ice-accumulation or to softer rocks in one part than in another. In both the valleys referred to there is much uniformity in the rock-formations throughout, and even if some lakes or chains of lakes had been formed, the enormous amount of debris still brought down may well have filled up and altogether obliterated them. The absence of lakes in certain valleys cannot be considered an argument of any value until it is ascertained by borings that none have been formed and filled up again. It must also be shown that the whole conditions are such as to produce that amount of differential grinding down, without which no lake can be expected to have been formed.

It certainly seems to me that all the facts, all the probabilities, all the converging lines of evidence, are in favour of the glacial theory, to which the only serious objection is the assumption that glaciers cannot move uphill. But that they can do so, and have done so, is now admitted by most students of glacier-motion. Mr. [Thomas Francis] Jamieson, and other Scotch geologists, have proved that glaciers, over 2000 feet thick, have travelled up lateral valleys, and up the slopes of many hills and mountains; and when we consider that the Rhone glacier was 5000 feet thick just above the lake of Geneva, and more than 2000 feet thick where it abutted against the Jura, we can have no difficulty in admitting that it might have travelled up the very gentle slope of the lake bottom, which appears to be less than 100 feet in a mile in its steepest parts.

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[a] These features are found in extreme northwestern Italy.

The Glacier Theory of Alpine Lakes (S472: 29 June 1893)

The Editor having given me the opportunity of reading Mr. Graham Officer's interesting letter, I will make a few remarks upon it.

It seems to me that, without further information as to the nature of the search for drift, erratics, or ice-worn surfaces, and judging from the statement that the plateau studded with lakes and tarns was only looked down upon from an adjacent mountain summit, we can hardly give much weight to the positive statements, "I am confident that evidences of glaciation do not exist," and – "as I have shown, the glacier theory will not account for by far the greater number of the Alpine lakes on the great central greenstone plateau." Some light may perhaps be thrown on the matter by the consideration that the undoubted marks of glaciation in many parts of Australia are believed to have been caused by, comparatively, very ancient glaciers, since some of the glaciated surfaces are overlain by pliocene deposits, while others are believed to be of palæozoic age. If the Tasmanian glaciation was also of pliocene age, most of the superficial indications may have been destroyed by denudation, or, if preserved, may be hidden by vegetation or by alluvial deposits. We must therefore wait for a much more thorough examination of the district and of other parts of Alpine Tasmania before it can be positively stated that no evidences of glaciation exist.

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The Recent Glaciation of Tasmania (S482: 2 November 1893)

In a paper read before the Royal Society of Tasmania in June last, Mr. R. M. Johnston, F. L. S.,[a] gives a sketch of what is known of the glaciation of the island, or rather of the western portion of it, for no indications of glaciers appear to have been discovered in the eastern half. This difference is supposed to be due to the fact that on the western side of the island the rainfall is from 50 to 76 inches annually, while in the central valley it is but little over 20 inches. Indications of glaciation among the western mountains were noticed by Mr. Charles Gould, Government geologist, about forty years ago, and from information received from him through the late Chief Secretary of Tasmania, the Hon. J. R. Scott, Mr. Johnston took up the inquiry, and for many years has made explorations in the western plateaus and mountains. Mr. C. P. Sprent was another explorer who published some account of the glacial phenomena in 1886, while more recently Mr. T. B. Moore and Mr. Dunn have recorded similar observations. Mr. A. Montgomery, the present Government geologist, has also just published a paper on the same subject.

Mr. Johnston tells us that he has personally explored the whole of the western mountains, from the Picton and Craycroft Rivers, southern branches of the Huon, in the extreme south, along the mountain ranges forming the western border of the central plateau, quite through to Emu Bay on the north coast; and that he has found the clearest evidences of glaciation in almost every valley throughout this great extent of country. From the Arthur Range in the south to Mount Bischoff in the north, are numerous moraines, *roches moutonnées*, tarns and lakes in great abundance, polished and striated rock-surfaces, and numbers of true erratics. Near the sources of the Franklin River, under Mount Hugel, and

[a] Robert Mackenzie Johnston (1843–1918), Scottish-Australian statistician and scientist.

only six or seven miles west of Lake St. Clair, are Lakes Dixon and Undine, of which Mr. Johnston writes:

> The valley of Lake Dixon is *par excellence*, the ideal of a perfect glacier valley. No one, however ignorant of glacial action, could in this neighbourhood gaze upon these beautiful scooped, or rather abraded lakes or tarns, the snow-white, polished, billowy, and cascade-like *roches moutonnées*, composed of quartzites, on the upper margin of Lake Dixon, together with the tumbled moraines and large erratics on the lower banks – at a level of about 2000 feet – without being impressed with the idea that its singularly characteristic features must have been produced by the slow rasping flow of an ancient river of ice.

Further north, the Murchison, Macintosh and Huskisson rivers, all branches of the Pieman River, contain similar glacial markings; and Mr. Dunn has recently described others of the same character about Lake Dora, nearer to the west coast. The latter observer lays special stress on the rounded planed and scored rocks, on hard quartzite and conglomerate rocks rounded and polished, on numerous tarns in rock-basins, on moraines covering hundreds of acres, and on numerous huge erratics and perched blocks. (See Annual Report of the Secretary for Mines, Victoria, 1893, p. 21.)

Mr. T. B. Moore states that he found the rocks polished and striated within 25 feet of the top of Mount Tyndall, or 3850 feet above the sea, a sufficient indication that the great central plateau at an average elevation of nearly 4000 feet must have been buried in ice or *névé* to a considerable depth, and have formed the feeding ground for the glaciers, whose effects are so visible in the adjacent western valleys. The Tasmanian geologists are united in the belief that the glaciers never reached the coast or descended much below the 2000 feet level, and that the ice did not extend to the central valley or the eastern side of the island. They therefore speak of it as a *glacier*, not a *glacial* period, the conditions being somewhat similar to those of the Alps at the present time; but, owing to the great difference in the rainfall, there was a more marked contrast between the western and eastern districts, while the lofty central plateau afforded a much more extensive snow-field than Switzerland now possesses.

The facts here stated on the authority of Mr. Johnston, supported by those of three other observers, two of them being the Government geologists, render more singular the statements of Messrs. Officer and Spencer (*Nature*, June 29, p. 198) as to their not finding any traces of glaciation in the country around Lake St. Clair, which they explored for a month. Lake Dixon, which Mr. Johnston describes as presenting all the evidences of glaciation in their fullest development, appears to be less than ten miles from the lower end of Lake St. Clair, according to the best map I can refer to; while Lake Petrarch, which Mr. Officer describes as seeing from the top of Mount Olympus, lies between the two in the Cuvier valley, and is also mentioned by Mr. Johnston as being within the highly-glaciated region. It is quite possible that the lakes on the great plateau may be due to damming up, owing to movements of the superficial gravels and clays by the ice or *névé* sheet; but there are evidently an abundance of small valley-lakes and tarns in the western valleys so surrounded by all the marks of extensive glaciation as to render it almost certain that they are true ice-eroded rock basins. It is much to be wished that a more detailed account of this interesting district, with a good map showing all the mountains, lakes, and valleys referred to, would be given us by one of the local geologists.

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Sir Henry H. Howorth on "Geology in Nubibus" (S484: 16 November 1893)

Having given my views on glacial geology in the current issue of the *Fortnightly Review*, to be followed by one dealing at some length with the ice-origin of lake-basins, I should not have thought any reply to Sir Henry Howorth's "Appeal" necessary except for the consideration that my articles may not be seen by many readers of *Nature*. And first, I would remark, that the mental attitude which Sir H. Howorth imputes to extreme glacialists I have myself been unable to detect in their writings. In fact, I was under the impression that the "scoffing" and "jeering" was chiefly from the other side; but it seems I was mistaken, and I must apologise for my ignorance. Those who read my articles will see that I make no appeal to "transcendental ice," but judge of its powers and properties by its admitted effects. Sir H. Howorth says that "ice is known to crush under moderate pressure," implying that a glacier a mile or perhaps half a mile thick is impossible. But will he or anyone else tell us what happens to the ice after it is crushed, and the pressure that crushed it is continued and slowly increased? Will it not suffer re-gelation and become denser ice; and if by sudden increase of pressure it is again crushed, will it not by still further pressure again suffer re-gelation? He stops at the first "crushing," as if that were the end of all things so far as a glacier is concerned. All this, however, is beside the question from my point of view. The work of ice on the rocks is as clear as that of palæolithic man on the flints; all the difficulties that may be suggested as to how he lived, or how he shaped the flints do not in the slightest degree affect our conclusion that the palæolithic flint implements are the work of *man*; and there is equally clear evidence that ice *did* march a hundred miles, mostly uphill, from the head of Lake Geneva to Soleure, whatever transcendental qualities it must have possessed to do so.

As to "perhaps the largest and most remarkable collection of rock-basins in the world" – the largest being of 50 acres and the deepest 30 feet deep – I must really decline to occupy your space in showing how simply these may have been produced by ordinary denuding agencies, or in denying that any glacialist, even of "the most extreme and aggressive school," would claim them as proofs of glaciation. As regards the question of Tasmanian glaciation, my last communication to *Nature* (Nov. 2) seems to me to render any further observations unnecessary. No doubt the conclusions of the various writers will be fully harmonised by a more complete study of the whole region.

The last point touched on by Sir H. Howorth – whether the advocates of the ice-origin of certain groups of lakes are "extravagant" in their views, following the methods of Aristotle rather than those of Bacon, and founding their beliefs on "purely hypothetical properties of matter and forces of nature" – I will leave to the judgment of those who do me the honour of reading my forthcoming article in the *Fortnightly Review*.

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"Geology in Nubibus" (S486: 30 November 1893)

Sir Henry Howorth wishes to continue the discussion of glaciation in the pages of *Nature*, but I find in his last letter very good reason why this cannot be done. No discussion can lead to definite results unless the parties to it accept as data what they themselves have recently and deliberately admitted. But when I stated that the Rhone glacier *did* reach the Jura, and deposit on it erratic blocks between Geneva and Soleure, I did so

because it was one of the data already admitted by Sir H. Howorth. In his "Glacial Nightmare," pp. 169–173, he gives a full summary of Charpentier's[a] first memoir on the erratic blocks of Switzerland, describing the glacial phenomena exhibited along the whole course of the old glaciers from the Alps to the Jura, and showing that they "even climbed that range and went over to the other side of it." Sir H. Howorth then says: "I have quoted at considerable length from this excellent memoir, because I look upon it as having definitely applied *inductive methods to this question* with results which are for the most part *sound and unanswerable*." (Italics mine.) In the same chapter (pp. 195–202) Charpentier's second memoir is summarised still more fully, and his general conclusion is thus quoted: "It goes without saying that not only all the valleys of the Valais were filled with ice up to a certain height, but that all lower Switzerland, in which we find the erratic débris of the Rhone valley, must have been covered by the same glacier. Consequently all the country between the Alps and the Jura, and between the environs of Geneva and those of Soleure has been the bed of a glacier." Agassiz and other writers are quoted as giving further evidence of the same kind. Nowhere in the whole of this chapter can I find a single objection to the conclusions of the chief writers quoted, and the concluding paragraph, at p. 208, frankly accepts them. It declares that they are supported by "every form of converging evidence," and that – "So far there is no question at issue." Yet, when I take these same conclusions of Charpentier as admitted data, Sir H. Howorth says: "This form of dogmatic argument is assuredly incomprehensible!" Charpentier's proof that the Rhone glacier reached Soleure, was, a year ago, "sound and unanswerable," and was an example of "definitely applied inductive methods"; but when I accept these same results as something to reason upon, I am told that I am making use of "hypotheses outside the laws of nature." I have now justified my opening statement that a discussion carried on in this manner can serve no useful purpose.

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The Origin of Lake Basins (S487: 28 December 1893)

In his last communication Sir Henry Howorth makes two statements which are so erroneous and so misleading that I cannot allow them to pass without correction. The first is, that Mr. Deeley[b] "repudiates Dr. Wallace's notion that regelation can in some way act as a compensating element when crushing supervenes in ice." Here is a double misstatement. Mr. Deeley "repudiated" no notion of mine, or he would, I am sure, have said so plainly, and he said nothing whatever about "crushing." Neither did I say a word about regelation acting as a "compensating element," for I do not believe in the crushing of glaciers by their own pressure. I asked Sir Henry what would happen to the ice after it was crushed, the pressure continuing; and I get no reply but the above double misstatement.

Then, further on, Sir Henry says: "Mr. Wallace confesses he does not like to face these mechanical issues." This is simply untrue. I "confessed" nothing of the kind, and I challenge Sir Henry Howorth to quote any words of mine which will bear such a meaning. I maintain that his "mechanical issues" are pure theories, and are beside the question of

[a] Johann von Charpentier (1786–1855), Swiss glaciologist.

[b] Richard Mountford Deeley (1855–1944), English railroad engineer.

the actual facts of glacier motion. Lastly, he attempts to evade the real issue between us, which is, that he himself accepted Charpentier's conclusions as to the extent of the Rhone glacier, but refuses to allow me to use these same conclusions as a datum in the discussion.

I have now shown ample reason why further discussion of this matter with Sir Henry Howorth must be unprofitable.

* * *

The Origin of Lake Basins (S489: 4 January 1894)

I welcome the criticism of my article on the glacial origin of a certain class of lakes by an experienced geologist like Mr. Oldham,[a] because it probably embodies the strongest argument that can be adduced on the other side – at all events as regards the one aspect of the problem which he alone touches upon. He urges that my paper contains a fallacy and a misrepresentation. The alleged fallacy is, that because the lakes in question are found in glaciated and not in otherwise similar non-glaciated regions, "therefore the rock-basins in which the lakes lie were excavated by glaciers." But this is not my argument, and therefore not my fallacy. What I say is – "there must be *some* causal connection between glaciation and these special types of lakes. What the connection is we shall enquire later on." That there is a "causal connection" Mr. Oldham asserts as strongly as I do myself, though it is a different, and as I have endeavoured to show, an untenable one.

This brings us to the alleged misrepresentation, which is, that I have imputed to the opponents of the ice-erosion theory, the view that the earth movements which, as they allege, produced the lakes, occurred in the period just before the ice-age came on. Mr. Oldham says, this is an unreasonable and unfounded limitation, since the movements in question probably occurred throughout the glacial period itself. I quite admit the validity of this criticism, and that I should have added, "or during the glacial period itself," to, "immediately before" it. I certainly had this probability in my mind, and the reason I did not express it was twofold. In the first place, all the advocates of the earth-movement theory appeared to assume, either directly or implicitly, the preglacial origin of the lakes; and secondly, this assumption gave them the strongest argument against my views, and I therefore gave them the benefit of it. Mr. Oldham appears to have overlooked this. Yet it is clear that the shorter you make the time since the formation of lake basins by earth-movements the more difficulty there is in explaining the total absence of valley-lakes from all the non-glaciated mountain regions of the world, since there is less time for them to have been all silted up. When arguing this point I said – in the passage evidently referred to by Mr. Oldham – "The only way to get over the difficulty is to suppose that earth-movements of this nature occurred only at that one period, just before the ice-age came on, and the lakes produced by them in all other regions have since been filled up." I thus gave my opponents the benefit of an extreme supposition which was all against myself; while the more reasonable view, that earth-movements are just as likely to have occurred during and since the glacial epoch as before it, renders my argument from the geographical distribution of lakes much stronger, since it is impossible to believe that, if lake basins as large and as deep as those of Geneva, Maggiore, Como, Constance, and

[a] Richard Dixon Oldham (1858–1936), English geophysicist.

Garda, were formed in non-glaciated regions as recently as the middle or latter part of the glacial epoch, a considerable number of them would not be still in existence.

Of course, if it can be shown that filled up lake-basins exist in tropical and subtropical regions, corresponding in number, position, size, and depth, with those of glaciated areas, the argument from geographical distribution will break down. At present I am not aware of any evidence that such is the case. But even if it were so, there remains the singular correlation between the size and depth of lake basins and the known size of the glaciers that occupied these valleys; together with the surface and bottom contours of the lakes themselves, so strongly opposed to their production by any form of valley-subsidence or earth-movements.

A friend has pointed out an unsound argument in my article on the above subject in the *Fortnightly Review*, and I therefore ask to be allowed to state what it is, and thus avoid its being possibly made the subject of discussion in the pages of *Nature*. As a proof of the very great erosive power of ice I have adduced Dr. Helland's estimate of the quantity of Scandinavian *débris* in Northern Europe. But it is evident that this only proves the great carrying power of the ice, since the rock and gravel would be mostly of sub-aerial origin. It, however, indicates a very long period during which the ice-sheet was at work, while the clayey element in it would be due to erosion. The larger part of this, however, would certainly have been carried away into the North Sea during the passage of the ice-sheet across the Baltic. The enormous quantity of boulder-clay in North America, which I have also referred to, is a better indication of true ice-erosion.

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Note on Mr. Jukes-Browne's Paper (S465)

Remarks on the Darwin–Wallace classification of islands, printed in the March 1893 issue of Natural Science.

The editor having kindly sent me a proof of Mr. Jukes-Browne's paper, I beg to make a few remarks thereon.

I cannot but think that Mr. Jukes-Browne's criticism of the Darwinian classification of islands, which I have adopted and more fully developed, is rather one of words and definitions than of realities. The very terms of the classification – "Oceanic" and "Continental" – show that it is a broad and wide-reaching one; and its main implication, the permanence of oceanic and continental areas, is equally broad and fundamental. That there should be islands situated upon the ever-fluctuating margin of these two areas which are difficult to class, or which may, at different geological periods, have possessed the characteristics of "oceanic" or of "continental" islands, is what might certainly be expected; the wonder is that there are so very few of them. Barbados is, technically, an oceanic island; but it is situated upon the old sea-margin of the American continent, and a portion of that old continental margin forms the base of its oceanic deposits. I recognised the possibility of such a base for some apparently oceanic islands in the passage quoted by Mr. Jukes-Browne, but was not then aware that any such existed. Of course, if old stratified rocks could be shown to form the base of any of the mid-oceanic islands, the whole classification, and the theory which is founded on it, might be imperilled; but this has not

yet been done.

It is evident that, with island groups whose components vary in size from many thousands of square miles to small sea-washed rocks, all definitions must be taken broadly and as applying to the group. Even among the pre-eminently continental British Isles there are many hundreds – out of the thousand of which they are said to consist – which have neither mammalian, amphibian, nor reptilian inhabitants; but it will hardly be objected that such cases as these upset the biological definition of continental islands. In the same manner the Seychelles are classed as belonging to the Madagascar group, and are, therefore, ancient continental, while Mauritius, Bourbon, and Rodriguez are true oceanic islands.

I do not know why Mr. Jukes-Browne should say that I regard New Caledonia as an oceanic island. At p. 473 of *Island Life* I refer to it as probably once connected with New Zealand; and again, at p. 485, I suppose it to have once formed an extension of New Zealand, which, though in some respects anomalous, has all the main characteristics of a continental island.

Looking at the question broadly, as a generalisation applying to all the well-marked islands and island-groups of the globe, I entirely deny the validity of the conclusions expressed in the last three paragraphs of Mr. Jukes-Browne's paper, conclusions which are founded exclusively on islands situated upon the margin of the continental area.

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The Supposed Glaciation of Brazil (S480)

A letter printed in the 19 October 1893 issue of the journal Nature.

In the second volume of *Nature*, p. 510, I reviewed the late Prof. Hartt's "Geology and Physical Geography of Brazil," and called attention to the author's views, as well as those of the late Prof. Agassiz, relating to the supposed glaciation of that country. From their very positive statements I concluded that the evidence as described by them did actually exist, and that until it was disproved it should not be ignored. In my "Darwinism," p. 370, I stated, on the authority of my friend, Mr. J. C. Branner, now Professor of Geology in the Stanford University, California, who succeeded Prof. Hartt in Brazil, and had a much more extensive knowledge of the country, that the supposed glacial drift and erratic blocks were all results of subaërial denudation. Recently, however, Sir Henry Howorth has quoted some passages from my review in illustration of the wild and incredible theories of some geologists, as samples, in fact, of the "Glacial Nightmare"; and, as no authoritative disproof has yet been given of the exceedingly strong and positive statements of Agassiz and Hartt, I beg leave to lay before the readers of *Nature* some extracts from a paper on "The Supposed Glaciation of Brazil," by Prof. Branner, which will shortly be published, and of which he has kindly sent me a type-written copy in advance. As a partial justification of what has now proved my too hasty acceptance of the statements of these gentlemen, I will give one passage in which Prof. Agassiz refers to the supposed glacial phenomena near Ceara:

> I may say that in the whole valley of Hasli there are no accumulations of morainic materials more characteristic than those I have found here, not even about the Kirchel; neither

> are there any remains of the kind more striking about the valleys of Mount Desert in Maine, where the glacial phenomena are so remarkable; nor in the valleys of Loch Fine, Loch Awe, and Loch Long, in Scotland, where the traces of ancient glaciers are so distinct.

Both Agassiz and Hartt were equally strong as to similar phenomena near Rio.

It is to be first noted that Hartt had only spent eighteen months in Brazil when he wrote his book, and his views on the glacial phenomena were thus based on a very hasty survey of that enormous territory. Prof. Branner went with him when he again visited Brazil in 1874, helped him in his geological work till his death in 1877, and himself remained five years longer making a geological survey of the country; and he states that, before his death, Hartt's views underwent a radical change. Prof. Branner says:

> Under his direction I did more or less work in the mountains about Rio de Janeiro for the purpose of sifting the evidence of glaciation in that region, and I am glad to say, in justice to the memory and scientific spirit of my former chief and friend, that long before his death he had entirely abandoned the theory of the glaciation of Brazil, and that the subject had ceased to receive further attention, even as a working hypothesis.

A few extracts must now be given showing to what causes the phenomena which deceived these observers are really due. And first as to what were supposed to be erratic boulders often embedded in boulder clay.

> The boulders believed to be erratics are not erratics in the sense implied, though they are not always in place. The first and most common are boulders of decomposition, either rounded or subangular, left by the decay of granite or gneiss. Sometimes they are embedded in residuary, and therefore unstratified, clays, formed by the decomposition in place of the surrounding rock. And everyone has heard of the great depth to which rocks are decomposed in Brazil. The true origin of these boulders and their accompanying clays is often obscured by the 'creep' of the materials, or in hilly districts by land slides, great or small, that throw the whole mass into a confusion closely resembling that so common in the true glacier boulder clays. In this connection too much stress cannot be placed upon the matter of land slides; they are very common in the hilly portions of Brazil, and aside from profound striations and facetting produce phenomena that, on a small scale, resemble glacial till in a very striking degree. . . .
>
> The second method by which these boulders have been formed is quite similar to the first, but instead of being cores of granite or gneiss, they have been derived by the same process of exfoliation and decomposition from the angular blocks into which the dikes of diorite, diabase, or other dark-coloured rocks break up. Their colour marks them as quite different from the surrounding granites, and the dikes themselves are almost invariably concealed. The residuary clays derived from the decomposition of these dikes are somewhat different in colour from those yielded by the granites, so that when 'creep' or landslides add their confusion to the original relations of the rocks the resemblance to true glacial boulder clays is pretty strong. The chance of discovering the source of such boulders is further decreased by the depth to which the mass of the rock has decayed, and by the inpenetrable jungles that cover the whole country, and so effectually limit the range of one's observations. Dikes, such as these last mentioned, are not uncommon in the mountains about Rio de Janeiro. Indeed, what have generally been regarded as the very best evidences of Brazilian glaciation, some of the boulders near the English hotel at Tijca, fall under this head, though some are of gneiss. The fact is that the great mountain masses about Rio are of granite or gneiss, while some of the boulders come from the dikes of diabase or other dark-coloured rock high on their sides – dikes which were not visited by Agassiz or Hartt.

Prof. Branner then describes a third class of supposed erratic, derived from certain sandstone beds of the tertiary deposits, which, by exposure, change to the hardest kind of quartzite, and when the surrounding strata are removed by denudation, and a few blocks of this quartzite are left, they are so unlike the rocks by which they are surrounded that unless the observer has given a special study to the tertiary sediments, he is liable to be misled by them.

The wide-spread coating of drift-like materials that covers considerable areas of the country, consisting of boulders, cobbles, and gravels, sometimes assorted and sometimes having clay and sand mixed with them, are then described, and are shown to be due to the denudation of the tertiary beds during the last emergence of the land, aided by subsequent subaërial denudation and surface wash. Prof. Branner thus concludes:

> I may sum up my own views with the statement that I did not see, during eight years of travel and geological observations that extended from the Amazon valley and the coast through the highlands of Brazil and to the head waters of the Paraguay and the Tapagos, a single phenomenon in the way of boulders, gravels, clays, soils, surfaces, or topography, that required to be referred to glaciation.

The very clear statement above given of the real nature of the phenomena which deceived Prof. Agassiz and Mr. Hartt, is very instructive, and it shows us that a superficial resemblance to drift, boulder-clay, and erratic blocks, in a comparatively unknown country, must not be held to be proofs of glaciation. We require either striated rock surfaces or boulders, or undoubted *roches moutonnées*, or erratics, which can be proved not to exist sufficiently near to have been brought by "creep" or land-slides. In view of these liabilities to error, we may be almost sure that the supposed evidences of glaciation described by the late Mr. Belt in his "Naturalist in Nicaragua" (p. 260), are explicable in the same manner as the Brazilian evidences, since be nowhere found glacial striæ or any boulders that could be proved to be true erratics; and this is the more certain because he himself states (p. 265), "I have myself seen, near Pernambuco, and in the province of Maranham, in Brazil, a great drift deposit that I believe to be of glacial origin."

All students of the past and present history of the earth are indebted to Prof. Branner for having relieved them of a great difficulty – a true glacial nightmare – that of having to explain the recent occurrence of glaciation on a large scale far within the tropics and on surfaces not much elevated above the sea-level.

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Section 6. Biogeography

Introduction

Considering Wallace's reputation as the "father" of modern zoogeography studies (and of evolutionary biogeography in general), the relative dearth of his public correspondence on related matters is a bit surprising. Perhaps he was more inclined to consider its questions *in extenso*, in his articles and great books on the subject, or perhaps this was an area where few dared to challenge his authority.

The communications that follow are on a scattering of issues, though several in one sense another concern the faunal regions classification scheme he and Philip Sclater developed. It held the stage for more than a hundred years, withstanding challenges throughout that period. Even today, with slight adjustments, it remains a useful structure, if largely for descriptive purposes only.

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Bone-Caves in Borneo (S97)

In this letter, printed in The Reader issue of 19 March 1864 and the Natural History Review issue of April 1864, Wallace alerted naturalists to an opportunity for research.

I ask permission to lay before your readers a few facts and suggestions on the above subject. Some weeks since I was informed by an old acquaintance, Mr. Robert Coulson, a mining engineer who has explored a good deal of North-Western Borneo, that he had found a quantity of bones in a cave in that country; and, having read, during his stay in London, Sir Charles Lyell's "Antiquity of Man," he thought the fact might be of some interest. On inquiring particulars, I found that the cave in question was situated in the district between Sarawak and Bruni, on a mountain some distance inland. Mr. Coulson had been searching for tin and other ores, which were reported to exist in these caves, and in this particular one he found the floor covered with a kind of fossil guano, very hard, and about two feet thick. He had some of this broken up with picks, and found it to contain abundance of bones, especially at the bottom, next the rocky floor of the cave. He assured me there were great numbers of bones of many sorts, and numbers of teeth of all sizes. The guano was so hard that they did not break up much of it. There were also some human skulls lying on the surface, about which the natives who accompanied him could tell him nothing.

The presence of this layer of solid guano in a cave is not easy to account for. If it is the accumulated dung of the small bats, and perhaps a few swifts or goatsuckers that may now frequent the caves, it would indicate a long period of time. There are now no animals

in Borneo that would be likely to frequent caves, the only moderately large carnivora, the Malay bear and the tiger-cats (*Felis macrocelis* and *F. javensis*), being arboreal animals. The mere fact, therefore, of large quantities of bones found in a cave with the accumulated dung in which they are buried, indicates a state of things which has now passed away; and the examination of those bones might throw light upon the changes which have resulted in the peculiar zoological character which the productions of the island present.

It may, perhaps, be advisable, in connexion with this subject, briefly to point out the chief characteristics of the fauna of Borneo, the anomalies which it presents, and which a knowledge of its most recent changes may assist us in explaining; as well as the promise it holds out of richly rewarding the researches of palæontologists. The natural productions of Borneo resemble on the whole so closely those of the other Indo-Malayan countries (Java, Sumatra, and the Malay Peninsula), that there can be little doubt of there having been a geologically recent connexion between them all. Of about seventy mammals known to inhabit Borneo, only ten are peculiar to it, the remaining sixty occurring, with but slight differential characters or none, in one or other of the adjacent islands. In birds and insects about the same proportion are really distinct, though there are many more which offer slight but constant peculiarities, and have, therefore, received distinct specific names. Notwithstanding, however, this great and very general similarity, there are in Borneo certain peculiarities and certain deficiencies which give it a marked character. Several genera are peculiar to it, as *Nasalis, Dendrogale* and *Ptilocercus*, and that singular bird *Pityriasis*. The tiger, which abounds on all the other islands and in the peninsula of Malacca, is absent, and yet several of the large herbivora, which, being free from the attacks of such a ferocious beast, one would expect to find in greater abundance, are very local and scarce, and apparently dying out. The elephant and rhinoceros, which in Sumatra and the Malay Peninsula exist in company with the tiger, are so local and scarce in Borneo that their very existence has been for some time doubtful, and even the tapir is by no means so plentiful as in the places mentioned above. The wild ox, also (*Bos sondaicus*), only exists in the north-east extremity of the island.

The most striking feature of Borneo is, however, undoubtedly the presence and comparative abundance of the great anthropoid ape, *Simia satyrus*, as well as a second species, *Simia morio*, Owen. One or both of these is met with over the whole extent of Borneo, whereas, though there can be no doubt that the former occurs also in Sumatra, it seems confined to a limited district, and in the whole southern half of the island is entirely unknown. Another important consideration is, that the Malayan region (of which Borneo forms an important section), though of limited extent, has a highly characteristic and peculiar fauna. It possesses many genera entirely restricted to it, and many families which attain their greatest development in it, and also presents us with some of the most singular and interesting forms in the animal kingdom.

From the foregoing facts the following conclusions are, I think, rendered very probable: – (1st) A great geological antiquity for the Malayan region as a whole; (2nd) a considerable antiquity for that portion of it which now forms the island of Borneo; and (3rd) great and varied changes in physical geography, and great concomitant changes in animal life, which have resulted in the present condition of that island's fauna; – and we may, I think, be certain, that the remains of the animals which inhabited Borneo at a comparatively recent period will be of great interest, and may serve to indicate the nature of the changes that have been recently, and are probably still, going on.

Quite independently, however, of the fact that bones are known to exist in a particular

cave in Borneo, it appears to me that that island offers a field for exploration unequalled perhaps in the globe. Limestone caverns abound in it. There are several very extensive ones in the Sarawak territory itself, and Mr. St. John[a] mentions others in the Bruni country. The character of the existing fauna, as well as the extent of the land and the height of the mountains, all prove it to be of some geological antiquity. Now, in every other country which has been explored, the animals which have recently become extinct are always allied to those now living in the same region, and are often of gigantic size or remarkable forms. Europe gives us elks, bears, and hyenas, Australia extinct kangaroos and wombats, South America giant sloths and armadillos; according to all analogy, therefore, we may expect that the caves of Borneo would reward a persevering explorer, not only with fossil tapirs, Malay bears, and scaly ant-eaters, but also with the precursors of the extraordinary lemuroid forms now inhabiting the country – *Galeopithecus*, *Nycticebus*, and *Tarsius* – and with fossil proboscis-monkeys, gibbons, and orangs, more or less resembling those which now abound in its vast and luxuriant forests. It is not improbable that some human remains may also be found to throw light upon the question of the origin of the Malayan races, and to prove whether a Negrito or some still lower race was formerly spread over the whole archipelago.

Should the naturalists of this country be willing to make an effort to carry out this most promising work, I can inform them that Mr. Coulson, who is now on his way to Singapore, is willing to undertake it, if he receives instructions within the next month or two, after which time he will probably have other engagements. The necessary expenses of going to such a remote part of the country would be rather heavy, as he must engage a native boat and crew, as well as labourers, at Sarawak; but I estimate that £150 would cover expenses and his remuneration for getting a good sample of the contents of the cave he described to me; and, if an additional £100 could be raised, he would be able, while in the country, to explore several other caves and ascertain whether any of them contain remains of greater antiquity and higher interest. It is needless to observe that to carry out a thorough examination of all the caves or recent deposits in the country (which would be necessary to reap the full benefits of any exploration) would require a much larger sum than I have mentioned. – Alfred R. Wallace, 5 *Westbourne Grove Terrace, W.*

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Mr. Wallace and His Reviewers (S262)

The publication of Wallace's Geographical Distribution of Animals in 1876 was a landmark in the development of biogeography as a discipline. But there were some complaints, as this letter printed in the 9 November 1876 issue of Nature shows.

I did not intend to take any public notice of reviews or criticisms of my book on "Geographical Distribution"; Mr. Gill's[b] letter, however, calls for a few remarks. I have first to thank him for pointing out the errors of a previous critic, and also for a list of *errata* in

[a] Spenser St. John (1825–1910), Secretary to Sir James Brooke.
[b] Theodore Gill (1837–1914), American ichthyologist.

the account of North American fresh-water fishes. He very truly remarks, that had I been acquainted with ichthyology and its literature these errors might have been avoided; but he has overlooked the fact that I have twice stated (vol. i, p. 101, and vol. ii, p. 168) that the part of my work relating to fishes is, practically, a summary of Dr. Günther's[a] Catalogue. The labour of going through such an extensive work for the purpose of extracting and tabulating summaries of the geographical materials it contains, was very great, and no doubt I have made some errors. Most of those indicated by Mr. Gill depend, however, either on differences of classification and nomenclature, or on additions to North American ichthyology since the date of Dr. Günther's work, and are therefore due to the plan of this part of my book, and not to oversight. Although possessing a tolerable acquaintance with the literature of ornithology, I had found the task of collating and combining the latest information into a uniform system of classification and nomenclature to be one which severely taxed whatever knowledge and literary ability I possessed. To have attempted to do the same thing in a class of animals which I had never studied would, I felt sure, have resulted in great confusion, and have been far less satisfactory and reliable than the course I have adopted. Had I been able to find any work giving a general account of the fishes of temperate North America, I should gladly have availed myself of it, but I do not gather from Mr. Gill's letter that any such work exists; and notwithstanding the great imperfection of the results (in the eyes of a specialist) as regards the fishes of the United States, I still think I exercised a wise discretion in confining myself to the vast mass of materials, classified on a uniform system, which Dr. Günther's Catalogue affords.

I may here add, that the "24 peculiar genera" mentioned by me are in addition to the "5 peculiar family types" – making together the "29 peculiar genera" referred to in the succeeding paragraph – so that the contradiction alluded to by Mr. Gill is only apparent. – Alfred R. Wallace, Dorking.

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The Zoological Relations of Madagascar and Africa (S275)

A letter printed in the 25 October 1877 issue of Nature.

Without entering into the details of this very difficult question I wish to be allowed to state some of the general reasons which have led me to a different conclusion from Dr. [Gustav] Hartlaub, and also to point out where he has not quoted my opinions with perfect accuracy.[1] Instead of saying that "the fauna of Madagascar is manifestly of African origin," my actual statement is as follows: – "We have the extraordinary fauna of Madagascar to account for, with its evident main derivation from Africa, yet wanting all the larger and higher African forms; its resemblances to Malaya and to South America; and its wonderful assemblage of altogether peculiar types" ("Geog. Dist. of Animals," vol. i. p. 286). My reasons for believing in the "*main derivation*" of the fauna from Africa can only be understood by considering the theory, now generally admitted, of the origin of the fauna of Africa itself. All the higher mammalia are believed to have entered it from the

[a] Albert Günther (1830–1914), German-British herpetologist and ichthyologist.
[1] *Nature*, vol. xvi. p. 498, and the *Ibis* for July, 1847 [*sic* 1877], p. 334.

northern continent during the middle or latter part of the tertiary period, and the occurrence of *Psittacus* and of forms supposed to be allied to plantain-eaters and to *Leptosomus* in the miocene of France, render it probable that many of the peculiar groups of African birds had their origin in the old Palæarctic region. Now Madagascar presents many cases of special affinity with South Africa, especially in insects, land-shells, and plants; and if we suppose it to have formed part of a South African land before the irruption of the higher mammals and birds from the north, we shall I think account for many of its peculiarities. Such facts as its possessing *Potamochærus* and the recently extinct *Hippopotamus*, while it has thirteen or fourteen peculiarly African genera of birds against four or five that are peculiarly Oriental; of its having many African genera of lizards and tortoises; of its butterflies being decidedly African; of its numerous African genera of Carabidæ, Lucanidæ, and Lamiidæ; while the specially Oriental affinities of its mammals, reptiles, and insects are hardly if at all more pronounced than the South American affinities of the same groups, – all seem to me to warrant the general conclusion that the "main derivation" of the Madagascar fauna is from Africa.

Dr. Hartlaub speaks of my "attempted parallel between Madagascar and Africa, and the Antilles and South America" in such a way that his readers must think I had dwelt upon this parallel in some detail as being special and peculiar. The fact is, however, that I have always referred to it in a very general way. At p. 75 vol. i. I say: "The peculiarities it (the Malagasy sub-region) exhibits, being of exactly the same kind as those presented by the Antilles, by New Zealand, and even by Celebes and Ceylon, but in a much greater degree." And again, at p. 272, vol. i., I speak of it as "bearing a similar relation to Africa as the Antilles to Tropical America, or New Zealand to Australia, but possessing a much richer fauna than either of these, and in some respects a more remarkable one even than New Zealand." This *general* comparison with the *two* other great insular sub-regions is, I think, justifiable, notwithstanding great differences of detail. There is in all a rich and highly peculiar fauna, a great poverty of mammalia, and a total absence of many large families of birds characterising the adjacent continent, together with special points of resemblance to distant continents or to remote geological periods.

It seems to me that such a problem as this cannot well be solved by means of a group which, like birds, do not require an actual land-connection in order to reach a given country; and, if *all* land animals are taken into account, the evidence does not appear to warrant the supposition of a recent land-connection of Madagascar with India or Malaya. At a very remote epoch such a connection *may* have taken place, but if we are to give any weight to the general facts of distribution as opposed to those presented by birds only, the union of Madagascar with South Africa is more recent and has had more influence on the character of the Malagasy fauna. The numerous and very remarkable points of affinity between Madagascar and South America in almost every group except birds, are not alluded to by Dr. Hartlaub, yet they would equally well support the notion of a former union of those two countries independently of Africa. It seems, however, more consonant with our general knowledge of distribution to consider these as cases of survival of ancient and once wide-spread types in suitable areas; and this is a principle that must never be lost sight of in attempting to solve the problems presented by such anomalous countries as Madagascar.

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The Comparative Richness of Faunas and Floras Tested Numerically (S280)

In an early consideration of the characteristics of biodiversity, printed in the 6 December 1877 issue of Nature, Wallace discusses some methods for comparing the faunas of different-sized areas.

In his letter in *Nature*, vol. xvii. p. 9, Prof. [Alfred] Newton has strongly brought out the absurdity of comparing districts of *very different areas* by the proportionate number of species to area in each. On this principle he shows that to be equally rich with the small island of Rodriguez, Madagascar ought to possess four times as many species of birds as exist throughout the whole world. It does not, however, by any means follow that the method thus exposed may not be of value in comparing regions of approximately equal area, as is the case with several of the primary regions, to determine the comparative richness of which Mr. [Philip Lutley] Sclater first applied it. I have not Mr. Sclater's paper at hand, but it is my impression that he made no attempt to show – "that the proper mode of comparing the wealth or poverty of one fauna with another was to state the proportion which the number of species composing it bears to the area over which they range" – as Prof. Newton implies that he did, but that he merely adopted this method as the only one readily available for the comparison of his regions. Although I took the opportunity of making some corrections in the figures, I never committed myself to the principle; and I very soon afterwards found that it was not to be trusted. As, however, several later writers have made use of it without remark, it will be interesting to consider where the exact point of the fallacy lies, and with what modifications the method can be trusted to give useful and consistent results.

If we compare two islands of almost exactly equal areas, such as Ceylon and Tasmania, and find that the one has twice or three times as many species of mammals or birds as the other, it will be generally admitted that we express the fact correctly when we say that, as regards such a group of animals, the one is twice or thrice as rich as the other; and the same may be said of two countries or two continents of identical areas. For on the supposition that there is a general correspondence between the numbers of rare and common, of local and of wide-spread species in the two areas compared (and this seems probable), then the average number of distinct species to be met with on one spot, or to be seen during a journey of equal length, will be proportionate to the total number of species in the two areas. But now let us divide one of the two continents or islands which we are comparing into two or more parts. We know, as a matter of fact, that one-half the area will always contain much more than half the total number of species, while one-tenth of the area will contain immensely more than one-tenth of the species. To take an example: the county of Sussex is about one-eightieth part the area of the British Isles, yet it actually contains full two-thirds of the total number of flowering plants, both being estimated by the same flora (Babington's "Manual," fifth edition, British Isles 1,536 species, Sussex 1,059 species). If we now compare either Britain or Sussex with an *equal area* on the continent of Europe or North America, we may obtain an instructive estimate of the comparative richness of their respective floras; but if we compare *unequal areas*, and then endeavour to equalise them by getting the proportions of species to area, we shall obtain erroneous results, which will become literally absurd when the areas compared are very

unequal.

The problem remains, how to compare unequal areas of which we possess the zoological or botanical statistics. We can only do so by equalising them, and this may not be so difficult as at first sight appears. For example, let us take the Palæarctic and North American regions, in which the species of birds are nearly equal in number, but the areas are as about seven to three. The number of the Palæarctic species have, however, been proportionately increased of late years, and if we take the western half of the Palæarctic region so as to include North Africa and Persia we shall have an area about equal to the Nearctic region, and a number of species perhaps one-sixth or one-eighth less, which will thus represent the comparative richness of these two areas. The eastern half of the region, including Japan and North China, is probably as rich as the western; while the intermediate portion is poorer in species. Combining these three portions, and taking the average, we should perhaps find the Palæarctic region about four-fifths or five-sixths as rich as the Nearctic, instead of less than one-half, as shown by the method of proportionate areas.

Whenever we know how many *peculiar* species any district contains, we can deduct its area from the total area of the region to be compared, and this number of peculiar species, from the fauna of the region; and by this means we may reduce two unequal regions to comparative equality. Again, all detached portions or islands should be omitted in estimating the comparative richness of regions, because they affect these regions very unequally. By adding Britain to Europe you increase the area without adding to the fauna, and thus make the region seem poorer; while by adding Madagascar to Africa, or New Zealand to Australia, you add to the fauna in a greater proportion than you increase the area, and thus make the region seem richer. For a fair comparison continents should be compared with continents, and islands with islands, and these should in every case be brought to an approximate equality of area by lopping off outlying portions with their peculiar species. We shall then get results which will be instructive, and which will afford us a true estimate of the comparative richness of different countries in the several classes of animals and plants.

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On the Value of the "Neoarctic" as One of the Primary Zoological Regions (S360)

Through the remainder of the nineteenth century various authors quibbled over the particulars of Wallace's faunal regions classification. In this letter, printed in the 22 March 1883 issue of the journal Nature, Wallace turned back one objection.

In the *Proceedings of the Academy of Natural Sciences of Philadelphia* (December, 1882) Prof. Angelo Heilprin has an article under the above title, in which he seeks to show that the Neoarctic and Palæarctic should form one region, for which he proposes the somewhat awkward name "Triarctic Region," or the region of the three northern continents. The reasons for this proposal are, that in the chief vertebrate classes the proportion of peculiar forms is less in both the Nearctic and Palæarctic than in any of the other regions; while, if these two regions are combined, they will, together, have an amount of

peculiarity greater than some of the tropical regions.

This may be quite true without leading to the conclusion argued for. The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations – peculiarity due to the absence of widespread groups, and geographical individuality. The absence of the families of hedgehogs, swine, and dormice, and of the genera *Meles*, *Equus*, *Bos*, *Gazella*, *Mus*, *Cricetus*, *Meriones*, *Dipus*, and *Hystrix*, among mammals; and of the important families of flycatchers and starlings, the extreme rarity of larks, the scarcity of warblers, and the absence of such widespread genera as *Acrocephalus*, *Hypolais*, *Ruticilla*, *Saxicola*, *Accentor*, *Garrulus*, *Fringilla*, *Emberiza*, *Motacilla*, *Yunx*, *Cuculus*, *Caprimulgus*, *Perdix*, *Coturnix*, and all the true pheasants, among birds, many of which are groups which may almost be said to characterise the Old World as compared with the New, must surely be allowed to have great weight in determining this question.

The geographical individuality of the two regions is of no less importance, and if we once quit these well-marked and most natural primary divisions we shall, I believe, open up questions as regards the remaining regions which it will not be easy to set at rest. There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions. But even after these two are united, there will remain discrepancies of almost equal amount among the rest, since in some groups the Neotropical, in others the Australian, far exceed all other regions in their speciality. The temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate, and have thus not been able to preserve so many ancient and specialised forms as the more uniformly warm areas. But, taking this fact into account, it seems to me that the individuality of the Nearctic and Palæarctic regions is very well marked, and much greater than could have been anticipated; and I do not think that naturalists in general will be induced to give them up by any such arguments as are here brought forward.

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Hardy Australian Plants (S452a)

In a letter that appeared in the 20 August 1892 issue of The Garden, Wallace discussed the potential of Australia as a source of plants for English gardens.

Every gardener knows that very few Australian plants, whether shrubby or herbaceous, are hardy in this country, except in such favoured districts as the southern coasts of Cornwall and Devon, Guernsey, or the Scilly Islands. But I am inclined to believe that this want of hardiness depends on the circumstances under which the greater number of Australian plants reached this country, and that there would be now no difficulty in obtaining representatives of all the more characteristic and interesting groups of these plants which would be perfectly hardy over a large part of the south and west of England, Scotland and Ireland.

If we look for the chief genera of Australian plants in Johnson's "Gardeners' Dictionary" or Loudon's "Encyclopædia of Plants," both of which give the date of introduction of

each species, we shall find that almost all the Acacias, Correas, Bossiæas, Kennedyas, Epacrises, Banksias, Grevilleas, Brachycomas, Pimeleas and scores of other Australian plants which once decorated our greenhouses were introduced in the early part of this century, a large number before 1820 and almost all by 1840 or 1850; but at that time the only settled country was in the coast districts. Victoria only began to be settled in 1835, and Mount Kosciusko with the alpine region around it was only discovered in 1840. Hence it happened that all the Australian plants brought to England for cultivation were obtained in the low-lying and warm districts near the coast, and were therefore all tender greenhouse plants, the attempt to grow which in the open air is hardly ever successful. Then, at a later period, when Orchids and bedding-out came into fashion, there was less demand for Australian plants, and many of the species once so valued went out of cultivation, and probably no attempt has ever been made to obtain plants from the cold uplands for cultivation in this country. The present time, however, seems very favourable for making the attempt. There is now an enormous demand in this country for new and beautiful forms of hardy plants for outdoor culture, while the extension of railways to all the mining districts of Australia, and the settlement of many of the higher plateaux for dairy farming, render it comparatively easy to obtain seeds and plants from suitable localities. In the south-eastern corner of Australia there is a tract of mountainous country covering about 15,000 square miles, all at an elevation of more than 3000 feet above the sea level. In a central position is the mining town of Kiandra, 4640 feet above the sea, and surrounded by mountains from 5000 feet to 7000 feet elevation. At Kiandra snow often lies on the ground for weeks, and the thermometer not unfrequently falls below zero (Fahrenheit), so that there can be little doubt that the plants growing in the country around it, and at still greater elevations, would be quite hardy. In Tasmania nearly one-third of the whole area is above 3000 feet elevation, while there are extensive plains and mountain slopes up to 4000 feet where the climate is still more severe than at Kiandra.

Now the peculiar vegetation of Australia is sure to be represented at these altitudes by alpine and sub-alpine species or varieties, and our gardens might thus be adorned by species of the numerous genera which have so long been favourites in our greenhouses, as well as by many others which are altogether peculiar to the sub-alpine and alpine regions. Collectors might no doubt be obtained in Australia itself, and these lines are written in the hope of inducing either one of our nurserymen or some wealthy amateur to obtain their services for the purpose of stocking English gardens with hardy Australian plants. – Alfred R. Wallace.

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Mr. H. O. Forbes's Discoveries in the Chatham Islands (S469)

Part of a discussion printed in the 11 May 1893 issue of Nature.

In a paper read before the Royal Geographical Society on March 12th, and again in an article on "The Chatham Islands and their Story" in the *Fortnightly Review* of this month, Mr. H. O. Forbes has described his very interesting discoveries in these islands, and has founded thereon certain conclusions as to the past history of the New Zealand group. The most startling new fact is the proof of the recent existence on the Chatham Islands of two

birds whose nearest allies inhabited the distant group of the Mascarene Islands within the historical period. These are a flightless rail very closely allied to the Aphanapteryx of Mauritius, and a coot which is hardly different, except in its somewhat larger size, from the extinct *Fulica newtoni* of the same island.

It is on the flightless rail that Mr. Forbes mainly dwells in his deductions of past changes which it is supposed to imply, and it is on these deductions only that I wish to make a few remarks. He quotes Prof. A. Newton and his brother as stating that the solitaire of Roderiquez and the Dodo of Mauritius, being evidently of one stock, and there being analogous facts in the adjacent islands, they are compelled to believe that "there was once a time when Roderiquez, Mauritius, Bourbon, Madagascar, and the Seychelles were connected by dry land"; and he then argues that there must also have been a continuous land surface between this land and the ancient land comprising New Zealand and the surrounding islands. This connecting land he supposes to have been the Antarctic continent during a mild period and with great extensions over the southern ocean. When the Antarctic ice age came on the inhabitants of this continent had to migrate northwards, and some, "such as the genus Aphanapteryx, would seem to have split into parties, which, travelling by divergent roads, finally arrived in regions so far apart as Mauritius and the Chatham Islands, unaffected by the varying climates and surroundings they experienced, being of an ancient dominating type."

It is this tremendous hypothesis which appears to me to be not only quite unnecessary to explain the facts, but also to be inadequate to explain them. If one thing more than another is clear, it is that these comparatively small flightless birds were developed, as such, in or near to the islands where they are now found, since they could not possibly have arisen on any extensive land inhabited by carnivorous mammals and reptiles, and, if introduced into such a country, could not long survive. So far as I am aware, no doubt has ever been expressed on this point, the evidence for it being so clear and its explanation on the theory of evolution so complete; and I hardly think that Prof. Newton would now maintain that the affinities of the flightless birds of Mauritius, Bourbon, and Roderiquez implied the former union of these truly oceanic islands. Allied forms of ancestral flying birds may have reached the islands without such union; and, owing to the total absence of terrestrial enemies and the abundance of food, may have developed into the allied flightless birds whose remains are found there.

But Mr. Forbes speaks of the genus Aphanapteryx itself, presumably therefore flightless, inhabiting the Antarctic continent, and migrating northwards by two routes of about 2000 miles each, in which case, this enormous extent of land must have been as free from all carnivorous land mammals and reptiles as New Zealand and Mauritius are now. If however, the birds in question lost their powers of flight in or near the islands where their remains are found, all difficulties of this kind disappear. The Aphanapteryx belongs to a family, the Rallidæ of rails, of world-wide distribution, while many of the component genera are also almost cosmopolitan, and are represented by closely allied species in distant regions. What difficulty, therefore, is there in the same or closely allied species of this widespread group finding their way at some remote epoch to Mauritius and the Chatham Islands, and, from similar causes in both islands, losing their power of flight while retaining their general similarity of structure? To put the matter briefly: if the common ancestors of the Aphanapteryx of Mauritius and the Chatham Islands were flightless, they could not have reached those islands from the Antarctic continent owing to the length of route and the presence of enemies; while if they possessed the power of

flight no important change in land-distribution is required.

I have discussed this one point only, because it illustrates the very common practice of explaining each fresh anomaly of distribution by enormous changes of physical geography, when a much more satisfactory explanation can be given involving no such vast and unsupported revolutions in the earth's surface. I am aware that Mr. Forbes adduces many other facts and considerations in support of his view as to the former extension and habitability of the Antarctic continent, some of which appear to me to be valid and others the reverse. On most of these I have already expressed an opinion in my "Island Life"; and I only write now in order to point out that the very remarkable and interesting facts, whose discovery we owe to Mr. Forbes's energy and perseverance, do not add anything to the evidence already adduced for that view, but may be best explained in a far simpler manner, and without requiring any important changes in the geography of the southern hemisphere.

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Is New Zealand a Zoological Region? (S575)

Wallace's reply to this question, printed in the 18 January 1900 issue of Nature.

Will you allow me to make one remark on the letter of Mr. H. Farquhar (p. 246) advocating an affirmative answer to the above question. It is this: Throughout the whole argument there is an assumption which vitiates it, namely, that the amount of resemblance of the New Zealand fauna to that of *Australia* is what alone determines its resemblance to that of the *Australian Region*.

Apparently, Mr. Farquhar does not believe that New Caledonia and the New Hebrides belong to the Australian Region, otherwise he would not adduce the fact of the land-shells of New Zealand being related to those of the above-named islands as an argument in his favour; and if these are omitted, then must New Guinea be also omitted. And if Australia by itself is to become a "Zoological Region," New Guinea and its surrounding islands must be also a "Region," the Central Pacific Islands another, and the Sandwich Islands yet another! This indicates the difficulties that arise if the Australian Region, as originally defined by Dr. Sclater and myself – and which I still hold to be far more natural than any subdivision can make it – be rejected. – Alfred R. Wallace.

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Section 7. Land Reform and Economic Issues

Introduction.

It will surprise many readers that this section is more than five times the length of the preceding one, and longer than the one covering evolutionary subjects, but the fact of the matter is that Wallace was deeply committed to the eradication of societal ills, and spoke out whenever the occasion presented itself, which was often. Although his suggestions for improvements were not always taken seriously by the ruling classes, they were highly appreciated by the rank and file, and he became something of a "working class hero."[a]

Wallace's economic, especially land economic, positions were complex, and have not been studied in much detail. He was self-taught in economics, and his wide reading on the subject is evidenced by his frequent citations to works by John Elliott Cairnes, Richard Cobden, Henry Fawcett, Henry George, Thomas Malthus, John Stuart Mill, Adam Smith, and Herbert Spencer, among others. Mill and Spencer were particularly strong influences on the development of his ideas on land reform, which featured a model of long-term, but progressive, buying-out of large land holders through a mechanism based on terminable annuities. Some of his monetary policy views were progressive enough to attract the attention of the prominent economist Irving Fisher, who later dedicated a book to him.

The writings to follow wind their way through a variety of topics: how to make best use of public lands, the complexities of free trade and nonrenewable resources, landlordism, land nationalization *per se*, Georgeism, interest and the definition of capital, land enclosure, commons lands, taxation, paper money, utility and railroad nationalization, etc., etc. Most of the positions he took were distinctly anti-establishment, and those in power probably gave into a good bit of head-shaking as they read his words in the leading papers of the day.

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Public Gardens. Parks and Building Ground. (S208a)

This letter to the Editor was published in the first volume of The Garden, in its issue of 9 March 1872.

No one can be more desirous than myself that parks, gardens, and open spaces should be multiplied in the vicinity of all our large towns, and that they should be made as

[a] It should be noted that Wallace's upbringing put him in the middle, not working, class as he grew up, but that is a statement about his family history, not his popular appeal in his middle and later years.

extensive as possible; but certain considerations, left entirely out of sight in the article on Victoria Park in your last week's issue, seem to render it advisable, with the view of furthering this very object, that the strips of land in dispute should, as originally proposed, be let for building on. With your permission, I will briefly state what these considerations are.

1. When a sum of public money is voted for a park, and a special provision is made to enable the park to become in time self-supporting, and even to have a surplus revenue which may eventually pay back to the nation its original cost, it seems to me to be bad policy to endeavour to annul these provisions, and thus make it a perpetual charge on the revenue. For, if this is done, it must inevitably render any Government both less willing and less able to entertain the question of establishing new parks. The fact of the great increase of population round the park, which is adduced as an argument for keeping the building land open, is the very circumstance which has rendered the surrounding land so valuable, and which will enable it to produce the required revenue.

2. There is, however, a very important principle involved in this question, which has been strongly advocated by Mr. John Stuart Mill, viz.: – that as much as possible of the *increase* in the value of land which is directly caused by the public, should belong to the public. Now there is no more certain way of increasing the value of the surrounding land than by making a beautiful park in a densely peopled district; and by reserving a strip of land all round that park at the outset, *expressly to be built upon when the demand arises for it*, you do actually secure a large share of the increased value to the public. The strip of building land around Victoria Park, for instance, is certain to increase in value; so that, besides producing a good revenue for the first term of the leases, it will probably, as those leases fall in, be re-let at a much higher rate, and so produce an increasing revenue, which may not only suffice to pay for the present park, but may also supply funds towards the formation of new parks in outlying districts where they will be then more needed.

3. But if the strips of land in question are now permanently attached to the park, we not only lose all this present and prospective benefit ourselves, but we make a free gift of the wealth we have created to men who have no earthly right to it. For there will then be a most valuable building frontage to the park, about three miles in extent, in the hands of private persons, whose property will rise to double or treble its previous value the moment we extend the park up to their boundary, and give them the certainty of a perpetual view over it. Many of these freeholders will have purchased their ground at a low price, because it was believed that they would be entirely shut out from the park by a continuous line of houses on the reserved land.

4. It is of the very first importance to establish the practicability of the principle of always securing, at the time when great improvements are first made at public expense, an additional tract of cheap land, the enhanced value of which, created by the improvement, may at some future time repay its cost; and I cannot but think that it is very short-sighted policy, under *any* circumstances, to claim this reserved land, and so neutralise this highly desirable result. It is almost as suicidal as the practice of those Governments which, having obtained a loan on the faith of the establishment of a sinking fund, appropriate the revenues set apart for that purpose on the first monetary pressure.

5. On looking at your very clear map of Victoria Park, it is easily seen that the strips in question form a very small part of the whole; and although twenty-nine acres in one lump is a good-sized piece of land, it is of far less importance when in a strip nearly three miles long. For a large portion of this extent, the strips are only one hundred feet wide;

and it cannot much affect the park as a place of recreation whether the houses, which will soon inevitably encircle it, are built on the outer or the inner side of the surrounding roads. On the other hand, it is a matter of the highest importance to prove, that in populous districts parks can be made self-supporting, after a few years, by the simple method of surrounding them with a belt of land reserved for building, the constantly increasing rents of which shall benefit the public instead of private landowners. I therefore maintain that it is the true interest of the people at large that the original scheme should be carried into effect, because it is founded on a true and most important principle, which will favour (as surely as the opposite course will check) the multiplication of parks and gardens for the people.

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Free-Trade Principles and the Coal Question (S231)

A letter printed in the The Daily News (London) issue of 16 September 1873.

Sir, – It has now become an axiom with all liberal thinkers that complete freedom of exchange between nations and countries of the various products each has in superabundance and can best spare, for others which it requires, is for the benefit of both parties; and this principle is thought to be so universally applicable, that even when it produces positive injury to ourselves and is certain to injure our descendants, hardly any public writer who professes liberal views ventures to propose a limitation of it. It seems clear, however, that there are limitations to its wholesome application, and that there are certain commodities which we have no right to exchange away without restriction, for others of more immediate use to the individuals or communities who happen to be in possession of them. These commodities may be briefly defined as those natural products which are practically limited in quantity, and which cannot be reproduced. What is meant may perhaps be best explained by taking what may be considered a very extreme case as an illustration. Let us suppose, for instance, a country in which the springs or wells of water were strictly limited in number, but sufficiently copious to supply all the actual needs of the community who had always had the use of them, on making a nominal payment to the owners of the land on which they were situated. Acting on the principles of unrestricted free trade, and anxious to increase their wealth, one after another of the landowners sold their springs to manufacturers, who used up all the water except that required to supply the wants of their own workpeople, thus rendering the remainder of the country almost uninhabitable. A still more extreme case, but one rather more to the point, would be that of a country possessing a surface soil of very moderate depth, but of extreme fertility, and supporting a dense population on its vegetable products. The landowners might find it very profitable to them to sell this surface soil to the wealthy horticulturists of other countries; and if the principle of free trade is unlimited, they would be justified in doing so, although they would permanently impoverish the land, and render it capable of supporting a less numerous and less healthy population in long future ages.

Most persons will admit that in both these cases the exercise of the unrestricted right of free trade becomes a wrong to mankind, and should on no account be permitted; and it will perhaps be said that such cases could never occur in a civilized community, as public

opinion would not allow the landowners to act in the manner indicated even were they disposed to do so. I believe, however, it may be shown that, under circumstances far worse than those here supposed, the landowners in the most civilized community on the globe do act in a very analogous manner, and, moreover, are not yet condemned by public opinion for doing so. Let us first, however, deduce from such supposed cases as those above given a general principle determining what articles of merchandise are and what are not the proper subjects of free trade. A little consideration will convince us that most animal or vegetable products or manufactured articles, the reproduction and increase of which are almost unlimited in comparatively short periods, are those whose free exchange is an unmixed benefit to mankind; the reason being that such exchange enriches both parties without impoverishing either, and, by leading to improved modes of cultivation and an increased power of production, adds continually to the sustaining power of the earth, and benefits future generations as much as it does ourselves. On the other hand, all those articles of consumption which are in any way essential to the comfort and well-being of the community, and which are, either absolutely or practically, limited in quantity and incapable of being reproduced in any period of time commensurate with the length of human life, are in a totally different category. They must be considered to be held by us in trust for the community, and for succeeding generations. They should be jealously guarded from all waste or unnecessary expenditure, and it should be considered (as it will certainly come to be regarded) as a positive crime against posterity to expend them lavishly for the sole purpose of increasing our own wealth, luxury, or commercial importance. Under this head we must class all mineral products which are extensively used in domestic economy, the arts or manufactures, and which are in any way essential to the health or well-being of the community, and more especially those which from their bulk, weight, and extensive use could not be imported from distant regions without a very serious addition to their cost, such as is pre-eminently the case with coal and iron.

Now, it will be seen that we have here to deal with a case quite as extreme in reality as those supposititious cases with which we commenced this inquiry. For coal and iron are almost as much necessaries of life to the large population of this country as are abundance of water and a fertile soil; but there is this difference, that the water might be restored to its legitimate use, and the soil might be renewed by a sufficient period of vegetable growth; whereas coal burned, and iron oxydised, are absolutely lost to mankind, and we have no knowledge of any restorative processes except after the lapse of periods so vast that they cannot enter into our calculations. It may be replied, that the quantity existing on the globe is vast enough for the necessities of mankind for any periods we need calculate on; but even if this be so (of which we are by no means certain), it may none the less be shown that numerous and wide-spread evils result from our present mode of recklessly expending the stores in certain countries, while the same products remain totally unused in many of the countries they are exported to. For a number of years we have been increasing our production of coal and iron at an enormous rate, and sending vast quantities of both to all parts of the world, civilized and uncivilized, and have thereby produced, so far as I can see, little but evil in various forms, some of which have hitherto received little attention.

Briefly to state these: – In the first place, we have seriously, and perhaps permanently, increased the cost of one of the chief necessaries of life in so changeable a climate as ours – fuel. This is in itself so great and positive an evil that no considerations of mere convenience to remote nations, such as the construction of railways in New Zealand or in

Honduras, ought even to be mentioned as an excuse for it. Coal in winter is a question of comfort or misery, even of life or death, to millions of the people whose happiness it is our first duty to secure; and shall we coolly tell them that the Antipodes must have railroads, and that landowners, coalowners, and contractors must make fortunes, although the necessary consequence is the yearly increasing scarcity of one of their first necessaries and greatest comforts?

In the second place, by destroying for ever a considerable and ever-increasing proportion of the mineral wealth of our country, we have rendered it absolutely less habitable and less enjoyable for our descendants, and we have not done this by any fair and justifiable use for our own necessities or enjoyments, but by the abuse of increasing to the utmost of our power the quantity we send out of the country, never mind for what purpose, so that it adds to the wealth of our landowners, capitalists, and manufacturers.

In the third place, we have brought into existence a large population wholly dependent on this excessive production and export of minerals, and therefore not capable of being permanently maintained on our soil. In proportion as other nations make use of their mineral productions, and as our own minerals, from the increasing difficulty of procuring them, become necessarily more costly, so must our excessive exports diminish, and with it must diminish our power of maintaining our present abnormal population. A period of adversity will then probably set in for us, only faintly foreshadowed in intensity and duration by those arising from mere temporary fluctuations in the demand for minerals and their manufactured products.

Fourthly, we not only injure ourselves and our successors by thus striving to get rid of our mineral treasures as fast as possible, but we probably do more harm than good to the nations to whom we export them; for we prevent them from deriving the various social and intellectual benefits which would undoubtedly arise from their being compelled to utilise for their own purposes the mineral products of their own lands. The working of mines and the establishment of manufactures bring into action such a variety of the mental faculties, and so well vary and supplement the labours and the profits of agriculture or trade, that a people who wholly neglect these branches of industry can hardly be said to live a complete and healthy national life. By considering our rich stores of coal and iron as held in trust by us for the use of the present and future populations of these islands, we should probably stimulate and advance a healthy civilization in many countries which the most lavish expenditure of our own minerals, aided by our capital and engineering skill, fail to benefit.

Lastly, I would call attention to the way in which the lavish production of minerals disfigures the country, diminishes vegetable and animal life, and destroys the fertility (for perhaps hundreds of generations) of large tracts of valuable land. It would be interesting to have a survey made of the number of acres of land covered by slag-heaps and cinder-tips at our iron and copper works, and by the waste and refuse mounds at our various mines and slate quarries, together with the land destroyed or seriously injured by smoke and deleterious gases in those "black countries" which it pains the lover of nature to travel through. The extent of once fertile land thus rendered more or less permanently barren would, I believe, astonish and affright us. How strikingly contrasted, both in their motive and results, are those noble works of planting or of irrigation which permanently increase both the beauty and productiveness of a country, and carry down their blessings to succeeding generations.

This brief sketch of some of the more salient features of the subject of mineral export

will serve to show how many and various are the evil results which flow from allowing these invaluable treasures to be wasted at the dictates of mad speculation and the eager race for wealth. These considerations have a very practical bearing at the present time. The recent enormous rise in the price of coal has brought up the question of the advisability of an export duty upon it. The press, almost without exception, have opposed this as being "contrary to the principles of free trade;" and it has further been argued that such a duty would have little or no effect, because the real cause of the high price of coal is that so much is used in the excessive manufacture of iron. But it is evident, from the considerations here set forth, that the export both of coal and iron requires to be regulated or forbidden, and for the same reasons; and if the "principles of free trade" are opposed to this, so much the worse for those "principles," since they will be opposed not only to the true economy of human progress, but also to the clearest principles of social and national morality. Many persons will now ask whether those can be true principles which lead to the exhaustion of our coal-fields for the purpose of lighting South American cities with gas or building railways in every insolvent South American Republic, while our own hard-working population has to suffer the pangs of cold in winter, in consequence of the high price of coal which such reckless projects tend to cause. And the fact that all parties concerned – landowners, colliery proprietors, speculators, and legislators – are so far from seeing anything wrong in what they are doing that their one aim at the present moment is to secure a larger annual output, and an increased export, will be to many an additional argument for taking the property in land altogether out of private hands. Waiving that question, however, for the present, I maintain that it is a wrong to our own population, and a still greater wrong to the next generation, to permit the unlimited export of those mineral products which are absolute necessaries of life, but which once destroyed we can never reproduce. To do so is to sell and alienate for ever a portion of our land itself, and should no more be permitted to private individuals than the selling of the land surface to a foreign State.

Whether or not the period of the total exhaustion of our coal-fields can be approximately estimated, it is clear that the present vast and increasing rate of consumption must be stopped. The numerous evils of the present system I have briefly indicated – where are the benefits which counter-balance them? And the benefits, if they exist, must be large and clear and positive indeed to justify us in recklessly scattering over the whole world the mineral products of our land. It is to their possession that we attribute much of our wealth and power and national prosperity, yet we are doing our best to deprive future generations of any of the advantages we have derived from them.

It appears, then, to be clearly our duty to check the further exhaustion of our coal supplies by at once putting export duties on coal and iron in every form, very small duties at first, so as not to produce too sudden a check on the employment of labour, but gradually increasing, till, by stimulating an increased production in other countries, they may no longer be required. If other nations should see the wisdom and justice of following our example, each may in future develop and enjoy its own mineral products, may help to supply what is necessary to the welfare of those countries which do not possess these natural gifts, and may still leave an ample supply to their descendants.

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Bounties and Countervailing Duties (S310)

A letter printed in The Spectator issue of 26 April 1879.

Sir, – It was with much pleasure I saw in your last week's issue the following editorial remark: – "That a countervailing duty to any exporting nation's bounty, if it could strike the right article, and the right article only, from whatever port it came, would be a Free-trade and not a Protective measure, is really beyond question." This is exactly what I maintained in my *Nineteenth-Century* article,[1] but it is still denied by almost all Free-traders, as witness Mr. Lowe's strong protest and Sir Stafford Northcote's indignant disclaimer in Tuesday night's debate on the Sugar question.

But if a countervailing duty is a Free-trade measure when applied to neutralise a "bounty," it must be equally so when applied to neutralise a protective duty. The "export bounty" and the "import duty" produce the same result, by different means. Both enable the foreign producer to sell his goods in our market at or under cost price, while still leaving him a profit. The bounty does this directly; the import duty indirectly, by giving him a monopoly of his home trade, and therefore larger profits. It then becomes advantageous to him to increase his production to the utmost, as he thereby decreases the proportionate amount of fixed charges; and then, by selling cheap in our market and dear in his own, he strikes a fair average of profit, at the same time that he undersells us. It is clear, then, that countervailing duties, exactly balancing the unfair advantage given to foreigners by bounties and protective import duties, are justifiable on Free-trade principles; and this is the exact form of "reciprocity" which I have maintained to be "true Free-trade." I cannot myself believe that the practical difficulties in the way of its application are insuperable, because circuitous routes and reshipments from foreign ports would, in many cases, be unprofitable. If, however, the principle of such countervailing duties was adopted, and carried into execution as far as was found practicable, it would relieve almost all our domestic industries from a heavy burthen, while it would certainly have considerable effect in inducing foreign Governments to relax their present policy of almost universal Protection. – I am, Sir, &c., Alfred R. Wallace.

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A Few Words in Reply to Mr. Lowe (S312)

A note concerning the reciprocity of free trade, printed in the July 1879 issue of The Nineteenth Century.

Although the subject of 'Reciprocity' is not yet of sufficient popular interest to be the subject of another article in the *Nineteenth Century*, I beg to be allowed to say a few words in reply to Mr. Lowe's[a] very forcible, not to say violent and contemptuous, article.

I have often been at once amused and disgusted at a common practice in the House of Commons, of flatly denying facts which a previous speaker had alleged as undisputed, or

[1] "Reciprocity the True Free Trade," *Nineteenth Century* 5: 638–649 (April 1879).

[a] Robert Lowe (Lord Sherbrooke), M.P. (1811–1892), British-Australian statesman.

had proved on good evidence; but I hardly expected that, in an article deliberately written and published, so eminent a politician as Mr. Lowe would condescend to similar tactics, and attempt to overthrow an adversary by the mere force of his weighty *ipse dixit*. Yet the most important part of his reply to me, that which he thinks – 'so complete and absolute that I am convinced, had it occurred to Mr. Wallace, his article would never have been written' – consists in the assertion that my proposal, even if carried out, would be quite inoperative, because, when foreign countries protect any class of manufactures, they thereby acknowledge that they cannot compete with us in our own or in any neutral markets, and that 'by the conditions of the problem it is impossible' that they should do so.

But the fact that such protected goods *are* imported into this country, and *do* compete successfully with our own, must surely be known to Mr. Lowe; and I am afraid the most charitable view we can take is, that his article was written with some of that want of consideration which he so confidently alleges against myself. What does he say to the fact that the United States sent to this country in 1877 manufactured goods to the value of 3,559,521 *l.*, including large quantities of cotton and iron goods, sugar, and linseed oil-cake, although every one of these manufactures is protected by almost prohibitive duties? Again, we have paper imported to the value of more than half a million a year, although the manufacture is heavily protected in every country but our own; and the competition of this protected foreign article, which, according to Mr. Lowe, *cannot* compete with ours, has yet ruined many of our paper manufacturers. So iron goods of all kinds are heavily protected in France, Belgium, America, and some other countries; yet iron and steel in various forms were imported in 1877 to the value of over 1,500,000 *l.* Our total imports of manufactured goods (including metals) in 1877 amounted to 64,635,418 *l.*; and almost the whole of these goods are protected in the countries which export them. Most of them, in fact, are sent to us because they *are* protected, the manufacturers finding it to their advantage to work to the full power of their plant and capital, selling the larger portion of their output at a good profit in the home market, and, with the surplus, underselling us, which they are enabled to do *because all the fixed charges of the manufacture are already paid out of the profits of the domestic trade*.

Having thus disposed of Mr. Lowe's main attack, and shown that what he declares to be 'impossible' nevertheless occurs, I have only to notice his singular attempt to put me in the wrong by giving a new and unjustifiable meaning to one of the plainest words in the English language. He says that I am quite mistaken in considering 'free trade' to be essentially mutual – to mean, in fact, what the component words mean – free commerce, free exchange, free buying and selling. On the contrary, says Mr. Lowe, it means free buying only, though selling may be ever so much restricted. But surely buying alone is not 'trade,' but only one half of 'trade.' Just as imports cannot exist without exports of equal value, so I have always considered that buying cannot long go on without selling, and that the two together constitute trade. Mr. Lowe, however, says I am historically wrong, but he does not give his authorities; and without very conclusive proof I cannot admit that the English language, as well as the English commercial system, was revolutionised by the free-trade agitation.

One of the most important of my arguments – that reciprocal import duties are just and politic, in order to secure 'stability and healthy growth' to our manufactures – Mr. Lowe, with more ingenuity than ingenuousness, converts into a plea on my part for stagnation and freedom from competition; and he maintains that the power of foreign

governments to alter their import duties and bounties at pleasure, with the certainty that we shall take no active steps to neutralise their policy, is a healthy incentive to activity and enterprise!

The remainder of Mr. Lowe's arguments and sarcasms may pass for what they are worth; but, while so many of our manufacturers, and that large proportion of our population who are dependent directly or indirectly on manufacturing industries, are suffering from the unfair competition brought upon them by foreign protection, the allegation that these form an insignificant *class*, and may be properly spoken of as 'particular trades' whose prosperity is of little importance to the rest of the community in comparison with that *summum bonum* – cheap goods – deserves a word of notice. I therefore beg leave to call attention to Richard Cobden's opinion of the supreme importance of these manufactures to England's welfare. He says:

> Upon the prosperity, then, of this interest [the manufacturing] hangs our foreign commerce; on which depends our external rank as a maritime state; our custom-duties, which are necessary to the payment of the national debt; and the supply of every foreign article of domestic consumption – every pound of tea, sugar, coffee, or rice – and all the other commodities consumed by the entire population of these realms. In a word, our national existence is involved in the well-being of our manufacturers.
>
> If we are asked, To what are we indebted for this commerce? we answer, in the name of every manufacturer and merchant in the kingdom, The *cheapness* alone of our manufactures. Are we asked, How is this trade protected, and by what means is it enlarged? the reply still is, By the *cheapness* of our manufactures. Is it inquired how this mighty industry, upon which depend the comfort and existence of the whole empire, can be torn from us? we rejoin, Only by the *greater cheapness* of the manufactures of another country.[1]

In another passage in the same volume he says: 'The French, whilst they are obliged to prohibit our fabrics from their own market, because their manufacturers cannot, they say, sustain a competition with us, even with a heavy protective duty, never will become our rivals in third markets where both will pay alike;' from which it appears that he never contemplated the state of things that has actually come about, when, by means of protective duties, and our open markets supplying all the world with cheap coal, iron, and machinery, other nations have been enabled to foster their manufactures till they have reached such a magnitude as not only to supply themselves, but, with their surplus goods, produced cheaply by means of protection, are actually able to undersell us at home. That time has, however, come; and I feel sure that if Cobden were now among us, his strong sense of justice and clear vision as to the true sources of our prosperity would lead him to advocate some such course of action as I have proposed, in order to bring about those benefits to the all-important manufacturing interests of our country, which the system of free imports – miscalled 'free trade' – has not procured for it. – Alfred R. Wallace.

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correspondence on land nationalization (S339, S341 and S343)

In three lengthy letters to the Editor of The Mark Lane Express in late 1881,

[1] Cobden's *Political Writings*, vol. i. p. 227.

Wallace discussed the plan for nationalization of the land set out by the then newly-formed Land Nationalisation Society.

Nationalisation of the Land (S339: 3 October 1881)

Sir, – As you have done me the honour to refer in your issue of the 19th September to my practical scheme of Land Nationalisation, I ask permission to occupy a little of your space with a brief exposition of the scheme, with some indication of its wide scope and of the numerous social evils it is calculated to ameliorate. This is the more necessary because so novel a proposal is sure to be misunderstood, and denounced as utopian or revolutionary; while I am informed that the mere reference to it in an editorial article as worthy of attention has already called forth an uncompromising attack in the columns of a contemporary journal.

It is necessary to premise that Land Nationalisation is not proposed in the interest of any class, but as a reform, vital to the national welfare, and at the same time directly beneficial to every class and every individual. By its means the farmer will obtain that freedom of action, that fixity of tenure, and that absolute security of possession of all the proceeds of his labour, skill, and capital, which is what he sorely needs, but which he will assuredly not get by means of any probable or possible English Land Bill. So long as he is subject to landlords and agents, to law-courts and lawyers, to valuers and surveyors, he will often have to keep up a hard and costly struggle in order to obtain that simple right to the fruits of his own labour which he ought to have and may have, without the interference of any man and without the possibility of dispute. In fighting for an English Land Bill on the lines of that just passed for Ireland, he will have to fight almost alone, for no other class will have a sufficiently direct interest in the matter to help him with any energy or enthusiasm; but in claiming Land Nationalisation he will have cordial and earnest assistance from all classes, especially from the agricultural labourers and the rural population generally, from mechanics and tradesmen, and from that large class who look forward with longing eyes to a rural retirement for their latter years, now rendered almost unattainable under land monopoly.

In the space of a single letter it is impossible to discuss those general principles and practical examples which prove private property in land to be inconsistent with personal freedom and antagonistic to true national welfare; I will, therefore, pass on at once to the practical proposals by which Land Nationalisation may be brought about, and in doing so, I shall be able briefly to advert to its far-reaching beneficial influence on every portion of the community.

Much of the difficulty and confusion of thought attending questions of this nature arise from not clearly distinguishing the two distinct elements in all landed property, the payment for the use of which is improperly included in the term "rent." True rent is money paid for the use of land or other natural agents; and its value is determined by two factors – the quality or productiveness of the land itself, and the additional value given to it by the community at large, in providing public roads, railroads, or canals, in supplying labour as well as social, religious, and educational advantages, and in furnishing good markets and a surrounding population able at once to satisfy the wants and to be purchasers of the produce of the agriculturist. None of this value has been created either by the owner or occupier of land, and it is this alone which it is proposed shall become the property of the State, the holder paying a *quit-rent* or *ground-rent* to the State, just as he

now pays his land-tax, but being free from all Government supervision or interference whatever. The other portion of the value included in "rent" (but which is really interest on money expended and compensation for deterioration) is derived from the outlay or labour of the owner or occupier, in houses and buildings, fences, private roads, drains and other permanent improvements. These are private property, and there is no need to interfere with the possession or use of them other than to declare that their owner for the time being must be the State's tenant and be thus liable for the quit-rent; or, to put it more clearly, whoever holds land from the State must be the owner of the "improvements" of whatever kind on that land. It will therefore be convenient to term these improvements collectively the "tenant right" of the land in question, since their owner is necessarily and by "right," the State's tenant of the land. This "tenant-right" will follow the law of all other personal property, so far as its capability of being bought and sold or bequeathed at the will of the owner, and it thus carries with it all the rights and privileges which pertain to a freehold, with this important reservation, that it can be held only for personal occupation and enjoyment – not as an investment. All subletting of land will thus be illegal, since, if it were once permitted, large quantities of land would be accumulated by capitalists as State tenants, and *their* tenants would be in exactly the same position as the tenants of existing landlords, equally subject to their capricious interference, equally unable to secure the fruits of their own labour.

It will now be asked, How are present or future farmers to obtain possession of this "tenant-right," without which, they cannot hold farms? This question can be best answered while explaining the process by which the land may actually become the property of the State and the new *régime* be inaugurated.

The Act of Parliament effecting nationalisation will provide:

(1) That ten years (more or less) after the passing of the Act the whole land of the country (as above defined) will become the property of the State, the existing landowners being compensated in a way to be presently explained.

(2) That a careful valuation of the land of the whole kingdom be made, separating the annual value of the land (or the "quit-rent") and the improvements (or "tenant-right"). The "quit-rent" will be the amount payable to the State, while the "tenant-right" must be purchased or otherwise acquired by the occupier. The value of the tenant-right will be estimated by the official valuers, as it will depend upon the more or less permanent character of the improvements; and it will have to be paid to the landholder by any farmer who wishes to continue in his farm, either in one sum or by means of a terminal annual charge for a moderate number of years. The ten years' interval between the actual passing of the Act and its coming into operation will not only give the necessary time for making the required valuation (which must be on every separate plot or enclosure), but it will also allow farmers to make all necessary arrangements for acquiring the tenant-right of their farms or of others more suitable to them. No doubt an extensive re-arrangement of holdings would then take place. A man with the power of getting a farm which he would be absolutely free to cultivate or improve as he pleased, and with a permanent tenure, would often prefer a much smaller one than that which he now holds under a landlord, since it would be his interest to farm highly and make all possible permanent improvements to the property.

The farmers, as a class, would thus obtain all they have ever asked or can possibly desire – freedom of cultivation, freedom of sale or transfer, a permanent tenure, and a really fair rent; and, accompanying this, there would accrue, in a very short time, dimin-

ished taxation, diminished poor-rates, and better local markets.

Turning now to the present landholders, or landlords, they will be paid, as we have seen, the fair value of all profitable outlay on the land made by themselves or their immediate predecessors, and often for that made by successive generations of tenants as well. For that portion of the value of the land which was primarily derived from the State, and should never have been given up by it, they will be compensated by means of an annuity of its full estimated value. In order that no valid claim or expectancy may be left unsatisfied, it is proposed that this annuity should extend to all heirs living at the time when the Act comes into operation, or, if thought fit, it might be extended to two generations of heirs beyond the present landholder. The absolute security of this Government annuity for three generations, free from all risks and liabilities, would render it a very fair equivalent for the land taken; and as no land whatever could then be obtained on any other terms than as a State tenant, it is not improbable that the selling value of farms after the Act was passed might be quite up to their previous average market value, because most farmers with capital would seek the opportunity of obtaining, at the earliest possible period, such farms as would suit them for permanent occupation.

Having thus shown how the scheme would affect the existing landholders and the farmers, let us turn to that portion of it which most interests other classes, and which, when clearly understood, will enlist them all as powerful advocates for its adoption. But this must form the subject of another letter.

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Nationalisation of the Land (S341: 10 October 1881)

Sir, – Having shown, in my letter last week, how the scheme of land-nationalisation which I have proposed would affect farmers and landholders, we have now to consider its action as regards other classes of the community, as well as those incidental effects which would benefit the whole nation. We are so accustomed to the enormous and often prohibitory price which has to be paid for land for residential or trade purposes, that, although a large portion of the population are thereby debarred from a full enjoyment of existence and are seriously injured in their health, few persons consider that these are the inevitable results of the pernicious system of private property – and consequent monopoly – of the land which should have been retained by the nation itself for the full and equal enjoyment of all its members. Without the use of land even existence is impossible. It follows that – granted the right of the great bulk of the population to live – they have a right to the use of land. Our present system, however, denies this right to any but the landlords, who have, legally, full power to destroy their fellow citizens by simply denying them land on which to live. The mere statement of this indisputable fact demonstrates the iniquity of private property in land; and when we know that the power which the law gives to landlords is often actually used – as in the wide devastation of many fertile valleys of the Highlands of Scotland, where the descendants of the old clansmen have been driven to emigrate, or to throng the slums of Glasgow and other great cities, in order that the land may be devoted to sporting purposes and thus bring in a greater revenue to the landlord – it is surely time that a system at once so unjust and so evil in its effects should be exposed, with a view to its speedy abolition. The monopoly of land by the rich, aggravated by the enormous increase of our accumulated wealth during the last forty years, has so completely divorced

the labouring and middle classes from any rights of property in their native soil that it is now absolutely necessary to afford special facilities for bringing back a more healthy state of things.

The impossibility of obtaining land in most parts of the country for any purposes of free cultivation or enjoyment, and the enormous revenues derived by landlords from the extension of building around dense centres of population, have been the direct causes of that inordinate growth of cities and simultaneous depopulation of the rural districts which are now admitted and deplored by all public writers. With the rapid growth of towns and cities there come numerous attendant evils – air and water polluted by smoke and sewers, and a waste of fertilising matter which is a disgrace to our boasted civilisation. All these difficulties arise from land monopoly and over-crowding. Every human dwelling, to ensure the health of the occupants, should have ample open space around it, giving garden ground to supply fresh and wholesome food, while the land is kept in a state of fertility by that house-refuse which is now worse than wasted, since it costs vast sums to get rid of and is also the direct cause of the most fatal class of zymotic diseases. The inability of the bulk of the population to obtain land leads also to the system of building-leases, with houses erected by speculative builders whose chief aim is to place the greatest number of dwellings on the smallest quantity of ground; and thousands are obliged to live in these crowded, ill-built, and unhealthy houses, sorely against their will. The absence of land around houses is a fertile source of social evil. With the middle and lower classes it is a direct and, I believe, the most powerful cause of drinking habits, since it leaves the head of a family with no interest or occupation in his home. Give every working man an acre or two of land attached to his cottage with a perpetual tenure, and he will have little time or inclination for the public-house. The land would be used in various ways, according to his taste or knowledge; but vegetables and fruit, poultry and eggs, rabbits and pork, milk and butter, would be largely produced by the labour of the owner and his family during spare hours. The innumerable little details always requiring attention in a house and grounds, adding to the comfort or enjoyment of the family, would be another source of occupation and interest if every man's house were his own. Children brought up in such a home would receive a valuable practical education in handiness and industry, while the profits would be enough to keep the family from want during periods of illness or the absence of regular employment. All this is not theory, but a mere statement of what actually does happen whenever the peasant or labourer occupies his own house and has a useful plot of land attached to it. Arthur Young, Sismondi, Inglis, Laing, Howitt, Kay, Thornton, Laveleye, Boyd Kinnear, and many other observers have noted the facts in every country in Europe. Whether in Norway or Italy, in France, Spain, or Germany, occupying-ownership of house and land is invariably attended with comfort and well-being, with sobriety, contentment, and the absence of pauperism; while the opposite condition – of large estates and a peasantry divorced from the land – as invariably co-exists with pauperism and misery, vice and discontent. The same good results have occurred in England whenever landlords have been wise enough to give their labourers land, as on Lord Tollemache's estates in Cheshire and in several other cases; and it may be asserted as a conclusion supported by an overwhelming mass of evidence, that the divorce of any population from the free use of land is a direct cause of pauperism, disease, vice, and crime, and tends in a variety of ways to deteriorate the whole social condition of the people among whom it prevails.

In due time and place I am prepared to substantiate this statement by a body of

detailed evidence. Accepting it now as true, it fully justifies the proposal contained in my scheme – that, when the State acquires possession of the land, it should retain power to remedy this vast evil by permitting every Englishman, *as his right* once in his life, to obtain a plot of land, for personal occupation at its current agricultural value. To render this "right" beneficially available to its widest extent, such restrictions only as are absolutely needful should be placed upon the choice of land. For instance, to avoid needlessly dividing or cutting-up fields, land should only be available for this purpose alongside the public roads, and the consent of the owner might be required for land within a limited distance of his house or private grounds. A limit might also be placed to the quantity of land taken from any one farm or estate (say five per cent. of the whole, for instance); but, with such obvious exceptions, it is evident that the field of choice should be as unrestricted as possible, in order that population might take a free course, instead, as now, of one district being kept without population, while, in another, men are forcibly crowded together in overgrown towns and cities.

The effects of such freedom of choice in fixing upon a permanent residence would be gradually to check the increase of the towns and to repopulate the country districts. Rural villages would begin a natural course of healthy growth, and if the minimum of land to be taken for one house were fixed at an acre (the maximum being four or five acres) these could never grow into crowded towns, but would always retain their rural character, picturesque surroundings, and sanitary advantages. The labourer would choose his acre of land near the farmer who gave him the most constant employment and treated him with most consideration; and besides those who would continue to work regularly at agricultural labour, there would be many with more land of their own or with other means of living, who would be ready to earn good wages during hay or harvest time. With a million of agricultural labourers, each holding an acre or more of land, and at least another million of mechanics doing the same thing, and all permanently attached to the soil by its secure possession, that scandal to our country, the scarcity of milk and the importation of poultry, eggs, and butter from all parts of the Continent would come to an end, while the vast sums we now pay for this produce would go to increase the well-being, not only of the labourers themselves, but of all the retail and wholesale dealers who supply their wants. Our most important customers are those at home, and there is no more certain cure for the now chronic depression of trade than a system which would at once largely increase the purchasing power of the bulk of the community.

Farmers will, no doubt, at first be inclined to object to any such extensive power of preemption of land as I have here indicated. But a little consideration will show them that they would be gainers rather than losers by it. In the first place, many large districts would for a long time be unaffected by it, except to the extent of the plots chosen by the labourers who cultivate the land; and to have a sober and industrious body of workmen permanently settled near them would certainly be to the farmers' advantage; – for it must be remembered that only the industrious and provident labourer could save the money necessary to purchase the tenant-right of his chosen lot. The public generally would avail themselves of the privilege by degrees, and as compared with the large area of the country, to a very small extent, because the strict limitation of the privilege to a *personal occupation*, and to a *single occasion*, would lead to its being exercised chiefly in the vicinity of towns and villages where people's occupations obliged them to live, and in remoter rural districts only by persons retiring from business or such as could afford a country house.

The two main points of this branch of the subject are – firstly, that the vast and overwhelming social importance of the free acquisition of land for a healthy home at its agricultural value is such as to overpower all the sentimental objections of a class who, it must be remarked, now willingly submit to the same or worse annoyances when imposed upon them by a landlord; secondly, that this free choice of land for a home is so great and tangible a boon to all classes, from the agricultural labourer up to the retired merchant, that, once convince people of its practicability, and you will set up a movement powerful enough to overwhelm the opposition of the vast landlord interest in all its ramifications. The man who, in his native country, cannot live where he wishes to live, but is dependent entirely on the pleasure of landlords and the interest of land-speculators, is not a free man as regards one of the most essential of the attributes of true freedom. When the people of this country clearly understand that nothing but an immoral system of land monopoly stand between them and freedom to enjoy their native soil; and, further, when they are shown that this system may be abolished without wronging any individual, while it will certainly tend to eradicate from our land that great blot on our civilisation, persistent pauperism in the midst of ever-increasing luxury and wealth, – and when they see further that, as Mr. George[a] has demonstrated by a strict logical deduction, this connection is a necessary one, and that private property in land is the actual cause of the strange phenomenon of poverty and even famine in our midst, notwithstanding the vast forces of nature now enlisted in our service and producing ever-increasing stores of wealth – when they clearly understand this, the end of landlordism will not be far off. The farmer may agitate for his English Land Bill as a temporary palliative, but he must look forward to land-nationalisation as the only means of obtaining that absolute freedom of action and that permanent interest in the soil which alone can renovate British agriculture.

In conclusion, I must call attention to the vast revenues derived from the soil (now enriching one limited class and to that extent impoverishing all other classes), which would take the place in the national treasury of the whole of our indirect taxation, enabling us to abolish custom duties and so have really free imports, and at the same time setting free an army of unproductive officials, now paid by the productive workers of the country. To the far-sighted politician no less than to the social reformer Nationalisation of the Land thus commends itself, as offering a solution of many difficult problems and a remedy for many crying evils.

* * *

Nationalisation of the Land (S343: 14 November 1881)

Sir, – "An Inquirer" has so well answered one part of Mr. Leadam's criticism of my proposals for Land Nationalisation as to relieve me of one-half the trouble of a reply, and in the remaining part I find so much of those vague assumptions and forebodings which have always appeared when any great reform was proposed, and have almost always turned out to be erroneous, that little of direct or tangible objection remains. I will, however, with your permission, make a few remarks on some of his statements.

And first, I can hardly believe that Mr. Leadam supposes your readers to be so simple as to be hoodwinked by his fourth paragraph, in which he endeavours, in elaborate and

[a] Henry George (1839–1897), American economist and reformer.

logical form, to show that 200 years ago the "landowners were relieved by Parliament, that is by the State, from burdens on the land which the State had originally imposed;" and he therefore argues that the State has now no right to interfere with this settlement. Poor landlords! How hardly they have been used by "Parliament" – that is, by themselves; by "the State" – that is again by themselves! For the facts are that the landlords as a body originally obtained their lands by grants from the *Crown*, on condition of certain onerous and costly services constituting the *feudal* tenure. By degrees they relieved themselves of these burdens, which they threw upon the townspeople, merchants, and generally on the landless classes; and finally, by means of self-made enclosure acts, they deprived the labourers and the people generally of the best remnant of their inheritance in the soil of their native land. Every one of these "Acts" was an act of robbery, spoliation, and confiscation, although it was done by form of law; and now, when after centuries of struggle the people have at length obtained some voice in the making of laws, they are actually told that it will be "unjust" and "inconsistent" to make any claim for the restitution to the whole people of the land which has been taken from them by a class, except at the monopoly prices which the landlords, legislating always in their own interests, have created!

I, however, have never rested my case on the undoubted fact that the landlords have unjustly thrown off the burdens which originally attached to their lands, but rather on the countless economical, social, and moral evils produced by "landlordism" – evils so great and so clearly demonstrable, and so prejudicial to the entire community, that the State, as the representative now of the whole community and not of a mere section of it, has a right to take the necessary steps to cure those evils, even if in doing so the landlords – who have hitherto alone benefited while all others have suffered – should have their wealth somewhat diminished and their power to injure their fellow men taken away from them. On the principle that "the public safety is the highest law," it is necessary that the land should ultimately be held by the nation for the benefit of all, unencumbered by perpetual pensions to the descendants of the present holders; and I maintain that my scheme effects this with the very minimum of injury to existing owners or presumptive heirs. That the evils of the present system *are* as great as I have indicated above I am prepared to prove, and on this fact I rest my whole case.

In the remainder of Mr. Leadam's letter I find nothing but misrepresentations of my proposals, or the most wild and improbable forecasts of their effects, and I am quite willing that they should remain side by side with my letters for your readers' impartial judgment.

To Mr. J. Boyd Kinnear's questions I answer as follows:

1. The present and all future tenants are to be holders of their farms in perpetuity, under the sole conditions of punctual payment of the State quit-rent and personal occupation.

2. The State selects no tenant, because the owner of the Tenant-Right for the time being *is* the State tenant, and is liable for the quit-rent. If at the supposed commencement of the new order of things the then tenant declines to continue, it is for the landlord to find a purchaser for his Tenant-Right, and till he does so he holds the land himself as the State tenant. He will be in the receipt of the Government annuity as an equivalent for the quit-rent taken by the State, and he must, of course, be responsible for it till he finds a purchaser who will assume the responsibility; but I feel convinced that under the *régime* of a nationalised land such cases will rarely if ever occur.

3. No limit whatever will be placed on the quantity of land one person may *bonâ fide*

occupy; because all interference of such a kind would be hurtful and totally unnecessary. With some men and in some districts a thousand or even five thousand acres might perhaps be profitably farmed, with other men and in other districts ten or twenty-acre farms might be wanted, and might succeed best. Perfect freedom both as to the area farmed and the mode of farming are essential to progress in agriculture, as they are to the welfare of individuals and of the community. As to a definition of what is "personal occupation," I apprehend that in practice there will not be much difficulty. A capitalist farmer might, of course, employ any number of foremen or bailiffs – so long as they were not sub-tenants in disguise; but I apprehend that, when every man who can farm will be able to get land under the favourable terms my scheme supposes, skilful and trustworthy bailiffs will be so scarce that the farmer will find it far more profitable to hold no more land than he can personally superintend, and expend his surplus capital in bringing that into the highest possible state of productiveness.

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In another trio of letters on "The Land Question," this time appearing in The Times (London), Wallace continued to describe his plan for land nationalization.

S344: 26 November 1881

Sir, – There is a fallacy which underlies the whole argument of Mr. Caird's[a] letter in *The Times* of November 23, as well as all similar arguments, and as it is not often distinctly stated by writers on political economy, I ask permission to lay it before your readers.

Admitting – though only for the sake of argument – that all the facts stated by Mr. Caird may be correct, his position amounts to this – that the system of cultivation which produces the greatest return with the smallest expenditure of labour is best for this country, under its present system of land tenure, and without any regard to collateral result on the well-being of the people. Now, if this argument is a sound one, it will hold good under any possible or conceivable extension of the system, and it must also apply to other industries as well as to agriculture. Let us then suppose an extension of science and labour-saving machinery to such a degree that the whole land of the kingdom can be, and is, cultivated with one half the number of labourers now employed, while an even larger produce than at present is obtained. What would be the inevitable result? In the first place, it is certain that the rent of land would rise greatly; in fact, so much, that the landlord would absorb almost all the saving in cultivation, because capital would always flow to agriculture so long as it gave the same average return as when invested in other ways, and thus competition would prevent the farmer getting more than a very small share of the increased wealth produced. And what will become of the half-million of labourers discharged from the farms? They will necessarily flow to the towns (as they have been flowing from the same cause for some time past), adding to the overcrowding and increasing the already seething mass of poverty, misery, and vice. But if, at the same time, all other industries are equally affected by labour-saving machinery, and half the labourers of all kinds can be dispensed with in every trade and manufacture, what is to become then of

[a] Sir James Caird (1816–1892), British agriculturist.

our surplus labouring population? Are they to be supported as paupers? Or are they to be exported to other lands like so many horses no longer required by the landlords and capitalists? That is the millennium to which the people – the producers – are asked to look forward with satisfaction, a millennium in which the total wealth produced will be greatly increased, but in which there will be no room for half the present population of workers in their native land, and in which those who remain will exist, not to seek after and earn their own well-being and social advancement, but solely as a means of adding to the already excessive wealth of great landowners and great capitalists.

This is the necessary result of all advances in industrial economy so long as a limited class have absolute possession of the land, and are thus able to absorb much of the surplus wealth produced by the entire community, and it is a result which the actual producers of this wealth can hardly be expected to look forward to with the same satisfaction as the landowners and their advocates. Unless our teachers and legislators clearly recognize that men are to be considered before wealth, and that a system which supports fewer labourers in the country does not tend to better their physical and social condition, does not give them more independence or more leisure for mental occupation or physical enjoyment is a political blunder as well as a crime against humanity, they must not expect that the people who labour and who suffer will accept much longer either their teaching or their rule. – Godalming, Nov. 24.

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S345: 7 December 1881

Sir, – In replying to "M. P.," who, I presume, represents the views of Mr. Caird, I would first assure him that my letter was written in very serious earnest, and the fact that it was to him so new and unreal as to appear like an elaborate satire is the best proof of the necessity of calling attention to opinions and conclusions which are now steadily making their way among the working classes.

"M. P." says I have put an impossible case. This I deny. I have put an extreme case, no doubt, but one well within the bounds of possibility, and it is by supposing extreme cases that the fallacy of many political and social doctrines may be best exposed. He then proceeds to point out that the very process I assume as the basis of my argument has been going on for the last 40 years, and he implies – but is careful not directly to assert – with beneficial results. This, however, is the very point at issue. I maintain that the results have been most disastrous, as a few indisputable facts will show.

We have no means of arriving at the total increase in the value of landed property during the last 40 years, but from *data* supplied by Mr. Caird and Mr. Brodrick (in "English Land and English Landlords") it cannot be less than 50 per cent., if we take account of the enormously increased value of land in towns. The income-tax returns for 35 years show that the assessment has more than doubled in that period, indicating a very great advance in the wealth of a section of the community. During the last 40 years our imports have increased more than five-fold and our exports nearly three-fold, while great fortunes have been made by contractors, colonists, and traders in all parts of the world. The largely increased wealth of the very wealthy is admitted, as is also the far more luxurious and expensive mode of life of the professional and upper middle classes – all due primarily to that increased command over nature which the extensive use of machinery and

utilisation of natural forces have given us. But how has all this increase of wealth affected the labourers who have actually created it? "M. P." says their wages have increased. That is true; but, I maintain, barely in the same proportion as the necessaries of life have increased in cost. Mere wages prove little unless we know the whole of the accompanying circumstances. Forty years ago the labourers had advantages they seldom now enjoy. They usually had land for gardens, while the more numerous commons and road-side wastes enabled them to keep some domestic animals, and skim milk was often obtained free or at a nominal rate. But there is one infallible test of wellbeing, and that is the amount of pauperism. Had the labourers derived any benefit from the increased wealth of the country corresponding to that of the landowners and great capitalists, they should have been so much raised in their general condition that actual pauperism should have almost disappeared. Instead of that we find it nominally stationary, but actually increased; for, taking the average of the first and last 12 years of the period, 1849–1880, the latter is slightly in excess (1849–1860, average number of paupers 1,036,005; 1869–1880, ditto, 1,057,278); but we know that in the later period the Poor Law has been more strictly administered, while the advance in education has increased the number of those who annually starve rather than seek parish relief, so that there is a vast body of the abject poor who never appear in the Union records. These facts demonstrate our social failure. The increase of our wealth has not diminished our poverty. Professor Cairnes (in his volume on "Some Leading Questions of Political Economy Newly Expounded") states – referring to our enormous growth in wealth during the present century: – "The large addition to the wealth of the country has gone neither to profits nor to wages, nor yet to the public at large, but to swell a fund ever growing, even while its proprietors sleep – the rent-roll of the owners of the soil." And only the other day, at Rochdale, Mr. Bright told us that the rural districts were becoming depopulated and deteriorated, because the younger labourers, finding "that they can never become anything but labourers at very low wages, are leaving the rural parishes in which they have been born," and emigrating to the great towns or to countries across the ocean. "Our landed system," he adds, "with its great estates and farms, cuts off the labourer almost entirely from the possibility of becoming either a tenant or an owner of the land, and as he has no object in remaining there he goes away." It is not only Mr. Bright who says this; it is an admitted fact which writers in the Press continually deplore, and this fact, taken in connexion with persistent pauperism in the midst of our ever-increasing wealth, is the condemnation of the system upheld by Mr. Caird and "M. P." as all but perfect. That system is, to treat the land as existing solely to create wealth for the landowners and the capitalists, not for the people of England as such. Just so many of the people as are required to create most wealth with least expenditure will, of course, be retained; the rest may shift as they can or leave the country. But the people who labour and actually create all capital and all wealth have now some political power, and will soon learn how to use it. When they do so, it is hardly likely that they will rest contented with a system which treats them as mere creators of wealth for others, who can no more participate in that wealth than their fellow-labourers, the farm horses, and the farm steam-engines.

The subject is far too wide and too difficult to be adequately discussed in the columns of a newspaper, but I have now a volume ready for the press[a] in which the facts and the

[a] *Land Nationalisation*, Trübner & Co., London, 1882.

arguments are systematically, though briefly, set forth, and the remedy to which they all logically point is fully explained. – Godalming, Dec. 3.

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S346: 3 January 1882

Sir, – Will you permit me to say a very few words in your columns in reply to the gentlemen who have criticized my letters on this subject?

Lord Borthwick puts a still more extreme case than mine, and supposes the earth to produce its fruits without labour of any kind. I accept this case, and maintain that, under our present system of land tenure, this great apparent blessing would in reality be a curse to the country. The crops, it is true, would still be there, ready to be consumed by the nation, while all persons now employed on the land would merge into other industries. In other words, the rural districts would become vast unpeopled tracts devoted to the profits and the pleasures of the rich landowners, while the whole population of the country would be massed into manufacturing towns and cities. This would be the inevitable result because it would be (even more than it is now) the landlord's intent to let no land for building but around densely peopled centres of industry, where it commands from ten to 100 times its value in the open country; and this unnatural crowding in towns and cities, with its innumerable evils and ever-increasing difficulties of sewage utilization and water supply, and all for the aggrandizement of one class, the landowners, is the necessary result of the extension of labour-saving appliances in agriculture under our present system of land tenure. Lord Borthwick's supposed case, then, so far from furnishing an argument against me, furnishes the most powerful illustration of the evil results of that system even under the most favourable conditions. The statement that the conditions supposed would lead to shorter hours of labour is so opposed to all the facts – the notoriously increased struggle and competition for a living during the last 40 years, the constant high pressure of modern business life, and the need for the vigilant enforcement of penal laws against overworking women and children – that it requires no further refutation.

To Mr. Hardcastle I reply that his belief or supposition that labourers have now more land than formerly is totally opposed to the facts. The old cottages, with their ample gardens, have largely given place to rows of cottages in towns and villages, built by speculators and let to weekly tenants. Mr. Hardcastle takes two recent dates to show that pauperism has diminished. This is really trifling with the public. I can take two others, 1853 and 1880, which show that it has increased. I took the averages from the "Financial Reform Almanack" for the first and last 12 years of the 32 years of which statistics are there given, and though I find that I made a mistake in my actual figures (having divided by 10 instead of 12), yet my statement that the average pauperism of those two periods has somewhat increased is strictly correct. I also beg to call Mr. Hardcastle's attention to the fact that during the very period when our exports and imports were increasing most rapidly – from 1863 to 1873 – reaching their *maximum* in the latter year, and when our commercial prosperity was at its very highest, our total number of paupers of all classes, as well as our able-bodied paupers taken separately, was at a *maximum*, a striking comment on the current opinion that commercial prosperity is a true test of national well-being.

Of the same character as the comparison of the number of paupers at two fixed dates

is Mr. Hardcastle's quotation of the statistics of incomes during the last five years, when the whole question under discussion refers to the progress of the country during the last 30 or 40 years. It is, of course, a necessary result of the long-continued commercial and agricultural depression that large incomes should diminish, and smaller incomes, many of which are fixed, should correspondingly increase, but what bearing this has on the question at issue it must puzzle your readers to discover.

To avoid further misconception, I will add that I am as firm a believer in the value of the application of labour-saving machinery and improved methods to every department of human industry as any man can be; but the whole object of my letters has been to show that, under the present system of land tenure, the community at large has not derived an adequate benefit from the vast accessions to human power and the vast saving of human labour due to the progress of the arts and sciences during the last half-century, and therefore that the system stands condemned. – I am yours, &c., Alfred R. Wallace, Godalming.

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Mr. Wallace on Land Nationalization (S358aa: 1883)

Another sizable letter on land nationalization, printed in the Pall Mall Gazette issue of 3 February 1883.

Sir, – Allow me to make a few observations on some statements referring to my supposed views by the writer of the article "Short Notes on Land Nationalization," which appeared in your issue of the 24th ult. The writer acknowledges that much of what I say with regard to the unequal distribution of wealth and the general evils of landlordism is correct; but in the latter part of the article he so mixes up my proposed remedy with that of Mr. George and of other land reformers that I do not feel disposed to reply to this portion of his remarks, though I shall be glad to do so if he will confine himself to my own facts and proposals, as stated in my book on "Land Nationalization," which I hardly think he can have read through. It is with his statistical argument that I now wish to deal, as he maintains that it "cuts away the whole foundation for the special remedy suggested by Mr. Wallace and Mr. George." He states that the total increase in the value of land, houses, and mines, as shown by income tax returns, was 65½ millions from 1862 to 1880, and he adds (without a particle of proof) that "the largest proportion" of this increase was due to improvements. He then states that the increase in the income of the country for the last twenty years was reckoned by Mr. Dudley Baxter at between 600 and 700 millions, and from these alleged facts just as they stand he draws the inference given above. Surely never was a more striking example given of "how not to use statistics." For note, first, that one side of the comparison is taken for eighteen years and the other side for twenty; that one side is taken from one authority, with a deduction at hap-hazard, the other from another authority, and professedly "an estimate." Secondly, no notice is taken of the fact that the wealth-producing population has increased about 20 per cent. in the twenty years, while the land, the owners of which absorb so much of the proceeds of their labour, has not increased at all. Thirdly, we have to consider that the whole increase in the rents of land and houses is a deduction from the increased incomes of the people. Fourthly, that food and many other necessaries, as well as all rates and taxes, have increased enormous-

ly, and form important deductions from the increased incomes of the people. And, lastly, that an unknown but enormous amount of income is drawn directly or indirectly from foreign countries. These various considerations render such rough and undigested figures as your correspondent gives utterly worthless and misleading. Let us, however, see what we can learn from one official authority – the income tax returns, as given in the "Financial Reform Almanack for 1881," the only book I have at hand to refer to. Taking the last twenty years therein given, 1858 to 1878 (and making the necessary corrections for the transference of railways, canals, waterworks, mines, &c., from Schedule A to Schedule D in 1866), I find the increase of the annual value of lands, tenements, and tithes to be about 60 millions on 85 millions, or 70 per cent. Referring to Schedule B, we find that agricultural land increased 17 millions on 52, or 33 per cent. This leaves an increase of 37 per cent. estimated on house property, &c.; and if we allow even 20 per cent. for improvements (probably more than the truth), and add the remaining 17 per cent. to the 33 per cent. on agricultural land, we arrive at 50 per cent. as the unearned increment on landed property generally during the twenty years, equal to 42½ millions.

Now, turning to Schedules D and E, which give all the incomes from trades, professions, salaries, &c., we find an increase of 135 millions on 120 millions, or about 11.2 per cent. This looks at first sight as if the gain by the people had been much greater than that by the landlords, but a little consideration will show that it does nothing of the kind. For, first, of this 135 millions the above 42½ millions at least have been absorbed by the landlords in increased rents; and we must also take into account that tenants are subject to many charges – for renewals of leases, for repairs, for taxes on the landlord's property, for loss by game, and for improvements which the landlord confiscates, and which, together, make the payments to the landlords very much greater than they appear to be. Next, we have an enormous reduction from the apparent increase of income owing to the increase of rents of houses (as distinct from land), of rates and taxes, and in the cost of food and of wages. These items make probably a reduction of one-third in addition to the third paid in increased land rents; so that the effective increase in the average incomes of the classes enumerated above is only about 37½ per cent., or 45 millions. But this reduced amount is divided among a population greater by about 20 per cent., so that the real increase is less than 30 per cent. Yet, again, we have to remember that a large but unknown portion of this is drawn from foreign and colonial sources. Let us suppose this part to be only 10 millions, and we have left 35 millions of increased income during twenty years for the whole of the middle classes of the country, as the reward of greatly increased activity and knowledge and generally harder work. Contrast this with the 42½ millions increase of land values, absorbed by a very limited and already enormously wealthy class, who have done nothing but receive and spend it, and we shall be impressed with the truth which Mr. George so eloquently expounds, but which the father of political economy, Adam Smith, clearly perceived more than a century ago – that the landlords necessarily absorb most of the surplus wealth produced by an advancing community. In the "Wealth of Nations," at the end of Chap. XI. of Book I., he says: – "Every improvement in the circumstances of society tends, either directly or indirectly, to raise the real rent of land, to increase the real wealth of the landlord. . . . The real value of the landlord's share, his real command of the labour of other people, not only rises with the real value of the produce, but the proportion of his share to the whole produce rises with it."

In my critic's second paper (January 29) it is objected that I propose a perpetual quit rent to be paid by all future tenants to the State, and that this would practically transfer the

future unearned increment to these tenants. I thank the writer for pointing out this oversight in my book, though I cannot admit that the point was not foreseen and provided for. In the scheme of the Land Nationalization Society, drawn up by myself a year before my book was published, it is declared that all quit rents are to be "subject to revision (say once in a generation) to adjust any important changes in inherent value." This proviso has, I now find, been unfortunately and quite unintentionally omitted in my book. As to the further objection, that unless speculative prospective values of land are taken into account injustice will be done, I totally differ from the writer, though this is a matter of detail which may well be left till nationalization takes place. Such speculative values have no doubt been paid for in the past, but it is now generally admitted that the principle is unsound, especially when the prospective increase of value is an "unearned increment" due wholly to society at large. Such increments already realized must be recognized, because some simple principle must be acted on in order to avoid such tedious and costly investigations into every single case as would render any scheme of nationalization unworkable.

The objection as to houses depends on the assumption that there are hardly any houses already in existence. There would be really no more difficulty than there is now; and it is really absurd to suppose that society could not adapt itself to a condition of things which already largely prevails in other countries. With the lower rents produced by the absence of land monopoly and building speculation, every occupier of a house would be able to purchase it by a terminable rental no heavier than that which he now pays for a mere tenancy.

I will only remark, in conclusion, that your correspondent is mistaken in supposing that I believe that the persistence of poverty and the low condition on the labouring poor is "entirely due to the existence of private property in land." If he will turn to my "Land Nationalization," p. 17, he will find that I suggest two sources of the evil and two remedies; but I maintain that landlordism is the greatest and most fundamental evil, and that its abolition in the way I have advocated will greatly facilitate the removal of those evils which depend on the tyranny of capital, the helplessness of workmen, and the unequal distribution of wealth.[a] – Alfred R. Wallace.

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open letters to Alfred Marshall (S358a and S360a)

Wallace wrote a pair of letters directed to economist Alfred Marshall that were printed in The Western Daily Press (Bristol, U.K.) issues of 17 March and 23 March, 1883. Marshall's reply to Wallace's first letter, given the title "Progress and Poverty" in the 19 March issue of the Press, is added below, between them, and his second reply, with the same title, at the end. Wallace's first letter was read at a public lecture by Francis W. Newman the night before; it was printed in the Press as part of coverage of the event titled "The Nationalisation of the Land." Wallace's second letter was also titled "Progress and Poverty."

[a] Ten days later Wallace revised some calculations given here. See "Land Nationalization – A Correction," transcribed at http://people.wku.edu/charles.smith/wallace/S358AB.htm .

Wallace's first letter (S358a)

. . . "I note a fallacy in Professor Marshall's lectures on 'Progress and Poverty.' He endeavours to show that the condition of the labourer has greatly improved in the last century, by comparing wages at the two periods estimated in wheat. Now this is quite as fallacious as to estimate it in money, and is, in fact, no test at all. In the last century the bulk of the labourers lived in the country, and had cottages and some land in permanent tenure, with the use of commons and woodlands. They obtained a considerable portion of their income from the produce of their gardens, from pigs and poultry which they could keep. They had milk often free from the farmers; they had wood and turf free from commons and woodlands; and they used, to a considerable extent, rye or oats or barley bread instead of wheat. Their cottages, too, were often copyhold, or at mere nominal rents. Now, the bulk of the labourers are town-dwellers, with no land or common rights. Rents are high, and every scrap of food and fuel has to be bought, while cheaper bread than the finest wheat is not to be had, and thus beggars and paupers eat it, though it is dearer, less wholesome, and often less palatable than the old brown bread! Consequently the value of four pecks of wheat now, in wages, may leave a man worse off than the value of two pecks in the last century. Such a fallacy ought to have been exposed at once, but I cannot see that it was noticed. The political economists always ignore the difference of condition of labourers formerly as regards "use of land," when comparing wages, yet it is the essential thing. Another supposed error of George's was attacked by equal fallacies. Interest was said to be high and wages low in Asia – ignoring the fact that interest there includes enormous risk owing to plundering and bribable government, while wages are only low estimated in money, food and all that land produces being cheap, and fuel and house rent being usually nothing. I hope Professor Newman will expose these errors. Perhaps you will bring them to his notice."

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Marshall's first reply (printed 19 March 1883)

Gentlemen, – You publish to-day a letter from Mr Wallace, in which he brings two charges against my recent public lectures. The first is that I overlooked many advantages which the agricultural labourer enjoyed a hundred years ago. Now it happens that we have more detailed and trustworthy accounts of the diet, dress, and mode of living of the labourer at that time than at any other, with the exception of the last 30 years. I had re-read some of these accounts just before my lectures, and had carefully considered all the points to which Mr Wallace refers. A hundred years ago the labourer's common rights had already been much curtailed; philanthropists regretted that he could not afford to rent land on which to keep a cow; they did not propose that he should keep one on common land. His house rent averaged 7d a week in 1770 and 1s a week a little later on. Considering the vile accommodation that he had, this can hardly be called a nominal rent. Mr Wallace thinks he often had milk free from the farmers. No doubt skimmed milk was given away in some places when it was plentiful; but so it is now. There are good reasons for thinking that the amount of milk produced per head of the population was not much greater then than now; while the amount per head that was consumed without passing through the churn or the cheese vat was probably less than now. The farmer kept on an average three pigs on the produce of every ten cows, and this fact confirms the direct evidence of Eden and Arthur Young, that the labourer did not get very much even of skimmed milk.

Still, as I said in my lecture, milk was one of the very few things with regard to which he was in some cases better off than now. I agree with Mr Wallace that it is a pity that brown bread is not generally eaten now. But he is, I think, mistaken in supposing that it was largely eaten a hundred years ago. At that time only white wheaten bread was commonly eaten in the South of England, though in the North brown bread was sometimes eaten and porridge generally. Mr Wallace says that the labourer got his fuel very easily. But the fact is that wood had become so scarce that the labourer who was not near coal mines was often terribly pinched for fuel, the cost of inland carriage of coal being very high. The average of a vast mass of statistics collected by Arthur Young gives £1 3s 11d as the sum expended on firing by the labourer in 1770. But the supply he got for this price was so small that in order to save firing he went in the South of England almost entirely without warm food of any kind, except tea.

Mr Wallace's second attack relates to the rates of interest and wages in Asia. Mr George had said that it was a necessary and universal law that when wages are low, interest is low. I asserted that wherever capital is scarce and population abundant, interest will be high, though wages are low; and I said that this was the case in Asia. Of course, bad government has been one of the causes of the small supply of capital in Asia; and in some parts of Asia, though not in all, want of perfect security now makes it necessary to deduct a good deal of the insurance from the nominal rate of interest before finding the real rate. But that interest is really higher in Asia than in Europe is proved by the fact that when a railway has to be built there, it is cheaper to borrow the capital in Europe than on the spot. Again, when I say that wages are low in Asia, I mean of course, not only money wages, but real wages – *i.e.*, the food, clothing, and houseroom which the labourer obtains. Mr Wallace denies this, but I do not think your readers will expect me to prove it. Had I been wrong on all the points on which he attacks me, my main argument that the adoption of his scheme would injure the farmer and labourer as well as the landlord would have remained practically intact.

Perhaps you will allow me to take this opportunity of explaining a quotation from Mr Gladstone's Midlothian speeches that Mr Henry Rogers made after my last lecture. It was the only objection raised in the lecture room that I did not attempt to answer at the time. According to Mr Rogers, Mr Gladstone said that French peasant proprietorship had increased the earnings 40 per cent. in 14 years, while the English system has only increased it 20 per cent. in 30 years. I felt sure that Mr Gladstone's meaning had been misunderstood, but could not at the time say how. I now find that he is reported to have said at West Calder that "in 1842 the agricultural income of England was £42,000,000, and that in 1876 it was £52,000,000." But this sum includes no earnings, it is simply the rent of land. The agricultural income proper is the sum of the net incomes of all the agricultural classes, or, in other words, it is the sum of the values of all agricultural net produce. Mr Caird tells us that for the United Kingdom this amounts to about £260,000,000. The complaint of the land-nationalisers is that wages are kept down by the rapid rise of rents in England. Mr Gladstone's figures have so far the opposite tendency to that which Mr Rogers ascribes to them. But Mr Gladstone further said that the agricultural income of France (by which I suppose he meant the assessed rental value) rose from £75,000,000 in 1851 to £106,000,000 in 1864. I should like to offer an explanation of this. We have Lavergne's very careful statistics as to 1847. At that time rents were 25s an acre in England, and 10s an acre in France; that is, for lands of equal natural fertility they were probably in France about a third of what they were in England. The great gold discoveries were made about 1850, and from that time to 1864 there was a vast rise in prices. Meanwhile the Imperial Government had restored the security which was shaken in 1848–52; and this, of course, specially enhanced the value of land. But it was a very expensive Government, and according to general report it took every opportunity of screwing up assessments. Lastly, the free trade measures of 1860 had immensely increased the export of wine and the value

of French vineyards. Under these circumstances the land system must indeed have been bad if it had prevented the assessed rental value from rising rapidly. Probably the value of the land in France will go on rising more rapidly than here; for it is still far behind and has therefore more room for improvement, and America is a market for and not a rival to French vineyards. No one doubts that the French peasant works hard and is thrifty, but I believe that with less work the English labourer is generally better fed, clothed, and housed, and that with equal thrift he would soon become richer. I do not contend that the English system is well adapted to the French character. The fact that their wheat crops are less than half as much per acre as ours is chiefly due to the fact that wheat is a large farm crop, and that French large farms are often badly managed. – Yours, &c., Alfred Marshall, University College, Bristol, 17th March.

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Wallace's second letter (S360a)

Gentlemen, – My objection to Professor Marshall's estimate of the comparative condition of the agricultural labourer now and in the last century was that he founded it wholly on wages estimated in wheat, and made no allowance for the labourer's different relation to the land then and now. In his letter in your columns on the 20th inst., he says that I have overestimated the advantages the labourer formerly possessed, but he admits that these advantages did exist to some extent, and it follows that to that extent his estimate was misleading. I have not the extensive knowledge of the subject which Professor Marshall no doubt possesses, but I would ask permission to point out that other good authorities do not hold his opinions on this question. In the report of the Women's and Children's Employment Commission (1868), paragraph 251, it is stated that: – "Previous to 1775 the agricultural labourer was in a most prosperous condition. His wages gave him a great command over the necessaries of life; his rent was lower, his wearing apparel cheaper, his shoes cheaper, his living cheaper, than formerly; and he had on the commons and wastes liberty of cutting furze for fuel, with the chance of getting a little land, and, in time, a small farm." Mr Brodrick, too, in his "English Land and English Landlords," speaking of those few benevolent landlords who let their labourers have plots of land of from two and a-half to three and a-half acres, with their cottages, at an ordinary farm rent, the results of which are eminently beneficial, adds: "This practice, after all, is but the revival of a custom once almost universal among the peasantry of England, and it is found to be fraught with manifold advantages. The most obvious of these is an abundant supply of milk for the farm labourer's children, who in many districts grow up without tasting the natural diet of childhood." Mr Brodrick also agrees with me in my main contention, for in reference to this very question of wages as estimated by Malthus and Arthur Young in the last century, he remarks: "But the value of a labourer's wages is not to be measured by the price of bread alone." I think, therefore, that my objections to Professor Marshall's estimate of the comparative condition of the labourer at different periods by wheat-wages alone are fully justified.

As to the second point, I will remark that, in the part of Asia I am personally acquainted with, at all events, wages, though low in money are really, in relation to purchasing power and habits of life, very much higher than in Europe, since they furnish the labourer with all the necessaries and many of the luxuries of existence in return for a very easy day's work; and from what I have read of other parts of Asia, I believe this

statement will very generally apply. Interest, therefore, may be higher without being higher in proportion to wages. – Yours, &c., Alfred R. Wallace.

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Marshall's second reply (printed 24 March 1883)

Gentlemen, – Mr Wallace does not understand my position with regard to wages a hundred years ago. In my lecture I admitted that a peck of wheat would purchase more animal food and more of a few other things a hundred years ago than now; and I admitted that he still retained some fragments of privileges which he has now lost. But I contended that a peck of wheat will now buy many things of great importance for the physical, mental, and moral wellbeing of the labourer and his family, which it would have cost him very many pecks of wheat to purchase a hundred years ago. I concluded that his real income has risen in at least as great a ratio as his wages measured in wheat have. Mr Wallace has looked only at one side of the shield, and even in this he has pointed out nothing that I had not taken account of. The quotations he now gives do not appear to traverse my statements. It is not necessary for me to inquire in how narrow a sense the term peasantry is to be interpreted in Mr Brodrick's statement that it was once an almost universal custom among the peasantry to rent two or three acres of ground. The custom had disappeared a hundred years ago nearly as completely as it has now.

There is no reason, in our present land system, to prevent its being revived now. Mr Wallace cannot desire its revival more heartily than I do.

Again, he mistakes my point with regard to wages in Asia. It is true that while the English labourer has not enough clothes, the South Sea Islander has as many as he wants, because he wants scarcely any. No doubt those who desire a more animal existence can have it for very little labour in a tropical climate where population is sparse. But the economists whom Mr George assails use the term real wages to mean the amount of food, clothing, houseroom, and other necessaries, comforts, and luxuries of life which the money wages will purchase. Using it in this sense, I am not contradicted by Mr Wallace when I assert that wages in India and China are lower than in England, while interest, allowing for risk, is higher. – Yours, &c., Alfred Marshall.

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Lord Jersey on Land Nationalization (S366)

A letter to the Editor printed in The Times (London), issue of 30 October 1883.

Sir, – Perhaps you will allow me very briefly to reply to some statements in a paragraph headed as above in *The Times* of Friday last.

Lord Jersey is there reported to have said: – "The State would be an unfeeling landlord, holdings would be put up to competition, with the result that rents would rise very much and hardships be created. If they did not have competition they must have favouritism – the latter would mean jobbery, the former high rents." This seems a very pretty dilemma; but surely Lord Jersey cannot already have forgotten the Irish Land Act, and that it is possible for the State to fix "fair rents" by valuation? If this were done in England, and if the occupiers of land were obliged to become the owners of the

"improvements" on their farms, the State being owner only of the "bare land," for which it would receive a fixed quit-rent, then there need be no "competition" and no "favouritism," for farms would pass from hand to hand by sale of the "improvements," just as they often did in Ireland under the old *regime* by sale of the "tenant-right;" and the State would have no voice in the matter, but would collect its quit-rent from the new occupier.

Lord Jersey then proceeds to express the opinion that "it would be a good thing for the country if those who tilled the land could feel, when they had a few spare hours, that they could supplement their wages by cultivating some land for themselves." This is undoubtedly true, and its good effects have been again and again proved by actual experiment. But even this would be comparatively useless unless it were accompanied by "fixity of tenure" at "fair rents," with complete freedom to dwell upon the land as well as to cultivate it. Under these conditions alone would the benefits of the system by fully realised; and these benefits are so great, tending surely to the extinction of rural pauperism and drawing back from the towns some of those crowded masses who now live there in a state of the most degrading and hopeless misery, that we cannot afford to leave so important a process to the slow and altogether uncertain action of local effort, subject always to the permission, or refusal, of landowners, who, be it remembered might, had it so pleased them, have done this "good thing" themselves, but who, except in a very few cases, have done nothing, or worse than nothing.

We who advocate land nationalization would make this "good thing" universal by giving to every man, be he poor or rich, the right to the permanent occupation of a plot of land to live and labour on. This is the one thing needed to make our agricultural labourers the strength and the safety, instead of, as they now are, the weakness and the danger of England while it offers the only effectual means of relieving the fearful pressure of population in great towns, which is the true cause of that "housing-the-poor" difficulty now again exciting attention – a difficulty which, with free access for all to rural land, will gradually but surely cease to exist. – I remain, Sir, your obedient servant, Alfred R. Wallace, President Land Nationalization Society, Godalming.

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Mr. S. Smith, M.P. on Land Nationalisation (S368a)

A letter to the Editor originally printed in the 4 December 1883 issue of the Liverpool Daily Post. The source of this transcription is a reprint of the letter added as an Appendix to a pamphlet by Samuel Smith entitled The Nationalisation of the Land, released in 1884.

Sir, I trust you will grant me a little of your space to make a few observations on the lecture by Mr. S. Smith, M.P. so fully reported in your paper of Tuesday, because it is right that your readers should be informed that the bulk of the lecturer's criticism does not apply to English land nationalisers, whose contentions and proposals were passed over with the most studious silence. Surely it is a very remarkable and suggestive fact that a member of the British Parliament, lecturing in one of our chief cities on the applicability of land nationalisation to England, should yet not say one single word about the English Land Nationalisation Society or the proposals it makes, but should confine himself strictly

to the proposals of an American for land nationalisation in America. Before Mr. George's remarkable book was noticed by a single English review, and while it was still unknown to the vast majority of English readers, an article on land nationalisation appeared in the *Contemporary Review* for November, 1880. This article led to the foundation of a Land Nationalisation Society, and was followed by the publication of a small volume on "Land Nationalisation: its Necessity and its Aims," in which the large body of facts on which the society founds its proposals are set forth. Only a few months back Professor Fawcett's article on "State Socialism and the Nationalisation of the Land" was answered by myself in two successive issues of *Macmillan's Magazine*. I mention these facts because they raise the question of Mr. Smith's competence to treat the subject at all. He appears to be completely ignorant that any such book, articles or society exists, for he says, speaking of the Trades Congress, "I much doubt if they or any of their sympathisers in this country have clearly thought out the subject, or perfected any plan for the acquisition of the soil or its cultivation after it was acquired." Now, this is exactly what we have done; and yet, in total ignorance of our proposals, our facts and our arguments – for "I much doubt" surely implies ignorance – Mr. Smith comes forward to instruct the people of Liverpool. If he is not ignorant of the facts I have stated, still less reason have his audience to be satisfied that the chief English proposals for land nationalisation should be purposely ignored, while they were led to believe that those of the American writer were universally adopted by English nationalisers.

I will now briefly reply to a few of Mr. Smith's statements from the standpoint of the Land Nationalisation Society.

1. Mr. Smith says that "human misery is deepest where the land is not appropriated, and human happiness and civilisation most advanced where the land is held by private owners." This assertion I directly contradict. There is no such connection as alleged, but rather the contrary, if we eliminate such factors as ignorance, barbarism and bad government, and compare only countries which are fairly comparable. I will give two examples which sufficiently demonstrate the incorrectness of Mr. Smith's generalisation. In a very remarkable article in *La Nouvelle Revue* (15th March, 1883) on "La Famille Chinoise," it is stated that the land of China is really national, every one holding it from the State, and paying a fixed rent to the State. Holdings are small, the average being seven acres, while estates of more than 200 acres are exceedingly rare. Every family also holds a small portion of "patrimonial land," which is invaluable and inviolable. The result of this excellent system, says the writer, is that every hamlet forms a complete community, where the inhabitants find their school, their guildhall, their court of justice. In these hamlets each home is independent, yet all are bound together by the ties of relationship, and all assist each other in the various troubles and labours of life. The writer dwells at some length on the peace and contentment, the simplicity, and the happiness of Chinese village life under this beneficent land system, and this, be it remembered, in spite of a very imperfect civilisation and a despotic Government. The next case I will quote is that of Switzerland, where the old system of communal land still largely prevails, and where its influence is felt even in the districts where it has been abolished. Here we have at once the freest, the best educated, and the most really civilised people in the globe, if we measure civilisation, not by the height reached by the few, or the luxury and refinement of the rich, but by the general well-being, intelligence and contentment of the great majority of the people. In Switzerland landlords and landlordism, as we understand them, are

almost unknown; and in Switzerland pauperism, famine and social degradation are almost equally wanting.

2. Mr. Smith very justly says that none but a dreamer would seriously impugn titles to land because Alaric or William the Conqueror acted unjustly; but he omits to notice the much more important fact that possession of land, except so far as it is personally occupied, never can arise otherwise than by force or fraud. Take any plot of land you like in Great Britain, and if you trace its history far enough you inevitably come to an owner who obtained it by force or fraud. There is no other way in which land can be obtained, except in the case of a piece of land cultivated by its owner in unbroken continuity from the time it was first enclosed; and almost the only land thus held in England is by some of the squatters on our commons and wastes. There is no other form of property whatever which inevitably has its origin in wrongdoing, and this alone goes far to prove that such property cannot be good for the community.

3. In reply to the argument that land should not be private property because it is limited in quantity, is essential to human existence, and is not producible by human labour, Mr. Smith asserts that "the productiveness of the soil is mainly the result of ages of careful cultivation." This is simply not true, since the productiveness mainly depends on the physical characters of the soil and subsoil; but even were it true, it would prove that the land belonged to the successive cultivators, not to the landlords, who, as a rule, never cultivate: that is, the land should belong to the whole people whose ancestors from time immemorial have given it its "main value."

4. Another gross misstatement is, that "most other kinds of property" as well as land increases in value with increase of population and wealth. The very reverse is the case. Broadly speaking, all property except land is destructible, and more or less rapidly deteriorates in value; the few apparent exceptions, as old pictures and books, and our public funds, do so because they are in the nature of monopolies. The funds, too, are not property, but debt, and they rise in value merely because the payment of interest on no other debt is guaranteed by the State. "House property," Mr. Smith, with a strange confusion of ideas, declares to increase in value! But surely he knows that it is the land that increases, while the house upon it steadily deteriorates in value, and has to be kept up by an ever-increasing outlay in repairs.

5. In answer to Mr. George's proof that landlordism keeps down wages to the minimum necessary to sustain life, Mr. Smith adduces the oft-exploded fallacy of the rise of wages in most trades; but he ignores the facts that house rents and the prices of meat, butter, eggs and milk have risen in a far greater ratio, and that labourers, on the average, have to work as hard and have as much difficulty in earning a bare subsistence as ever they had, while they have been creating an enormous increase of wealth and luxury for all the classes above them. For the proof of this fact, and of the probable increase of pauperism and misery – notwithstanding official statistics – I must refer your readers to my article in *Macmillan's Magazine.*

6. If Mr. Smith had examined our English proposals for land nationalisation, and not those of an American, he would have seen that we do not consider the transference of the rents of land to the State to be the only or even the most important benefit to be obtained. The most vital point is that all English people who wish it shall have the use on equal terms of some portion of English land, and that the fruits of every hour's labour upon the land shall belong to the labourer. In order that labourers may not be forced to compete for

any wages that will keep them and their families from starving, or from being turned homeless from the cottage they occupy at a weekly rent, we would provide that every man shall have the opportunity of acquiring a plot of land direct from the State, on which he may live, and from which he can never be ejected so long as he pays the rent of the land at its fair value. Every village and country town would then grow, in all the natural development of rural life; our country would be soon dotted over with groups of cottages, gardens and small farms; such rural produce as milk, butter, eggs, cheese, poultry and bacon would be produced and consumed on the spot, instead of being imported from a score of foreign countries, while our labourers are crowded into the slums of great cities simply and solely because landlords will not let them live in the country. Millions of acres, now neglected and almost worthless pasture, could and would be cultivated like a garden, only allow the labourer to have it on the same terms as the farmer, with absolute security of tenure, and every one of these cultivators would not only help to diminish the intensity of the struggle for existence in towns, but would spend their gains almost wholly on home manufactures, and thus create a demand for labour in all the industries of the country.

7. Mr. Smith then adduces Professor Fawcett's argument against the possibility of the State acquiring the land by purchase; but he knows nothing of our proposal to allow the existing landlords and their living heirs to continue to enjoy their present net incomes, while at once taking the land for the use of the people; and declaring that no unborn person shall inherit any portion of the national land. This disposes of the terrific picture he draws of widows and orphans beggared by confiscation. Such has been the result of the Irish land legislation, but by our scheme no living person would suffer.

8. Finally, Mr. Smith admits that perhaps the State ought to aid labourers to buy their cottages and gardens, which he says would be an "immense boon." He declares that Highland landlords should not be allowed to shut out tourists; that village commons should not be enclosed; that the rights of landlords "should not be allowed to override the necessities of life for the toiling masses of the country;" that the State "shall give a fair chance to every one, and free play to all the powers and capacities of its citizens;" and other such suggestions. But every one of these things would be done once and for all by our system of land nationalisation, without costing the nation – that is, the taxpayers – one penny; while all of them are so completely opposed to "the rights of property," as they are now interpreted, that so long as those rights exist each detail of reform will be fought against by the whole power of the landlords. In the meantime all the evils of a pauperised community, depopulated villages, and "horrible cities" must go on and increase, notwithstanding our frantic efforts to ameliorate the outward symptoms, so long as the fundamental cause – private property in land – remains. I would ask your readers to ponder on the facts stated by the chairman, Sir James A. Picton, and then say whether a system which permits such things can be longer permitted to exist. Our public writers are never tired of assuring us that "property has its duties as well as its rights," but those duties are neither defined nor enforced either by equity or by public opinion, as shown by the continuous confiscation of tenants' property by hundreds of Irish landlords, and the wholesale misery and death caused by evictions in Ireland and the Highlands, without a single example of the prime cause of such horrors – the landlord – even suffering in reputation or social position. In the present day in Great Britain the great landlords have, as a matter of fact, no duties, while their power for evil is practically unlimited. I appeal

to the records of Ireland and the Highlands to bear out this assertion. Such power is inconsistent with freedom and national well-being, and as it is inherent in the system of landlordism, that system must be abolished. – Yours, &c., Alfred R. Wallace. Godalming.

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Mr. [Henry] George's Theories (S369: 1884)

A letter printed in The Times (London) issue of 29 January 1884.

The writer of the review of Mr. George's "Social Problems" in *The Times* of Wednesday last comments with some show of justice on the deficiency of proof of his fundamental position of "increasing want with increasing wealth," and remarks: – "Yet this is what Mr. George really needs to prove. Any one can see that rich men constantly get richer, but assuredly it does not necessarily follow that poor men get, not merely comparatively and relatively, but absolutely poorer."

Now, although I think Mr. George has proved this fundamental position fairly well, yet he has done so in a very discursive manner, and chiefly by illustrations and general historical comparisons; whereas there are other and very powerful arguments which he has altogether omitted. Although to develop these fully would require an elaborate essay, yet their general nature can be briefly set forth, and as the subject is one of the greatest possible interest, I ask permission to state them in *The Times*, and to show that, when rich men constantly get richer, poor men necessarily get poorer.

Let us suppose, to begin with, a society in which all men are engaged in productive work and there is no great difference of condition. Much wealth would be produced, and, being distributed by free exchange, there would be no poverty and no exceptional riches. But under our present laws and customs inequality would very soon arise, and after a time we should find a certain proportion of rich men who have no need to work and who do not work. Let this proportion at first be small – say, 2 per cent. of the whole population: and the first effect evidently is that the productive labour formerly done by the whole is now done by 98 per cent. of the population, and to that extent each has to work harder. But that is only a very small part of the effect produced; for the rich men, just in proportion to their riches and to increasing luxury, divert labour from productive to unproductive channels. Not only do they employ large numbers of personal servants, but they keep a host of men employed in producing luxuries who before produced necessaries and comforts for the whole population. Showy dress and furniture, horses and carriages, feasting, entertainments, and all the varied forms of wasting money on useless trifles which arise with superfluous wealth, employ an army of labourers who are absolutely unproductive as regards the rest of the community. If in these various ways each rich man on the average employs only eight men in his service, then we have one tenth of the population practically idle; and all the food, clothing, and comforts they require have to be produced by the remaining 90 per cent., who will therefore have to work harder. Now, as wealth increases, not only does the number of those who live in luxury increase, but the amount of individual wealth increases, till millionaires, at first rare phenomena, become common as they are now. With this increase, therefore, the number of those employed unproductively in ministering to luxury increases still more rapidly, because the very servants and depend-

ants of the rich are now themselves rich and live in luxury. Hence the proportion of unproductive to productive labour increases continually, which means in other words that the productive labourers have to work harder than ever and for longer hours, and this they would certainly do only under stress of necessity – that is, of poverty. Poverty, therefore, increases with increasing wealth; and this result is contained in the following passage from Adam Smith, Book II., chap. 3: – "Both productive and unproductive labourers, and those who do not labour at all, are all equally maintained by the annual produce of the land and labour of the country. Accordingly, therefore, as a smaller or greater proportion of it is employed in maintaining unproductive hands, the more in the one case, and the less in the other, will remain for the productive."

The same general result may be reached through another principle laid down by Adam Smith – that wealth is really command over labour or the power of purchasing labour to a practically unlimited extent. It follows that great wealth can only arise when numbers of men are forced to labour by their necessities – that is, when they are poor; and the increase of wealth necessarily implies the increase of those who are obliged to work and create that wealth, not for themselves, but for others.

Combining these two arguments, we see that if wealth goes on continually increasing, as it is admitted that it does, then poverty must go on also increasing, absolutely as well as relatively, because a smaller and smaller proportion of the population have to do the productive work of the whole population, and they can only be forced to do this by the pressure of poverty. There will be a steady progress from a period when all worked and none were either rich or poor – as in all newly-settled countries – to one where half the population are either rich or engaged in ministering to the rich, and the other half forced by poverty to continuous labour, and each step of the progress must intensify the difference. It is therefore almost equivalent to a contradiction in terms to maintain that poverty can be diminished while individual wealth goes on increasing.

I feel only too painfully that I have not put this argument so clearly and convincingly as it might be put; but I feel sure nevertheless that economists will see that it expresses a fundamental truth.

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The Morality of Interest – The Tyranny of Capital (S370)

A long letter published in the March 1884 issue of the London magazine The Christian Socialist.

Having read Professor Newman's[a] defence of interest and your remarks thereon, I wish to make a few observations on the general question.

Your position, and also that of Mr. [John] Ruskin, appears to be that money should be lent only as an act of benevolence or charity, and that lending it any other way is not only, in most cases economically and socially, injurious, but is also morally wrong. With the first part of this proposition I am very much inclined to agree, but not with the second. Looked at broadly, I believe that the *power* of obtaining interest on capital, however great,

[a] Francis William Newman (1805–1897), English scholar and writer.

with the corresponding *desire* of the owner of capital to obtain interest on it, is, next to the private monopoly of land, the great cause of the poverty and famine that prevail in all the most advanced and most wealthy communities. To prove this would occupy too much space; but I may just notice that bankruptcies, with the widespread misery they inflict – the speculations of promoters and financiers often bringing ruin on hundreds or thousands of deluded investors – and the vast loans to foreign despots, which can only be paid by the sweat and blood of their unfortunate subjects, are the direct, and in the present state of society, the necessary results of the interest-system. Professor Newman says that if it were to cease, business would be lessened by one-third. But only rotten and speculative business would be stopped; commercial men would then *be* what they now only appear to be, and no really necessary business would cease to be carried on. The late William Chambers has stated (in his "Life of Robert Chambers") that their vast book-selling, printing, and publishing business was established and carried on from first to last without one penny of borrowed capital; and that, as a result, panics and financial crises which brought ruin to some of their competitors, only caused them a little temporary inconvenience. I believe, therefore, that it would be for the benefit of the community if loans of money, or advances of goods on credit, were not recognised by the law, but were made wholly at the risk of the lender; but I do not see that it follows that he who lends, even under these circumstances, and takes interest for his loan, is doing what is wrong. For I cannot perceive any essential difference in principle between lending on interest, and selling at a profit. If I buy a shipload of drugs or any other goods at wholesale price, warehouse them, and sell them in the course of a year at the current market rate, making a profit of, say, 15 per cent. on my money, am I doing that which is morally wrong? Of the amount gained by me, we may put perhaps 1 or 2 per cent. for my personal trouble in the matter, 2 or 3 per cent. for risk of loss, 5 per cent. for interest on capital, and the other 5 per cent. for surplus profit. Is this 10 per cent. illegitimate gain? and am I morally bound to sell my goods at so much below the market rate as to leave me only fair payment for my time and risk? If it is wrong to take interest for the money which, when lent to another man enables him to do this, surely it is wrong to take a larger share in the shape of profit; and this really means that all trade is immoral which returns more than payment for personal labour, and insurance of the capital employed. But if so, it should be so stated, and the question should not be confined to interest on money loans only, and, in fact, Mr. Ruskin does not so confine it. The quotation you make from Mr. Ruskin does not, however, seem to me at all to the point. You freely lend your friend an umbrella in his need, and you would even do the same to the merest acquaintance or neighbour, but if your neighbour called every day for your umbrella on his way to the City, and other neighbours followed his example, so that you ceased to have the use of your own umbrellas, you would soon have either to refuse to lend, or to charge a rent for the use of them, and if this were convenient to your neighbours, and they were willing to pay you sufficient to cover the wear and tear of umbrellas, your time and trouble in looking after them, and interest on your capital invested in them, it will require arguments very different from any yet advanced to satisfy me that you would be morally wrong in doing so. In like manner, though you lend your friend or neighbour cab-money, or give him a bed for a night on rare occasions when he urgently requires such aid, you would give none of these things repeatedly to a mere acquaintance. Yet, if circumstances rendered such accommodation very useful to a considerable number of persons, and you or someone else found it profitable to supply such accommodation, you would charge rent for your beds, and interest for your

loans, and the transaction would differ nothing in principle from that of every tradesman who sells goods at a profit, of the innkeeper who charges beds in his bill, or of the jobmaster who charges for the use of his horses or his carriages. Nothing deserving the name of proof has yet been given that either of these things are immoral. Whether it is a good and healthy state of society, in which large numbers of persons get their living by such means, is another matter altogether.

The difference of opinion on this question of usury arises mainly from the different standpoints of the disputants. Seeing that it is bound up with many of the evils of modern society, and believing that it should have no place in a system of true Socialism, you and Mr. Ruskin denounce it as immoral. Professor Newman, on the other hand, looks at it as a question of modern society, and finds nothing *in its essential nature* contrary to justice, and here he seems to me to have the best of the argument. No doubt, in a more perfect state of society, in which private accumulations of capital were comparatively small, and the land and its products were freely open to the use of all, usury would have little place, because loans of money would rarely be needed; but when they were needed, I cannot see any grounds for maintaining that it would be morally wrong to lend money on interest. On the contrary, such loans would then retain their use without the evils their wide extension now brings. There would be no great capitalists, and if one man lent to another it would be a convenience to the borrower, and certainly some loss to the lender, because, as Professor Newman well puts it, £100 paid a-year in ten years hence is *not* as valuable as £100 paid to-day. To say that it is so is really to say that it has *no value* to-day, for if its payment can be delayed one year without loss it can two, or three, or ten, or a hundred, or a thousand! Where are we to stop? If we suppose a perfect social state, we suppose all men to be producers, and as capital is an aid to production, no man can give up the use of his capital to another without loss. The true solution to the problem is, I believe, to be found in the proposition that all loans should be *personal*, and, therefore, *temporary*; and that, as a corollary, the repayment of the capital should be provided for in the annual payments agreed to be made by the borrower, either for a fixed period (if he live so long), or for the term of his life. This would abolish the idea of perpetual interest, which is as impossible in fact as it is wrong in principle, while it would avoid the injustice of compelling one man, or set of men, to pay the debts of a preceding generation from which they may have received no real benefit.

This question of interest thus becomes involved in the wider question of the tyranny of capital over labour and its remedy. At present civilised Governments act on the presumption that great accumulations of capital are beneficial, and even necessary, to the well-being of the community, and all legislation favours such accumulations. When the people are once convinced that the reverse is the case, and legislation is directed to favour small holders of capital, and to check its inordinate accumulation, most of the evils complained of will cease. To this end the first step would be to get rid of all Government funds, guaranteed loans, railway stocks, &c., which are the main agents and tools by which capital is accumulated and money is made to breed money. This could be done in every case by making such stocks non-transferable after a certain date, and then declaring the payments to be terminable at the death of the holders and their living heirs, just as I propose to do in the case of landlords. The railways should be taken by the State, existing shareholders receiving annuities of the amount of their average dividends, payable in like manner to themselves and their living heirs. The Limited Liability Act should be repealed, because it has served only to foster the worst and most iniquitous speculations,

and has deluded the public into the idea that they could safely share in the profits of commercial enterprises of the nature and management of which they are profoundly ignorant. There would remain no safe investments for money, except in some branch of agriculture, manufactures, or commerce in which either the investor or some relation or friend was personally interested, and thus would be brought about the diminutions and practical abolition of usury as a system, and of whole classes living idle lives on the interest of money derived from the accumulations of previous generations. Of course, it will be said that the plan here proposed is wholesale confiscation and repudiation; but a little consideration will show that it is nothing of the kind, and that it is really the best thing that can happen even to the individual holders of the stocks dealt with. In the case of the National Debt, for example, fundholders are now threatened with a reduction of interest of a quarter per cent., and later on of a half per cent.; and *they will be forced to accept it*, because the interest on the public debt regulates that of all other good investments, which will inevitably rise in price enormously if any considerable portion of the amount now invested in the funds seeks other investments. The offer to pay off fundholders at par will, therefore, be illusory, and the vast class who *live* upon their dividends will inevitably have their incomes reduced one-twelfth or one-sixth, while the cost of living goes on continually increasing. Would they not be far better off to have their present incomes secured to themselves and their living heirs? And when they fully realise their position, will they not choose the latter alternative if offered them? If the series of changes here sketched out were effected, the reign of capital as the tyrant and enemy of labour would be at an end. When the tools with which the financier and the speculator work no longer exist, the piling up of great fortunes will be impossible, and much personal care and attention will be required in order to make capital produce a steady return. Industry and commerce will be the sole means of acquiring wealth, and by these means alone – *under the new conditions of society* – very great wealth can never be accumulated by one man. For the land being nationalised, and the use of some portion of it obtainable by all, the minimum of wages will rise far above the starvation point which now prevails, and every village or other community, however small, will consist of small capitalists, who will be ever ready to unite for the safe employment of their capital. Then will arise a variety of industries on a scale adapted to the size and wealth of the district, and calculated to utilise the surplus labour and spare time of the surrounding population; and these small industries will compete successfully with the establishments of individual capitalists, because they will have an ample and a cheap supply of labour, and because most of their labourers, or their relations, will be shareholders, and will thus be working for themselves. The individual capitalist will then find himself paralysed for want of labour, unless he offers great temptations in the form of high wages and participation in the profits. For when a large proportion of the population are settled upon the land, and are able to devote their savings and their spare time to local industries, they will not, as now, be forced to become parts of a huge manufacturing machine in the success of which they have little personal interest.

By the methods here sketched out the labourer will receive, as Karl Marx and other social reformers maintain that he should do, the whole produce of his labour, and he will obtain this general result without any aid from Government, except what consists in remedying injustice, and removing the restrictions on freedom which now hamper him. Without any laws against usury, usury will practically cease to exist. Without any direct restrictions on wealth, those vast and injurious accumulations of wealth which now

prevail will be impossible. The "stealers" and the "beggars" who now, as Mr. Girdlestone has shown,[a] are so numerous among us, will steadily give place to "workers," and just in proportion as that happens, poverty will diminish, and will ultimately disappear. Now, a large portion of the working population are employed in the production of useless and often tasteless luxuries and trifles, the direct consequence of the large number of persons who have surplus money to spend after all their reasonable wants and comforts are fully satisfied. It is this, much more than the mere number of idle people, that is the dead weight which keeps thousands starving in the midst of so much wealth. When mere extravagant luxuries are less in demand great masses of labourers will be set free to produce the necessaries and comforts of life; these will be more abundant and cheaper (whatever their money price may be), and if all those who are now idle aid in the production of these necessaries and comforts, it is evident that, with free exchange, none can want.

I would particularly call attention to the fact that the results here indicated would all be brought about by carrying out the true system of *laissez-faire* now so much abused as if it had failed, when really it has never been tried. Labour, the sole source of all wealth and well-being, has been fettered in all her limbs, and harassed in all her actions, and then because she often stumbles or faints by the way, they cry, "See, she cannot do without help!" But first unloose your bonds, and cease to hamper her with your legal meshes, and then see if she will not achieve a glorious success. Let Government do its duty, and no more. Let it secure peace from external foes, and safety from internal violence; let it give free and speedy justice between man and man; let it secure to all alike free access to the land and all natural powers; let it abolish every monopoly of individuals and classes – either the local or central authority having the management of all institutions or industries which are essential to the public welfare, but which in private hands tend to become monopolies; and let it enact that all debts contracted by individuals shall be payable by those individuals only, and those contracted by the municipality or the State be payable by the generation which contracts them, so that they may never remain a burden on the succeeding generation. When it has done all this, then alone will labour be really free, and, being free, it will work out the well-being of the whole community without any Government interference whatever. This is the true *laissez-faire*; and this, I believe, will enable us to realise the best social state which, *in its present phase of development*, humanity is capable of. The distant future will take care of itself; let us try to improve the future that is immediately before us. I have here very briefly and imperfectly sketched out a series of measures which I believe are best calculated to promote this object, and they have the great and inestimable advantage that they all tend to the diminution of governmental interference with labour and industry, instead of that indefinite increase of it which the German Socialists advocate, and which, as the greatest political thinkers maintain, and as all experience shows, must inevitably fail, while in the present condition of civilisation it will probably lead to evils not less grave than those it attempts to cure.

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[a] Presumably, in his tract *Society Classified*.

Mr. Auberon Herbert on Land Prophets (S376aa)

A sharp letter on the subject of property, printed in the Pall Mall Gazette issue of 21 March 1885.

Sir, – Will you allow me to reply to the question Mr. Herbert has asked me in your issue of Tuesday last? He wishes to know if I will submit my house, furniture, books, &c., to be taken or rented, at a valuation, by anybody who wishes to do so. I reply that I would certainly not propose legislation for this to be done either to myself or other people, and I will remind Mr. Herbert that I have never proposed that land, personally occupied by the owner, should be taken from anybody, even though it be a thousand acres of park occupied by a duke. My house, furniture, and books are manufactured goods, the labour to produce which was paid for, and others of like nature can be produced and can be obtained whenever demanded. But land was not manufactured, was never originally paid for, but always either stolen or otherwise appropriated. Land is the source of all existence and all wealth; without it neither houses, furniture, nor books can be produced; but these things are not absolute necessities of existence or essential sources of wealth, and they are not limited in quantity as land is.

Mr. Herbert's whole argument (so far as he adduces any argument) is that land is, and ought to be, absolute private property, like any other articles. It is, in his view, a right and good thing for one man to hold a hundred thousand acres, and limit its use as he pleases. It is right that a man should have the power to turn thousands of people out of their homes at his pleasure. The two million acres of deer forests in Scotland must, on this theory, not only be let alone, but allowed to grow to four millions, if English and American millionaires bid higher for them than those who have been born on the land, and whose ancestors defended it with their blood. It was right and proper that the inhabitants of the village mentioned by Mr. Froude,[a] whose forefathers had lived in it since the Conquest, should have been all cleared away at the whim of a duke or a duke's agents. It is right that the tenants' improvements both in Ireland and England should be confiscated by the landlord, and that nobody should live in his native land except by permission of a limited body who hold the soil, and on any terms they may choose to dictate. Every word of Mr. Herbert's arguments would apply with equal force to defend the territorial rights of the French nobles which brought on the Revolution – which was evidently a wicked attempt to plunder other people's property and to prevent landowners from doing what they liked with their own, unhappily too successful! Nay, more, every argument will equally apply in favour of slavery; the Abolitionists wanted "to take away other people's property," and to prevent people from doing what they liked with that which they had legally bought and paid for.

Mr. Herbert may be assured that such arguments and ridicule as his will help the supporters of Mr. George as much as the action of the American, Mr. Winans; and that the people of England will not much longer consent to hold their very lives at the pleasure of a body of men whose only claim to the power they possess is that it has come down to them by inheritance or purchase from those who once took it by force and have misused it ever since. Let our distressed agriculture, our depopulated fields, our overcrowded towns,

[a] James Froude (1818–1894), English historian.

and our pauperized labourers bear witness. – I am, Sir, your obedient servant, Alfred R. Wallace.

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Illegal Roadside Enclosures (S383aa)

An unusual letter to the Editor printed in The Daily News (London) issue of 24 September 1885.

Sir, – If your correspondent A. B. W. has correctly described the enclosure at Boreham Wood, Herts, there can be no doubt that it is illegal, since the question has been decided by Baron Martin, in the case of "The Queen v. The United Kingdom Electric Telegraph Company," tried at the Bucks Spring Assizes in 1862, where the law as to roadside strips was laid down in the following terms: – "In the case of an ordinary and varying highway, although it may be of unequal width, running between fences, one on each side, the right of passage on such highway, primâ facie, and unless there is evidence to the contrary, extends to the whole space between the fences; the public are entitled to the use of the entire of it as a highway, and are not confined to the part which may be metalled and kept in order for the more convenient use of carriages and foot passengers." This ruling of the judge was appealed against, and a new trial was moved for on the ground of misdirection, but it was refused, and the ruling of Baron Martin affirmed by Cockburn, C. J., Crompton and Blackburn, J. J., in the Court of Exchequer Chamber. (See Best and Smith's Queen's Bench Reports, vol. ii., page 647.)

Attention should be particularly directed to the fact that by this authoritative declaration of the law, confirmed by a court of appeal, all roadside strips "between hedges" are declared to be parts of the highway "primâ facie, and unless there be evidence to the contrary." Whenever such roadside strips are enclosed it rests on the encloser to first prove his right to the land, the primâ facie right being with the public. It is the duty of the Highway Boards, as representing the public, to prevent every such inclosure until the proprietary right of the encloser is proved; but this they rarely or never do, probably because these boards usually consists mainly of landowners and farmers, who almost all look upon such enclosures with favour.

In the current number of the *Nineteenth Century* Mr. H. R. Grenfell advocates the enclosure of these roadside wastes on the ground that it gives employment to labourers in winter, and provides sites for cottages, gardens, and orchards for the poor; and he terms it an "economic improvement" which the Commons' Preservation Society, Mr. Chamberlain, and Mr. Jesse Collings are trying to prevent by threats of claiming restitution. But he entirely ignores the question of who is the rightful owner of the roadside wastes. The law of the land, as declared by the highest legal authority, says they are primâ facie public property, and therefore the person who encloses and appropriates them is a robber and the possessor of stolen goods. It may fairly be asked why do not the benevolent landlords, who are so anxious to find work for the poor in winter, employ them on land which is legally their own instead of on that which they first steal from the public? Are their estates in such perfect order as to need no improvement? Can gardens and orchards be

formed in no other way than by illegally converting public property to private uses? – I am, Sir, your obedient servant, Alfred R. Wallace, Godalming.

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Three Acres and a Cow (S384aa)

A somewhat surprising letter to the Editor printed in the 26 December 1885 issue of The Daily News (London).

Sir, – Will you allow me to state, as representing a body of very Radical land-reformers, that I am glad to find that Mr. Edmund S. Hanbury did not intend to make a present of three acres of land to a labourer? and I am greatly surprised that "Your Correspondent" should appear to think that he ought to have done so, or that he would have done any good whatever to the cause of Liberalism or of land reform by doing so. On the contrary, I cannot imagine anything worse than initiating a system of partial favouritism to an individual labourer – the first comer apparently – which could not possibly be applicable on a large scale, and which would neither prove anything nor satisfy anybody. We ask, on behalf of the labourers of England, not charity but justice, not to have land given them for nothing, but to have the secure and permanent use of it on fair terms: and it is because this has been and still is almost everywhere denied them that the terrible depopulation of our rural districts and diminution of our food-production has been brought about, the details of which I have given in my little book on "Bad Times." The landlords whose conduct we hold up for commendation are those who, like Lord Tollemache and a very few others, allow not only labourers, but the public generally, to have land at fair rents and on a secure tenure, not in one spot only, but almost wherever the tenant desires it. If Mr. Hanbury and other landlords wish to give a fair trial to the system of peasant-culture under the most favourable conditions (and under no other conditions is it worth trying the experiment), let them offer land on any part of their estates, and in any quantity desired, at the same rents as are paid by farmers, and on a permanent tenure, which will amount practically to a perpetual lease, with no restrictions as to the mode of cultivating the land, and in fact with no restrictions whatever except as regards nuisances. The tenant will then have all the advantages of a freehold without the necessity of finding capital for the purchase as well as for stock, while he will be saved from what all experience shows to be a real disadvantage, the temptation of the money-lender, and the not improbable ruin and loss of his holding, which so frequently results from farming on borrowed capital. For this reason I object to all the projects for advancing public money to labourers, and I firmly believe that they do not need it and will do better without it. Let every working man feel that he can at any time and wherever he thinks best secure a plot of land on which he may hope to establish "a homestead of his own" in which to spend his old age, and the money requisite to stock and work the land will be saved with amazing rapidity, and this will serve as a natural selective process, so that only the industrious, the thrifty, and the energetic will at first obtain land. The experience and success of these will be an encouragement and a guide to others, and the system of small holdings will thus spread surely and safely, which will certainly not be the case if it is attempted to be forced on by means of borrowed money. It is this free access to land on fair terms which appears to me

to be all that land reformers should at first endeavour to secure by legislation; and as it would take no land from any landlord, but only secure him a body of improving tenants, whose rents would be far more secure than those of farmers, and which would besides be all clear revenue, since the owner would never have to make any outlay upon the land, I can hardly think the plan would meet with much serious opposition. It is both much simpler and far more likely to succeed than any method involving purchase at the expense of the public, followed by sale to labourers who do not want to buy, and who would be far better off as permanent tenants than as the owners of mortgaged land. – I am, Sir, yours obediently, Alfred R. Wallace, Godalming.

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Peasant Proprietorship or Land Nationalization for Ireland (S410a)

Another letter on land nationalization, printed in the 19 October 1888 issue of the Pall Mall Gazette.

Sir, – I hope you will grant me a little of your space to reply briefly to the article by "An Irishman" in a recent issue of your paper, in so far as he misrepresents, no doubt unintentionally, the opinions and proposals of land-nationalizers. Your contributor states that "the chief objection made by the nationalizers to the conversion of occupiers into owners is that this would be a replacement of the present landlords by a multitude of petty landlords; and to this supposed "chief objection" he replies at length, and then seems to think he has disposed of that part of the question. But, instead of this being our "chief objection," it is but one of a series of objections, and cannot be fairly appreciated except as a part of the series. Our real objections are four in number:

1. We object to the sale of farms to their present occupiers because it gives to a class the future unearned increment of the land, which is the creation of the community, and by every principle of justice should belong to it. And this gift will not even be distributed over the entire class, but will accrue to certain individuals only; for, as both the population and the prosperity of the country advance – as they certainly will advance under Home Rule and any form of improved land system – certain farms will become the sites of manufactures or mines, as will be required for the growing population of industrial centres, and will then become enormously increased in value. And if the owner refuses to sell, and only lets or leases his land, he may become a wealthy landlord, with all the powers for good or evil of existing landlords.

2. We object to any legislation which does not give to every citizen equal rights to the use of a portion of his native land. Why, we ask, should those who happen to be tenants of existing landlords have the privilege of becoming owners of land, to the exclusion of the whole body of labourers, mechanics, or other Irishmen, who may also desire to have land bought for them by means of British money, and have an equal right to it? Just as the Encumbered Estates Act of a past generation gave the new purchasers of Irish estates a statutory title to all the tenants' improvements on the land, and thus legalized the most cruel robbery, so will the transformation of the present tenants into owners rob the labourers and all who are not tenants of their legal right to use and enjoy a portion of their native

soil. For, having once obtained possession of their farms, each of these new landowners will have all the prejudices of our English farmers against allowing labourers to acquire land; and we shall thus permanently divide the country between a landed and a landless class, and surely create in the future a new land problem not less difficult of solution than that which now presents itself.

3. Then, again, we object that even as creating a peasant-proprietary the scheme has no permanence. Whenever one of the new proprietors falls into difficulties he will borrow money on his land from his well-to-do neighbour or from an attorney or a money-lender; and the same process of land-accumulation by individuals will begin which is in full operation in many lands where peasant-proprietorship prevails – especially in France, in Flanders, and in India. Then we shall see a new landlordism worse than the old one, since it is universally admitted that none are such harsh and grasping landlords as the small proprietors who invest their hard-earned savings in buying the land which their less prosperous neighbours are obliged to sell.

4. It may be said, all these evils can be corrected by special legislation: municipalities may acquire land for labourers, taxation may intercept the unearned increment, and mortgaging may be forbidden. But – apart from the objection that all these things require complex and difficult enactments after you have created ownership of the land, but are the direct results of a proper system of state or municipal tenancy – there arises our fourth objection, that such legislation can only take place by the will of the constituencies, and in establishing peasant owners over the whole country we shall have placed a stumbling-block in the way of any such legislation. For none are so tenacious of their rights as small proprietors, and the unlimited increase of their numbers would be the greatest difficulty in the way of all future land reform, or in obtaining for the rest of the community any rights over their native soil. – I am, &c., Alfred R. Wallace, President of the Land Nationalisation Society.

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The Instability of Peasant-Proprietorship – The Necessity of Rent (S422)

A position statement published with several others as "The New Round Table: Land Nationalisation" in the Westminster Review, in May 1890.

Politicians of to-day, no longer able to withstand the ever-growing public opinion in favour of the radical reform of our land system, profess themselves willing to favour in every possible way the creation of peasant-proprietors; and even the present Tory Government has introduced a Bill, which, if carried, must logically be extended so as to transfer the fee-simple of the entire agricultural land of Ireland to existing occupiers. And the principle of this measure is accepted by both parties, the only difference of opinion being as to *how*, and *when*, and by *whom* it ought to be carried into effect. But not a single voice has yet been raised, in Parliament or out, to proclaim the utter futility of such a proceeding on account of the absence of the equalising agency of rent, an absence which must certainly lead to the failure of some of these new landowners and the aggrandisement of others, till, in a comparatively short period, we shall again have a body of wealthy

landlords and rack-rented tenants all over the country.

In order more clearly to see how this result must be produced, let us suppose we have arrived at the period, about half a century hence, when all the land of Ireland has become the property of the tenants and nobody pays any rent. We shall then have a compact body of peasant proprietors holding small farms of very different values, some holding land worth but five or ten shillings an acre, while that of others is worth three or four pounds. Now, it is quite clear that the man with good land and no rent to pay can afford to sell his produce lower than the man who has poor land equally rent-free, and wherever there is competition between them he will do so. When seasons are bad or prices low, the latter will be ruined by this competition, will have to borrow money on his land from his richer neighbour, and will inevitably, sooner or later, have to sell his land, which will be added to the richer land adjoining and be worked together with it. It is to avoid this inevitable result that, almost everywhere on the Continent, the land has been divided up into small detached plots so that each holding consists of a similar proportion of all the different qualities of land in the parish or commune – heavy or light soil, pasture meadow or coppice – a farm of ten or fifteen acres often consisting of twenty or thirty separate patches, all completely isolated and unfenced, and often scattered over a square mile of ground. This, of course, is a dreadfully inconvenient and wasteful mode of cultivation, but it serves rudely to equalise the different holdings; and it is this equalisation which has caused it to be upheld so tenaciously by the peasant proprietors of many different countries.

To understand how peasant-proprietorship would work with us, we may suppose that one half of the cotton manufacturers of England used the old-fashioned machines of thirty or forty years ago, whilst the other half used the very newest and most improved machinery. Is it not absolutely certain that the former would soon be undersold by the latter and would become bankrupt, unless all were taxed exactly in proportion to the benefit derived by the various qualities of the machinery employed? But the land itself is to the cultivator what machinery is to the manufacturer, and it is permanently and necessarily as different in value as would be the machinery of various periods during the last hundred years if brought into competition to-day. In order to equalise this difference in land value there are the two methods in use – the wasteful and imperfect continental method of each cultivator having small detached plots of the different qualities of land, and the far more economical and complete method of *Rent*, by which the advantages of various soils and situations are equalised, and every occupier is able to compete on fair terms with all other occupiers.

Rent, then, is a necessary factor in successful agriculture by small farmers, the only question being as to who shall receive the rent and what shall be the conditions of the occupation. The present method of private landlords and rack-rents we nationalisers hold to be the very worst method possible. That of permanent and secure occupation under the State, with the payment of an economic rent, revisable at long intervals and only on changes of value produced by general causes – that is, by the growth or advancement of the whole community – with perfect freedom of action by the cultivator who will be the owner of all improvements of whatsoever kind, to be the very best.

It may indeed be urged that, if by ownership of the land food can be produced and sold cheaper than by tenancy, it must be better for the whole community who are the consumers of food. But this cheapness would be only temporary, because so soon as the land became again the property of the few, owing to the failure of the owners of the

poorer lands, it would be let out in farms as now, rent would be paid to equalise the various values of the land, and we should return again to the existent system of landlord and tenant. Under State ownership, however, the rent paid would ultimately take the place of all other taxes, and thus the whole community would benefit far more than by a temporary cheapness of food accompanied by the ruin of a considerable portion of the poorer cultivators.

Rent, therefore, is essential to the stability of any system of the occupation of land. Rent paid to the community, through State or municipal authorities, is the only system which is beneficial to the whole community.

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Taxation or Compensation (S426)

A letter printed in the 1 August 1890 issue of The Democrat.

Sir, – I have much pleasure in replying to the four questions which you think I ought to have answered in my address to the Land Nationalisation Society. (1) I do *not* suppose that the Land Restorers' programme ends with the 4s. tax; but I have always found that they propose it as the first step. (2) Whatever arguments are valid against the 4s. tax are equally valid against each successive step while the tax is being increased up to 20s. It is, no doubt, possible in the end to make the landlords pay to the tax-collector the "whole annual value" of their land, but so long as they remain landlords and monopolists they will assuredly get it back from the tenants, not ostensibly as more rent, but in the form, perhaps, of a "voluntary bonus" enforced by a speedy and certain notice to quit. (3) I said nothing about the effect of taxing vacant land, because that is a different question, and I wished to deal only with the main problem, whether or no a land-tax for the relief of tenants will or will not ultimately be paid by the tenant. If landlords can always recover the tax from the tenant, the main purpose of land taxation fails. We do not deny that taxation, if heavy, would lead to the utilisation of vacant lands, but we prefer a method which would do this far more effectually, by placing all land required by the people in the hands of their local representatives, and thus securing not only the most complete utilisation of the land but the whole of the future increase of value for the people. (4) Buying land, as required, at a fair present value will *not* necessarily raise its price. A general valuation might be made based on the actual net rentals of land during the last ten years or so, and all land taken might be paid for on the basis of that valuation. So far from giving a new legal sanction to the monopoly of land it would absolutely destroy it, since it would place the whole land of the country in the hands of the people whenever they required it, and at a fair price. Taxation, on the other hand, does not recognise private property in land, and, by leaving the landlord his power to deal with the land as he pleases, gives to that power the sanction of fresh legislation.

Mr. [Arthur J.] Ogilvy has shown, in his excellent tract on "The Ethics of Compensation," how the payment for the land may be made to fall exclusively on those who have benefited by land-monopoly – not landlords only, but capitalists of all kinds whose wealth has been derived through power of obtaining labour for an inadequate and unfair wage which that monopoly has alone rendered possible. By thus making those who have

hitherto benefited by land-monopoly pay for extinguishing it, we shall do justice all round, and remove the only valid argument against land-purchase as a means of effecting complete and speedy Land Nationalisation. – Alfred R. Wallace.

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Commons (S443)

A brief suggestion printed in the November 1891 issue of Land and Labour, the monthly publication of the Land Nationalisation Society.

Our President, Dr. Alfred Russel Wallace, writes:

"I think it would be a good thing if you could get some local residents in the wilder districts to look up the Enclosure Act of their parish or district, and see how much land has been *nominally* enclosed and rendered private property and yet left waste for twenty, thirty, or forty years till it becomes valuable as building land. Enclosures were all made on the ground that the land would be cultivated, labour employed, and food produced. In scores of cases this has *not* been done, and the enclosure has thus been obtained on *false pretences*. Facts of this kind would be valuable to lay before the next Parliament with a demand for the restoration of all such land to the people."

This suggestion is well worthy to be acted on. Will friends please bear it in mind?

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Footpaths Along Railways (S482a)

This letter, suggesting an initiative to install public footpaths, was printed in the 8 November 1893 issue of The Leeds Mercury.

A letter has been addressed by Dr. Alfred Russel Wallace, as President of the Land Nationalisation Society, to Mr. Fowler, the President of the Local Government Board, in which the great scientist says – "I beg leave to call your attention to a great want in many parts of the country which can, I think, be remedied by means of a clause in the Local Government Bill, of which you have charge. During many years, I have noticed the great inconvenience to which large numbers of persons are subject, owing to the want of foot-paths, or rights-of-way, in growing centres of population, and more especially in connection with access to railways stations. Almost everywhere the approach to these stations, from several directions, is very circuitous, involving unnecessary fatigue and loss of time to all foot-passengers; while the difficulties and expense of obtaining new paths are so great that I have never known an instance of one being made. In a great many cases, however (partially, perhaps, in all), the desired short path could be obtained by a right of way along the railway itself. And, for many other reasons, such as affording pleasant walks where footpaths are scarce, or providing a short-cut between villages and hamlets, such right of way would be beneficial. I believe that railways are legally public

highways, subject to special conditions of use. If the company does not provide means of transit, they are bound to allow the use of the road on fixed terms to those who will provide it, and Parliament has interfered in many ways to protect the public. Unfortunately, the use of the lines as footpaths was not specially secured to the public, but I submit that such use follows from the general principle that Railway Acts are granted not for private gain, but for the public benefit, and I urge, therefore, that it be now given by the Legislature in all cases where the Parish or District Councils think it would be useful, such Council making the necessary gates or stiles, and keeping the path in order. The path might in most cases run alongside the railway fence, where there is usually ample room for a single person to walk either at the top of the cutting or the bottom of the embankment, as the case may be. It is hardly likely that the companies would seriously object, since everything that facilitates access to their stations must be for their benefit. It they ask for compensation, the reply will be, 'You obtained your powers solely for the public benefit; your lines have in many ways affected the public injuriously; the convenience now claimed for the public will do you no injury; you will be put to no expense; nothing will be taken from you; for what, then, do you claim compensation?' It may no doubt be objected that such a clause will add to the difficulty of passing the bill. I am inclined to think, however, that it would satisfy such a very common want as to be exceedingly popular, and therefore would not be seriously opposed. In no other way can so great a public convenience be obtained with so little difficulty and expense."

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Freeland Colony. – Pioneers Sail for East Africa. (S492a)

A public notice authored by Wallace and Theodor Hertzka, apparently sent out to many newspapers. The text below, possibly the full message, comes from the 31 March 1894 issue of The Western Mail (Perth, Western Australia), but other newspapers around the world may have carried the notice as much as two months earlier.

Sir, – The adherents and friends of the Freeland movement, who now amount to thousands, and are to be found in all parts of the civilised world, consider that the time has arrived to make a practical attempt at the solution of the social problem, on the lines laid down in Theodor Hertzka's book, "Freeland: A Social Anticipation" (Chatto and Windus), and in the sequel, "A visit to Freeland," by the same author. It is proposed to establish a community on the basis of perfect economic freedom and justice, a community which shall preserve the independence of its members, and shall secure to every worker the full and undiminished enjoyment of that which he produces. By placing the means of production at the disposal of the workers, we shall enable them, without exception, to work in the most advantageous manner. For the site of the new community a suitable area will be selected on the recently discovered and still unoccupied highlands surrounding Mount Kenia, in the interior of Equatorial Africa. According to the unanimous accounts of trustworthy explorers, these highlands are remarkably well adapted for colonisation by Europeans; the climate is excellent, the temperature throughout the year being very much like of that of spring time in Europe, and the land is extraordinarily fertile and rich in

mineral products. Great Britain, within whose sphere of influence the district lies, has promised her protection, as well as complete freedom in the matter of internal economic arrangements. The Freelanders are already sufficiently numerous, and command the necessary capital, to commence operations, and their preparations are now complete. Certain members have been actively engaged in our interests for a month past at Zanzibar and Lamu; and a first party of selected pioneers will start at the end of this month to be followed by the remainder a few weeks later. In a shallow-draught steamer, purchased expressly for the expedition, they will ascend the River Tana as far as the Falls – some 350 miles up stream. Thence, after forming a well-provided camp, some of their number will push on into the Kenia district, and make preparations for the later comers. The pioneers will be equipped with all necessaries both for reporting, from a scientific point of view, upon the districts traversed, and for commencing the actual work of cultivation. The larger the means, and the more numerous the *personnel* with which our enterprise is begun, the more sure and speedy will be its success, and the sooner will it begin to re-act upon the condition of the whole civilised world, which, step by step, has become untenable. We have therefore good reason for believing that few words are necessary to gain for our undertaking, which speaks aloud for itself, the moral and material support of all friends of humanity, of all who understand their own best interests, and of all who believe in a brighter future and desire to aid its realization.

Enquiries or offers of assistance may be addressed to the Central Executive Committee, 53 Langegasse, Vienna VIII. The following bankers have kindly undertaken to receive subscriptions or donations: – Messrs. A. Rueffer & Sons, 39, Lombard-street, London; Mr. Henry Hohenemser, 69, Neue Mainzerstrasse, Frankfort-am-Main, III. As soon as our enterprise is fairly started in British East Africa, an International Congress of all friends and supporters will be convened.

(Signed on behalf of the Executive Committee)

Alfred R. Wallace, F.R.S., Vice-President, President of the Land Nationalization Society, Parkstone, Dorset.

Theodor Hertzka, President, 53, Langegasse, Vienna VIII.

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Progressive Death Duties and Income Tax (S493a)

This astonishing suggestion may bring a smile, but apparently Wallace was serious about it. It was printed in the 9 March 1894 issue of The Daily Chronicle (London).

Sir, – One source of difficulty and unfairness in most schemes of progressive increase of taxation arises from the progression being made by a limited number of steps, so that a small change in the taxable amount leads to a considerable increase in the rate of taxation, as seen in the case of certain stamps and licenses. What is required is a tax which shall increase steadily with the taxable amount, so that there shall be no steps or jumps, and, therefore, no inducement to falsify returns in order to bring the amount within the lower rate of charge. Many years ago I worked out a method of doing this automatically, which

it may be useful to make known, now that such progressive taxation is demanded by all advanced reformers.

The proposal is – first, to fix upon an amount as the unit of taxation in each case, this unit to be charged one per cent. The percentage on all higher amounts is to be determined by taking the *square root* of the *number* of units. An example will make the working of the system clear. In the case of the death-duties we will suppose the unit to be £1,000, on which the duty will be one per cent. It will then increase gradually to two per cent. on £4,000, three per cent. on £9,000, till it reaches ten per cent. on £100,000, and 31 1-3 per cent. on £1,000,000. On all intermediate amounts the percentage will be fractional, but easily calculated on the same principle, so that each increase, whether of £1,000, or of £100 or less, will bear its proportionately increased rate of taxation. A valuable incidental result would be, that on very large properties the tax would increase more rapidly, so that when the amount exceeded £5,000,000 the sum receivable by the heirs would be at a maximum, and would be about £1,500,000, while if any man died worth £10,000,000 the tax would be 100 per cent. and would thus absorb the whole. The effect would be that very rich men would be more inclined to utilise their wealth for public purposes when alive, since it would not benefit their heirs to leave more than a few millions at their death.

A great advantage of this system is its flexibility. The principle of thus progressively increasing any form of tax being decided on, the amount of the tax could be increased or diminished by diminishing or increasing the unit. A lower unit will make the tax increase more rapidly on higher amounts, and will also fix the maximum of profitable wealth lower. Thus, if in the case of a progressive income tax the unit is only £100, then £10,000 will pay ten per cent., and the maximum net income, of about £15,000, will result from a gross income of £50,000. This result might be usefully reached by periodical decrements of the unit of taxation.

For the purposes of such a tax or death duty, tables would, of course, be constructed showing the approximate amount of tax on varying incomes. The lowest amount of income or property liable to taxation might be higher or lower than the unit, but in all cases that amount should be deducted from higher incomes for purposes of the tax.

A considerable additional revenue might be derived from the estates of intestates leaving no direct descendants or close relations. Either a much heavier duty might be charged in such cases, or, as many think, the whole might equitably revert to the State. Certainly all property for which no legal heirs can be found should, after a period of twenty years, be taken for the benefit of the community. – Alfred R. Wallace, Corfe View, Parkstone, Dorset.

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Land Laws and the Agricultural Depression (S518ac)

This "resolution" by Wallace and other officers of the LNS was printed in the 11 October 1895 issue of the The Manchester Courier and Lancashire General Advertiser (and other papers).

We have received the following resolution, which gives the views of land nationalisers upon the agricultural question:

"The Executive Committee of the Land Nationalisation Society rejoice that the critical condition of British agriculture is at length attracting that widespread attention which is a necessary precedent to the improvement that all alike desire. They heartily endorse proposals for such reforms as the provision of technical instruction for working agriculturists, and the transmission of farm produce by the railways at low and uniform rates. But at the same time they desire to record their conviction that the root of the present trouble lies neither in foreign competition, nor in indifference to, or ignorance of, new methods, nor yet in the heavy charges of monopolist railway companies; but, rather, in the landlord system itself. For that system of private property in land gives to one class an unjust power to levy tribute upon the industry of all other classes. It has ruined thousands of tenant-farmers by rack rents, and thrown vast tracts of land out of cultivation; while the cultivators of the soil are generally denied that security of tenure and freedom of initiative without which the most productive cultivation is impossible. Moreover, access to the land is extensively denied to small farmers and labourers, the very classes which are specially adapted to supply the home market with such produce as dairy and fruit goods, now imported to the annual value of more than £30,000,000. The committee would, therefore, earnestly impress upon the Government that there is only one way to get rid of agricultural depression, and that is by the steady diminution and ultimate abolition of the present private monopoly of the nation's land, with a view to establishing in its place a system of State ownership with local control under which the interests of the whole community may be effectually safeguarded and fostered." – (Signed) Alfred R. Wallace, president; A. C. Swinton, chairman of committee; Joseph Hyder, general secretary.

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Lord Penrhyn and the Quarrymen (S533)

A letter printed in full in the February 1897 issue of Land and Labour, after it was originally submitted to The Daily Chronicle, which published only part of it.

The occasion seems now to have arisen for giving practical effect to a principle, which, though universally admitted, has hitherto never been applied so as to produce any useful result. I refer to the well-known formula – "Property has its duties as well as its rights." Surely, if this principle is ever applicable it is in this case, where not only the well-being and the very means of existence of thousands of hard-working men and their families are endangered and through them the whole community suffers, but the property itself is of a nature which, it is almost universally admitted, ought never to have been allowed to pass into private hands. The working of minerals is not like the ordinary uses of land, since it actually destroys a portion of the wealth of the country, wealth which can never be reproduced. Our land is thus permanently deteriorated for succeeding generations; and when the produce is exported, as much of it usually is, the whole nation is injured in order to increase the wealth of private individuals.

It is often said that Parliament is omnipotent. Cannot it for once use its unrestricted power in the interest of the community when an irresponsible individual endangers that interest? When a railroad or other work of importance to the public ceases to fulfill its function, owing to the bankruptcy of the owners or from other causes, the Courts appoint

a Receiver to work it for the benefit of the creditors. Why should not the Government, in a case like this, appoint a Receiver to work the quarries in the interests of the whole community, on the just grounds that Lord Penrhyn has abused the trust that has been given him, and that he has ignored the "duties" while claiming the most extravagant "rights" of property? We are told again and again that, by the law of England, no man *owns* land, but only *holds* it from the Crown, and can be dispossessed of it whenever it is required for public purposes. Why cannot this principle be applied here? Another legal maxim informs us that "public rights are to be preferred to private," and here, surely, the rights of many thousands of innocent persons, and of the community which must, to some extent, suffer with them, is to be preferred to the private right which manifests itself in injustice and contempt of lawful authority.

If the present Government desires to make itself ever gratefully remembered by the workers, it should, when Parliament meets, at once pass a short Act placing the Penrhyn Quarries in the hands of a Receiver, to be carried on mainly in the interests of the quarrymen and of the public, the surplus profits being paid to Lord Penrhyn during his life. This would serve as a grand precedent and object lesson as to the duties of those who have been permitted to hold and to profit by almost all the land and mineral wealth of the nation, and would probably render it unnecessary to pass a general Act of the same nature, which, however, could be passed whenever desired, but as involving many complicated and disputed questions would take too much time now.

It would be interesting to know how many members of the present House of Commons would openly oppose such a law as is here suggested, and on what grounds. The interference with private property will not be so great as when a railroad is made across an estate against the wishes of the landlord, while the importance to the local community is far greater and more direct. The five or six thousand persons who will be immediately impoverished, and many of them pauperised, through no fault of their own, may well ask what use to them is an all-powerful legislature and a costly Government which is yet unable or unwilling to save them from such cruel and undeserved suffering. Unless the grand maxims of law and policy, which I have here referred to, can in this case be acted upon, it will be advisable to have an authoritative statement that they no longer apply to our existing Society and Government. Let our judges and our legislators declare openly that – "The Rights of Property are absolute," and that it "has no duties which are obligatory"; that – "The land is absolutely the landlord's to deal with as he sees fit"; and that – "Private rights are always to be preferred to Public." We shall thus avoid further hypocrisy, and bring our avowed principles into harmony with our practice.

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letters to *The Clarion* (London) on monetary policy (S552, S553 and S556)

In late 1898 Wallace published several short essays defending the idea of a paper money standard.

letter on social/economic issues (S552)

A letter to columnist 'Dangle' printed in The Clarion on 8 October 1898.

My dear Dangle, – I am rather sorry you have put forth your *Clarion* Referendum before a much fuller discussion. The whole subject is divisible into two parts: (1) such improvements in the law-making machine as shall give adequate representation to workers and Socialists, and so render advanced legislation possible; and (2) those reforms which are of most vital immediate importance for raising the condition of what you term "the half-starved drudges dwelling in pigstyes," and through them of the whole working population, which will thus be rendered more open to our proselytising influence. For the first of these purposes, among your eight alternative proposals Nos. 4 and 6 seem to be the only ones which are essential, and on which all our forces should be at first concentrated.

Then, just as we get increased parliamentary power, let us work, first for the Initiative and Referendum, which will add to our power of useful legislation. Afterwards, I would claim the immediate provision of free bread, to stop starvation, as proposed in the Appendix to my "Wonderful Century" (which I believe was sent for review to the *Clarion* office.) For carrying out this and other more permanent reforms we should require large funds, to be provided by a scheme of progressive death duties and progressive income-tax. Old-age pensions on a liberal scale would come next, then self-supporting colonies for the unemployed, not merely work, which may mean stone-breaking, oakum-picking, or any other stupidity.

I agree very largely with Leonard Hall, especially as to the land. But I think this would be most easily acquired by such an extension of the death duties as to take all above a certain amount left by any one person – say, 100,000 acres – with a diminishing percentage on lesser amounts – say, 90 per cent. on all above 90,000 acres and under 100,000, down to 10 per cent. on amounts above 10,000 and under 20,000 acres. Or the proportions may be taken in estimated land-values instead of by acreage, and of course a similar proportion of the value of all personal estates. This would furnish funds for carrying out all required measures of permanent social reform.

The one point on which I wholly differ from Leonard Hall is on what he terms "money monopoly." There is no "money monopoly" in the sense that there is a "land monopoly"; and the source of all the evil in our fiscal system is not at all in the money, but in the existence of permanent interest-bearing securities, by means of which an ever-increasing number of persons are enabled to live on the labour of the community, while doing no productive work themselves. These securities of various kinds can best be got rid of by the system of progressive death duties and progressive income-tax, culminating in the State being made the universal inheritor of all accumulated wealth, and the establishment of "equality of opportunity," to be soon followed by the co-operative commonwealth.

But this is for the future, and must as yet be a matter of education. What we want immediately is (1) Socialist candidates; (2) support of advanced Liberals who will advocate the Referendum. Till that is got, any real and far-reaching reforms are hopeless. Would it not be well at present to concentrate our energies on these two matters only, continuing at the same time with all possible earnestness our educational propaganda? – Yours, &c., Alfred R. Wallace.

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Is There Scarcity or Monopoly of Money? (S553: 29 October 1898)

Dear Dangle, – If you can spare room, I think it may be well to try and clear up some of the mental confusion that exists on this question. Leonard Hall says:

> By money monopoly I, of course, mean monopoly of means of exchange . . . The laws now regulating (restricting) the currency and banking are the last links in the long chain of that private and class monopoly which has limited, taxed, and hocussed the exchange medium from the beginning. Monopoly depends upon maintaining artificial scarcity. The scarcer a thing is made, the easier it is monopolised, cornered, kept out of circulation.

Now, the whole of this I maintain to be erroneous. There is no monopoly of money, and no scarcity; and neither workers nor producers of any kind suffer loss or inconvenience to any appreciable amount from such alleged scarcity. This is a subject which has been so obscured by vague generalisation and a misleading terminology that it is necessary to come down to plain concrete facts in order clearly to see what happens.

If, owing to some continuous increase of business, an increasing amount of money (gold and silver) is required for payments of weekly wages, &c., and the banks have any difficulty in supplying the amounts needed, any of the largest houses who are inconvenienced may purchase gold and send it to the Mint to be coined; but usually the Bank of England does this, because it first feels the scarcity, and the coinage goes on till the supply of coin in circulation is found to be sufficient. So, when silver and copper coin become scarce, and the Bank of England cannot supply the increasing demand of their customers, they apply to the Mint, which then coins more silver and copper till the demand ceases. The supply of metallic money is therefore strictly regulated by the demand; the Mint exists for the purpose of supplying the demand, and no scarcity that can affect producers to any perceptible extent ever occurs.

And on the side of the producers and workers the same result is everywhere seen. Anyone who holds goods or produce of any kind for which there is an efficient demand can always sell it for cash, at retail prices if he takes it to market himself; at wholesale prices if he prefers selling it to a merchant or dealer. No doubt the dealer will sometimes say that he has no money and will offer to buy the goods on credit; but that is not in any way due to there being any real scarcity of money in the country, but to the unsound credit-system on which almost all business is carried on, so that the money the dealer is receiving every day from his customers has to be saved to meet bills periodically falling due. Were there double the amount of money in circulation, the man who carries on his business by credit, and in competition with wealthier and larger dealers, will always be short of money.

People are deceived by the terms “money market,” “dear money,” “cheap money,” &c.; but these terms have no application to the quantity of money in circulation, but solely to the amount of interest charged for loans on personal security and of discounts on bills; and this depends on the general stability of trade, which again is dependent on politics, on the prospects of war, on the amount of speculation, and other similar causes, but has nothing whatever to do either with the quantity of money in circulation, or the amount of wealth and credit in the country. Often, indeed usually, when money is said to be “dear,” any amount of money can be had at very low interest on good security, or for sound enterprises.

It must not be supposed that I think our system of money and finance is a good one; quite the reverse. But I maintain that the imperfection of the system does not directly affect workers or producers. The whole amount of gold and silver in our current coin is so much loss to the country, and a sound system of credit-notes might well take its place, but this would make practically no difference to producers. Whether a man receives 40s. for his week's labour or a credit-note for the same amount, it is the same to him, if the two sovereigns and the credit-note are alike in purchasing power. If it is said that, by means of co-operative stores and credit-notes, he will be able to purchase *more* for the same nominal amount than if he is paid in money, that has not been proved. With co-operative stores in full working order which would receive his produce at fair wholesale prices and sell him goods at fair retail prices, he would no doubt obtain a considerable advantage; but the advantage would be due to the co-operation, and the small margin of difference between wholesale and retail prices not at all, or if at all, in an infinitesimal degree, to the use of notes instead of money. That the amount saved by not using metallic money would be very small, if perceptible, can be shown in two ways. (1) For a long time, at all events, the stores must have a considerable money capital to buy the various goods not produced by the co-operators. (2) The amount of the money required as permanent cash capital would be very small compared with the whole business done, because every day and hour, on the average, more money would be received for sales than would be paid out for purchases, and thus a very small permanent cash balance would suffice to guard against the purchases in any one day or week exceeding the sales; and this sum, as compared with the total amount of the sales in the year, would be quite insignificant.

It appears, then, that the supply of coined money is always such as to satisfy the demand, acting automatically by the agency of the Bank of England and the Mint. There is, therefore, *never* any scarcity of the circulating medium. For the same reason, monopoly of it is impossible, since the first attempts at a monopoly would lead to increased coinage, and the monopolists would then have to export their hoarded gold or turn it into bullion at a loss.

It is also clear that the actual amount of money in circulation, though absolutely large, is, relatively, exceedingly small, when compared with the amount of work it does. Every sovereign probably buys a hundred pounds' worth of goods in a year, and the very same sovereign may go on buying for fifty, or even a hundred, years, so that although the total amount of coin in circulation is enormous, yet it is a very small fraction as compared with the exchanges it facilitates before it is worn out or replaced, and thus the saving effected by the universal use of credit notes might probably not average a shilling a year to each worker. I think I have now shown that there is, as a fact, no monopoly, no artificial scarcity, no restriction, no hocussing of the circulating medium as it affects the workers; while for traders and merchants on a large scale the supply of banknotes, cheques, bills of exchange, &c., &c., is unlimited. In the case of this form of money it is undue inflation, never restriction, that produces evil results.

The use of metallic money as a standard is also disadvantageous on account of fluctuations in the intrinsic value of gold or silver as compared with other commodities; but these fluctuations are certainly not great or rapid in the case of gold, and do not therefore affect the workers, because any changes arising from this cause will affect both wages and prices in the same way. The only real and important evil of our financial system is due, as stated in my former letter, to the existence and continual increase of interest-bearing funds, bonds, and shares, which not only encourages that form of gam-

bling termed financial operations, but enables the surplus savings of each year to be permanently invested, and thus increases year by year the number of persons who are able to live in idleness upon the labour of the productive workers, and therefore to their injury and impoverishment. It is for this reason that the continuous increase of our commerce and our wealth is, and must necessarily be, accompanied by a corresponding increase of poverty and starvation. This I have demonstrated by indisputable facts in my recent work "The Wonderful Century," and it is the one thing above all others that should be continually brought before the public, till it at last penetrates the thick armour of optimism with which the middle and upper classes, and especially politicians and the literary, artistic, and scientific cliques, protect themselves against the contemplation of the terrible realities and heartrending miseries which are the necessary results of our barbaric competitive system.

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A Complete System of Paper Money (S556: 3 December 1898)

My dear Dangle, – Your correspondent, A. P. Hazell, asks me to explain how a safe and effective system of credit notes or other form of paper money can be established and worked. I will endeavour to do so as briefly as possible; but to explain the matter fully would require a lengthy article.

A gold currency is supposed to be necessary in order that we may have money which is a measure of value as well as a tool of exchange. It is, however, now admitted that gold is not a permanent and stable measure of value, though I believe it is much more nearly so than is generally supposed. Most of the money specialists believe that for many years past the value of gold has been rising, basing their conclusion on the continual reduction in price of most commodities. But it is evident that the price of goods may be greatly reduced by improved machinery and production on a larger scale, and it seems to me that in the case of most of our manufactured goods this cause alone is sufficient to have reduced prices much more than they have actually been reduced; and in that case gold will have *diminished*, not increased in value, as the enormously increased production during the last half-century would lead us to think it should have done.

The usual objection to paper money is that it will change in value according to the amount issued, as is well seen in all countries where the Governments have tried to raise funds by such over-issues. This is quite true; but it is this very property of paper money that makes it easy to keep its value stationary, and, therefore, renders it, when the issue is properly regulated, a better and more stable measure of value than gold, or than any single commodity whatever. How this stability can be attained, I will now endeavour to explain.

Stability, or equality of purchasing power at different times, can only be known by the same nominal amount of money – say, £100 or £1,000 – being able to purchase the same quantities of all the chief necessaries of life on the average. Luxuries used by the few – ornaments, jewellery, works of art, &c. – may be left out of consideration. As necessaries of life, we may take the four great groups of food, clothing, houses, and fuel; and each of these may be represented by a limited number of the most important items, as bread, meat, potatoes, sugar, tea, and beer, to represent food; timber, iron, bricks, and glass for houses, or a larger number of items if thought advisable by experts. Having fixed upon the list of commodities – perhaps 50, perhaps 100, in all – which are considered to be amply sufficient as the basis of an estimate of the purchasing power of money,

the next step will be to estimate the proportionate quantity of each consumed in the whole kingdom, or in some representative part of it, during a year. This is necessary in order to give to each its proper weight in the estimate; for if 100 tons of A and 1,000 tons of B are used per annum, it will lead to very erroneous conclusions if we were to use equal quantities of each in our estimate, and I believe that this very mistake has been made in the estimation leading to the conclusion that gold has for many years been appreciating in value. Having now got our typical list of commodities with the proportionate quantities of each, we next have to get the average price for a series of years – seven, ten, twenty, or whatever number may be fixed upon as the basis on which to calculate the standard purchasing power of our new national currency. All these facts can be got at with sufficient accuracy by means of agricultural and commercial statistics and market prices. When completed, a table will be constructed something in this form:

Proportions of standard products consumed, and their value on the average of seven years – 1890–1896.

Bread	10,000 lbs.	value £50
Meat	4,000 "	" £200
Sugar	1,500 "	" £10
Tea	500 "	" £40
Timber	1,000 cub. ft. . .	" £100
Coal	200 tons	" £200
&c., &c., &c. . . .		£600

These proportions and prices are put down at a mere guess, but when obtained as accurately as possible for the whole of the 50 or more commodities chosen, we shall have, as a result, that these quantities of these commodities have, on the average of the last seven (or 10 or 20) years' cost a certain gross sum. Now, what I maintain is, that paper money (called credit-notes, or anything you like) can be so issued as, for any number of years, to continue to purchase the same quantities of this whole series of commodities for approximately the same nominal amount. Some of the items will, of course, rise in value from one year to another, and others will fall: but the paper currency will always, within very small limits of variation, purchase the same total amounts.

To do this, a Minister, or Commissioner of Currency, with a sufficient staff of clerks, will be appointed, whose duty it will be to have regular returns made of the market prices of the standard commodities week by week, and to have the averages calculated. If during any month or quarter these averages are seen to fall continuously, that is, everything becomes cheaper, he will advise the Treasury to issue more notes which they will bring into circulation (by using them to pay salaries and current expenses) till the fall is checked and the true average reached. When, on the other hand, the standard goods show a rise in price, it indicates that there is a slight surplus of the currency, which is to be checked by cancelling old notes as they come back to the Treasury. This process could be so nicely regulated that, practically, there would be no rise or fall of prices on the average, since either would be remedied before it could possibly be detected by the public.

Here, then, we should have a most useful and portable currency – which could be issued for any amounts in very thin but tough cards about the size of railway tickets, and of different colours for the different denominations – and which would be a stable measure of value as well as a convenient instrument of exchange. And it would have the great advantage of working almost automatically and preserving an unchanged purchasing

power by the very act of supplying the demands of the community. And as, with an increasing population, more and more currency would be required, and as many small notes would be lost, burnt, or otherwise destroyed, this currency would be a constant source of revenue to the Government.

During the process of change from metal to paper the gold paid into the Treasury for taxes, duties, stamps, &c., would be accumulated, and form a reserve fund for pressing purchases from other countries in case of war. But the great point is, that by regulating the amount of notes issued in the way above described, this money would become a real *measure of value*, which gold can never be so long as its production is a matter of private speculation, and its cost, and consequent value in exchange, liable to indefinite variation.

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Paper Money as a Standard of Value (S557)

Printed in The Academy issue of 31 December 1898. Wallace's observations, later noted by the leading American economist Irving Fisher, turned out to be a significant precursor to twentieth century discussions on currency stabilization theory. Fisher went so far as to dedicate one of his books (Stabilizing the Dollar) to Wallace.

The proposition embodied in this heading will seem to most persons to be an absurdity; but I hope to be able to show from the statements and admissions of orthodox authorities that paper money, under proper regulations, would be the most permanent, and therefore the best, possible standard of value. I presume that the late Prof. W. Stanley Jevons was a trustworthy authority on the subject; and in his volume on *Money and the Mechanism of Exchange* he gives some important facts and principles bearing upon this question, and these I shall take as the basis of my argument.

1. He shows that gold has undergone great changes of value during the last hundred years, as determined from the average prices of fifty or a hundred of the chief necessaries of life. The difference amounted to a fall of 46 per cent. from 1789 to 1809; while from 1809 to 1849 it rose 145 per cent. Since 1849 it fell about 20 or 25 per cent.; while in the last twenty or thirty years all the authorities declare that it has risen considerably.

2. Having thus shown that gold does not even approximate to a permanent standard of value – though I believe the alleged fluctuations are enormously exaggerated, for reasons which it would take too long to give here – he goes on to explain the various proposals which have been made to obviate the evils of such fluctuations by means of a "Tabular Standard of Value." A Government official – who might be called the Registrar of Prices – would collect the market prices of the list of commodities fixed upon to determine the value of money, and would publish the result monthly or quarterly, and the value of money so determined would be used to regulate all payments of debts, salaries, &c. "Thus, suppose a debt of £100 was incurred on July 1, 1875, and was to be paid July 1, 1878, and the Registrar's table showed that in that interval gold had fallen in value six per cent., then the creditor would claim to be paid an increase of six per cent., while, if there had been a rise in the value of gold then the debtor would have a right to pay proportionally less than the amount nominally due."

He says there are only two difficulties – the determination of the commodities chosen to fix the standard value, and the complexity introduced into the relations of debtors and creditors. The latter is, no doubt, a real objection, but it does not arise (as I shall presently show) when paper money alone is used. Neither is there any real difficulty in the former. What is needed is to take a representative selection of all the *necessaries* of life. These may be roughly classed as food, clothing, houses, fuel, and literature. For the first we might take meat, bread, potatoes, sugar, tea, butter, and beer; for houses timber, bricks, iron, glass, lime, cement, slates, and building land – and so on under the other headings. But the most important consideration is, that each item be taken in the proportion in which it is consumed in the country. This was seen by the original proposer of this method – Joseph Lowe, in 1822 – but has been neglected by some modern writers. It would, therefore, be necessary, first to estimate the total quantities of each item consumed in the kingdom in a year, and then, representing the smallest quantity by one or ten, to give all the others their due proportions. The prices of these several commodities being ascertained on the average of a number of years to be fixed upon, a table would be formed, giving the money-value of the due proportion of each of the commodities. Then, by adding up these values, we should have a sum total which would represent with considerable accuracy the average cost of all the chief necessaries of life in the proportions in which they are consumed by the whole community. In order that money may retain the same purchasing power, and thus constitute a real standard of value, this same amount of money must always purchase the same amounts of all these commodities. This can never be the case with gold or silver money, or with the two combined, but I will now show that paper-money may be so regulated as to have always the same purchasing-power.

Prof. Jevons states the chief objections to inconvertible paper-money as follows:

1. The great temptations which it offers to over-issue and consequent depreciation.

2. The impossibility of varying its amount in accordance with the requirements of trade.

The first of these objections does not arise when the whole purpose of adopting a paper-currency is to secure a permanent standard of value. The second objection must have been stated without due consideration, since nothing is more simple than to produce this "variation of amount"; and when the variation is such as to keep average prices steady, that steadiness will exist *because* the quantity issued *is* in accordance with the requirements of trade. This objection, which is stated at length under the heading "Want of Elasticity of Paper Money" (p. 237), is really completely answered by the method of the tabular "Standard of Value" (p. 329), but the two things are not brought together.

In order to show how Prof. Jevon's "impossibility" may be easily overcome, let us suppose the transition period to have been passed over: all gold coin being called in or having ceased to be a legal tender, and the paper-currency issued to the same amount. The Registrar of Prices, having determined that during the preceding year the purchasing power of this money is two or three per cent. greater than that of the standard as determined by his table of average values, and having had experience of the effect produced by a given increase or diminution of the currency, instructs the Mint to issue fresh money at a given rate per week. This money is sent to the Treasury and is at once brought into circulation by being paid away in salaries, wages, purchase of materials, &c., in the various Government departments. There is thus no difficulty whatever in increasing the amount of the currency and thus diminishing its purchasing power. The Registrar of Prices carefully watches the effect upon the markets week by week, and month by month,

and when he sees that the standard is very nearly attained he instructs the Mint to stop further issues. On the other hand, when prices are rising, owing to there being rather more money in circulation than is necessary, instructions are sent to the Treasury to cancel a certain amount of the money paid in for taxes, stamps, &c., till the balance is restored. But this will very seldom, perhaps never, be necessary. The continuous increase of the population requires a constant increase in the currency, while another constant renewal is required to make good the losses by fire, water, and other accidents. And as the amount required to keep average prices steady would be so carefully watched, the mere stoppage of the normal issues would in most cases suffice to bring back average prices when they showed any tendency to rise above the standard amount.

The total gain to the country of such a currency would be very great. All the additions required to keep up with increase of population and to make good losses would be clear gain, and would probably amount to a considerable annual revenue; while during the transition from gold to paper an enormous amount of coin would be accumulated by the Treasury which might be kept as a reserve against foreign war expenses, or might be supplied to merchants as bullion of guaranteed quality for foreign payments. Silver and bronze coins for wages and small transactions might be continued in use, as they are both customary and convenient, but their actual value in metal might be reduced, thus giving a larger profit to the Government on their issue than there is now.

A convenient form for the £1 and £5 notes would probably be very thin tough cards of the size of railway tickets, and of different colours. They would thus be very portable and easily distinguishable. They would be the legal tender of the country, and would always purchase, on the average, the same quantities of the chief necessaries of life. They would thus constitute a permanent standard of value – the ideal perfection of money; and would have the additional advantage of being a steady source of revenue to the country.

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correspondence concerning railroad nationalization (S628, S628a and S628aa)

The following single-paged letters to the Editor of The Daily News (London), discussed a Wallace plan for nationalizing the railroads.

How to Nationalise Railroads (S628: 24 September 1906)

As the Trade Union Congress has unanimously requested the Labour Party to introduce a Bill with the object of nationalising all railroads, canals, mines, and minerals, it is evident that this great subject has now come within the sphere of practical politics. It is, therefore, of the greatest importance that the ways and means by which such a gigantic transfer may be effected should be very carefully considered, before we are committed to any definite scheme of operations which may endanger its success, or which, if successful, may be opposed to the public interest.

Having for many years given careful consideration of the various methods that have been suggested for acquiring these and other properties the possession of which is of vital interest to the nation, I propose now to describe, very briefly, that which I consider to be

in every way the best.

In all previous cases of the transfer of what are, or ought to be, essentially public services, from private individuals or companies to local authorities or to the Government, there has been a severe struggle between the two parties concerned – as buyer and seller – regarding the mode of valuation of the property, the purchase-money to be paid, or the compensation to be given; and in this struggle the sellers have always succeeded in obtaining for their property very much more than it was worth, to the great loss and often to the permanent injury of the public. These facts are notorious, and if the same methods are applied in the case of the railroads and mines, the same scandals will be repeated on a still larger scale, so as largely to discount any benefit the public would derive from the transfer.

I think, therefore, that the great majority of advanced thinkers will be with me in the determination that this great blunder must not and shall not be repeated; and the main purpose of this article is to suggest a method of dealing with the problem which shall entirely avoid any struggle or bargain of the nature above referred to. By the method I propose there will be no sale or purchase, no valuation or compensation; yet the just property rights of existing shareholders or owners will be fully recognised, while at the same time the public will derive the utmost possible benefit from the transaction. The plan by which this result is to be attained will be very simple when once the general principles involved are accepted and acted on; and in order to explain the process as clearly as possible I will now show how it would work in the case of the railways.

The Act establishing Nationalisation will be based upon the fact that the management of railway traffic by antagonistic companies is necessarily wasteful, and is in many ways opposed to the interests and convenience of the public. It will, therefore, enact that on a certain day the management of all the railways will be taken over by a Government department under a Minister responsible to Parliament, and the whole combined system will be reorganized with the object of giving to the public the best possible service and accommodation. The change would not necessarily imply the discharge of a single official or servant of the existing companies, but probably a considerable addition to their number. This first step in the process of Nationalisation would be a simple transfer of the management, not the purchase, of a property. The question of capital value would not arise.

Coming now to the interests of the shareholders in the several companies and how they shall be dealt with, we reach the essential feature of the present scheme. Without entering into minute details the method proposed to be adopted would be somewhat as follows. The interest on debentures or dividend on shares will be averaged for a period of three, five, or seven years, dependent on the stability of the traffic, and on the need of repairs or replacement of the permanent way or rolling stock in the case of each system.

A fair average interest for each class of shareholder in the respective lines will thus be determined, and will be paid to each shareholder, as an annuity for his life and also to his widow for her life. In the case of there being orphan children the payment will be continued till they reach the age of twenty-one, in order to assist in providing education and industrial training. Further, in cases of special necessity, as when other relatives than children were dependent on the shareholder, the interest would continue to be paid to them, the principle being adopted that not only the shareholder himself should feel that his own income from these investments was absolutely secure for his life, but also that no relatives or dependents in whose welfare he felt a strong personal interest should be

deprived of what he would have left them whenever it was necessary to save them from destitution. When these various claims were satisfied the annuity would cease to be paid.

No doubt some thoughtless people will raise the parrot-cry of confiscation, on the ground that a terminable annuity is of less capital value than perpetual or permanent one. But it is evident that no railway securities are really permanent, while they are liable not only to fluctuation, but to complete suspension in case of accident or mismanagement. The absolute guarantee of a steady income both for the holder's life and for the subsequent benefit of his family and dependents must be considered to be a full equivalent for the sentimental value of benefiting persons or institutions, other than his own family, at some indefinite future time. In addition to this consideration, every shareholder would himself benefit by the great improvement in the general railway service which public management and the co-ordination of the whole railway service of the kingdom would bring about, involving, as it probably would, some reduction in the cost of many articles of daily use.

By the adoption of the mode of transfer here suggested both of the railroads and of canals, mines and minerals, the nation would, in the course of about half a century, obtain full possession of a great mass of properties which should never have been allowed to fall into the possession of individuals, to the enormous advantage of the public. In this way, and in this way only, can we be strictly just to the living while ceasing to transmit the cruel burden of our debts, our errors, and our follies to our descendants.

As some of your readers may wish to see the principles on which my proposal is based more fully set forth I will refer them to two essays in my "Studies Scientific and Social" (Vol. II). One is entitled "Interest-bearing Funds: Injurious and Unjust"; the other is headed "True Individualism the Essential Preliminary of a Real Social Advance." The first bears upon the more special problem here discussed; the second shows how the "Law of Social Justice," established by Herbert Spencer, necessarily implies the injustice of permitting unrestricted inheritance of wealth – an inference which he himself strangely overlooked.

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How to Buy the Railways. Dr. A. R. Wallace's Reply to Critics. (S628a: 29 September 1906)

Sir, – Will you allow me to make a few remarks on the various suggestions of my critics?

I will deal first with Mr. W. Bennett, because his plan is based upon a common fallacy in dealing with similar questions. He proposes that the Government shall issue legal tender notes of the full market value of the railway shares, etc., with which to pay the shareholders in full, after the plan adopted in the case of the Guernsey Market. This is an excellent method in its right place and with proper limitations, and I have myself argued in favour of it for the purpose of enabling local authorities to execute reproductive public works without the cost of a loan. But to pay the whole body of railway shareholders in this way would entail upon them a considerable loss. The majority of private persons hold their shares for the sake of the income derived from them, and when paid the capital value, whether in notes or gold, they would at once seek new investments from

which to obtain an income of the same amount and equally safe. But the number of such safe investments is limited, and the sudden demand for such, to the amount of about a thousand millions (and our railway capital is considerably more than this) would immediately raise the price of such securities. The result would be that although all the shareholders were paid the full value of their shares, they could not obtain for the money an equally large and equally safe income to that they now possess. The anxiety and distress caused to many of the poorer of these shareholders when they found that although paid in full their income would be diminished, perhaps considerably, would be very great, and I feel sure that the large majority of them would much prefer the perfectly secure family life annuity which the Government would guarantee them if my suggestion be adopted.

The same general considerations apply to Mr. C. E. Smith's plan, inasmuch as he also proposes paying "the full market value of the securities in cash"; while his alternative proposal of giving Government securities of equal value in exchange, and then raising an equal amount by "loans on terminable annuities" to redeem these securities, the loss on the transaction being covered by "a graduated income tax and differential death duties," involve a series of complex financial operations on a gigantic scale which might benefit great capitalists, but would almost certainly result in loss to the public. The writer's remark about "penalising one set of capitalists and favouring another" is quite beside the question at issue, since the method I propose is equally applicable to the acquisition for great public purposes of all kinds of property. But, in this country at all events, we cannot do everything at once, and the problem now being discussed is how best to nationalise the railways in the interest of all parties concerned.

There remains the question, raised by "Carshalton," of how to deal with the great masses of railway shares held by insurance and other public companies, and their case no doubt demands special and liberal treatment, but on the same general principle that is applied to individuals. It is clear that these companies would suffer almost as much as individuals if they were all paid off in full, and had at once to seek fresh investments; and they will certainly not expect to have allotted to them Government funds bringing in the same income as do the railways, and therefore of much higher value. A fair and even very liberal mode of treatment seems to be as follows. The whole body of policy-holders and shareholders at the time of passing the Act will be taken as representing a single shareholder, and the "life" of this shareholder will be held to continue till the decease of the last survivor of them, when the shares, as in all other cases, will lapse to the Government. If the company forms a sinking fund by investing annually the difference between the interest received on these railway shares and that from any other Government stock, or even half or two-thirds of this difference, the capital would be replaced during the 70 or 75 years of the possible "life" of the company as already explained. But even if they do not do this, none of the persons interested when the Act is passed will suffer the least pecuniary loss.

I think I have now proved that the method of railway nationalisation which I have suggested, while very favourable to the interests of the whole community, is also more beneficial to existing shareholders than any of the alternative proposals. It also has the advantage of being exceedingly simple and direct, producing the minimum of disturbance to financial and general interests. To the enormous body of small shareholders it offers security to themselves and family with no disturbance or break in their annual income. I

therefore confidently submit it to the consideration of those on whom will devolve the responsibility of drafting the proposed Railway Nationalisation Bill. – Yours, etc., Alfred R. Wallace. Broadstone, Sept. 27.

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How to Buy the Railways. Dr. Wallace's Reply. (S628aa: 1 October 1906)

Sir, – Mr. W. T. Fox's proposal (in your Friday's issue) is the least advantageous and the most unjust of any yet proposed. He actually considers it a good thing for the Government during a whole century to pay the railway shareholders and their successors a higher rate of interest than they obtain now, rendering improvements more difficult during the whole of that period, in order that in the year 2010, or thereabouts, the whole population may suddenly be able to travel at the cost of mere working expenses! In other words, the railway service is to be crippled for a century in order that millions of people now unborn are to receive dividends they have never earned!

The enormous advantage of my plan is that, owing to the numerous deaths annually of shareholders with no direct heirs, a continually increasing revenue will accrue from the very first, so that the great majority of people now living will obtain the benefit not only of improved service due to a single management, but to lower and lower fares and rates due to the yearly diminishing amount of dividends payable.

And we are to give up all this advantage and this great motive power urging us to the reform in order to carry out the utterly immoral and unjust principle of compelling millions of unborn travellers to pay higher fares for the benefit of other millions of unborn travellers who have themselves done nothing to earn it or deserve it. Mr. Fox considers this proceeding to be honest, and, therefore, beneficial. I maintain that it is fundamentally dishonest, and, like all dishonesty, is injurious to everyone concerned in it. – Yours, etc., A. R. Wallace. Broadstone, Wimborne, Sept. 28.

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Nationalisation, *not* Purchase, of Railways (S661)

Another letter on railways, this one printed in the 19 September 1908 issue of <u>*The New Age*</u>.

In your "Notes of the Week" (September 5th) you remark that railway companies are combining "for the sole purpose of selling their lives dearly" in anticipation of nationalisation. As it seems to be a very general idea, even among Socialists, that the two operations – nationalisation and purchase – must go together, will you allow me space to point out that, while nationalisation is in the highest degree advisable, and may be effected at once by a very simple enactment, purchase is equally unadvisable and unnecessary, and had far better be left till a much later period, when in all probability some general method of dealing with similar claims to other forms of nationally produced wealth may be found practicable.

I may take it for granted that every reader of *The New Age* recognises the advantages

to the public, as individuals, in the whole of the railways being worked with the sole view of the maximum of use and enjoyment of the people, so far as is consistent with the safety and well-being of the great army of employees, which will itself tend to secure the safety of the public; while to the nation, this complete unity of organisation and management will be of incalculable advantage as a safeguard against foreign invasion.

But these, and many other collateral advantages will accrue, just as certainly, and even more rapidly, by the State taking over the fixed and rolling stock of the whole of the railways, to be managed and worked in the public interest, while continuing to pay to the present owners of the railways – the shareholders and possessors of every kind of railway stock – that proportion of the net profits to which they are now equitably entitled.

It is, I believe, generally estimated that the economies which would be effected by the co-ordination of the whole system would amount to many millions annually, and this great saving would all be expended in reduced fares, better services, higher wages, and shorter hours of work, by which all shareholders and employees, as well as the whole of the public, would greatly benefit.

But the increased facilities to all who use the railways, and the abolition of the needless and often irritating restrictions of most of the existing managements, would certainly lead to a large increase of traffic, and thus render any considerable discharge of existing railway employees unnecessary, while the position of all would be much improved.

It is needless here to go into the question of the exact future status of the shareholders. As one mode of dealing with them, I would suggest that the relative market-value of each kind of railway security having been ascertained, with due regard to the condition of the line and rolling-stock, the holders of these securities should be offered in exchange government annuities for their own lines, and that of the legal heirs, in the direct line, living at the time of the owner's death, these annuities to be for amounts approximately equal to the dividends or interest they had received on an average of the three preceding years. This fixed and certain annuity would be fully equal in value to the less secure and fluctuating railway stocks. Those who declined to accept this mode of payment would receive whatever dividends the Government should declare to have accrued, after full provision for the upkeep of the line, efficiency of the service, and reduction of debt.

I believe myself that a majority of railway shareholders would accept the annuities, and this would lead to the possibility of the railways becoming unencumbered national property in two or three generations. Debenture-holders would, of course, be gradually paid off at par out of profits.

The special advantage of such a mode of nationalising our railways is, that it involves no vast financial operation of valuation and purchase, certain to be disadvantageous to the public – an operation which so many people think an insuperable objection to nationalisation. It secures all the advantages of public management, and at the same time safeguards the equitable interests of the shareholders.

The method is, of course, applicable to the acquisition of every kind of property in the hands of corporations, which should belong to the community.

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The Development Fund (S679)

A letter printed in the October 1909 issue of Land and Labour.

It occurs to me that the debate on the *Development Bill* [a] will afford a good opportunity for calling attention to the question of the illegal enclosure of roadside strips which still goes on all over the country.

The Local Government Act showed that Parliament saw the importance of this matter, by enacting that the district councils not only had the *power* to reclaim such land, but that it was their *duty* to do so.

The power and duty, however, have been quite ineffective owing to the overpowering influence of the landlords, their agents, or their tenant farmers, who are always well represented on these Councils.

It therefore seems necessary on such an occasion as is now afforded by this Bill, which will not only authorise the formation of new roads, but the improvement and widening of existing ones, that the Authority established for this purpose should have power to enquire into all such cases of illegal enclosure as may be brought to its notice, to hear evidence on both sides, and, when such enclosure is proved, to reclaim the land for the public.

The law on this question is perfectly clear, and it has been laid down in a well-known case by a Court of Appeal (1) That the whole of the space between the fences of any public road, however wide this may be, is just as much public property as the metalled part of it; and (2) that land that has ever been public property is always so, notwithstanding any lapse of time since it was illegally enclosed.

Although it may possibly be ruled that this matter is beyond the scope of the present Bill, it yet affords an excellent opportunity for any of our members who are interested in this subject, and who are M.P.'s to have it discussed on the second reading, and if possible an amendment moved in Committee.

Even if it does not result in any action *now*, it will show the whole body of our members and the public that the intentions of Parliament in this matter have not been fulfilled, and thus show the necessity, by amendment of the Local Government Act, or in some other way, of putting a stop to such encroachments and reclaiming the land as well as any improvements on it *without compensation*.

Will you be so good as to communicate this suggestion to any members who you think will act upon it.

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[a] The Development Act of October 1909 was called by Keir Hardie "the most 'revolutionary' measure ever introduced by a government and an implicit recognition of the 'right to work'" (M. P. Cowen and R. W. Shenton, *Doctrines of Development*. Routledge, 1996, on p. 284).

Section 8. Social and Political Issues

Introduction

Most readers, if they are familiar with Wallace at all, will know of his work as an evolutionist and biogeographer. But, as we have seen in the last section, these associations, and even his efforts in other areas of natural science, hardly exhausted the breadth of his attention. Indeed, it was social evolution – and its deficiencies to that point – that more than anything else energized him from his middle years onward. In fact, one is hard-pressed to identify any science figure in history who spoke up more regularly, more passionately, and on a greater variety of issues than did Wallace. He legitimately deserves the accolade "humanitarian," not only for his tirades against the unjust, but for never giving in to a defeatist attitude regarding the basic goodness of humankind (note the final observation in one of the coming selections: "Truly, we will *not* despair of the Republic of Humanity.")

In his earlier years observations occasionally surfaced as to society's deficiencies (as in the final paragraphs of his book *The Malay Archipelago*, in 1869), but it was not until the 1870s that he became more regularly involved. Writings such as "Government Aid to Science" (1870), "Disestablishment and Disendowment" (1873), and "Limitation of State Functions in the Administration of Justice" (1873) set the stage for the many works that followed.

After surveying the items in this section, it will also become clearer to the reader why so many have sought to minimize his place in history, both in science and as an observer in general. Simply, in a world in which so much was "rotten at the core" (as he put in his essay "Human Selection" in 1890, and book *Social Environment and Moral Progress* in 1913), some individuals had to take responsibility, and these often were not happy at having fingers pointed at them.

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Public Responsibility and the Ballot (S110)

Once Wallace felt he had grasped the essentials of a question, he was never afraid to question even the most celebrated figures' opinions. Here, in a letter to the Editor printed in the 6 May 1865 number of the London review The Reader, he takes on John Stuart Mill.

In the review, headed as above, in your last number, and signed "J. S. M.," the well-known writer does not appear to me to have been quite successful in answering the arguments of the pamphlet he criticizes. Indeed, on the most important point, his own reasoning seems equally applicable to the opposite side of the question. I beg leave,

therefore, to make a few observations on what appears to me, as I doubt not it must to many of your readers, a very inconclusive part of his article.

Mr. Mill truly says, that a voter is rarely influenced by "the fraction of a fraction of an interest, which he as an individual may have, in what is beneficial to the public," but that his motive, if uninfluenced by direct bribery or threats, is simply "to do right," to vote for the man whose opinions he thinks most true, and whose talents seem to him best adapted to benefit the country. The fair inference from this seems to be, that if you keep away from a man the influences of bribery and intimidation, there is no motive left but to do what he thinks will serve the public interest – in other words, "the desire to do right." Instead of drawing this inference, however, it is concluded that, as the "honest vote" is influenced by "social duty," the motive for voting honestly cannot be so strong "when done in secret, and when the voter can neither be admired for disinterested, nor blamed for selfish conduct." But Mr. Mill has not told us what motive there can possibly be to make the man, voting in secret, vote against his own conviction of what is right. Are the plaudits of a circle of admiring friends necessary to induce a man to vote for the candidate he honestly thinks the best; and is the fear of their blame the only influence that will keep him from "mean and selfish conduct," when no possible motive for such conduct exists, and when we know that, in thousands of cases, such blame does not keep him from what is much worse than "mean and selfish conduct," taking a direct bribe?

Perhaps, however, Mr. Mill means (though he nowhere says so) that "class interest" would be stronger than public interest – that the voter's share of interest in legislation that would benefit his class or profession, would overbalance his share of interest in the welfare of the whole community. But if this be so, we may assert, first, that the social influence of those around him will, in nine cases out of ten, go to increase and strengthen the ascendency of "class interests," and that it is much more likely that a man should be thus induced to vote for class interests as against public interests, than the reverse. In the second place, we maintain that any temporary influence whatever, which would induce a man to vote differently from what he would have done by his own unbiassed judgment, is bad – that a man has a perfect right to uphold the interests of his class, and that it is, on the whole, better for the community that he should do so. For, if the voter is sufficiently instructed, honest, and far-seeing, he will be convinced that nothing that is disadvantageous to the community as a whole can be really and permanently beneficial to his class or party; while, if he is less advanced in social and political knowledge, he will solve the problem the other way, and be fully satisfied that in advancing the interests of his class he is also benefiting the community at large. In neither case, is it at all likely, or indeed desirable, that the temporary and personal influence of others' opinions at the time of an election, should cause him to vote contrary to the convictions he has deliberately arrived at, under the continued action of those same influences, and which convictions are the full expression of his political knowledge and honesty at the time?

It seems to me, therefore, that if you can arrange matters so that every voter may be enabled to give his vote uninfluenced by immediate fear of injury or hope of gain (by intimidation or bribery), the only motives left to influence him are his convictions as to the effects of certain measures, or a certain policy, on himself as an individual, on his class, or on the whole community. The combined effect of these convictions on his mind will inevitably go to form his idea of "what is right" politically, that idea which, we quite agree with Mr. Mill, will in most cases influence his vote, rather than any one of the more or less remote personal interests which have been the foundation of that idea. From this

point of view, I should be inclined to maintain that the right of voting is a "personal right" rather than a "public duty," and that a man is in no sense "responsible" for the proper exercise of it to the public, any more than he is responsible for the convictions that lead him to vote as he does. It seems almost absurd to say that each man is responsible to every or to any other man for the free exercise of his infinitesimal share in the government of the country, because, in that case, each man in turn would act upon others exactly as he is acted upon by them, and thus the final result must be the same as if each had voted entirely uninfluenced by others. What, therefore, is the use of such mutual influence and responsibility? You cannot by such means increase the average intelligence or morality of the country; and it must be remembered, that the character and opinions, which really determine each man's vote, have already been modified or even formed by the long-continued action of those very social influences which it is said are essential to the right performance of each separate act of voting. It appears to me that such influences, if they really produce any fresh effect, are a moral intimidation of the worst kind, and are an additional argument in favour of, rather than against, the ballot.

Two other questions remain. Is the ballot necessary to prevent bribery and intimidation? Is it so injurious to independence of character as to overbalance its undoubted utility? I think Mr. Berkeley's letter in the *Times* in reply to Mr. Mill, and the experience of every general election, are sufficient to answer the first question in the affirmative. The answer to the second entirely depends upon the state of civilization and independence to which we have arrived; and it seems to me that in the days of standing armies, of an elaborate Poor Law, of State interference in education, of the overwhelming influence of wealth and the Priesthood, we have *not* arrived at that stage of general advancement and independence of thought and action in which we ought to give up so great and immediate a benefit to thousands as real freedom of voting, for the infinitesimal advantage to the national character which might be derived from the independent and open voting of the few who would feel it compatible with their duty to their families to struggle against unfair influence and unjust intimidation.

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Government Aid to Science

In a pair of letters entitled "Government Aid to Science" printed in the Nature issues of 13 and 20 January 1870, respectively, Wallace horrified the scientific community by suggesting that most scientific research should not be funded by the government.

S157

I venture to hope that you will allow me space in your columns to express opinions on this subject which are not popular with scientific men, and which are evidently opposed to your own views as indicated in your recent article on Science Reform.

The public mind seems now to be going mad on the subject of education; the Government is obliged to give way to the clamour, and men of science seem inclined to seize the opportunity to get, if possible, some share in the public money. Art education is

already to a considerable extent supplied by the State, – technical education (which I presume means education in "the arts") is vigorously pressed upon the Government, – and Science also is now urging her claims to a modicum of State patronage and support.

Now, sir, I protest most earnestly against the application of public money to any of the above specified purposes, as radically vicious in principle, and as being in the present state of society a positive wrong. In order to clear the ground let me state that, for the purpose of the present argument, I admit the right and duty of the State to educate its citizens. I uphold national education, but I object absolutely to all sectional or class education; and all the above-named schemes are simply forms of class education. The broad principle I go upon is this, – that the State has no moral right to apply funds raised by the taxation of all its members to any purpose which is not directly available for the benefit of all. As it has no right to give class preferences in legislation, so it has no right to give class preferences in the expenditure of public money. If we follow this principle, national education is not forbidden, whether given in schools supported by the State, or in museums, or galleries, or gardens, fairly distributed over the whole kingdom, and so regulated as to be equally available for instruction and amusement of all classes of the community. But here a line must be drawn. The schools, the museums, the galleries, the gardens, must all alike be *popular* (that is, adapted for and capable of being fully used and enjoyed by the people at large), and must be developed by means of public money to such an extent only as is needful for the highest attainable *popular* instruction and benefit. All beyond this should be left to private munificence, to societies, or to the classes benefited, to supply.

In art, all that is needed only for the special instruction of artists, or for the delight of amateurs, should be provided by artists and amateurs. To expend public money on third-rate prints or pictures, or on an intrinsically worthless book, both of immense value on account of their rarity, and as such of great interest to a small class of literary and art amateurs and to them only, I conceive to be absolutely wrong. So, in science, to provide museums such as will at once elevate, instruct, and entertain all who visit them is a worthy and a just expenditure of public money; but to spend many times as much as is necessary for this purpose in forming enormous collections of all the rarities that can be obtained, however obscure and generally uninteresting that they may be, and however limited the class who can value or appreciate them is, as plainly, an unjust expenditure. It will, perhaps, surprise some of your readers to find a naturalist advocating such doctrines as these; but though I love nature much I love justice more, and would not wish that any man should be compelled to contribute towards the support of an institution of no interest to the great mass of my countrymen, however interesting to myself.

For the same reason I maintain that all schools of art or of science, or for technical education, should be supported by the parties who are directly interested in them or benefited by them. If designs are not forthcoming for the English manufacturer, and he is thus unable to compete with foreigners, who should provide schools of design but the manufactures and the pupils who are the parties directly interested? It seems to me as entirely beyond the proper sphere of the functions of the State to interfere in this matter as it would be to teach English bootmakers or English cooks at the public expense in order that they may be able to compete with French *artistes* in these departments. In both cases such interference amounts to protection and class legislation, and I have yet to learn that these can be justified by the urgent necessity of our producing shawls and calicoes, or hardware and crockery, as elegantly designed as those of our neighbours. And if our men

of science want more complete laboratories, or finer telescopes, or more expensive apparatus of any kind, who but our scientific associations and the large and wealthy class now interested in science should supply the want? They have hitherto done so nobly, and I should myself feel that it was better that the march of scientific discovery should be a little less rapid (and of late years the pace has not been bad), than that Science should descend one step from her lofty independence and sue *in formâ pauperis* to the already overburthened taxpayer. So if our mechanics are not so well able as they might be to improve the various arts they are engaged in, surely the parties who ought to provide them with the special education required are the great employers of labour, who by their assistance are daily building up colossal fortunes; and that great and wealthy class which is, professionally or otherwise, interested in the constructive or decorative arts.

I maintain further, not only that the money spent by Government for the purposes here indicated is wrongly spent, but also that it is in a great measure money wasted. The best collectors are usually private amateurs, the best workers are usually home students or the employés of scientific associations, not of governments. Could any Government institution have produced results so much superior to those produced by our Royal Institution, with its Davy, Faraday, and Tyndall, as to justify the infringement of a great principle? Would the grand series of scientific and mechanical inventions of this century have been more thoroughly and more fruitfully worked out, if Government had taken science and invention under its special patronage in the year 1800, and had subjected them to a process of forcing from that day to this? No one can really believe that we should have got on any better under such a *régime*, while it is certain that much power would have been wasted in the attempt to develop inventions and discoveries before the age was ripe for them, and which would therefore have inevitably languished and been laid aside without producing any great results. Experience shows that public competition ensures a greater supply of the materials and a greater demand for the products of science and art, and is thus a greater stimulus to true and healthy progress than any Government patronage. Let it but become an established rule that all institutions solely for the advancement of science and art must be supported by private munificence, and we may be sure that such institutions would be quite as well supported as they are now, and I believe much better. If they were not, it would only prove more clearly how unjust it is to take money from the public purse to pay for that which science-and-art-amateurs would very much like to have, but are not willing themselves to pay for.

The very common line of argument which attempts to prove the wide-spread uses and high educating influences of art and of science, are utterly beside the question. Every product of the human intellect is more or less valuable; but it does not therefore follow that it is just to provide any particular product for those who want it, at the expense of those who either do not want, or are not in a condition to make use of it. Good architecture, for instance, is a very good thing, and one we are much in want of; but it will hardly be maintained that architects should be taught their profession at the public expense. The history of old china, of old clothes, or of postage stamps, are each of great interest to more or less extensive sections of the community, and much may be said in each case to prove the value of the study; but surely no honest representative of the nation could vote, say, the moderate sum of a million sterling for three museums to exhibit these objects, with a full staff of beadles, curators, and professors at an equally moderate expenditure of £10,000 annually, and a like sum for the purchase of specimens. But if we once admit the right of the Government to support institutions for the benefit of any class of students or

amateurs however large and respectable, we adopt a principle which will enable us to offer but a feeble resistance to the claims of less and less extensive interests whenever they happen to become the fashion.

If it be asked (as it will be) what we are to do with existing institutions supported by Government, I am at once ready with an answer. Taking the typical examples of the National Gallery and the British Museum, I maintain that these institutions should be reorganised, so as to make them in the highest degree entertaining and instructive to the mass of the people; – that no public money should be spent on the purchase of specimens, but what they already contain should be so thoroughly cared for and utilised as to make these establishments the safest, the best, and the most worthy receptacles for the treasures accumulated by wealthy amateurs and students, who would then be ready to bestow them on the nation to a much greater extent than they do at present. From the duplicates which would thus accumulate in these institutions, the other great centres of population in the kingdom should be proportionately supplied, and from the Metropolitan centres trained officers should be sent to organise and superintend local institutions, such a proportion of their salaries being paid by Government as fairly to equalise the expenditure of public money over the whole kingdom, and thus not infringe that great principle of equality and justice which I maintain should be our guide in all such cases.

This communication will doubtless call forth much opposition, but I trust it will also elicit the support of some of those eminent scientific men, who I know hold similar general views, and who are so much better able than I am to explain and support them.

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I cannot but feel flattered that my letter on this subject should have been thought so dangerous as to require a leading article in the same number by way of immediate antidote, but I must beg you to allow me to correct one or two errors into which you have fallen as to the views I really hold, and which it seems I failed clearly to express. You say, you "understand Mr. Wallace to mean that the main result of cultivating science is merely the gratification of those directly engaged in the pursuit, and that they who do not take this personal interest in it derive little or no benefit from it."

The first half of this passage does express, though imperfectly, what I believe to be the truth; the latter half expresses the exact opposite of what I have ever thought or intended to write on the subject. The main result of the cultivation of science I hold to be, undoubtedly, the elevation of those who cultivate it to a higher mental and moral standpoint; while the secondary, but not less certain result, is the acquisition of countless physical, social, and intellectual benefits for the whole human race. But if these are the *secondary* and not the *primary* results of cultivating science, it seems to me to be radically unsound in principle, and sure to fail in practice, if by means of any system of State support we seek to find a short cut to these secondary results.

The only logical foundation for advocating the furtherance of scientific discovery by the expenditure of public money, would be the belief that science can be most successfully pursued by those whose chief object is to make practical and valuable discoveries; whereas the whole history of the progress of science seems to me to show that the exact opposite is the case, and that it is only those who in a noble spirit of self-sacrifice give up

their time, their means, even their lives, in the eager and loving search after the hidden secrets of Nature, who are rewarded by those great discoveries from which spring a rich harvest of useful applications.

One more point. I do not admit that it is just to tax the community for all the Government institutions you name, but in the short space at my command I could not go into details. I have stated how I think some of these institutions require modification to make them accord with the fundamental principle of just government; and if that principle is a sound one, it is easy to see in what way the others should be dealt with. As an example I may indicate, that a detailed survey, like that of the large-scale Ordnance-maps, being primarily a boon to the landowners of the country, should not be *wholly* paid for by the public.

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The Immediate Relief of Bulgaria (S257b)

The so-called "April Uprising" in Bulgaria in 1876 caught the attention of the public in the United States and Europe, with general outcries of condemnation resulting. Wallace contributed some related comments in a letter printed in the 8 September 1876 issue of The Daily News (London) (and some other newspapers).

Sir, – In the resolutions passed at the various meetings now being held throughout the country the point which presses most for immediate action seems to be generally overlooked. I refer to the rescue of the Bulgarian population from the barbarities and persecutions they are even now suffering, and from the famine that must be surely overtaking them; and, what is equally important, to some efficient protection against the imminent danger of worse atrocities than ever at the hands of the irregulars of the Turkish army on their return from Servia. Allow me to suggest that an immediate and universal expression of opinion is needed (embodied, if thought fit, in petitions to the Queen), to the effect that our Government should at once join Russia in the occupation of Bulgaria with a sufficient body of troops, for the sole purpose of efficiently protecting the remnant of the cruelly oppressed Bulgarian people till such time as the Great Powers of Europe decide on the future status of the country. Bulgaria is now in the hands of assassins and robbers, and its own rulers profess their inability to help her. Let us declare, also, that if our Government refuses in this matter to represent the wishes of the people of England, and Russia alone gives that succour to the oppressed which common humanity demands, we will not allow her to be interfered with. Let us express our wishes on this point with sufficient unanimity, and the Ministry will hardly refuse to carry them into effect. – Alfred Russel Wallace, Dorking.

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The Evils of the Drink Traffic and Their Remedy (S266b)

Wallace's scheme was printed in the 4 January 1877 issue of The Daily News.

Sir, – Will you allow me to make a few remarks on this important subject, suggested by your article of Saturday last, though the question is one to which I have long given attention?

Mr. [Robert] Lowe's treatment of the problem appears to me to be strangely one-sided, inasmuch as it omits to deal with some of its most essential characteristics. He assumes that the only grounds for special legislation and a special licensing system for this trade as compared with others is the fact that its customers are apt to be disorderly, and that it is therefore necessary to have power to deal with them peremptorily and effectually. If this were an adequate statement of the points in which the liquor traffic differs from any other, we might admit Mr. Lowe's proposed treatment of it to be logical. But surely it differs from all other trades in far more essential features. Its customers are truly apt to become disorderly, but that is not all; they are also unfortunately apt to become paupers, criminals, or lunatics. Those who have the best means of knowing the facts trace at least half the crime, half the pauperism, and a considerable portion of the insanity of the country, directly or indirectly, to drink. The judges, the clergy, and the medical profession are almost unanimous on these points; and as pauperism, crime, and insanity are not only vast evils, but are evils which the Legislature, notwithstanding all its efforts, is as yet almost powerless to subdue, these effects of the drink traffic should surely be taken into account in any statesmanlike attempt to define its special character with a view to remedial legislation. Again, the passion for drink is itself a disease approaching insanity, inasmuch as its victims are unable to resist the temptation to indulge in it, however injurious and immoral they to know such indulgence under the circumstances to be. The mere sight of a drinking-house is to one of these men irresistible, and his scanty wages are spent, even though he knows his wife and children to be destitute and hungry. And yet Mr. Lowe makes the monstrous assertion that diminishing the number of public-houses – that is, diminishing the temptation – will not check drunkenness, though the fact that it does check both drunkenness and crime has been again and again demonstrated. Some striking evidence of this is given in Mr. Prettyman's recent work on "Dispauperization." A practical solution of the question, what is to be done, is, however, more important than the exposure of erroneous views, and to this I now address myself. I fully admit that Mr. Lowe's objections to allowing municipalities to purchase and manage public-houses are serious, and I believe conclusive; but there is another and a far better mode of carrying out the proposed reformation, and it has the great merit that it can at once be put in operation without any demand for legislative powers. I propose, then, that a society or association be established for the purpose of buying up public-houses and other licensed drinking-shops as occasion offers, and for carrying on the business of licensed victuallers in such houses, with the aim of discouraging the excessive use of stimulants. The society is to have no power to make or appropriate any profits out of the trade beyond 5 per cent. on subscribed or borrowed capital, all surplus profits being spent in increasing the accommodation and comforts of such houses, and making them places of cheerful and instructive recreation for those who frequent them. If our Bishops and clergy, our judges and magistrates, our wealthy legislators, country gentlemen, and philanthropists, with Sir Wilfrid Lawson and the United Kingdom Alliance, would cordially unite in the establishment of one such association to begin with, it is not too much to suppose that for such a great purpose a million of money could be easily raised, and the good work of furthering temperance and diminishing the vast mass of crime and pauperism that disgrace our country be at once begun. When after a few years the beneficial effects of the proposed

society's action were generally acknowledged, the Legislature might be applied to for a charter, and for compulsory powers to purchase licensed houses; and if any fears were entertained that so powerful a body might not adequately supply the public wants it might be made a condition that no house so purchased should be closed, or cease to sell the usual alcoholic drinks without the consent of the licensing magistrates. It must be remembered, however, that even without such legislative powers of compulsory purchase, the sphere of operations of the association might be continually extended – perhaps quite fast enough – owing to the fact of its being a permanent institution, always read to purchase but never to sell. And after a time we might expect that some of our great brewers would voluntarily sell their public-houses to the society, while others might be philanthropical enough to bequeath it such property. Others, again, thinking less of increasing their wealth than of furthering the public good, might be induced to adopt the principles of the association in the management of their own licensed houses; so that if the principle of such an association is sound, as there seems every reason to believe, it would assuredly grow and produce all its beneficial results, without agitation, without asking for any exceptional legislation, but submitting itself throughout to existing licensing laws and to the influence of an enlightened public opinion. It is hardly possible to exaggerate the social improvement which might result were the whole of our public drinking-houses regulated on the principles here advocated. There would be, of course, as at present, a variety of grades of such houses, some adapted for labourers, others for mechanics, and others again for the middle classes, but all alike would offer clean rooms with fire and light, together with cheap and wholesome food, without the necessity for purchasing intoxicating drink. In the lowest class of houses, a small cup of coffee or a glass of ærated water, at the cost of a halfpenny, might be sufficient to admit to its privileges. There would, of course, be no fixed limit to the sale of intoxicating drinks, except that it would be refused on the slightest approach to intoxication; and this alone would almost abolish the worst kind of drunkenness among the lower orders. In the better class of houses, newspapers and magazines, draughts, chess, and other innocent games would be supplied; and, as the profits would continue to be spent in improving the accommodation and increasing the attractions, libraries and billiard-rooms might be added; and every poor man, would ultimately find, instead of the debasing public-house, a place where he could obtain wholesome food and drink in great variety, as well as social intercourse, innocent amusement, and useful information. This, it appears to me, is the best mode of grappling with the great evil of drunkenness and its attendant vices. Its action will be so gradual, and its restrictions so mild and beneficial, that even habitual drinkers could hardly object to them; and I venture to think that the proposal, when fairly considered, will be found to be entirely free from every serious objection which has been urged against Mr. Chamberlain's Birmingham scheme or against Sir Wilfrid Lawson's Permissive Bill. – I am, Sir, yours truly, Alfred R. Wallace.

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Church Funds: How to Use Them (S374a)

Wallace's plan for Church disestablishment was outlined in the 19 January 1885 issue of The Daily News (London).

Will you allow one who belongs to no sect to make a few remarks on this question, and to put before your readers a suggestion which he believes will accord with the wishes and aspirations of many? As to the general principle of the complete severance of religious teaching from State control, I am quite in accord with the most ardent Liberationists; but at the same time I feel with Mr. Page Hopps that there is something in our national Church well worth preserving. The numerous parish churches and cathedrals form an important chapter of the unwritten history of our country, and I have as yet seen no practicable suggestion as to how, under complete disestablishment and disendowment, they are to be adequately utilized by the public and properly secured for posterity. The proposals of Mr. Hopps to widen and liberalize the existing Church I cannot but feel are quite impracticable. Fifty years ago such a plan might have worked, but the time for such a compromise is past.

The Established Church of England at the present day fulfils two distinct functions, the one religious but sectarian and of limited scope, the other secular and more widely beneficial to the community. These two functions are, moreover, to some extent antagonistic to each other. Every one must admit that the parish clergyman at his best, as an educated and disinterested gentleman who feels it to be at once his duty and his pleasure to become the friend and counsellor of his parishioners, who is the constant promoter of peace and goodwill among diverging classes and conflicting interests, and serves as the best medium of communication and sympathy between the wealthy and the poor, is a most important and valuable factor in our present social organization; but unfortunately his good influence is often greatly diminished, and sometimes completely neutralised, by defects in his education or his character, and by his sectarian prejudices. Let us then retain these educational and moral influences at their highest possible efficiency by severing altogether their connection with dogmatic religious teaching. This may, I believe, be effected by the following scheme:

Let the Church, as a religious sectarian body, be completely disestablished and disendowed, but let the whole of the revenues and buildings now belonging to it as the Church of the nation be placed under the control of a body of specially educated men, who shall hold them in trust and administer them for good of the entire population. These officers – who might properly retain the time-honoured name of rectors – should be rigorously selected for their high moral character, energy, temper, and intellect. They should be thoroughly trained in a good elementary knowledge of medicine, sanitation, law, and natural science, and should rank socially with the higher members of the liberal professions. Their duties would comprise much of the parish work of the existing clergy, but being unsectarian and secular it would be co-extensive with the population. Being specially educated, they would be able to give simple medical assistance to the poor in cases of pressing necessity, to assist them in misfortune, to protect them from oppression, and to aid them in securing their legal rights; and they would thus establish their position as true friends, both able and willing to help all in trouble with comfort, advice, and assistance. Another important function of the rector would be to guard and preserve the rights and privileges of the public. He would see that commons and highways were not encroached upon, that footpaths were kept open, that charity or common lands were used for the benefit of the poor, that nuisances were abated, and that unsanitary conditions were amended or brought to the notice of the authorities. He would be an ex officio member of the educational and administrative boards of his district, and when local self-government

becomes established the presence of such a body of men over the country might not improbably ensure success instead of failure.

The churches and other ecclesiastical buildings would be under the sole guardianship of the rectors in trust for the public; and they would be required to make arrangements for their utilisation both for religious and secular purposes. Any religious body desiring to have the use of the church for either a Sunday or weekday service would apply to the rector, and it would be his duty to decide what accommodation could be afforded to each, so as to deal equally and fairly with all. As most of the Nonconformists possess chapels of their own they would probably not desire the use of the parish church, except on special occasions, when their own building was expected to be inadequate, and thus all might in turn have the use of the public property. The church would further be available for lectures of an instructive or elevating character, as well as for public meetings connected with any important moral, social, or political movement. Generally speaking, the church or cathedral would be available for all purposes calculated to benefit the community, and would thus become intimately associated with the daily life and moral advancement of the whole people to a far greater extent than in any recent period of our history. All who thus used the buildings would contribute either by fixed fees or by voluntary collections to a fund for their maintenance, and thus we should ensure that these interesting historical edifices, often of grand proportions and admirable alike in design and in construction, should be handed down uninjured to our descendants. I think it is not too much to expect that under the guardianship of gentlemen of the character here indicated, our churches would become in the future a bond of union among sects and parties instead of, as heretofore, a source of contention and enmity.

Whenever disestablishment is effected I would strongly urge that the whole of the property of the National Church be preserved intact, and that compensation to all vested interests be made by way of terminable annuities, so that the next generation may receive its heritage undiminished. This vast property, if fairly distributed, would not only afford a liberal stipend to each of the parish rectors and their assistant curates, but would leave a large surplus which might be placed at the disposal of local committees, of which the rectors would be chairmen, to be expended in such ways as were held to be most conducive to the intellectual, moral, and social advancement of the entire community.

Under some such regulations and conditions as have been now sketched, the office of rector would be one of so much dignity and independence, would afford such ample scope for active benevolence, and so many opportunities for harmonising conflicting sects and parties, and for advancing true civilization, that it would be sought after by some of the very best men of the age. Such men, untrammelled by creeds or dogmas, and representing the highest moral and intellectual level of their time and country, would form centres of social illumination in every parish, and exert a beneficial influence which can hardly be exaggerated.

Religion would then be absolutely free, while all sects and parties without distinction would again benefit by those endowments and institutions, which were originally intended to minister to the spiritual, intellectual, and physical needs of the whole people, and have only in comparatively recent times become the monopoly of a single religious sect.

Somewhat similar views to these were advanced in a paper on “A True National Church” in “Macmillan’s Magazine” some ten or twelve years ago, and I now again bring them before the public, in the belief that the time is ripe for them. I sincerely trust that the Liberation Society will on consideration see the necessity of a programme which is

constructive as well as merely destructive, and I submit that by adopting some such scheme as I have here suggested they will gain support from many thoughtful Liberals who, while convinced that disestablishment is right in principle and must surely and quickly come about, yet look forward with dread to the inevitable scramble for the property of the Church and the almost inevitable neglect, misuse, or destruction of many of those buildings which have been so intimately associated with our intellectual and moral progress as a nation, and which it is at once our interest fully to utilise for the public benefit and our duty to hand down unimpaired to our posterity.

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The Condition of the Working Man (S385a)

Wallace often questioned statistics offered up by authorities. In this letter, printed in the 28 January 1886 issue of The Daily News (London), he targeted opinions of Robert (later Sir Robert) Giffen on the level of wages earned by working men.

Sir, – In your article yesterday on Mr. Giffen's paper on the condition of the working classes you accept his facts and figures as substantially correct, while pointing out that certain considerations may diminish the value of the improvement claimed. There are, however, many persons who, like myself, doubt the correctness both of the figures and the reasoning adopted by Mr. Giffen. We maintain that he has only set forth one side of the case, and that side not accurately, while he has altogether omitted some important considerations which render his conclusions worthless. I ask you, therefore, to allow me to adduce a few facts bearing on this important point.

In his former paper Mr. Giffen made the increase in the wages of skilled labourers from 20 to 100 per cent., and he now maintains that the higher figure is nearer the truth than the lower one; but he has said nothing whatever about the cases in which even money wages have actually decreased, and these affect, as I shall show, a considerable body of workers. A gentleman, who has close relations with large numbers of working men and women, has taken the trouble to collect from them the actual wages they received from forty to fifty years ago and now, and I have the results before me. The wages are reckoned by the week, and though the hours are less, because it is not now the custom to work on Saturday afternoon, this is no money gain to the mechanic. Out of thirty-seven trades which are tabulated, the earnings of fourteen have increased and those of the rest have diminished. The increase (from 1835 to 1884) has varied from 12½ to 30 per cent., while the decrease has been very much greater, most of these, however, being paid by piecework, and therefore not necessarily representing the same decrease of earnings. As examples, I may mention French polishers, 30 per cent. lower; boxmakers, upholsterers, chairmakers, turners, wood and stone carvers, artificial flower-makers, and some others, who work from 20 to 50 per cent. lower now; while another extensive class, including ironers, shirt-makers, card board and matchbox makers, trunk makers, ulster and trouser finishers, &c., are now paid from 30 to 150 per cent. less than at the earlier period. The large class of seamstresses are at the present day far worse off than when Hood wrote the "Song of the Shirt," their actual payment being much lower, while their rent is enormous-

ly higher. In Hood's time a shilling was paid for work for which they now receive fourpence! It is also the general belief that work is more uncertain and more difficult to get now than fifty years ago, and that the nominal wages are not so generally earned in full as they were then. Surely an estimate of the "condition of the working classes" which leaves out of account altogether those whose condition has deteriorated is not of much value; while the omission of all reference to the greater struggle and competition, and the consequently larger proportion of time lost in getting work, is hardly less important.

In order to magnify the supposed improvement in the condition of the people, Mr. Giffen makes the extraordinary statement that "meat fifty years ago was not an article of the workman's diet as it has since become." In opposition to this strangely incorrect statement, I have the evidence of an old man who fifty years ago was potman at the Three Tuns public-house in Blackheath village, and he states that at that time and for some years after as much as from 200 to 300 pounds weight of steaks and chops were cooked daily for the mechanics, labourers, and others working in the neighbourhood who came there at their dinner hour. It is very doubtful whether the same thing occurs now.

Another important consideration is that whereas now full two-thirds of our population live in towns, more than half lived in the country fifty years ago, and had many advantages which served as a real addition to their wages. Gleaning was then a common practice, and a labourer's family would often thus obtain from three to eight bushels of wheat, barley, peas, and beans. Milk was abundant, and good skim milk could be had at a halfpenny a quart, whereas now few labourers' children ever taste it. Eggs were much cheaper; rent was one-half, or one-third what it is now in towns; meat was about half its present price, and even bread, though sometimes very dear, was, on the average, very little dearer than now. (Average price of wheat, 1824–32, 61s. a quarter; 1870–80, 56s. 3d. a quarter.) Clothing, though nominally a little cheaper now, is so inferior in quality that many workmen maintain it to be really dearer. Everywhere in the country sticks could be gathered for firewood, watercresses and mushrooms gathered and sold, all which is now usually forbidden, under penalty of fine and imprisonment. The commons then had only been partially enclosed, and most labourers were able to keep geese, poultry, and pigs, and had gardens, in which they grew potatoes and other vegetables. All this is lost to the labourers who, to the amount of many millions, have been driven from the country to the towns, and the result is seen in the enormous increase of imported food, which we used to grow at home. Instead of being an indication of well-being, this increase is, to some extent, a measure of the peoples' loss, for they used to grow much of this food in addition to their money wages.

I also wish to point out, in conclusion, that Mr. Giffen has adopted a most unfair and misleading mode of estimating increase of wages, having taken (for mechanics) Manchester and Glasgow instead of London prices. But fifty years ago what may be termed "country prices" ruled in these towns, whereas now they approach much nearer to London prices. Why did he not take the London wages of fifty years ago as given by the contract prices of Greenwich Hospital and compare them with present London prices? These tables show that from 1820 to 1832 the wages of carpenters, bricklayers, masons, and plumbers, were 33s. a week, and they now average from 36s. to 40s. a week, an increase of from 10 to 20 per cent., instead of from 24 to 85 per cent. as given by Mr. Giffen.

I think that the facts and statements I have now adduced will show that Mr. Giffen has not approached the investigation of this subject with an impartial mind, and that neither his facts nor his conclusions are trustworthy. Perhaps if you, Sir, were to open

your columns to statements by working men and women who can give their own experience of life forty or fifty years ago and now, you would enable the public to form a more accurate judgement on this interesting and important question. – I am, yours, &c., Alfred R. Wallace, Godalming, Jan. 26th.

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letter concerning socialism, and Edward Bellamy's *Looking Backward* (S418)

In this untitled response to an enquiry, printed in the Land Nationalisation Society's publication Land and Labor, issue of November 1889, Wallace first committed himself publicly to the cause of socialism.

The *Star* recently contained the following paragraph, which went the round of the press. – "It is said that Dr. Alfred Russel Wallace has come to the conclusion that Land Nationalisation does not by itself offer a sufficient solution of the social problem, and has in consequence proclaimed himself a socialist."

In response to an enquiry, Dr. Wallace writes: – "I did not authorise the paragraph in the *Star*, and such a bald statement does not at all represent my views. Hitherto I have been doubtful whether Socialism in any form would be the future of humanity, and altogether sceptical of the possibility of carrying out any scheme of Socialism in the present phase of human development. But my opinions on both these points have been changed by a careful study of that remarkable book, *Looking Backward*, which for the first time – so far as I know – sets forth a practicable and altogether unobjectionable scheme of socialistic life, and solves *all* the difficulties of the problem in a most complete and satisfactory manner. My conclusions as to the practicability and advisability of such a scheme of social economy as Mr. Bellamy expounds in no way affects my advocacy of Land Nationalisation, which I believe to be – *so far as this country is concerned* – the indispensable preliminary to any realisation of Mr. Bellamy's views. I shall therefore continue to advocate it as earnestly as I have hitherto done, while I shall not advocate any of the less complete and more or less objectionable forms of Socialism usually propounded in this country."

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An English Naturalist. Alfred Russell Wallace Converted by Bellamy's Book. (S431)

In an anonymously-penned feature story printed in the 1 February 1891 issue of the New York Times, Wallace provided some perspective on his adoption of socialism.

Baltimore, Jan. 31. – Mr. Alfred Russell Wallace, the distinguished English naturalist,

in a letter recently written to Prof. R. F. Ely of Johns Hopkins University, called forth by reading the latter's "Political Economy," said:

"I am much pleased with it, especially with your fair and sympathetic treatment of Socialism. Some of your illustrations of the weakness of Socialism would have satisfied me a year ago, but it seems to me they are all well answered in Bellamy's 'Looking Backward.'

"From boyhood, when I was an ardent admirer of Robert Owen, I have been interested in Socialism, but reluctantly came to the conclusion that it was impracticable, and also, to some extent, repugnant to my ideas of individual liberty and home privacy. But Mr. Bellamy has completely altered my views in this matter. He seems to me to have shown that real, not merely delusive, liberty, together with full scope for individualism and complete human privacy, is compatible with the most thorough Socialism, and henceforth I am heart and soul with him.

"It is, however, a long way to such a goal, and your book will, I think, help men to a knowledge of the evils that have immediately to be remedied. I cannot see how the greatest evils of our present system, involuntary idleness and consequent pauperism, can ever be got rid of under the system of unrestricted competition and capitalism with labor as a marketable commodity."

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Life in Our Villages (S439b)

A self-explanatory letter printed in The Daily News (London) issue of 16 September 1891.

Sir, – In his delightful article last Friday, your Special Commissioner writes as follows: "Away to the left, the pretty cottages with their thatched roofs, their steps of unhewn rock, their windows full of geraniums and fuchsias, and their porches overgrown with autumn roses and canariensis, are hobbling down the broken pathway in a picturesque irregular line, their red chimney-stacks gently streaming out into the trees above the soft blue smoke of the wood fires." Compare this charming picture of an old English village – many of which still exist in every part of our country – with the modern cottages built by the farmer or speculator to pay five or seven per cent. interest. There are usually square brick boxes, built by contract in pairs or rows, and roofed with cold blue slates. The bricks are soft and porous, and the walls thin, so that these houses are cold and damp in winter, while to save timber and slates in the roof the eaves project only a few inches, giving an aspect of bareness and meanness that is absolutely oppressive. Such houses as these, which are spreading over the country by thousands yearly, destroy the charm of many a rural landscape; and so long as we act on the erroneous principle of providing dwellings for the people instead of allowing them to build for themselves in the manner most convenient for themselves, these ugly, inconvenient, and unhealthy productions of the jerry-builder will continue to increase.

In order to find the true remedy for this evil let us ask ourselves, why are the old cottages so invariably picturesque, so harmonious with the surrounding landscape in form and colour as to be a constant delight to the artist and lover of nature? The answer, I

believe, is, because they were the natural product of the time and locality, being built by the very people who were to live in them, with materials found in the district, and in the style which experience had shown to be at once the most convenient and the most economical. The first owners of these old cottages, the men who built them, were either freeholders or copyholders, or those who had obtained land on lease for several lives, and they were actually erected in part by the men themselves, with the assistance of their neighbours, the village carpenter and mason. The materials were obtained either from their own land or from the moors, wastes, and woodlands, which were then open for the use of all the inhabitants of the manor. The walls were of rough stone or of brick, or timber-framed with rough-hewn wood in the upper storey, forming those charming wood-framed houses of Surrey, Sussex, Hereford, and some other counties. In Dorset and Devon the walls were often of clay mixed with straw, called "cobble," and this makes a far warmer, drier, and altogether more desirable dwelling than the modern brick, as many of the old cottages, which have lasted for centuries, prove. The roof, framed with rough posts, and with poles for rafters, has a slight irregularity of outline very pleasing to the eye when compared with the rigid straightness, flatness, and angularity of roofs built with machine-cut timber; while the thick covering of thatch, broken by the small rounded dormer windows and with the broadly-overhanging eaves, is not only far warmer in winter and cooler in summer than any tiled or slated roof, but has the inestimable advantage to the labourer that he can repair it himself without having to pay a skilled mechanic.

Of course so long as our labourers and country mechanics have no land they can build no houses; but if we so arrange that every labourer, young or old, can obtain an acre or two of land on a permanent tenure, the cottage problem, which so much disturbs our legislators and philanthropists, will solve itself far better than they can solve it by legislative action. It will prove as easy as the three-acres-and-a-cow problem. A friend of mine was once talking to a labourer, and, having heard it stated over and over again that even if the labourer had the land he would in most cases have no money to buy the cow, he asked this man how he supposed it could be done, and received this answer, accompanied by a smile at the questioner's ignorance of such a very simple matter – "Why, sir, we usually gets a calf, and her grows into a cow." Just in the same way the labourer will get a house. He will first build a hut or cabin of the rudest description, and this by continual additions will grow into a comfortable cottage. An unmarried labourer could put up a hut with walls of turf or clay and roof of sods or heather-thatch, which would shelter him till he got his land into cultivation and could invest his profits in materials to add to or build his cottage. Your correspondent "Agricola," who has lived for twenty years and saved money on a two-acre holding, shows how this may be done even while paying a high rent. The result of this system of letting the people provide their own houses would be that we should have, as of old, individuality and variety in our rural cottages, with that harmony and picturesqueness which results from the use of local materials and hand-work in place of machine-work. We should have stone, or brick, or clay, or timber walls, thatched or tiled or stone-slabbed roofs, one-storey or two-storey houses, porches or verandahs, fantastic gables and chimneys, and those pleasing irregularities which result either from individual taste or the growth of a small house into a larger one by repeated additions; the whole set in a groundwork of shrubs and fruit trees, and a foreground of vegetables and flowers.

The man who had built such a cottage for his permanent home would love it as we all love our own handiwork, and would spend much of his time and savings in adding to its

convenience, comfort, and beauty. With such houses of their own, and with their garden and orchard, their cow, pigs, and poultry to attend to, the labourer would have constant interest and occupation at home, the public-house would remain empty, and the great drink question would perhaps cease to be so serious as it is now. Everyone admires the cottages of past centuries, and regrets their rapid disappearance. If we give our labourers and village residents of all kinds a secure tenure of land suitable to their respective needs, there seems no reason why they should not build for themselves as cosily and picturesquely as did their ancestors. There are many and cogent reasons why we should do this as speedily as possible. By this means alone we shall be able to repopulate the rural districts and relieve the terrible pressure of competition in our towns. By this means also we may hope to destroy that deadly curse of poverty, the increasing amount of which may be estimated by the terrible statement of the Registrar-General that the deaths which occur in English workhouses have steadily increased from 5.6 per cent. of the total deaths in 1875 to 6.9 of the total in 1888. By this great reform we shall give our workers the best incentive to sobriety and industry – the sure prospect of a homestead of their own in which they may live in comfort and security relieved from the dread of ending their days in the workhouse. This, too, will enable our cottages to grow for their own use, or for sale, abundance of bacon, butter, poultry, eggs, and fruit, in the place of the many millions worth of these articles we now import from abroad. And, lastly, this will perhaps save us from the crowning disgrace of covering our beautiful land with the very ugliest of houses for our labourers' use, and of thus destroying, for some generations to come, much of the picturesque charm of rural England. – Yours, &c., Alfred R. Wallace, Parkstone, Dorset.

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A Plea for Mercy (S446aa)

Wallace's was one of several letters printed under this title in the 15 March 1892 issue of The Daily News (London).

Sir, – Seeing in your paper to-day Mr. Robert Buchanan's letter with the above heading, I beg leave most earnestly to support his plea. If the facts were as stated by Mr. Buchanan, not only must the offence of these men be reduced to manslaughter, but to manslaughter not of the worst kind, since it was probably committed in self-defence against men who themselves began the affray by an illegal assault. For though gamekeepers discovering poachers in the act of pursuing or killing game are legally entitled to take them into custody, they are not, I believe, entitled to use violence of any kind unless the poachers refuse to submit after being called upon to do so. But gamekeepers are so systematically supported in all their acts of violence against poachers by magistrates who are usually themselves game preservers, that they look upon poachers as men who are altogether outside of any protection by the law, and who may therefore be attacked and violently assaulted without notice and with complete impunity as regards any action of the law against themselves. I presume the three men were personally known to the keepers, in which case there was no justification whatever for assaulting them, as they could have been legally apprehended the following day. Believing that our game laws are utterly immoral and unjustifiable, I earnestly support Mr. Buchanan's plea, not for mercy only,

but for such a moderate term of imprisonment as will satisfy the demands of justice, giving to the condemned men the benefit of every reasonable doubt in the case. – I am, &c., Alfred R. Wallace.

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The Alleged Increase of Poverty (S510a)

A long letter to the Editor printed in the 5 March 1895 issue of The Daily Chronicle (London).

Sir, – Your correspondent C. N. Nicholson, in your issue of the 15th inst., asks for some tangible evidence that poverty has increased during the last thirty or forty years notwithstanding the decrease of official pauperism. With your permission I will adduce certain well-established facts which seem to me to demonstrate that such an increase has taken place, notwithstanding the great increase of wealth and luxury, and of the well-being of certain classes of the community.

In 1885 I published (in my *Bad Times*, p. 49), a diagram showing the comparative increase, from 1850 to 1882, of population, of indoor paupers, and of wealth as indicated by the income tax. The two former showed curves which were roughly parallel, indicating a tolerably fixed proportion, while wealth exhibited an increase more than three times as great. I also showed that the decrease in outdoor paupers, chiefly in certain London unions, was coincident with a great increase in charitable organisations, which were admitted to have taken the place of outdoor relief and to have alone rendered its decrease possible. The *Charities Register* of 1885 showed that there had been formed in the preceding twenty years no less than 132 charitable institutions of a general character, besides large numbers of new local charities in various London districts. An examination of *Low's Handbook of London Charities* for 1894-95 shows that this increase has continued, since I find the record of nearly fifty institutions for purely charitable purposes which have been established since 1883, and this does not represent the total number, since the date of origin is not always given. But these entirely new institutions constitute but a small portion of the increase of charitable work in London, which is mainly due to the continuous growth of the older and better-known institutions. Dr. Barnardo's *Homes*, for example, beginning on a very small scale in 1866, have steadily grown, till now 5,000 children, who would otherwise be paupers, are supported, educated, and started in life either at home or abroad; and the *Church of England Society for Providing Homes for Waifs and Strays*, established only in 1882, has increased year by year till it now supports nearly 2,000 children.

There are in London at least forty other institutions of like character, each supporting from over 250 to 1,000 children, widows, or aged poor, besides about fifty others having each from 50 to 250 inmates. There are also a considerable number of smaller orphanages, almshouses, and hospitals, as well as numerous charity-schools and reformatories, having a total of about 8,000 inmates. Then we have the enormous recent extension of societies for giving free meals to school children and adults, the number of such meals given in one year in London being about two millions. Beyond all this, and entirely without any public record, is the ever-flowing tide of individual charity, largely adminis-

tered through the clergy of all denominations in the poor districts of every part of London, or by ladies and others who devote their time and a portion of their means to the relief of distress.

This continuous increase of charity is further augmented by a never-failing supply of legacies. In the year 1893 more than half a million sterling was bequeathed to various charities in London, exclusive of other amounts left for educational or religious purposes. Probably an approximately equal amount is now bequeathed every year, and this alone must lead to a continual increase in the vast sum total of London charity. The direct relief of distress through the agency of the *Salvation Army* and of the *Charity Organisation Society* is not taken account of in this enumeration of London charitable work, no estimate of the amount of relief afforded by these institutions being given in any of the works at my command.

The total amount of the charitable relief afforded by the various agencies here referred to is very great. A rough addition of the more important, as given in *Low's Handbook*, shows that over 40,000 children, widows, and aged or destitute persons are permanently maintained by them; while it is probable that a much larger number – perhaps twice or thrice as many – receive temporary assistance calculated to save them from actual destitution, from pauperism, or from suicide. If this estimate is anywhere near the truth, it shows us that the gross numbers of the recipients of private or unofficial charity are not very much less, and may be considerably greater, than those relieved by the guardians and alone termed "paupers" by persons whose aim appears to be to conceal the real facts as to the condition of the people. The London official paupers in January 1894 amounted to 122,840, not including those relieved in the casual wards. If, now, we add to the 40,000 persons who are entirely provided for in the various private charitable institutions, only a little more than twice as many who receive partial or temporary relief in their own homes – surely not an extravagant estimate, and very likely much below the truth – we arrive at the conclusion that official pauperism only represents one-half of the extreme poverty and destitution that is actually relieved; while there still remains the indisputable fact that an unknown multitude are constantly dying from the direct or indirect effects of insufficient food, clothing, rest, and fresh air. The facts here given demonstrate that private charity has very rapidly increased, during the last thirty years especially, while the number of official paupers has diminished very slowly; whence it follows that there has been a considerable increase in London poverty, and probably also in that of all our great cities and other centres of dense populations.

The unwelcome conclusion thus reached is supported in a striking manner by the statement of the Registrar-General as to the continuous increase of deaths in workhouses and other public institutions in far greater proportion than that of the general mortality. In his fifty-first annual report (1888), at page 71, he says: – "The proportion to total deaths was **6.9** per cent. in workhouses, **3.4** in hospitals, and **1.1** in lunatic asylums. The proportion of deaths recorded in workhouses, which steadily increased from **5.6** per cent. in 1875, to **6.7** per cent. in 1885, further rose after a slight decline in 1886 and 1887, to **6.9** per cent. in 1888." On referring to the latest report issued (1893), we find that the increase has continued, the proportion in that year being **7.12** per cent.

This refers to the whole of England and Wales; but in London the increase is still more alarming. In the earliest year of which I have the official report (1865), the deaths in London workhouses were **9.1** per cent. of the total deaths. From 1881 to 1888 the proportion varied from **12.2** to **13** per cent.; in 1891 it had increased to **13.8** per cent., and in

1893 to **15.2** per cent. During the same period the deaths in workhouses, hospitals, and lunatic asylums combined, increased from **16.5** per cent. in 1865, to **25.5** per cent. in 1893.

The significance of these facts will perhaps be more clear if we give the actual figures. The total deaths in London during the twenty-nine years increased from 73,460 in 1865 to 89,707 in 1893, or about **21** per cent. During the same period the deaths in workhouses increased from 6,715 in 1865 to 13,624 in 1893, or more than **100** per cent.! Equally suggestive is the fact that during the same period deaths by suicide in London have increased more than **59** per cent., or nearly three times as fast as the general mortality; for although many causes drive men to this mode of death, there can be no doubt that one of the most potent is destitution or the dread of destitution – the terrible strain caused by inability to procure the barest necessaries for wife or children.

The interpretation of these weighty facts – the continuous increase of deaths in workhouses and by suicide at a far greater rate than the general mortality – seems to me perfectly clear. The ever-increasing intensity of the struggle for life – for a competence among the middle, and for bare existence among the lower classes – inevitably leads to the crowding-out of aged, feeble, or diseased workers, who thus find it more and more difficult to support life. Hence it is that, despite all the efforts of philanthropy, and the most lavish expenditure in charity, an ever-increasing proportion of the aged poor drift into the workhouse, the hospital, or the lunatic asylum, where they end their wretched lives; or, in dread of this hateful consummation, prefer a voluntary death. Many, however, will accept neither alternative, and thus, week by week, winter and summer alike, large numbers die in their miserable attics or cellars, either from direct starvation or as the inevitable result of long-continued privation of all the conditions essential to a healthy existence. And all this goes on in the midst of the ever-increasing wealth and luxury of the upper classes, of which it is really the logical sequence.

In view of the two classes of facts now briefly summarised, viz. – the great and continuous increase of private charity, resulting in a total aggregate probably not inferior to that of official pauperism, and the coincident increase of deaths in workhouses and by suicide – facts perfectly well known to all who care to know them – it is, in my judgment, little less than criminal to set forth with all the weight of authority the decrease in Official Pauperism as indicating a diminution of poverty and a real amelioration of the condition of the whole people. The actual facts, if they could be brought home to the public mind, would serve as a veritable "handwriting on the wall" denouncing the rottenness of our whole social system. The younger generation of workers whom we have been educating and enfranchising, are beginning to ask why these things are. The time has come when our legislators and politicians *must* grapple with the fundamental causes which permit this mass of unspeakable human misery to continue in one of the richest – if not the richest – country in the world. If they persist in shutting their eyes to the facts, or in declaring that they have no remedy for them, they will assuredly bring about their own destruction as utterly incompetent rulers.

To myself, the rapid spread of Socialism affords the only gleam of light amid the pervading darkness. Socialists, at all events, believe that in a rich country with an industrious and skilful population no man, woman, or child should either die of starvation or linger out their shortened lives, as millions now do, in degradation and misery. They know that the labour expended each year, if properly applied and organised, would not only provide necessaries and comforts for all, but would also allow of ample leisure and a

full rational enjoyment of life; and they are convinced that it is *not* beyond the wit of man (as our present legislators would have us believe it is), to bring about this result. They are for the most part young, energetic, and earnest, and they have a great and inspiring ideal for which to work. They will doubtless make mistakes and meet with unforeseen difficulties, but every mistake corrected and every difficulty overcome will only the more surely point the way to ultimate success. The state of society is now so bad, so utterly rotten, that it cannot well be made worse. A continually increasing flood of charity has left things just as bad as it found them. Legislation on the old lines, of ameliorating symptoms without touching causes, has utterly failed.

The Social Problem should now be the one great subject of discussion and of future legislation. It must be dealt with on principles of fundamental justice rather than, as hitherto, of a narrow expediency. The most fundamental and far-reaching principle is that which has recently been set forth as in the direct road of Social Evolution – that in a country claiming to be free and civilised, equal opportunities for maintaining life and securing happiness should be afforded to all. Let the people demand assent to this great, and simple, and just principle from all who offer themselves as their representatives, so that legislators of the old school may give way to more hopeful and more earnest men. Thus alone will any real progress be made. – I am, &c., Alfred R. Wallace.

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letter to Keir Hardie[a] (S528)

An open letter on the occasion of a major labor congress, printed in the 25 July 1896 issue of Hardie's magazine The Labour Leader.

Dear Mr. Keir Hardie, – There is no brighter spot in the long vista of human progress than the international labour movement, with its effort at a realisation of the dreams of saints and sages – the true brotherhood of man, and peace between all the so-called "civilised" nations.

But to bring about this era of peace and fraternity something more than speeches and resolutions are required. The time for action has now arrived, and the best and most effective form of action against the existing systems of militarism and dynastic wars is that of passive resistance. The whole power of the aristocratic government of England in the seventeenth and eighteenth centuries was unable to overcome the passive resistance of the small and insignificant body of the Quakers, who not only refused to perform military service in any form, but also to take oaths, to pay tithes or church rates, or to conform to those ceremonial observances by which all other classes recognised those who were their rulers and claimed to be their superiors.

It is not, however, necessary or advisable to refuse military training. That will some day be useful in defending your own liberties. What is needed is that all organised workers should combine in a solemn promise, in the first place, never to use their arms against such of their fellow-workers who may be peacefully striving to gain their political or social rights; and, in the second place, never to use their arms against any other nation, the

[a] Keir Hardie (1856–1915), Scottish socialist and labor leader.

workers of which have joined the international movement, except within and in defence of their own country, or in cases where the workers of any country appeal for help against oppressors and tyrants.

I believe some of the German labour organisations have proposed or have adopted some such principle; and it seems to me that if in the present International Congress the English workers will use their influence to get some such principle discussed and ultimately accepted, a blow will be struck in the interests of labour and of peace which will be more effective than all the efforts of philanthropists and philosophers, because it will cut away at the very root the power of diplomacy and militarism to bring about dynastic wars.

The numerical power of organised labour in all the chief European countries is now so great and is so rapidly growing that by this system of passive resistance in military affairs it would be absolutely irresistible. Let it once be shown that the trained soldier will not fire upon his fellow-countrymen who are assembled for discussion or for combination in defence of their rights, and all attempts at governmental interference with the freedom of meeting and free discussion will cease. And let it further be realised that the men who will not fire on their fellow-countrymen are equally pledged not to fire on their fellow-workers of other countries, except in defence of their own hearths and homes or to assist the oppressed of other lands to gain their freedom, and no invasion of the territory of another country will occur since the rulers will not risk the *fiasco* of the fraternising of the two armies on the field of battle.

And any compulsion or punishment would be equally impossible. No government can or dare punish the whole body of the organised workers, members of which constitute the largest and best portion of the armies. And even if some compulsion or punishment is attempted, surely the workers of to-day, who are everywhere showing that they are not afraid of imprisonment or even of death in defence of their liberties, can do what the small and unpopular body of Quakers did more than a century ago.

At present there may be some difficulty owing to the fact that in many European countries recruits are so young and so often belong to the unorganised ranks of labour that they cannot be depended upon to join in the action of their fellows. But this is a matter than can soon be cured if the principle is once seriously and earnestly adopted. For every worker will bring up his children to look upon the principle of not fighting against his fellow-workers as constituting the very charter of his own liberties as well as his first duty to his order; and against his home teaching no influence of church, school, or authority will be able to prevail.

My excuse, if one is needed, for venturing to give advice in this matter is my lifelong interest in liberty and progress. As a boy I was a disciple and ardent admirer of Robert Owen; and though in middle life, while chiefly engaged in scientific work, I was influenced by the individualistic teaching of Herbert Spencer, I have now returned to my first love, and am a firm advocate of the co-operative commonwealth as giving the best promise of human happiness, and as affording the only prospect of a speedy cure for the terrible evils of existing capitalistic society. With earnest wishes for the success of the congress, and with the hope that it will sink all minor differences to further the definition and adoption of fundamental principles – of which that relating to militarism is certainly one of the most important.

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The Queen's Reign. Its Most Striking Characteristic and Most Beneficent Achievement. (S535ad)

One of a number of solicited responses printed in the September 1897 issue of Temple Magazine.

In my opinion, the most striking characteristic of the Queen's Reign is the continuous growth of ideas of broad Humanity and Justice, as shown in our factory legislation, in checks to injurious manufactures, and in the protection from cruelty of children and animals. We are also beginning to recognise – far too late – that the inferior races have a right to justice and fair treatment as well as ourselves. Though these principles are as yet very imperfectly recognised and but partially carried into action, there seems to be no doubt that they have attained to a force in public opinion far beyond that of any earlier period of our history, and they may be thus said to form a most gratifying characteristic of the present long reign.

Among the most beneficent achievements I would rank –

(1) Anæsthetics and the Antiseptic treatment of wounds.

(2) The spread of Arbitration in settling international differences; and our reparation of an act of national injustice, by the restoration of the Transvaal to the Boers in 1881.

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letters to *The Eagle and the Serpent* (S540 and S549)

Although he initially expressed approval of the intent of the Nietzschean magazine The Eagle and the Serpent, Wallace soon found himself at odds with its supporters' views on a number of occasions. The following two responses were printed in the 15 April 1898 and 1 September 1898 issues, respectively.

Nietzsche as a Social Reformer, Or, The Joys of Fleecing and Being Fleeced (S540)

Sir, – If Mr. Common's[a] statement of Nietzsche's teaching and the social reforms at which he aims, are accurate, then, even though some of his *methods* of obtaining social reforms may be good, the reforms themselves seem to me to be both impracticable and worthless, if they are not even retrogressions. Mr. Common tells us that Nietzsche is the apostle of "a true aristocracy," and of apportioning "advantages and disadvantages respectively to merits and demerits." If by "advantages" he means material superiority or greater wealth, and that the aristocracy of merit claim this superiority as their right, that alone would, in my opinion, show that they were *not* a true "aristocracy" and that they did *not* really "merit" what they claimed. Again, what is merit, and who is to decide on the merits and demerits of individuals? If it means intellectual, moral, or physical, superiority, or any combination of them, and if these qualities are fully exerted for the benefit of

[a] Thomas Common (1850–1919), Scottish editor, translator, and Nietszchean.

society at large, those who possess and so use their superiority will, under any rational condition of society, receive the greatest reward men can receive – the respect, honour, and affection of their fellows. But such men can only *prove* that they possess such superior qualities and that they are worthy of the honour they will receive, by working and living under *equal* conditions and *equal* advantages with their fellows. Without this absolute "equality of opportunity,"[a] there can be no possibility of accurately determining "merit and demerit" as regards society; hence, I maintain that the only object worth working for, as the first and essential stage towards utilising all the best powers and faculties of a nation for the common good, is, to bring about this "equality of opportunity." This, however, is simple justice, as between man and man. It is a fundamental axiom of ethics. It is not an "esoteric" doctrine, and it does not need to be upheld by "falsehood," as apparently does Nietzsche's system of aristocracy – and from falsehood, esoteric teaching, and a ruling aristocracy, nothing that is of permanent good ever has arisen or can arise.

I believe, absolutely, in truth, in justice, and in the free development of human nature, as the only and the essential methods leading to true social reform; and I therefore dissent as strongly as possible from Mr. Common's principles and methods. – Alfred R. Wallace.

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Darwinism in Sociology (S549)

Mr. Common's reply to his critics, while containing much with which most of us will agree, is yet full of obscurities, unfounded assumptions, or positive errors, some of which need to be pointed out, since they are fundamental to the question at issue.

There is little to object to in the first four principles he lays down, except that they imply, as clearly shown in (4) [*i.e.*, the fourth principle], an exclusive application to "physically and mentally superior individuals" instead of to all. This would lead, logically and practically, to a restricted and perhaps hereditary oligarchy of the stronger and less moral individuals, with the slavery or destruction of all who opposed them. This is more plainly indicated in (5) where "greater advantages" are claimed for the "better class of individuals," which really means the stronger, more cunning and more self-seeking, who, believing themselves to be the best, will enforce that belief, as to a considerable extent they always have done and do now. Mr. Common nowhere suggests that these "superior individuals" are likely to be restrained by any moral considerations in their efforts to obtain those "greater advantages" which he declares are their due.

Under such an oligarchy, once established, what chance would there be for the inventors, the thinkers, the artists, the moralists – the real aristocracy of the human race – except so far as they helped to increase the wealth and luxury and power of the "superior individuals." The slavery and tyranny, the assassinations and poisonings, of Greece and Venice, would inevitably be reproduced in any society whose fundamental principle was, that social and economic inequality was right, and that the stronger – physically or mentally – were justified in using their alleged inferiors for their own ends. Such a system

[a] Wallace was much impressed by Benjamin Kidd's 1894 book *Social Evolution* and that author's use of the term "equality of opportunity" in it. Wallace adopted the term and made frequent use of it thereafter.

would bring back the old bad days of – "Truth for ever on the scaffold, Wrong for ever on the throne" – and I challenge Mr. Common to prove that it could lead to anything else.

One would think that the evils of class-privilege and every other form of aristocracy had been sufficiently demonstrated in the past, to render it certain that this is *not* the road by which the future welfare of humanity is to be attained. True social excellence, to use Mr. Common's own term, can only be reached by utilising all the powers and capacities of the whole community, under the conditions best adapted to develope them; and no other means of doing this has been suggested but that "equality of opportunity" and of economic status which Mr. Common declares to be absurd. But his statement on this point, in his principle (5), shows that he entirely misunderstands the grounds upon which it is advocated. His argument as to agriculturists not giving the same chance to inferior plants and to weeds as to the crops they are raising, is a false analogy, and is entirely beside the question. The true analogy of men wishing to improve men, or rather of communities seeking their own highest well-being, is not that of the farmer and his crops, but of the plant-world seeking to improve itself, and having some power, though limited, of doing so. In that case it is quite certain that many of the best, in our opinion, that is the most beautiful and highly developed in flower and foliage would be exterminated, while the quickest growing trees and the coarsest and most vigorous weeds and climbers would alone survive. They would claim to possess the highest plant-excellence; they would therefore claim greater advantages of situation, soil, etc., and would only permit the existence of what they would term inferior types so far as not to interfere with their own well-being.

Again, when he says that semi-idiots should not have a university education, he equally misunderstands the principle of equality of opportunity. This does not mean the *same* education for all – that is the present absurd plan – but equal facilities for developing all that is best in their natures, so as to enable them to spend happy and useful lives; for there is probably no human being that could not attain to this condition by proper nurture and training. The greatest scientific worker and thinker of the century, Faraday, and the greatest artistic genius, Tinworth, were both rescued by a mere chance from a life of mean drudgery, to which hundreds and perhaps thousands of similar bright spirits are condemned. We claim, that complete equality of opportunity, including economic independence, is essential for the development and utilization of *all* the good and useful qualities of *every* human being and therefore for the true advancement of the race.

But perhaps the most erroneous and most vicious of Nietzsche's principles, according to Mr. Common, is that enunciated in the last sentence of (5) – "And it is still more absurd to advocate, . . . that the inferior class should be allowed to breed like vermin, and that their spawn should be supported at the cost of the better classes." In order to understand what this really means we want a tolerably accurate definition of the terms "inferior" and "better." We may take either of two extreme definitions – that the inferior are the criminal classes only, or that they are the working as opposed to the professional and propertied classes. If we take the latter and more popular definition we have to remark first that the facts as to who supports who, are the exact opposite of Mr. Common's statement, since the workers undoubtedly support the non-workers, and the children of the non-workers as well as their own. Secondly, it is generally admitted that the "better classes," as above defined, would either remain stationary in numbers or would even decrease if they were not recruited from the "inferior" class. The more rapid increase of these latter is therefore a benefit to the nation.

Now considering the alternative view, that the criminals are to be considered as the inferior class (compared by Mr. Common with vermin), who are not to be allowed to breed, we shall at once see that this is not only impracticable, but would be probably hurtful to society. It is a mistake to assume that those whom we now punish as criminals are essentially bad in nature, or are even, on the average, at all inferior to the majority of those who lead ordinarily respectable lives. Many highwaymen of the last century and some burglars of to-day, are men of exceptional ability who, under favourable conditions of education and nurture would become esteemed and useful members of society. A considerable proportion of the population of New South Wales, before the gold discoveries, consisted of released convicts and their descendants, and these were in no way inferior to the average of honest emigrants. So, the waifs and strays from the gutters of the East End of London, when rescued and trained by Dr. Barnardo, are found to be quite up to the average of respectable citizens. These examples support the view so strongly urged by Herbert Spencer, that what are termed the lower, the middle, and the upper classes are fundamentally alike, morally and intellectually. Their differences depend wholly on their environment and early education, and there is no reason to believe that the average of inherent vice or passion is greater in one class than in the other. But owing to the great differences in the environment and economic condition of the three classes their vices are exhibited in widely different ways, and those of the upper and middle classes, though equally great, do not so often bring them within the meshes of the law. The aristocratic bankrupts or gamblers, and the middle-class promoters or directors of bubble companies, are often worse morally than the bulk of our criminals, but they are not so often found out.

The idea that society can be improved by forcible interference with the increase of the various classes is one of the most wildly impracticable as well as one of the most dangerous and unnecessary of all the panaceas for remedying some of the evils of our social system while leaving the real causes of those evils absolutely untouched. The dread of too rapid increase of the population, always brought forward by the opponents of real social reform, is a mere figment of the imagination, which will altogether vanish under a rational organisation of society. The actual facts at the present day all point to such a result. Those who marry earliest and breed fastest, are those who are economically worst off – the casual and unskilled labourers; the higher class, of skilled labourers, marry later; the mercantile and professional middle classes later still; while the landed and capitalist aristocracy marry latest, and increase slowest or not at all. Yet, in face of these palpable facts, the objection is continually made, that if *all* are well-off and free from anxiety as to a comfortable living, population will increase so rapidly that *all* will soon sink to starvation-point. No reason whatever, founded on actual facts, is given for this statement, and some of the most important of the factors that would come into play and which would lead to an opposite result, are left wholly unnoticed. These factors I have set forth in an article on "Human Selection" in the *Fortnightly Review* (Sept. 1890). I can here only enumerate them. The first is the great principle established by Herbert Spencer from a survey of the whole animal kingdom, that individuation and reproduction are antagonistic, the fuller development of the brain and nervous system leading inevitably to a diminished rate of increase. Hence, when *all* are well educated and the mental and moral side of their nature fully satisfied, the animal passions and appetites will be less predominant than they are now. The surest way, therefore, to check the too rapid increase of population is, the full development and training of the intellectual and moral faculties, along with a corresponding training and useful exercise of the bodily organs.

The next important point is the fact pointed out by Galton, that every year's delay of the marriage period acts in a threefold manner in diminishing the rate of increase – by reducing the number of offspring, by lengthening the time between successive generations, and by reducing the number of generations alive at the same time.

The third and most important of all the factors is, that if women were better educated, more fully occupied, and absolutely secure of an independent life, a happy home, and congenial society, the forces which impel many of them to early marriages would be withdrawn, and the period of marriage would, on the average, be considerably delayed. Taking all these factors into consideration we see that the supposed difficulty of the population question vanishes altogether.

But another and still more important effect would result from the action of the same causes. There would arise a form of selection by which the best would tend to increase and the worst to be weeded out – a true "natural selection" by which the race would be continuously improved. This would result from the free action of woman's choice in marriage, so soon as she was economically independent. It is certain that the majority of women admire the healthy in body and mind, and the good rather than the bad. Failing to secure their ideal husband many women of independent means even now do not marry. When all were independent these would no doubt increase. Hence, there would be a greater range of choice for the women who wished to marry, and the result would be that those men who were deformed or diseased in body or in mind would not find wives and would leave no offspring. Hence would arise a continuous improvement of the race, far more certain and more beneficial than could be brought about by the compulsory or inhuman means so often advocated by neo-malthusians, and apparently suggested by Mr. Common as one of Nietzsche's principles.

Many other points call for notice, but I need here only mention that Mr. Common is in error when he refers to myself as having shown that "falsehood, imposture, and hypocrisy" play a large part in nature. He quite misunderstands the facts of mimicry and protective resemblances, which are wholly involuntary, and cannot therefore imply falsehood, etc. But this, and all other arguments drawn from the animal or vegetable kingdoms are valueless, because in social and civilised man the mental and moral nature rules over the physical; and as I have just shown, a new and higher kind of selection will come into action as soon as he learns how to subordinate the latter to the former, and how to so organise his social state as to satisfy the economic requirements of all. His power over the forces and products of nature is now so great, that, not only necessaries and comforts, but all beneficial luxuries could be enjoyed by every human being, with an amount of labour less than half of that now expended in giving hurtful luxury and idleness to some, while millions remain subject to a degree of want and misery greater probably than has ever been seen at any previous era in the world's history. The method of bringing about social excellence here suggested, works with nature, not against her. It depends upon the natural play of the higher qualities of human nature, and will therefore be both self-acting and efficient; while any forcible intervention of authority will be as certain to produce evil as it must be powerless for good.

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"The Wonderful Century." – A Correction. (S554)

Printed in the October 1898 issue of Land and Labour.

Almost every reviewer who has referred at all to the Appendix to my book, headed, "The Remedy for Want in the midst of Wealth," has stated, with more or less of contempt, that my remedy is "free bread." This mis-statement was to be expected in the case of the ordinary reviewer, but I certainly did not expect to find the same mistake in *Land and Labour*. On page 381 (the second of the Appendix) I say – "we must recognise the absolute inefficiency of the old methods of charity and other small ameliorations, *except as admittedly temporary measures*." I then propose the method indicated in my Address to the Land Nationalisation Society in 1895, which is – *Reoccupation of the Land, with the Organisation of Labour in Production for the Consumption of the Labourers*. In the next two pages I sketch the outlines of this proposal, and give reasons why it is certain to succeed; and I then point out that in Bellamy's new book, it is proved theoretically that it *must* succeed. *This* is my Remedy for Want, plainly stated; yet no single reviewer has yet noticed it. I then have a heading, *How to Stop Starvation*, and the first lines of this section are as follows: – "But till some such method is forced upon our legislators, the horrible scandal and crime of men, women, and little children, by thousands and millions, living in the most wretched want, dying of actual starvation, or driven to suicide by the dread of it – MUST BE STOPPED! I will, therefore, conclude with suggestions for stopping this horror *at once*; and also for obtaining the necessary funds, both for *this temporary purpose* and to carry out the system of co-operative colonies already referred to."

Then, for *this temporary purpose*, I propose *free bread*. And after describing exactly how it is to be carried out, I say (on p. 385) "Now, there are only two possible objections to this method of *temporarily* stopping starvation while *more permanent* measures are preparing," – and I then answer these objections. Yet such papers as *Literature*, *The Literary Guide*, and, alas! – *Land and Labour*, charge me with proposing *free bread* as the "Remedy for Want"!

To prevent misunderstanding *this time*, I have put a few of the expressions referring to the *temporary* character of this proposal, as opposed to the *permanent* remedy previously described, in *italics*, which are not used in the book. But, I submit that my statements on the matter are perfectly clear, and that no one who really reads them can possibly believe that I have deliberately proposed free bread as the only remedy I have to offer for that terrible condition of destitution and misery which I have described in my twentieth chapter. There could hardly be a more striking example of the carelessness of a good deal of modern reviewing. – Alfred R. Wallace.

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National Defence for Small Communities (S557a)

This response to an inquiry from Herman Heijermans, Jr. (1864–1924) was published in 1898 as part of an article entitled 'Holland's Militairisme' in

Volume Two of Heijermans' socialist journal De Jonge Gids (Amsterdam).

However great an evil may be a large Army and Navy, the loss of national independence would be a still greater evil; and in the present state of Europe, with all the Great Powers armed to the teeth, and their military rulers eagerly awaiting the time when they can use their vast armaments for further aggrandisement, it is to be feared that, in case of war, the independence of their smaller and weaker neighbours would not be respected. Against this danger, however, the most ruinous expenditure on army and navy would be of no avail, and it would therefore appear, at first sight, that no preparations for national defence are needful or would be of any use. But opposed to this view there are two important considerations. The first is, that in case of war between Germany and France or between either of those countries and England, it is certain that, were Holland and Belgium completely undefended, they would be overrun by one or both of the combatants, and would thus have to endure some of the worst horrors of war, and perhaps ultimate loss of national freedom. Some force therefore is necessary to preserve the inviolability of the frontiers.

In the second place, although the forces of a small country would necessarily be powerless against those of any of the great powers, yet they might be of the greatest importance as an ally; and the knowledge that any invasion of the frontier by one of the combatants would not only be vigourously opposed but would place the smaller country at once on the side of the enemy, would probably serve to safeguard the territory of the small nation.

But in order to serve these two purposes all that would be required would be a small but well-armed and well-trained body of soldiers, with a reserve of all adult males trained as they now are in your country. The whole question is, as to the size of the permanent army, and of that you are the best judges.

The greatest economy might probably be effected in the Navy, which should exist solely for coast protection. It seems to me to be madness for a small power to expend money on huge ironclad war-ships. All that is needed for defence are a few forts at critical points, thoroughly well armed: together with a sufficiency of small torpedo-vessels, and of torpedoes and mines at the entrances of all the ports. A very effectual defence may thus be obtained at a comparatively small cost.

But the agency to which I look with the greatest hope, is the extension of the feeling of social brotherhood among the workers of the different nations, leading them to refuse to invade other countries, or to fight at all except for purposes of national defence or in aid of nations justly struggling for freedom. This should be a fundamental principle of all Democratic or Socialistic associations; and when the armies of the great powers become permeated with it, peace and disarmament will follow, since kings and generals would never run the risk of bringing armies into the field when the majority of the soldiers would refuse to fire at their fellow men with whom they had no quarrel.

Till this state of feeling becomes general, I fear it will not be safe for the smaller nations to refrain altogether from military training and preparation for defence. As regards the standing army, however, the cost might be considerably reduced, and as regards the navy almost wholly abolished.

In conclusion, I wish to add that I have made no special study of this subject. I write only as a lover of peace and of human progress, and in response to the request you have done me the honour to send me. – Alfred R. Wallace, Parkstone, Dorset, England.

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America, Cuba, and the Philippines (S559)

Colonial rule is the subject of this wonderfully acrimonious letter printed in The Daily Chronicle (London) issue of 19 January 1899.

Sir, – Will you allow me a little space in your widely circulated paper to express the disappointment and sorrow which I feel – in common, I am sure, with a large body of English and Americans – at the course now being pursued by the Government of the United States towards the people of Cuba and of the Philippine Islands.

I do not for a moment intend to imply that we Englishmen have any right to criticise the action of the American Government on account of any better conduct of our own Government in past times. It must be admitted that we have acted in a similar, or even in a worse manner towards weak or inferior[a] populations – notably in South Africa, China, and India. Yet the *tu quoque* argument is in this case an especially weak one on account of the antecedents of the two nations. Politically, Americans look down upon us on account of our continued submission to a monarchical Government and a hereditary aristocracy, and, with these, to a long-established and powerful militarism. We have not the advantage of a "declaration of independence" recalling to our rulers those great principles of freedom and humanity which should be the foundation of all government – that all men have an equal right to life, liberty and the pursuit of happiness, and that Governments can only derive their just powers from the consent of the governed. From a nation which has twice fought and conquered for these great principles we have a right to expect a higher standard of international ethics than that which prevails in countries still to some extent dominated by aristocratic influences and feudalistic institutions; and many of us had hoped to see the great principles of the declaration of independence applied in the protection of the Cubans and Filipinos from external enemies, while leaving them free to establish their own internal government in accordance with their several national peculiarities of race, customs, language, and religion.

It is generally admitted that a people who fight strenuously for liberty against powerful oppressors deserve to obtain it; and that such a people should be presumed, till the contrary is proved, to be fit to possess it. Both the Cubans and the Filipinos have done this, and both with a large measure of success, which could never have been attained without courage, patriotism, and a considerable capacity for organisation and combined action. America's demand was that Spain should withdraw her army and officials from Cuba, giving the people complete independence. This was refused, and America declared war against Spain for the liberation of Cuba from terrible and inhuman oppression, and by the right of the conqueror has forced her to give up all her possessions in the West Indies. We had hoped that this war would be the one exception which we could adduce as a war waged solely for right and justice. Yet now that the last Spanish soldier has left Cuba an American army is still to occupy it, "to preserve order." That has always been the excuse

[a] Wallace uses the term "inferior" in a number of ways: meaning "smaller in number," "less civilized," "less organized (in both biological and societal contexts)," "of lower social class status," and, simply "lesser." In this instance it is not immediately clear which of these usages applies.

of conquerors, but it is hardly the method for liberators, because military occupation by an army differing from the natives in race, language, and religion, in customs and in prejudices, is the surest way to provoke disorder. Inevitable misunderstandings will lead to harshness and oppression on the one side, with resistance on the other; and this resistance will form the excuse for further restriction of liberty, leading probably to permanent occupation.

The case of the Philippines is in some respects even more regrettable than that of Cuba. The war there was only for the purpose of crippling the Spanish power and thus leading to an early peace. Considering the inferiority of race, the success of the natives against their Spanish rulers was even more remarkable than in the case of the Cubans, a large proportion of whom are of pure Spanish blood. In the Philippines the two higher native peoples, the Tagals and Bisayans, with numerous Chinese and Spanish half-breeds, constitute almost the whole civilised population, are fairly educated, and by their successful resistance to the established rule and military organisation of Spain have gained the right to freedom and self-government in their native land.

The Americans, however, claim "the rights of sovereignty obtained by treaty," and have apparently determined to occupy and administer the whole group of islands, with a population of over 6,000,000, against the will and consent of the people. They claim all the revenues of the country and all the public means of transport, and they have decided, according to the latest advices, to take all this by military force if the natives do not at once submit. Yet they say that they come "not as invaders and conquerors, but as friends," in order "to protect the natives in their homes, their employments, and their personal and civil rights," and for the purpose of giving them "a liberal form of government through representative men of their own race." But these people who have been justly struggling for freedom are still spoken of as "insurgents" or "rebels," and they are expected, apparently, to submit quietly to an altogether new and unknown foreign rule, which, whatever may be the benevolent intentions of the President, can hardly fail to become a more or less oppressive despotism.

It may be asked what else can the Americans do? They cannot allow Spain to come back again, or permit any of the European Powers to take possession of the islands, since having conquered Spain they are responsible for the future of the inhabitants. This will be admitted. But surely it is possible to revert to their first expressed intention of taking a small island only as a naval and coaling station, and to declare themselves the protectors of the islands against foreign aggression. Having done this, they might invite the civilised portion of the natives to form an independent government, offering them advice and assistance if they wish for it, but otherwise leaving them completely free. It might be advisable at first to leave the great island of Mindanao, mostly inhabitated by Mohammedans, to form its own separate government; and some guarantees might properly be asked for the fair treatment of the uncivilised portion of the population; such as the presence of a few American residents as protectors of the aborigines.

By some such method as here suggested, the great Republic of the West might aid in the production of a new type of social development adapted to the character of the Malayan race. They might thus benefit humanity by giving full play to the benign influences of freedom and responsibility in the case of one of the lower races, which have never had a fair opportunity of profiting by the example and the advice of a higher race in order to develop their own characters free from the depressing influence of even the most benevolent of conquerors.

In conclusion, I again emphasize the fact, that we, as a nation, have no right whatever to claim any superiority as regards our treatment of those less civilised people with whom we come in contact. Our conduct towards the Boers and Zulus in South Africa, the Burmese, and many of the hill tribes on our Indian frontier, and the Chinese in our wars growing out of the opium trade, has been certainly not better than what the Americans have done or are likely to do in Cuba and the Philippines. But many of us have always protested against our own unfair dealings with those inferior races, and have denounced the conduct of our Governments as unworthy of a civilised and professedly Christian people. And if we now venture to express our disappointment that our American kinsfolk are apparently following our bad example, it is because, in the matter of the rights of every people to govern themselves, we had looked up to them as being about to show us the better way, by respecting the aspirations towards freedom, even of less advanced races, and by acting in accordance with their own noble traditions and Republican principles.

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The Inefficiency of Strikes: Is There Not a Better Way? (S560)

Printed in The Labour Annual, 1899.

Has not the time come when the workers should cease to employ so rude, inefficient, and wasteful a method of improving their condition as by means of STRIKES? In most cases a strike effects little or nothing of a permanent nature, nothing but what may be lost within a year or two, nothing that tends to raise the whole body of the workers in any country. The strike may have been an essential weapon in the past – perhaps the only weapon the worker possessed. Now, however, all the higher grades of workers are better educated, better organised, and have higher ideals. They have learnt the benefits of co-operation and of union; they have accumulated funds which may be reckoned by millions; and to waste those funds in keeping thousands and tens of thousands of men idle during a strike is one of those economic and social blunders which, in their effects, are often worse than crimes. Instead of keeping men idle for months, in order to obtain a small and perhaps temporary advance in wages or reduction of working hours, would it not be wiser to adopt a totally different method, one which would be much more dreaded by the employers, because it would tend to produce a permanent, instead of a temporary, rise of wages. That method is, *competition with the employers instead of strikes against them*; and it is to be effected by saving and accumulating all the money now spent in keeping men *idle*, and, as occasion arises, using it for the purpose of acquiring shops and tools by which the unemployed in each trade may be gradually absorbed and kept at *work*. Then, step by step, wage-earners would be withdrawn from employers' shops or factories to work in those of their union. Even if, at first, some of these shops were not able to pay full rates of wages, still the men would earn *something* instead of *nothing*, and they could hardly earn less than the usual pay during a strike.

Of course this could not be done all at once, but only step by step; each step, however, rendering the next step easier. The great thing is, to adopt the principle of never spending money in keeping men idle when it is by any means possible to keep them at

work. The larger trade unions could probably carry out this method themselves after a few years' preparation, but to work it effectively a federation of unions would be needed; and when the need was clearly seen, this would soon be effected.

I venture to submit for the consideration of the workers that the principle of action here advocated is the only sound one. It alone tends in the direction of enabling them to become their own employers; while every step they take on this road, by withdrawing labour of all kinds from the cruel competition which compels men, and women, and children to work for the barest living rather than actually to starve, would inevitably raise the minimum as well as the maximum wage, and thus permanently benefit the whole body of their fellow-workers.

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The Causes of War, and the Remedies (S567)

Wallace's letter-essay on this subject first appeared in abbreviated form in French, but it was printed in toto in English a short time later (on 8 July 1899) in the London socialist newspaper The Clarion.

In response to a request from the Editor of *L'Humanité Nouvelle*, I here give my views, briefly, on the questions submitted to me. [These were: "(1) Is war among civilised nations still necessary on the grounds of history, right, and progress? (2) What are the effects of militarism – intellectual, moral, physical, economic, and political? (3) What is the best solution of the problems of war and militarism in the interests of the future civilisation of the world? (4) What are the most rapid means of arriving at this solution?"]

(1) Under the existing conditions of society in all civilised communities, and as a consequence of the principles and methods of government which prevail in them, war cannot cease to be more or less prevalent among them.

The conditions which almost inevitably lead to war are the existence of specialised ruling and military classes, to whom the possession of power and the excitements and rewards of successful war are the great interests of life. So long as the people permit these distinct and independent classes to exist, and – more than this – continue to look up to them as superiors and as necessary for the proper government of the country and for the effective protection of individual and national freedom, so long will these rulers continue to make wars.

All civilised governments, whatever may be their professions, *act* on the principle that extension of territory and the absorption of adjacent or remote lands, so as to increase both the extent of country and the population over which they have sway, is a good in itself, quite irrespective of the consent of the peoples so absorbed and governed, and even when the peoples are alien in race, in language, and religion. Although they may not openly avow their acceptance of this doctrine, yet they invariably *act* upon it, though in some cases they think it necessary to make excuses for their action. They declare that such conquest and absorption is necessary for the national safety, for the increase of trade, and for many other reasons. The majority of the workers, and of educated people who do not belong to the ruling or the military classes, however, do not accept this principle. They more or less decisively hold the opinion that governments can only justly derive

their power from the consent of the governed, and that all wars for territory and all conquests of alien peoples are wrong.

The reason of this difference of opinion is very simple. Every addition of territory, every fresh conquest even of barbarous nations or of savages, provides outlets and additional places of power and profit for the ever-increasing numbers of the ruling classes, while it also provides employment and advancement for an increased military class in first subduing and then coercing the subject populations, and in preparing for the inevitable frontier disputes and the resulting further extensions of territory. Wars and conquests and ever-expanding territories are thus found to be essential to their existence and continued power as superior classes. But the people outside these classes derive little, if any, benefit from such extensions, while they invariably suffer from increased taxation, either temporarily or permanently, due to increased armaments, which the protection of the enlarged territory requires. Almost without exception every war of modern times has been a dynastic war – a war conceived and carried out in the interests of the two great governing classes, but having no relation whatever to the well-being of the peoples who have been forced to fight each other. In every case the people suffer by the loss or disablement of sons, husbands, and fathers, by the destruction of crops, houses, and other property, and by increased taxation, due to the increase of armaments that always follows such wars even in the case of the victors. Hence the material and moral interests of the mass of the people of every country are wholly opposed to war, except in the one case of defending their country against invasion and conquest. They are therefore more open to the influence of moral and humane considerations, while they alone feel the numberless evils which war brings upon them. Except in very rare cases, a plebiscite fairly taken would decide against any other than a defensive war.

(2) To discuss the effects of militarism under the various heads suggested in the question would require much space and some special knowledge which I do not possess. That these effects have both good and evil aspects may be admitted. The evil effects have been often set forth and are sufficiently known, both in their vast extent and far-reaching consequences, while the greatest of them – the perpetuation of war and the desire for military glory – has already been alluded to. I will, therefore, confine my remarks to the partial good that undoubtedly exists in this fundamentally evil thing, chiefly for the purpose of showing that whatever good there is in it may be obtained in other ways which are as essentially humane, moral, and beneficial as war is essentially cruel, immoral, and hurtful.

The good that results from militarism arises wholly from the perfection of its organisation, of its training, of the habits of order, cleanliness, and obedience which the soldier soon learns are essentials to efficiency, from the social and brotherly life of the soldier, whether in camp or in the field, from the *esprit de corps* which grows out of its systematic organisation and companionship, leading to generous rivalry and to those deeds of heroism and self-sacrifice which are universally admired. And, further, every soldier learns by experience the marvellous power of organised labour under skilled direction to overcome what to the ordinary man seem insurmountable difficulties. He sees how foaming torrents or broad rivers can be rapidly bridged; how roads can be made over morasses or across mountains; how the most formidable and apparently impregnable defences are attacked and taken; and how a few bold men in a "forlorn hope," by the sacrifice of their lives, often insure the success of the army to which they belong. Many of the finest qualities of our nature are thus called into action by the soldier's training and during his struggle

against the enemy; and so greatly has humanity developed among us that it may be fairly argued that these good effects more than balance the evil passions of cruelty, lust, and plunder which even now are to some extent manifested in every great war, though to a far less degree than even 50 years back.

But every one of these good results of militarism could certainly be obtained by any equally extensive and equally skilful organisation for wholly beneficial purposes. If labour, where organised for military ends, is so effective in results and so beneficial as a training, it would be equally effective and equally beneficial when devoted to overcoming the obstacles to man's progress presented by nature to the production of the necessaries of civil life, to sanitary works for the preservation of health, and to everything that facilitates communication and benefits humanity. If the same amount of knowledge, the same amount of energy, and the same lavish expenditure, where absolutely required, were devoted to the training of great industrial armies, to their maintenance in the most perfect health and efficiency, and to their employment in that great war which man is ever waging against Nature, subduing her myriad forces to his service, guarding against those sudden attacks by storm and flood, by violence and earthquake, which he cannot altogether avoid, and in the production of all the essentials of human life and of a true and beneficent civilisation, the good effects on character would surely be much greater than those produced by mere military training, as the objects aimed at and the results achieved would be more beneficial and more calculated to promote the higher interests of man. And if these industrial armies were allowed to reap the full advantages, material as well as moral, which they created, the results would be so striking that almost the entire population, male and female alike, would claim to be so trained and organised for their own physical, moral, and economic benefit. And the enjoyment of life under such a system of voluntary organised labour would be so enhanced that few indeed would wish to escape from it. Labour in companionship for the common good almost ceases to be labour at all. Friendly emulation takes the place of unfriendly competition, and *esprit de corps* urges each local organisation to surpass other local organisations in efficiency. In such a grand industrial organisation, with equal opportunities of education and training for all, there would necessarily be numbers of inventors and students whose aim and delight would be to so improve the machinery and the methods of work as to continually diminish all the less pleasant forms of labour, and thus proportionately increase the amount of leisure and the higher enjoyments of social life.

It has been objected to all such proposals for the organisation of industry that it would deteriorate character by destroying individuality; but no such objection is made to the military organisation, while under its best forms the reverse is found to occur. In point of fact, *all* organisation is beneficial to character just in proportion as it rises above slavery. And when it shall have reached the point of being the organisation of social equals, for the equal benefit of all, it will attain to its most beneficial influence. Then, character and merit will alone give authority, and the highest and best will inevitably rise to the highest positions. And, just in proportion as the rank and file became educated, and felt the inspiring influences of comradeship and emulation, they could be left more and more to their own initiative; each one's individuality would have the fullest play, controlled only by the influence and opinion of his immediate fellow-workers, and the whole great organisation would become almost automatic in its harmonious working.

Such is found to be the case in the best military organisations, in which the intelligence and individual action of both commissioned and non-commissioned officers, and

even of privates, is cultivated, and becomes of the greatest value, giving to the army in which it most generally exists an undoubted superiority. In any army thus intelligently and sympathetically trained and organised none of the results so dreaded in industrial organisation are found to occur. Men are *not* brought to a dull level of mediocrity; interest in the work they have to do is *not* lost; skulkers, malingerers, and deserters do *not* abound in any appreciable or hurtful proportion; nor is there any indication that men of superior abilities refuse to exercise their talents for the common good because the money rewards of such ability are small as compared with those often obtained in civil life; and lastly, the fact that all are provided with food and clothing, and are thus removed from the influence of economic competition, is not found to have any injurious effect on their effectiveness as workers, fighters, or organisers. And that these effects are not caused by compulsion and the severe penalties of military law is shown by the fact that during the civil war in America, where compulsion and punishment were rarely used, the whole of the opposing armies being practically volunteers, cheerfully submitting to military drill and organisation for the common good, these high qualities were equally manifested.

Yet objections of this class are held to be fatal to any proposal for national industrial organisation for the benefit of all, and the very system of training and co-operation which in the one case is admitted to have beneficial effects on character, and is undoubtedly, even under very unfavourable conditions, attractive in its comradeship and freedom from care, is condemned as being injurious and unworkable when applied industrially. Oh! that some great ruler of men would arise to benefit humanity by organising industrial armies, leading to the elevation and happiness of a whole people, and thus proving that peace may have its victories, far greater and more glorious than those of war!

(3–4) The two last questions – as to the solution of the problems of war and militarism, and the means of arriving as rapidly as possible at such a solution – have already been partly answered in the preceding discussion of the problem itself, but a few words may here be added.

It is, I think, clear that no hope of a complete solution – hardly even of amelioration – is to be expected from the ruling classes, urged on as they are on the one hand by those who are ever seeking for place and power, or for official appointments in newly-acquired territories, and on the other hand by the military class, who ever seek to justify their existence and the enormous burden they are to the nation by obtaining for it extensions of territory or military glory, and with either of these an extension of their own influence. It is, therefore, the *people*, and the people alone, that must be relied upon to banish militarism and war, and for this end every possible effort must be made to educate and enlighten them, not only as to the horrors and iniquity of war, but as to the utter inadequacy and worthlessness of almost all the causes for which wars are waged. They must be shown that all modern wars are dynastic; that they are caused by the ambition, the interests, the jealousies, and the insatiable greed of power of their rulers, or of the great mercantile and financial classes which have power and influence over their rulers; and that the results of war are *never* good for the people, who yet bear all its burthens.

In the course of this education of the people there are certain points that should be specially advocated. For example, nothing is more inconsistent, more foolish, and more wicked than the universal practice of civilised and Christian nations in selling all the most improved weapons and instruments of destruction to semi-civilised, barbarous, or savage rulers, thereby rendering it more difficult – more costly in blood and treasure – to deal with such rulers when their crimes against their own peoples or against humanity become

too great to be borne. This practice also renders it ever more and more difficult for advanced nations to disarm, and thus gives to militarism an additional reason for its existence. From every point of view, whether of Christianity, humanity, or human progress, the supply of modern instruments of war to barbarous rulers, for the coercion of their own subjects, and as a standing menace to civilisation, should be absolutely forbidden. For this purpose, and in order that legal enactments to this end may be effective, we must try and create a sentiment of horror against those who continue thus to betray the cause of civilisation, as being not only traitors to their country, but enemies to the human race. In my opinion, men who, after due notice, and in spite of its declared illegality, continue to supply these weapons to the possible enemies of their country should be declared outlaws in every Christian or civilised community. Hardly less foolish and wicked is the free trade in these instruments and armaments of war, so that directly one or more of the civilised nations are preparing for war the workshops of all the other civilised nations are at once engaged in supplying every kind of destructive appliance, even though they may in a year or two be used against themselves. The time will surely soon come when this conduct will be looked upon as the very culminating point of combined folly and wickedness that the world has seen. The only rational mode of procedure would be to forbid altogether the private manufacture or sale of war material. War is a national act, and so long as it exists all preparation for it should be kept strictly in the hands of national governments.

This supply of the implements of war is the work of capitalists in their own interests; but even worse, if that be possible, is the action of the great civilised governments themselves in allowing their trained officers to engage in the organisation of the armies of semi-barbarous rulers, thus rendering it more difficult to coerce these rulers in the interests of civilisation, and indirectly, yet most certainly, leading to a vast extension of the horrors of war. The entire absence of ethical principle created by militarism is especially shown in the fact that no effective protest has been raised against this most pernicious and suicidal practice. Here, again, the people alone can take effective action, and the people want educating. Common justice, common humanity, even common sense, alike demand that this practice be absolutely forbidden, and that any officer engaging in the organisation of the armies of semi-barbarous or alien rulers should be declared an outlaw by the Government in whose army he was trained, be demanded from the employing Government as a traitor to his country, and the refusal to give him up be followed by an instant declaration of war from *all* the civilised governments.

Yet another point on which the people should be educated is, that they should claim and exercise the right to refuse, as soldiers, to act against their fellow-countrymen or against other countries with whose people they have no quarrel. Accepting the principle that the only just rights of governments rest upon the consent of the governed, what is termed rebellion is not a crime, but is usually the just demand of a community for self-government, a demand which, instead of being repressed by force, should be tested by a plebiscite. And smaller disturbances, termed riots, always arise from some injustice or supposed injustice, and are not proper subjects for massacre by armed soldiers. To use fire-arms against a crowd, and kill or maim innocent persons, women and children, as almost always happens, is to authorise murder. Whenever it may be necessary to prevent violence by a mob, and the available force of police is not sufficient, special constables should be enrolled. But a far better plan would be to organise the fire-brigades as coadjutors of the police, since it is certain that no unarmed (or even armed) mob can stand

against the jet of a fire-engine or of several fire-engines. The mob would instantly disperse, and be rendered ridiculous without endangering life.

Of course, any proposed system of arbitration to settle disputes between nations should be strongly supported; but the existing condition of all the great civilised governments renders it certain that, so long as the ruling and military classes exist, and are allowed to possess the almost absolute powers they now exercise, war, as the ultimate mode of settling national disputes, will not cease.

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Is Britain on the Down Grade? (S568aa: 1899)

One of a number of solicited responses published in the July 1899 issue of The Young Man.

Although I quite sympathise with Mr. William Clarke's powerful statement of the indications of moral deterioration that meet us everywhere to-day, yet I do not accept all his premises nor agree with his somewhat pessimistic conclusion. He tells us that he believes in the necessary decline and death of nations as of individuals, apparently on account of the somewhat forced analogy between the social and the individual organism. Yet a little further on he points out that the civilisation of our country is almost identical with that of the rest of Europe and of America, and that the indications of deterioration, as of progress, are alike in all. It is therefore not a question of the decay and death of England or of any other nation, but of civilisation itself, that we have to deal with, and to my mind there are no indications whatever of such a catastrophe, nor the least evidence or even indication that it will ever occur.

The proofs of deterioration dwelt upon by Mr. Clarke are the growth of gambling, the vast extension of the factory system, the enormous increase of millionaires and the money power, and the immoral greed of kings and governments in their struggle for the partition of the uncivilised world. With every word that he says on these subjects I agree, and I have to the best of my ability set forth similar views in a recently published volume, and have further enforced the doctrine of deterioration they imply by a body of unimpeachable facts taken from the successive Reports of the Registrar-General. I have shown that insanity is increasing in a far greater ratio than the population, even after the fullest allowance for those causes of apparent increase by which the Lunacy Commissioners and medical writers attempt to explain away the increase. Suicide, again, has increased at a still greater rate during the last thirty years, and, as this is a form of insanity, it supports the reality of the former increase. Notwithstanding the growth of the temperance movement, it will startle most persons to learn that deaths from alcoholism and delirium tremens have increased nearly seventy per cent. faster than the population in the last thirty years, and that such deaths have increased much faster in women than in men. Another and even more terrible indication of deterioration is the large and steady increase during the same period of premature births and congenital defects in children. As might be expected with such a state of things, our prison population – including those in reformatories – has increased fifty per cent. faster than the population, notwithstanding all the efforts of official apologists to prove the contrary. And, lastly, the deaths in public

institutions (workhouses, hospitals, etc.) have steadily increased during the same period, till they now amount, in London, to twenty-seven per cent. of the total deaths. And perhaps the most terrible feature of all is, that in all these cases the *rate of increase* is itself increasing, so that we are going downhill now much faster than we were ten, twenty, or thirty years ago.[1]

Now, surely these glaring proofs of physical deterioration afford the strongest confirmation of the reality of that moral degeneration which Mr. Clarke as so forcibly set forth. Yet I wholly disagree from his gloomy outlook. For, along with this moral and its resulting physical deterioration there are undoubted signs of moral advance. True humanity is increasing everywhere, and the conscience of the nation is being stirred as it never was before. The people are everywhere better than their rulers, better than the land and wealth grabbers. And so far from there being no "commanding vision," no generous faith in great causes, I doubt if there has ever been so much of both. The rapid and irresistible spread of socialism in every civilised country, destroying national antagonisms and introducing a true brotherhood of labour throughout the world, is a fact of the highest importance. It permeates every class of society; it absorbs the best intellect of the workers, and is yearly gaining converts from our great national universities, from the liberal professions, and from the Church itself; and it has this advantage over all previous attempts at reform, that it does not deal merely with symptoms or with the machinery of government, but goes down to the very roots of all the evils which afflict our civilisation. And this great cause is upheld and guided by that very "commanding vision" the supposed absence of which Mr. Clarke deplores. It is taught by Carlyle and Ruskin, by William Morris and Lewis Morris, by Edward Bellamy and Robert Blatchford, and by that truest saint and greatest seer now living – Leo Tolstoi.

Truly, we will *not* despair of the Republic of Humanity.

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Protests Against War (S569)

Printed in the September 1899 issue of The Manchester Guardian, as a protest against events taking place in the Transvaal.

I feel very strongly that our Government has no right whatever to interfere in the internal affairs of the Transvaal, and that all questions of the franchise, of taxation, and of education are essentially internal, and are, I believe, always held to be out of the sphere of diplomatic action between independent nations. I hold, further, that in the relations of England to the Transvaal we have been *almost* always in the wrong – that to talk of our having '*given* them *independence*,' as most newspapers do, is an insult, since we only *restored* that independence which we had taken from them admittedly *unjustly*, and on such *false representations* as are now being continually made. Although a Radical of the extremest type, I uphold liberty for *nations*, however small, as well as for *individuals*; and I hold that the Boers are fully justified in being extremely cautious in allowing political rights to the mob of gold-grabbers, speculators, and swindlers who are striving to get the

[1] For the figures and authorities for all these statements, see *The Wonderful Century*, chap. xx.

government into their hands. I hold that the so-called *development* of the country by these gold-grabbers, &c. is really its curse – that the Boers are fully justified in allowing *their* gold to be worked on *any terms* they please, and that they would have done *well* to claim much higher royalty than they do, even 50 per cent of all the produce if they had thought proper; and, *lastly*, I hold that [Prime Minister Joseph] Chamberlain's aggressive tone and conduct is the cause of all the present trouble. I see nothing to be done but to get up petitions all over the country in favour of *non-interference* in the internal affairs of the Transvaal, and to let it be known that it will be made a *test question* in all future Parliamentary elections. I will vote for no man who will not accept and vote for that elementary principle of justice to a weak nation.

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The Transvaal War. Wanted Facts. (S571)

Printed in the 18 November 1899 issue of The Clarion.

It appears to me that much of the difference of opinion on this subject arises from the endless flood of "misstatements" which, after having been once or twice quoted, are thereafter referred to as "facts." I have been trying to get at the actual facts for some months past, and it is with much regret I find that even the *Clarion* writers (some of them) state as "facts" what I have never been able to find any valid evidence for. I have neither time nor inclination to write at length on this subject, but with your permission will give one or two examples of what I mean.

1. I have read daily all the reports and telegrams from the seat of war, and I find no single reference to the Boers carrying any arms but a rifle. Neither sword nor revolver has been once mentioned, but it has been distinctly stated that their rifles have no bayonets. Hence, whenever our troops – infantry and cavalry – get to close quarters, the Boers are practically in the position of unarmed men, and are, as has been described, ridden through and through, slashed and bayonetted "with great slaughter!" I presume these are *facts* which will be universally admitted. Yet in the latest *Clarion* we have Mont Blong[a] saying: "They have a well-drilled, splendidly-armed, and equipped force (native and foreign) of some 50 or 60 thousand men." "Splendidly armed and equipped!" Surely this is not a statement of *fact*, if words have any meaning. And this huge misstatement is also at the bottom of Nunquam's deprecatory statement in the previous *Clarion* that their methods of fighting were those of "the bandit, the redskin, and the Afridi." I must say this seems to me ungenerous to a brave enemy. How else can they fight, imperfectly armed as they are? Then Mont Blong says they are "well drilled." Where is the evidence of this? It was stated in the newspapers about a month or so ago that the new Mauser rifles were only distributed quite recently, and that numbers of the Boers did not know how to load them; and so far as I know, no *evidence* has been given of the systematic drilling of these fifty thousand men, almost all workers on their farms or elsewhere.

2. Again, the present theory that the Boers have been preparing, ever since 1884, to form one South African Republic, independent of Great Britain, is adopted by Mont

[a] *Clarion* columnist "Mont Blong."

Blong, as it is now put forth by the Government as their reason for going to war. But I have never seen one particle of evidence of this intention – often called "a great conspiracy" – while their preparations are said to have made them "a great military power." There is, on the contrary, direct and good evidence that no great preparations, even for defence, were made before Jameson's raid.[a] Just before that incident the Chartered Company sent a British officer – Colonel the Hon. R. White – to Pretoria to find out the exact state of the defences of that city, and his report is published in the Blue Book on the raid. He says he saw only a few old guns and mortars of different dates, a cavalry troop with 250 horses in miserable condition, three Maxims and three batteries of three and six pounders. He also writes: "The system of conscription consists in the commandants sending two men from their districts to be trained every two years." This statement is made in a letter to the *Daily Chronicle* of last Thursday (November 9), signed "Alfred Marks," and I presume the facts thus precisely stated may be taken to be "facts." It was this report that led the organisers of the raid to believe that Jameson's 500 troopers, with help from Johannesburg, could easily capture Pretoria, and thenceforth, with the help of our Government, keep hold of the whole country.

Neither do I like Mont Blong's accusation, that "for years the Boers have been smuggling arms into their country." I thought the *Clarion* men were, at all events, literary – that they used words in their right meanings. Now, *smuggling* implies illegality and secrecy. The Johannesburg Committee *did* smuggle arms. The Boer Government did not (and could not). For them to import arms was certainly *not* illegal, and it certainly could not be, and, as a fact, was not, secret.

Again, in the 6th column, front page, of the latest *Clarion*, I find a par. beginning: "Is it not true that the Boers treated the British residents in the Transvaal as an inferior race?" followed by four other questions implying other supposed iniquities; and the par. ends: "What are all these facts but overwhelming proofs of Boer arrogance?" Well, I have read through the Blue Book – "Complaints of British Residents in the South African Republic" – and neither there, nor elsewhere, have I been able to find any *evidence* of real grievances. The alleged "facts" are either exaggerations of incidents that are liable to occur to any residents in a country where the laws and customs are different from his own, or even when true are not important grievances when fair consideration is given to the altogether unprecedented state of society in Johannesburg, where a good deal of the worst dregs of the great cities and mining camps of the world are gathered together. Thousands of men have lived in Johannesburg for years without finding out that they were oppressed. As an example, there is the Johannesburg engineer, now at Leeds, who was interviewed for the *Leeds Mercury*, and declared, when asked about his grievances: "I didn't know I had any really serious grievances till I started taking the newspapers. The *Star* told me I had some, and the *Leader* told me the same so often that I came to the conclusion I must have a lot which I hadn't noticed"; and to the detailed questions as to many of the statements you have made, as above quoted, he gave practical denials to all. Mr. Thomas Ratcliffe, a miner, of Preston, also just returned, says practically the same thing; and as regards the Outlanders' petition to the Queen, he says: "To my own knowledge the names of men were signed who had been dead two or three years," adding that miners who did not sign

[a] The Jameson Raid was an unsuccessful action against Paul Kruger's Transvaal Republic carried out by British sympathizers over the New Year's weekend of 1895–96.

were not given work, and he concludes: "Taking the whole thing, it is a most rotten and corrupt enterprise we have entered into war about – a dishonour to our nation and a dishonour to the British flag. That is the opinion of the working class in the Transvaal."[1] Of course, these men may be wrong or may be liars, but that seems very unlikely. I have read scores of similar statements by men of all classes, who have lived in the Transvaal as Outlanders, and declare that they were as well off as anywhere in the world, and were well treated by the Boers of all classes, as were all decent and well-behaved people.

I do not say that these witnesses, who are certainly not bribed to tell lies on the unpopular side, are always right, but I do say that the very fact of there being such a body of evidence on the other side should make us hesitate to accept as indisputable "facts" any accusations against the Boers, unless supported by reference to some individual cases of hardship and oppression quite beyond what is liable to occur to every person living in a foreign country. The Blue Book can adduce only two or three such cases, and these are simply ridiculous as the foundation for a general accusation against the Transvaal Government. I venture to hope, therefore, that the *Clarion* will not again quote these very disputable statements as if they were demonstrated facts.

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Mottoes for the New Year. Wise Words from Famous People. (S573)

One of many messages printed in the 30 December 1899 issue of The Daily News Weekly (London), as part of a special feature.

Dr. Alfred Russel Wallace, in sending his motto for the twentieth century, accompanies it with a brief statement of its origin and application:

1. The century that is now passing away has been characterised by an enormous and unprecedented increase of intellectual advancement and material wealth, accompanied by an increase rather than a decrease of want and misery. The truth of this latter statement is demonstrated in the twentieth chapter of my "Wonderful Century."

2. The coming century, along with the diminution of luxury, but the further increase of real wealth, may, let us hope, witness the abolition of want with all its direful consequences through the general recognition of the great principle of social justice – equality of opportunity.

3. This principle is a logical deduction from Herbert Spencer's fundamental law of social justice, which he gives as the equivalent of the law of the survival of the fittest in the animal world, and expresses as follows: – "Each individual ought to receive the benefits and evils of his own nature and consequent conduct: neither being prevented from having whatever good his actions normally bring to him, nor allowed to shoulder off on to other persons whatever ill is brought to him by his actions." (Justice, p. 17.) This law forbids all such gifts or bequests of property as may enable any person to live permanently without work on the labour of others, and thus demands for each "equality of opportunity."

[1] For these quotations see *Daily Chronicle*, November 9th.

4. If we earnestly endeavour to apply this principle to every department of social life – intellectual as well as material – the twentieth century will witness an advance in true civilisation and in human happiness such as the world has never seen before.

5. The public conscience is now so deeply stirred by the complete failure of our present system of wealth-distribution, even under the most favourable conditions, to abolish want, that it will welcome the adoption and application of this grand and far-reaching principle.

6. Let us, then, declare that notwithstanding the certain opposition of plutocrats and politicians, this principle shall be ever kept in view as a beacon-light to guide us in all our social legislation. Thus only shall we make sure our steady advance towards social justice till we reach the wished-for goal of general well-being.

Our motto must therefore be:

"Equality of Opportunity" as the fundamental principle of Social Justice, and the only means of attaining the well-being of all.

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Labour and the Next General Election (S576)

Wallace's response to a general inquiry concerning this subject was printed in *The Labour Annual: The Reformers' Year-book for 1900*.

Dr. Alfred Russel Wallace, writing on 17th Nov., from Parkstone, Dorset, says: "I believe the only way to get any needed reform is to make it a question at the polls, refusing to vote for any candidate who will not promise, clearly and without any evasion, to advocate and support the required reform. Taking the three heads so admirably laid down by Mill, I would suggest that, under the head of individual liberty; first and most pressing is the total abolition of the abominable vaccination laws. Not only do they take away the liberty of honest and conscientious parents, but they are actually the cause of numerous deaths of both infants and adults – a legalised form of murder. Next, I would claim the abolition of the cruel imprisonment of the workhouse and the casual ward, and require instead liberal old-age pensions and industrial colonies for the unemployed. Under the head of ownership of the raw material of the globe – the land – claim first (as the simplest and least hurtful mode of obtaining it) the abolition of *all rights of bequest or inheritance to the unborn*, that is, that at the time of the enactment of the law, none but persons then living in the direct line – children, grandchildren, &c. – shall inherit it, and that after their death it shall revert to the nation.[a] Thus no one will be injured, no one will lose anything; yet the people will, beginning at once, gradually but surely, in about fifty or sixty years, regain the whole of the land which throughout many past centuries has been unrighteously taken from them by Kings and Parliaments. As it falls in, it must be held by local authorities for the *use* of all who need it, and for the *benefit* of all. Under the third head, of an

[a] In earlier writings Wallace had expressed his view that wills, trusts, etc. should not extend beyond the lifetime of the person involved. See "Limitation of State Functions in the Administration of Justice," *Contemporary Review* 23: 43–52 (December 1873). Available online at: http://people.wku.edu/charles.smith/wallace/S236.htm .

equal participation in the benefit of combined labour; – little can be done till the land is obtained. But the best use of the land would be, in my opinion, the establishment of co-operative communities of considerable population, so as to include the producers of all the necessaries and comforts of life. And, perhaps, the best guide to the successful organisation of such communities is to be found in that wonderful experiment at Ralahine under the supervision of that good man and admirable organizer, the late E. T. Craig. Ralahine offers us a model and guide of what to do and what to avoid, and how to combine the greatest freedom with the most economical management. Every worker should study the account of it, either in Mr. Craig's own book, or, what is better, in the clearer and more continuous narrative of Mr. William Pare, entitled: – *Co-operative Agriculture a Solution of the Land Question, &c., &c.* No more instructive work than this exists in the English language, and if it is carefully studied and accepted as a teacher co-operative production will be a certain success."

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From Our Readers [re: the Boer War] (S576aa)

One of about a dozen letters to the Editor printed in The Morning Leader (London) issue of 5 February 1900.

If anyone desires to know the fundamental causes that have led to the present most unjust war, he should read carefully the third and fourth chapters of Mr. Froude's[a] "Oceans," in which is given, in his incisive manner, a sketch of the history of our treatment of the two Republics during the first three-quarters of the present century. From personal observation during two visits to the country – one official, the other private – he was well able to judge of the character of the Boers, and he had no private interests to serve. He tells us that "the Boers had been so systematically abused and misrepresented that the English scarcely regarded them as human beings to whom they owed any moral consideration," and that they "had despised them and had not treated them with ordinary honesty." He assures us that the Dutch of South Africa, though obstinate as mules, are emotional and affected easily through their feelings; and that, when the Colonial Office admitted that they had not been treated fairly in the annexation of the diamond fields (in 1870), and awarded them the altogether inadequate sum of £90,000 as compensation, they were satisfied. The money was nothing; the acknowledgment of wrong was everything. He considered that if English Governments and the Press would try to make the best of the Boers instead of the worst, all would be well.

On the question of the treatment of the natives Mr. Froude's conclusion is that although the Dutch are accused of harsh treatment their method is in the long run more merciful than ours. We have killed hundreds of natives where the Dutch have killed tens. And the fact that in the two Republics they have been always living in the midst of a warlike black population ten or twenty times their number, shows that they have solved the problem of how the two races can live side by side to the advantage of both.

Even more important is the opinion of the late Sir George Grey, who had been Gov-

[a] James Anthony Froude (1818–1894), English historian and Editor of *Fraser's Magazine*.

ernor of the Cape Colony, and who was one of the greatest colonial administrators we have ever had. He told Mr. Froude that he had gone to the Cape with the prejudice against the Boers generally entertained in England, and he had found the Boer of the English newspapers and platform speeches to be a creature of the imagination which had no real existence. He found them to be in reality a quiet, orderly, hard-working people, hurting no one if let alone, but resentful of injuries, and especially of calumnies against their character. Had the charge of cruelty to the natives been true, Sir George Grey would have been one of the last men in the world to pardon it. But he declared that it was no more true of them than of us, and necessarily of all colonists who come in collision with the original owners of the soil; and he thought our perpetual interference with them to be foolish and unjust. Our interference alone had created all the troubles in South Africa. Finally, he declared his opinion that the Boers were a people who could not be driven, but, if treated frankly and generously, they would be found among the very best colonists in any part of the world.

At the present time, when the policy of interference and continuous misrepresentation has brought upon the nation a war whose results, even if entirely successful, can never compensate for a thousandth part of the blood spilt, it is well to call to mind the opinions and warnings of the two great men here quoted – men who had ample opportunity for forming an accurate judgement, and no personal interests to induce them to conceal or pervert the real facts. The character of a people does not change in a single generation, and we may be sure that the Boers as known to Grey and Froude are the very same which our Government has to-day goaded into a war of independence.

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Imperial Might and Human Right (S579)

A letter to The Clarion responding to comments made by George Bernard Shaw. Printed in the 21 July 1900 issue.

It is to me very distressing to see Mr. Bernard Shaw exercise his great talents and his caustic wit in paradoxes and verbal quibbles calculated to make the enemy rejoice at the dissensions among Socialists; and at the apparent absence of agreement among them even on fundamental principles of politics and of ethics. Passing by Mr. Shaw's doctrine that we Socialists should uphold "robbery under arms" of any land or property which we think we could make a better use of than the present possessors – the doctrine that might is right for Socialists as for burglars – I will now only say a word or two on his statement that "independence and liberty produce not freedom, but slavery." Of course, Mr. Shaw can prove that he is right by taking only one meaning of the words, and that not the generally accepted meaning. Independence in the individual means that he is not the slave or the servant of another man, either directly or indirectly, as a serf, tenant, or wage-thrall. It does *not* mean that he must be absolutely self-suffering, without any help from his fellows either through friendship or co-operation, or social organisation. Yet it is only by adopting this last sense – a sense only justifiable etymologically – that Mr. Shaw's statement has a shadow of truth.

And as regards communities or nations, independence has but one meaning – self-

government as opposed to government by an outside power which has annexed, purchased, or conquered some smaller and weaker people. Whether such an enforced government is relatively good or bad, it is still slavery for the weaker people, and, like all slavery, is demoralising to both parties. With all my heart and soul I protest against and condemn the doctrine that we have any right to force our rule upon people who do not want it, under the pretence of better government. I maintain that force is *never* the better way, and that every people should be left to develop their own civilisation and their own government, aided by advice and example, but never by compulsion. No truth is, I believe, more certain than that stated (I think) by Mill, that the worst government of a people by themselves is better than the best government by foreign conquerors. To my mind, Socialism can only come about voluntarily. Compulsion, whether of individuals by the majority or of weak nations by stronger ones, is not only ethically wrong, but is antagonistic to all real progress towards the hoped-for Co-operative Commonwealth.

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letter regarding 'Might vs. Right' (S580)

An untitled letter printed in the July 1900 issue of The Eagle and the Serpent concerning the views of Dr. Ragnar Redbeard on this subject.

I can understand Dr. Redbeard's position, though I cannot accept it. If men were only "herds of animals" his view might be the true one. But the mere fact that men, everywhere, and throughout all history, have had words and ideas corresponding to *truth*, *justice*, *virtue*, *right*, and that there have always been men who would sacrifice even their lives for these ideas, proves that mankind is *more* than an aggregation of "herds of animals."

The mere physical struggle – the rule of the biggest and strongest brutes among men – is not therefore conducive to man's advance, prosperity, happiness.

Again, something *may* be said in favour of the struggle of individual with individual man, as leading to the survival of the best physical types. It may be said that such a survival is good for humanity. But no such advantage can be predicated of the struggle between communities very unequal in numbers. Forty millions, even though mostly fools and scoundrels, may be able to destroy half a million of far higher average type mentally, morally, and physically. The massacre of St. Bartholomew was such an exercise of *might*; but it was not a benefit to the race, it was opposed to our ideas of justice and morality both now and then, and was therefore not *right*.

Dr. Redbeard has given us a very brilliant and rhythmical poem – "The Logic of Today" – glorifying *might* as always and everywhere being identical with *right*. I admire his verse, but I decline to alter the meaning of such words as *justice* and *right* to make them accord with his theory that men are merely – herds of brute beasts. – Alfred R. Wallace.

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A Democratic Union (S581a)

Unable to attend a labor convention to which he had been invited, Wallace sent a position letter that appeared in the 28 October 1900 issue of Reynolds's Newspaper.

Now that the forces of militarism and privilege have obtained a new lease of absolute power, with every prospect of its being used to still further extend and consolidate that power to the permanent injury and degradation of the misguided people, some organization for combined action of all the true friends of national and individual liberty is more than ever needed.

But in order that such an union shall be really effective it must rest upon principles far deeper and broader than those which either of the great political parties have yet acknowledged. We must not waste our energy on the advocacy of further improvements of the political machine, however much it may need improvement – that is the work which the existing Liberal party is pledged to undertake; neither must we occupy ourselves with further patching up of ameliorative laws which deal only with the *symptoms* of our various social evils, while their *causes* are altogether untouched. That, too, is the congenial work of the Liberal politician – work that would be quite unnecessary if our social system were founded either upon justice or common sense.

If we *really* mean to work for freedom, for justice, for the economic and moral well-being of every man, woman, and child in our country, we must decide upon some fundamental *principles of action*, which, in proportion as they are carried out, will tend to secure that well-being. And in deciding upon these principles let us not be afraid of the parrot cries of "Impracticable!" "Robbery!" "Outside of practical politics!" "Un-English!" and such like. Many of you remember, as I do, when the *ballot* was declared to be "un-English," that it would degrade the Legislation and deteriorate character. At that time hardly any Liberal or Tory papers advocated it and the Radical papers that did so were utterly despised by privileged classes. This state of things lasted till the death of Lord Palmerston, one of the fiercest opponents of the ballot, but less than ten years afterwards it became the law of the land. It was a common-sense reform. It did some little good, but not much, because it left untouched the powers of wealth and landlordism to bribe and intimidate – because it dealt with a *symptom*, not with the fundamental causes of the evil.

What principles, then, must we adopt as our guides in politics and legislation? In my opinion there are only two which are sufficiently broad in their foundations upon social justice, and sufficiently far reaching in their effects, as to ensure to every Englishman economic and moral freedom.

The first of these is *equality of opportunity*, as established by Herbert Spencer in his "Justice" and advocated by Mr. Benjamin Kidd in his "Social Evolution." This great principle I have explained and illustrated in Chapter xxviii. of the second volume of "Studies Scientific and Social" under the title "True Individualism, the Essential Preliminary of a real Social Advance." (The book will be published by Macmillan on the 30th of this month.) When this principle is thoroughly grasped, it will be found to embody the minimum of *absolute Social Justice*; and, when carried out to its logical results, it will secure to all alike the same means and opportunities of attaining to economic and social

well-being. It will moreover serve as a *test*, by which to judge of proposed new legislation; and just in proportion as it is carried into effect, will it diminish and ultimately destroy the worst economic and social evils that now abound.

But this great principle, though now becoming generally known to advanced thinkers, and admitted to be – as a principle – absolutely sound, is yet too new to the ordinary politician to produce much immediate result. For many years to come its advocacy must necessarily be of a purely educational nature. We require, therefore, something more concrete, less remote from ordinary political ideas and more adapted to immediate beneficial application. We must have something that is, demonstrably, a fundamental remedy and yet, is now, or very quickly may be brought, within the sphere of "practical politics."

Such a principle I find in the statement, that it is the most important *duty* and a true *function* of government to give every needful assistance for the *voluntary organization of labour for the good of all*. The mode in which this can be best and most effectively carried out is explained in some detail in the twenty-sixth chapter of the volume already referred to.

These two principles might, in fact, be worked together. The first would bring about a *true* individualism which can only exist under "equality of opportunity." The second would bring about a simple and purely voluntary Socialism, but in a form and by methods which could hardly be objected to by any true Democrat.

By adopting these two principles as the platform of a new Democratic Union, we should, I believe, secure a very wide support and in a comparatively short time be able to create a united party powerful enough to bring about a peaceful, but effective, social and political revolution.

At first, of course, the work of the Union would be purely educational, but the education would be most effectively carried on by applying the test of these principles to every proposed social legislation, showing how far these proposals were in agreement with or antagonistic to them and thus deciding whether they should be supported or opposed by members of the Democratic Union.

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letter on "The Trend to the Towns" (S582a)

Over the last several decades of his life Wallace often lamented the general migration of rural residents to the towns and cities. One such commentary appeared in the 28 November 1900 issue of The Morning Leader *(London).*

Sir; – I regret to find that both Mr. Fletcher and most of your correspondents on the subject of "The Trend to the Towns" assume that the real and sufficient cause of the fact is the absence of social pleasures and amusements in villages as compared with those in large cities, and that a contributory, but not the most important, cause is the generally low wages of agricultural laborers.

The true – the fundamental cause – goes much deeper. It is simply the hopelessness of the laborer's life, no secure prospect of advancement let him work as hard as he can, no security even after half a century of labor of a restful and independent old age.

In the few cases where landowners do allow every kind of worker to have as much land as he wishes or requires, at agricultural rents and on a secure tenure, all the supposed attractions of the towns are powerless, and the rural population increases instead of diminishes.

One of the best-known cases is that of Lord Carrington, who has estates in Buckinghamshire and Lincolnshire. In the former county, and within 50 miles of London, Lord Carrington has nearly a thousand tenants of small holdings of various sizes; and so far from these people being attracted to London, the demand for these holdings continues to be beyond the supply. The tenants are not only agricultural laborers, but mechanics and small tradesmen, who all find the secure possession of land of the greatest value in their various occupations often making all the difference between success and failure. On the land thus let out the produce is on the average about three times that of the same kind of land when let to tenant-farmers; and Lord Carrington informs us that these small holders almost invariably succeed, hardly ever give up their holdings, but frequently want to increase them, and all pay their rents with the greatest punctuality. We have here, therefore, two enormous gains. The country gains by a threefold amount of produce, especially in wheat, showing that were the people allowed to cultivate the land for themselves we should be quite independent of foreign food; while laborers of all kinds remain prosperous and contented in the country.

Exactly similar results are described on the Lincolnshire estates; while even more remarkable are those on the late Lord Tollemache's estates in Cheshire, where every laborer has land at an agricultural rent and on a secure tenure, sufficient for the keep of a cow, while additional land is given whenever it can be profitably employed. Outsiders were also encouraged to build upon the estate by being offered leases at low ground rents, with the option of having five or ten acres of pasture for a cow or horse at agricultural rents. Retired tradesmen and professional men thus became settled on the land, all employing a certain amount of labor, and being customers to the various farmers and others for farm and garden produce. A self-sufficing and prosperous community was thus initiated; and even the tenant-farmers, who at first were strongly opposed to Lord Tollemache's system of settling the laborers on the land, as making them too independent and thus raising wages afterwards came to acknowledge that they were mistaken, and that the system was as beneficial to them as to the laborers. This was because a large body of workers of all kinds was retained in the district, and these were always ready to work for good wages in hay-time and harvest, and thus crops were often saved which without such prompt help would have been lost.

It would require a volume to give every published case of similar results wherever similar causes have been at work, but enough has been said to show what are the fundamental causes of the "drift to the towns," and that the alleged superior attractions of the towns and the supposed "dulness" of the country have the very smallest share in it. If your various correspondents who have adopted the latter view would test its accuracy by putting the case fairly to any young laborers in a number of villages, and accurately record their answers, I have no doubt as to the result. – Yours, &c., Alfred Russell Wallace.

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letter to Julia Dawson on socialism and peace (S595)

This friendly letter to columnist Julia Dawson of The Clarion was printed in its issue of 18 May 1901.

My dear Julia Dawson, – I congratulate you heartily on your *two* Vans.[a] They will perhaps do as much for Socialism as the *Clarion* itself. However good the written word may be, the spoken word is far better, when you get the right people to speak it, with knowledge, and from the heart – and that I know will be the case with your Vanners. I would like to say a few words on a subject which, just now, is nearest to my heart – the subject of *Peace*.

If there is one subject in the world – one great subject affecting humanity which is more important than any other as essential for social progress, social improvement, the spread of Socialism – it is the question of peace, as opposed to all militarism, and especially to wars of conquest, and more especially still to the crushing out of small communities by powerful nations.

I have been reading lately three wonderful books – books very different from each other, but all tending to hope for down-trodden humanity. These books are: Mr. J. Richardson's "How It Can be Done," the most beautiful, true, and practical statement of the essential first steps towards Socialism yet put forward, and two books by a lady,[b] "A Colony of Mercy" and "Cities and Citizens," describing what *has* been and is being done in Germany to abolish the worst forms of want, far, far in advance of anything we have done in this country. These two books make me ashamed of my country; and how much more ashamed of a country which joins in a *Peace Conference*, takes a prominent part in it, declares for *arbitration*, and almost before the ink of its declarations is dry, refuses arbitration, and goes to war to crush two small Republics, which, whatever their faults, were better governed than we are! And our foolish people return their false and cruel men again to power, to spend hundreds of millions in wholesale murder and plunder, and thus render impossible for years to come any such vital measures as old age pensions, while the crushing taxation to pay for this war will inevitably increase the need for such an instalment of justice to the workers.

Peace is the Socialist's first need, and peace will never be got unless we make it the *first* and *only* question at the polls. As it is the first necessity for all reform, let us put everything else aside till we get it. Let every Socialist, every reformer, vote only for men who will promise distinctly, *and without any reservation whatever*, to support *Arbitration*, according to the Hague Conventions to which we have agreed, for *every* international dispute. When that is our rule of action, our army, instead of being indefinitely increased, as now threatened, may be diminished to the amount necessary to defend our own country only.

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[a] *The Clarion*, a socialist newspaper, helped promote its message by sending around horse-drawn vans with volunteers to proselytize and circulate literature on the streets of London. This strategy was also used by Wallace's Land Nationalisation Society.

[b] Wallace must have had a temporary lapse of memory here in not naming the author of these two works: it was Julie Sutter. Wallace would later publish a short "appreciation" of another Sutter work, *Britain's Hope*, in 1907.

Now, I should like our Vanners to give half their time and half their speeches to the *Peace Question*. Take as our text that fine old saying, "War is a game which, were their subjects wise, kings would not play at." For "kings" read "governments," and this is true and applicable to-day. War is the game, the excitement, the means of living of the wealthy classes and of speculating capitalists, and, whoever wins, it is the people – the workers – who lose and pay.

Show the people that there is never any occasion for war, that *no war* has ever produced any permanent good. All history teaches this. Show the demoralisation caused by war. Some of the letters written home by soldiers in South Africa and from China, gloating over plunder and devastation, massacre and deliberate killing, are enough to make the angels weep.

Surely there must be a large majority of our people who have sense enough, justice enough, pity enough, Christianity enough, to oppose *all war*. Let every one of these publicly promise to vote no longer for Conservative or Liberal or Radical, but only for thorough supporters of arbitration and opponents of war and militarism, and there will be some faint gleam of hope that this century will bring us to the threshold of the Co-operative Commonwealth.

If we do not do this, but let such Governments as the present have their way, the result will surely be, first, CONSCRIPTION, and with it the omnipotence of capitalism and the degradation of the people. – Yours very sincerely, Alfred R. Wallace.

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Is Tolstoy Inconsistent? (S595ab)

This Wallace commentary was printed in the July 1901 issue of the I. L. P. News. Wallace greatly admired Tolstoy, as easily can be seen here.

In the June Issue of the I.L.P. News, J. Bruce Glasier[a] has a leading article the keynote of which is the glorious inconsistency of the great Russian teacher, with especial reference to his recent appeal to the Czar. But throughout the article I can find no proof of real inconsistency. Every statement of the kind only shows that his opinions have changed and developed during his whole life – as those of every independent thinker must change – and that his earlier *acts* are often opposed to his later *opinions* and *beliefs*. Not one fact is given to prove that Tolstoy has ever deliberately acted in opposition to the principles he held at the time. Probably, no living individual has more earnestly sought after the principles which should determine conduct, and, having arrived at what he believes to be such guiding principles, has so earnestly and so unselfishly adopted them to regulate his own life.

And the one special act which is supposed to form the culminating point of his inconsistency – his appeal to the Czar – is on the contrary perfectly consistent with all his later teachings; and it is to me very strange that neither Bruce Glasier, nor other writers who have expressed the same view, have seen this. For, what is this Appeal? If carefully read it will be seen that it is wholly and entirely in accordance with the fundamental

[a] John Bruce Glasier (1859–1920), Scottish socialist editor and politician.

principle of anarchism – the principle that government by force is wholly evil and is the source of almost all the unhappiness and misery of humanity. In accordance with this great principle he urges the Czar to forbid certain oppressive governmental acts, to repeal some of the most oppressive laws, and to abolish some of the most unjust, cruel, and degrading punishments. In no one case, so far as I remember, does he ask the Czar to do any one thing for the people, but simply to remove some of the fetters with which he has bound them, to leave them a little more free to do things for themselves. And yet Bruce Glasier can so mistake this grand appeal as to write – "The appeal to the Czar is an appeal to the State, and Tolstoy in issuing it has abandoned his anarchism. He has abandoned the principle that we must look to individual conversion and sanctification of life as the sole means of social regeneration, and he has thus definitely conceded the inutility of the central precept of his own Tolstoyan and anti-political creed." Here are strong and positive statements without any reference to facts in support of them, and, I venture to say, wholly opposed to the facts. Tolstoy advocates non-resistance, but he does *not* advocate dumb acquiescence in wrong. He maintains that the whole fabric of compulsory government is wrong and evil, but he never teaches that it is wrong even to ask the head of the State to undo some of its evil acts and allow the people a little opportunity to better their condition, to work out their own salvation. And he wisely limits himself to asking the repeal of a few only of the worst and most oppressive of the governmental acts which a just and humane man, even though a Czar, might be supposed to be able to see were not only tyrannical and cruel, but absolutely useless. Where is the inconsistency here? Where is the abandonment of principle? On the contrary, it is the appeal to pure anarchism. Tolstoy says, in effect – "You think repressive and penal laws are necessary. I call your special attention to certain enactments, official acts and punishments, which, on consideration, you must see to be quite useless, very cruel, and highly injurious. Abolish these, and you will find that what I say is true. The people will be happier and peaceable without them." If this appeal were successful – as it yet may be – it would bring about the first instalment – a very small and insignificant instalment but of vital importance as a matter of principle – of the abolition of government by force; in other words the first step towards Anarchism.

In conclusion, I wish to say that I am a great admirer, though by no means a disciple, of Tolstoy. On several points I differ from him. He is sometimes unfair to his opponents, though this is usually from imperfect knowledge. But of all modern teachers he is, to my mind, the one against whom the charge of inconsistency can with the least justice be made.

We socialists should especially be careful in depreciating the work of the great thinker and moralist who is doing more than any other living writer to expose the evils of all government by force, and who maintains, as we do, that human nature is even now good enough and sensible enough, by voluntary combination, to protect itself against evil doers and to work out its own moral and physical well-being. I am myself wholly opposed to any attempt to establish a compulsory socialism (the very term is self-contradictory) as to all other governments by force, and I owe this conviction mainly to Tolstoy. Here, as in Russia, what we need first, is the repeal of bad laws, and especially of all those laws which either enforce or permit the existence of privileged classes, and of any inequality of opportunities as between man and man. Just in proportion as we are relieved from the most oppressive of the bonds and shackles with which our government binds our bodies and our minds, shall we adopt that system of voluntary co-operation for

production as well as for all other useful purposes which will inevitably result, by a natural process of development, in a true Co-operative Commonwealth. In all this teaching we should hail Tolstoy as a master, and as a co-worker with us for the salvation of down-trodden humanity; and we should therefore be especially careful to avoid any unjust criticism, or any depreciation of his life and work which may tend to diminish their influence for good.

It is for this reason alone that I venture to oppose my view to that of so good a socialist as Bruce Glasier, and to maintain that, among all the great moral and political teachers of our age, Leo Tolstoy is among the greatest, if not the very greatest of all, that he is the most truly consistent in his life and conduct, and therefore the most worthy of our admiration and respect. We need not hold him to be infallible. He should not be exempt from criticism. But when we do criticise we should deal with important matters only, and above all things we should avoid dwelling upon vague generalities which, while not affecting the great question of the truth or error in his main contentions, yet tend to diminish his influence upon the rising generation.

A charge of almost universal inconsistency between his principles and his conduct, if well founded, must certainly have this effect, and I have therefore thought it my duty to say a few words to show, that on the main point, the address to the Czar, and I believe also on most if not all other points, the charge is an unfounded one.

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Anticipations and Hopes for the Immediate Future (S610)

This short essay was commissioned by a German newspaper, but on seeing what Wallace had written they were unwilling to print it. It ended up in the 1 January 1904 issue of The Clarion.

I am looking to the coming year with no expectation of any great change, political or social, but with a hope and belief that the great movement among the workers in favour of a more rational and more equitable system of government and of social organisation will continue to grow, as it has been growing during the last few years. I trust that, in the more advanced countries – especially in Germany and France – it may become sufficiently powerful, even within the coming year, to exercise a decided control over the reactionary party, and even be able to initiate, and perhaps to secure, some important legislation for the extension of individual freedom, and for checking military expenditure.

As to the future (limiting ourselves here to the twentieth century), I look forward to the same movement as destined to produce great and beneficent results.

The events of the past few years must have convinced all advanced thinkers that it is hopeless to expect any real improvement from the existing governments of the great civilised nations, supported and controlled, as they are, by the ever-increasing power of vast military and official organisations.

These organisations are a permanent menace to liberty, to national morality, and to all real progress towards a rational social evolution. It is these which have given us during the first years of this new century examples of national hypocrisy and crimes against

liberty and humanity – to say nothing of Christianity – almost unequalled in the whole course of modern history.

Scarcely was the ink dry of the signatures of their representatives at the Hague Conference, where they had expressed the most humane and elevated ideas as to the necessity for reduction of armaments, for the amelioration of the horrors of war, and for the principle of arbitration in the settlement of national difficulties, than we find all the chief signatories engaged in destroying the liberties of weaker peoples, without any rational cause, and often in opposition to the principles of their own constitutions, or to solemn promises by their representatives or in actual treaties.

England carried fire and sword into South Africa, and has robbed two republics of the independence guaranteed to them after a former unjust annexation; a crime aggravated by hypocrisy in the pretence that British subjects were treated as "helots," whereas their own Committee of Inquiry into the War has now demonstrated that it was a pure war of conquest, in order to secure territory and gold mines, determined on years before, and only waiting a favourable opportunity to carry into effect.

The United States, against their own "Declaration of Independence" and the fundamental principles of their Constitution, have taken away the liberties of two communities – the one, Porto Rico, by mere overwhelming power; the other, the Philippines, after a bloody war against a people fighting for their independence, the only excuse being that they had been purchased, land and people, from their former conquerors and oppressors.

Russia itself, the originator of the Peace Conference, forthwith persecutes Jews and Doukhobors on account of their religion, and takes away the solemnly guaranteed liberties from the Finns, a people more really civilised than their persecutors.

All three of these Governments, as well as Germany and France, invaded China, and committed barbarities of slaughter, with reckless devastation and plunder, which will degrade them for all time in the pages of history.

Such are the doings of the official and military rulers of nations which claim to be in the first rank of civilisation and religion! And there is really no sign of any improvement. But, for the first time in the history of the world, the workers – the real sources of all wealth and of all civilisation – are becoming educated, are organising themselves, and are obtaining a voice in municipal and national Governments. So soon as they realise their power, and can agree upon their aims, the dawn of the new era will have begun.

The first thing for them to do is to strengthen themselves by unity of action, and then to weaken, and ultimately to abolish, militarism. The second aim should be to limit the bureaucracy, and make it the people's servant, instead of its master. The third, to reorganise and simplify the entire legal profession, and the whole system of law, criminal and civil; to make justice free for all, to abolish all legal recovery of debts, and all advocacy paid for by the parties concerned. The fourth, and greatest of all, will be to organise labour, to abolish inheritance, and thus give equality of opportunity to everyone alike. This alone will establish, first, true individualism (which cannot exist under present social conditions), and, this being obtained, will inevitably lead to voluntary association for all the purposes of life, and bring about a social state adapted to the stage of development of each nation and of each successive age.

This, in my opinion, is the ideal which the workers (manual and intellectual workers alike) of every civilised country should keep in view. For the first time in human history, these workers are throwing aside international jealousies and hatreds; the peoples of all nations are becoming brothers, and are appreciating the good qualities inherent in each

and all of them. They will, therefore, be guilty of folly, as well as crime, if they much longer permit their rulers to drill them into armies, and force them to invade, and rob, and kill each other.

The people are always better than their rulers. But the rulers have power, wealth, tradition, and the insatiable love of conquest and of governing others against their will. It is, then, in the People alone that I have any hope for the future of humanity.

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The Immigration of Aliens (S616a)

Wallace's somewhat surprising conclusions on this subject were expressed in a brief communication printed in the 3 June 1904 issue of The Clarion.

Our great philosopher, Herbert Spencer, who has so recently left us, in one of his earliest works stated the important principle that when a state of society is fundamentally unsound there is, as regards many social problems, no right course, but only a choice between greater and less degrees of wrong-doing. This principle well applies to the case now under discussion – the restriction of the immigration of aliens into England.

That our social system is absolutely wrong and fundamentally unjust admits of no question. With enormously greater wealth in proportion to our population than at any earlier period of our history, we yet have millions of our people living in the most degrading want and misery, although often working for longer hours, and under more unhealthy conditions, than any slaves or serfs; other millions are supported by charity because under our competitive system no employer needs them – and all this occurs in the midst of greater luxury, more wasteful public and private expenditure, than at any previous epoch. So long as this unutterably vicious system exists, how can we expect that any of the evils or injustices that arise from it can be dealt with by considerations of pure ethics? We must right the fundamental wrong before we can deal ethically with any of the subordinate wrongs. That any of our own citizens willing to work should yet have to live idle lives, and be supported in a state of semi-slavery in our workhouses, is a grievous wrong; but it is a still greater wrong to let them starve. Here, then, we have an instance of there being no choice but between a greater and a lesser wrong.

So as regards aliens. We all feel it to be wrong to refuse admission into our country to any foreigner able and willing to work. But when thousands and millions of our own people are struggling for work, and often cannot obtain it, and other thousands are working long hours for barely enough to keep body and soul together, then it may be – and I believe it is – a greater wrong to permit free immigration from every other country, whose people may, perhaps, be enduring a similar struggle, but rarely a severer one, than our own. Our first duty is surely to our own people. The question of the numbers of such immigrants is wholly immaterial to the matter at issue. With so deadly a struggle as ours, any addition to the number of strugglers must tend to lower wages, to make work more difficult to obtain, to force some from a miserable insufficiency into the gulf of absolute starvation.

There is another and more general reason for this view. When free immigration is allowed in any country, it has two bad effects. In the country to which immigrants are

admitted it blinds people to the real causes of unemployment and starvation in the midst of superfluous wealth, while in the country from which the emigration takes place it to some extent relieves the pressure of competition, and enables both the Government and the people to shut their eyes to the real causes of the evil. These causes are, the gigantic social wrong of the private monopoly of land and capital, which, with the right of inheritance, production for profit, and competition for employment, inevitably lead to all the misery and starvation, and most of the vice and crime, that now exists in every civilised nation, and in nearly direct proportion to its wealth.

For these reasons, which I have not leisure to give at greater length, I believe that restriction of immigration is the lesser of the two evil courses at present open to us; and it has this advantage over the other course, that it compels each nation to solve its own social problems. Thus, perhaps, the people's eyes may be the sooner opened, and the cause of humanity advanced.

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Practical Politics (S617)

An impassioned letter printed in the 30 September 1904 issue of The Clarion.

I have for some time been impelled to say a few words on the position taken by our friend, Robert Blatchford,[a] on the question of National Defence, and have only abstained because I have neither time nor inclination for controversy, and in the expectation that some more authoritative and better known Socialist would enter the lists on behalf of what seems to me to be the side of true Socialism – at all events, of what I understand by it. But last week's article contains so many disputable statements, and what seem to me so many erroneous conclusions, that I ask leave to make a few remarks on one or two of them; and though I have no right whatever to speak for Labour or for Socialism, I venture to hope that a considerable number of both these parties will agree with me.

I will first say a few words on the, to me, extraordinary statement that, though fifty years of continuously increasing expenditure on our national defences has resulted in "an inadequate and imperfect" outcome, and what a military writer in the July "Nineteenth Century" called "our pitiable military situation," yet, only give to our rulers unlimited money and conscription, and our defences will instantly become "adequate and efficient." With all respect, this seems to me nothing less than pure delusion. One Government after another has had a free hand to reform our military and naval forces, and all have utterly failed. They have wasted countless millions with no adequate result. And now we are asked to give them more millions to waste, and the very same body of official rulers and organisers and titled officers will suddenly be imbued with wisdom, unselfishness, and economy, and all will be well. Our defences, as by a miracle, will become "adequate and efficient." For what has to be done must be done at once. Germany, we are told, is ready; we are not. Therefore the money and the men must be given to the Government *now*. To any such proposal I venture to hope that, by an overwhelming majority, the Socialist and Labour Parties will reply in the now historic words: "Never again."

[a] Robert Blatchford (1851–1943), English socialist and Editor of *The Clarion*.

But this is only preliminary. We will now come to the real issue. Robert Blatchford proceeds to ask a number of questions, and to offer a number of alternatives, as if they were exhaustive and there was nothing more to be said or done. Shall we leave the Empire defenceless? Shall we abandon our country and our Colonies to the invasion of any Power that cared to take them? Russia covets India. We must either defend India or surrender it to Russia. If we made India a self-governing nation, the result would be civil war and a Russian conquest. More than one foreign Power envies us our possessions. And so on, and so on; with the one conclusion: We must increase Army, Navy, and Home Defences, and be prepared to fight all the world. Not one word about there being any alternative to all this blood-and-iron bluster and defiance; not one syllable to show that the writer is a great Socialist teacher, a believer in the goodness of human nature and the brotherhood of man. "But," he replies by his heading, "this is very good in theory, and very true, but it is not Practical Politics. The danger is urgent. Tell us, ye Labour leaders, what you propose to do *now*."

I am not a Labour leader, but I hope I am a true friend of Labour and a true Socialist; and I will now state the case as it appears to me, and suggest what, in my opinion, is the only course of action worthy of Socialism or politic for Labour, and, besides, the only course which has the slightest chance of succeeding *in the long run*: in one word, the only RIGHT course.

It is a notorious and undeniable fact that we – that is, our Governments – are, with a few exceptions, hated and feared by almost all other Governments, especially those of the Great Powers. Is there no cause for this? Surely we know there is ample cause. We have either annexed or conquered a larger portion of the world than any other Power. We have long claimed the sovereignty of the sea. We hold islands and forts and small territories offensively near the territories of other Powers. We still continue grabbing all we can. In disputes with the powerful we often give way; with the weak and helpless, or those we think so, we are – allowing for advance in civilisation – bloody, bold, and ruthless as any conqueror of the Middle Ages. And with it all we are sanctimonious. We profess religion. We claim to be more moral than other nations, and to conquer and govern and tax and plunder weaker peoples for *their* good! While robbing them we actually claim to be benefactors! And then we wonder, or profess to wonder, why other Governments hate us! Are they not fully justified in hating us? Is it surprising that they seek every means to annoy us, that they struggle to get navies to compete with us, and look forward to a time when some two or three of them may combine together and thoroughly humble and cripple us? And who can deny that any just Being, looking at all the nations of the earth with impartiality and thorough knowledge, would decide that we deserve to be humbled, and that it might do us good?

Now the course I recommend as the only true one is, openly and honestly, without compulsion and without vain-glory, to do away with many of the offences to other peoples, and to treat all subject peoples and all foreign Powers on exactly the same principles of equity, of morality, and of sympathy, as we treat our friends, acquaintances, and neighbours with whom we wish to live on friendly terms.

And, to begin with, and to show that our intentions are genuine, I would propose to evacuate Gibraltar, dismantle the fortress, and give it over to Spain; Crete and Cyprus should be free to join Greece; Malta, in like manner, would be given the choice of absolute self-government under the protection of Britain, or union with Italy. But the effect of these would be as nothing compared with our giving absolute internal self-government to

Ireland, with protection from attack by any foreign Power; and the same to the Transvaal and Orange Free State; and this last we should do "in sackcloth and ashes," with full acknowledgment of our heinous offences against liberty and our plighted word.

Now we come to India, which our friend Blatchford seems to consider the test case. And so it is; for if ever there was an example of a just punishment for evil deeds, it is in the fact that, after a century of absolute power, we are still no nearer peace and plenty and rational self-government in India than we were half-a-century ago, when we took over the government from the "Company" with the promise to introduce home-rule as soon as possible. And now we have a country in which plague and famine are chronic – a country which we rule and plunder for the benefit of our aristocracy and wealthy classes, and which we are, therefore, in continual dread of losing to Russia.

If we had honestly kept our word, if we had ruled India with the one purpose of benefiting its people, had introduced home-rule throughout its numerous provinces, states, and nations, settling disputes between them, and guarding them from all foreign attack, we should by now have won the hearts of its teeming populations, and no foreign Power would have ventured to invade a group of nations so united and so protected. Such a position as we might have now held in India – that of the adviser, the reconciler, and the powerful protector of a federation of self-governing Native States – would be a position of dignity and true glory very far above anything we can claim to-day.

But, it will be replied, all this is foolish talk; it will be a century before the British people will be persuaded to give up its possessions and its power; and, in the meantime, if we do not defend ourselves we shall not have the opportunity of being so generous, hardly shall we keep our own liberties. I have not so low an opinion of my countrymen as to believe that they really *wish* to keep other peoples subject to them against their will: that they are really *determined* to go on denying that freedom to others which is so dear a possession to themselves. And if there is not now a majority who would agree to act as once as I suggest, I am pretty confident that there is, even now, a majority who would acknowledge that such action is theoretically just, and that they would be willing to do it by degrees, and as soon as it is safe, etc. To look forward to it, in fact, as an ideal to be realised at some future time, but not just now.

Now, what I wish to urge is, that it is of the most vital importance to us, now, that all who agree with me that there can be no national honour or glory apart from justice and mercy, and that to take away people's liberty and force our rule upon them against their will, is the greatest of all national crimes, should take every opportunity of making their voices heard. If, for instance, every Socialist in our land, and I hope a very large proportion of workers and advanced thinkers who may not be Socialists, would agree to maintain this as one of their fundamental principles, to be continually brought before the people through the Press and on the platform, to be urged on the Government at every opportunity, and to be made a condition of our support of every advanced Parliamentary candidate, we should create a body of ethical opinion and feeling that would not only be of the highest educational value at home, but which would influence the whole world in their estimate of us. It would show them that though our Government is bad – as all Governments are – yet the people at heart are honest and true, and that it will not be very long before the people will force their Governments to be honest also.

This, I submit, *would* be really "practical politics." At the present day we *have* got so far as this – that none of the Great Powers wages a war of aggression and conquest against another Power without some quarrel or some colourable pretence of injury. But surely the

fact of there being such a party as I have outlined, and especially if it would (as I think it certainly could) compel the next Government to make some of the smaller concessions here indicated and adopt the general principle of respecting the liberties of even the smallest nationalities, would so reduce the amount of envy and hatred with which we are now regarded as to considerably reduce the danger of combined aggression upon us.

I should have liked to say something about Russia, and the fact that *we* are answerable for the present war in the Far East, by so long upholding Turkey, and preventing Russia from acquiring free egress into the Mediterranean, in exchange for which concession she would (after the Russo-Turkish War) have willingly agreed to the neutralising of Constantinople as a free port under the guarantee of the Powers. We had at that time a preponderance of power in Europe, as shown by what occurred at the Peace Congress; but D'Israeli used that power for a bad purpose, as Lord Salisbury afterwards admitted.

I greatly regret being obliged to differ so radically from a man I admire and respect so much as I do Robert Blatchford; but, as I am known to be a Socialist and a constant reader of the *Clarion*, it might be thought that my silence would imply some degree of agreement. The present letter is merely for the purpose of making my views clear on this vitally important question, and with the hope that others who agree with me will not longer keep silence.

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From the Doyen of Science (S618b)

This 1905 letter concerning employee buy-outs was sent to politician and editor George N. Barnes, who printed it in the first issue of a new series run of his magazine Amalgamated Engineers Monthly Journal.

Dear Sir, – I know really nothing of your circumstances, your needs, or your ultimate objects, and, therefore, it is difficult for me to say anything that can be of service to you. I am also too much occupied to do more than say a very few words. But I have long held and expressed the opinion that organised Labour is not doing the best for itself and the community. Whatever may have been the case in the past, it is *to-day* a waste of energy and of means to endeavour to raise your wages by means of strikes. The employers being organised also, are stronger than you are. The time, I believe, has come when organised Labour should devote the funds hitherto spent on strikes upon industrial competition with the employers. It seems to me incredible that a society such as yours cannot among its 90,000 members produce knowledge and ability sufficient to carry on any ordinary engineering works as well and as profitably as can a capitalist employer. It would be worth your while to make any sacrifice to do this, and thus absorb your unemployed members, paying them wages for profitable work instead of allowances while remaining idle.

The economies of such a system would be so great that in a few years you would not have an unemployed member, and the inevitable, the absolutely certain, result, would be that *wages would rise automatically*, and would remain permanently high. Then, with your accumulated capital you would always be ready to purchase the works and factories of bankrupt employers at low rates, because no capitalist would buy without the certainty

of obtaining labour, whereas *your* supply of labour would be inexhaustible. It will be a grand day for the workers when this principle is adopted, of fighting the capitalists by competition instead of by strikes. This is what they will dread, because this method will give *you* the advantage, will render *you* the stronger.

I do not see how this plan can possibly fail, always supposing that you carry it out on thoroughly business lines, and make yourselves a reputation for the highest quality in materials and workmanship. The employers now can demand the highest business capacity, the most skilled workers, the most talented designers and inventors. You would have the same in your own ranks and if not could as readily obtain them; and it is to be presumed that your own members, working for themselves and for the elevation of their class, would not work less efficiently than they do for the capitalist.

If energetically and persistently carried out, and combined with a system of co-operation and thorough education, the movement once begun must inevitably extend, and by the middle of the century almost the whole, if not the whole, engineering work of the country (excluding, I suggest, war material) might be in the hands of the workers themselves.

But as soon as you have successfully shown the way, other Labour societies will certainly follow your example, and we shall then be marching steadily on to the realisation of the co-operative commonwealth.

With best wishes for the cause of Labour, in which alone there is now hope for civilisation and for humanity.

I subscribe myself, your very sincere friend, Alfred R. Wallace.

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Why Not British Guiana? Five Acres for Half-a-Crown (S627a)

In this letter published in The Daily News (London) issue of 27 August 1906
Wallace again (see pages 3–4) extolled the virtues of emigration.

Under this heading two of your correspondents have referred to a passage in my "Travels on the Amazon" (pp. 230–33), in which I describe how a few families of workers might live in certain districts on this river in comfort and even luxury by their own labour. But, to avoid misconception, I think it is necessary for me to say a few words in explanation. In the first place I premise that there must be some half-dozen industrious, fairly-educated working men and boys, and that they must have a clear capital of £50 to tide them over the first year or two.

While not withdrawing one word of what I have there said, I did not, and do not now, put it forward as a means of coping with unemployment and poverty at home. For that purpose, I entirely agree with such of your correspondents as claim that the only immediate and effective remedy is to be found in the various forms of co-operative land cultivation. To those who wish to have some details as to how this is to be practically carried out, I will refer to my chapter upon "Reoccupation of the land: the only immediate solution of the problem of the unemployed," first published in "Forecasts of the Coming Century" (1897), and reissued in Vol. II. of my "Studies, Scientific and Social."

Recurring to my remarks upon the Rio Negro, I wish to point out that the essential feature of my suggestion has been overlooked by Mr. H. H. Smith (in his volume on "Brazil, the Amazon, and the Coast"), who adduces the case of a very industrious German family, settled in the forest a few miles inland from Santarem, on the Amazon, who had encountered numerous difficulties and were quite unable, after several years' hard work, to make a tolerable living. But in this case, as in almost all the other cases of European settlers in Brazil and other tropical regions, failure arises from the fact that they all try to grow produce to sell, instead of for their own consumption, while they buy the necessaries of life at the nearest town. Thus, whether they grow coffee or tobacco, sugar or cocoa, or any other tropical produce, they have to sell it to merchants or middlemen at very low rates, in competition with the large estates which grow a hundred or a thousand times the quantity; while all they buy is in small quantities from the retail traders. They thus sell cheap and buy dear, and being obliged to sell, however adverse the market may be, they often suffer great loss and are unable to procure even the ordinary necessaries of life.

Even less labour than that spent in growing and marketing a crop for sale would suffice to produce all the necessaries of life in abundance after the first year, while in succeeding years more and more of the comforts, and even the luxuries, of life could also be produced. There would, however, be always a sufficient surplus of fruits, vegetables, poultry, etc., to sell at good prices to passing boats, or to exchange for the few tools or utensils that cannot be made at home. The great economies of this mode of procedure are pointed out in my article already referred to, while the variety of necessaries and luxuries that can be easily grown in the most favoured parts of the tropics are indicated in the passage of my book on the Amazon which your correspondents have noticed.

I would like to add here that I should not now recommend Englishmen to go to the Amazon or Rio Negro to try such a self-supporting life among people of a different language and religion, and where their very success might subject them to excessive Government exactions. The experiment might be tried, however, in British Guiana, in an almost identical climate, and with even greater chances of success. In that Colony a genuine settler can have five acres of unoccupied land (comprising almost the whole of the interior forests) for the small sum of half-a-crown, the sole condition being to reside on it for ten years; while any larger amount up to 250 acres can be purchased at a total cost of about 2s. an acre. On any of the numerous rivers that intersect these forests – the Demerara, the Berbice, and the Essequibo, with its many large tributaries – there are doubtless hundreds of localities equally favourable to the settler with those I have described on the Rio Negro, while, as these rivers are all traversed by steamers communicating with the mines and inland settlements, there would be easy communication with the seaports, and a ready market for all surplus produce.

For healthy and hard-working men, with a small capital, who wish to form permanent self-supporting homes, under the most favourable conditions and in an easily accessible country, I do not know of any more suitable Colony than British Guiana.

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Should Women Have Votes: A Symposium (S628b)

Wallace's short reply to an opinion survey as to whether women should be given the vote was printed in The Daily News (London) issue of 12 November 1906.

[The questions sent were:] 1. – Is the franchise desired by any considerable body of women? 2. – Ought it to be granted, and on what grounds? 3. – Should it be on the terms of adult suffrage or of equality with the existing male franchise? 4. – What are the best methods of inducing Parliament to concede it?

DR. ALFRED R. WALLACE.

To the four questions you ask I reply:
1. I think so.
2. Yes, on ground of justice, and also of expediency.
3. As a manhood suffrage at 31 years of age, including both sexes.
4. Petitions, public meetings, motions in Parliament.

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Mr. Carnegie's Greatest Gift (S635b)

The following letter, printed in the March 1907 issue of The Review of Reviews (London), responded to ideas posed by the industrialist Andrew Carnegie in an earlier number of the magazine.

Mr. Carnegie's latest exposition of the "Gospel of Wealth" will be welcome to all advanced thinkers. It is, in my opinion, the greatest benefit to humanity yet rendered by himself or by any other multi-millionaire, since he unreservedly admits the right of the people to inherit the bulk of his and their accumulated wealth whenever, by legal enactment, they so will it. He supports this view by excellent reasoning, on the grounds that in every case the accumulation of these great fortunes is very largely and sometimes wholly due to the industry or the talent of the people and the density of population. Pre-eminently, he points out, is this the case in the increase of land values in great cities and towns, the whole of which is the creation of the community itself, as we land nationalisers have long urged. But for great industrial enterprises he claims that the originators and organisers have some personal claim, since they aid in "the development of our country's resources." The Stock Exchange speculators, however, he declares to be wholly evil, doing no service whatever to the community; but he does not suggest how they are to be dealt with except by taking their whole accumulated wealth at their deaths.

These views he bases on justice as well as on expediency. He objects, however, to taxing incomes, except where these arise from rents, interest, or dividends, for two very good reasons; first, that a general income-tax (as in England) causes the honest man to pay for the dishonest; and, secondly, that its collection is enormously expensive. To collect the taxes on dividends, interest and rents, however, hardly costs anything; while as it taxes realised wealth, leaving earned incomes free, it is in accordance with the soundest principles of taxation. But to make up for this loss he would take the bulk of very large incomes

by means of graduated death-duties, leaving of course a moderate share to direct heirs.

With all this I cordially agree; but while Mr. Carnegie founds his proposals on an enlightened expediency, combined with an effort to determine the just claims of the people to share the millionaire's wealth in individual cases, I have arrived at a similar result by logically applying Herbert Spencer's "law of social justice," which, as I have elsewhere fully explained, is identical with the law of "equality of opportunities," which necessarily implies "equality of inheritance"; and this can only be attained by the State becoming the *sole* inheritor of accumulated wealth. But without equality of opportunity there can be no real individualism, which, as Mr. Carnegie maintains, has led to "the steady progress of civilisation." He is very careful to declare that he is utterly opposed to Socialism or Communism, which, he thinks, would "sap the springs of enterprise"; and he therefore wants the inventor, the manufacturer, and the monopolist to be left with a free hand.

But here I think he is illogical, because, under the present system of unequal opportunity are unequally inherited wealth, a large portion of the invention, intellect, and energy of the community is either lost or misapplied. Only by absolute "equality of opportunity" for every child, from birth through childhood to manhood – in nurture, education, and economic training – can individualism be given full play, and all the powers and talents of men and women be fully utilised for the benefit of the nation.

On such a perfect individualism I would base my hopes for the future of humanity. It would inevitably result in the voluntary organisation of industry and in a widespread co-operation, which might or might not result in a socialistic or communistic state.

I maintain, therefore, that Mr. Carnegie, as an individualist, should adopt my extreme view of *absolute equality of opportunities*, without which the advantages of individualism can be only very imperfectly realised. Neither does my friend Mr. J. H. Levy, the chief exponent of individualism in England, ever refer to this very fundamental point. It seems rather curious that it has been left to a Socialist to uphold the standard of complete and thoroughgoing individualism, founded upon the "law of social justice," set forth in one of this latest works by the great philosopher and individualist, Herbert Spencer! – Yours very truly, Alfred R. Wallace.

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"Economic Chivalry": Some Replies to Bishop Gore (S639)

The 24 May 1907 issue of Public Opinion (London) contained comments by Wallace on an address by Bishop Charles Gore entitled "The Moral Witness of the Church on Economic Subjects."

Dr. Alfred Russel Wallace writes: – "I really do not think Bishop Gore's views worth discussing. He recognises a few of the fundamental evils of the competitive system – shoddy and sham, and adulteration and lying everywhere, affecting our whole system of production and trade, to the deterioration of our race in body and mind, the horrible increase of preventable disease and death, and the still more horrible massacre of millions of infants in all our great cities; and to remedy all this he gives us a few pious opinions – a few small bits of sticking-plaster as the sole remedy for the perpetual wounding and

slaughter on the competitive battlefield of modern civilisation!

"All this is a little too late in the day. Fortunately, the workers themselves, who have seen all these evils at first hand, are beginning to know their power, and to be determined to exercise it. The Bishops and their like recognise not one of the fundamental injustices of our social system – the land monopoly, the capital monopoly, the whole system of wild-beast competition. They will not even recognise that, if they *will* still have competition – individualism – it should be a fair one – all should start even. There should be absolute 'equality of opportunity' for every child from birth to manhood, then an equal start in life, and then only shall we have true individualism.

"But they will not even have that. Oh, no! That would involve interference with property. Sacred property! Hereditary property! Even multi-millionaires must not have their property taken from them, even after their deaths! even though it certainly demoralises their heirs and the thousands that prey upon them. No! Property once acquired, by whatever means, becomes sacred in the eyes of these 'chivalric economists.' Else, why do they not openly and boldly advocate the resumption by the State of all wealth above, say, £10,000 or £20,000 at the death of its owners, for the establishment of equality of opportunity – of a true civilisation, without that daily massacre of the innocents – that deterioration of all that is best in humanity – that has gone on increasing with our increasing wealth?

"This very year, they tell us, our trade – our commerce, our wealth – has increased more than ever before; yet we are still too poor to abolish the life-long misery and actual starvation in the class that creates that wealth. Shame on the men in power, whether lords or commoners, Bishops or commercial princes, who see all this and now talk of 'chivalry' as an adequate remedy!"

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letter to Julia Dawson (S647)

A letter from Wallace to columnist Julia Dawson giving "advice to the Socialist women of Great Britain" was printed in the 27 December 1907 issue of The Clarion.

My friend Julia Dawson asks me for a few words of Christmas greeting and advice to the Socialist women of Great Britain. That is a large order – a great subject – and I must not waste words in compliments. They have done and are doing much good work, and I can only urge them to go on doing it in the firm assurance that every effort will produce its effect however hopeless it may seem at first. The good old rules of patience and perseverance are above all things required. No one is a stronger advocate than I am for the *absolute equality* of men and women in all *rights* and *duties*, *political* and *social*. Many of these rights, especially the right to use whatever powers they possess for the benefit of themselves and their fellows, have already been won and others will follow, until complete freedom is attained.

On one thing, however, I am satisfied – that the right to vote for members of Parliament is in reality *not nearly so important*, in the immediate future, as the right you already possess of the municipal vote and to some extent a share in municipal representation. We

are learning every day that good administration of the laws that exist is often more important to society than the making of new and more stringent laws. It is to *this object* that I would especially direct the attention of all educated Socialist women who have some leisure. They can insist on the various local authorities using the powers they already possess, and whenever any fresh powers are given can see that they do not become dead-letters. *That* is work which they can do *to-day*. To choose legislators and to help in legislation, however much we may wish them to have the right of doing, is yet too great a change to be rushed through a divided Parliament by any amount of energy.

Our various Reform laws, giving workers the vote, have for a long time produced little effect on legislation – but a considerable effect upon administration. Again, therefore, I urge upon women to make the fullest use of the powers they already possess, in the full assurance that they will thus be doing a large amount of social service and be preparing themselves for the fuller freedom that will in due time assuredly be theirs. – Alfred R. Wallace.

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Dr. Russel Wallace and Woman (S650)

A sly letter printed in the 18 January 1908 issue of The Outlook (London).

Sir, – As you have been so good as to call my attention to your correspondent "Eve's" letter, as being "a very shrewd attack" not only upon men in general but upon myself in particular, I presume you will not object to my making a few observations thereon.

"Eve" may be *shrewd* in opening her attack upon myself by the definite statement that I can see "no difference between Cetewayo and Charles Gordon," but I cannot consider it fair fighting to begin with such a "blow below the belt," which nothing in my article[a] (or in any of my writings) justifies.

Again, it maybe a sufficient proof of her sense of humour (a faculty which she seems very anxious to claim for her sex, but which I have never doubted) to ridicule an important portion of my argument without referring to my original and fuller statement of it, as she does when she implies that I ignore altogether "the foolish sentiment we call love," but to do so is certainly neither honest nor even politic. But I suppose the pseudonym – "Eve" – is meant to imply that your correspondent speaks for the primitive woman, who was, according to modern anthropology, a very low type of humanity. If however she had taken the trouble to read the chapter on "Human Selection" to which I called attention, she would have found, among other important considerations, the following sentence: "It would probably come to be considered a degradation for any woman to marry a man she could not both love and esteem, and this feeling would supply ample reasons for either abstaining from marriage altogether, or delaying it till a worthy and sympathetic partner was encountered."

But surely "Eve" is a little out of date in her suggestion as to the "foolish sentiment" being *now* a main factor in determining marriage. Do not such influences as wealth, social position, desire for independence, uncongenial homes, the dread of want, and many

[a] "Evolution and Character." *Fortnightly Review* 83 n.s.: 1–24 (1 Jan. 1908).

other causes arising from our very imperfect social economy, drive large numbers of women into matrimony either without, or in direct opposition to, the attraction of love?

Equally beside the point is "Eve's" "humorous" suggestion (for it is *not* mine) that love of children can only "be tested by competitive examination." I regret that I am compelled to differ so completely from a lady who poses as my maternal ancestor, but I am glad to call attention to *one* statement in her letter with which I entirely agree; and that is, that in discussing the vast and important problem of the future well-being of humanity, I have never had "the least idea of being funny." Yet, if "Eve" will credit the fact, no one is fonder of genuine humour than I am! – I am, Sir, yours, &c., Alfred R. Wallace.

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Is It Peace or War? A Reply by Dr. Alfred R. Wallace (S659)

In 1908 Europe was in a nervous mood. Wallace sent some comments on the situation to the Editor of Public Opinion (London), who printed them in its issue of 14 August 1908.

DR. ALFRED R. WALLACE has sent us the following reply to our article of last week on "Is it Peace or War?":

"The statements to which you call attention in your issue of Aug. 7 are most serious, and, if they are wholly correct, most alarming. In the *Clarion* of same date, R. Blatchford reiterates his former statements as to the preparations of Germany, not only of an enormous fleet, but of a correspondingly extensive flotilla of transports, capable of landing 200,000 men on our Eastern coasts, but of serving no other imaginable purpose.

The "Clarion's" Statement

"In the same paper, Mr. A. M. Thompson declares it to be an equally well-known fact that the fundamental reason for these preparations is not the mere lust of power and conquest by the military party, but the need for expansion of *financial* and *commercial* interests. He declares that 'The German manufacturing districts, overbrimming with exportable products,' lie landlocked behind Denmark and Holland, whose integrity is guaranteed by Great Britain, backed by France and Russia.

"The implication is that these vast preparations of army, fleet, and transports are for the express purpose, when the fitting moment comes, of attacking our Home Fleet on at least equal terms, invading our country with an overwhelming force, and at the same time, with an equally powerful army, taking possession of Denmark and Holland with all their ports, to which the transports can return to bring reinforcements for the invading army in the shortest possible time.

Germany in 1870

"When we remember the marvellous precision with which, in 1870, the German armies invaded France, and marched, ever-conquering, to the siege of Paris, we can hardly doubt that the suggested programme against ourselves can be successfully carried out; and, if the facts and intentions are as stated, and some means of satisfying the aspirations of German commerce and German militarism are not devised, will almost certainly be

attempted, and, if attempted, will probably succeed. Then, of course, such of our Colonies and possessions as Germany specially covets will be either seized by force or obtained as the price of the evacuation of our country.

The "Clarion" and Labour

"In the same issue of the *Clarion* Robert Blatchford has an article on 'The Danger of War,' which, I regret to say, is in my opinion, most unfair to the Labour Party, whose resolution as to the relations between the British and German peoples he either misrepresents or misunderstands. To this the leaders of that party will, no doubt, reply. But his article is, I fear, at the same time, by its almost Jingo spirit, calculated to provoke German criticism or even animosity. For he strongly maintains that *we* are quite innocent, the Germans alone being the guilty party. He says: 'I may point out that it is not a danger of collision between two equally guilty Governments, but a danger of unprovoked, unjustifiable, and wicked attack by an autocratic ruler upon an inoffensive people.' Then, after challenges to the Labour leaders, he goes on as follows:

" 'I repeat my statement:

" '(1) There is grave and present danger of a German attack upon this country.

" '(2) This danger exists because we are not prepared for war.

" '(3) We are not prepared for war because the ruling classes do not trust the people, and because the people do not trust the ruling classes.'

"Although no one admires more than I do Robert Blatchford's great life-work for humanity and Socialism, he here, in my opinion, stultifies himself by adopting the tone and the arguments of the most aggressive Jingoism. *Our* hands, forsooth, are clean! *We* are wholly in the right! Germany has no just cause of offence against *us*! *Our* only course is to prepare at any cost to resist her attacks! This surely is Jingoism run mad, and, as coming from a Socialist and a Humanitarian, is nothing less than amazing.

Our Imperialism

"Let us, therefore, try to look at the matter not from any high ethical standpoint of right and justice, which, unfortunately has no place in practical politics, but from those ideals of national superiority and well-being which prevail among the governing classes of all the Great Powers (our own included), and which they all, either openly or secretly, are aiming at. These are – extent of territory, world-wide commerce, great and increasing population, enormous wealth, and, finally, the Imperial rule over vast territories of subject peoples. Every one of these desired results are, in my opinion – and, I had supposed, in the opinion of all Socialists and lovers of freedom as the first essential of human progress – false ideals, except in so far as they arise naturally as the concomitants of a true civilisation; but they are, nevertheless, the ideals of modern Governments.

Our Superiority

"Now, owing to a series of favourable conditions, we ourselves, more than a century ago, obtained a decided superiority in most of these elements of greatness, and during the past century we have taken every opportunity of increasing our advantages, due mainly to our naval and manufacturing superiority, which culminated about fifty years ago; and we have done this from the one point of view of supposed self-interest, with no regard whatever for the actual or possible desires of other nations. As the result of this abnormal growth, the other Great Powers now find all the most suitable regions for European colonisation and commercial expansion monopolised by us, or our countrymen. The

whole of temperate North America is thus held by people of English race. We have also the whole of Australia and New Zealand, and by far the largest and best portions of South Africa. In addition to all this, we have an enormous Empire in India, and our full share of the tropical regions of all the continents and islands.

Our Boast of Empire

"And we are proud of all this. We boast of our Empire on which 'the sun never sets'; and lose no opportunity of expressing our determination and vaunting our ability to keep it. Again and again we have waged unjust wars, as those against China, Burma, Egypt, and North-West India; while our last exploit – the most unjust and disgraceful of all – was the conquest of the two Boer Republics, after a petty quarrel deliberately founded on fraud and aggression, and a war begun on false pretences and culminating in such an orgy of devastation and bloodshed and the breaking of treaties as to make every lover of justice and humanity ashamed of being the fellow-countrymen of those who were responsible for it.

"To Be Left Alone"

"And now, Robert Blatchford assures the world that all we want is peace! All we ask is to be left alone to enjoy our ill-gotten wealth and territories! How very good of us! How virtuous of us to want peace when there is nothing more to be got by war! And how conciliatory it is to tell the Germans that *they* must be content to let us possess the earth in quiet and that we cannot allow them even to absorb a few remaining bits of Europe or Asia, which we do not want ourselves, but which we are determined they shall not have. And *We* have just taken final possession of Egypt – also in defiance of the most solemn promises to Europe of a speedy and definite evacuation.

"Our Own House"

"And to think that all the time we have been thus plundering and blundering over the whole globe, our people at home remain plunged in a slough of pauperism and misery of greater extent than ever before. Oh, the pity of it! That we have been ever seeking to conquer *other* lands, to subjugate *alien* peoples, to heap up the wealth of the few, but have always found it quite impossible so to order our own house that no willing worker should ever be in want of the mere necessaries of a decent, a healthy, and a contented life.

The Two-Power Standard

"The outlook is, indeed, for us a desperate one; and I see myself only two possible ways of honourably avoiding a not remote catastrophe – though both courses will, I know, be regarded as Utopian.

"The first is that our Government, our rulers generally, and the most influential of our newspapers should renounce altogether our claim to interfere (except peacefully as the friend of all parties alike) in the internal relations of the Continental Powers; that we should abandon the insolent determination to keep *Our* navy equal to that of the two most powerful navies of the other Great Powers, and that we begin at once to amend our ship-building programme so as to keep it up to the one-Power rather than the two-Power standard.

"In the existing state of the political world, with Germany and the United States already superior to us in population and fully our equals in wealth and civilisation, any other pretension must lead to ultimate ruin. Is it not probable that German-speaking Austria will before long throw in her lot with existing Germany and form an Empire

which will hardly be content to allow us any longer to interfere with whatever it considers to be its legitimate expansion?

But if, instead of persisting in these irritating and provocative claims, for which there is no logical or ethical justification, we directed our whole energy to the amelioration of the *social* condition of our people at home, and also took some immediate and effective measures for the initiation of *self-government* in Ireland, in Egypt, and in India, we should almost certainly disarm the active opposition of the other Powers. Thus alone shall we secure 'Peace with Honour.'

The Socialist Alternative

"But if we will not do this – if we continue making claims to superiority which are an offence, and increasing our armaments, which are a defiance, then the only hope I can see for us is that the growth of Socialism, both among ourselves and in Germany, may be so rapid during the next few years that Socialists may become powerful enough in both Legislatures to ensure a programme of peace and mutual goodwill, before which all the objects for which wars are now waged will sink into insignificance.

"With the earnest hope that one or the other of these solutions may commend itself to all real lovers of freedom for others equally with ourselves, – I am, Yours truly, Alfred R. Wallace."

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1909. For What Should We Strive? (S665)

In the 30 December 1908 issue of The Christian Commonwealth Wallace (and a number of others) framed a response to this question.

We boast of our country as being the wealthiest in the world, yet millions of men, women, and children are without the bare necessaries of a healthy life – food, clothing, warmth, fresh air, rest, and recreation. Many thousands die annually from actual want or from unhealthy occupations.

To abolish this monstrous disgrace is the first duty of the British people in 1909: as I declared it to be 1898. To do this thoroughly and permanently there is only one way – to organise and train these suffering millions in rural communities, so that they may produce the necessaries of life for their own consumption. This is the simple, direct, and economical method of abolishing this hideous feature of modern civilisation; and it is a method which is certain of success, if we wish it to succeed.

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Flying Machines in War. Dr. A. R. Wallace Calls to Action. (S670)

A new kind of threat brought a Wallace letter to the 6 February 1909 issue of The Daily News (London).

Sir, – For several months past I have been hoping to see some protest made in the more advanced journals against the assumption, tacit or openly avowed, that the first and most important use of aeroplanes and other flying machines or dirigible balloons will be to drop explosives or use other implements of destruction, in case of war or as the first act of war.

No doubt thousands of persons, besides myself, have received a programme of the "Aerial League of the British Empire," whose aims are stated to be: "To secure and maintain for the Empire the same supremacy in the air as it now enjoys on the sea." A preliminary list of vice-presidents of this "League" contains the names of three peers, three bishops, seven members of the House of Commons, and eleven men of "war" from Rear-Admiral and Major-General downwards. Sir Hiram Maxim is quoted as saying that in less than a year there will be machines in Paris which can reach London in four hours carrying a load of half a ton over and above all necessaries, and that such machines will be in use in the very next war.

It is clear, therefore, what is in preparation, and what is, apparently, held to be inevitable by the great war party at home and abroad; and this culminating iniquity of our civilization will certainly be consummated if the parties of peace, of humanity, of social reform, and of common sense do not at once bestir themselves. Just as we were assured that war with the Boers was "inevitable" by an influential body which had determined to bring on a war for the conquest of the two free Republics (whose freedom we had guaranteed), so now we are inferentially told that this new horror is "inevitable," and that all we can do is to be sure and be in the front rank of the aerial assassins – for surely no other term can so fitly describe the dropping of, say, ten thousand bombs at midnight into an enemy's capital from an invisible flight of airships.

If there ever was a time to call upon a Liberal Government to dissociate itself from this proposed crime against humanity it is now. If ever there was a time when we should take the initiative against adding this new horror to the horrors of war (which all civilized Governments profess to be eager to diminish) it is now. Surely the peace party, the Labour Party, the Irish Party – all who are Liberals in thought and act as well as in name, the party of humanity – perhaps even the Christians, if such a body still exists among us – will for once unite to declare that Britain shall not disgrace itself by silent acquiescence in this absolutely evil deed – this crowning wickedness of the combined forces of war and capitalism.

Surely, for this great and holy purpose, the whole body of true womanhood and true manhood will unite, and call upon our Government instantly to open negotiations with other civilized nations, individually, proposing to each one, separately, a mutual agreement or treaty declaring that no airship of theirs shall carry explosives or any destructive implements; that doing so beyond their own territory shall be held to be piracy on the part of non-combatants and an act of war on the part of Governments. Let them propose this agreement, first, with our brothers in race and language across the Atlantic, next with Germany and France, in each case the contracting Powers to support each other in giving effect to this imperative extension of international law.

If we thus take the initiative, I can hardly conceive the great American people refusing to join us; and then the probability is that the new law will be universally accepted. There are certain considerations which cannot be discussed now which render it probable that the Great Powers have even more to gain than the smaller ones in this restriction of war to the two elements, on which it has raged from time immemorial. But whether or not

this is the case, it is quite certain that all must be losers by it to an, at present, incalculable extent; and perhaps this consideration, if all others fail, may cause them to accept the lead of any Great Power which first declares its determination to have no part in this deliberate and almost demoniac extension of the cruelties and the horrors of war. – Yours etc., Alfred R. Wallace, Old Orchard, Broadstone, Wimborne.

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Dr. A. R. Wallace and Woman Suffrage (S671)

A short article printed in The Times (London) issue of 11 February 1909 contained a letter from Wallace read at a political event.

At a meeting in support of woman suffrage at Godalming last night, at which Sir William Chance presided, a letter was read from Dr. Alfred Russel Wallace, O.M.

Dr. Wallace wrote: – "As long as I have thought or written at all on politics, I have been in favour of woman suffrage. None of the arguments for or against have any weight with me, except the broad one, which may be thus stated: – All the human inhabitants of any one country should have equal rights and liberties before the law; women are human beings; therefore they should have votes as well as men. It matters not to me whether ten millions or only ten claim it – the right and the liberty should exist, even if they do not use it. The term 'Liberal' does not apply to those who refuse this natural and indefeasible right. *Fiat justitia, ruat cœlum*."

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Dr. Russel Wallace on Insurance Act (S691)

A letter of support published in the 25 January 1912 issue of The Daily Chronicle (London).

We publish below from the pen of Professor Alfred Russel Wallace, O.M., F.R.S., a striking letter on the Insurance Act, in which he forecasts its beneficent effect on the nation.

The views of Professor Wallace, who celebrated his 89th birthday a few days ago, will command widespread interest and respect, coming as they do from the greatest living representative of the Victorian era.

Professor Wallace has won world-wide fame as a scientist and philosopher, and as the co-discoverer with his friend Darwin of the principle of natural selection.

Sir, – By an extraordinary mistake, the exact source of which I have not been able to trace satisfactorily, it has been very widely stated in the Press that I am an opponent of the National Insurance Act. This is not only untrue, but the more I learn about its provisions and mode of working the more inclined I am to look upon this Act as perhaps the greatest

and on the whole the most beneficial of all the attempts yet made to grapple with the great problem of poverty. Yet some of the most advanced Socialist papers declare it to be the very worst Act ever passed by our Parliament, and one against which the reaction of public opinion will be so strong as in a year or two to result in the downfall of Mr. Lloyd George and of the present Government.

Compulsion Justified.

The reasons given for this adverse criticism are, firstly, that it is compulsory, which fact it is supposed will neutralise all the chief advantages of insurance when voluntarily undertaken; and, secondly, that the tax upon employers will be so onerous that it will inevitably be counterbalanced by a reduction of wages or by the discharge of less efficient men, so that the workers will ultimately pay for the whole of it themselves, and will, therefore, be worse off than before. The additional statement is generally made that Mr. Lloyd George and the Government know this very well, and that it is only a temporary bid for popularity in view of the next election. I should like to make a few remarks on these two objections, which, being a kind of half-truths, their fallacy is not always apparent.

As to compulsion, all remedial legislation in social matters, to be effective, must be enforced by law. All our taxes are severely compulsory, but rarely, except when they involve some gross interference with personal liberty or with matters of conscience, do they lead to any strong opposition. Our Poor-laws, administered by so-called "guardians" of the poor, involve compulsory taxation, while they have produced such a state of things that thousands of those who are in want from no fault of their own prefer starvation to the workhouse. Yet there is no great outcry against the payment of poor rates. Voluntary charity in a myriad form is constantly increasing, but it is so badly administered that, as the most exact inquiries demonstrate, in all our great cities about one-fifth of the whole population exists in a state of want, which is liable at any moment to become actual starvation, involving chronic illness, and culminating in preventable death.

Incidence of Contributions.

The second objection – that the whole of the benefits to be received under the Act will be ultimately paid for by the workers, and will therefore make the poor still poorer than they are now – is an even more insidious one, because it is founded on the indisputable truth that all taxation, whether actually paid by the rich or by the poor, ultimately falls upon the real producers of wealth, the workers themselves. This is clearly laid down by Adam Smith in the very first paragraphs of his immortal "Wealth of Nations." But the ultimate may be very different from the immediate results, and in this case they certainly are so, because the whole process by which the incidence of taxation is transferred from one class to another is a highly complex one, and is often difficult to follow with accuracy. It is assumed, for instance, that the employers' contribution to the insurance fund will certainly and almost immediately be counterbalanced by a fall in the rate of wages. But those who assume this forget that it may be easier for the employer to make economies in other directions, and this will almost certainly be done when the combined workers are prepared to resist any reduction of wages by a strike, which they will be better able to do because the number of persons withdrawn from the labour market in the early stages of illness or by treatment in the sanatoria will constantly tend to raise wages, and the employers will often find it better to shift the burden on to the consumer by a slight rise in prices.

Again, it is rarely noticed by these objectors that those receiving the lowest wages

pay no contribution at all, those a little better off contribute only one penny a week, while it is only the better paid among the skilled labourers who will pay the full contribution of 3d. a week. Among the wage-earners themselves, therefore, the very lowest will be comparatively the best off; and it is the most absurd travesty of the facts to say, as is often said, that the very poorest will be worse off than before.

Gain to the Community.

I look upon it as almost certain that the employers will not attempt, and, if they do attempt it, will not succeed in, recouping themselves by the apparently simple but really most difficult method of reducing wages, since it is much more in accordance with the natural tendency of the Act to produce automatically a slight rise of wages sufficient at least to compensate the better organised workers for their own contribution to the fund; while the amount contributed by the State will certainly be a clear gain to taxpayers in general, because it will be very largely paid by the wealthier classes in the form of land taxes, death duties, &c., and also because it will, to a considerable extent, be balanced by a reduction in poor rates and in the need for private charity. Again, though the wealthy who employ many servants at high wages will pay a large portion of it, yet, as the tax is on the number of servants and not on the amount of their wages, this class of people will really pay less in proportion than the middle classes, and will thus have no reason for complaint. As the amount is so small in each case, the middle or upper classes will rarely, if ever, reduce the number of their servants or lower their wages, however much they may threaten to do so.

I conclude, therefore, that this great Act in its immediate results will be an enormous boon to the poorer classes of the community. It will save thousands of lives now being lost through inability to have early medical care, and will in thousands of other cases remove the hated spectre of the workhouse which darkens their latter years.

No one knows better than Mr. Lloyd George himself that this Act is only a beginning, but a very comprehensive and well-thought-out beginning, which may be, and probably will be, carried out to its legitimate conclusion – the diminution of extreme poverty and the extinction of unmerited starvation.

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Opposition [to Irish Home Rule] Due to Ignorance (S692a)

In its February 1912 issue Nash's Magazine printed a collection of opinions – including Wallace's – it had solicited as to whether Home Rule for Ireland seemed to be a "foregone conclusion."

Opinions however violent, or epigrams however clever, are absolutely worthless on the question of "Home Rule." All opposition to it is due either to ignorance or prejudice: especially to ignorance of *Irish history* and of *Irish character* and to prejudice against the Roman Catholic Church.

The *facts* as to the history of Ireland are an eternal disgrace to England; the facts as to the *character* of the Irish people place them at a considerably higher level than Englishmen as regards morality, imagination, and many of the more attractive intellectual qualities.

Even if the Irish were a minority in Ireland, they would be better fitted to rule their own country than we are. – Alfred Russel Wallace, Old Orchard, Dorset.

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A Policy of Defence (S698)

A costs-saving suggestion published in The Daily News & Leader (London and Manchester) issue of 9 August 1912.

Sir, – I wish to give my cordial assent to Mr. Massingham's proposal that Liberals in Parliament who disagree with the present mad and apparently indefinite extension of our already monstrous fleet should, as a matter of principle, vote against the Estimates whenever they involve any further increase of expenditure upon it. The present policy seems to me to constitute an incitement to war, instead of being a safeguard against it.

But I also wish to lay before your readers, and also to invite the opinion of non-official experts, on an alternative policy, which, if begun now, will, in a few years, and at a comparatively small cost, render us absolutely secure against invasion, even if every one of our warships were scattered over the seas in defence of our Colonies or of our world-wide commerce.

It is, I believe, an admitted fact that a battery on shore with the same armament as those of our Dreadnoughts is much more effective, both on account of its absolute stability and of the means available for getting the exact range of any attacking vessel. If such batteries are composed almost wholly of earthwork, each one would cost, perhaps, not a hundredth part that of a ship of corresponding power; while instead of being liable to destruction by accident even in time of peace, and of becoming obsolete in a few years, the fortification would be almost indestructible either by an enemy or by the forces of nature. The disproportion in cost is so enormous that it seems probable that all our seaports, as well as every vulnerable point of our entire coast line, could be absolutely protected against the combined navies of Europe at a cost less than that of our existing navy. Such a defence would comprise sunken mines and torpedoes wherever considered necessary; while searchlights might be so placed as to enable us to see an enemy at night without affording any knowledge of the exact position of our guns. The earthworks might be constructed by our Territorials, who would be afterwards in charge of them.

The colonies would probably follow our example, and all necessity for incurring the enormous cost of building more warships, as well as the tremendous annual burden of their manning and upkeep, become available for great social reforms. Of course, this proposal will be rejected with scorn or ridicule by the various parties interested in our mad expenditure being continued and indefinitely increased. Naval officers of every grade will look for better chances of employment and promotion; while the whole army of contractors for the ships, their armament, and the supply of fittings, food, etc., will be clamorous for even more powerful vessels, and insistent on the necessity for adequate fleets in every part of our vast Empire. But the opinions of all such interested parties should be treated as absolutely worthless.

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letter to *The Daily Citizen* (S703b)

One of Wallace's last writings, printed in The Daily Citizen (London & Manchester) issue of 8 October 1913.

Most heartily do I congratulate *The Daily Citizen* upon its first birthday. I have read the paper from the commencement. I feel that its clear, outspoken policy and fearless enunciation of principles are to be commended. I think you are doing wonderfully well; better than I expected, realising what it must mean to run a daily newspaper. Such a paper as *The Daily Citizen* was greatly wanted, and you must be doing an enormous amount of good. I hope the circulation will increase and that the workers will realise the value to them of such papers. Much is yet to be accomplished, and it is for *The Daily Citizen* to hammer away for improvement in the conditions of living and other things. Experts declare that the great bulk of the workers are receiving less than a decent living wage, and *The Daily Citizen* should continually insist upon every worker having a continual increase, until his wage is really a living one. Any idea of decrease should be most strenuously opposed. We now want to see the fruits of the Parliament Bill realised, but to have a General Election now would be utter madness. I trust *The Daily Citizen* will press upon the Government the importance of the proposed land legislation being thorough and going to the root of the matter.

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Section 9. The Anti-Vaccination Campaign

Introduction

The reasons for Wallace's attraction to the anti-vaccination movement were essentially two in number. First (but not necessarily more importantly), his review of the available statistics on the incidence of smallpox and other diseases for the previous hundred years or more suggested to him a pattern of response to improving public health facilities than it did a response to vaccination. Second, he perceived in the mandatory administration of vaccination an infringement of individual rights. On the second matter the tide was eventually turned, as some years after Wallace's death mandatory vaccination was revoked.

To what extent Wallace was correct in his assessments on this matter is still in some question. Certainly a number of points the anti-vaccination lobby made were valid; for example one can easily understand the doctors of the time being unwilling to register deaths on the basis of their administration of vaccines, whether lethal infections or possibly impure vaccines might have been involved.

There is a second aspect of Wallace's approach that perhaps merits greater historical interest: his decision to mount criticism on the basis of statistical, as opposed to anecdotal, evidence. Wallace was a good descriptive statistician, and his efforts may be regarded as an early attempt to put epidemiology on statistical grounds. Sometimes he went too far; the American philosopher Charles Peirce once referred to his efforts as being "lawyerly" in nature, implying that there were occasions when he ignored counterfactual evidence. This may be a fair criticism, but it is also true that many of the statistical arguments he mounted have never been completely met.

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Vaccination Judged by Its Results (S376a)

Wallace was not afraid to pull out the numbers when the situation warranted, as in this letter printed in the 24 March 1885 issue of the Pall Mall Gazette.

It is a very common notion that the utility or uselessness of vaccination is a question for medical men alone, and that no other persons are competent to give a judgement upon it. But this is not the opinion of the best authorities. In the Parliamentary Report on "The History and Practice of Vaccination" (1857), it is rightly asserted that "evidence on the protectiveness of vaccination must now be statistical." Now in order to deal properly with the statistics of any question it is necessary to be completely unprejudiced, and also to have some special aptitude for figures. In both these qualities the medical profession are deficient. Having upheld the practice of vaccination for eighty years, and drawn from it

£100,000 a year of public money, both professional prestige and personal interest are at stake, and we should be unwise to expect from men in this position a perfectly unbiassed judgement. At the same time much evidence can be adduced to show that they have an especial faculty for making mistakes in figures. I can only here refer to such cases as that of the National Vaccine Establishment, whose annual reports gradually increased the one unchangeable fact of the London small-pox mortality before vaccination from 2,000 to 5,000 annually, the true number, as given by Dr. [William] Farr, being 1,740. Sir Lyon Playfair gave this same fact as 4,000. Dr. W. B. Carpenter published the statement that in the last century the small-pox mortality in London in six months was often greater than it now is in all England in any whole year; the greatest year's mortality in London being 3,992 in 1772, while it was 23,000 in England and Wales in 1871! Surely, then, we are justified in doubting the special arithmetical and statistical skill of doctors of medicine. We now possess a valuable body of facts as to mortality from small-pox and other diseases in the Registrar-General's Reports for the last forty-six years, and in other official returns. I propose, therefore, briefly to review these and other statistics, and to point out the conclusions which may logically be deduced from them.

The supporters of vaccination rely mainly on four classes of facts as proving their case: – (1) The great decrease of small-pox mortality after the discovery of vaccination; (2) the continued decrease of small-pox in proportion as vaccination becomes more general; (3) the almost complete security afforded by revaccination; and (4) the much greater mortality of the unvaccinated than the vaccinated.

1. That a very great and sudden decrease in small-pox mortality occurred about the time of the discovery of vaccination is quite true, but that it was the result of vaccination there is no proof whatever. Small-pox attained its maximum in London more than thirty years before vaccination was heard of, and during the following thirty years it steadily declined. A real and valid cause for this decrease existed in the disuse of inoculation, which it is universally admitted tended rather to spread the disease than to check it. Hence the more rapid decrease of small-pox mortality under incipient vaccination than under inoculation. Small-pox began to diminish under improved conditions of life, just as leprosy, plague, and scurvy have successively diminished and disappeared. Our greatest statistician, Dr. Farr, tells us that "Small-pox attained its maximum mortality after inoculation was introduced. The annual deaths from small-pox (in London) from 1760 to 1779 were on an average 2,323. In the next twenty years, 1780 to 1799, they declined to 1,740. The disease, therefore, began to grow less fatal before vaccination, indicating, together with the diminution of fevers, the general improvement of health then taking place."

In Sweden, where accurate registers have been kept for more than a century, the same phenomena are noticed. Small-pox mortality decreased suddenly after the year 1800 to an amount quite incommensurate with the very partial adoption of vaccination. In 1816 vaccination was made compulsory in Sweden, and since that date small-pox mortality has rather increased than diminished, the epidemic of 1874 being more severe than any since 1800.

2. Figures are often adduced to show that smallpox decreased after vaccination was made compulsory in 1853, and again after it was made penal in 1867. But the disease manifests itself in a series of epidemics of differing intensities and at irregular intervals, and it is easy so to manipulate the figures as to show any proportionate decrease that may be desired. In my pamphlet, entitled "Forty-five Years of Registration Statistics," I have laid down the small-pox mortality per million in a curve, and have shown by correspond-

ing curves the official vaccination per million and the mortality from typhoid fevers. These curves show that, with much irregularity, there has been a slight progressive decrease of small-pox mortality for the whole period of official registration, while there has been a decidedly greater decrease of typhoid fever. What has caused this latter decrease of mortality? Surely the answer will be, improved general sanitation with improved medical treatment. But that which will account for the greater effect will surely account for the less effect in an allied disease. Again, if we look at the curve of vaccination we find that during the later epoch of the penal law there has actually been less official vaccination than during the preceding epoch, except during the panic caused by the great epidemic of 1871-2, and that epidemic followed immediately after eighteen years of a very high rate of official and enforced vaccination! Surely, then, the assertion that more complete vaccination is always followed by a decrease of small-pox mortality is not supported by the evidence.

3. For fifty years re-vaccination was pronounced to be impossible or useless. Now we are assured that is it our only safeguard, that it affords "full security," and that our re-vaccinated soldiers and sailors hardly ever die of small-pox. By a recent return we have full statistics of small-pox in our two services. Both are rigidly re-vaccinated, both consist of picked men, in both there is constant medical and sanitary supervision; yet the average of twenty-three years gives 83 per million as the annual mortality in the army, and 157 per million in the navy. Whence this difference? Surely some different conditions of life in the navy must be the cause; yet the advocates of vaccination assure us that sanitary conditions have little effect on small-pox, but that vaccination is everything. Now let us compare the navy mortality with that of some large towns during the same twenty-three years, and we find this astounding result, that five towns – Manchester, Leeds, Brighton, Bradford, and Oldham – with all their unsanitary conditions, poverty, overcrowding, and deficient physique, had a much lower adult small-pox death rate than the navy, the highest (Manchester) being 131 per million, and the lowest (Oldham) 89 per million. The fair conclusion from these facts is, I maintain, that the re-vaccination of our sailors is injurious to them, for with all the advantages they enjoy of special physique, abundant food, and constant medical supervision, their small-pox mortality ought to have been very much less than that of any dense manufacturing town.

4. The last and most frequently adduced argument of the vaccinators is the alleged greater mortality of unvaccinated than of vaccinated small-pox patients. Dr. Lyon Playfair stated in the House of Commons that "an analysis of 10,000 cases in the metropolitan hospitals shows that 45 per cent. of the unvaccinated patients die and only 15 per cent. of the vaccinated." This and similar allegations form the sheet-anchor of the vaccinationist; but we are prepared to show that they are in part untrue in fact and altogether delusive in the conclusion drawn from them. We first say they are, *primâ facie*, untrue in fact, because we have ample hospital statistics of the last century, both in England and abroad, and they all agree that the mortality of patients, when all were unvaccinated, was about 18 per cent., exactly the same as the average mortality of our hospitals to-day! Next, we say that the system of determining the vaccinated is radically unsound, since direct evidence that the patient was vaccinated is set aside at the will of the medical attendant. Lastly, we can show that the greater death-rate of the unvaccinated, as a whole, is compatible with an equal or lower death-rate at each corresponding age, because the proportion of unvaccinated infants under one is enormously higher than at any other age, while the mortality of infants from whatever epidemic is excessively great. If we substitute the word "bap-

tism" for "vaccination" exactly the same conclusion would be arrived at. The small-pox mortality of the "unbaptised" would be always greater than that of the "baptised," and thus we might have baptism equally recommended as a sure preservative against small-pox.

To make this clearer let us give an illustrative case with such figures and proportions as are known to be probable. Of 10,000 cases of small-pox let 3,000 be infants and 7,000 adults. Of the infants let 2,000 be unvaccinated, and of the adults let only 500 be unvaccinated. Let the mortality of the infants be 60 per cent., and of the adults about 3 per cent., in vaccinated and unvaccinated alike. The result will be as follows: –

Vaccinated 1,000	infants …..... 600 deaths (= 60 per cent.)
Vaccinated 6,500	adults …..... 195 deaths (= 3 per cent.)
Totals 7,500	795 = 10 ½ per cent.
Unvaccinated 2,000	infants ..... 1,200 deaths (= 60 per cent.)
Unvaccinated 500	adults ..... 15 deaths (= 3 per cent.)
Totals 2,500	1,215 = 49 per cent.

Thus we see that even if the mortality of infants and adults is exactly the same in the vaccinated and the unvaccinated, yet by lumping the two together an enormously greater mortality is shown for the unvaccinated, merely because the unvaccinated predominate among infants, whose mortality is always greatest, and the vaccinated predominate among adults, whose mortality is always less. The whole thing is a mere arithmetical puzzle, utterly valueless and delusive, yet it has been again and again adduced as conclusive evidence of the value of vaccination, and probably has had more weight with the public and the Legislature than any other argument whatever.

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Forty-five Years' Registration Statistics. A Correction. (S509)

In this letter, published in the 1 February 1895 issue of The Vaccination Inquirer, Wallace makes a small but important point.

Sir, – While thanking my friend Mr. Alex. Wheeler for his too complimentary references to the little I have done for the cause of freedom as regards the tyranny of the Vaccination laws, I wish to make a remark as to one portion of his article which conveys an erroneous impression. Mr. Wheeler says that he could not agree with my conclusion that "Vaccination may have caused more deaths than smallpox itself." This I am not surprised at, because I do not myself accept such a statement, which is certainly not mine. My words, carefully chosen, are – "an operation which has admittedly caused many deaths, which is probably the cause of greater mortality than smallpox itself" – and I call attention to the change from the past tense in the first part of the passage to the present tense – "is probably the cause" – in the latter part. This clearly means, not that "Vaccination may have caused more deaths than smallpox" – as Mr. Wheeler states it, without any limitation of time, which would of course be an absurdity – but that, at the *present time*, as the result of general Vaccination for about fifty years, it may *now* be the cause of more

deaths than smallpox. This conclusion is drawn from the table of the steadily-increasing mortality from certain inoculable diseases (page 24 of my pamphlet), which *increase*, in thirty years (1850–1880), was 357 per million (an increase which has continued since), while the deaths from smallpox have not, for many years, averaged more than *one-fifth* of this amount. If, therefore, only *one-fourth part* of the large and steady increase of these diseases is due to Vaccination, then my belief that Vaccination *is now* the cause of greater mortality than smallpox itself is fully justified; and in the contention that this is "probably" the case I do not think that I shall find myself in the minority among the readers of the *Inquirer*. This indirect effect of Vaccination is further increased by its direct effects, which are now known to be far more terrible, and to produce far greater mortality than was formerly suspected or admitted.

I wish to take the opportunity of requesting such of your readers as may have copies of my pamphlet to erase from line 11 on page 21, to line 9 on page 22, 2nd edition (or, in the first edition, from line 8 on page 20 to line 4 on page 21 – Ed. V. I.), as the figures and conclusions therein are erroneous.

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Mr. A. R. Wallace and Vaccination (S538)

A letter to the Editor printed in the 26 March 1898 issue of Lancet – not one of Wallace's supporters on the matter of the efficacy of vaccination.

Sirs, – I thank you for the notice of my pamphlet, "Vaccination a Delusion," in your issue of March 12th, at page 734, and especially for pointing out a verbal error in the introductory passage which, however, has no bearing on the main argument. It shall be corrected in future issues and your notice of it leads me to hope that there are few such errors in the more important parts of the work. I also thank you for giving what purports to be a quotation, being duly enclosed in inverted commas, but which is *not* what I have said and has a different meaning (see p. 13 at top). It is really a compliment to be thus misquoted by an opponent, because it implies such a plentiful lack of fact or argument against the author's real statements or contentions. Besides, it furnishes me with another example, to add to those I have given, of the inaccuracies of the medical profession when dealing with this question. Equally complimentary is it that immediately after commenting upon your own misquotation you run away from my book to quote some quite unimportant portions of my examination before the Royal Commission eight years ago. I tendered evidence on the statistical side of the subject only, but the Commissioners insisted upon questioning me on medical and other matters as to which I knew little or nothing and of which ignorance – not claiming omniscience – I am not in the least ashamed. Again thanking you for your unintentional, but none the less acceptable, testimony as to the unanswerable character of the facts and arguments in my book. – I am, Sirs, yours truly, Alfred R. Wallace, Parkstone, Dorset.

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correspondence concerning Dr. Bond and small-pox vaccination (S542, S544 and S548)

Three single-paged letters to The Daily Chronicle (first letter) and The Echo (last two letters), respectively, commenting on criticisms of Wallace's anti-vaccination position raised by Dr. Francis T. Bond of the Jenner Society.

Dr. Bond and Mr. A. R. Wallace (S542: 12 May 1898)

Sir, – Dr. Francis T. Bond, honorary secretary of the Jenner Society, has, in several local papers, referred to my essay, entitled "Vaccination a Delusion; its Penal Enforcement a Crime," in uncomplimentary terms, and I beg you to allow me, through your widely-circulated paper, to challenge Dr. Bond to give a proof of his statements.

In the "Shrewsbury Chronicle" (April 28) he says that my essay "is crammed full of misrepresentations and fallacies"; in the "Cumberland Advertiser" (March 19) he declares that it is a "collection of wild assertions, flagrant fallacies, and incredible blunders"; and in the "Leicester Daily Post" (April 9) that it is a "specimen of wild and inconsequential rhetoric, and utterly fallacious statistics on this subject." Now my alleged "wild assertions," "flagrant fallacies," and "inconsequential rhetoric," are all based upon a large body of statistics, which Dr. Bond declares to be "fallacious." If they are "fallacious" – that is, either actually erroneous, or directly opposed by other statistics of at least equal extent and of greater authority – I will admit that some of Dr. Bond's assertions may be justified. I ask him therefore to prove, by indisputable facts, first, that the statistics I have set forth in my twelve diagrams (all the authorities for which are given by me) are either themselves unreliable, or have been falsified by me; or, secondly, that there exist other statistics, equally extensive and of greater authority, which I have not made use of, and which lead to directly opposite conclusions.

For example, my first diagram exhibits the death-rates in London from small-pox, from all other zymotics, and from all diseases; and, if correct, proves that these various death-rates all generally agree; sinking and rising together during the same periods; thus strongly suggesting the same general causes, and showing no indication of a special cause affecting smallpox more favorably than other diseases. This diagram alone is conclusive against the claims of vaccination; it is founded on statistics given by the Registrar-General and the Royal Commission reports, extending over a period of 136 years. Will Dr. Bond kindly inform your readers what are the "fallacies," "blunders," or "wild assertions" as regards this most important part of my work, or where are the body of statistics of higher authority which contradict its teachings?

Passing over my diagrams relating to England and Wales, to Scotland and Ireland, to Sweden, and to Leicester, all of which are founded on equally authoritative statistics, and always for the whole period given in the tables, I come to Diagrams XI. and XII., showing the smallpox and total mortalities of revaccinated Army and Navy, as compared with those for similar ages of imperfectly vaccinated Ireland. The comparison of the smallpox death rates of the picked men of these revaccinated and medically-cared-for services with those of impoverished Ireland, at p. 65, and with almost unvaccinated Leicester, at p. 67, show, in the first case a practical equality; in the second, a great superiority for the unvaccinated manufacturing town – thus again proving the total inutility of vaccination. Again,

I ask Dr. Bond to point out the "fallacies," "blunders," or "wild assertions," in these diagrams and comparisons; or, failing that, to adduce more complete and more reliable statistics which directly contradict the conclusions to which these lead us.

Finally, I would remark that, unless the seriously erroneous character of these statistics can be proved, the mere production of other statistics apparently showing the utility of vaccination in preventing smallpox is of no avail whatever; because, both conclusions – viz., that vaccination is useless and is also useful, cannot be true, and the more extensive and more trustworthy body of evidence must prevail. I therefore confidently challenge Dr. Bond to show, either that the statistics of the Registrar-General for England and for Ireland, and of the Army and Navy authorities, are erroneous, or that I have falsified them in my diagrams, or that there exists more authoritative statistics, of equal or greater extent, leading to an opposite conclusion. Failing which, his accusations against me must be held to be in his own words, "wild assertions" and "inconsequential rhetoric," and should be unreservedly withdrawn. – I am, Sir, yours truly, Alfred R. Wallace, Parkstone.

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Dr. Bond and Mr. A. R. Wallace. Mr. Wallace Replies. (S544: 16 June 1898)

Sir, – Dr. Bond has now written four long letters without approaching the real point at issue between us, which is, whether the large masses of national statistics exhibited in my diagrams are, as he alleged, "fallacious;" and, if not, whether they prove or disprove the alleged value of vaccination. His first two letters dealt only with the origin and spread of zymotic diseases, as to which I stated what I believe to be the opinion of the best authorities; but Dr. Bond holds different views, and he is entitled to his own opinion; though it is doubtful whether this and other small details were worth discussing at such portentous length.

In his last two letters, however ("Echo," June 7th and 11th), he comes to a definite though still very small point, disputing the accuracy of a single case I quoted to illustrate the phenomenon of pro-vaccinist "bluff," of which I have given several examples; and he accuses me of having "doctored" a death-rate, of "statistical sleight of hand," and of "suppressing" essential facts. Let us see how the matter really stands.

(1) I quoted from the appendix to Sir John Simon's Parliamentary Paper on the "History and Practice of Vaccination" (1857), a statement by Dr. T. Graham Balfour as to vaccination and small-pox in the Royal Military Orphan Asylum at Chelsea. Dr. Bond, however, does not consult this original paper, but the reprint of it in the first report, from which reprint Dr. Balfour's statement is omitted, as I stated to the Royal Commission (Third report, Q. 7,211). Hence arises a long series of blunders. Dr. Bond says: – "The quotation of Dr. Balfour by Sir John Simon, on which Mr. Wallace bases his comparison, is to be found on page 72 of the first report of the Royal Commissioners" – and then he quotes what Sir John says about another paper of Dr. Balfour's, not the one in the appendix to his original Parliamentary Paper!

Having thus blundered at the very beginning, he goes on to say that I have "for some unexplained reason inflated 5,744 into 31,706." But Dr. Balfour, in his original article, gives 31,705 as the number of boys during 48 years (evidently obtained by adding together the numbers in the asylum for each of the 48 years), among whom there were 39 cases and four deaths; and he himself gives the death-rate at 126 per million, a rate which Dr.

Bond accuses me of having "doctored." It is true that I added the words "on the average number in the Asylum," because it is only thus you can compare one death-rate with another. That average number is 660 and a little more than a half over, and whether you calculate the death-rate among 31,705 in 48 years, or among 660 each year, the result is the same, 126 [per] million. Of course, the numbers are too small to have any real statistical value. But they were adduced by Dr. Balfour himself to show the value of vaccination, and he took it for granted that 126 million was an exceptionally low rate. But I show that among the very imperfectly vaccinated outside population of the same ages the rate was only 94 per million, so that the case, if it proves anything, is dead against vaccination. The number 5,744, given, apparently, in Dr. Balfour's other paper, may probably be the actual number of the boys who passed through the asylum in the 48 years, a number which, for statistical purposes, is unimportant.

(2) Dr. Bond's accusation as to my "suppression" of the alleged fact that the four boys who died, were all unvaccinated because they were "believed already to have suffered from small-pox" is equally unfounded, for that statement is not made in the original article. Dr. Balfour says, most clearly, that every boy was vaccinated unless he had been vaccinated before, or had had small-pox; and he states positively that they were "all protected." It is only Sir John Simon, in his second-hand statement, who hints a doubt by his "believed to have had small-pox;" and the fact of the whole four deaths occurring after previous small-pox is too improbable to be accepted except on the very best first-hand evidence. But, if it were the fact, it is not one to be suppressed by us, but rather by the pro-vaccinists. For they all admit that protection by vaccination can never exceed, and very rarely equal, that afforded by an attack of small-pox; so that if small-pox does not protect against the disease still less does vaccination.

I have now shown that every word and figure of my reference to the Chelsea Asylum case (at pp. 50–51 of my "Vaccination a Delusion") is strictly accurate; and that my conclusion, which, as Dr. Bond tells his readers, "seems on the face of the evidence to be justifiable," is really so.

To use the very words of my critic against himself, "This is not a bad illustration of Dr. Bond's trustworthiness as a controversialist, as well as of his competency as a statistician." He has misread my very plain reference; he has, therefore, quoted the wrong authority; he has got hold of the wrong figures; he has thus left my facts and my conclusions wholly untouched, and he has occupied a column and a-half of small print in thus convicting himself of incompetence as a critic. I shall be perfectly ready to acknowledge any real and important errors he can point out, but unless he can do something much better in the way of real criticism, I shall not think it necessary to waste any more time in elucidating his obscurities, or correcting his misrepresentations. – I am, Sir, yours truly, Alfred R. Wallace, Parkstone.

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Dr. Bond and Mr. A. R. Wallace. Mr. Wallace's Final Reply. (S548: 15 August 1898)

Sir, – Dr. Bond's three long letters on the Army and Navy statistics of small-pox really call for no reply from me, since he leaves my main statement, that the best re-vaccination possible has produced absolutely no effect in diminishing their small-pox mortality, entirely unaffected. The crucial fact that, in both Army and Navy for the last 34

sears, the total mortality from all diseases has decreased more rapidly than the small-pox mortality, is an unanswerable proof of this, and neither the Royal Commissioners nor Dr. Bond have attempted to answer it. And the further fact that these two bodies of picked and re-vaccinated men have suffered from small-pox about as much as the whole population of Ireland of the same ages, and more than twice as much as almost unvaccinated Leicester, still further shows the absolute inutility of the best re-vaccination the medical staff of the Army and Navy can give. In the face of these two indisputable facts Dr. Bond's various petty criticisms, which never touch the main points, may be neglected.

My general conclusion, founded on the very best and most extensive body of statistics in existence, as given in my essay, remains totally uncontroverted by anything in Dr. Bond's numerous letters. It is, that the long series of records of mortality in London (the largest and most continuous known), the whole modern registration statistics of England, Scotland, and Ireland, the accurate official records of the Army and Navy, and those of the almost unvaccinated town of Leicester, all give the same consistent results. By comparing in each case the small-pox mortality with the total mortality, or with that from zymotic diseases, we see that the diminution occurs in all alike, and that there is absolutely nothing that can be attributed to the special influence of vaccination in the case of small-pox. In the large town of Leicester the steady decrease of vaccination for a quarter of a century, till it is now only about 2 per cent. of the births, has been accompanied, not only by an almost total absence of small-pox, but also by a continuous and striking diminution of infant mortality. As if to enforce this lesson, the opposition to vaccination, which has led, as officially stated, to only two-thirds of the infants born being now vaccinated, has been accompanied not by an increase but by a diminution of small-pox mortality both in London and over the whole kingdom, and also in a gratifying diminution of infant mortality.

Against this overwhelming consensus of the best evidence available, we have nothing but an array of individual experiences not founded on authoritative evidence, and in any case worth nothing, since an equal array can be and has been adduced on the other side. The broad and consistent teachings of the great masses of national experience we now have at our disposal, combined with that of extensive communities exceptionally treated – as in the case of the Army and Navy and the town of Leicester – cannot be affected by any amount of more limited or personal experience. Evidence most be weighed as well as counted. Yet the bulk of the medical profession and the great majority of our legislators, without paying the slightest attention to these facts, content themselves with again and again declaring their "firm belief in the immense benefits of vaccination." Like the historic Bourbons, they learn nothing and forget nothing, but blindly cling to their early beliefs.

Such of your readers as may wish to see a full discussion of the question from a medical point of view, with the arguments of the pro-vaccinators fairly given and fully answered, should read Dr. Scott Tebb's recently published volume, "A Century of Vaccination." – I am, yours truly, Alfred R. Wallace.

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The Vaccination Question (S551)

An overview published in the 1 September 1898 issue of The Times (London).

Sir, – As you have printed various letters on this important subject, perhaps you will kindly allow me to relate my experience.

I may state that, like almost all opponents of vaccination, I was brought up a firm believer in it, but about 25 years ago I was induced to read some pamphlets against it which startled, but did not convince me. For the last 20 years I have given much attention to the whole question as a problem of statistics; I have read almost everything that has been written on both sides; I gave evidence before the Royal Commission in 1890; and I have carefully studied the reports of that Commission. In March last I published a small work dealing with the statistical evidence as given in these reports and in those of our Registrar-General, and I show that this evidence demonstrates the total uselessness of vaccination as a preventive or a mitigator of smallpox. The difficulty is to get people to give the time and attention necessary for a thorough mastery of the evidence. But I have already met with three cases in which strong believers in vaccination have given the necessary time and study to my book, and have in each instance become satisfied that the case against vaccination is so strong that Government is not justified in giving it either direct or indirect support.

The assertion is continually made that statistics can be so treated as to prove anything, and that all statistical arguments are therefore untrustworthy. But this is wholly untrue, and no competent student doubts the value of statistical inquiry. Our whole insurance system is based on statistics, and almost all questions as to the national health as affected by locality, by occupations, and by other conditions, can only be answered by the use of those great bodies of national statistics – the Census reports and those of the Registrar-General. Such accurate statistics as these, dealing with great populations and extending over long periods of time, give the most accurate and trustworthy results. They really surpass all other kinds of evidence, inasmuch they furnish conclusions which approach very nearly to mathematical demonstration.

But there is one thing essential in order to arrive at accurate conclusions from the study of statistics – the method of comparison. For example, suppose it to be asserted by teetotallers that abstinence from alcohol lengthens life; and they adduce as evidence certain communities – towns or villages or societies – in which abstinence has increased for the last 50 years, and show by statistics of death-rates in those communities that the average duration of life has steadily increased during the same period. But all your readers will see that these facts are nothing to the point, until you have compared these communities with others in which temperance or total abstinence has not increased, or very slightly so. If the comparison proves that these other communities also show the same improvement in life-duration, then it is clear that, so far as these statistics go, there is no proof of the beneficial results of abstinence.

Now the case of vaccination and smallpox is exactly of the same nature as that above supposed. The fact that smallpox mortality has decreased during the period of vaccination is of itself no proof whatever of any cause and effect, unless it can be shown that there has been no corresponding decrease in other diseases during the same period, and for this purpose we must always compare smallpox mortality with total mortality and with that

due to the other zymotic diseases.

The main purpose of my little book, entitled "Vaccination a Delusion," is to make these comparisons and others of like nature – comparisons which the Royal Commissioners have not made, and the absence of which renders their conclusions worthless. I show that the great decrease of smallpox mortality during the present century, to which the writer of your article alludes, is not an isolated phenomenon due to a special cause – vaccination, but is a part of the general improvement in health due to a general cause – sanitation. During the 136 years from 1760 to 1896 the decrease (in London) of smallpox mortality, of the mortality from all other zymotics taken together, and of the total mortality, correspond very closely, and are represented on my diagram by three lines, which, with many irregularities, are yet generally parallel. For England and Wales during the period of accurate registration (1846–96) there is the same parallelism. In the Army and Navy, which, being wholly vaccinated, possess the utmost protection that vaccination can give, the total disease mortality has decreased as much as that from smallpox, and more regularly; while these highly-vaccinated forces have a smallpox death-rate about equal to that of the adult population of Ireland of the same ages, and more than double that of the town of Leicester, where vaccination has been continuously neglected for a quarter of a century, till now only 2 per cent. of the births are vaccinated!

None of these comparisons have been made by the Royal Commissioners; and they demonstrate the very opposite conclusion to that which the Commissioners have arrived at – the total inefficacy of vaccination.

I do not make these statements in order to initiate a discussion, which would be impossible within any reasonable limits, but for the purpose of making a proposal to any of your readers who may be disposed to give some time and trouble to the question, which is certainly a most important one. I shall be glad if any such person will call upon me, when I will lend him my book on the statistics of the question, Dr. Scott Tebb's recent volume on the history and medical aspects of vaccination, together with the full reports of the Royal Commission, by which almost all the facts adduced by myself can be tested.

The work of Dr. Scott Tebb, though very full, is quite readable, and is a mine of accurate information on every aspect of the question. It is also of especial value and weight because the author, having been imbued with the traditions of his profession, was, till a few years ago, a believer in vaccination. But when it became a question of submitting his first child to the operation, he determined to investigate the whole subject for himself, with the result that he found the danger of vaccination to be so great and its benefits (if any) so infinitesimal, that he refused to have his child vaccinated, and has been duly summoned and fined by the Christchurch magistrates. It is clear that the evidence of an inquirer of this kind outweighs the belief, however positively stated, of any number of persons who have never given an hour's serious study to the question, but have merely adopted the opinions in which they have been educated. When we add that more than a dozen other medical men (several of them public vaccinators) gave evidence before the Royal Commission of the serious dangers of vaccination, and in several cases expressed their entire disbelief in its protective influence; that Dr. C. Creighton, the greatest authority on the history of epidemic diseases, and Professor Crookshank, equally eminent as an authority on animal diseases and Professor of Comparative Pathology and Bacteriology at King's College, both reject vaccination, as being utterly unscientific in theory, founded on erroneous observations, and having no value whatever as a preventive of smallpox; and when we further consider how painful and difficult it must have been for all these men,

first to overcome the influence of their early teaching and prepossessions and then to set themselves in open opposition to their professional colleagues, with the certainty of being subjected to misrepresentation and abuse by parliamentary speakers and press writers, we may be sure that only the strongest conviction after the most thorough investigation could lead them to reject their early beliefs as altogether erroneous and to make their convictions public.

If, therefore, we take these various facts into consideration it will be seen that the real weight, even of medical authority, may actually be greater on the side of anti-vaccinationists, although the great majority of the medical profession still loudly proclaim their belief in the value of the operation.

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"The Wonderful Century" (S608)

Wallace was still standing up for his position as of 1903, as evidenced by this letter printed in The Academy and Literature issue from 24 October of that year.

Sir, – Will you be so good as to allow me to correct two important mis-statements in a review of the new edition of my "Wonderful Century" which appears in your last issue.

The first is contained in the following passage:

> The vaccination chapter has disappeared to make way for four new chapters on astronomy, but unfortunately the book gains nothing in truth thereby. For Dr. Wallace uses the space thus gained mainly to support his recently promulgated theory as to "Man's Place in the Universe."

Now the four chapters referred to occupy 122 pages, summarizing to the best of my ability the whole range of the New Astronomy. Less than six pages of this new matter are occupied by very brief statements of the facts which go to prove that the solar system is situated near to the centre of the visible universe, and of those which render it probable that no other planet can have developed the higher forms of life. I submit, therefore, that your reviewer's statement quoted above entirely misrepresents the facts, and is calculated to mislead your readers.

The other mis-statement is, that what your reviewer terms "the ludicrous chapter on vaccination," in my first edition, "has been replied to a thousand times and in a thousand ways," the latest alleged reply being contained in Dr. Garrett Anderson's figures, showing the enormous mortality of unvaccinated children. But such figures as these, however often repeated, are in no sense whatever a reply to my arguments, because they rest on unverified statistics which have again and again been proved to be erroneous, and also because they are based on individual and local as opposed to general and national experience. Sir John Simon himself, the greatest official advocate of vaccination, stated in 1857 (in a Parliamentary Paper reprinted in the Reports of the Royal Commission) that the earlier evidence of the value of vaccination was necessarily founded on individual cases, but that now "from individual cases the appeal is to masses of national experience." It is upon these masses of national experience, as embodied (1) in the Reports of the Registrar-Generals for England, Scotland, and Ireland; (2) in the official statistics of the revaccinat-

ed Army and Navy; and (3) in the experiences of large populations – such as London and Leicester – that I rest my case. Moreover, my arguments are founded upon the whole series of the available statistics, not on selected portions of them, and all are taken direct, either from the original Reports, or from official reprints in the Reports of the Royal Commission on Vaccination. I have also shown, wherever possible, the comparative mortalities from "other zymotic diseases" and from "all causes," demonstrating that there has been no exceptional influence acting favourably in the case of small-pox, but rather the reverse: and the absence of such comparisons from the final Report of the Royal Commission entirely vitiates their conclusions, as every statistician will admit. But this great body of reliable statistics is now wholly ignored by the medical supporters of vaccination, who, in place of it, bring forward individual experiences, and utterly unverified and untrustworthy figures, collected exclusively by one of the parties to the controversy. I therefore again repeat my statement, that the best available statistical evidence which I have given, in my pamphlet, and have rendered easily intelligible by a series of comparative diagrams, has never been replied to, and, I am convinced, never can be replied to. I appeal to any of your readers having some acquaintance with general and statistical reasoning and who are interested in this question – one affecting individual liberty and often life – and death, and therefore far more important than most political questions – to give a few hours study to my pamphlet (a second edition of which appeared in 1901), and thus be in a position to judge for themselves whether the facts and conclusions there set forth, or the statements of the medical apologists for vaccination, are the most trustworthy. – Yours, &c., Alfred W. [*sic*] Wallace.

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The New Vaccination Bill (S640)

An objection printed in the 31 May 1907 issue of The Clarion.

Dear Sir, – I object to Mr. Burns' "Vaccination Act, 1907," chiefly because it continues the cruel and unjust discrimination against the poor. The wealthy and middle classes will be to some extent relieved, but to the poor man there will be little or no relief. He must still go before a magistrate or Commissioner of Oaths, either of whom may be miles away from his home. He must go to them in office hours, and so he must lose perhaps half a day's wages. For the "Statutory Declaration" he will be called upon to pay a fee, equalling perhaps another half-day's wages; and if he escapes the actual cross-examination, he will certainly not always escape contemptuous looks, rude remarks and unnecessary delay. That a Liberal Government, with a Radical and humane Prime Minister, and a Working-man President of the Local Government Board, should thus treat the poor workers – who for these nine years past have borne the chief brunt of "the oppressor's wrong, the proud man's contumely," who have been badgered and denied their certificates, have been fined and imprisoned because they would not have their children blood-poisoned – is to me one of the most amazing outcomes of the present great uprising of the nation against *injustice* and *class-legislation*.

One would think that the very first care of such a Government would be (pending the total abolition of all vaccination laws whatsoever) to make regulations that shall involve

the *least* possible inconvenience and *not one penny* of expense to the wage-earners, who are not only the most numerous but the most indispensable portion of the population. Why continue this absurd legal mockery, with its "solemn declaration" and its "conscientiously believe" ungrammatically repeated twice over in four lines of a "Statutory Declaration?" Do they think that English parents of *any* class would take the trouble to apply for an exemption if they really believed vaccination to be beneficial? Why not, then, be satisfied with a simple declaration: "I decline to have my child vaccinated" – a printed form to that effect to be obtained at any post office, where it can be filled up and signed, and posted (*free*) to the Vaccination Officer, or, preferably, to the Registrar of Births?

We, who *know* the evils of vaccination without one particle of redeeming good, wholly deny the right of any Parliament to enforce it. And when we consider that it was first introduced more than a century ago, in the pre-scientific era of medicine, that all the predictions of its upholders have been falsified, that to inoculate healthy but helpless infants with pus from a diseased animal is an outrage on Nature and a crime against humanity; and, lastly, that probably not one in a hundred of those who voted for the successive vaccination laws ever gave a day's serious study to the question, but voted blindly, trusting to the erroneous statements of a profession which has, in all ages, made too many mistakes to be considered infallible, we, who know all this, "CLAIM" – not as a favour, but as OUR RIGHT – complete freedom from this medical tyranny.

I have myself made a careful study of the facts and statistics of vaccination for more than a quarter of a century, and I feel able to form a more trustworthy judgment as to the uselessness and the dangers of the operation than any number of class-biased doctors, or – in this matter, at all events – ignorant legislators. I therefore speak plainly what I *know* to be true, and claim boldly what I *feel* to be just and right. – Yours truly, Alfred R. Wallace, D.C.L., F.R.S., Broadstone, Wimborne.

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Section 10. Miscellaneous Subjects

Introduction

The preceding nine sections have treated particular subjects or periods of Wallace's life, and now, ultimately, we must come to the "miscellaneous" items. These give evidence of yet further interests on Wallace's part, some of them that he actually gave some significant amounts of attention to, if not necessarily for the same reasons.

Apart from several truly "miscellaneous" items below are indications of Wallace's interest in three additional directions: astronomy, museum collections, and literature. His works in astronomy included two books and a few shorter writings, but these had some fair influence despite being based on the limited empirical knowledge of the heavens at that time. In fact, Wallace may be reasonably considered one of the founding fathers of the field of astrobiology, for his insistence on the careful measure and interpretation of planetary surfaces, and his arguments as to the possible uniqueness of humankind in the universe. His position on the latter subject has secured him an association with the anthropic principle, which discusses the various extents to which *Homo sapiens* might be considered the object of cosmological evolution.

Wallace perhaps deserves a footnote in the history of museum design for his several essays on the subject, but his interest in literature led to nothing permanent, despite his various notes on related questions. This last section is dominated by none of these three connections, however, but instead by correspondence on one of the great blunders of his life: an attempt to prove to a flat-earther that the earth is not – flat. In 1870 Wallace took up a wager involving the then-considerable sum of £500., but, though designing an experiment that made his point, through a technicality he never received his reward. Instead, he was harassed for years by the other party, who was jailed for slander on more than one occasion.

The amount of attention Wallace was forced to give this matter is suggested by the volume of letters represented below (and even these are only a sample of the total eventual correspondence). Actually, Wallace's experience as a surveyor made him an ideal candidate for this test, but the outcome was revealing in at least three ways. First, it says something about his financial situation that he felt the need to take this action to begin with. More importantly, it gives evidence of a mind that believed that given the facts, people will see reason. Lastly, and perhaps expectedly, it shows how a general faith in the goodness of humanity cannot always meet ignorance or malevolence on their own terms.

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Atlantic and Great Western Railway (S142ab)

After returning from the East in 1862 Wallace attempted to invest some of his

profits from collecting. Most of these efforts did not turn out very well. This related message was printed in the 30 July 1868 issue of The Daily News (London).

Sir, – Will you permit me, in your columns, to call the attention of the divisional bondholders of this railway to a matter which vitally affects their interests? By an advertisement which appears in to-day's papers we are asked to send our unpaid coupons for two or three years to the board of directors, to be exchanged for "Income Bonds," in pursuance of their scheme of arrangement; and we are at the same time promised that – "all coupons are to be placed in trust as a guarantee for the due payment of principal and interest of the Income Bonds." Truly a most necessary precaution, without which our Income Bonds are so much waste paper. Yet the board of directors seem to expect us to be so weak as to deliver up our coupons to them before any trustees are appointed, or even named. If they wish their plan to be generally accepted let them call us together to appoint trustees in whom we can really trust, and to discuss the details of their scheme. – I am, &c., Alfred R. Wallace, 9 St. Mark's-crescent, N.W.

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A Scientific Club (S144)

A sensible proposal printed in the 17 March 1869 issue of Scientific Opinion.

Sir, – You propose for discussion the advisability of establishing a club for the working men of science, and I beg to offer a few remarks on the subject. There can be no doubt that such a club would be most valuable for scientific men, and would also greatly add to the popularity of scientific meetings by making them more accessible. A member of a society who lives a few miles from the centre of London, and who may be engaged during the day in the City or the West End, has now to choose between a journey home and back again, or going to some hotel or coffee-house to dine and spend the time from 4 or 5 till 8 p.m. Many go home, and do not feel inclined to come out again, and thus the meetings of the societies miss the presence of many valuable members.

Your suggestion of an arrangement with some hotel seems to me excellent. Any large hotel proprietor would probably give up at a moderate rent the exclusive use of two large rooms (for coffee-room and library) and supply the members with all they required at the same scale of prices as the less expensive clubs. It seems probable that several hundred members would soon be obtained, and the annual subscription need not be high to cover rent and leave a handsome surplus for scientific periodicals and books of reference, stationery, bookcases, &c.

You seem to think the name a difficulty, but what so simple, expressive, and unexclusive as "The Scientific Club"? the qualification being membership of some recognized scientific society, not including in that term artistic or professional societies. The locality chosen should be determined by that of Burlington House, the focus of scientific gatherings, and Charing Cross, the most central London railway-station. Anywhere between these points would be convenient, and I feel sure that such a club, forming a centre for

social intercourse and for obtaining scientific information, would be eminently successful. – I remain, &c., Alfred R. Wallace.

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letters to the Editor concerning the Bedford Canal "flat earth" experiment (S162 and S163)

A pair of letters printed, respectively, in the 2 April and 16 April 1870 issues of The Field. In 1870 Wallace accepted a wager offered by John Hampden, a flat-earth advocate, to prove that the earth was not flat. This resulted in the famous Bedford Canal experiment, in which Wallace used his surveying experience to show that the freely sitting water surface was indeed rounded. Wallace won the wager, but not, on a technicality, the five hundred pounds that had been put up. In these two letters, Wallace disputes the interpretation of the results of the experiment given by another flat-earther, William Carpenter, who had served as Mr. Hampden's referee during the event.

Experiments on the Convexity of Water (S162)

Sir, – As the experiments made by me at the Old Bedford River are elaborately criticised by Mr Carpenter's report, may I be permitted to point out the fallacies and misstatements with which it abounds, and which may perhaps confuse and mislead some of your readers who are not very conversant with practical geodesy.

1. Mr Carpenter defines a "straight line" in a manner totally new, as being absolutely identical with a "level line," thus introducing at the outset confusion of terms, and rendering all clear reasoning impossible. It is an abuse of the English language to confine the general term "straight line" to the one special meaning of a "level or horizontal straight line."

2. Mr Carpenter objects to the value of the view in the large telescope, "because it showed but two points, when a comparison had to be instituted between three;" but he omits to state that the telescope itself was placed accurately at the third point, just as was the spirit-level telescope – to the view shown by which he makes no such objection.

3. He objects that the telescope was not levelled, and goes into a long argument, in which the words "straight line" occur four times, and which, whatever meaning is given to that term, is utterly confused and misleading. He says that I intended to prove that the central signal was five feet above a "straight line" joining the two extreme points. This I both intended to prove and did prove, using the word "straight line" in its proper sense; but Mr Carpenter should not impute to me the absurd mistake he makes himself of thinking that there is, or is supposed to be, a *rise* above the level at the centre point.

4. Mr Carpenter's "argument" exhibits a total ignorance of the use of the spirit level, and of the simplest principles of optics and geometry. We have three points taken, at equal distances, above what Mr C. maintains to be a true horizontal straight line – the surface of standing water. The eye is placed at one of the extreme points, and, looking at the other two points, they do not coincide, as they must do if in a straight line with the eye. Again, the cross hair in the telescope of the spirit level marks the direction of "the

straight line at right angles to the plumb line at the point of observation" (as Mr C. very accurately defines the true level); and as the middle signal appeared considerably below this line, that alone proved that the water surface was not truly level. The distant signal being apparently as much below the middle signal as that was below the cross hair, is absolutely inconsistent with the three being in any straight line, still less with their being in the Carpenterian "straight line," but is perfectly consistent with the three being points in a circle of about the assumed radius of the earth. This is a question of elementary geometry about which there can be no dispute. I may add that the fact of the apparent "equality" of these distances (so dwelt upon by Mr C. in his "argument"), and the views from both extremities of the six miles agreeing so closely, both prove the very great accuracy of the level used, and that it may be depended on to show that the surface of water does really sink below the true level line in a continually increasing degree as the distance is greater; but the proof of convexity in no way depends on this accuracy, as it was shown still better by the large telescope without a spirit level.

5. Mr Carpenter's objection No. 3 is answered above. No. 4 is an entire delusion. No. 5 is an assertion destitute of proof. Nos. 6 and 7 are verbal quibbles with which I have nothing to do. Nos. 8, 9, and 10 rest on the fallacy of there being a "rise" shown by the observations, which is a pure figment of Mr C.'s brain. I have never used the word "rise" in connection with these experiments, and all the observations go to show, not a *rise*, but different degrees of *depression* below the true level line. Nos. 11, 12, and 13 are misconceptions. The curvature shown by the large telescope, according to the diagram, is about 5½ ft. at the middle signal, three miles distant – equal to 11 ft. if measured at the distant signal; and the depression below the cross hair or true level line, being, according to Mr Carpenter, an equal amount, makes 22 ft. in all, leaving less than 2 ft. for refraction to bring it to the full theoretical amount, which is something less than 24 ft.

In conclusion, I beg to state that I rely on the three views as shown by the diagrams, which substantially agree, and which demonstrate that the three points equidistant from the surface of water were not in a straight line, but deviated in a vertical direction very nearly as much as is required by the assumed dimensions of the earth; and I challenge Mr Carpenter to place three objects at equal distances apart in a true straight line (three oranges on the parapet of Waterloo-bridge, for instance), and then with a telescope at either end, in the place of one object, make the centre object appear considerably raised above the distant one. Till he can do that, all his wordy argumentation is utterly valueless.

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The Convexity of Water (S163)

Sir, – I should hardly have thought it worth while to answer Mr Carpenter's letter in your last had you not invited me to do so, as the question of my verbal accuracy is one quite beside the main Issue.

In Mr Carpenter's "Objections," 8, 9, and 10 (see *Field*, March 26), he speaks of there being "a rise" shown from the point of observation to the central signal, and argues that, if so, the point of observation must be in a depression or "circular concavity." Here then "a rise" is used in the surveyor's sense of "rise above the level of the point of observation," and I replied that I had not used the word "rise" (of course meaning in the same sense) in connection with these experiments. It is, therefore, quite beside the question for Mr

Carpenter to quote me as saying that the middle signal would be seen "rising" above the others. His own diagrams show that it did so; but at the same time it "fell" below the point of observation (as every surveyor will tell him) by its being seen below the cross hair in the level telescope, allowing of course for the inverted image.

The fallacies in the remainder of Mr Carpenter's letter have been so ably refuted (by anticipation) by your correspondent Mr J. Tanner, that I need say no more about them. I would ask Mr Carpenter, however, to state, for the information of your readers, whether the universally-accepted and only known method of deciding whether three distant points are in a straight line is true or false. That method is to place the eye (whether aided by a telescope or not) at or behind one of the extreme points, and see whether the other two or all three coincide, the nearer hiding or covering the more distant. If so, they are in a straight line. Every carpenter who looks along the edge of a floor board, every surveyor who runs his base lines across the country, every builder who sets out a long wall, uses this method. Does Mr Carpenter say they are all wrong, and that every line thus set out is a crooked or curved line? If so, let him prove this elementary point by experiment and diagrams, and thus found a totally new and hitherto unimagined geometry. If, on the contrary he admits that lines so set out are straight, then the middle and end signal which did not so coincide when seen from the other end signal could not be in a straight line, or there would be two diverging straight lines terminating in the same points, and inclosing a space!

Mr C. has confounded actual with apparent equi-distance in the field of view of a telescope, between which there is no connection, as Mr Tanner's diagrams show. If Mr Carpenter will not try any such simple experiment as I proposed in my last, I must decline to spend any more time in refuting arguments founded on total ignorance alike of facts and of geometrical principles.

The "men of common sense" to whom Mr C. so confidently appeals are very slow in coming forward. The solitary individual he so triumphantly quotes against me (Mr Westlake) now confesses to an oversight, and cruelly deserts him. Mr Hampden, in his letter to me, continually appeals to "public opinion" as being against the fairness of your verdict. It has, however, now clearly spoken through your widely-circulated columns, and, unless he can prove that letters on the other side have been refused insertion, he would do well, as a man of honour and of sense, to bow to its decision.

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Recent Neologisms (S199)

A short note printed in the 20 July 1871 issue of Nature.

In using the word Mr. Ingleby objects to as hideous, I was not aware that I was coining a new one. If so, it was quite unconsciously on my part; but a word was wanted to express the property of being prolific, and if the choice lies between "prolificness" and "prolificacity," as I think it does, I am inclined to believe that the former will survive, as being the shorter, the easier to pronounce, and perhaps the less hideous, even though it may not be constructed on the best etymological principles. "Fertility" and "fecundity," which are often used, do not quite answer the purpose, although the latter has very nearly

the same meaning. Our language must and will grow; and its growth will be determined by convenience rather than by grammatical rules. – Alfred R. Wallace.

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letters to *The English Mechanic* regarding the Bedford Canal experiment

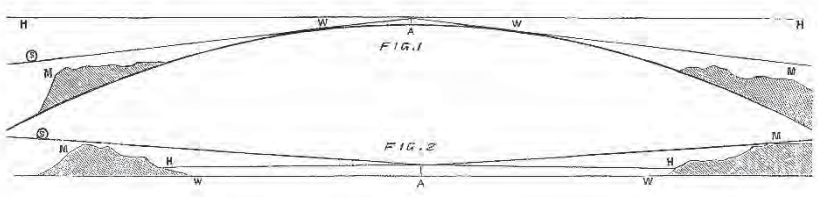
Wallace's attempt to silence the claims of Mr. Hampden produced a bigger row than he could have imagined. News coverage of the event and its aftermath was extensive; one lengthy discussion of the subject took place over a two-month period in the popular technical magazine The English Mechanic and World of Science in 1871. Following are Wallace's four contributions to the discussion.

Proofs of the Rotundity of the Earth (S200aa: 13 October 1871)

The fact that the lecturer who styles himself "Parallax" still makes numerous converts, and that Mr. Hampden and Mr. Carpenter are quite unable to see the true meaning of the observations made on the Bedford canal, show, I think, that other proofs than those usually relied on are required to enable persons of a peculiar frame of mind to see the impossibility of the earth being flat. With your permission, therefore, I will point out two classes of facts which are absolutely incompatible with the flat theory, but which I do not remember to have seen adduced in any works as special proofs of the rotundity of the earth.

1. The position of the visible horizon, or that line at which the sun, moon, and stars set and rise, is an absolute test of the rival theories. If the earth is round, the sea horizon seen from a point moderately elevated above it, will be always much below the true horizontal line, while if it is a plane the same horizon will in most cases be above the horizontal line. To make this clear it will be necessary to refer to diagrams.

In Fig. 1, let A be the observer, elevated say 50 ft. above the sea, and with a clear sea view on both sides, such as may be had on the North Foreland, Beachy Head, Start Point, and many other places on our coasts. If the earth be round, and of the accredited dimensions, his view will be bounded by the water at W W, between eight and nine miles distant, and though there may be high land beyond at M M., this will not be visible unless it rises above the line A W, behind which line the sun (S) will appear to set, and will become invisible. The two lines A W, A W, in directly opposite directions will both be below the horizontal line H A H.



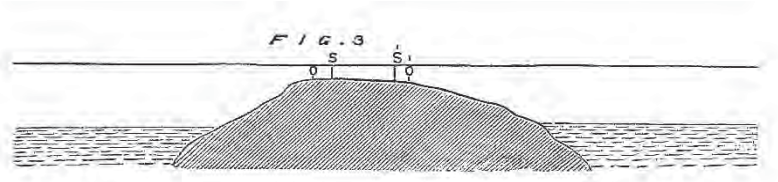


Fig. 2 is identical with Fig. 1, but is drawn on the supposition that the earth is a plane. The observer at A will have some land higher than himself in almost every direction from him. On the N. Foreland, for instance, he will have to the right or S.E., the Alps, at a distance of 450 miles, and three miles high; and his horizon in that direction would be elevated, as shown by the line A M, which must rise above the horizontal line A H about 0° 20" (twenty minutes of arc). This is equal to the angle formed by an object three feet high at a distance of 450ft., and therefore easily to be seen by the unassisted eye. In exactly an opposite direction to the Alps will be found the mountains of North Wales, 250 miles off and more than half a mile high, making an angle above the horizon of about 0° 6", or six minutes of arc, an amount also easily seen by any one, being about that of a three foot target 500 yards off. Now as these mountains are solid and opaque objects, the sun, moon, and stars cannot be seen through them, and must therefore "set" or disappear behind them a long way above the water at W W (Fig. 2). The apparent horizon lines A M, A M, on or at which the heavenly bodies rise and set, will be considerably above the true horizontal line H A H. Here, then, we have a positive test of the two theories which any one, without instrument of any kind, can apply for himself. Almost anywhere on our south or east coasts the moon may be seen to rise or set over the sea. Let the observer notice if it first appears or disappears on the surface of the water or some distance above it. If the former, let him consult a good map of Europe, and see if in the direction of the moon there was any mountain range which ought to have hid it at a considerable distance above the water. As the moon shifts its point of rising every night, a few observations will soon settle this point; and if any number of competent observers see the moon in a given direction rising, in a clear atmosphere, not from the sea, but from a point considerably above it, and at an elevation proportionate to that of a mountain range known to be in that direction, it will be a fact, if constantly to be observed, greatly in favour of a flat earth. Such a fact, however, has never yet been recorded, and it can hardly have escaped notice till now. The other point, the elevation or depression of the apparent horizon lines (A W, A W, in Fig. 1, A M, A M, in Fig. 2) above or below the horizontal line H A H, can also be determined by any one without any instrument but a couple of sticks. I tried it last year on the North Foreland as follows: – Choosing a flat spot with an uninterrupted view right and left, I stuck up two sticks (S S') some 20 or 30 yards apart. Standing behind one of them at 0, I looked over their tops, and by cutting one shorter, I soon got the two to range exactly in a line with the distant horizon. Standing now on the other side at 0', I found the tops to range considerably above the horizon. I therefore cut a piece off the stick at S sufficient to bring the line half way down to the horizon, and then found that by changing my position from 0' to 0 the tops of the two sticks ranged clear above the horizon to an equal amount in both directions, as in the line H A H, Fig.1. If the earth had been flat a horizontal line should have cut below the visible horizon in both directions, because in both directions there was land considerably higher than that on which I stood, the mountains of Wales and Cumberland on one side, and the Alps on the other. A

perfectly straight board or table might be used instead of two sticks, but being shorter this would be less accurate. It must be observed that these tests cannot be evaded by saying that the mountains are too far off to be seen, and that they are confounded with the blue haze of the lower strata of the atmosphere. For even if this were the case they would still differ from the atmosphere in being opaque, and in hiding the heavenly bodies before they reach the visible horizon. But as a fact mountains are, when high enough, distinctly seen at 300 miles distance, as in the case of the Himalayas from some of the lower mountains of India.

2. The other proof of rotundity, is the fact of the sun not being visible all over the earth at once, as it must be if the earth were a plane. Now that locomotion is so rapid and good watches so common, this can be and is tested by every traveller. Any good watch can be trusted to within five or ten minutes in a week. But in eight days a man may go from Liverpool to Halifax (N.S.), and during the voyage he will find that every day the sun rises by his watch half an hour later; and when he arrives at Halifax he will know that when his watch shows 8 o'clock a.m., his family at Liverpool will be having their breakfast with the sun shining brightly in at the window, while with him it will be pitch dark and want two hours to sunrise! So in the afternoon he will find the sun shining high above the horizon when his watch marks 8 p.m., and when he knows it is pitch dark at home and the sun has set two hours before. If he has been taught nothing about longitude and time, he may think his long-trusted watch has gone wrong; but on returning a week afterwards to Liverpool he finds it all right, and not more than a few minutes fast or slow. Now this fact, of the sun being two hours (30°) above the horizon at Liverpool at the same moment that it is as much below if at Halifax – bright sunlight at the one, pitch darkness at the other – is absolute demonstration that the earth is not a plane, for over the whole surface of a plane earth the sun must shine at the same instant. Even more convincing is it to go to Hammerfest in Norway, or any other place within the arctic circle in June; for there the sun is to be seen, for some weeks, all night long above the horizon, while in Scotland, only a thousand miles off, it dips far beneath it. Here no quibble about unknown changes in watches while travelling east or west is available, since no watch is required to show the sun shining at midnight, and day and night continuously for weeks together. The fact that these changes of time and of the sun's altitude are exactly such as to agree with the curvature of a globe 8,000 miles in diameter, proves not only that the earth is not flat, but that it is a globe of the dimensions usually assigned to it.

I think it would be interesting if you, Mr. Editor, would open your columns to receive any replies that "Parallax," Carpenter, Hampden, or any of their supporters may make to the facts here adduced; and I am sure they can have no fairer or more competent judges than the intelligent readers and correspondents of your valuable paper. – Alfred R. Wallace, Holly House, Barking.

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The Shape of the Earth Controversy (S200ac: 3 November 1871)

"Parallax" replies to my arguments by denying my facts. This is certainly going to the root of the matter, but we can hardly take his *ipse dixit* against the universal testimony of all practical men. He denies that the sun rises and sets at an angle of more than 90º from the zenith; but all navigators and surveyors know, not by one experiment but by

thousands and tens of thousands, that it does; and in all works on navigation and geology is to be found a table of the "dip of the horizon," or the number of degrees and minutes to be added to 90º to give the angle between the zenith and the horizon for different altitudes. "Parallax" might as well maintain that the sun has twice the angular diameter of the moon, and his followers would doubtless accept his statement without ever testing it, in one case as well as the other.

In my turn I deny "Parallax's" fact of a small boat being seen six miles off by an eye only twelve inches above the water. When I placed my eye about two feet above the water at Welney-bridge, only the upper part of the arch of the Old Bedford-bridge (six miles off) was visible, the lower half being concealed by the water line. I will join "Parallax" in no experiment till I have better proof of his honesty of purpose. This may be tested by his submitting to an experiment in which any dishonesty will be detected, such as the following: – Let six coach or railway lamps be provided at, say, Welney-bridge. Let a committee of two persons (appointed by neither "Parallax" nor myself) have charge of them, and exhibit them on a clear night at a fixed hour, say for two minutes, with an interval of two minutes, six successive times, *each time changing the number and position of the lamps exhibited*. Let "Parallax" be stationed six miles off with his telescope (which detected the boat, flag, and man) also fixed one foot above the water, and let him note down on paper the number and position of the lamps at the six successive exhibitions. Let him and the committee send to you, Mr. Editor (that night, and without meeting each other), their respective statements to be published. If "Parallax" describes all the six positions and numbers accurately, as he must easily be able to do if his theory is sound and his former observation correct, I hereby undertake to pay all the expenses of the experiment. A friend of mine will see that the experiment is fairly conducted, as far as the position of "Parallax" and his telescope are concerned. I think all impartial readers will acknowledge that this is a fair test; the "boat" experiment, with "the public" for witnesses, could not be so. Boats are continually passing along the canal, and I would defy the most experienced observer to tell whether a boat was four miles or six miles off, or to distinguish anything accurately in the vibrating atmosphere that is almost always present in the daytime; and I can quite understand that the Norfolk "public" would readily see what they were told to see in so unaccustomed an instrument as a powerful telescope. Allow me to make another remark. If the telescope used by "Parallax" showed him an object (a man) about *one foot* across (and oars of a still smaller diameter) at *six miles* distance, the same telescope would certainly show him objects *one hundred* feet diameter at *six hundred miles* distance, the angle subtended being the same in both cases. Why, then, does not any telescope ever show the range of the Alps, less than five hundred miles from the east coast of England, and extending for more than one hundred miles across the field of view, many single mountains being miles in diameter, and the marked contrasts of dark pine forests and shining show fields being most easily visible objects. All this *must* be seen if the earth be a plane, and if "Parallax" saw the oars at six miles distance. How strange that of the hundreds of good telescopes upon our east coasts none have ever shown a glimpse in that direction of anything but sea and sky! Let "Parallax" take *his* telescope to Southend or Margate, and exhibit this beautiful sight to an admiring crowd. When he can do so we shall, many of us, become his converts. – Alfred R. Wallace.

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The Shape of the Earth Controversy (S200ab: 10 November 1871)

In my letter last week (hastily written), it would be more intelligible to substitute in the 12th line, "elevations" or "heights," for "altitudes," the latter word being generally applied to angular measurements.

As regards your offer of a discussion with "Parallax" in your columns, I can only continue it on condition that "Parallax" keeps to the point, and fairly answers my arguments and facts, one by one, before advancing fresh ones on his own side. Unless this is strictly complied with, I must decline a useless controversy. – Alfred R. Wallace.

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Last Words on the Shape of the Earth Controversy (S200b: 17 November 1871)

I quite agree with Mr. Proctor's[a] excellent letter, which would, however, not have come with a good grace from me. In order that you may judge of the consistency and good faith of Mr. Hampden and his supporters, I inclose you two papers. 1. p. 17 of a pamphlet by Mr. Hampden, in which he printed my letters to him. From the marked paragraphs you will see that Mr. Hampden had proposed to me the experiment of lamps at night, and that I had agreed to it! 2. A report of experiments made for and published by Mr. Hampden, in which the most elaborate experiment is this very one of lamps, but without the precautions which could alone render it of any value. – Alfred R. Wallace, Holly House, Barking.

[Though "Parallax" has challenged Mr. Proctor, it is not likely that Mr. Proctor will accept it. Should he decide to do so some other organ must be selected as a medium of the controversy. Mr. Wallace inclosed the documents referred to in his letter. We have also received per post several unsealed letters from Mr. Hampden, in which the characters of public men are traduced. Every one of these letters would subject Mr. Hampden to a criminal prosecution. – Ed. E. M.]

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The "Bedford Level" Experiments (S214a)

A letter to the Editor of The Zetetic printed in its August 1872 issue.

Sir, – I beg to protest against the gross mis-statements which appear in your first number, and to which attention is specially called as being "a carefully prepared statement of facts" about the Scientific Wager.

It is *not true*, as therein stated, that it was "*at the suggestion of Mr. Wallace*" that the Bedford Canal was chosen as the place of experiment. I suggested Bala Lake, as may be seen in my first letter published by Mr. Hampden. *He* then suggested the Bedford Canal, in a letter still in my possession, and *I* accepted that suggestion.

[a] Richard Proctor (1837–1888), English astronomer and writer.

Again it is stated that Mr. Wallace expressed his opinion that "a good signal at each end, and one the same height in the centre, would answer every purpose." This is put in inverted commas as a quotation, but it is a quotation not from me, but from Mr. Carpenter's "Water not Convex," p. 10; yet this "carefully prepared statements of facts" neither gives the source of this quotation, nor adds that the proposal was "*agreed to unanimously*" – which is to be found in the very next line!

Still worse than this, is the assertion that Mr. Walsh decided that "taking into consideration the *theory of the Earth's rotundity*," Mr. Wallace was entitled to the stakes. What is this a quotation from? It represents Mr. Walsh as expressing a foregone conclusion founded on *theory*, not a decision founded on evidence. It is a gross misstatement and a libel, and unless you unreservedly withdraw this "statement" in your next issue as an *erroneous and unfair statement of facts*, and give in its place a strictly accurate one, with no sham quotations, but with references to authorities, and Mr. Walsh's decision in his own words, I must decline to make any further communication to what, will, in that case, be stamped as an unfair and prejudiced periodical. – Alfred R. Wallace.

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Misleading Cyclopædias (S218)

This curious letter, printed in the 28 November 1872 issue of Nature, concerns a minor problem still commonly encountered.

Can any of your readers inform me if there is such a thing as a good and honestly constructed cyclopædia – one that does not send you hunting for information from one volume to another, and refer you backwards and forwards to articles that do not exist?

I have been repeatedly annoyed by this kind of will-o'-the-wisp, but have to-day met with such an outrageous example of it, that, although it involves some trouble, I feel it to be a duty to make a public exposure of it in your columns.

Requiring some facts on unusual atmospheric refraction, I turned to "Refraction" in the "English Encyclopædia." This article referred me to "Mirage, Fata Morgana," &c., for information on this branch of the subject. Turning to "Mirage," I found not a word, but another reference to "Reflection and Refraction, Atmospheric, Extraordinary." Next I tried "Fata Morgana," again the same reference. Coming back to letter R, I found the article "Reflection and Refraction," but was here referred to "Light, Optics, Refraction, Refrangibility;" then to letter A, "Atmosphere, Atmospheric" – nothing on the subject. Letter E, "Extraordinary Refraction" – nothing but a reference back again to "Mirage!" "Light, Optics, and Refrangibility" contain nothing on the subject.

I was thus sent on a search through five volumes of the work, and made to hunt out nine distinct headings for what does not exist; and what makes the matter worse is, that the writer of the article "Refraction," at the end of the work, must have *known* that it did not exist when he referred back to "Mirage, Fata Morgana," &c., which words have not a word of information appended to them.

An alphabetical cyclopædia is so much the most convenient for reference, and might be such an invaluable addition to a library, that it is the more to be regretted that it should

be brought into disrepute by the absence of all efficient editorial supervision.

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East India Museum (S226)

A suggestion printed in the Nature issue of 1 May 1873.

Allow me to make yet another suggestion (in addition to those of P. L. S. and Prof. Newton[a]), with regard to the disposal of the natural history collections at the India House. It seems to me to be one of the greatest popular delusions, that specimens of natural history necessarily require lofty halls and spacious galleries for their preservation and exhibition in a useful manner. I hold, on the contrary, that, with few exceptions, they far better serve educational and scientific purposes when arranged in ordinary apartments. All the scientific work in the British Museum is done in small rooms; and the palatial galleries with their crowded myriads of specimens and miles of glass cases, however instructive they may be (or might be made) to the public, are a positive hindrance to scientific work. I am very much mistaken if all the India House natural history collections might not be suitably placed in two or three ordinary sitting rooms, and so arranged in cabinets and boxes as to be far more convenient for reference and study than they have ever been. The rent of a moderate-sized house in an airy situation, say 250*l*. with an equal sum for the salary of an efficient Curator, and a small grant for cabinets and the necessary books of reference, is all the expense required to make this interesting collection completely accessible to all who wish to consult it. Every one interested in Indian natural history would then visit it. It would again receive gifts of collections from travellers, Indian Officers, and other persons interested in the natural history of the East; and its increase in value from this source alone might go far towards furnishing a tangible equivalent for the expense incurred, while it would certainly render the collection a better representation of the Indian fauna than it is at present, and more worthy of a place, at some future time, in the proposed grand Indian Museum.

Such a modest establishment would also, I believe, do much good by showing at how small an expense a really useful scientific museum may be kept up, and would thus encourage the formation of local museums in cases where 20,000*l*. or 30,000*l*. cannot be raised for a building. It would not, of course, be a show museum for the uneducated public to wander and gaze in; – the British Museum serves that purpose. But it would prove greatly superior to any such mere exhibition, as a means of furnishing definite information on Indian zoology, and enabling any intelligent inquirer to obtain some idea of the many wonderful and beautiful forms of life which characterise, what is at once the smallest and the richest in proportion to its extent, of the great zoological regions of the globe. – Alfred R. Wallace.

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[a] Philip Lutley Sclater (1829–1913) and Alfred Newton (1829–1907).

The Challenge: Its Acceptance, Trial, and Postponement (S228a)

Some three years after the Bedford Canal "flat earth" experiment was carried out in 1870, it was proposed that the experiment be repeated, albeit with a certain difference of approach. Wallace was agreeable, but the arrangements became complicated and the effort bogged down. Related correspondence was published as part of a summary of the discussion in the August/September 1873 issue of The Zetetic and Anti-Theorist (a "cosmography" journal edited by one of the chief flat-earth proponents, known as "Parallax"). Some twenty-three letters were printed; following is one of Wallace's five contributions to the discussion.

Sir, – You have sent me your issue for July, containing a mis-statement with respect to myself. I never "agreed to try the boat experiment," and had never heard such an experiment proposed till after the trial referred to.

As to the challenge to try such an experiment, I have no hesitation in admitting it to be a conclusive one, and will cheerfully abide the result of the trial with any impartial judges to decide whether the boat continues to be seen or not. But I would not accept the dictum of any person unaccustomed to the use of the telescope, because it requires practice to distinguish between small objects at such a distance and so near the surface.

I propose therefore that you should invite any professional land surveyor or civil engineer from the neighbourhood to decide the simple question of fact whether the boat in question remains visible at a distance of six miles under the conditions named by you, the telescope being one quite capable of rendering it visible when taken to a sufficient elevation.

I will ask Mr. Coulcher, of Downham, to represent me, so far as to see that the experiment is fairly tried; the statement of the surveyor on the matter of fact being agreed to be accepted by us both.

I also agree to pay half the surveyor's fee. – Alfred R. Wallace, Grays, Essex.

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The Hampden-Wallace Libel Case (S248b)

Unfortunately for Wallace, John Hampden continued to plague him and his family for years. Wallace made some final comments on the matter in a letter printed in the 19 March 1875 issue of the Chelmsford Chronicle.

Sir. – I should not have thought it necessary to say another word on the matter between myself and Mr. Hampden, had you not last week addressed a kind of expostulation to myself. Your remarks show that you are ignorant of certain facts which in the opinion of impartial lookers on, as well as of all my friends, render the course you suggest altogether out of the question. I beg, therefore, to be allowed to state, as briefly as I can, what these facts are.

1. It is not the case, as you seem to suppose, that the experiment I tried was sure to succeed. It was to be made on a canal in a fen district, and was strictly limited by Mr. H.

to the space of a few weeks in the months of February and March, 1870. It is well known that unusual refraction sometimes raises distant objects so as to neutralize or even reverse the evidence of curvature. Had this happened on the only fine day we found for the experiment I should most certainly have lost my £500. (See Flammarion's work on "The atmosphere" for such cases of unusual refraction).

2. Mr. Hampden, however, had no such doubts on his side, for the excellent reason that he believed the experiment had been repeatedly tried on the very spot in question, by Parallax himself, the teacher of Mr. H. on this matter. In "Zetetic Astronomy, the earth not a globe," by Parallax, pages 11 to 13, are full details of those experiments, with diagrams, which he states have been repeated many times during 24 years! This was the very book which converted Mr. Hampden. If either of us, therefore, is liable to the charge of having wagered on what he believed to be a certainty, it is Mr. Hampden, not me.

3. Mr. Hampden chose for his referee a man who had for years been a disciple of "Parallax" – had written a book adverting the flatness of the earth – and this book (stock and copyright) had been purchased from him by Mr. H. about two months before the experiment. Yet, in face of my written stipulation that Mr. Hampden's referee should not be a "personal acquaintance" of his, this man was appointed, and accepted by me in full confidence that I should not be deceived. I and my friends, as well as the Editor of the *Field* (Mr. J. H. Walsh) considered the appointment of this man by Mr. Hampden as grossly improper and unfair at the very outset. (*Field*, March 26th, 1870.)

4. From the moment the experiment was made Mr. Hampden has claimed to have won the wager, on the ground that the water is really a plane, and that my experiment showed it to be so! And he still persists in this view, claiming not only the money he lost but my money which he says he has won. I have numerous letters extending over nearly five years, in which he declares that he will never leave me in peace until I have paid him the whole £1000.

I leave your readers therefore to judge what my position would have been had I given Mr. Hampden back his money (and thus, in his view, have acknowledged my failure to prove what I had undertaken to prove) unless I were also prepared to pay him another £500. They will also judge whether Mr. Hampden's conduct from the first has deserved any such consideration on my part. And finally I leave them to form their own judgment as to whether, after suffering five years of unceasing libels, and after having spent very large sums in the civil and criminal litigation he has forced upon me, and which is not yet terminated, I owe him any such consideration now.

The facts here stated can be almost all proved by reference to papers issued by Mr. Hampden himself and his referee Mr. Carpenter. – Alfred R. Wallace.

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letters on British and American Museums (S402 and S405)

Wallace published several essays on museum collections, and at times his colleagues were not appreciative of what he had to say. Wallace responded to some criticisms in the following letters, printed in the 6 October 1887 issue of Nature and the 1 November 1887 issue of Fortnightly Review, respectively.

The British Museum and American Museums (S402)

I very much regret to learn that my friend Prof. [William Henry] Flower thinks I have done great injustice to the British Museum of Natural History in my article on "American Museums," which has appeared in the September number of the *Fortnightly Review*. The article was sent to England last February, and I had no opportunity of correcting the proofs, as some very bad misprints will sufficiently indicate. Nothing was farther from my mind than to make any reflections on the management or arrangement of the Museum by Prof. Flower and the able heads of departments, for all of whom I have the greatest respect; and I am further convinced that much credit is due to them for doing the very utmost that is possible under the circumstances of the case. My strictures on the Museum were intended to apply solely and exclusively to the fundamental *principle* underlying its arrangement, which principle is embodied in the new building as in the old one. I contrasted strongly the principle of moderate-sized rooms as compared with large galleries, – the principle of exhibiting, to the public, on the one hand, strictly limited typical collections; on the other, almost complete series of species, – the principle of making a geographical arrangement the main feature of a museum, as compared with that in which almost no provision at all is made for such an arrangement.

I had always understood that for this fundamental system of arrangement neither the present Director nor the heads of departments of the Museum were in any way responsible, and that in criticising it frankly I should not be considered to reflect on them. So clear was I in my own mind that I was discussing this general system only, that I used some expressions which I now see, with much regret, were capable of being misunderstood. After referring to some of the improvements in the New British Museum, I say, "but the great bulk of the collection still consists of old specimens exhibited in the old way in an interminable series of overcrowded wall-cases, while all attempt at any effective presentation of the various aspects and problems of natural history as now understood is as far off as ever." To the latter part of this sentence, Prof. Flower objects, as not recognizing the many improvements recently made and still making; but I intended it to apply, as I think the whole context of my article shows, to the *system* and the *building*, which themselves, from the point of view I have taken throughout the article, render any attempt at an "effective" presentation of these aspects and problems impossible. Again, at the end of my article I speak of Prof. Agassiz having said that he intended his museum "to illustrate the history of creation as far as the present state of scientific knowledge reveals that history," and then go on: "It is surely an anomaly that the naturalist who was most opposed to the theory of evolution should be the first to arrange his museum in such a way as best to illustrate that theory, while in the land of Darwin no step has been taken to escape from the monotonous routine of one great systematic series of crowded specimens arranged in lofty halls and palatial galleries, which may excite wonder, but which are calculated to teach no definite lesson." Here I was referring to the fact that the new Museum at South Kensington was constructed and arranged substantially on the same lines as the old one at Bloomsbury, and regretting that the only effective step towards inaugurating a new system of arrangement was not then taken. Prof. Flower, I find, thinks that I imply that no steps are being taken now to render the Museum more instructive and generally interesting. This was very far from my meaning, and I am exceedingly sorry that such an interpretation of my words should have been possible. I visited the Museum several times last summer before leaving for America, and I noted many improvements that were being

introduced in all departments; but I could not fail to see that the main principle of the arrangement, both of the building itself and of the collections in it, had not been changed, and it was to this that all my criticisms were directed. – Alfred Russel Wallace, Godalming.

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Note on American Museums and the British Museum (S405)

Sir, – I regret to learn that my article on "American Museums," which appeared in the September issue of the Fortnightly Review, has been supposed to reflect unfairly on the Director and heads of departments of the British Museum of Natural History. I therefore beg leave to add a few words of explanation. Every reference that I made to the British Museum was intended to apply exclusively to the fundamental principles which have always governed its arrangement, and which is embodied in the new building at South Kensington as distinctly as in the old one at Bloomsbury. I contrasted, as strongly as I could, the principle of numerous moderate-sized rooms as compared with that of a few extensive galleries – the principle of exhibiting to the public carefully chosen and strictly limited typical collections only as opposed to that of making public exhibition of enormous series of closely-allied species – the principle of making a geographical arrangement a main feature of the Museum as compared with that in which almost no provision at all is made for such an arrangement. The former set of principles, which are carried out at the Museum of comparative Zoology at Harvard University, I hold to be greatly superior in every way, and I regretted that on the erection of a new building at South Kensington provision was not made for carrying out some such arrangement. Believing it to be generally known that the present Director and heads of departments are not responsible for the system, but have to make the best of it under considerable difficulties, I did not think it necessary to say explicitly that my remarks had no application to them, I therefore now wish to state my belief that they have already made considerable improvement in the methods of arrangement and illustration, and will, have no doubt, succeed in rendering the Museum as interesting to the public and useful to naturalists as it can be made under the existing system. – Alfred R. Wallace.

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letters concerning Bellamy's *Looking Backward* (S421a and S421b)

Wallace was a big fan of Edward Bellamy's highly successful futuristic novel Looking Backward, as is evident from the following pair of letters, printed in the 26 April and 10 May 1890 issues, respectively, of Light (London).

Mr. Bellamy's "Looking Backward" (S421a)

Sir, – I have not read *Miss Luddington's Sister*, but I have read *Looking Backward* three times, and I must protest against the reviewer of the former work giving your readers an erroneous impression of the latter, which he has evidently never read through. If he

had done so he could not have written this sentence: "As a story-teller in both *Looking Backward* and *Miss Luddington's Sister* he can hardly be said to have succeeded, for in both books the last chapters entirely upset the apparent meaning and interest of the story." This is wholly untrue of *Looking Backward*, the interest and perfect consistency of the story being kept up from the opening words of the preface to the closing paragraph. Neither is the charge of "vagueness" a more accurate one; for the pre-eminent merit of the book, and that which has given it its great reputation, is the entire absence of vagueness. It is because, for the first time, it has shown how a thorough system of Socialism may be established, and how it may be applied in all the countless ramifications of modern civilisation, while reserving home privacy and individual liberty to far greater extent than is possible under our existing social arrangements, that the book has had such an enormous success, and has initiated a movement in the direction of the new social economy which will in all probability have important effects on the future of humanity. – Alfred R. Wallace.

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Mr. Bellamy's "Looking Backward" (S421b)

Sir, – As your correspondent "π." still thinks he has read *Looking Backward*, I must ask your permission to make a few short quotations from the book:

> I panted, I sobbed, I groaned, and immediately afterwards found myself sitting upright in bed in my room in Dr. Leete's house, and the morning sun shining through the open window into my eyes. . . . As with an escaped convict who dreams that he has been recaptured and brought back to his dark and reeking dungeon, and opens his eyes to see heaven's vault spread above him, so it was with me, as I realised that my return to the nineteenth century had been the dream and my presence in the twentieth the reality.
>
> The cruel sights which I had witnessed in my vision, and could so well confirm from the experience of my former life . . . were, God be thanked, for ever passed by. (p. 248.).

The book concludes with the interview of the supposed writer with his affianced bride, Edith Leete, the great-granddaughter of the Edith Bartlett, to whom he had been engaged more than a century before.

This dream of the nineteenth century, which deceived your correspondent as it has done others, and, I presume, so disgusted them that they did not care to read to the end, is really the most artistic and lifelike portion of the book, since it brings before the reader in a most forcible manner the overwhelming differences between the two states of society pourtrayed. – Alfred R. Wallace.

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Laurence Oliphant and T. L. Harris (S439a: 1891)

A letter printed in the 8 August 1891 issue of Light (London).

It may help to a better comprehension of the relations of these two remarkable men if we take account of the early history and teachings of the former, of which Laurence

Oliphant's biographer, and, presumably, most of her readers, appear to be entirely ignorant. In the introduction to Harris's "Lyric of the Golden Age," Mr. S. B. Brittan gives a sketch of the writer, showing that he was gifted with a power of mediumship equal, and in some respects superior, to that of Andrew Jackson Davis or the most remarkable trance mediums of our day. The late William Howitt, who was one of Mr. Harris's hearers in England, says: "His extempore sermons were the only perfect realisation of my conceptions of eloquence; at once full, unforced, outgushing, unstinted, and absorbing. They were triumphant embodiments of sublime poetry, and a stern, unsparing, yet loving and burning theology. Never since the days of Fox were the disguises of modern society so unflinchingly rent away, and the awful distance betwixt real Christianity and its present counterfeit made so startlingly apparent." The life and writings of Laurence Oliphant show that he was always seeking for this "real Christianity," and we can thus understand the power of Harris over him.

The work that Oliphant, his mother, and his wife were set to perform under Harris's teaching in America, and which their biographer finds so useless and even degrading – though they themselves do not seem to have found it so – may be looked upon as a valuable training for the higher life of the future – the true golden age – which must consist in every one according to their ability, taking their share of the manual labour necessary for our existence on earth, and thereby rendering possible *for all* the needful leisure for intellectual enjoyment and spiritual development; and even in the biography it is clearly indicated that they themselves felt it to be so, and did not regret it. Surely the one great lesson that modern society requires to learn is, that to live lives of pleasure and luxury, rendered possible only by the continuous toil and mental degradation of others, is the thing that is really degrading, and, from a Christian no less than from a social or a spiritual point of view, absolutely sinful.

I know nothing of Harris but what I learn from his poems and from the statements in Mrs. Oliphant's book; but it seems to me that it will be only charitable to apply to him the same lenient judgment that we apply to Madame Blavatsky, and for the same reason – that those who are still in most intimate association with him uphold his teaching and his conduct as being on the whole worthy of respect and admiration…. – Alfred R. Wallace.

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The Fourth Dimension (S502a)

In this, one of the most remarkable of all of Wallace's published writings, he argues that space has no set number of dimensions.[a] *It appeared in the 29 September 1894 issue of the British spiritualist journal Light, in response to a discussion that was going on.*

Sir, – The discussion on this subject seems to me to be wholly founded upon fallacy and verbal quibbles. I hold, not only that the alleged fourth dimension of space cannot be

[a] The transcript of an apparently never-published manuscript on this subject which extended the argument may be found online at: http://people.wku.edu/charles.smith/wallace/Supposed_Dimensions.htm .

proved to exist, but that it *cannot* exist. The whole fallacy is based upon the assumption that we *do* know space of one, two, and three dimensions. This I deny. The alleged space of one dimension – lines – is not space at all, but merely directions in space. So the alleged space of two dimensions – surfaces – is not space, but only the limits between two portions of space, or the surfaces of bodies in space. There is thus only one Space – that which contains everything, both actual, possible, and conceivable. This Space has no definite number of dimensions, since it is necessarily infinite, and infinite in an infinite number of directions. Because mathematicians make use of what they term "three dimensions" in order to measure certain portions of space, or to define certain positions, lines, or surfaces in it, that does not in any way affect the nature of Space itself, still less can it limit space, which it must do if any other kind of space is possible which is yet not contained in infinite Space. The whole conception of space of different *dimensions* is thus a pure verbal fantasy, founded on the terms and symbols of mathematicians, who have no more power to limit or modify the conception of Space itself than has the most ignorant schoolboy. The absolute unity and all-embracing character of Space may be indicated by that fine definition of it as being "a sphere whose centre is everywhere and circumference nowhere." To anyone who thus thinks of it – and it can be rationally thought of in no other way – all the mathematicians' quibbles, of space in which parallel lines *will* meet, in which two straight lines *can* enclose a definite portion of spaces, and in which knots *can* be tied upon an endless cord, will be but as empty words without rational cohesion or intelligible meaning.

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comments on socialism and social justice (S535ab)

One of a number of solicited responses published in the June 1897 issue of The Review of Reviews (London) in an article (probably by the magazine's editor, W. T. Stead) titled "'The Notables of Britain': A Portrait Galley of Contemporaries."

. . . "*Fiat justicia [sic* justitia*], ruat cœlum*." The above saying is that which has most influenced my thoughts on social questions. More than thirty years ago Herbert Spencer's application of it – "Equity does not permit property in land" – took firm hold on me, and thus led to the formation of the Land Nationalisation Society. The great principle of justice to all and before all has led me on towards Socialism; and here, too, I claim Herbert Spencer as a teacher, for his fundamental principle of social justice is, that each person "shall receive the benefits and evils due to his own nature and conduct." But this is directly opposed to any unequal inheritance of wealth, and is thus a considerable step on the road to Socialism. – Alfred R. Wallace.

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The Problem of the Tropics (S554a)

In a letter published in The Daily Chronicle issue of 2 November 1898, Wallace returned to a favorite subject: living in the tropics.

Sir, – In the interesting article on Mr. Kidd's book, "The Control of the Tropics," in "The Daily Chronicle" of last Monday, there is a pervading assumption – I presume made also by Mr. Kidd – that white men cannot live and work there. Your reviewer makes this statement three times, as if it were an absolute fact, undisputed and indisputable, and it is probably this assumption which has made it so difficult for Mr. Kidd to give any satisfactory solution of the "Problem of the Tropics." As one who has lived (and worked) for twelve years in the tropics, perhaps you will allow me space to discuss this interesting question.

No great problem can be solved if we begin by assuming data which are erroneous, and I maintain that the assumption as to white men not being able to live and work in the tropics, in good health and in full enjoyment of existence, is not only untrue, but is the very opposite to the truth. It is because white men, as a rule, do *not* work enough in the open air in the tropics that they so often suffer in health, and for anyone who lives rationally as to food and clothing, and who conforms in his dwelling and surroundings to ordinary sanitary laws, a fair amount of bodily exertion is, there as much as here, one of the conditions of perfect health, and to those who thus live I affirm that the tropics, as a whole, are more conducive to good health than the temperate regions. A large body of facts go to prove this contention, and I will briefly enumerate them.

First, I may say that I owe to my twelve years' residence in the tropics the comparatively good health I now enjoy. When about seventeen I nearly died of lung-disease, but breathing the pure, warm air of the equatorial zone for twelve years completely restored them, so that, ten years after my return home, a physician informed me that my lungs were perfectly sound, and that, in fact, I had the chest of an athlete. Is it not also a well-known fact that, in India, the men who suffer least from the climate are the enthusiastic sportsmen, who seize every opportunity of getting away from civilisation, and who often submit to privations and fatigue with benefit rather than injury to their health. But, turning to a better illustration, do not the rank and file of our European soldiers work, and work pretty hard, too, in every part of India, especially on a campaign, and has it been ever alleged that they "cannot live and work" there, or that they suffer in health from the mere fact of working? On the other hand, the class that does no outdoor work at all in India, and which has fewest outdoor occupations and amusements – the women of the ruling classes – are those who suffer most from the climate. But more striking still is the object lesson we have just had in the Soudan campaign, where English soldiers and officers have been continuously working and fighting for two or three years in one of the hottest and most trying parts of the tropics, and with certainly not more illness than in similar campaigns in temperate climates.

Again, turn to our sailors. In our numerous warships stationed in the tropics, is it found that our sailors cannot work? and, as a matter of fact, have they not always done their regular work just as well in tropical as in temperate climates? And it has never been proved that there is any deterioration in their health due to this work alone, and not to other conditions. Perhaps an even more striking case is that of our Australian miners, who have now for many years been working in the tropics in Queensland and North-West

Australia. How is it that these men, by thousands, actually do work in the tropics, and we do not find it stated that they do much less work than in the more temperate parts of the same country, or that they suffer permanently in their health from so working?

Then, again, as to there being anything injurious to white men who are permanently settled in the tropics, all the evidence is favorable. In the Moluccas there are many Dutch families who have been there for two or three hundred years, and who are not only perfectly healthy and prolific, but who retain the fair complexions of their European ancestors. In many of our West Indian islands there are, I believe, Creole families of pure English blood, and there are considerable populations of pure Spanish blood in various parts of South America.

It is only when we come to agricultural labor that we find white men refuse to work, and the demand is made for a supply of native colored laborers, and the reason for this is not difficult to see. Agricultural labor among us has always been considered the lowest class of labor, as it is the worst paid, though, as Mr. Ryder Haggard has recently told us, it is really skilled labor of a very pronounced kind. It is also work in which there is no great excitement, and no chance of getting wealth, except when practised on a large scale with a full supply of very cheap labor. But there is, really, no occupation so full of interest, so enjoyable, so health-giving as agriculture to him who practises it for himself; and in the tropics nature is so productive and lavish that five or six hours' work a day would give a larger return than double the amount in our own country.

The more favorable portions of the tropics, extending about 15deg. on each side of the equator, afford, I believe, the most healthy and the most enjoyable abodes for man, where with the least labor he can obtain the greatest amount of the necessaries, the comforts, and the luxuries of life, and can at the same time develop and cultivate his higher nature. But to do this he must go there not with the object of making a fortune and coming home to live in luxurious idleness, but as a true settler, determined to make his home there. And he must not go with the intention of hiring native labor – a more or less modified form of slavery – but determined to work with his hands as well as with his head. This can be best done – can only be successfully done – by some form of co-operative colonies, of which the Ruskin Colony in Tennessee is perhaps the best type. There, associated labor loses all its terrors, while all the members being approximately equal in education and refinement, there is ample scope for healthy and varied social enjoyments. Such a colony established in some healthy part of the tropics, guided by adequate experience, and with a moderate capital to start with, would soon attain to a condition of social and economic prosperity that could hardly be reached elsewhere. The economies of such a colony as will be shown by the fact that at Ruskin the whole cost of three good meals a day is less than a dollar a month a head. And in a tropical colony of sufficient size, when once fully established, every necessary of civilised life could be produced, such as sugar, coffee, cocoa, &c., while the cost of houses and clothing would be a minimum.

Here then is a clear and definite solution of the "problem of the tropics." They must be gradually occupied by white men in co-operative association to establish permanent homes, which, surrounding by the glories of tropical vegetation, may in time become something like the legendary paradise. – Yours, &c., Alfred R. Wallace.

* * * * *

The Storage of Gunpowder (S562a)

This interesting suggestion was printed in The Daily Chronicle (London) issue of 24 March 1899.

Sir, – The terrible explosion of gunpowder at Toulon having again directed attention to this subject, I hope you will allow me briefly to describe a mode of storage which would ensure absolute safety. In 1882 I forwarded my detailed plan to Sir Thomas Brassey, then Civil Lord of the Admiralty, who sent me a memorandum of the Director of Naval Ordnance upon it, in which it was said: – "For permanent depots of powder the idea seems worthy of attention, and Mr. Wallace might address the War Office on the subject." Being engaged in literary work at the time, and knowing something of the enormous difficulty of satisfying the authorities as to the advantage of anything so completely new, I took no further steps in the matter.

The plan proposed was, to store powder and all other explosives in hexagonal or cylindrical metallic drums, fitted with air-tight but easily removable lids or caps. The drums to be all of uniform height, and to be stored in shallow tanks and kept covered with a few inches of water by an automatic regulator. By this simple method all the elaborate and costly precautions against accidental explosion would be unnecessary, while even wilful explosion would be almost impossible. I shall be happy to lend my paper to any Member of Parliament or other person who will urge its adoption by the authorities. – Yours truly, Alfred R. Wallace, Parkstone, Dorset.

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Man's Place in the Universe (S604)

In his final decade Wallace turned out several influential writings on astronomical subjects; these would later establish him as one of the founders of the field of astrobiology. In this letter, printed in the May 1903 issue of Knowledge, he responded to criticisms of his 1903 article "Man's Place in the Universe."

Sirs, – As I do not wish your readers to suppose that I have altered the words of an author whom I quote in order to make them agree more closely with my own opinions, will you permit me to state, in reply to Mr. [E. Walter] Maunder's criticism of my article in the *Fortnightly Review*, that when that article was sent to press I had not seen Prof. Newcomb's book on "The Stars"; but I quoted from his "Chapters on the Stars," which appeared in *The Popular Science Monthly*. The quotation beginning "If we should blot out," is verbatim, as at page 323 of that periodical for January, 1901, except that I have, inadvertently, substituted "Milky Way" for "galaxy."

A writer in the *Daily News* of March 31st has so well answered Mr. Maunder's criticisms that it is unnecessary for me to refer to them here. I am, however, indebted both to Mr. Maunder and to Prof. H. H. Turner, who has criticised my article in this month's *Fortnightly*, because, although I consider their objections to be rather weak, and with one exception not much to the point, they are yet of great use to me, as showing me where my

argument needs strengthening or where I am likely to be misunderstood.

In a volume I am now preparing I hope to be able to present my views in a more complete and more convincing manner. – Alfred R. Wallace, Broadstone, Dorset.

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Does Man Exist in Other Worlds? "A Reply to My Critics." (S609)

A few months later Wallace replied to some critics of his newly published book Man's Place in the Universe. His words appeared in The Daily Mail (London) issue of 12 November 1903.

The letters of three eminent men – Sir Oliver Lodge, Mr. H. G. Wells, and Professor Ramsay – in your issue of November 4 contain no substantial or detailed criticism of my book, and do not therefore require any reply from me.

But as they express opinions adverse to my conclusions – these conclusions having been reached by a careful survey of the available evidence – and as you seem to think that these opinions show that "the great weight of scientific authority" is against me, I will, with your permission, state briefly why I hold that these expressions of opinion are wholly valueless from the critical or scientific standpoint.

My book is in some degree a protest against deciding a great question of both scientific and popular interest by an appeal to mere weight of opinion, or vague general argument, unsupported by any careful examination of the whole of the facts. I have endeavoured myself to avoid any expression of my own opinions or beliefs as to the subject-matter of my work, considering all such a priori opinions to be wholly worthless; but I have limited myself to stating the conclusions which seem to me to be either the logical inferences from ascertained facts or probabilities from the preponderating weight of the available evidence.

I should welcome any real and thoughtful criticism, even if it should demonstrate important errors in my facts or fallacious reasoning in my conclusions from them, since my only object is to determine whether my conclusions or those of my opponents most nearly approach to the actual truth.

The Complexity of Facts.

Sir Oliver Lodge, while apparently agreeing with my conclusion as to the inhabitability of any other planet of the solar system – a conclusion which, so far as I am aware, no other writer has reached – entirely fails to go further with me. He says: "But to suppose that of all the myriads of solid bodies in space this particular lump of matter is the only one inhabited by intelligent beings seems to me absurd." And he considers the astronomical arguments with which I have supported my view to be "of a futile description."

But the absurdity or reasonableness of such a belief cannot be settled a priori, or even by an appeal to such facts as immensity of numbers or superiority of size. It will depend upon a careful consideration of all the facts which have influenced or rendered possible the whole course of life-development on the only planet on which we know it has developed.

I believe I have shown, for the first time, how very numerous and very complex are

these facts, and therefore how enormously improbable it is that an almost identical combination should have occurred elsewhere. I feel sure that in such a first attempt I cannot have exhausted the list of these essential conditions, and each additional fact of this nature enormously increases the improbability I have pointed out.

It must be remembered that this improbability applies also to any hypothetical planet of any other sun than ours, and, even if the astronomical arguments I have adduced against the probability of there being any considerable number of suitable suns may be weak standing alone, they become exceedingly strong when compounded with the improbability of all the requisite planetary conditions simultaneously occurring, and persisting during the enormous periods of time essential for the development of intelligent beings. Such an argument as this is not, I submit, to be disposed of by a mere allegation of absurdity.

The Most Weighty Argument.

My critic then restates Professor Turner's argument, or rather allegation, that our position in regard to the Milky Way makes no difference at all as to the habitability of our planet, and by prefixing the expression "I would urge" he evidently considers it a valid argument. But in the first place the statement is a mere supposition unsupported by any evidence, and although it might perhaps have been permissible in the discussion of my original article, it becomes altogether worthless as against my book, in the last chapter of which I have given reasons, founded upon a paper by Lord Kelvin, showing that a nearly central position is probably the only one where sufficiently stable conditions could be maintained during the enormous periods needed for the entire course of organic evolution. This argument, perhaps the most important and weighty in the whole book, appears to have been entirely overlooked.

The last paragraph of Sir Oliver Lodge's letter deals with a portion of the argument to which I expressly attach little importance.

Mr. H. G. Wells, whose claims for a careful, unbiassed, and enlightening criticism of new books I have just been reading with the respect and admiration such an excellent piece of work deserves, affords, by his letter, a painful illustration of the not uncommon divergence between a man's theory and his practice.

He begins by disclaiming knowledge of any facts on which either belief or denial can be based. If he had stopped there, there would be nothing more to say; but he goes on to ridicule the whole inquiry by introducing "buttons" and "saucepan-lids" in relation to the grandest phenomenon the human mind can contemplate – the form and structure of the starry universe – and, further, goes out of his way to compare a work which, whatever its shortcomings, is founded upon a careful study of the results of modern science with the most trivial speculations of the Middle Ages.

Probabilities and Speculations.

Coming now to the third eminent writer, Sir William Ramsay, I read his first lines with some surprise, since he implies that facts are "wholly lacking" in regard to the subject-matter of my book. Yet he at once goes on to refer to some of the very facts I have made use of, and founds upon them a speculation as to the possibility of animals existing on some of the "legions" of planets "which doubtless exist."

I have never denied such possibilities, but I absolutely deny their value as a foundation for a rational belief. I claim that probabilities, derived from and based upon a careful survey of all the available facts, have a higher claim as trustworthy guides for our conclu-

sions and beliefs than any amount of speculation as to what may possibly exist under unknown or imaginary conditions. The last sentence in Sir W. Ramsay's letter I hardly see the point of, or what he means by the term "such a book." When carefully considered my present work will be found to have exactly the same merits or defects as my other scientific books, which have gained me a reputation as an expounder of the logical results of other men's work perhaps higher than they deserve.

It is, I confess, a disappointment to me that the first two men of science who have noticed my book should have thought proper to express a bald adverse opinion which will doubtless be accepted as conclusive by thousands of readers, and that they should not have deemed it necessary to point out some few of the numerous errors as to facts or fallacies of reasoning which are usually considered needful to justify such a course. In a work having so wide a range of subject-matter there must inevitably be some such oversights, but, as the entire argument is a cumulative one, I venture to think that it will not be seriously impaired.

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Britain's Greatest Benefactor (S631)

Wallace's short response to an opinion survey as to whom the most important figure in British history might have been appeared in The Clarion (London) issue of 28 December 1906.

I doubt if any individual can be said to have conferred the greatest benefits (as ordinarily understood) on our people. King Alfred, Caxton, Cromwell, Wilberforce, Dickens, Robert Owen, William Morris, may be mentioned among great benefactors, and even William the Conqueror – perhaps greater, as having welded us into a civilised nation. But as a great, refining, elevating, and moral influence, the man who has given us most household words, the man whose name is a glory to us among the nations, the man whose works and whose memory we could least afford to lose, is

WILLIAM SHAKESPEARE.

– Alfred R. Wallace.

* * * * *

The "Double Drift" Theory of Star Motions (S642)

Wallace's last astronomy-related letter to the Editor was printed in the 25 July 1907 issue of Nature.

I have been greatly interested in Mr. Eddington's account in *Nature* of July 11 (p. 248) of Prof. J. C. Kapteyn's investigations of this subject. Although I do not quite follow his argument for the existence of two overlapping systems of stars (more dramatically termed "two Universes" by Prof. Turner), I yet venture to suggest an explanation of the

apparently (perhaps really) opposite "drifts," which seems to me to agree sufficiently with the observed facts.

If we adopt Lord Kelvin's postulate of a single vast stellar universe very slowly condensing towards its common centre of gravity, we might expect that the component stars would move for the most part in ellipses or spirals of very varying degrees of eccentricity and of inclination to the mean orbit – perhaps indicated by the Milky Way. If we further postulate (what is very generally admitted) that our sun is situated towards the central rather than towards the outer portion of the whole system, then, just as the planets, through differential angular motions as regards the earth, appear sometimes to move in a retrograde direction or to be quite stationary, so a certain proportion of the stars might be expected, at any given period, to exhibit the same phenomena.

But further, considering the enormous distances that are known to separate the stars and star-groups from each other and the extreme slowness of their angular motions, there seems no reason why their respective orbits should not be almost as frequently in a right-hand as in a left-hand direction in regard to the central plane of general motion.

Our knowledge of the actual motions of the stars may not inaptly be compared to what astronomers would possess of the solar system supposing the whole of their observations had been limited to a period of about twenty-four hours, and that the sun was invisible. The motions of the planets and their satellites thus determined would seem as strange and incomprehensible as do those of the stars at the present time, our accurate observations of which have been limited to a few centuries.

It will probably be of interest to many of your readers (as it certainly will be to myself) if some of your mathematical correspondents will explain why, and in what way, some such system as is here suggested is incompatible with the facts set forth by Prof. Kapteyn and others.

* * * * *

Dr. Alfred Russel Wallace on Socialist Poets (S644)

A letter to columnist A. E. Fletcher printed in the 13 September 1907 issue of The Clarion.

Dear Sir, – I always read your literary articles in the *Clarion* with pleasure, and was glad to see extracts from Sidney Lanier's poems, which I had never heard of. But when you call his 'Symphony' the greatest Socialist poem in the English language I cannot agree with you from the passages you give, as it seems to me too rugged and crude to be *great* poetry – or, at least, great *as* poetry.

To me the greatest *poet* of Socialism, living or dead, is *Edwin Markham*. He is a most perfect master of rhythm and language – full of grand ideas, beautifully expressed, and has the most intense poetic feeling for the truest and highest aspects of Socialism as an ideal state of society and a true religion of humanity.

No doubt his poems are a little above the crowd, but I think they must impress every *lover* of poetry.

In two small volumes of Poems – 'The Man with the Hoe,' etc., and 'Lincoln,' etc., published by McClure, Phillips, and Company, are the following: 'The Muse of Brother-

hood,' I should say the *finest* Socialist poem for the majority of Socialists yet written; and 'The Muse of Labour,' almost as good.

In the 'Man with the Hoe,' etc., are 'The Desire of the Nations,' one of the most exquisite and grandest pictures of the coming of the 'King' who will bring Justice and happiness to the nations; and 'Song to the Divine Mother,' very ideal and beautiful.

Here is one little bit of the 'Desire of the Nations':

> He comes to make the long injustice right –
> Comes to push back the shadow of the night,
> The gray Tradition full of flint and flaw –
> Comes to wipe out the insults to the soul,
> The insults of the Few against the Whole,
> The insults they make righteous with a law.

Grant Allen was also a fine poet of Socialism. In his little volume – 'The Lower Slopes' – is one on (I think) 'Sunday on Braemar' – which is an exquisite poem both for its descriptions and its Socialism. – Yours very truly, Alfred R. Wallace.

* * * * *

Dr. A. R. Wallace and Honours (S658)

In this short note, printed in the 17 July 1908 issue of Public Opinion (London), Wallace decried the amplification of a rumor that he was about to be knighted.

I am surprised at your taking the absurd statement in the 'Table Talk' column of the *Daily News* about me being knighted – as genuine! As if I would accept such a gawd! and so try to put myself before Charles Darwin: and as if the 'Table Talk' is the place in which 'Birthday Honours' are announced. I would not trouble you to correct it but that I get letters from my friends asking about it, and this will increase them.

* * * * *

A Veteran Scientist's Testimony (S664)

This short but revealing communication was printed under this title in the The Christian Commonwealth issue of 9 December 1908.

Dr. Alfred Russel Wallace, O.M., recently wrote, in response to an inquiry:

"I have never given any special study to the character and influence of Jesus Christ. But as one who from boyhood till middle age was a confirmed materialist, and in his later life has become a confirmed spiritualist – but at no time a believer in dogmatic Christianity – my growing impression of late years has been that Jesus of Nazareth was, in his moral and human aspect, the finest character of whom we have any record, and that his influence upon humanity has been – notwithstanding the gross perversion of his teaching by priests and rulers – of *supreme value*.

"The story of his life impresses me as being on the whole a truthful one, as handed

down by his disciples; and, of course, as a Spiritualist, I have no difficulty in accepting the record of his miracles as being, substantially, truthful also.

* * * * *

Who Are the Twenty Greatest Men? (S691a)

In its January 1912 issue The Review of Reviews (London) printed a collection of responses it had solicited from prominent persons, commenting on Andrew Carnegie's answer to this question (Carnegie's list of twenty included only four figures who were not inventors!).

Dr. Alfred R. Wallace.

Our greatest modern man of science is Dr. Alfred Russel Wallace, who . . . writes: – "Mr. Carnegie's list of the twenty greatest men is the most preposterous I have ever seen! I can only retain one of them – namely, Shakespeare. I daresay I should alter mine a good deal if I had more time to give it. I take 'greatness' to apply to *character* more than to any one or more striking or useful discoveries which have often been made by very small – and what a Yankee might call a 'one-horse' man. The great difficulty is that around any one supremely great man there is a cluster of others almost as great, who might almost monopolise the whole twenty, as in the case of Socrates and Michaelangelo. I think my list fairly shows the different types of greatness. Scott, Dickens, and R. Owen will be most objected to, but I could give very good reasons for including each of them. I think Jenner in Mr. Carnegie's list is perhaps the very smallest of over-estimated men. Both Columbus and Lincoln seem to me second-rate."

Homer, 10th or 11th century B.C.
Buddha, 5th century B.C.
Pericles, about 490 B.C.
Phidias, about 490 B.C.
Socrates, about 469 B.C.
Alexander the Great, B.C. 356–B.C. 323.
Archimedes, B.C. 287–B.C. 212.
Jesus of Nazareth.
Alfred the Great, 849–901.
Michael Angelo, 1475–1564.
Shakespeare, 1564–1616.
Newton, 1642–1727.
Swedenborg, 1688–1772.
Washington, 1732–1799.
Walter Scott, 1771–1832.
Robert Owen of Lanark, 1771–1858.
Faraday, 1791–1867.
Darwin, 1809–1882.
Charles Dickens, 1812–1870.
Tolstoi, 1828–1910.

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comments on Charles Dickens (S692b)

This reply, one of many to a query posed by The Bookman (London), appeared in the February 1912 issue of the magazine under the title "Charles Dickens: Some Personal Recollections and Opinions."

Although a life-long admirer of Dickens, and a reader of almost the whole of his works, many of them several times over, I have little to say of him, as I never had the opportunity of making his acquaintance. I first heard his name during the last year of my school-life at Hertford (1836) when the four masters in the school were in a state of excitement about a story which was appearing in monthly parts, and was handed about from one to another. It was spoken of by them as something quite new, and exhibiting marvellous humour and talent. The title, however "The Posthumous Papers of the Pickwick Club," was not very attractive to a schoolboy of thirteen, and I do not think I read it till some years afterwards. A little later, however, I heard my brother William speaking of it to a friend, and saying that the style of humour was above less-educated readers. As an example he referred to the description of the scene in the club meeting, when Mr. Winkle "threw himself upon the chair" to stop the quarrel between Mr. Pickwick and the "haberdasher."

I only saw Dickens once, when I heard him give a reading in St. James' Hall, one of the passages read being the account of the young doctor's supper party, and strange to say, I thought it was not well-read and did not bring out the humour of the scene as many other public readers would have done.

My opinion of his novels is a very high one. I have recently ranked him with Sir Walter Scott as the two most remarkable novelists the world has produced. His greatest story is, I think, "A Tale of Two Cities," followed very closely by "Barnaby Rudge." I owe most to his teaching as to the unity of human nature, showing, as did Herbert Spencer, that virtue and vice, wisdom and folly all pervade all classes in an approximately equal degree; while he has confirmed my deep-seated conviction of the inherent injustice and cruelty of our whole system of law, criminal and civil, which another great man, Jeremy Bentham, has pierced with scathing ridicule.

Notwithstanding all that can be said against his mannerisms and exaggerations, I believe that the myriad characters Dickens has given us constitute a portrait gallery of English life and manners during the mid-nineteenth century that will be read with delight so long as the English language continues to be spoken.

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Finis

Index

Alfred Russel Wallace (1823–1913), colleague of Charles Darwin, co-discoverer of the principle of natural selection, "father" of the field of evolutionary biogeography, vocal socialist and spiritualist, land reform theorist, intense social critic, etc., etc., was one of the most captivating figures of his time. Wallace began his professional career through two great natural history collecting expeditions, one to the Amazon and the other to the Australasian Archipelago; so successful were these that many observers would place him as the front-ranking field naturalist of all time. After he returned to England in 1862, however, his professional emphasis shifted toward writing. His published works included more than twenty books and close to a thousand other items: technical scientific papers, essays, commentaries, book reviews, and, not least, some three hundred letters to the Editor. It is in the last that his temperament comes out most strongly, and it is our privilege in the present work to reproduce more than two hundred of these, extending to all of his many intellectual passions. The philosopher Charles Peirce once wrote of Wallace that he "never wrote a dull line in his life, and couldn't if he tried", and the reader here can expect to be entertained accordingly.

Charles H. Smith, Ph.D., FLS, has been studying Wallace's work for more than thirty years and has several other books on him to his credit; he also maintains the ever-expanding research website *The Alfred Russel Wallace Page* at Western Kentucky University, Bowling Green, where he is Science Librarian and Professor of Library Public Services. Kelsey Patterson is a recent graduate of the Honors Program at the same institution, where she double-majored in Financial Planning and Business Informatics. She is currently employed by Unified Trust Company, N.A., of Lexington, Kentucky.

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ISBN 978-0-9574530-7-4
9 780957 453074 >