

## Alfred Russel Wallace and the Road to Natural Selection, 1844–1858

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**Abstract.** Conventional wisdom has had it that the naturalist Alfred Russel Wallace and his colleague Henry Walter Bates journeyed to the Amazon in 1848 with two intentions in mind: to collect natural history specimens, and to consider evidential materials that might reveal the causal basis of organic evolution. This understanding has been questioned recently by the historian John van Wyhe, who points out that with regard to the second matter, at least, there appears to be no evidence of a “smoking gun” variety proving it so. In the present essay the circumstances of Wallace’s interest in the matter are reviewed, and van Wyhe is taken to task with alternate explanations for the facts he introduces in his argument. The conclusion is that Wallace almost certainly did have the second objective in mind when he left for both the Amazon, and the Far East. **Keywords:** Alfred Russel Wallace, Henry Walter Bates, evolution, natural selection.

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### Introduction

On the basis of a number of lines of evidence extending back to events of the 1840s, it has always been supposed that the naturalist Alfred Russel Wallace (1823–1913) traveled to South America and the Far East with two intentions in mind: to make a living through biological specimen collecting, and to conduct observations that might lead to an

understanding of factors causal to organic evolution. On the first of these objectives, nothing further needs to be said, as both Wallace and his colleague Henry Walter Bates (1825–1892) in fact became prodigious collectors, sending back to England an enormous number of specimens, especially of insects and birds. The second assumption, however, has recently been challenged in an article by John van Wyhe (2014) appearing in the pages of this journal. Van Wyhe argues that there is no “smoking gun” which supports this interpretation – that is, that can be taken as absolute proof that this is what Wallace (and possibly Bates) had in mind when he left England in the spring of 1848 for Brazil, and 6 years later for the Far East. Centrally, he disputes the previously most obvious source of support for the assumption, statements made by Bates in his 1863 journal of travels, *The Naturalist on the River Amazons*.

In the 1863 first edition of that work, Bates famously wrote in the Preface:

In the autumn of 1847 Mr. A. R. Wallace, who has since acquired wide fame in connection with the Darwinian theory of Natural Selection, proposed to me a joint expedition to the river Amazons, for the purpose of exploring the Natural History of its banks; the plan being to make for ourselves a collection of objects, dispose of the duplicates in London to pay expenses, and gather facts, as Mr. Wallace expressed it in one of his letters, “towards solving the problem of the origin of species,” a subject on which we had conversed and corresponded much together (Bates, 1863, vol. 1, p. 3).

But in the next edition, issued a year later, this passage was deleted. Van Wyhe interprets this, and the lack of any known correspondence on the “solving” subject between Bates and Wallace, as suggestive of embroidery on Bates’s part. But no “smoking gun” exists here either. As a result van Wyhe is forced to explore a number of peripheral matters which, while valid ideas on their own account, in the end shed little light on the particular subject at hand. To these we will turn in a moment.

The feeling here is that there is plenty of evidence available to support the conclusion that one of the two main objectives of Wallace’s travels was to investigate possible causes of evolution. No one objects to the idea that his expeditions were financed by natural history specimen collection, which in that sense was a prior consideration, but it is a question of whether this was its own reward, or not. To explore this

statement here the focus will be, as it should be, Wallace, and not Bates.<sup>1</sup>

### Some Initial Questions

The issue of whether Wallace was seeking to investigate evolution during his travels actually has three components, all of which are relevant to the present issue. The first is: Was Wallace an evolutionist (or as they were termed in those days, a “transmutationist”) when he set out on his journeys? Were he not, then the issue of why he would be attempting to tease out such an explanation to begin with would raise itself, considerably complicating matters. Thankfully, there is sufficient evidence on this score, both in the form of period letters and later statements, to convince everyone, including van Wyhe (2014), that he was. His early reading, *circa* 1844 or 1845, of Robert Chambers’s originally anonymously-published *Vestiges of the Natural History of Creation* seems to have been the key influence here; a 28 December 1845 letter to Bates<sup>2</sup> and later referrals in his published writings (Wallace, 1855, p. 185; 1898, p. 139; 1905, vol. 1, p. 354) provide satisfactory substantiation.

A second matter is who it was, Wallace or Bates, who suggested the two travel to South America to become professional collectors. No evidence of the time provides any conclusive information on this matter, but after the fact both Wallace (1892) and Bates (1862, 1863) stated that it was Wallace who provided the push. Again, there is no disagreement on this from van Wyhe. This is not quite so small a matter as it initially might appear, as there seems to be no evidence regarding when exactly Bates himself became a convert to transmutationist thinking. The 28 December 1845 letter from Wallace to Bates suggests that at that point, at least, Bates was unimpressed by the arguments given in *Vestiges*. Bates came around, of course – note the 19 November 1856 letter from him to Wallace (Marchant, 1916, vol. 1, p. 53), in which he says “the

<sup>1</sup> In treating this question here I concentrate both on points made by van Wyhe, and general elements of Wallace’s evolution as a thinker. It should be noted, however, that a recently published book by Costa (2014) also investigates the matter, by looking closely into Wallace’s activities and field book notes from this period. These leave little doubt as to what Wallace had in mind at the time. At one point Costa writes: “Wallace’s most important field notebook of the period – his Species Notebook – is to me, the best antidote for the malady of Wallace nay-saying, dismissal, or minimizing” (p. 277) – seemingly with the remarks of van Wyhe in mind.

<sup>2</sup> Natural History Museum, London, NHM WP1/3/17.

theory I quite assent to, and, you know, was conceived by me also” – but 10 years earlier the prospect of going to South America for the purpose of developing a theory of the emergence of species might have had less allure for him than for Wallace. Interestingly, van Wyhe quotes from a letter dated 16 April 1863 that Bates sent to a prospective reviewer of his book that bears on this question: “...what (I venture to say) merits some attention about Mr. Wallace, and in a very far less degree of myself, is that his main object – which he never lost sight of – was the study of the objects collected with a view to philosophical conclusions” (van Wyhe, 2014).

It is too much at this point to suggest that Wallace “tricked” Bates into accompanying him to South America by playing up the collecting side of the venture, but it is nevertheless arguable that Wallace would have had perfectly good reasons for downplaying any additional motives – not only to Bates, but to everyone else as well. Van Wyhe argues that it is surprising that Wallace’s writings of the time do not specify the supposed intent, but should we expect he would have wanted to advertise the fact? Apart from Bates, to whom would he have wished to entrust this information? Family probably would not have cared, and even if they did, Wallace might not have wanted them to inadvertently spread the intelligence. Consider the position of Wallace and Bates as rank amateurs. Professionals of the time likely would have found such a stated goal a matter for amusement, a Don Quixote-esque plan that could only end in failure. Ultimately, it *did* end in failure, and by not advertising his goals beforehand Wallace exhibited a certain coyness that saved him from possible later ridicule, and a reduced likelihood of obtaining support for further fieldwork. The kinds of attitudes involved are evidenced by the criticism Wallace received for his “theorizing” after publishing the Sarawak Law essay in 1855 (Wallace, 1855) – collectors were supposed to be out there collecting, not spinning abstract speculations. This same coyness explains his not mentioning any related ideas in *Travels on the Amazon and Rio Negro*: at that point he didn’t even have a theory to test, much less evidence bearing on one. Under such conditions one says – nothing.

An isolated point that escapes van Wyhe is the mere fact of the Sarawak Law essay itself. If Wallace had no philosophical agenda as of 1855, why would he have been so upset by the Edward Forbes article (1854) and responded with his theory to begin with? If Wallace only became “philosophically engaged” upon observing the ecologically-correlated coloration patterns of tiger beetles in 1858 (as van Wyhe

suggests in his 2013 book *Dispelling the Darkness*), how does one explain this previous act?

The third question, regarding his goals, is the one of central interest here, and can only be answered fully by first examining Wallace's influences and activities in the periods prior to 1848 and 1854.

### **The Evolution of Wallace as a Thinker**

There is a basic question to be addressed on this overall matter before coming to a conclusion based on the absence of evidence: What kind of person was Alfred Russel Wallace in his early days, and is it consistent with his later behavior that he took an active interest in intellectual exploration, especially as related to evolutionary subjects? From his autobiography *My Life* (Wallace, 1905) we get a good picture of this person. Pages could now be filled with many relevant details of his exploratory nature as a teen and young man, but let us concentrate on a few items only.

First, it is apparent that even before he met Bates and became fully immersed in natural history collecting, Wallace had been taking a strong interest in certain philosophical elements of both societal and personal evolution. Two of his earliest essays, from around 1843 but first printed in *My Life* in 1905, give evidence of his drive to discover. Both, it seems, are relatable to early experiences of his with a London Owenite group, and the writings of Thomas Paine and Robert Dale Owen. In the essay he titled "The Advantages of Varied Knowledge," he writes:

There is an intrinsic value to ourselves in these varied branches of knowledge [*i.e.*, history, biography, art, and science], so much indescribable pleasure in their possession, so much do they add to the enjoyment of every moment of our existence, that it is impossible to estimate their value, and we would hardly accept boundless wealth, at the cost, if it were possible, of their irrecoverable loss. And if it is thus we feel as to our general store of mental acquirements, still more do we appreciate the value of any particular branch of study we may ardently pursue.... Can we believe that we are fulfilling the purpose of our existence while so many of the wonders and beauties of the creation remain unnoticed around us? While so much of the mystery which man has been able to penetrate, however imperfectly, is still all dark to us? While so many of the laws which govern the universe and which influence our lives

are, by us, unknown and uncared for? And this not because we want the power, but the will, to acquaint ourselves with them. Can we think it right that, with the key to so much that we ought to know, and that we should be the better for knowing, in our possession, we seek not to open the door, but allow this great store of mental wealth to lie unused, producing no return to us, while our highest powers and capacities rust for want of use? (Wallace, 1905, vol. 1, pp. 201–203)

In the other essay, entitled “The South-Wales Farmer,” he notes:

Their [*i.e.*, Welsh farmers’] system of farming is as poor as the land they cultivate. In it we see all the results of carelessness, prejudice, and complete ignorance. We see the principle of doing as well as those who went before them, and no better, in full operation; the good old system which teaches us not to suppose ourselves capable of improving on the wisdom of our forefathers, and which has made the early polished nations of the East so inferior in every respect to us, whose reclamation from barbarism is ephemeral compared with their long period of almost stationary civilization. (Wallace 1905, vol. 1, p. 207)

Clearly, this is an admonition concerning those, and their societies, who do *not* pursue the acquisition of “varied forms of knowledge”.

In an even earlier essay concerning the function of mechanics institutes, apparently written about 1841, he muses:

... it is the society’s duty, both to supply the materials for diffusion of scientific and historical information, and by subsequent proceedings, which we shall point out, endeavour to create or increase a taste for seeking it.... The materials for diffusing information being thus prepared by the institution, it becomes desirable to create a taste for its acquirement... As the means of inciting to the acquirement of knowledge on all subjects, of creating a wish for information on what have been hitherto considered as abstruse branches of knowledge, but which are frequently among the most interesting and generally useful,— and of inspiring a desire for diving deeper into its inexhaustible stores not yet exposed to the scrutinizing gaze of man, such an institution as this, conducted in the way we have described, will be invaluable... all who have become really great have had the desire, and in some degree at least, the means of obtaining knowledge; and may we not conclude that as knowledge and the means of acquiring it are more generally

diffused, increased numbers of men... will burst forth and shine in all the splendour of their talents.... Many more instances might be adduced, if more were necessary, to shew that great discoveries do not arise from the chance thoughts of the uneducated, but from the laborious studies of the wisest men; and is it not therefore the surest way to increase the number of such men... It is our duty then to disseminate all the information in our power, and to use every means for exciting all, especially the young, to the acquirement of knowledge – knowing that we shall thereby promote their own happiness as well as benefit the community. (Wallace, 1845, pp. 67–70)

Later in life, Wallace reflected several times, in various contexts, on this urgency of his. In a 25 April 1859 letter from the field to his brother-in-law Thomas Sims, he noted: “So far from being angry at being called an enthusiast (as you seem to suppose), it is my pride and glory to be worthy to be so called. Who ever did anything good or great who was not an enthusiast?”<sup>3</sup> Many years later he wrote to T. D. A. Cockerell:

As to my interest in biology... I doubt if I had or have any *special* aptitude for it, but I have a natural love for *classification* and an inherent desire to *explain things*;– also a great love of beauty of form and colour. When... I was about 15... I purchased... a little book on botany... which... was a revelation to me, and kept me employed for a year or two determining the flowers I met with... [Later] I met with Loudon’s ‘Encyclopædia of Plants,’ and finding that this contained brief characters of all British plants, I amused myself by copying them *all*, except I think the grasses and sedges, on sheets of note paper, which I interleaved in Lindley’s volume, and by means of these I was able to determine most of the species I met with, and made a considerable herbarium... [I then met] H. W. Bates at Leicester... [who started me] as a beetle and butterfly collector. The enormous *variety* of form and structure in the beetles attracted me, and I think during all my tropical experiences the collection of these gave as much enjoyment as even the gorgeous birds and butterflies. Classification then began to fascinate me, through Swainson, and the ‘Vestiges of Creation,’ with the works of Herbert Spencer, started me on the problem of the origin of species; and thus my various mental tendencies had full occupation in the contemplation and study of natural objects. (Cockerell, 1903, p. 517)

<sup>3</sup> NHM WP1/3/46.

In 1908, on accepting the first Darwin-Wallace Medal from the Linnean Society, he reminisced about his actions during the Ternate essay (Wallace, 1858) period, describing himself thusly: “I was then (as often since) the ‘young man in a hurry’” (Wallace, 1909, p. 7).<sup>4</sup> And finally, in 1912, Wallace wrote the following as part of a letter to students at the University of Colorado:

The wonders of nature have been the delight and solace of my life. From the day when I first saw a bee-orchis (*Ophrys apifera*) in ignorant astonishment, to my first view of the grand forests of the Amazon; thence to the Malay Archipelago, where every fresh island with its marvellous novelties and beauties was an additional delight – nature has afforded me an ever-increasing rapture, and the attempt to solve some of her myriad problems an ever-growing sense of mystery and awe (Anon., 1912, p. 487).

The preceding words, drawn from across his entire adult life, expose Wallace – including “the early Wallace” – as an individual *driven* by a Spinozian agenda of self-improvement through the acquisition of knowledge. In the early mid-1840s, this focused agenda engaged several influences, nearly simultaneously, that turned him toward natural history investigations, and in particular the development hypothesis, as an outlet for this enthusiasm.

The first influence was possibly his discovery of the writings of Charles Lyell. Lyell’s adoption of Huttonian uniformitarian thinking most likely appealed to Wallace for its rejection of catastrophism and creationism. Here, too, he probably first encountered the ideas of Lamarck on organic change, which he quickly dismissed. But it was this particular model of organic change that he dismissed, and not the notion of “development” altogether, as witnessed by his general support for the message of *Vestiges* a short time later.

Much of this discovery was going on immediately before and during Wallace’s short tenure as a teacher in Leicester, *circa* January 1844 to Easter 1845. At some point during this period he apparently digested

<sup>4</sup> In Smith (2013a) I listed five occasions on which Wallace discussed the timing of the delivery of his 1858 essay to Darwin, but overlooked a further one, the sentence preceding the one with the “young man in a hurry” remark: “The idea came to me, as it had come to Darwin, in a sudden flash of insight: it was thought out in a few hours – was written down with such a sketch of its various applications and developments as occurred to me at the moment, – then copied on thin letter-paper and sent off to Darwin – all within one week” (Wallace, 1909, pp. 6–7). This “one week” comment is further evidence that Wallace’s letter and manuscript were sent to Darwin in March of 1858, not April, as van Wyhe (2013) argues.

von Humboldt's *Personal Narrative of Travels*; according to his autobiography *My Life*, a few years earlier he had read Darwin's *Journal of Travels*.<sup>5</sup> These were especially influential in starting him to think about travel (as probably were the employment-related tours of his sister Fanny in France and the United States during this time; on her return in 1847 he even wrote a letter to the Editor (Wallace, 1848) extolling the virtues of emigration). It was also during this time that he became interested in mesmerism and phrenology, which undoubtedly re-ignited his interest in personal evolution and its characteristics.

But the most important of Wallace's new influences was quite possibly from a source that has not generally been recognized: Alexander von Humboldt (Smith, 2013b). Though Humboldt has frequently been mentioned as an early inspiration for Wallace's *travel* activities, his possible *philosophical* influence on the young naturalist has been entirely ignored. The *Personal Narrative of Travels* is only one of several von Humboldt books that Wallace might have read before travelling to South America in 1848 or the Malay Archipelago in 1854. In the same 28 December 1845 letter to Bates mentioned earlier he expresses a strong interest in reading the first volume of von Humboldt's new work *Cosmos* (then already available in English translation), which summarizes the latter's views on "terrestrial physics" (roughly, physical geography, including biogeography). These featured notions of an interconnectedness of nature based on a "general equilibrium" of mechanical forces, notions summarized in a famous early writing of his, "Essai sur la Géographie des Plantes" (1805):

The general equilibrium obtaining in the midst of these disturbances and apparent disorder is the results of an infinite number of mechanical forces and chemical attractions which balance each other; and while each series of facts must be examined separately to recognize a specific law, the study of nature, which is the main problem of general physics, demands the gathering together of all the knowledge dealing with modifications of matter. (Robin et al., 2013, p. 214)<sup>6</sup>

Such views, entirely consistent with Wallace's already-stated philosophy, apparently gave him a starting point for his investigations; many of von Humboldt's ideas from *Cosmos* and possibly other works (for example

<sup>5</sup> It is uncertain as to which editions of these works he read.

<sup>6</sup> This quotation is from a recent translation, "Essay on the Geography of Plants". Wallace may also possibly have known of this work, in its original French (of which he had some fair comprehension), before he left England in 1848 or 1854.

*Aspects of Nature* or *Political Essay on the Kingdom of New Spain*) later show up in a variety of contexts in Wallace's writings (see below). I am currently looking into the nature of this influence in more detail.

Further, it was not only Humboldt himself who provided an impetus, but also a number of his many followers. Lyell and Darwin were among these, but their debt to Humboldt lay more in the method of scientific inquiry – fact collection, etc. – than in any extension of Humboldtian “terrestrial physics.” They were, after all, historical scientists, mainly: geologists. Humboldt, on the other hand, was primarily a geographer, and his focus was more on the immediate inter-relationships of the environment. The work of two of his protégés in this direction, the chemist Justus von Liebig and the botanist Franz Julius Ferdinand Meyen, are also known to have caught Wallace's attention, and likely helped to fuel his intellectual fires (Smith, 2013c).

The best evidence of von Humboldt's influence on Wallace, arguably, is the natural selection model itself. Bateson (1972) and Smith (2004, 2005) have viewed Wallace's “steam engine governor” analogy in the Ternate essay (Wallace, 1858) as identifying what would now be considered a negative feedback loop – a cybernetics concept – but it has also sometimes been viewed as an early depiction of the concept of “stabilizing selection.” This is not quite correct, however, as is apparent from the title of the essay: “On the Tendency of Varieties to Depart Indefinitely from the Original Type.” Wallace thus took the Humboldtian position a step further: while an equilibrium in the “general forces” manner is envisioned, it is advanced, effectively, to a dynamic equilibrium embracing the notion of irreversible change.

All of this adds up to the picture of a man who likely was consumed by philosophical questions during every moment of his free time. Yet van Wyhe suggests that his only purpose in traveling to the Amazon in 1848 and the Far East in 1854 was the fairly mundane activity of collecting specimens. We will now try to answer van Wyhe's individual objections, in the process introducing some facts that he ignores.

### **The Particular Complaints of John van Wyhe**

Van Wyhe's approach to this particular “Wallace question” features several treatments of what he argues are sometimes forgotten principles of professional research in the history of science. I think this is the most useful aspect of his paper, but, unfortunately, I don't see that they much pertain to the matter at hand. It is probably best to respond here to these remarks, serially.

(1) Perhaps the most important, and generally valid, of the points van Wyhe makes is the warning that we should not attempt to use terminology invented after the fact to describe the thoughts leading up to some important conceptual revolution. In this instance he draws attention to the use of the term “origin of species,” which according to him “was not in use” before Darwin’s promotion of it, as evidenced by the fact that it appears nowhere in the literature before 1859. I was therefore surprised to be able to find, in less than an hour’s time, about a dozen places in the literature where it *does* appear before 1859, and in most instances as parts of discussions concerning the development hypothesis and related thinking. For example, in 1819 Thomas Forster wrote: “The origin of species generally may be reckoned very doubtful. Species may be only varieties become permanent in their character by time” (Forster, 1823, p. 380). In volume three of the English edition to his *Political Essay on the Kingdom of New Spain*, Alexander von Humboldt wrote: “Whatever relates to the origin of species, to the hypothesis of a variety become constant, or a form which perpetuates itself, belongs to problems in zoonomy, on which it is wise to avoid pronouncing decisively” (Humboldt, 1822, vol. 3, p. 66). Some other pre-1859 employers of the term include Schouw (1852, p. 20), LeConte (1852, p. 35), Chambers (1845, pp. 126, 129), Hooker (1853, pp. 1, xix), Powell (1856, pp. 423, 546), and the conchologist Adams (1850, pp. 79, 87, 140), none of whom were obscure figures.<sup>7</sup> Further, closely analogous terms such as “origin of the species,” “origin of organic life,” “origin of the organic kingdoms,” etc., were also in usage. Thus, one cannot reasonably argue that Wallace’s use of the term in his 11 October 1847 letter to Bates<sup>8</sup> was a freak occurrence to which no meaning can be attached, as van Wyhe insists – Wallace very likely knew the Chambers work<sup>9</sup> and quite possibly the von Humboldt one (it was one of his most widely circulated titles, and surely Wallace could have found a copy of it in the Leicester library and/or elsewhere), and one or more of the others, conceivably. And this doesn’t even count the relatively common application of the terms “transmutation” and “development” to

<sup>7</sup> To this list may be added Van Amringe, 1848, p. 737; and Gray and Adams, 1857, pp. 86, 89. An all-out search for the term would doubtless turn up many further instances, but such efforts are complicated by the “uncorrected” nature of plain-texts in the databases covering this period of literature, line-ending carriage returns in same that split up phrases, and the not-infrequent presence of compound word sequences separating the words “origin” and “species.”

<sup>8</sup> NHM WP1/3/19.

<sup>9</sup> There is evidence that Wallace actually owned a copy of Chambers’s *Explanations* (a sequel to *Vestiges*) (J. T. Costa, personal communication).

describe, generally, what we would now term “evolution,” though of course not as associated with any particular causal model.

(2) Van Wyhe also complains that the Wallace letter to Thomas Sims dated 20 January 1851,<sup>10</sup> which includes a list of the publishing projects he has in mind, makes no mention of anything related to the development hypothesis. Why should we expect that it would? At that point, Wallace had not even come up with a provisional causal model to test, much less any evidence that such a model was valid. Note, however, that after he returned to England in 1852 he wrote, obliquely to be sure, on development three times over the next 2 years:

Are very closely allied species ever separated by a wide interval of country? What physical features determine the boundaries of species and of genera? Do the isothermal lines ever accurately bound the range of species, or are they altogether independent of them [an allusion to the work of von Humboldt and his followers]? What are the circumstances which render certain rivers and certain mountain ranges the limits of numerous species, while others are not? (Wallace, 1852, pp. 109–110)

...In all works on Natural History, we constantly find details of the marvellous adaptation of animals to their food, their habits, and the localities in which they are found. But naturalists are now beginning to look beyond this, and to see that there must be some other principle regulating the infinitely varied forms of animal life.... It has been assumed by some writers on Natural History, that every wild fruit is the food of some bird or animal, and that the varied forms and structure of their mouths may be necessitated by the peculiar character of the fruits they are to feed on; but there is more of imagination than fact in this statement: the number of wild fruits furnishing food for birds is very limited, and the birds of the most varied structure and of every size will be found visiting the same tree. (Wallace, 1853, pp. 83–85)

All these groups are exceedingly productive in closely allied species and varieties of the most interesting description, and often having a very limited range; and as there is every reason to believe that the banks of the lower Amazon are among the most recently formed parts of South America, we may fairly regard those insects, which are peculiar to that district, as among the youngest of species, the

<sup>10</sup> NHM WP1/3/73.

latest in the long series of modifications which the forms of animal life have undergone. (Wallace, 1854, p. 258)

These utterances clearly demonstrate that even though he had come to no conclusions regarding causation, he was still actively engaged with the subject. In 1862 Bates gave a talk before the Leicester Literary and Philosophical Society entitled “Travels on the River Amazon”; unfortunately no trace of the full paper seems to remain, but there was extensive coverage of the event in the local newspapers. In one of them Bates is quoted as saying “In the autumn of 1847, Mr. A. R. Wallace... proposed to me, that we should make together a voyage to explore the Natural History of the banks of the great river Amazon”<sup>11</sup>; in another newspaper this came out as how the two had “arranged to go together and explore the natural history on the banks of the river Amazon.”<sup>12</sup> Although particular “exploration” objectives are not alluded to in either account, neither is the “collecting of specimens” reported as the special or only object of the trip.

(3) Van Wyhe quotes the well-known sentence in Wallace’s Sarawak law essay (Wallace, 1855) – how it had been “about ten years since the idea of such a law suggested itself to the writer of this paper and he has since taken every opportunity of testing it by all the newly ascertained facts with which he has become acquainted, or has been able to observe himself” – and directly comes to the astonishing conclusion that “there is nothing in the passage about searching for a solution.” It is difficult to put the “taken every opportunity of testing it” remark into any other context, however – isn’t this exactly what one does when “testing” is involved? Van Wyhe tries to argue that Wallace’s discussion does not extend beyond considerations of species replacements/successions, a possibly creationist position. Darwin thought this of Wallace’s discussion too, but there is more going on here than just this.

First, the Sarawak law concept is a good deal deeper than just a remark about succession, but Wallace would not have been foolish enough to use any more precise language at that point. It is of course about succession in general, but Wallace’s analysis concerns change over time accompanied by a diffusion process in space: an obvious nod not just to “replacement” or “succession,” but to evolutionary change. Michaux (2000) has written: “it is difficult to believe that Wallace had not worked it all out at least 3 years earlier than generally admitted [*i.e.* 1855 instead of 1858]. Wallace’s use of language to conceal his evolu-

<sup>11</sup> *Leicestershire Mercury*, 13 December 1862, p. 8.

<sup>12</sup> *Leicester Journal*, 12 December 1862, p. 7.

tionary leanings is instructive. He has two particularly difficult moments. The first comes quickly when he refers to what are clearly common ancestors as “antitypes.” He even refers to ‘common antitypes’ at one point!” Wallace also uses the famous “branching tree analogy” in the essay, at one point writing:

It is evidently possible that two or three distinct species may have had a common antitype, and that each of these may again have become the antitypes from which other closely allied species were created. The effect of this would be, that so long as each species has had but one new species formed on its model, the line of affinities will be simple, and may be represented by placing the several species in direct succession in a straight line. But if two or more species have been independently formed on the plan of a common antitype, then the series of affinities will be compound, and can only be represented by a forked or many-branched line. (Wallace, 1855, p. 186)

Meanwhile, this is exactly the process model that has been adopted by modern-day vicariance biogeographers, who, it has been noted (e.g. Lieberman, 2005; Michaux, 1991), were anticipated by Wallace’s words in this essay. Vicariance models, we should be reminded, also do not necessarily subscribe to a particular model of divergence – or even Darwinism at all – just to the observable results of an assumed evolutionary process.

Most importantly, van Wyhe completely ignores Wallace’s discussion of how vestigial organs may be related to the process envisioned:

Another important series of facts, quite in accordance with, and even necessary deductions from, the law now developed, are those of rudimentary organs. That these really do exist, and in most cases have no special function in the animal œconomy, is admitted by the first authorities in comparative anatomy.... To every thoughtful naturalist the question must arise, What are these for? What have they to do with the great laws of creation? Do they not teach us something of the system of Nature? If each species has been created independently, and without any necessary relations with pre-existing species, what do these rudiments, these apparent imperfections mean? There must be a cause for them; they must be the necessary results of some great natural law. Now, if, as it has been endeavoured to be shown, the great law which has regulated the peopling of the earth with animal and vegetable life is, that every change shall be gradual; that no new creature shall be formed

widely differing from anything before existing; that in this, as in everything else in Nature, there shall be gradation and harmony,—then these rudimentary organs are necessary, and are an essential part of the system of Nature. Ere the higher Vertebrata were formed, for instance, many steps were required, and many organs had to undergo modifications from the rudimental condition in which only they had as yet existed. (Wallace, 1855, pp. 195–196)

It is beside the point that Wallace had the wrong interpretation of “rudimentary” structures (*i.e.*, as *incipient* features); clearly he was thinking in terms of gradual change. In this quote, by the way, we see additional indications of von Humboldt’s influence: the notion “that in this, as in everything else in Nature, here shall be gradation and harmony” (this being a central tenet of the earlier-mentioned “general equilibrium” stance taken by the Humboldtian camp).

(4) In the 11 October 1847 letter to Bates, as van Wyhe notes, Wallace says “I begin to feel rather dissatisfied with a mere local collection... I shd. like to take some one family, to study thoroughly – principally with a view to the theory of the origin of species.” As I have already discussed, it is not unlikely that Wallace already had a relatively conventional post-1859 interpretation of the concept “origin of species,” which, contrary to van Wyhe’s claims, was in use, if sparsely, in the decades prior to that date. It is possibly true that, as van Wyhe suggests, Wallace was only interested in studying intensively a family of insects without actually travelling, but it is difficult to say exactly what he had in mind from the words in the letters. At this time he was returning from an examination of some excellent natural history collections in Paris, and may well have just read the W. H. Edwards book *A Voyage Up the Amazon*, then newly published. Van Wyhe cites the *Publishers’ Circular* for 1847 as recording its publication in England as “September 1847,” but this is incorrect: the publication notice appeared in the 1 September issue, which listed those works that had been released over the second half of August. In addition to the 7 August *Spectator* review van Wyhe mentions, it was noticed in the 21 August issue of the *Exeter and Plymouth Gazette*, and advertised as “this day published” in the *London Daily News* issue of 12 August and *The Times* issue of August 5. If the book was available as early as mid-August, or even late August, Wallace may well have seen it before his trip to Paris and already been primed by it. The exact date of his proposal to Bates to travel would be interesting to know, but we do not have this information. Note however that even if the date of such discussion were after the October letter, it has no necessary bearing on what Wallace’s mindset was in the months preceding.

(5) I find van Wyhe's long discussion of the absence of the word "problem" (of the origin of species) in pre-1859 writings unconvincing, at least as applied here. While it may be true that the matter only assumed the word label of "problem" after 1859, it is clear that people, in considerable number, were discussing transmutation before then. This is a distinct issue from what happened after 1859, when Darwin's book made the matter, to many trying to defend natural selection and many decrying it alike, a problem, *ipso facto*. I fail to see why an investigation based on interest in the related subjects necessarily must specifically be labelled "a problem." The *OED* gives as its most general definition of the term "a thing thrown out or put forward," but also distinguishes between "a question proposed for academic discussion" and "a doubtful or difficult question" (e.g., "the *problem* of social discontent"). The most likely reason for the general lack of instances of the phrase "the origin of species problem" in the pre-1859 literature is the combination of the infrequent use of the term "origin of species" with the fact that it was not a "problem" until Darwin made it so. Thus, while it is *technically* true that post-1859 sources had no business using the word "problem" to describe its pre-1859 usage in the second sense above, it still actually *had* been a "problem" in the first sense noted above.

(6) For further evidence of what Wallace had in mind, consider the following items. First, we have two Bates letters, from 1860 (Darwin to Bates) and 1861 (Bates to Hooker). Van Wyhe mentions the two letters (van Wyhe, 2014), but, strangely, passes by them without comment:

The earliest occasion I have found when Bates claimed that the Amazon expedition had a higher purpose than collecting was in a now lost letter to Darwin in 1860. Darwin replied [22 November 1860]: "I did not know that you had worked with high philosophical questions before your mind."... Bates to Hooker 19 March 1861: "during the whole time [I] directed my attention to the modifications of species and kindred subjects."

Perhaps these remarks can be interpreted, as van Wyhe hints, as fibs Bates told to help him improve his professional reputation, but this is just unsubstantiated conjecture.

Also notable are remarks on events of 1854–1855 made by Spenser St. John, Rajah James Brooke's personal secretary, in a book he published some years later:

We had at this time in Sarawak the famous naturalist, traveler, and philosopher, Mr. Alfred Wallace, who was then elaborating in his

mind the theory which was simultaneously worked out by Darwin – the theory of the origin of species; and if he could not convince us that our ugly neighbours, the orang-utangs, were our ancestors, he pleased, delighted, and instructed us by his clever and inexhaustible flow of talk – really good talk. The Rajah was pleased to have so clever a man with him... (St. John, 1879, p. 274).

It is true these words were written years later, but is there any reason to suspect St. John was also deliberately setting out a half-truth? Would the Rajah been so “pleased” to have “so clever a man” staying with him had their talk only extended as far as the catch of the day?

(7) Finally, van Wyhe returns to the subject of Wallace’s notice of the coloration properties of a species of tiger beetle (genus *Cicindela*) he encountered in early 1858. In so doing, he risks the reader’s conclusion that the only reason for his entire paper is an attempt to support his previous argument (van Wyhe, 2013) that Wallace’s “ah-ha” moment *vis-à-vis* natural selection happened just then, and not some weeks later, during his famous malarial episode at or near Ternate. Thus, we are asked to believe that Wallace, until that point merely pursuing an agenda of specimen collection, had an epiphany unrelated to any prior questioning. Now it is in fact known from his notebooks (Costa, 2013) that Wallace did take notice of the species’ varying coloration under different ecological settings, and one might be prepared to accept that this encounter provided some new and intriguing data, but it is difficult to go much further than this. The event is not mentioned in any of Wallace’s subsequent writings (though he does, of course, discuss it and the fact that he has “lately worked out a theory which accounts for them naturally” in the famous 2 March 1858 letter to Frederick Bates<sup>13</sup>), and even the adaptive significance of the phenomenon itself is given but very short notice in his later works. This, as opposed to the eight times he specifically wrote up descriptions of the malaria-related event. So overall the evidence is that Wallace himself attached relatively little significance to the event.

Van Wyhe’s treatment of the facts at hand almost seems to point to a facile and unacceptable argument: that because Wallace was not actually in possession of the natural selection idea before he embarked on his expeditions, he could not possibly have viewed the greater subject (“the origin of species”) as a research agenda during them. But this is an impossible limitation to accept.

<sup>13</sup> NHM WP1/3/42.

**Bates**

When I first heard of van Wyhe's theory that Bates might have "forged" his comments in the Preface to the first edition of his book in 1863, and then removed them in subsequent editions, I began to imagine possible scenarios to account for this. Perhaps Bates felt guilty about telling a fib, and rectified the situation. Or perhaps he was fed up with having most of the attention on the matter fall on Wallace, and cut him out of the story. Or maybe Wallace contacted him to tell him that his words gave some kind of distorted appreciation of the situation, and asked him to change them. Conversely, maybe Wallace suggested to Bates that he delete the comments, as not being generous enough to himself (Bates). Or could those couple of sentences just have experienced the editor's chopping block (the new edition was to be three hundred pages shorter than the first), because a lot of things had to go?

But all of this questioning was pointless. The explanation is clear enough, without recourse to accusations of plagiarism or invented scenarios. First, it turns out that in a letter from Wallace to Bates dated 1863, and referring to Wallace's first reading of Bates's newly-published book, he writes: "...On the whole I must congratulate you on having produced so extremely pleasant and interesting a book, which I am sure will delight all who know you, and if the general public do not also appreciate it, it will show that they have no taste left for unadulterated and unsensational books of travel. Thanks for the kind manner in which you have mentioned my name" (Clodd, 1916, p. 65). It would appear from this that Wallace was happy enough with what Bates had written.

More significantly, van Wyhe has not given a full picture of the extent of the "deletion" of Bates's comments on the reasons for the Wallace-Bates expedition. The second edition of his book, published the following year, *completely replaces* the original preface with another one matched to a totally different agenda. It begins and ends as follows:

Having been urged to prepare a new edition of this work for a wider circle than that contemplated in the former one, I have thought it advisable to condense those portions which, treating of abstruse scientific questions, presuppose a larger amount of Natural History knowledge than an author has a right to expect of the general reader... The probability of general curiosity in England being excited before long with regard to this hitherto neglected country, will be considered, of itself, a sufficient reason for placing an account of its natural features and present condition within reach of all readers. (Bates 1864, pp. v-vi)

So, the new edition was to be a “dumbing-down” of the original text, eliminating introductory chatter and excessive technical talk – to the end of attracting readers with an economic, rather than a natural history, interest. What more needs to be said? Scenario-spinning notwithstanding, there is no reason to suspect Bates was creating an invented story, especially as the 1847 letter from Wallace to which he was referring actually exists, uses the very term “origin of species” he quotes, and as I have shown can be related to other contemporary uses of it.

### **Conclusion: On “Smoking Guns” and Historical Study**

In coming to the conclusions he has reached on Wallace’s intellectual journey *circa* 1844–1858, John van Wyhe leans heavily on a “smoking gun” kind of thinking. Actually, he even goes a step further, deciding that it is worthwhile to theorize in certain instances on the basis of there *not being* a smoking gun. In the present story, the absence of contemporary writings (at least, by Wallace) *specifically* stating a primary intent to travel to the tropics to investigate the causes of evolution is interpreted as evidence that this was not on his mind, his existing interest in the development hypothesis and disposition toward travel notwithstanding. There are serious problems with this kind of thinking, however.

Anyone who has followed the plots of movies or television programs over the years, or noted the number of real-life criminal convictions overturned on the basis of new ways of assessing evidence, will realize that there are few instances in which a dependable “smoking gun” situation presents itself. Any situation short of a “policeman-barges-in-and-views-a-man-standing-over-a-bleeding-body-with-a-still-smoking-weapon” event ends up depending on other evidence to produce a conviction. This indeed is why the police are so interested in establishing motives. In a historical setting, only rarely is there a “smoking gun” at hand to provide decisive information, especially when the subject of discussion is ideas.

Importantly, people don’t always tell the truth, either verbally or as the written word. Neither do they always tell the full truth, if this is in their interest. (Even our justice system operates in this fashion, as it is not the job of lawyers to uncover the truth, but instead to protect the interests of their clients.) Thus personal letters or words spoken aloud and recorded, can only rarely provide the final say. It goes without

saying, then, that *lack* of evidence – that is, content *absent* from personal letters or the spoken word – presents a practically worthless argument.

There is an old adage that bears on this matter, and especially so in the present instance: “actions speak louder than words.” Here, Wallace’s actions over his entire adult life speak for themselves. From his teen years on, Wallace was always a man on a mission.

Ironically, the picture van Wyhe is trying to paint of Wallace as an “enthusiast who got lucky” better fits Darwin than it does Wallace. It was Darwin’s discovery of fossil remains in Patagonia during his *Beagle* adventure that provided him with a first “ah-ha” moment as to evolution, and he only committed to a transmutation agenda once the trip was over.

In conclusion, it appears clear to me that Wallace did in fact – absence of a “smoking gun” indicator notwithstanding – conduct his field studies with a view toward trying to discover how new species originated. We should only go so far with this verdict, of course. Obviously, this interest, in itself, could not have paid the bills, so it was a prior consideration that professional natural history collecting should be the immediate goal of the expeditions. But this was no hardship, as both Wallace and Bates were the most enthusiastic of collectors. And it is also apparent that both young men were truly attracted to the prospect of experiencing the romance of tropical nature. In the Preface to his *A Narrative of Travels on the Amazon and Rio Negro*, Wallace wrote:

An earnest desire to visit a tropical country, to behold the luxuriance of animal and vegetable life said to exist there, and to see with my own eyes all those wonders which I had so much delighted to read of in the narratives of travelers, were the motives that induced me to break through the trammels of business and the ties of home, and start for “Some far land where endless summer reigns.” My attention was directed to Pará and the Amazon by Mr. Edwards’s little book, ‘A Voyage up the Amazon,’ and I decided upon going there, both on account of its easiness of access and the little that was known of it compared with most other parts of South America. I proposed to pay my expenses by making collections in Natural History, and I have been enabled to do so. (Wallace, 1853, p. iii).

Wallace, however, as we have seen, was driven to be more than “just” a collector (Bates, perhaps, not as much so). “Beholding” might have been fun, but with Wallace the gears were always turning.

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