

WALLACE, DARWIN AND TERNATE 1858

by

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Recent debates on the mailing date of Alfred Russel Wallace's 'Ternate essay' to Charles Darwin in the spring of 1858 have ignored certain details that, once taken into account, alter the matter considerably. Here, a closer look is taken at the critical question of whether Wallace's manuscript-accompanying letter represented a reply to the Darwin letter that arrived in Ternate on 9 March; it is concluded that it very probably did not.

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Recent publications<sup>1</sup> have set forth theories as to whether Darwin acted legitimately on his reception of Alfred Russel Wallace's 1858 'Ternate' essay on natural selection. I have also commented on this matter,<sup>2</sup> but it seems appropriate at this time to extend these arguments.

The exact chronology of the events involved has been critical to related discussions over the years.<sup>3</sup> In particular, when (and under what circumstances) did Wallace send his materials to Darwin, and when did Darwin actually receive them? As the original documents have been lost, only two concrete pieces of evidence remain: (i) a letter dated 2 March 1858 that Wallace sent to the brother of his friend Henry Walter Bates, Frederick, that Frederick received in England on 2 June, and (ii) a letter dated '18' (ostensibly June) that Darwin sent to Sir Charles Lyell lamenting the reception 'to day' of Wallace's essay and note.

A first point to be made is this: that, notwithstanding the studies by van Wyhe, Rookmaaker, Davies, Brackman and Brooks on the steamship mail routes in that time and place, what Darwin received from Wallace in June 1858 was a letter and manuscript, not a ship. The main current protagonists, van Wyhe and Davies, do not agree on what ships might have been able to relay such mail at what times, the result being a 'he said—he said' deadlock. With regard to end results, all we know for sure at this point is that a letter mailed from Ternate on 9 March 1858 *could* have reached England by 2 June 1858, because one (the Bates letter) actually *did*. Van Wyhe and those who accept his interpretation hold that Wallace's materials must have been sent from Ternate in April 1858, not March, and present a mail route scenario for Darwin's receiving them within a day of his letter to Lyell. Those who follow Davies defend the March 1858 mailing, arguing that Darwin must have received Wallace's package at about the same time the

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Frederick Bates letter was received, and that Darwin was up to no good in the weeks that followed (i.e. just before he contacted Lyell).

It should be noted, however, that the mail *itself* went through about a dozen handling operations, as boxed batches and as individually sorted pieces. Although, as Davies notes, most of these operations involved transfers of sealed boxes, this does not absolutely guarantee error-free results, especially if the Bates-bound letter found itself placed in a different box at an early stage.<sup>4</sup> Hand sorting would have taken place at least at Ternate itself, then Surabaja and finally London. So, Davies's objections notwithstanding, there were still opportunities for temporary misdirectings that would only have been righted at the next handling destination. A resulting two weeks' delay, the period between Bates's reception of his letter on 2 June and Darwin's note to Lyell on the 18th, thus looms as a realistic possible result.

Moreover, this all assumes that Wallace's letter and manuscript went out at Ternate with the mail on 9 March. Davies claims it did, on the same day that Wallace would have received a letter from Darwin stating that others, including Sir Charles Lyell, had taken positive notice of his work, especially the 1855 'Sarawak essay'.<sup>5</sup> Van Wyhe counters that evidence suggests Wallace never responded to mail on the same day he received it, and that Wallace must have sent the letter and manuscript out by the next mail, on 5 April 1858. Both writers agree that Wallace's letter and manuscript must have been a reply to Darwin's letter, because before this time he would not have been aware of Lyell's enthusiasm for his work and would not have requested the materials be sent to him if Darwin thought them relevant.

There is, however, a big problem with this surmise: it is all based on some words that Wallace wrote on page 363 of volume 1 of his autobiography, *My life*,<sup>6</sup> in 1905: 'I asked [Darwin] if he thought it sufficiently important to show it to Sir Charles Lyell, who had thought so highly of my former paper.' Peter Raby<sup>7</sup> draws attention to this sentence in his biography of Wallace, claiming it as proof of Wallace's knowledge of Lyell's support, and therefore as an impetus for Wallace to send his materials to Darwin and Lyell at that juncture. This 'evidence' has been used as proof of the 'reply' interpretation ever since.

But no confirmation of this interpretation seems to exist. Neither Wallace nor any of the other main actors in the story (Darwin, Lyell, Hooker, and so on) seem ever to have referred to Wallace's letter and manuscript as a reply to the Darwin letter that arrived in Ternate on 9 March 1858 (although it is not clear that anyone other than Wallace would have been able to make that decision, depending on just how brief Wallace's accompanying note was). And, in any case, this 'evidence' itself should be fully dismissed, as follows.

In 1906 the American philosopher Charles Peirce placed a review of Wallace's autobiography in *The Nation*.<sup>8</sup> In this review he noted:

The vestiges of some knowledge of Latin still appear, now and then, in his sentences, especially in constructions that are bad in a language [i.e. English] in which the order of succession of the words is the only clue [at this point Peirce enters a footnote giving a particularly egregious example in *My Life* of this tendency in Wallace's writing]... More than once in this book he deploras an incapacity for language which he attributes to himself. But, as to this, it is necessary to distinguish between a natural incapacity and early want of facility due to one's self-communions not having been such as to exercise one's faculty.

Otherwise put, Wallace was prone to constructing long compound sentences based on independently occurring events (seemingly in an effort to 'condense' his historical recollections).

Before returning to Wallace's sentence on page 363 of *My life*, we must look at his words on page 355 of chapter 21 addressing his writing of the 'Sarawak law' essay in 1855. After describing its actual writing, he says:

Soon after this article appeared, Mr. Stevens wrote me that he had heard several naturalists express regret that I was 'theorizing,' when what we had to do was to collect more facts. After this, I had in a letter to Darwin expressed surprise that no notice appeared to have been taken of my paper, to which he replied that both Sir Charles Lyell and Mr. Edward Blyth, two very good men, specially called his attention to it.

Chapter 21 ends two sentences later. Chapter 22 begins on page 356 and slowly builds up, through several pages, to a discussion of his discovery of the principle of natural selection. In this discussion Lyell is not mentioned until page 363, and our subject sentence. Recalling Peirce's complaints, it seems clear to me that this sentence has nothing to do with the contents of the letter that Wallace received from Darwin on 9 March, and everything to do with reminding his readers in 1905 of just how Lyell fitted into the story.

One might object that Wallace must have known of Lyell's support, as he otherwise would never have tried to contact him, great figure that he was. I find this argument unconvincing, on several counts. First, Wallace was already a known figure, having presented several papers before prominent bodies (the Zoological Society, the Entomological Society and the Royal Geographical Society), published more than a dozen technical writings, and written two books; he also had at least some friends in high places, including William Hooker, Roderick Murchison and James Brooke, all of whom had been knighted years previously. Further, he had a history of this kind of behaviour, having 15 years earlier (at the age of 20 years, with no publication record at the time) sent an essay on telescope lens construction to Fox Talbot, the leading figure in England in the development of the field of photography.<sup>9</sup> One should also remember his aside in his autobiography to the effect that so long as he felt he understood the basic elements of a given question, he never shied away from taking on even the most celebrated minds. His targets would, in fact, include the likes of Darwin, Lyell, John Stuart Mill, George Bernard Shaw, Herbert Spencer, Edward Drinker Cope, Alfred Marshall, Karl Marx, Andrew Carnegie and Edward Tylor, to name just a few.

More importantly, perhaps, and irrespective of any such information about Lyell's support, Wallace had a real reason for trying to contact him at just that time.<sup>10</sup> Wallace's expedition to the Aru Islands had ended in July 1857, and he used his discoveries there as the basis for a challenge to Lyellian biogeographical principles in an article entitled 'On the natural history of the Aru Islands',<sup>11</sup> the first of three papers he wrote on the natural history of that archipelago. This was published in December 1857, in a year-end supplement to volume 20 of *Annals and Magazine of Natural History*. Even if it was actually ready somewhat earlier (either in print or as proofs), say in November, Wallace could not possibly have known of its release before January, or more probably February, 1858. Within weeks he had had his epiphany on natural selection, and now, armed with this specific model to back up the general arguments posed in 'On the natural history of the Aru Islands', he would have been eager to get Lyell's thoughts. Even had Darwin not been in the picture, he might ultimately have tried to contact him.

Significantly, however, Wallace had found a way to assuage his curiosity without actually attempting to contact Lyell *directly*. His earlier exchange with Darwin had paved the way for this; he would now leave it in Darwin's hands as to whether such a forwarding should take

place. In all likelihood Wallace deposited the letter and manuscript in the mail sometime between 2 March, the date of his letter to Frederick Bates (in which he mentions having 'lately worked out a theory' to account for colour variations in species he was collecting), and 9 March. He very probably did not see Darwin's incoming letter until his own materials were long out of his hands. There was no need to respond further; what he had already put in the mail was self-explanatory.

Other examples of Wallace's tendency to pull together disparate pieces of information into single sentences may be found in Smith,<sup>12</sup> in the five places in which Wallace discusses his issuance of the Ternate letter and manuscript and their fate. More can be found in sources published throughout Wallace's literary career. If we again recall Peirce's observations, Wallace's occasional tendency to draw together separate events may be most reasonably linked to his writing style as opposed to faulty memory, as van Wyhe in particular has claimed on several occasions. For example, in his latest book<sup>13</sup> van Wyhe, citing earlier-stated suspicions by Secord,<sup>14</sup> discounts Wallace's memory of having heard, in his teens, Robert Owen speak in London—despite the evidence presented by Claeys,<sup>15</sup> which fully documents Owen's appearances at the times, if not *exactly* the place, that Wallace mentions. Van Wyhe also complains that Wallace reports reading the tract 'Consistency', by Robert Dale Owen, in 1837 (it was not published until 1840 or so), but Wallace does not actually cite a particular year in his account. Along these lines it is also very difficult to believe that Wallace, on five occasions over a period of nearly 40 years, could have reported the remembrance that he had sent out the letter and manuscript 'a few days later' and/or 'in the next mail' if indeed the sending had been several weeks later.

As a final point in this direction, it should be noted that the remark about Lyell is absent from the report of the events Wallace included in the then most recent of his remembrances published before his autobiography's account<sup>16</sup>—which otherwise largely seems to be based on it.

It has also been suggested to me that Darwin's 18 June letter to Lyell contains evidence of the 'reply' theory to the extent that he mentions how in his previous letter to Wallace he had spoken to him of Lyell's approval. But regardless of the exact recent chain of events—that is, whether Wallace's letter *was* a reply; whether Darwin thought it was, but it was not; or whether Darwin did not think it was—it seems likely that Darwin would have brought up this highly relevant fact in his letter to Lyell. Thus Darwin's words to Lyell in this context provide us with little of value.

Another possible issue concerns the fact that the Ternate essay had a different point to make from either his Sarawak essay or his application of its thoughts in the Aru work. If so, why would he have been so interested in sending it to Lyell? Could one somehow argue that the Ternate essay was *specifically* 'aimed at Lyell', and sent to him for that reason—or conversely argue that because it was not, why did he send it at all? I do not think so, in either case; mainly, it seems to have been constructed through a largely separate thought process dwelling on biological, not biogeographical, considerations. Nevertheless, its application to biogeography would have been apparent immediately to Wallace, as it could be used to understand the fact of analogous adaptations having arisen in different places, a process that Lyell put off on creationism. Wallace first discusses this matter in an aside in *Travels on the Amazon and Rio Negro*<sup>17</sup> in 1853, but he had probably been considering the issue as far back as 1846 and his reading<sup>18</sup> of Meyen's *Outlines of the Geography of Plants*, which presents a pointed question in this direction.<sup>19</sup> With a transmutation model that could be used to explain these biogeographical particulars, Wallace, right after criticizing Lyell in print, and now with some new

ammunition, would probably have been very interested in receiving some feedback from him.

Until we are graced with further information, to me the most likely sequence of events runs as follows. In January or February 1858 Wallace receives notice that his Aru Islands paper has been published. Within a month or so the theory of natural selection is revealed to him, and this, together with his eagerness to query Lyell, causes him to write to Darwin, now an exploitable contact, in the hope that his thoughts might be relayed to the older naturalist for comment. Wallace sends this letter, together with the manuscript, out in the 9 March mail, its issuance preceding his reception of the incoming message from Darwin. The mail proceeds towards England, perhaps being delayed along the way by an accident of handling, arriving in that instance on 17 June or thereabouts, or in the other about 2 June, and leaving Darwin either a villain or a victim of historical coincidence. Thus I would argue as follows: (i) the Davies model is correct insofar as the mailing date (9 March) goes, but for the wrong reasons; (ii) Davies and his supporters are possibly wrong that Wallace's materials arrived at the same time the Bates letter did; (iii) the van Wyhe camp's mail route reconstruction is probably beside the point; however, (iv) their surmise of the 17 June reception date may turn out to be correct.

It seems to me improbable, moreover, that Darwin stole ideas from the materials that Wallace sent him, given that (i) accounts of Darwin seem to reveal a man who, if rather stingy with his credits, lived a life largely free of acts of bad character, and, perhaps more importantly, (ii) it is difficult to believe that Darwin would have taken the chance that Wallace had not made a copy of his letter and manuscript (and in fact he had, as Wallace stated in a letter to Adolf Bernhard Meyer in 1869<sup>20</sup>) that later could be used to settle issues of priority.

Still, it must be admitted that this is not the end of the matter. It is still possible that Darwin received Wallace's mail in early June, lying about this to Lyell after somehow determining that the next mail had just arrived from Indonesia. It is also unlikely, though still possible (the chances being something like a not-impossible 1 in 15), that it was coincidental that Darwin chose to write to Lyell at just about the same time as the next mail came in. Regarding the intellectual theft question and the materials that Darwin added to his 'big book' manuscript during June, will we ever receive a full-scale content analysis of these that settles the issue? Meanwhile, in a new work, Davies<sup>21</sup> has raised the interesting possibility that Darwin might even have lifted material from some of Wallace's pre-1858 writings.

Finally, we should not entirely dismiss the possibility that Darwin *did* receive Wallace's materials on 2 June but then simply sat on them for two weeks, doing nothing as he agonized over the situation (and in embarrassment telling a white lie to Lyell in the 18 June letter: 'He has to day sent me . . .'). Further, could he actually have *written* the letter to Lyell on 2 June without dating and sending it just then (thus accounting for the 'to day' comment)?

In sum, our knowledge that Frederick Bates received his letter from Wallace in early June no more proves that Darwin received Wallace's materials at the same time than does van Wyhe & Rookmaaker's reconstruction of mailing routes and assumption of a later mailing date prove that Darwin received them in mid-June.

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## NOTES

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- 3 Van Wyhe, *op. cit.* (note 1); Van Wyhe and Rookmaaker, *op. cit.* (note 1); Davies (2008), *op. cit.* (note 1); A. C. Brackman, *A delicate arrangement* (Times Books, New York, 1980); J. L. Brooks, *Just before the origin: Alfred Russel Wallace's theory of evolution* (Columbia University Press, New York, 1984).
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- 17 A. R. Wallace, *A Narrative of Travels on the Amazon and Rio Negro* (Ward Lock, London, 1889), at pp. 58–59.
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- 19 F. J. F. Meyen, *Outlines of the Geography of Plants* (Ray Society, London, 1846). On page 99 of this book Meyen writes: 'We have now learned some of the external causes which place the more developed and nobler forms of vegetation in the hot zones; but we know no cause, why the same species of plants are not always produced in the same conditions of climate.' Wallace is listed at the end of this work as a subscriber.
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- 21 Davies (2013), *op. cit.* (note 1).