

2013

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## Recommended Repository Citation

Smith, Charles H.. (2013). A Further Look at the 1858 Wallace-Darwin Mail Delivery Question. *Biological Journal of the Linnean Society*, 108, 715-718.

**Original Publication URL:** <http://www3.interscience.wiley.com>

**Available at:** [http://digitalcommons.wku.edu/dlps\\_fac\\_pub/63](http://digitalcommons.wku.edu/dlps_fac_pub/63)

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# A Further Look at the 1858 Wallace–Darwin Mail Delivery Question

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**[[ Accepted for publication 17 September 2012. This is a preprint of the work that appeared in the *Biological Journal of the Linnean Society* 108: 715-718 (Wiley-Blackwell, for the Linnean Society of London, 2013). The definitive version is available at <http://www3.interscience.wiley.com> ]]**

**Abstract:** Recent investigations have led to a conclusion that Alfred Russel Wallace probably mailed his ‘Ternate’ paper on natural selection to Darwin a month later than some have thought, thus freeing Darwin from possible accusations of plagiarism. Further examination of the question suggests this conclusion is premature, as the evidence in favor of the later mailing date appears to be shakier than first thought.

**Additional Keywords:** natural selection, correspondence, Charles Lyell

Two recent essays (van Wyhe and Rookmaaker, 2012; Davies, 2012) have again stirred up the controversy about whether Charles Darwin might have received Alfred Russel Wallace’s 1858 ‘Ternate’ paper on natural selection earlier than Darwin acknowledged (18 June 1858). Those who believe in a ‘Darwin conspiracy’ suggest Wallace’s materials left Ternate on the 9 March 1858 mail boat; most of those who do not now argue that they left Ternate on 5 April of that year. I do not dispute here any of the mail route or exchange information described by these and earlier sources, but wish to point out a few things that appear to have been glossed over in the discussion.

To my knowledge, Wallace discussed the circumstances surrounding his natural selection epiphany on seven occasions that found their way into print, but in only five of these does he mention the mailing of his letter and manuscript to Darwin. The following are the relevant passages:

As soon as my ague fit was over I sat down, wrote out the article, copied it, and sent it off by the next post to Mr. Darwin. (Wallace, 1869/1895)

In the two hours that elapsed before my ague fit was over I had thought out almost the whole of the theory, and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin. Up to this time the only letters I had received from him [Actually, he apparently had also received an earlier note requesting he be on the lookout for specimens Darwin wanted (Darwin Online, CUL-DAR206.34-35).] were those printed in the second volume of his *Life and Letters*, (vol. ii. pp. 95 and 108), in which he speaks of its being the twentieth year since he ‘opened his first note-book on the question how and in what way do species and varieties differ from each other,’ and after referring to oceanic islands, the means of distribution of land-shells, etc., added: ‘My work, on which I have now been at work more

or less for twenty years, *will not fix or settle anything*; but I hope it will aid by giving a large collection of facts, with one definite end.' The words I have italicised, and the whole tone of his letters, led me to conclude that he had arrived at no definite view as to the origin of species, and I fully anticipated that my theory would be new to him, because it seemed to me to settle a great deal. (Wallace, 1891, pp. 20–21)

. . . in the two hours of my fit I had thought out the main points of the theory. That same evening I sketched out the draft of a paper; in the two succeeding evenings I wrote it out, and sent it by the next post to Mr. Darwin. I fully expected it would be as new to him as it was to myself, because he had informed me by letter that he was engaged on a work intended to show in what way species and varieties differ from each other, adding 'my work will not fix or settle anything.' (Wallace, 1898, p. 140)

So deeply impressed was I with the importance of this theory and of its far-reaching consequences, that the very same evening I sketched its outlines, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin, in the full expectation that it would be as new and startling a revelation to him as it had been to myself. I also asked him, if he thought well of it, to show it to Sir Charles Lyell, but I said nothing about its publication. (Wallace, 1903)

. . . I waited anxiously for the termination of my fit so that I might at once make notes for a paper on the subject. The same evening I did this pretty fully, and on the two succeeding evenings wrote it out carefully in order to send it to Darwin by the next post, which would leave in a day or two. I wrote a letter to him in which I said that I hoped the idea would be as new to him as it was to me, and that it would supply the missing factor to explain the origin of species. I asked him if he thought it sufficiently important to show it to Sir Charles Lyell, who had thought so highly of my former paper. (Wallace, 1905, p. 363)

The first thing to be noticed here is that in all five of these recollections, Wallace says nothing about there being a delay of a month in sending out his essay. In the last excerpt he even specifies that the next post would leave 'in a day or two.' Clearly, he would have wanted to send it out right away, and it is very difficult to believe that he would have forgotten – five times – to mention a delay in later recalling what was surely the most important event in his life.

Van Wyhe and Rookmaaker argue, however, after Raby (2001), that because the third letter Darwin sent Wallace (dated 22 December 1857) almost certainly arrived on the 9 March boat, Wallace would not have had time or opportunity to write up a letter responding to it for posting on the same boat (they also note finding 'from Wallace's surviving correspondence from Ternate that he never replied to a letter by the same mail boat on which it arrived'). They use as corroborative evidence Wallace's statement above that 'I asked him if he thought it sufficiently important to show it to Sir Charles Lyell, who had thought so highly of my former paper,' because it seemingly indicates Wallace's knowledge of Lyell's support, which he could not have known of before this. One might add to this evidence the Darwin quotations in the second and third selections above, as some of these words appeared in the third letter Darwin sent to Wallace, not the second.

The available evidence regarding the postal procedures in Ternate at that time seems to indicate that Wallace *probably* would not have had time or opportunity to produce a same-day turn around, but it hardly seems that either Davies' or van Wyhe and Rookmaaker's interpretations are airtight. Critically, moreover, I believe the remarks about Lyell should be discounted as satisfactory evidence that Wallace's letter and manuscript did not leave Ternate on 9 March. Regarding the *My Life* recollection in particular, it is more likely that the words 'who had thought so highly of my former paper' were written for the benefit of the reader of the autobiography, and not as an explanation of why he sent the letter and

manuscript when he did. Several pages before these words (on page 355 of volume one of *My Life*), in discussing the writing of his earlier milestone work ‘On the law which has regulated the introduction of new species’ (Wallace 1855), Wallace had just described how Darwin told him (in his third letter to Wallace) how Lyell was one of ‘two very good men’ who had ‘specially called his attention’ to this work. The more one looks at this sequence, the more it seems the words on page 363 were just a literary device reminding the reader of 1905 who was who, and how Lyell was related to the story. This would not be the first time that Wallace conjoined thoughts in an awkward fashion (see below).

Note also that the 1903 recollection given above, on which the later *My Life* one seems to be based, mentions Lyell but does not include the ‘who had thought so highly...’ passage. The 1903 essay does not refer to any connection between Lyell and the ‘On the law...’ paper, and this supports the surmise that the 1905 passage included a purpose specific to *My Life*.

Further, it should be emphasized that it was likely Lyell’s opinion, not Darwin’s, that Wallace really sought. Although he had written to Darwin three times and met him in person once, he had never been in contact with Lyell. Later, Wallace refers to his reasons for first writing Darwin: ‘I must have heard from some notices in the *Athenæum*, I think (which I had sent me), that he was studying varieties and species, and as I was continually thinking of the subject, I wrote to him giving some of my notions, and making some suggestions’ (Wallace, 1887/1892). His reasons for getting Lyell’s opinion were much stronger at the time. Consider two of his later writings on this:

. . . having also read through . . . books . . . giving a mass of facts as to the distribution of animals over the whole world, it occurred to me that these facts had never been properly utilized as indications of the way in which species had come into existence. The great work of Lyell had furnished me with the main features of the succession of species in time, and by combining the two I thought that some valuable conclusions might be reached. I accordingly put my facts and ideas on paper, and the result seeming to me to be of some importance, I sent it to *The Annals and Magazine of Natural History*, in which it appeared in the following September (Wallace 1905, p 355).

. . . But along with Malthus I had read, and been even more deeply impressed by, Sir Charles Lyell’s immortal ‘Principles of Geology,’ which had taught me that the inorganic world – the whole surface of the earth, its seas and lands, its mountains and valleys, its rivers and lakes, and every detail of its climatic conditions, were and always had been in a continual state of slow modification. Hence it became obvious that the forms of life must have become continually adjusted to these changed conditions in order to survive. The succession of fossil remains throughout the whole geological series of rocks is the record of this change; and it became easy to see that the extreme slowness of these changes was such as to allow ample opportunity for the continuous automatic adjustment of the organic to the inorganic world, as well as of each organism to every other organism in the same area, by the simple processes of ‘variation and survival of the fittest.’ Thus was the fundamental idea of the ‘origin of species’ logically formulated from the consideration of a series of well-ascertained facts (Wallace, 1909, p. 118).

Importantly, Wallace was undoubtedly especially eager to have Lyell weigh in at that exact time, as he had just written a stinging criticism of his ideas in his most recent major paper, ‘On the natural history of the Aru Islands,’ published only a few months earlier:

. . . We know (with a degree of knowledge approaching to certainty) that at a comparatively recent geological period, not one single species of the present organic world was in existence; while all the *Vertebrata* now

existing have had their origin still more recently. How do we account for the places where they came into existence? Why are not the same species found in the same climates all over the world? The general explanation given is, that as the ancient species became extinct, new ones were created in each country or district, adapted to the physical conditions of that district. Sir C. Lyell, who has written more fully, and with more ability, on this subject than most naturalists, adopts this view. He illustrates it by speculating on the vast physical changes that might be effected in North Africa by the upheaval of a chain of mountains in the Sahara. 'Then,' he says, 'the animals and plants of Northern Africa would disappear, and the region would gradually become fitted for the reception of a population of species *perfectly dissimilar in their forms, habits, and organization.*' Now this theory implies, that we shall find a general similarity in the productions of countries which resemble each other in climate and general aspect, while there shall be a complete dissimilarity between those which are totally opposed in these respects. (Wallace, 1857, p. 480)

Wallace continues with a discussion of how this theory does not measure up to the facts. Wallace did not comment in print on any of Darwin's ideas for another two years, in his work 'On the zoological geography of the Malay Archipelago' (Wallace 1860).

Having already corresponded with Darwin, Wallace probably thought this was the easiest way to get to Lyell, at that point one of the most famous naturalists in the world. Importantly, Wallace was not afraid of being forward – witness his out-of-the-blue communication twelve years earlier to Fox Talbot (Smith, 2006), then the most famous photographer in England (neither should we forget the passages in his autobiography in which he describes how he never shied away from debate once he felt he had grasped the fundamentals of a particular issue) – but here a gentler route was open. There appears to be no further evidence that the letter accompanying Wallace's manuscript was in any sense a 'reply' to the Darwin letter that arrived in Ternate on 9 March; *i.e.*, as far as I am aware, nowhere does Wallace actually say that he sent his materials to Darwin on that occasion *because* of anything written in the arriving letter (though see below). This will also explain the minor puzzle of why Wallace apparently never responded directly to Darwin's third letter in a manner comparable to his earlier reply; that is, by discussing a number of matters Darwin raised. According to van Wyhe and Rookmaaker's scenario, Wallace would now have had an extra full month to compose a reply, yet the only matters now known to have been treated in Wallace's third letter, the one accompanying the manuscript, are mentioned in the five excerpts given earlier. I suggest that on reading Darwin's 22 December 1857 letter, if just after sending out the manuscript the same day, Wallace would have learned of Lyell's support, and felt comfortable with his communication. No further response was warranted for the time being.

This leaves the second and third sets of remarks quoted at the top. In the third, especially in the sentence beginning 'I fully expected it would be as new to him . . .', Wallace is guilty of sloppy writing. The middle part of the sentence ('. . . engaged on a work intended to show in what way species and varieties differ from each other . . .'), refers to remarks in the *second* letter Darwin sent to Wallace, whereas the end section ('. . . adding 'my work will not fix or settle anything') comes from the *third* Darwin letter. One cannot use this as evidence of anything; it is simply a brief and unfortunate combining of separate events in the name of literary expediency. I would argue that he did pretty much the same thing in the 1905 passage.

The second set of remarks listed above is the most contentious for my interpretation. Here Wallace makes the same mistake just noted in combining remarks from the two Darwin letters into a single thought, in this case, however, linking his own actions to the third letter. But it is difficult to say, given

his other confusions, whether this is historically accurate or, again, just him remembering the whole affair as a package deal thirty-plus years on. (And his remarks in the second sentence in the excerpt, implying he wrote the essay *after* receiving the two later Darwin letters, further confirms this.)

Obviously, there were some problems with Wallace's memory. Yet in his 1887 letter to Newton he recounts nearly the same story as he did to Meyer in 1869, and in Meyer's reprint of this in 1895 notes how he had "entirely forgotten" he ever sent anything to Meyer. Thus, perhaps his memory of the original event, at least, was not so bad at that.

I advance, therefore, that the third Darwin letter may well be irrelevant to the matter of which day Wallace sent out his materials from Ternate. Meanwhile, however, there remain complications, notably: When *exactly* did Wallace write the essay (early February? late February? earliest March?), so when would the 'next mail' have been? Van Wyhe and Rookmaaker (2012) notwithstanding, could Wallace have managed a same-day turn around, as Davies (2012) claims? How much can we trust Wallace's recollections, and are some (the less complex ones) likely to be more accurate than others? And even assuming that a letter mailed from Ternate on 5 April 1858 *could* have reached Darwin on 18 April of that year, was there a way he could have found this out without actually receiving a letter? I believe it is premature to claim that this matter has been put to rest, and indeed it may never be.

## Acknowledgments

My thanks to George Beccaloni, Peter Raby, Roy Davies, and John van Wyhe for their remarks on an earlier draft of this work, and to an anonymous reviewer for his or her comments.

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