Why Did Wallace Write To Darwin?

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The answer to this question has been sought since 1858. In fact, the answer was given in 1905, but it has lain almost unrecognized since then. Pantin (1959), Woodcock (1969), Eiseley (1979), and Bowlby (1990, citing Pantin) have hinted at it, but none of them seems to have recognized its significance. Except for Raby (2001), Shermer (2002), Mallet (2008), and more briefly Slotten (2004), this is also true for the most recent Darwin and Wallace biographers, whose works proliferated during the 2009 Darwin bicentennial year.

Background

A decade ago, I was invited to visit Alfred John Russel Wallace and Richard Russel Wallace, Alfred Russel Wallace’s grandsons, in Hampshire. They had contacted Cambridge University Library for advice on the archiving of the remaining memorabilia of their grandfather in their possession. (These were sold to the Natural History Museum in 2002 (Beccaloni, 2009)). Thus Adam Perkins, Curator of Scientific Manuscripts at the Library, Dr. Samantha Evans, an Assistant Editor with The Darwin Correspondence Project, and I, then Director of the Project, drove down from Cambridge. We spent a pleasant afternoon with the Wallace brothers and their wives, during which we diligently looked through several boxes of papers and letters. In one of them, I found a few letters, which I read. One of the letters was from Wallace, written 4 January 1858 to his friend and fellow collector of natural history specimens, Henry Walter Bates. Bates was still in South America, where he and Wallace had collected natural history specimens from 1848 to 1852. There was also an annotated copy of the so-called “joint paper” of Wallace and Charles Darwin announcing natural selection (Darwin & Wallace, 1858). Wallace’s letter to Bates generated the present paper.

On his return from the momentous second voyage (1831-1835) of HMS Beagle to South America (and then around the world), Darwin, naturalist and gentleman-companion to captain Robert FitzRoy, began a series of notebooks that listed, abstracted, and discussed his readings and musings on the subject of transmutation (Barrett et al., 1987). Darwin’s personal voyage to this point has been well documented (Sulloway, 1982; Browne, 1995; Keynes, 2002).

We also now know with whom Darwin discussed transmutation and his own explanation as to how species formation took place, natural selection (Porter, 1993, table 2). For his geology mentor, Sir Charles Lyell, these discussions took place in 1838 and probably 1844, respectively. For his best friend and confidant, the botanist Joseph Dalton Hooker, they were in 1844 for both. Darwin wrote to Hooker on 10 September 1845, following a letter to him in which Hooker criticized Darwin’s views on species. Hooker criticized Darwin for failing to offer a "natural explanation of the variety and diversity of species". When Hooker demurred, Darwin wrote again on 18
September 1845: “All which you so kindly say about my species work does not alter one iota my long self-acknowledged presumption in accumulating facts & speculating on the subject of variation, without having worked out my due share of species.” (Burkhardt & Smith, 1987, pp. 253, 256). The next year, Darwin began a study of the barnacles that he collected on the Beagle voyage, which turned into an eight-year systematic survey of the Cirripedia (Darwin, 1851a, 1851b, 1854a, 1854b). Darwin’s sensitivity to Hooker’s remarks on species clearly played a role in his writing the barnacle monographs.

In September 1855, Wallace published a paper that indicated he was studying the evolution of new species, “On the law which has regulated the introduction of new species” (Wallace, 1855), which he had written that February in Sarawak. It was the result of a number of years of field studies and observation of animals and plants in tropical South America and the East Indies, beginning in 1848. Wallace wrote that 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.” His conclusion was that, “Every species has come into existence coincident both in space and time with a pre-existing closely allied species.” (Wallace, 1855, pp. 185, 196). Darwin wrote on the margin of the paper in his copy of the journal “Laws of Geograph. Distrib. nothing very new —” (Burkhardt & Smith, 1989, p. 522).

On the other hand, upon reading Wallace’s paper on 26 November 1855, “This seems to have struck Lyell so forcibly that he entered some notes on it in the first of the series of seven notebooks that he was to devote to the species question and that are published here.” (Wilson, 1970, p. xli). Lyell visited Darwin 13-16 April 1856, and his notebook entry for 16 April begins: “With Darwin: On the Formation of Species by Natural Selection — (Origin Query?)”. It concludes: “The reason why Mr. Wallace [s] introduction of species, most allied to those immediately preceding in Time, or that new species was in each geol. period [p. 139] akin to species of the period immediately antecedent, seems explained by the Natural Selection Theory.” (Wilson, 1970, pp. 54, 55). This conversation with Lyell engendered Darwin’s letter to Hooker of 9 May 1856 asking for advice: “… I very much want advice & truthful consolation if you can give it. I had good talk with Lyell about my species work, & he urges me strongly to publish something.” Hooker’s answering letter has not been found, but Darwin wrote in his journal for 1856, “May 14th Began by Lyell’s advice writing species sketch. —” (Burkhardt & Smith, 1990, pp. 106, 522). By June 1858 his “sketch” had become a hefty manuscript of 11 chapters. The first two chapters served as the basis for Variation under Domestication (Darwin, 1868), the remainder were not published until over 100 years later (Stauffer, 1975).

Edward Blyth, curator of the museum of the Asiatic Society of Bengal, a correspondent who supplied Darwin with much information about Asian animals, wrote to him on 8 December 1855: “… What think you of Wallace’s paper in the Ann. M. N. H.? Good! Upon the whole! … Wallace has, I think, put the matter well; and according to his theory, the various domestic races of animals have been fairly developed into species.” (Burkhardt & Smith, 1989, p. 519). So Lyell was not the only one who was aware of the importance of Wallace’s paper to evolution.
After completing the barnacle monographs, Darwin had returned to gathering data on transmutation by writing letters to naturalists around the world, asking for information about variation in native and introduced species. A memorandum written by him in December 1855 begins, “Skins Any domestic breed or race, of Poultry, Pigeons, Rabbits, Cats, & even dogs, if not too large, which has been bred for many generations in any little visited region, would be of great value, or even if recently imported from any unfrequented region.” On the back is pasted a list of over 30 naturalists to whom this query was sent. It is titled “I have written to for Pigeon & Poultry Skins”; fourth on the list is “E. Blyth”, thirteenth is “R. Wallace” (Burkhardt & Smith, 1989, p. 510). On 21 August 1856, Wallace wrote to his London agent Samuel Stevens that his latest shipment of specimens contained some for Darwin: “The domestic duck var. is for Mr. Darwin & he would perhaps also like the jungle cock, which is often domesticated here & is doubtless one of the originals of the domestic breed of poultry.” (Burkhardt & Smith, 1990, p. 290).

Wallace and Darwin

Wallace wrote to Darwin on 10 October 1856, but unfortunately this letter has not been found. However, it is known because of Darwin’s answer to it of 1 May 1857:

I am much obliged for your letter of Oct. 10th from Celebes received a few days ago: in a laborious undertaking sympathy is a valuable & real encouragement. By your letter & even still more by your paper in Annals, a year or more ago [Wallace, 1855], I can plainly see that we have thought much alike & to a certain extent have come to similar conclusions. In regard to the Paper in Annals, I agree to the truth of almost every word of your paper; & I daresay that you will agree with me that it is very rare to find oneself agreeing pretty closely with any theoretical paper; for it is lamentable how each man draws his own different conclusions from the very same fact. –

This summer will make the 20th year (!) since I opened my first note-book, on the question how & in what way do species & varieties differ from each other. — I am now preparing my work for publication, but I find the subject so very large, that though I have written many chapters, I do not suppose I shall go to press for two years.—

[Burkhardt & Smith, 1990, p. 387]

In his now partially lost answer of 27 September 1857, Wallace states that,

… of May last, that my views on the order of succession of species were in accordance with your own, for I had begun to be a little disappointed that my paper had neither excited discussion nor even elicited opposition. The mere statement & illustration of the theory in that paper is of course but preliminary to an attempt at a detailed proof of it, the plan of which I have arranged, & in part written, but which of course requires much research in English> [editorial addition] libraries & collections, a labour which I look….

[Burkhardt & Smith, 1990, p. 457]

Darwin answered on 22 December 1857:

I thank you for your letter of Sept. 27th.— I am extremely glad to hear that you are attending to distribution in accordance with theoretical ideas. I am a firm believer, that without speculation there is no good & original observation. … You say that you have been somewhat surprised at no notice having been taken of your paper in the Annals: I cannot say that I am; for so very few naturalists care for anything beyond the mere description of species. But you must not suppose that your paper has not been attended to: two very good men, Sir C. Lyell & Mr. E. Blyth at Calcutta specially called my
attention to it. Though agreeing with you on your conclusion in that paper, I believe I go much further than you; but it is too long a subject to enter on my speculative notions.— [Burkhardt & Smith, 1990, p. 514]

Wallace’s next letter, in response to this one, has not been found. It was sent in March 1858 and contained Wallace’s manuscript that was published as part of the “joint paper” (Darwin & Wallace, 1858). The story of how Lyell and Hooker arranged for its publication along with notes from Darwin’s 1844 essay on transmutation (F. Darwin, 1909) and an enclosure with a letter of 5 September 1857 to the Harvard botanist Asa Gray (Burkhardt & Smith, 1990, pp. 447-449) to establish Darwin’s priority is well known (e.g. Browne, 2002). Only Darwin’s letter of 1 May 1857 had previously been published (F. Darwin, 1887, 2: 95-96, and many subsequent researchers) before these letters quoted above appeared in The Correspondence of Charles Darwin in 1990.

Wallace and Bates

Wallace and Bates met in Leicester in 1844, where Wallace was teaching in a school and Bates was an apprentice in his father’s hosiery factory. They shared an interest in entomology and spent much time together collecting beetles and other insects. They also shared an interest in evolution through reading Vestiges of the Natural History of Creation ([Chambers], 1844), a popular, though flawed, treatment of the origin and evolution of life. Bates (1863, p. iii) described what happened next:

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.

In late 1847 or early 1848, Wallace wrote to Bates in Leicester from London after visiting the insect-room at the British Museum that, “I should like to take some one family to study thoroughly, principally with a view to the theory of the origin of species. By that means I am strongly of opinion that some definite results might be arrived at.” (Wallace, 1905, 1: 256). So Darwin was not the only one searching for the answer to how species originated.

The answer to the question posed by the title of this paper is given in the letter to Bates of 4 January 1858, which I found in Wallace’s grandsons’ cache. It followed Wallace’s receipt of Darwin’s letter of 1 May 1857:

I have been much gratified by a letter from Darwin, in which he says that he agrees with ‘almost every word’ of my paper [Wallace, 1855]. He is now preparing his great work on ‘Species and Varieties,’ for which he has been collecting materials twenty years. He may save me the trouble of writing more on my hypothesis, by proving that there is no difference in nature between the origin of species and of varieties; or he may give me trouble by arriving at another conclusion; but at all events, his facts will be given for me to work upon. Your collections and my own will furnish most valuable material to illustrate and prove the universal applicability of the hypothesis. The connection between the succession of affinities and the geographical distribution of a group, worked out species by species, has never yet been shown as we shall be able to show it. [Wallace, 1905, 1: 358]
Conclusion

Upon receipt of Wallace’s manuscript, Darwin then went on, intending to write a 30 page abstract of his ideas on natural selection for the *Journal of the Linnean Society (Zoology)*. This culminated over a year later in the almost 500 pages of *On the Origin of Species* (Darwin 1859), which he considered to be an abstract of his “Big Book”. We have seen that Darwin later used the first two chapters of the “Big Book” as the basis for *Variation Under Domestication* (Darwin 1868). This was to be the first of three books that Darwin intended to write to provide the data for his statements in *Origin*, but it was the only one to be published. “In a second work, after treating of the Variation of organisms in a state of nature, of the Struggle for existence and the principle of Natural Selection, I shall discuss the difficulties which are opposed to the theory.” (Darwin, 1868, 1: 8). “In a third work I shall try the principle of natural selection by seeing how far it will give a fair explanation of the several classes of facts just alluded to.” These “several classes of facts” included “the geological succession of organic beings, their distribution in past and present times, and their mutual affinities and homologies.” (Darwin, 1868, 1: 9). There is no evidence that either of these two works were begun.

On 2 July 1866, Wallace wrote Darwin a letter in which he argued that a better term than Natural Selection to describe the evolutionary process would be Survival of the Fittest, coined two years before by the philosopher Herbert Spencer. Darwin disagreed, and answered on 5 July 1866 that, “The term Natural Selection has now been so largely used abroad & at home that I doubt whether it could be given up, & with all its faults I should be sorry to see the attempt made. Whether it will be rejected must now depend ‘on the survival of the fittest’.” (Burkhardt, et al. 2004, p. 236). Nevertheless, in the fifth edition of *Origin* Darwin (1869) did change the title of chapter IV from “Natural Selection” to “Natural Selection, or the Survival of the Fittest.” In Wallace’s copy of the “joint paper” mentioned above, he penciled a line through “Natural Selection” wherever it appeared in Darwin’s paper and wrote “Survival of the Fittest” in the margin. He did the same in his copy of the first edition of *Origin* now in the Keynes Room of Cambridge University Library. Curiously, Beccaloni (2008) does not refer to Wallace’s editorial additions to the copy of the “joint paper” he examined. Perhaps it was a different offprint than the one I saw.

Afterward

In the late twentieth century, several authors alleged that Darwin actually derived the principle of natural selection from the works of others, rather than having deduced it himself. The anthropologist, historian of science, and popular writer Loren Eiseley (1979) concluded that Darwin got the idea from two papers of Edward Blyth (Blyth, 1835, 1837). Eiseley stated that Darwin never cites these papers anywhere in his publications (Eiseley, 1979, p. 51). However, Blyth’s papers are quoted or cited in Darwin’s notebooks (Barrett, et al., 1987, pp. 261, 300, 301, 658, written in 1838) and the Big Species Book (Stauffer, 1975, pp. 323, 473, 592, 594, written in 1856-1858). They are cited for information on colouration and instinct in animals, not natural selection. Darwin did cite Blyth five times in *Origin* (Darwin, 1859), 48 times in *Variation under Domestication* (Darwin, 1868), and 51 times in *The Descent of Man* (Darwin, 1871). Furthermore, although Stauffer (1975) was published two years before
Eiseley’s death in 1977, it is not cited by him or his editor. Although Barrett, et. al (1987) was published well after Eiseley’s death, an earlier version (De Beer, 1960) was available and was not quoted or cited by Eiseley (1979, pp. 83, 91, 247, 249). De Beer (1960, p. 26) pointed out that some 50 pages were missing from Darwin’s first transmutation notebook. Eiseley (1979, p. 92) hypothesized that the missing pages “may have contained more detailed references to Blyth’s works. Since these pages compose the first part of the diary, their disappearance, taken with other evidence, cannot fail to hint of a genuinely ‘missing link’ in the story of natural selection.” The missing pages subsequently have been found in the Darwin Archive at Cambridge University Library and are restored in Barrett, et al. (1987). They do not contain any mention of Blyth.

More serious were the allegations of the journalist Arnold Brackman (1980) and the zoologist and scientific administrator John Brooks (1984). They claimed that Darwin had received Wallace’s letter of March 1858, which contained his manuscript “On the tendency of varieties to depart indefinitely from the original type”, earlier than 18 June 1858, the date he stated to Lyell that it arrived at Down House (Burkhardt & Smith, 1991, p. 107). According to Brackman and Brooks, this would have given Darwin ample time to use information from Wallace’s manuscript to augment his own. Brackman (1980, p. 17) alleged that it had arrived two weeks earlier than Darwin stated, about 3 June. This was the date of arrival in England of a letter from Wallace to Bates’ brother Frederick, dated 2 March 1858. Brooks’ (1984, 252-257) examination of the schedules of mail boats between the Dutch East Indies and England on which Wallace’s letter to Darwin apparently was carried indicated to him that the letter might have arrived as early as 28 or 29 May 1858. Brackman and Brooks both assumed that the two letters were sent the same day and travelled by the same route, perhaps in the same mailbag. The first assumption is plausible, the second is questionable, as the mail was sorted several times en route. I refereed Brooks’ book manuscript for Columbia University Press, concluding that it told a good story, but that it did not prove that Darwin had lied about when he received Wallace’s letter. Close examination of Wallace’s and Darwin’s manuscripts and letters shows little evidence that Darwin used Wallace’s 1858 manuscript, or any other, to augment his own (Kottler, 1985). Furthermore, although both Brackman and Brooks cited Wallace (1905) in their books, neither mentions his 4 January 1858 letter to Bates, which is the key to why Wallace wrote to Darwin.

Postscript

Wallace wrote to the Oxford University zoologist and evolutionist Professor Edward Poulton on 19 February 1895 that “As to your question about myself and Darwin, I had met him once only for a few minutes at the British Museum before I went to the East.” (Marchant, 1916, 2: 62). Brian Gardiner (1995, p. 13) points out that this was “in early 1854, shortly before he sailed for Singapore”. But the last word goes to his recent biographer Peter Raby: In preparation for his collecting trip to the East Indies, Wallace “spent long hours in the insect room of the British Museum. There, one day, he was introduced to another visitor, Charles Darwin, or so he recollected [41 years later]. The meeting, if it took place, made little impression on either.” (Raby, 2001, p. 01).
Acknowledgments

My colleagues Professors Richard Bambach, Peter Graham, Gren Lucas, and David West, and my wife Sarah, have kindly read and commented on this manuscript.

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