

Wallace's Annotated Copy of Darwin's *Origin of Species*

BARBARA G. BEDDALL

*Crosslands, Box 90
Kennett Square, Pennsylvania 19348*

On November 24, 1859, Charles Darwin's *On the Origin of Species* was published in London. The first printing was sold out to the book trade almost immediately, necessitating a second printing the following month, on December 28.¹ Darwin had been corresponding since May 1857 with the naturalist Alfred Russel Wallace, then traveling in the Malay Archipelago collecting birds and insects. A year later, in June–July 1858, had come the famous “joint papers,” precipitated by Darwin's receipt from Wallace of his paper “On the Tendency of Varieties to Depart Indefinitely from the Original Type,” read before the Linnean Society of London on July 1, along with contributions by Darwin.² This in turn hastened the publication of Darwin's *Origin*, as he noted in the introduction:

My work is now nearly finished; but as it will take me two or three more years to complete it, and as my health is far from strong, I have been urged to publish this Abstract. I have more especially been induced to do this, as Mr. Wallace, who is now studying the natural history of the Malay archipelago, has arrived at almost exactly the same general conclusions that I have on the origin of species.³

1. Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life* (London: Murray, 1859; facsimile reprint, Cambridge, Mass.: Harvard University Press, 1964).

2. The collective title for the joint papers and the accompanying letter is entered as follows: Charles Darwin and Alfred Russel Wallace, “On the Tendency of Species to Form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection,” *J. Linn. Soc. London (Zool.)*, 3 (1858), 45–62. There are many accounts of the details of this event, including that of the present author: Barbara G. Beddall, “Wallace, Darwin, and the Theory of Natural Selection: A Study in the Development of Ideas and Attitudes,” *J. Hist. Biol.*, 1 (1968), 299–311.

3. Darwin, *Origin* (1859), pp. 1–2.

On November 13, 1859, Darwin had written to Wallace that he had told Murray (the publisher)

to send you by Post (if possible) a copy of my Book & I hope that you will receive it at nearly same time with this note. . . . If you are so inclined, I sh^d very much like to hear your general impression of the Book as you have thought so profoundly on subject & in so nearly same channel with myself. I hope there will be some little new to you, but I fear not much. Remember it is only an abstract and very much condensed. God knows what the public will think. No one has read it, except Lyell, with whom I have had much correspondence.⁴

Some six months later, on May 18, 1860, a letter from Wallace arrived, and Darwin responded that he had

received this morning your letter from Amboyna dated Feb 16th, containing some remarks & your too high approbation of my book. Your letter has pleased me very much, & I most completely agree with you on the parts which are strongest & which are weakest; the imperfection of geolog. Record is, as you say, the weakest of all. . . .⁵

What other comments — besides “the necessary imperfection” of the geological record — Wallace’s letter may have contained, cannot now be known directly, as the letter itself has disappeared,⁶ but it was not the detailed critique planned by Wallace.

On November 30, 1861, by then on his way home, Wallace wrote to Darwin from Sumatra (in a letter still unpublished):

On an evening in the wet season in these central forests of Sumatra, I occupy myself in writing a few lines to you to say a few things which I may otherwise forget altogether. About a

4. James Marchant, *Alfred Russel Wallace: Letters and Reminiscences* (New York: Harper, 1916), p. 115; see also *The Life and Letters of Charles Darwin, Including an Autobiographical Chapter*, ed. Francis Darwin, 3 vols. (London: Murray, 1887), II, 220 (hereinafter cited as Darwin, *LL*). Letter # 2529 in the Darwin *Calendar*, quoted here from a copy of the original, is in the British Library, BL(Add 46434).

5. Marchant, *Wallace*, p. 117; Darwin, *LL*, II, 309. Letter # 2807 in the Darwin *Calendar*, quoted here from a copy of the original, is in the British Library, BL(Add 46434).

6. This continues to be a problem up to the present day. For a list of the missing material, see Beddall, “Wallace, Darwin,” pp. 319–323.

year & a half back [spring 1860] I wrote to you from somewhere Eastward of Ceram, with more digested remarks on your book, but the letter with one to my agent Mr. Stevens never I believe reached Amboyna. All I can remember of them now is to the effect that repeated perusals had made the whole clearer & more connected to me & the general & particular arguments clearer & more forcible than at first.⁷

Although Wallace's critique is also missing, a probable source for it is the copy of Darwin's *Origin of Species* sent to Wallace by Darwin's publisher Murray and carefully annotated by Wallace. Wallace later gave this copy to his old friend from South American days, the botanist Richard Spruce. There are three entries on the inscription page: reading from the top down, "Alfred R. Wallace," "From the Author," and "Ricardo Spruce from A. R. Wallace." The first and last are in Wallace's hand, while the second would have been written in by the publisher.

Wallace had first met Spruce at Santarém, on the Amazon River, in late October or early November 1849. Spruce had only recently arrived, having been sent to this region by several London botanists, among them Sir William Jackson Hooker and George Bentham.⁸ On January 2, 1864, shortly before Spruce's return to England, Wallace wrote to Darwin that he had "just had a long & interesting letter from my old companion Spruce. He says he has had a letter from you about *Melastoma*. . . . He had never been able to get a copy of your book, though I am sure no one would have enjoyed or appreciated it more."⁹

In the end, Wallace gave Spruce his own copy of the first edition sent to him by Murray.¹⁰ This copy is now part of the Keynes Collection, housed in the library at the University of Cambridge, as I was kindly informed by Quentin Keynes, and I am now in possession of copies of the pages on which Wallace made notes.

7. Letter # 3334, DAR (Darwin MSS, Cambridge University Library) 181, in the Darwin *Calendar*, from Wallace to Darwin, November 30, 1861, unpublished, here quoted from the original in the Cambridge University Library.

8. *Dictionary of National Biography*, s.v. "Spruce."

9. Marchant, *Wallace*, p. 124; letter # 4378, DAR 106/7 (ser. 2): 8–11, in the Darwin *Calendar*. Wallace later collected and published Spruce's notes under the title *Notes of a Botanist in the Amazon and Andes, Being Records of Travel on the Amazon & Its Tributaries*, ed. and condensed by A. R. Wallace, 2 vols. (London: Macmillan, 1902). See also Edward J. Goodman, *The Explorers of South America* (New York: Macmillan, 1972), pp. 286, 289–292.

10. Darwin regularly gave Wallace copies of new editions of the *Origin* as they were published. See Marchant, *Wallace*, pp. 122, 195, 200, 222.

Wallace marked passages on more than fifty pages, usually with written comments and with or without vertical lines in the margins. As codiscoverer of the theory of natural selection, he was one of the most sophisticated readers of Darwin's book, and his marks and comments are worth noting. **Darwin** and **Wallace** are shown below in boldface to indicate the particular passages from the *Origin* and Wallace's accompanying notations.

There is an apparent purposefulness in Wallace's annotations that is shown both by the carefully placed marginal lines and by the well-thought-out comments. It seems likely that this can be attributed to Wallace's interest in presenting a serious critique of Darwin's *Origin*, rather than just a "general impression." The handwriting throughout is Wallace's, all comments having been checked against the manuscripts of Wallace's still-unpublished notebooks.¹¹ Where they appear, his comments are given in full.

The vertical lines in the margins share several characteristics. In particular, almost all seem to have been carefully drawn to encompass a full thought. At the same time, almost all such lines are also accompanied by a written notation in Wallace's hand, leaving little room for dispute over their originator. Only a handful of lines occur without written comment, indicated by the following: (may not be ARW's). Even if all of the lines without comment were to be attributed to someone else, they represent only a small fraction of the total and do not alter the value of Wallace's comments. Other people did have an opportunity to write in this book, including, for example, two friends in the Far East (mentioned to Darwin in the letter from Sumatra because of their intense interest in the subject), and Spruce and/or members of his family. But these few unaccompanied lines share a characteristic of the other lines, which is that they mark off a complete thought; thus, they might reasonably be considered to be Wallace's.

There are also examples of handwriting entered at different times. They are interesting in themselves, but they do not alter the case. Entries on the flyleaf form one group. Besides a note on albinism, there are references to pages 14, 166, 189, and 473, written at different times apparently, but clearly all Wallace's (see below, under appropriate page numbers). Another group can be found in chapter 4 of the *Origin*. Here the words "natural selection" have been crossed out and replaced throughout the chapter with "survival of the fittest," a term of Herbert Spencer's

11. Alfred Russel Wallace, various notebooks, MS, Linnean Society of London.

not introduced until 1864; thus this substitution could not have been made before that year. There is a marked difference in the brightness and fineness of the substitutions, but again, the handwriting is clearly Wallace's (see below, under page 104).

WALLACE'S ANNOTATIONS

Albinism [flyleaf]. Albinism was scarcely touched upon by Darwin in the *Origin* (see pages 12, 13, 163), but it had long been a matter of interest to Wallace, especially in regard to the races of man. Early attracted to the problem of species and varieties, he had read the books by James Cowles Prichard, *Researches into the Physical History of Mankind*, and William Lawrence, *Lectures on Comparative Anatomy, Physiology, Zoology, and the Natural History of Man*, in which both authors argued for the unity of mankind.¹²

In a letter to his new friend Henry Walter Bates,¹³ on November 9, 1847, Wallace discussed this subject, concluding that both Negroes and albinos were separate species. "But I would class both these as distinct *species*," he noted, "and I would only consider those to be *varieties* whose differences are produced by external causes, and which, therefore, are not propagated as distinct races."¹⁴ Here he overlooked the fact that there was no albino race or species, there were only isolated individuals.¹⁵ Adding natural selection to the mix showed why there is no such species, as **Wallace** noted on the flyleaf of his copy of the *Origin*:

The old objection that *albino* animals do not increase & form distinct species is now well answered since they cannot do so unless *albinoism* [*sic*] is profitable to them which there seems no reason to think it can possibly be.

12. James Cowles Prichard, *Researches into the Physical History of Man* (London: J. & A. Arch, 1813, and later editions); William Lawrence, *Lectures on Physiology, Zoology, and the Natural History of Man* (London: Printed for J. Callow, 1819, and later editions).

13. Wallace and Bates, who were both unsponsored, together planned to travel in the Amazon region of South America in search of natural history specimens. They arrived at Pará at the entrance to the Amazon River on May 26, 1848. Wallace was to spend four years exploring in the Amazon region, returning to Pará on July 2, 1852; Bates did not leave from this port until March 17, 1859. Spruce returned to England in 1864.

14. Marchant, *Wallace*, pp. 73–74.

15. Barbara G. Beddall, "Notes for Mr. Darwin: Letters to Charles Darwin from Edward Blyth at Calcutta: A Study in the Process of Discovery," *J. Hist. Biol.*, 6 (1973), 83–84.

Domesticated Animals and Reversion [flyleaf and asterisk to page 14]. There were certain points on which Darwin and Wallace never agreed, one of which was the role played by domesticated varieties in the theory of natural selection. In his 1858 paper, "On the Tendency of Varieties to Depart Indefinitely from the Original Type," Wallace noted:

One of the strongest arguments which have been adduced to prove the original and permanent distinctness of species is, that *varieties* produced in a state of domesticity are more or less unstable, and often have a tendency, if left to themselves, to return to the normal form of the parent species; and this instability is considered to be a distinctive peculiarity of all varieties, even those occurring among wild animals in a state of nature, and to constitute a provision for preserving unchanged the originally created distinct species.

In the absence or scarcity of facts and observations as to *varieties* occurring among wild animals, this argument has had great weight with naturalists, and has led to a very general and somewhat prejudiced belief in the stability of species. Equally general, however, is the belief in what are called "permanent or true varieties," . . . assuming that such varieties have strict limits, and can never again vary further from the original type, although they may return to it, which, from the analogy of the domesticated animals, is considered to be highly probable, if not certainly proved.

It will be observed that this argument rests entirely on the assumption that *varieties* occurring in a state of nature are in all respects analogous to or even identical with those of domestic animals, and are governed by the same laws as regards their permanence or further variation. But it is the object of the present paper to show that this assumption is altogether false, that there is a general principle in nature which will cause many *varieties* to survive the parent species, and to give rise to successive variations departing further and further from the original type, and which also produces, in domesticated animals, the tendency of varieties to return to the parent form.¹⁶

Wallace was convinced of the unsuitability of domestic varieties, and he listed further objections:

16. Darwin and Wallace, "Tendency," quoted here from Wallace's *Natural Selection and Tropical Nature: Essays on Descriptive and Theoretical Biology* (London: Macmillan, 1891), pp. 22–23.

We see, then, that no inferences as to the permanence of varieties in a state of nature can be deduced from the observations of those occurring among domestic animals. The two are so much opposed to each other in every circumstance of their existence, that what applies to the one is almost sure not to apply to the other. Domestic animals are abnormal, irregular, artificial; they are subject to variations which never occur, and never can occur, in a state of nature: their very existence depends altogether on human care — so far are many of them removed from that just proportion of faculties, that true balance of organization, by means of which alone an animal left to its own resources can preserve its existence and continue its race.¹⁷

In concluding this paper, Wallace stated:

We believe we have now shown that there is a tendency in nature to the continued progression of certain classes of *varieties* further and further from the original type — a progression to which there appears no reason to assign any definite limits — and that the same principle which produces this result in a state of nature will also explain why domestic varieties have a tendency, when they become wild, to revert to the original type.¹⁸

For Wallace, it was the distinction between domesticated and wild varieties that was fundamental. For Darwin, to the contrary, it was their similarity; indeed, it was important for the development of his theory that this be so, for he argued from domesticated varieties to wild varieties. “Having alluded to the subject of reversion,” **Darwin** wrote,

I may here refer to a statement often made by naturalists — namely, that our domestic varieties, when run wild, gradually but certainly revert in character to their aboriginal stocks. Hence it has been argued that no deductions can be drawn from domestic races to species in a state of nature. I have in vain endeavoured to discover on what decisive facts the above statement has so often and so boldly been made. There would be great difficulty in proving its truth: we may safely conclude [Wallace's vertical line ended here] that very many of the most strongly-marked domestic varieties could not possibly live in a

17. *Ibid.*, p. 31.

18. *Ibid.*, p. 33.

wild state. In many cases we do not know what the aboriginal stock was, and so could not tell whether or not nearly perfect reversion had ensued.¹⁹

In a “note on fly leaf” to which he referred on page 14 (with asterisk and line), **Wallace** made the following comment:

When domesticated animals are turned wild, *natural* selection begins to act & will speedily exterminate all the individuals which are least fit to struggle for their own existence. Among the offspring of the survivors those which lose in any degree the disadvantageous characters induced by domestication will have the advantage & will therefore necessarily *approach* to the characters of the original or some allied wild stock, & this is all that the evidence seems to prove to take place [*i.e.*, this argument would have no bearing on the matter of wild varieties].

In his 1858 paper, Wallace also noted:

Domestic varieties, when turned wild, *must* return to something near the type of the original wild stock, or become altogether extinct. [Wallace added later in a note: “That is, they will vary, and the variations which tend to adapt them to the wild state, and therefore approximate them to wild animals, will be preserved. Those individuals which do not vary sufficiently will perish.”]²⁰

Nearly thirty years later, in the preface to his book on *Darwinism*, published in 1889, Wallace emphasized this point anew:

It has always been considered a weakness in Darwin’s work that he based his theory, primarily, on the evidence of variation in domesticated animals and cultivated plants. I have endeavored to secure a firm foundation for the theory in the variation of organisms in a state of Nature; and as the exact amount and precise character of these variations is of paramount importance in the numerous problems that arise when we apply the theory to explain the facts of Nature, I have endeavored, by means of a series of diagrams, to exhibit to the eye the actual

19. Darwin, *Origin* (1859), p. 14.

20. Darwin and Wallace, “Tendency,” p. 31.

variations as they are found to exist in a sufficient number of species.²¹

Limits to Variability? [page 8]. Clearly this was an important issue. Those who believed in the permanence of species necessarily also believed that any variation that did occur, occurred within strict limits, thus preserving this permanence. Wallace had already taken notice of this problem some years earlier when working on a book of his own, whilst traveling in the Malay Archipelago. Disagreeing with the views on this point expressed by Sir Charles Lyell, whose book on the *Principles of Geology* he carried with him, he asked, "In fact, what positive evidence have we that species only vary within certain limits?"²²

Darwin remarked in the *Origin* [page 8] that "[n]o case is on record of a variable being ceasing to be variable under cultivation. Our oldest cultivated plants, such as wheat, still often yield new varieties: our oldest domesticated animals are still capable of rapid improvement or modification." **Wallace** noted in the margin that "there is therefore no necessary *limit to variation.*" With line.

Reproductive Systems [page 8]. **Darwin**: ". . . this system appearing to be far more susceptible than any other part of the organisation, to the action of any change in the conditions of life." **Wallace**: line only (may not be ARW's).

Reproductive Systems [page 9]. **Darwin**: Domesticated plants and animals breed freely even when weak and sickly, while healthy [wild] individuals, even when taken young, do not. **Wallace**: line only (may not be ARW's).

Domesticated Animals and Reversion [page 14]. See discussion above.

Uniformity of Character [page 15]. **Darwin**: "When we look to the hereditary varieties of our domestic animals and plants, and compare them with species closely allied together, we generally

21. Alfred Russel Wallace, *Darwinism: An Exposition of the Theory of Natural Selection, with Some of Its Applications* (New York: Humboldt, 1889), p. iii.

22. Beddall, "Wallace, Darwin," p. 283; Alfred Russel Wallace, "Notebook, 1855--1859," MS, Linnean Society of London, p. 39. See also Charles Lyell, *Principles of Geology; or, The Modern Changes of the Earth and Its Inhabitants*, 4th ed. (London: Murray, 1835), II, 435.

perceive in each domestic race, as already remarked, less uniformity of character than in *true species*." **Wallace**: "true species" underlined. With a question mark in margin.

Domestic Breeds [pages 18—19]. **Darwin**: "Mr. [Edward] Blyth [museum curator in India] . . . thinks that all the breeds of poultry have proceeded from the common wild Indian fowl (*Gallus bankiva*) [*sic*]." **Wallace**: "? the Japanese species." With line. **Darwin**: "In regard to ducks and rabbits, the breeds of which differ considerably from each other in structure, I do not doubt that they all have descended from the common wild duck and rabbit." **Wallace**: vertical line in margin only, but it is attached to the line of the previous example.

Differences in the Anatomy of Domestic Pigeons [page 22]. **Darwin**: Details of structural differences. **Wallace**: "Several of these differences only occur in distinct *genera* or even *families* in a state of nature." No line.

Domestic Pigeons [page 27]. **Darwin**: ". . . I can feel no doubt that all our domestic breeds have descended from the *Columba livia* [*sic*] with its geographical sub-species." **Wallace**: "proved!" (underlined twice). With line.

Accumulated Effects of Selection in Plants [page 33]. **Darwin**: ". . . I cannot doubt that the continued selection of slight variations, either in the leaves, the flowers, or the fruit, will produce races differing from each other chiefly in these characters." **Wallace**: "Very interesting." With line.

Edible Plants and Selection [page 38]. **Darwin**: ". . . the native plants have not been improved by continued selection up to a standard of perfection comparable with that given to the plants in countries anciently civilised." **Wallace**: "Tropical fruits with few exceptions are very inferior to European —." With line.

Checks to Cats, Mice, and Humble-bees [page 74]. **Darwin**: Mice destroy the combs and nests of bees. Therefore, when cats eat the mice they contribute to an increase in the numbers of bees and consequently to the numbers of certain flowers. **Wallace**: "? Does a cat ever eat a *field-mouse*, except when extremely pressed by hunger?" With line.

Forest Regrowth [page 74]. **Darwin**: "Every one has heard that

when an American forest is cut down, a very different vegetation springs up; but it has been observed that the trees now growing on the ancient Indian mounds, in the Southern United States, display the same beautiful diversity and proportion of *kinds* [underlining by Wallace] as in the surrounding virgin forests." **Wallace**: ". . . and these kinds are the same as those in the adjacent virgin forest, for the secondary vegetation here alluded to is only ephemeral." Asterisk, but no line.

Nutriments Stored in Seeds [page 77]. **Darwin**: These nutriments "favour the growth of the young seedling, whilst struggling with other plants growing vigorously all around." **Wallace**: "Plants when with albuminous seeds ought to be the most numerous, at least in individuals." With asterisk and line.

Natural Selection, or Survival of the Fittest? [pages 82–107]. **Darwin** discussed "natural selection" in chapter 4 of the *Origin*. **Wallace** crossed this term out, substituting for it the phrase "survival of the fittest," a term first used by the philosopher Herbert Spencer in 1864, as mentioned earlier. On pages 104 and 107, Wallace expanded his substitution as follows: "changes produced by the constant survival of the fittest" (with line), and "change produced by survival of the fittest." With line.²³

Wallace's reasoning on this point is well set out in a letter to Darwin written on July 2, 1866:

My dear Darwin, — I have been so repeatedly struck by the utter inability of numbers of intelligent persons to see clearly, or at all, the self-acting & necessary effects of *Nat Selection*, that I am led to conclude that the term itself & your mode of illustrating it, however clear & beautiful to many of us, are yet not the best adapted to impress it on the general *naturalist public*. The two last cases of this misunderstanding are, 1st the article on "*Darwin & his Teachings*" in the last "Quarterly Journal of Science," which, though very well written & on the whole appreciative, yet concludes with a charge of something like blindness, in your not seeing that "*Natural Selection*" requires the constant watching of an intelligent "chooser" like

23. Herbert Spencer, *Principles of Biology*, 2 vols. (London: Williams and Norgate, 1864, 1867), I, 444.

man's selection to which you so often compare it; and 2nd in [Paul] Janet's recent work on the "*Materialism of the present day*," reviewed in last Saturday's "Reader," by an extract from which I see that he considers your weak point to be that you do not see that "*thought and direction are essential to the action of Natural Selection.*" The same objection has been made a score of times by your chief opponents, and I have heard it as often stated myself in conversation. Now I think this arises almost entirely from your choice of the term "*Nat Selection*," & so constantly comparing it in its effects, to *Man's selection*, and also to your so frequently personifying *Nature* as "selecting," as ["]preferring," as "*seeking only the good of the species*," etc., etc. To the few this is as clear as daylight, & beautifully suggestive, but to many it is evidently a stumbling block. I wish therefore to suggest to you the possibility of entirely avoiding this source of misconception in your great work (if not now too late), & also in any future editions of the "Origin," and I think it may be done without difficulty and very effectually by adopting Spencer's term (which he generally uses in preference to *Nat Selection*), viz. "*Survival of the fittest.*" This term is the plain expression of the *fact*, *Nat selection* is a metaphorical expression of it, and to a certain degree *indirect & incorrect*, since, even personifying *Nature*, she does not so much *select* special variations, as *exterminate* the most unfavourable ones.

Combined with the enormous multiplying powers of all organisms, & the "struggle for existence," leading to the constant destruction of by far the largest proportion, — facts which no one of your opponents, as far as I am aware, has denied or misunderstood — "*the survival of the fittest*" rather than of those which were less fit, could not possibly be denied or misunderstood. Neither would it be possible to say, that to ensure the "*survival of the fittest*" any *intelligent chooser* was necessary, — whereas when you say *natural selection* acts so as to choose those that are fittest it *is* misunderstood & apparently always will be. Referring to your book I find such expressions as "Man selects only for his own good; Nature only for that of the being which she tends." This it seems will always be misunderstood; but if you had said, "Man selects only for his own good; Nature, by the inevitable 'survival of the fittest,' only for that of the being she tends," — it would have been less liable to be so. . . .

I could not venture to propose to any other person so great an alteration of terms, but you I am sure will give it an impartial consideration, & if you really think that the change will produce

a better understanding of your work, will not hesitate to adopt it.²⁴

Darwin responded immediately, writing to Wallace on July 5:

I have been much interested by your letter which is as clear as daylight. I fully agree with all that you say on the advantages of H. Spencer's excellent expression of "the survival of the fittest.["] This however had not occurred to me till reading your letter. It is[,] however, a great objection to this term that it cannot be used as a substantive governing a verb; & that this is a real objection I infer from H. Spencer continually using the words natural selection. I formerly thought, probably in an exaggerated degree, that it was a great advantage to bring into connection natural & artificial selection; this indeed led me to use a term in common, and I still think it some advantage. I wish I had received your letter two months ago for I would have worked in "the survival etc" often in the new edition of the "Origin" [the 4th] which is now almost printed off & of which I will of course send you a copy. I will use the term in my next book on Domestic Animals etc from which, by the way, I plainly see that you expect *much* too much. 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."²⁵

Indeed, he added the phrase to the title of his chapter 4, which now reads "Natural Selection; or the Survival of the Fittest."

Intercrossing of Plants [page 99]. By intercrossing, **Darwin** meant that "two individuals must always unite for each birth . . . that with all hermaphrodites two individuals, either occasionally or habitually, concur for the reproduction of their kind" [page 96]. These may be of the same or different varieties or species. **Darwin**: ". . . the pollen of a distinct *variety* [has] a prepotent effect over a flower's own pollen. . . . When distinct *species* are crossed the case

24. Marchant, *Wallace*, pp. 140–142. Letter # 5140, DAR 106/7 (ser. 2): 33–38, in the *Darwin Calendar*, quoted here from a copy of the original, is in the Cambridge University Library.

25. Darwin, *LL*, III, 45–47; Marchant, *Wallace*, pp. 144–145. Letter # 5145 in the *Darwin Calendar*, quoted here from a copy of the original, is in the British Library, BL(Add 46434).

is directly the reverse, for a plant's own pollen is always prepotent over foreign pollen." **Wallace**: the latter statement was marked by Wallace with a question mark, in addition to the following comment: "How so? After all this argument to show that there are *no distinct species?*" With line.

Intercrossing of Animals and Plants [page 104]. **Darwin**: "... I have already attempted to show that we have reason to believe that occasional intercrosses take place with all animals and with all plants." This produces offspring with a gain in "vigour and fertility over the offspring from long-continued "self-fertilisation." Uniformity of character [see above] is maintained among those not crossing by inheritance and natural selection. **Wallace**: "Hermaphrodite plants ought therefore to vary individually more than unisexual do. Or than animals." With line. Added later were two substitutions of "survival of the fittest" for "natural selection." With lines.

Inhabitants of Fresh Water [page 107]. **Darwin**: As fresh water is limited in extent, "the competition between fresh-water productions will have been less severe than elsewhere." **Wallace** marked this thought with a line and double exclamation points, and the words "change produced by survival of the fittest," with an arrow indicating that this phrase, added later, referred to the quotation above. With line.

Insects on the Island of Madeira [page 136]. **Darwin**: According to [Thomas Vernon] Wollaston [entomologist], ground-feeding beetles on Madeira are flightless. Darwin explains their wingless state as "mainly due to the action of natural selection, but combined probably with disuse." Flower-feeding insects "must habitually use their wings to gain their subsistence," thus "their wings [are] not at all reduced, but even enlarged." **Wallace**: "very interesting view & satisfactory." With line.

Winged Fruits [page 146]. **Darwin**: "For instance, Alph[onse] [d]e Candolle [Swiss botanist] has remarked that winged seeds are never found in fruits which do not open," to which **Wallace** added: "while all winged fruits do not open." Asterisk, but no line.

Horses' Stripes [page 166]. **Darwin**:

We see several very distinct species of the horse-genus becoming, by simple variation, striped on the legs like a zebra, or

striped on the shoulders like an ass. . . . I have stated that the most probable hypothesis to account for the reappearance of very ancient characters, is — that there is a *tendency* in the young of each successive generation to produce the long-lost character, and that this tendency, from unknown causes, sometimes prevails.

Wallace, on flyleaf: “The stripes on horses furnish a very strong argument”; and on page 166: “This argument is irresistible.” With line; see flyleaf comment concerning page 473.

Darwin had apparently asked Wallace, in a letter now missing, to report on any similar occurrences, and on January 25, 1859, wrote to him as follows: “Many thanks for your offer to look after Horses’ stripes; if there are any Donkeys, pray add them.”²⁶

A Common Parentage vs. Independent Creation [page 167].

Darwin:

He who believes that each equine species [domestic or wild horse, ass, the hemionus (Asiatic wild ass), quagga (extinct since 1883), and zebra] was independently created, will, I presume, assert that each species has been created with a tendency to vary, both under nature and under domestication, in this particular manner, so as often to become striped like other species of the genus; and that each has been created with a strong tendency, when crossed with species inhabiting distant quarters of the world, to produce hybrids resembling in their stripes, not their own parents, but other species of the genus. To admit this view is, as it seems to me, to reject a real for an unreal, or at least for an unknown, cause. It makes the works of God a mere mockery and deception; I would almost as soon believe with the old and ignorant cosmogonists, that fossil shells had never lived, but had been created in stone so as to mock the shells now living on the sea-shore.

Wallace marked this whole quotation with a line, double-marking the part beginning with “To admit,” against which he wrote “! good.”

Species with Anomalous Habits [page 184]. **Darwin:** “Can a more

26. Darwin, *LL*, II, 145–147; Marchant, *Wallace*, p. 111. Letter # 2405 in the Darwin *Calendar*, quoted here from a copy of the original, is in the British Library, BL(Add 46434).

striking instance of adaptation be given than that of a woodpecker for climbing trees and for seizing insects in the chinks of the bark? Yet in North America there are woodpeckers which feed largely on fruit," a claim substantiated by an examination of the stomach contents of various species.²⁷ **Wallace** added examples of his own: "Fruit eating Hawks, & fruit eating Trogons in S. America." No line.

The Eye [flyleaf and pages 186–189]. **Darwin**: "If it could be demonstrated that any complex organ existed, which could not possibly have been formed by numerous, successive, slight modifications, my theory would absolutely break down. But I can find out no such case." **Wallace**, on flyleaf and referring to page 189: "The origin of the eye well illustrated."

Bamboo [page 197]. **Darwin**: "A trailing bamboo in the Malay Archipelago. . . ." **Wallace**: "in Celebes."

Stinging Insects [page 202]. **Darwin**: "Can we consider the sting of the wasp or of the bee as perfect, which, when used against many attacking animals, cannot be withdrawn, owing to the backward serratures, and so inevitably causes the death of the insect by tearing out its viscera?" **Wallace**: "Solitary insects sh^d not have the sting so serrated; as it would be injurious to each *individual*." No line.

Vestigial Parts [page 205]. **Darwin**: "We may, also, believe that a part formerly of high importance has often been retained (as the tail of an aquatic animal by its terrestrial descendants), though it has become of such small importance that it could not, in its present state, have been acquired by natural selection, — a power which acts solely by the preservation of profitable variations in the struggle for life." **Wallace**: "very good." With line.

Instincts and Birds' Nests [page 212]. **Darwin**: "[John James] Audubon [American ornithologist] has given several remarkable cases of differences in nests of the same species in the northern and southern United States." **Wallace**: line only (may not be ARW's).

Individual Differences [page 212]. **Darwin**: "Several cases also,

27. Alexander C. Martin, Herbert Zim, and Arnold L. Nelson, *American Wildlife and Plants* (New York: McGraw-Hill, 1951), pp. 118–126.

could be given, of occasional and strange habits in certain species, which might, if advantageous to the species, give rise, through natural selection, to quite new instincts." **Wallace:** "sh^d have given *one*." With line.

Domestic Instincts [page 213]. **Darwin:** ". . . it cannot be doubted that young pointers (I have myself seen a striking instance) will sometimes point and even back other dogs the very first time that they are taken out. . . ." **Wallace:** "Have they not learnt at home?" With line.

Domestic Instincts [page 214]. **Darwin:** "Domestic instincts are sometimes spoken of as actions which have become inherited solely from long-continued and compulsory habit, but this, I think, is not true. No one would ever have thought of teaching, or probably could have taught, the tumbler-pigeon to tumble, — an action, which, as I have witnessed, is performed by young birds, that have never seen a pigeon tumble." **Wallace:** "Is not this & all other such habits the result of some slight peculiarity of organization which makes 'tumbling' agreeable to the animals." With line.

Cell-Making Instinct of the Hive-Bee [page 224]. **Darwin:** "We hear from mathematicians that bees have practically solved a recondite problem, and have made their cells of the proper shape to hold the greatest possible amount of honey, with the least possible consumption of precious wax in their construction." **Wallace:** "? this." With line.

Cell-Making Instinct (Continued) [page 232]. **Darwin:** "It was really curious to note in cases of difficulty, as when two pieces of comb met at an angle, how often the bees would entirely pull down and rebuild in different ways the same cell, sometimes recurring to a shape which they had at first rejected." **Wallace:** "What is this but rational building?" With line.

Castes of Worker Ants [page 241]. How are those of different size and structure within one nest formed? **Darwin:** "— a graduated series having been first formed, as in the case of the driver ant, and then the extreme forms, from being the most useful to the community, having been produced in greater and greater numbers through the natural selection of the parents which generated them; until none with an intermediate structure were produced." **Wallace:** "? may not these different [insect] eyes [see p. 240] be

produced by the feeding[,] as a queen bee can be fed up from a worker larva." No line.

Fertility of First Crosses and of Hybrids [pages 256—257]. **Darwin:** "Now the fertility of first crosses between species, and of the hybrids produced from them, is largely governed by their systematic affinity," that is, "the resemblance between species in structure and in constitution." **Wallace:** "All this shows that the external characters wh. [which] distinguish species are only indications of internal or constitutional differences, which may be very great when the two species differ little in external characters. We can imagine really distinct species under the same external form as occurs in minerals." No line.

Hybrid Vigour and Fertility in Crosses [page 267]. **Darwin:**

Hence it seems that, on the one hand, slight changes in the conditions of life benefit all organic beings, and on the other hand, that slight crosses, that is crosses between the males and females of the same species which have varied and become slightly different, give vigour and fertility to the offspring. But we have seen that greater changes, or changes of a particular nature, often render organic beings in some degree sterile; and that greater crosses, that is crosses between males and females which have become widely or specifically different, produce hybrids which are generally sterile in some degree. I cannot persuade myself that this parallelism is an accident or an illusion. Both series of facts seem to be connected together by some common but unknown bond, which is essentially related to the principle of life.

To which **Wallace** responded: "very suggestive!" With line.

Domestication vs. Nature [page 269]. **Darwin:** "... new races of animals and plants are produced under domestication by man's methodical and unconscious power of selection, for his own use and pleasure," but not affecting the reproductive system; "Nature acts ... for each creature's own good ... [modifying] the reproductive system." **Wallace:** "very satisfactory!" With line.

Intermediate Links [page 281]. **Darwin:** "So with natural species, if we look to forms very distinct, for instance to the horse and tapir, we have no reason to suppose that links ever existed directly intermediate between them, but between each and an unknown

common parent." **Wallace**: "So with the orangutan & man." No line.

Rarity of Fossil Mammals [page 289]. **Darwin**: "Nor is their rarity surprising, when we remember how large a proportion of the bones of [T]ertiary mammals have been discovered either in caves or in lacustrine deposits; and that not a cave or true lacustrine bed is known belonging to the age of our [S]econdary or [P]alaeozoic formations." **Wallace** marked the last part of this statement with a vertical line and a question mark.

Geological Formations along the West Coast of South America [page 290]. **Darwin**: "Along the whole west coast, which is inhabited by a peculiar marine fauna, [T]ertiary beds are so scantily developed, that no record of several successive and peculiar marine faunas will probably be preserved to a distant age." **Wallace**: with line and question mark.

Imperfections of the Geological Record [page 292]. **Darwin**: "During periods of elevation . . . there will generally be a blank in the geological record. On the other hand, during subsidence . . . though there will be much extinction, fewer new varieties or species will be formed. . . . Nature may almost be said to have guarded against the frequent discovery of her transitional or linking forms." **Wallace**: "In my notes is a similar remark to explain the poverty of the Pacific Islands." With line.

Transition Forms [page 298]. **Darwin** gives reasons why transition forms are not likely to be traced "in any one geological formation." **Wallace**: line only (may not be ARW's).

Finding Fossil Forms [page 299]. **Darwin**: How would future geologists determine whether the breeds of our domestic animals were descended from one or "several aboriginal stocks," or how closely North American and European species were related? "This could be effected only by the future geologist discovering in a fossil state numerous intermediate gradations; and such success seems to me improbable in the highest degree." **Wallace**: "a very good & forcible illustration." With line.

Rarity and Extinction [page 320]. **Darwin**: "I may repeat what I published in 1845,"

namely, that to admit that species generally become rare before

they become extinct — to feel no surprise at the rarity of a species, and yet to marvel greatly when it ceases to exist, is much the same as to admit that sickness in the individual is the forerunner of death — to feel no surprise at sickness, but when the sick man dies, to wonder and to suspect that he died by some unknown deed of violence.²⁸

Wallace: “*very good!*” With line.

Arctic and Alpine Plants [page 367]. **Darwin:** Here he explains their geographical distribution during the glacial period and afterwards, when the returning warmth caused the northward and upward movements of arctic forms that had earlier retreated to warmer regions because of the glacial coldness. **Wallace:** “How did the plants which were banished from Central Europe return? The Alps & Pyrenees w^d [would] prevent them. The ancient temperate flora of France for example must have been exterminated as they could not pass the Pyrenees or the Mediterranean on the S. or the Alps on the E. This wants explaining.” No line, but note begins at beginning of this paragraph.

Islands and Special Creation [page 390]. **Darwin:** “He who admits the doctrine of the creation of each separate species, will have to admit, that a sufficient number of the best adapted plants and animals have not been created on oceanic islands; for man has unintentionally stocked them from various sources far more fully and perfectly than has nature.” **Wallace:** vertical double line, “oh” in right margin.

Madeira and Its Land-Shells [page 391]. In his first letter to Wallace, dated May 1, 1857, Darwin had written, “Land-molluscs are a great perplexity to me.”²⁹ In a second letter, written on December 22, 1857, Darwin further noted:

You ask about land-shells on islands far distant from continents: Madeira has a few identical with those of Europe, & here the evidence is really good as some of them are sub-fossil. In the Pacific islands there are cases of identity, which I cannot at

28. Charles Darwin, *Voyage of the Beagle* (London: Dent, 1906), p. 168. Darwin altered this statement somewhat in the *Origin*.

29. Darwin, *LL*, II, 95–96; Marchant, *Wallace*, p. 109. Letter # 2086 in the Darwin *Calendar*, quoted here from a copy of the original, is in the British Library, BL(Add 46434).

present persuade myself to account for by introduction through man's agency; although Dr. Aug[ustus] Gould [American expert on molluscs] has conclusively shown that many land-shells have thus been distributed over the Pacific by man's agency. These cases of introduction are most plaguing. Have you not found it so in the Malay Archipelago? [I]t has seemed to me in the lists of mammals of Timor & other islands, that *several* in all probability have been naturalised. Since writing before, I have experimented a little on some land-molluscs, and have found sea-water not quite so deadly as I anticipated.³⁰

Darwin returned to this subject in the *Origin*: "Madeira, again, is inhabited by a wonderful number of peculiar land-shells, whereas not one species of sea-shell is confined to its shores." At this point **Wallace** noted: "This certainly indicates a former connection either by land or shoals." No line.

Former land connections were at that time a popular explanation for such distributions. Darwin had written in his second letter to Wallace that "... I can see that you are inclined to go *much* further than I am in regard to the former connections of oceanic islands with continents."³¹

Islands and Land-Shells [page 397]. **Darwin**: "I will here give a single instance of one of the cases of difficulty. Almost all oceanic islands, even the most isolated and smallest, are inhabited by land-shells, generally by endemic species, but sometimes by species found elsewhere." **Wallace** marked this passage with an asterisk, noting at the bottom of the page: "In Moluccas, small islands 20 to 50 miles from larger ones contain *Megapodii*, birds which can not fly 500 yards. How could they have been dispersed?" Asterisk, but no line.

Wallace gave an answer to this question some years later in his *Geographical Distribution of Animals*, published in 1876:

... the Nicobar bird. ... Instead of being a well-marked and clearly differentiated form, as we should expect to find it if its remote and isolated habitat were due to natural causes, it so

30. Darwin, *LL*, II, 109; Marchant, *Wallace*, p. 110. Letter # 2192 in the Darwin *Calendar*, quoted here from a copy of the original, is in the British Library, BL(Add 46434). The last sentence quoted here was omitted in the *Life and Letters*.

31. Darwin, *LL*, II, 109; Marchant, *Wallace*, pp. 109–110. Letter # 2192, see note 30.

nearly resembles some of the closely-allied species [of megapodes] of the Moluccas and New Guinea, that, had it been found with them, it would hardly have been thought specifically extinct [*sic*, distinct?]. I therefore believe that it is probably an introduction by the Malays, and that, owing to the absence of enemies and general suitability of conditions, it has thriven in the islands and has become slightly differentiated in colour from the parent stock.³²

How Are Structural Patterns to Be Explained? [page 437]. **Darwin:** "Why should the sepals, petals, stamens, and pistils in any individual flower, though fitted for such widely different purposes, be all constructed on the same pattern?" **Wallace** crossed out the words from "for such" to "pattern" and substituted the following: "for one invariable purpose, be constructed on such widely different patterns? That very 'invariable purpose' explains why the patterns are all reducible to (than spring from) one type." Asterisk, but no line.

Larvae [page 457]. **Darwin:** "Larvae are active embryos." **Wallace** substituted "free" for "active."

Variability of Specific Characters [page 473]. **Darwin:**

Why, for instance, should the colour of a flower be more likely to vary in any one species of a genus, if the other species, supposed to have been created independently, have differently coloured flowers, than if all the species of the genus have the same coloured flowers? If species are only well-marked varieties, of which the characters have become in a high degree permanent, we can understand this fact; for they have already varied since they branched off from a common progenitor in certain characters, by which they have come to be specifically distinct from each other.

Wallace, on flyleaf: "p. 473 very strong argument." On page 473, line only.

32. Alfred Russel Wallace, *The Geographical Distribution of Animals, with a Study of the Relations of Living and Extinct Faunas as Elucidating the Past Changes of the Earth's Surface*, 2 vols. (London: Murray, 1876; reprinted, New York: Hafner Publishing Co., 1962), II, 342.

Rudimentary Organs and Homologous Structures [page 480].
Darwin:

On the view of each organic being and each separate organ having been specially created, how utterly inexplicable it is that parts, like the teeth in the embryonic calf or like the shrivelled wings under the soldered wing-covers of some beetles, should thus so frequently bear the plain stamp of inutility! Nature may be said to have taken pains to reveal, by rudimentary organs and by homologous structures, her scheme of modification, which it seems that we wilfully will not understand.

Wallace: "admirable!" With line.

Miraculous Creation or Ordinary Birth [page 483]. **Darwin:** "But do they [various naturalists] really believe that at innumerable periods in the earth's history certain elemental atoms have been commanded suddenly to flash into living tissues?" To which **Wallace** added: "for the sake of producing a creature differing almost infinitesimally from others produced by ordinary generation —."

This last harks back to Wallace's 1858 paper, "Note on the Theory of Permanent and Geographical Varieties." "[W]hy," he had asked, "should a special act of creation be required to call into existence an organism differing only in degree from another which has been produced by existing laws?"³³ **Wallace** also added "& classes" to **Darwin's** sentence "Fossil remains sometimes tend to fill up very wide intervals between existing orders."

Conclusion [page 490]. **Darwin:**

Thus, from the war of nature, from famine and death, the most exalted object which we are capable of conceiving, namely, the production of the higher animals, directly follows. There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most

33. Alfred Russel Wallace, "Note on the Theory of Permanent Varieties," *Zoologist*, 16 (1858), 5888. See also Beddall, "Wallace, Darwin," pp. 280, 288.

beautiful and most wonderful have been, and are being, evolved.

Wallace: "grand!!!" With line.

POSTSCRIPT

There remains one other source of Wallace's thoughts regarding the *Origin*, and that is a lengthy letter that he wrote to his brother-in-law, Thomas Sims, on March 15, 1861, from Timor:

Now for Mr. Darwin's book. You quite misunderstand Mr. D.'s statement in the preface and his sentiments. I have, of course, been in correspondence with him since I first sent him my little essay [in 1858, but in fact, the correspondence began earlier]. His conduct has been most liberal and disinterested. I think anyone who reads the Linnean Society papers and his book will see it. I *do* back him up in his whole round of conclusions and look upon him as the *Newton of Natural History*. . . .

It is clear that you have not yet sufficiently read the book to enable you to criticise it. It is a book in which every page and almost every line has a bearing on the main argument, and it is very difficult to bear in mind such a variety of facts, arguments and indications as are brought forward. It was only on the *fifth* perusal that I fully appreciated the whole strength of the work, and as I had been long before familiar with the same subjects I cannot but think that persons less familiar with them cannot have any clear idea of the accumulated argument by a single perusal.

. . . He seems to me, however, as clear as daylight that the principle of natural selection *must* act in nature. It is almost as necessary a truth as any of mathematics. Next, the effects produced by this action *cannot be limited*. It cannot be shown that there *is* any limit to them in nature. Again, the millions of facts in the numerical relations of organic beings, their geographical distribution, their relations of affinity, the modification of their parts and organs, the phenomena of intercrossing, embryology and morphology — all are in accordance with his theory, and almost all are necessary results from it; while on the other theory they are all isolated facts having no connection with each other and as utterly inexplicable and confusing as fossils are on the theory that they are special creations and are not the remains of animals that have once lived. It is the vast *chaos* of facts, which are explicable and fall into beautiful order

on the one theory, which are inexplicable and remain a *chaos* on the other, which I think must ultimately force Darwin's views on any and every reflecting mind.³⁴

Acknowledgments

I would like to express my appreciation to Quentin Keynes (son of Sir Geoffrey Keynes and great-grandson of Charles Darwin), for informing me that Wallace's annotated personal copy of the first edition of Darwin's *Origin of Species* was in the Keynes Collection at the Cambridge University Library, and also to Peter J. Gautrey for arranging to have the pages on which Wallace made his notations microfilmed for me. The quotations of Wallace's annotations are published here with the permission of the Syndics of Cambridge University Library. Mr. Gautrey has informed me that this copy came to the Cambridge University Library in 1982/83 as part of the Keynes Collection, but that there is no catalogue of this collection.

Mr. Quentin Keynes, at my request for further information on the history of this volume, recently inquired of Mr. John Lawson, bookseller, whose father sold this copy to Sir Geoffrey Keynes in 1958. According to Mr. Lawson, "My father bought the Darwin in a small collection of books when he was in Yorkshire on holiday in 1958. They were among some books he purchased from some Literary Society (no record of which), and they had been left [to] them by Spruce's executor when he died (no record now of his name)." As previously noted, it was Wallace himself who gave this volume to his botanist friend Spruce.

I would also like to express my appreciation to the Syndics of Cambridge University Library for permission to quote from letters in their possession (see notes 7 and 24); to the British Library for permission to quote from letters in their possession (see notes 4, 5, 25, 26, 29, 30); and to the Linnean Society of London for permission to quote from a notebook of Alfred Russel Wallace's in their possession (see note 22).

34. Marchant, *Wallace*, pp. 61–63. Wallace had also written a note similar in tone, but much shorter, to H. W. Bates, December 24, 1860; see Marchant, *Wallace*, p. 59.