

# DARWIN'S MORAL LAPSE

James Rachels

[This essay originally appeared in the *National Forum* (Summer 1986), pp. 22-24.]

One reason Darwin's letters and journals are such a pleasure to read is that in them we meet a modest, decent man who commands our respect, and even our affection. He was not only a great scientist; he was an exemplary human being. Yet there was one famous episode in Darwin's life in which he and his friends acted badly. Perhaps because he was so admirable a man, historians have tended to gloss over this moral lapse, sometimes even to the point of misrepresenting the facts.

I refer, of course, to the celebrated episode involving Alfred Russel Wallace, who "scooped" Darwin by independently discovering the theory of natural selection. The standard accounts suggest that Darwin and his friends treated Wallace honorably. A closer look at what happened leaves a different impression: it is a lamentable story of human weakness, in which some good men treated another good man disgracefully.

## The Standard Account

The main outline of the story is familiar enough. Darwin first formulated the theory of natural selection in 1838, and then proceeded to sit on it for twenty years. He envisioned a "big book" that would present overwhelming evidence for the theory and that would answer every possible objection; he insisted that he could not make the theory public until this ambitious work was completed. He worked on the project intermittently, and shared his thoughts with a few close friends, but generally he kept this work a secret. Darwin knew, however, that this was a risky course. Evolutionary ideas were "in the air," and the longer he delayed, the greater the danger that someone else would discover the same theory.

That is exactly what happened. In 1858 Wallace, a young naturalist working in the Malay Archipelago, came up with the idea of natural selection, and unlike Darwin he had no hesitation in telling the world about his discovery. He promptly wrote up a short paper and sent it to one of England's leading naturalists for an opinion--he sent it, in fact, to Darwin! The paper

arrived on June 18. Darwin was devastated. "If Wallace had my MS sketch written out in 1842," he said, "he could not have made a better short abstract! Even his terms now stand as heads of my chapters."<sup>1</sup>

Darwin's modesty had its limits. Having labored for twenty years, he did not want to lose priority. Nevertheless, his first reaction was typically generous. He would have Wallace's paper published, and give the younger man full credit for the discovery. But Darwin's friends, Lyell and Hooker, intervened. They did not want to see Darwin lose all credit, and so they proposed a compromise: they would arrange to have Wallace's paper *and* a paper by Darwin read on the same day before the Linnean Society. Thus the two men would share credit for having discovered the theory. On July 1, this meeting took place, and shortly afterwards the two papers were published together in the Society's journal. Having been forced into the open, Darwin abandoned the big book, and published an "abstract" of it, *On the Origin of Species*, a year later. Thus did the great work stumble into the world.

What could have been more fair? Darwin's labor of twenty years was made public, and Wallace got credit for having independently discovered the theory's main idea. It seems a happy ending, and most commentators agree that justice was done. The chorus of approval includes some of the most widely admired writers on the subject. In his recent biography *The Survival of Charles Darwin*, Ronald Clark calls the proposal by Lyell and Hooker "a judgment of Solomon,"<sup>2</sup> while Howard Gruber, author of the important study *Darwin on Man*, says that they were "a sort of informal jury" who handled the matter "equitably and firmly."<sup>3</sup> Stephen Jay Gould adds his voice to the consensus when he observes, after recounting the story, "In public and private, Darwin was infallibly decent and generous to his younger colleague."<sup>4</sup>

### **Looking at the Details**

<sup>1</sup> Darwin to Lyell, June 18, 1858. Francis Darwin, ed., *The Life and Letters of Charles Darwin*, 2 vols. (New York: Appleton, 1888), vol. I, p. 473.

<sup>2</sup> Ronald W. Clark, *The Survival of Charles Darwin: A Biography of a Man and an Idea* (New York: Random House, 1984), p. 106.

<sup>3</sup> Howard E. Gruber, *Darwin on Man*, 2nd edition (Chicago: University of Chicago Press, 1981) pp. 28-29.

<sup>4</sup> Stephen Jay Gould, *The Panda's Thumb* (New York: Norton, 1980), p. 48.

Yet, when we examine the story more closely, we find details that should give us pause. We may begin with an obvious question: why did Wallace send his paper to Darwin, of all people? As it turns out, this was not simply a coincidence.

Wallace had been an evolutionist since 1844, but like all evolutionists of that time (except Darwin) he had no satisfactory explanation of how evolutionary change could take place. How *could* one species be transformed into another? Wallace had no answer. He was, however, busily seeking a theory. Then in 1855 he published an article in a British journal, the *Annals and Magazine of Natural History*, with the title "On the Law Which Has Regulated the Introduction of New Species." Wallace did not yet have an account of the law, but he was obviously a talented man headed in the right direction, and the appearance of his article caused alarm among Darwin's friends. Lyell urged Darwin to delay no longer. Darwin, who apparently did not read the *Annals* paper until early 1857, remained obstinate; he still would not publish. But he did begin to assert his claim to the theory.

First Darwin wrote to Wallace, letting Wallace know that he had already staked out this particular territory: "I can plainly see," he said, "that we have thought much alike and to a certain extent have come to similar conclusions. . . This summer will make the 20th year (!) since I opened my first note-book, on the question how and in what way do species differ from each other. I am now preparing my work for publication." However, Darwin was secretive about the exact nature of his work. "It really is *impossible*," he continued, "to explain my views (in the compass of a letter), on the causes and means of variation."<sup>5</sup>

Having said that it was impossible to explain his theory in the compass of a letter, Darwin then proceeded to do so--but not to Wallace. Five months later he wrote a long letter to Asa Gray, his friend in America, describing the idea of natural selection in some detail.<sup>6</sup> There was no obvious reason for Darwin to do this. It seems that he simply wanted to get his work "on the record" and establish his priority before a neutral witness. At any rate, three months after the letter to Gray, we find Darwin writing again to Wallace, repeating his claim of priority, and again saying that the theory couldn't be explained in a mere letter: "I believe I go much further than you; but it is too long a subject to enter on my speculative notions."<sup>7</sup> Darwin's anxiety is plain. It could not be more obvious that he regarded Wallace as a dangerous rival.

<sup>5</sup> Darwin to Wallace, May 1, 1857. *Life and Letters*, vol. I, pp. 453-4.

<sup>6</sup> Darwin to Gray, September 5, 1857. *Life and Letters*, vol. I, pp. 477-482.

<sup>7</sup> Darwin to Wallace, December 22, 1857. *Life and Letters*, vol. I, p. 466.

Wallace, for his part, saw Darwin as a friendly and eminent colleague, working on the same problem, who had been kind enough to write him a series of letters. It seems clear, then, that Wallace sent his paper to Darwin because of this previous correspondence. Darwin had told Wallace: "We have thought much alike." It was natural enough for Wallace to want to share his new idea with the distinguished naturalist, and solicit his opinion. And that is *all* Wallace intended: he did not intend that his paper be published, either with or without a companion piece by anyone else.

What happened next was the work of Lyell and Hooker, the "informal jury" to which Gruber refers. They were distinguished scientists, men whose judgment would have been respected by almost anyone in London. Darwin was relieved to let them take care of the matter. Not only were they offering him a way out of his dilemma, Darwin was distracted at this time by troubles at home. Two of his children were seriously ill, and the youngest, 2-year-old Charles Waring, was actually dying; he died of scarlet fever on June 28, ten days after Wallace's paper arrived, and three days before the Linnean Society meeting.

But, while Lyell and Hooker may have been a "jury" of sorts, they lacked the quality most important in juries--impartiality. They were Darwin's friends, out to see that Darwin got full credit for the discovery. To make sure of this, they presented not two but *four* documents to the Linnean Society: in addition to the formal papers by Wallace and Darwin, they presented a copy of Darwin's letter to Asa Gray, written the previous year, and a statement certifying that Hooker had known of Darwin's work fifteen years earlier. The latter documents, clearly intended to prove Darwin's priority, were also printed, alongside the formal papers, in the Society's journal.

Now a rather shabby picture begins to emerge: Wallace innocently showed his new idea to Darwin, whom he considered to be a friendly colleague, without saying anything about publication. Later he learned that, without his having been consulted at all, his work had in fact been published--surrounded by three documents designed to ensure that he would not be given credit for the idea!

### **Misrepresentations**

The facts of this episode are misrepresented in some excellent books. Consider the question of whether Wallace had asked Darwin to arrange for the publication of his paper. If one

were to read only the historical accounts, one would conclude that Wallace did make this request. In his book *The Darwinian Revolution* Michael Ruse writes:

Wallace had begun to correspond with other men interested in the organic origins question. One of these was Charles Darwin, who was rumored to hold heretical views on the problem. It was therefore not surprising that Wallace sent his paper on selection to Darwin, suggesting that if it seemed worthy, he send it on for publication.<sup>8</sup>

And in *The Growth of Biological Thought* Ernst Mayr comments:

In his letter, Wallace said that if Darwin thought his paper sufficiently novel and interesting, he should send it to Lyell and, presumably, submit it for publication (the original Wallace letter is no longer in existence).<sup>9</sup>

But it is clear that Wallace did *not* ask Darwin to arrange for publication. Unfortunately, as Mayr notes, the letter that accompanied Wallace's paper is lost. However, we have Darwin's word for it that there was no such instruction. On the same day that Wallace's paper arrived, Darwin wrote an anguished letter to Lyell, in which he refers to Wallace's "MS, which he does not say he wishes me to publish."<sup>10</sup> Then, a week later, he wrote to Lyell again, to express his misgivings about Lyell's and Hooker's plan. One of his reasons for worrying was that "Wallace says nothing about publication."<sup>11</sup>

Why should such distinguished writers as Ruse and Mayr make this particular mistake? This is the kind of error that might perhaps follow from a moral presumption. It seems wrong to publish someone's work, without consulting him, in a forum he has not approved. Thus, if we

<sup>8</sup> Michael Ruse, *The Darwinian Revolution* (Chicago: University of Chicago Press, 1979), p. 159.

<sup>9</sup> Ernst Mayr, *The Growth of Biological Thought* (Cambridge: Harvard University Press, 1982), p. 423.

<sup>10</sup> Darwin to Lyell, June 18, 1858. *Life and Letters*, vol. I, p. 473.

<sup>11</sup> Darwin to Lyell, June 25, 1858. *Life and Letters*, vol. I, p. 474.

are assuming that Darwin and his friends acted properly, it will be natural to assume that Wallace must have asked that his paper be published. But in fact he did not.

There is worse. Kenneth Korey has recently written this bizarre summary of the incident:

[Darwin] received a manuscript from Alfred Wallace, a naturalist working in Malaya, with a solicitation of his opinion. The manuscript was, of course, a close approximation to Darwin's own theory. After settling the matter of priority (Darwin yielded to Wallace, Wallace to Darwin, both agreed to become cofounders), a joint memoir was prepared and communicated to the Linnean Society by Lyell and Joseph Hooker, a distinguished botanist and close friend of Darwin.<sup>12</sup>

Korey pictures Wallace and Darwin talking it over and *agreeing* to the joint publication! But the "joint presentation" was accomplished in a great hurry, only thirteen days after the arrival of Wallace's paper, while Wallace was halfway around the world. (The publication of the papers followed immediately; Darwin was reading proofs within three weeks.) Wallace was not consulted. But although Korey's account is all wrong, his mistake is understandable: again, it is the kind of blunder that one might make if one were not too attentive to the record, *and* one were assuming the propriety of Darwin's behavior.

### **Darwin's Own View of the Matter**

There is one person who would probably agree that it was a shabby affair: Darwin himself. Darwin was never comfortable with what happened. When he received Wallace's paper, his first reaction was that he should not take advantage of the situation. He should simply write to Wallace, asking whether Wallace would like for Darwin to arrange publication for him. When Lyell proposed the joint presentation, Darwin responded:

I should be extremely glad now to publish a sketch of my general views in about a dozen pages or so; but I cannot persuade myself that I can do so honourably. Wallace says nothing about publication, and I enclose his letter. But as I had not intended to publish any sketch, can I do so honourably, because Wallace has sent me an outline

<sup>12</sup> Kenneth Korey, *The Essential Darwin* (Boston: Little, Brown and Co., 1984), p. 55.

of his doctrine? I would far rather burn my whole book, than that he or any other man should think that I had behaved in a paltry spirit. . . But I cannot tell whether to publish now would not be base and paltry. This was my first impression, and I should have certainly acted on it had it not been for your letter.<sup>13</sup>

Darwin concludes this letter by saying "I will never trouble you or Hooker on the subject again." But he does, the next day, in a letter that again worries the ethics of his situation, and concludes:

It seems hard on me that I should thus be compelled to lose my priority of many years' standing, but I cannot feel at all sure that this alters the justice of the case. First impressions are generally right, and I at first thought it would be dishonourable in me now to publish.<sup>14</sup>

But although his conscience never stopped bothering him about it, Darwin went along with the plan. After the deed was done, Wallace was informed. His reaction, expressed in letters to Darwin and Hooker, was gracious, and Darwin was much relieved. Darwin wrote to Hooker, "I admire extremely the spirit in which they are written. I never felt very sure what he would say. He must be an amiable man."<sup>15</sup> In fact, Wallace was *happy* about the whole affair. His main reaction was to be flattered that Darwin and the others thought so well of his work. Upon learning what had happened, he wrote to his mother:

I have received letters from Mr. Darwin and Dr. Hooker, two of the most eminent naturalists in England, which has highly gratified me. I sent Mr. Darwin an essay on a subject on which he is now writing a great work. He showed it to Dr. Hooker and Sir C. Lyell, who thought so highly of it that they immediately read it before the Linnean Society. This assures me the acquaintance and assistance of these eminent men on my return home.<sup>16</sup>

<sup>13</sup> Darwin to Lyell, June 25, 1858. *Life and Letters*, vol. I, p. 474.

<sup>14</sup> Darwin to Lyell, June 26, 1858. *Life and Letters*, vol. I, p. 475.

<sup>15</sup> Darwin to Hooker, January 23, 1859. *Life and Letters*, vol. I, p. 500.

<sup>16</sup> Wallace to his mother, October 6, 1858. James Marchant, ed., Alfred Russel Wallace: *Letters and Reminiscences* (New York: Harper, 1916), p. 57.

So far as we know, Wallace never considered himself to have been treated badly. He happily acknowledged Darwin's priority, and said many times that Darwin's work was not only older but deeper than his own. He wrote to Darwin in 1864:

As to the theory of Natural Selection itself, I shall always maintain it to be actually yours and yours only. You had worked it out in details I had never thought of, years before I had a ray of light on the subject, and my paper would never have convinced anybody or been noticed as more than an ingenious speculation, whereas your book has revolutionized the study of Natural History, and carried away captive the best men of the present age. All the merit I claim is the having been the means of inducing *you* to write and publish at once.<sup>17</sup>

He was indeed an amiable man. He could have taken a very different view; he could have brooded over what his place in history would have been if he had only sent his paper directly to a publisher, rather than to Darwin. But he did not. He wrote many books, and called one of them *Darwinism*. We can only speculate about whether, if Wallace had sent his paper straight to a publisher, Darwin might not have ended up having to call his book *Wallaceism*.

<sup>17</sup> Wallace to Darwin, May 29, 1864. *Wallace: Letters and Reminiscences*, p. 131.