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WHY DARWIN DELAYED, OR INTERESTING PROBLEMS AND MODELS IN THE HISTORY OF SCIENCE

ROBERT J. RICHARDS

Though Darwin had formulated his theory of evolution by natural selection by early fall of 1837, he did not publish it until 1859 in the *Origin of Species*. Darwin thus delayed publicly revealing his theory for some twenty years. Why did he wait so long? Initially this may not seem an important or interesting question, but many historians have so regarded it. They have developed a variety of historiographically different explanations. This essay considers these several explanations, though with a larger purpose in mind: to suggest what makes for interesting problems in history of science and what kinds of historiographic models will best handle them.

In October of 1836, Charles Darwin returned from his five-year voyage on the *Beagle*. During his travel around the world, he appears not to have given serious thought to the possibility that species were mutable, that they slowly changed over time. But in the summer and spring of 1837, he began to reflect precisely on this possibility, as his journal indicates: "In July opened first notebook on 'Transformation of Species'—Had been greatly struck from about Month of previous March on character of S. American fossils—& species on Galapagos Archipelago. These facts [are the] origin (especially latter) of all my views."¹ Darwin's views on evolution really only began to congeal some six months after his voyage. In the summer of 1837, he started a series of notebooks in which he worked on the theory that species were transformed over generations. In his first, second, and most of his third transmutation notebooks, he constructed several mechanisms, most of a Lamarckian variety, to account for the evolutionary process.² In September of 1838, a bit over a year and a half after he first began to reflect on the meaning of his South American findings, he chanced to read Thomas Malthus's *Essay on Population*; and this, as he related in his *Autobiography*, gave him "an hypothesis by which to work."³ Darwin credited Malthus with having furnished him the key to his formulation of the principle of natural selection—the principle that not only transformed species but also our very understanding of life. But here a problem arises for the historian of science, and it is this problem that I would like to consider.

THE PROBLEM OF DARWIN'S DELAY

Darwin read Malthus in late September of 1838, and his notebooks show that immediately thereafter he had the essence of what has become known as the theory of evolution by natural selection.⁴ Yet he did not publish his discovery in complete form until the *Origin of Species* appeared in 1859, over twenty years later. Certainly he was not slow to recognize the importance of his conception. In 1844 he wrote out a large essay sketching his theory, and had a fair copy made.⁵ (Part of this essay was read, along with a

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paper by Alfred Wallace, before the Linnaean Society in 1858 as the first public announcement of the discovery.) When he had finished the 1844 essay, he made arrangements with his wife for its posthumous publication, in case he should die before revealing his great idea.⁶ Darwin thus harbored few doubts about the significance of his discovery. What, then, caused him to delay publication of a theory that is perhaps the most intellectually and socially important theory of the nineteenth century, and arguably among the most important scientific conceptions of all time?

In discussing this problem I would like principally to do two things: first, to mention the several kinds of explanation that have been given for Darwin's delay, spending some time on one in particular; and second, to consider the reasons an historian of science might tackle a problem such as this—in general to offer a few reflections on the nature of the history of science, its problems, and its methods.

Explanations of Darwin's Delay

Darwin's delay may not seem like an important or historically significant problem. To see why it is, however, suggests that our first inquiry ought to be historiographic: what makes a problem in history of science interesting in the first place? But before touching on this, I would like to outline the various explanations that have been given for Darwin's delay. This will provide some concrete examples for discussing the larger problem of interesting problems.

The first sort of explanation derives from the conventional interpretation of the hypothetical-deductive method in science: it holds that Darwin formulated his hypothesis in 1838 and then set out collecting facts to support it, which took him twenty years. Charles Coulston Gillispie adopts this account in his *Edge of Objectivity*:

[Darwin] was held back from publication, and even from giving himself joyfully to his conclusions, by a fear of seeming premature. This went beyond scientific caution in Darwin. It is, perhaps, a disease of modern scholarship to hold back the great work until it can be counted on to overwhelm by sheer factual mass.⁷

Another explanatory strategy is a variant on the first. It contends that Darwin required the services of several correspondents and associates—among whom were Charles Lyell, Thomas Hooker, and Thomas Huxley—to gather facts for him, since he was ill a good deal of the time after his return to England and, really, was a bit lazy. To coordinate others to do one's bidding while one is indisposed would, of course, take time. Gertrude Himmelfarb, in her *Darwin and the Darwinian Revolution*, adds that Darwin was concomitantly attempting to convince his friends of the truth of his theory, but with little success. She implies that he failed for good reasons, since his theory lacked cogency and his arguments were crude.⁸

A third kind of explanation supposes that Darwin was hardly indolent or lazy. Rather, it was because of his work agenda that he was not able to get to his species book more quickly. Indeed, during the twenty years in question, he brought out: *Journal of Researches of the Voyage of H. M. S. Beagle* (1839 and revised in 1845); five volumes of *Zoology of the Voyage of H. M. S. Beagle* (1840-1843), which he edited; three volumes of the *Geology of the Voyage of the Beagle* (1842-1846); and almost thirty papers and reviews.

In 1846 he began an eight-year study of barnacles, resulting in four volumes completed in 1854.⁹ The barnacle project seduced Darwin. He initially planned merely to do a little study of one species and ended up investigating the whole group of Cirripedia. His work on barnacles has been singled out as both a necessary stage in preparation for the

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Origin of Species and a significant cause of its delay. Thomas Huxley, in looking back on his friend's accomplishment, wrote to Darwin's son Francis: "Like the rest of us, he had no proper training in biological science, and it has always struck me as a remarkable instance of his scientific insight, that he saw the necessity of giving himself such training, and of his courage, that he did not shirk the labour of obtaining it."¹⁰ Thus, so the explanation goes, he had to fit himself out as a real biologist before he felt confident to tackle the species theory.

A fourth explanation points out that at the time Darwin finished the sketch of his theory in 1844, Robert Chambers published, anonymously, his *Vestiges of the Natural History of Creation*. This book advanced an evolutionary hypothesis, but was extremely speculative and often silly—neither trait slipping past the attention of Darwin's scientific community. J. W. Burrow argues that Chambers's book would have cooled any enthusiasm Darwin might have had for quickly publishing his ideas: "Darwin regarded *The Vestiges* as rubbish, and Huxley reviewed it devastatingly, but the fear of being taken for simply another evolutionary speculator haunted Darwin and enjoined caution in announcing his views and patience in marshalling his evidence."¹¹

A fifth explanation looks to the impact Darwin presumably anticipated his theory as having. It was, after all, materialistic; it assumed the rise of human reason and morality out of animal intelligence and instinct. Howard Gruber, in his *Darwin on Man*, divines that "Darwin sensed that some would object to seeing rudiments of human mentality in animals, while others would recoil at the idea of remnants of animality in man."¹² Darwin closed the link between humankind and animals, and thus chained himself to the dread doctrine of materialism. Stephen Gould, supporting Gruber's argument, finds evidence for this reconstruction in Darwin's early notebooks, which

include many statements showing that he espoused but feared to expose something he perceived as far more heretical than evolution itself: philosophical materialism—the postulate that matter is the stuff of all existence and that all mental and spiritual phenomena are its by-products. No notion could be more upsetting to the deepest traditions of Western thought than the statement that mind—however complex and powerful—is simply a product of brain.¹³

The proffered hypothesis suggests, then, that Darwin was acutely sensitive to the social consequences of equating human beings with animals and therefore mind with brain, and that he thus shied from publicly revealing his views until the intellectual climate became more tolerable.¹⁴

The social-psychological approach, of which this last explanation discreetly makes use, is more overtly appealed to in another kind of explanation, the psychoanalytic. Some psychoanalysts emphasize that Darwin suffered from a variety of illnesses during his later adulthood—he was always taking the waters and different kinds of faddish cures for his nervousness, palpitations, exhaustion, headaches, and gastrointestinal eruptions.¹⁵ Anyone examining the letters written to Darwin, from about 1840 till his death in 1882, is struck by what seems their invariable salutation: "Dear Darwin, sorry to hear you've been ill." The analyst Rankine Good interprets Darwin's maladies as neurotic symptoms, expressing an unconscious hate for his father:

His illness was compounded of depressive, obsessional anxiety, and hysterical symptoms which, for the most part, co-existed, though he appears to have gone through phases when one or other group of symptoms predominated for a time. Further, there is a wealth of evidence that unmistakably points to these symptoms as a distorted expression of the aggression, hate, and resentment felt at an unconscious

level, by Darwin towards his tyrannical father. . . . The symptoms represent in part, the punishment Darwin suffered for harboring such thoughts about his father. For Darwin did revolt against his father. He did so in a typical obsessional way (and like most revolutionaries) by transposing the unconscious emotional conflict to a conscious intellectual one—concerning evolution. Thus if Darwin did not slay his father in the flesh, then in his *Origin of Species* and *Descent of Man*, he certainly slew the heavenly Father in the realm of natural history.¹⁶

Hamlet-like, then, Darwin hesitated to commit the symbolic murder of his despised father; he could not quite bring himself to plunge in the knife that the *Origin* represented.

A somewhat less dramatic explanation looks to Darwin's social and professional, rather than filial, relationships. Michael Ruse, in his recent book *The Darwinian Revolution*, sets some previous accounts within a sociological framework. He argues:

The true answer [for his delay] has to be sought in Darwin's professionalism. . . . Darwin was not an amateur outsider like Chambers. He was part of the scientific network, a product of Cambridge and a close friend of Lyell, and he knew well the dread and the hatred most of the network had for evolutionism. . . . When telling Hooker of his evolutionism, Darwin confessed that it was like admitting to a murder. It was a murder—the purported murder of Christianity, and Darwin was not keen to be cast in this role. Hence the Essay of 1844 went unpublished.¹⁷

In order to protect his status as a professional, a status that presumably included defending the faith, Darwin laid down his pen.

INTERESTING PROBLEMS AND MODELS IN THE HISTORY OF SCIENCE

The Context of Interesting Problems

I have mentioned some seven different explanations for Darwin's delay, but not yet the one I wish to propose. Before considering that, let me suggest why a question such as Darwin's delay is historically interesting in the first place. Historians of science, as well as philosophers of science, scientists, and other scholars want to work on interesting problems—not just interesting because of personal idiosyncracies, but problems that are in some sense objectively interesting, interesting in terms of their disciplines.

What, then, makes for an interesting problem in history of science? There are at least three contexts in which a problem can become historically interesting. The first is that of normal expectations. Initially those expectations derive from present circumstances. The historian might note, for instance, that in the contemporary period scientists rush to publish important discoveries, a feature of the modern temper vividly illustrated by James Watson's *Double Helix*. In this light, Darwin's delay becomes puzzling. But most historians do not regard the present context as the controlling one. The question is, what would be the expectation for a mid-nineteenth-century scientist? If it is presumed that Victorian intellectual life ambled at a more leisurely pace or that the social convention for scientists of the period was to publish their big books as the summation of a career's work—the usual practice during the Renaissance—then a solution is had for what turns out to be not a very interesting problem after all. But in Darwin's case, we know that neither of these explanations rings true. He published fairly rapidly and often throughout his career. And consider the keen anguish he felt when he got the letter from Alfred Wallace in 1858 announcing the discovery of virtually the same theory that he had been toiling over some twenty years—this feeling of intellectual emasculation clearly demonstrates that Darwin feared being anticipated as much as any contemporary scientist. The problem of his delay again becomes interesting—in terms both of our

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general expectations for scientific practice and of the professional situation of the nineteenth-century scientist.

A second context determining interest is that of scholarship: if other historians have treated a problem as interesting, ipso facto it becomes so—for the moment at least. In the case of Darwin's delay, scholars have, simply by dint of their explanatory attempts, made it a problem of interest. Anyone undertaking a comprehensive analysis of Darwin's accomplishment must therefore contend with the problem, if only to show that it is historically intractable or actually not very interesting—interesting, that is, in either the first or the third sense I have in mind.

The third context that determines the interest invested in a problem is provided by a particular scientific theory or a nexus of theories constituting a scientific movement. In this context, interest becomes a function both of the importance of the theory, or theories, and of the proximity of the problem to such a reference base. Thus a problem even at the heart, say, of the major theory of an obscure physiologist should hold little interest for the historian of science—unless the theory and problem are representative of some larger and more significant movements in science. Nor should it be of interest to the historian of science *as such* to discover whether Darwin was really neurotic—except that the question bears on the origin and development of his theory of evolution.

This last contextual control implies that the contemporary state of science ultimately fixes those problems of interest for the historian. Some scholars would find this suggestion destructive of the historical ideal, which, they believe, requires the reconstruction of the past only on its own terms, without use of present conceptual resources. To aim for less would be to indulge in Whig history, the unwarranted reading of contemporary ideas, motives, social conditions, and interests into the past.¹⁸ But the historicist ideal can be realized in neither practice nor theory. The historian is ineluctably a product of his or her time and therefore must bring to the study of the past the conceptual equipment of the present. Any historical analysis, explicitly or implicitly, steps off from the present. Every historian of science initially learns, for instance, the contemporary meaning of the concept of science itself, and in its light regressively traces the evolutionary descent of its past embodiments. Of course, the sensitive historian seeks continually to enrich the concept of science, recognizing that though ancient practices and notions evolved into those of the present, they may appear structurally very different—just as eohippus seems worlds apart from the modern horse.

In terms of this third context, Darwin's delay is certainly interesting. For the very fact of delay suggests either something not finished, something left undone for the theory to be logically acceptable, or something about the theory that made it unacceptable in the scientific and social climate of Victorian England. In either case, the problem beckons because it hints that there is something about Darwin's theory that we have not yet considered; and to understand its origins, development, structure, and impact, this something needs to be recovered.

Models in History of Science

Assuming that the historian has an interesting problem—and perhaps now it will be granted that Darwin's delay is interesting—what approach should be taken in attempting to resolve it? Initially, there seem to be two options.

Historians of science seem innately disposed to one of two basic approaches, internalism or externalism. Internalists focus on the development of scientific ideas and theories, tracing their internal logic and conceptual linkages. In extreme form, inter-

nalists treat the historical movement from one set of ideas to another much as Platonic philosophers, weaving together the logical forms of ideas while ignoring their physical and social embodiments. Externalists, by contrast, embed scientific ideas and theories in the human world, in the minds of scientists who move in a variety of interlocking societies. In the extreme, externalists cloak themselves in Durkheim or Freud; they suppose that ideas reflect only social relationships or psychological complexes. Of the several approaches to the problem of Darwin's delay, Gillispie clearly represents the internalist perspective, while Good represents the externalists; the others cluster more or less closely to one of these poles.

Historians disposed toward internalism or externalism specify their tendencies by adopting—usually unreflectively—an historiographic model, in light of which they articulate their subject. In this respect they function much like scientists. For historians, after all, do formulate theories, construct hypotheses, gather evidence, and, of necessity, employ models. Historiographic models comprise sets of assumptions concerning the nature of science, its developmental character, and the modes of scientific knowing. That historians *must* use models can be argued a priori: without antecedent conceptions about the character of science, they would have no idea where to look for their subject matter, nor could they define its limits or determine what evidence would be relevant. That models have in fact been used can be established easily by an empirical survey of histories of science since the Renaissance.¹⁹ So, for instance, a model familiar to most is Thomas Kuhn's paradigm model of science. Gillispie, more traditionally, employs a revolutionary model (not to be confused with Kuhn's conception of scientific revolutions). This model, introduced by historians in the eighteenth century, assumes that a discipline must undergo a fundamental upheaval to put it on the road to modern science—before the revolution (for example, that produced in physics by Galileo) there was not science; afterward scientists gradually laid a path of scientific truth leading right up to the modern age. A more recently formulated model, which has considerable advantage over the others available, is a natural selection model of scientific evolution. It treats conceptual systems as comparable to biological species, and regards this evolution as ultimately determined by a natural selection of scientific ideas against a variety of intellectual environments.

I will not rehearse here the whole litany of models available to the historian of science, but simply point out that some are more congenial to those of internalist temper, others to those of externalist, while a few will appeal to historians whose attitudes about the issue are a healthy mix. It is the latter class of models, the ones suitable to those of hybrid sentiment, that, I believe, will generally be the most successful. This is not merely because extreme positions—that of the hard-headed internalist or the soft-minded externalist—are generally to be avoided. Barry Goldwater once admonished, with some justice, that extremism in the cause of truth is no vice. These starkly restrictive approaches should be avoided because they lead historians down some very dark byways.

The internalists forget that ideas alone are causally impotent—one idea cannot, of itself, generate another. Moreover, the connections among sets of historically developed scientific ideas are not usually logical, at least not in any deductive sense. It is breathing human beings who produce ideas. Ideas become historically linked only by passing through embodied minds, which respond to logical implication and evidentiary support, of course, but also to emotion, prejudice, class attitudes, and, sometimes perhaps, oedipal anxieties. Hence, to deal with their subject—the growth of scientific

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ideas—historians of science cannot neglect the explanatory strategies of social, political, and cultural historians.

Extreme externalists, say of the Durkheimian or Marxian variety, those who interpret scientific ideas as totally determined by social structures (and who seem to ply their trade these days mostly in Edinburgh)²⁰ can be terminally infected, and, if gentlemen, will succumb to a simple *reductio* argument: their thesis of social determinism must also be determined; but why should we listen to those who take a position from extrinsic compulsion instead of relevant good reason? Even the less extreme sorts often forget that the most intimate society to which the scientist belongs and whose attitudes he or she most readily adopts is that of other scientists. Externalists thus usually ignore something that their own assumptions imply: that scientists are enculturated to respond to the logical and objective character of theories and evidence. Demonstrations of logical consistency and empirical confirmation usually bear the most weight, even for the natural philosophers of ages past. This suggests, incidentally, that well-trained historians of science will also know the more detailed workings of the science they profess to chronicle, as well as be apprised of what contemporary philosophers have had to say about the logical character of theories and explanations in science.

Hybrids between the internalists and the externalists enjoy advantage over both. They can adjust their considerations to the structure of the problem with which they are concerned. That is, they will be ready to construe the problem in terms of the internal structure of the science, which should logically be their first step, or in terms of external influences, if the evidence warrants. Usually they will find both approaches, in different measures, necessary. And this for a simple reason, which I will briefly mention and which will return us to the problem at hand, Darwin's delay.

DARWIN'S DELAY AGAIN

In arriving at a possible solution to the perplexity of Darwin's delay, one must recognize a critical difficulty which always faces the historian: scientific theories and the activities of scientists are overdetermined. A multitude of factors impinge on the scientist, and the historian must apportion different conceptual and causal weightings to these factors. It is conceivable, and I think likely, that most of the explanations mentioned earlier for Darwin's delay have some merit. The factors they isolate did bear on his delay. The mistake usually made, however, is to assume that one explanation is *the* explanation. Having offered this caveat, let me suggest which inhibiting factor did cause Darwin no end of difficulty and which, therefore, must be accorded considerable conceptual weight.

In reading several natural theological discussions of animal instinct in the early 1840s, Darwin came upon one particular example that the natural theologians made much of—the “wonderful” instincts of worker bees and slave-making ants. Only God, they argued, could have endowed the hive bee with a geometer's knowledge of how to construct perfect hexagonal cells, or *Formica rufescens* with the gentleman's unerring sense of what other species would make the best domestic servants.²¹ What struck Darwin about these instincts—actually whole sets of related innate behaviors—was that they were exhibited by sterile castes of insects. The account of instinctive behavior on which he had been working in the early 1840s—which likened the fixed patterns of instinct to anatomical structures and argued that both could be explained by natural selection—seemed precluded for neuter insects, since they left no progeny that could inherit profitable variations.

That this quickly loomed as a critical difficulty for the validity of his theory of evolution by natural selection can be fairly estimated from the annotations Darwin left in the

margins of those natural theological treatises he was reading in the 1840s.²² Moreover, in the *Origin of Species*, he stated flatly that he initially thought the problem of instincts of neuter insects "fatal to my whole theory."²³ This was precisely the kind of stumbling block—a conceptual failure at the heart of his theory—that would cause him to hesitate in publishing his views.

Manuscript evidence indicates that Darwin discovered this difficulty in 1843.²⁴ Shortly thereafter he attempted to construct several possible explanations compatible with the theory of natural selection. But these were weak, and he knew it. In his 1844 essay Darwin sketched several potential objections to his theory, and then, with a soft note of triumph, proceeded to answer them. Conspicuously absent, however, was any mention of that difficulty he thought fatal to his theory—he had no explanation for it. Further evidence shows that the problem of neuter insects continued to plague him. In 1848 he composed a four-page manuscript detailing the problem of the instincts of neuter insects, and concluded that it was "the greatest *special* difficulty I have met with."²⁵

Even after Darwin sat down, in 1856, to begin work on a manuscript that would be, he hoped, the definitive description and justification of his theory of evolution by natural selection, he still had not settled on one explanation of the wonderful instincts of social insects. In fact, he proposed several, only one of which contained elements of what we now accept as the correct explanation—kin selection: the idea that selection does not work on the individual, but on the whole hive or nest in competition with other communal groups of the same species. Darwin came to recognize the solution to his difficulty and to flesh it out only in late December of 1857, as he wrote what would become the chapter on instinct in the *Origin of Species*.²⁶ In the very act of writing the chapter, he resolved the difficulty he regarded as threatening the existence of his theory. In the explanation of Darwin's delay, much conceptual weight must thus be given to his struggles with the wonderful instincts of neuter insects. And this, I believe, is a good part of the solution to an interesting problem in the history of science.

NOTES

1. Charles Darwin, "Journal," ed. Gavin de Beer, in *Bulletin of the British Museum (Natural History)*, Historical Series 2 (1959): 7.
2. Gavin de Beer had edited and transcribed "Darwin's Notebooks on Transmutation of Species" and "Pages Excised by Darwin," in *Bulletin of the British Museum (Natural History)*, Historical Series 2 and 3 (1960, 1967). For an account of Darwin's early theories about evolution, see Sandra Herbert, "The Place of Man in the Development of Darwin's Theory of Transmutation," *Journal of History of Biology* 7 (1974): 217-258; 10 (1977): 243-273; David Kohn, "Theories to Work By: Rejected Theories, Reproduction and Darwin's Path to Natural Selection," *Studies in History of Biology* 4 (1980): 67-170; Camille Limoges, *La Sélection Naturelle* (Paris: Presses Universitaires de France, 1970); and Robert Richards, "Influence of Sensationalist Tradition on Early Theories of the Evolution of Behavior," *Journal of History of Ideas* 40 (1979): 85-105.
3. Charles Darwin, *Autobiography*, ed. Nora Barlow (New York: Norton, 1969), p. 120.
4. Darwin, "Third Notebook on Transmutation," MS pp. 134-135 (de Beer, "Excised Pages," pp. 162-163).
5. The essay is transcribed in *The Foundations of the Origin of Species*, ed. Francis Darwin (Cambridge: Cambridge University Press, 1909).
6. Charles Darwin, *The Life and Letters of Charles Darwin*, ed. Francis Darwin, 2 vols. (New York: D. Appleton, 1891), I: 377-379.
7. Charles Coulston Gillispie, *The Edge of Objectivity* (Princeton, N.J.: Princeton University Press, 1960), p. 312.
8. Gertrude Himmelfarb, *Darwin and the Darwinian Revolution* (New York: Norton, 1968), pp. 126-146, 203-215, 312-352.
9. Charles Darwin, *A Monograph of the Sub-Class Cirripedia*, 4 vols. (London: Ray Society, 1851-1854).
10. Thomas Huxley to Francis Darwin, quoted in *Life and Letters of Charles Darwin*, 1:315.
11. J. W. Burrow, "Editor's Introduction," in Charles Darwin, *The Origin of Species* (Baltimore: Penguin Books, 1968), p. 32.

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the existence of his theory. In the ex-
ght must thus be given to his struggles
d this, I believe, is a good part of the
of science.

tin of the British Museum (Natural History),

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evolution, see Sandra Herbert, "The Place of
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Rejected Theories, Reproduction and Darwin's
1980): 67-170; Camille Limoges, *La Selection*
Robert Richards, "Influence of Sensationalist
Journal of History of Ideas 40 (1979): 85-105.

York: Norton, 1969), p. 120.

4-135 (de Beer, "Excised Pages," pp. 162-163).

n of Species, ed. Francis Darwin (Cambridge:

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cton, N.J.: Princeton University Press, 1960),

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ia, 4 vols. (London: Ray Society, 1851-1854).

Letters of Charles Darwin, 1:315.

n, *The Origin of Species* (Baltimore: Penguin

12. Howard Gruber, *Darwin on Man* (New York: Dutton, 1974), p. 202.

13. Stephen Gould, "Darwin's Delay," in his *Ever Since Darwin* (New York: Norton, 1977), p. 24. Silvan Schweber, in "The Origin of the Origin Revisited," *Journal of History of Biology* 10 (1977): 310-315, concurs with Gruber and Gould that fear of materialism was a considerable restraining influence on Darwin.

14. In "Instinct and Intelligence in British Natural Theology: Some Contributions to Darwin's Theory of the Evolution of Behavior," *Journal of History of Biology* 14 (1981): 193-230, I have taken specific exception to this explanation of Darwin's delay.

15. Ralph Colp, Jr., gives an extensive account of Darwin's illness in *To Be an Invalid* (Chicago: University of Chicago Press, 1977).

16. Rankine Good, "The Life of the Shawl," *Lancet* (9 January 1953): 106.

17. Michael Ruse, *The Darwinian Revolution* (Chicago: University of Chicago Press, 1979), p. 185.

18. Whiggish history was carefully diagnosed in Herbert Butterfield's *The Whig Interpretation of History* (New York: Norton, 1965; originally published, 1931). There he described it as "the tendency in many historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasise certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present."

19. I have offered such a survey in "Natural Selection and Other Models in the Historiography of Science," in *Scientific Inquiry and the Social Sciences: A Volume in Honor of Donald T. Campbell*, ed. Marilyn B. Brewer and Barry E. Collins (San Francisco: Jossey-Bass, 1981), pp. 37-76.

20. See, for example, the work of the Edinburgh sociologists of science David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976), and Barry Barnes (*Interests and the Growth of Knowledge* (London: Routledge and Kegan Paul, 1977).

21. Darwin's authority for the habits of social insects was the work of two natural theologians and premier entomologists, William Kirby and William Spence, in their *Introduction to Entomology*, 2nd ed., 4 vols. (London: Longman, Hurst, Rees, Orme, and Brown, 1818); see especially vol. 2.

22. I have discussed these annotations in "Instinct and Intelligence."

23. Charles Darwin, *On the Origin of Species* (London: Murray, 1859), p. 236.

24. From Darwin's reading notebooks, we know that he read Kirby and Spence's *Introduction to Entomology* in 1843 (see the transcription of these notebooks by Peter Vorzimmer, "The Darwin Reading Notebooks [1838-1860]," *Journal of History of Biology* 10 [1977]: 130). On p. 55 of vol. 2 of the work, where Kirby and Spence describe some of the wonderful instincts of worker bees, Darwin scribbled his frustration in the margin: "Neuters do not breed! How instinct acquired." Darwin's books are held in the Manuscript Room of Cambridge University Library.

25. Darwin's four-page manuscript is in container-book #73, held in the Manuscript Room of Cambridge University Library.

26. The manuscript version of what Darwin abridged into the *Origin of Species* has been published by R. C. Stauffer as *Charles Darwin's Natural Selection: Being the Second Part of His Big Species Book Written from 1856 to 1858* (Cambridge: Cambridge University Press, 1975).