The Place of Man in the Development of Darwin's Theory of Transmutation.

Part II

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When I came to the conclusion that after all Lamarck was going to be shown to be right, that we must "go the whole orang," I reread his book, and remembering when it was written, I felt I had done him injustice.

– Charles Lyell, March 15, 1863

The word "place" in the title of this article is used in the double sense of ordering and judging, for I hope to show at what juncture the question of man entered Darwin's speculations and to estimate the importance of this line of inquiry when measured against other elements in his development of transmutationist theory. When contrasted with the ultimate ends of scholarship in this area, these are modest aims, so much so that they need to be placed in the context of other work in the history of evolutionary theory. Minimal goals in the study of the history of evolutionary science include unified treatment of the contributions of the major actors, including Darwin, and, for the theory as a whole, a similarly unified account of its development in relation to that of the sciences, both natural and social. To some extent, these goals have been accomplished. The availability of new manuscript sources, however, combined with recent appreciation of the complex origin of evolutionary theory, has outdated the general accounts written a decade or more ago.[1] What is required now, and what scholars are producing, is a series of more narrowly focused studies which will form the basis of new general interpretations of the history of evolutionary biology. This essay is one such study. While concerned with a short period in the life of a major figure, it is written with the histories, written or potential, of the careers of his contemporaries in mind. Consequently, while this assessment must stand or fall on its own merits, its full value is best determined

1. The best general works remain Loren Eiseley, *Darwin's Century: Evolution and the Men Who Discovered It* (Garden City, N.Y.: Anchor Books, 1961), and John G. Greene, *The Death of Adam* (Ames, Iowa State University Press, 1959). For a recent survey of the field, see John C. Greene, "Reflections on the Progress of Darwin Studies," *J. Hist. Biol. 8*, no. 2 (Fall 1975), 243-273.

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by its consistency with more general reformulations of the entire history of evolutionary theory.

In Part I of this study (*Journal of the History of Biology, 7,* Fall 1974, 217–258), I emphasized Darwin's professionalism as represented by his commitment to science as a vocation and his dependence on the activities and judgments of his coworkers. This emphasis will continue in Part II. For the subject of man, this is a more difficult approach than it was for zoology, since the avenues for scientific study were less well defined. While anatomy and physiology were commonplace subjects in medicine, the fields of anthropology, sociology, and – despite its richer past – psychology, did not yet have disciplinary status. Further, by the standards of method and organization, neither the classical approaches of philosophy and theology nor the newer tradition of political economy could be called predominantly scientific, though each had its scientific aspects. Very crudely, with room left for extensive qualification, and leaving medicine aside, one can say that man as a subject had not yet fallen under the domain of science by the 1830's. How then, if Darwin were intent on making his way only within science, could he have concerned himself with the subject?

The answer lies in the new directions Darwin took following his conversion to a transmutationist position in the spring of 1837. As I have already pointed out, Darwin had paid relatively little attention to the subject of man before that time. Subsequently, his interest in man increased dramatically. At one level, this change hardly requires explanation, for transmutationist views had always been seen, correctly, as requiring serious adjustment in traditional views concerning man's nature and origin. At this level, Darwin's new interest in man was a matter of his making emotional and intellectual adjustments to new views in other areas. These adjustments not only explain why he took up the subject of man, but also account for certain questions he felt required to address, as, for example, the continuities and discontinuities between man and other forms of life. But they do not explain two striking features of Darwin's approach to man, namely, why he consistently underestimated the importance of his pursuit of social and philosophical issues to the initial formulation of his theory, and why he waited so long before publishing directly on man. As evidence for the first claim, one may point to the passage infn Darwin's *Autobiography*[2] where he

2. Speaking of the period from October 2, 1836, the date of his return to England, to January 29, 1839, the date of his marriage, Darwin wrote, "As I was not able to work all day at science I read a good deal during these two years on various subjects, including

minimized his gain from his philosophical inquiries; as evidence for the second, the late dates of *The Descent of Man* (1871) and *The Expression of Emotions in Man and Animals* (1872). To explain these puzzling features of Darwin's handling of the human issue, it is necessary to broaden the scope of this essay by first considering Darwin as he considered himself, that is, within the context of contemporary science. Once the discipline-inspired rules governing Darwin's presentation of himself and his work are appreciated, his superficially disingenuous treatment of the human subject can be seen as consistent with his overall pattern of activity within science. At this point, one can look beneath his public presentation of himself and his work to see the actual relationship which existed between his development of transmutationist theory and his pursuit of topics relating to man. In outline form the argument is organized as follows:

1. Darwin as a theorist. In this section Darwin's importance as a theorist is asserted and the features of contemporary science affecting the presentation of himself in that role are considered.
2. The development of the theory of natural selection with respect to contributing traditions of research. In this section Darwin's transmutation notebooks are treated as representing three semiautonomous traditions of inquiry. Circumstances described in section 1 affecting the publishability of work in these three traditions will be applied to account for Darwin's own pattern of publication.
3. The subject of man in Darwin's notebooks. The argument will follow his activity chronologically, emphasizing three general features: the terms of his ascending interest in man, the coincidence of his interest with his identification of his own prospects as a scientist, and the influence of his pursuit of issues related to man on his general theory.

1. DARWIN AS A PROFESSIONAL SCIENTIST: THE LACK OF A ROLE FOR THE THEORIST

The key to comprehending Darwin with respect to his role in the rise of biological science during the nineteenth century lies in regarding him as a theorist. Beginning from this point allows one to interpret his work

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some metaphysical books, but I was not at [all] well fitted for such studies." Nora Barlow, ed., *The Autobiography of Charel Darwin* (London: Collins, 1958), pp. 84-85. This statement, while not a literal falsehood, is misleading in that it does not suggest the extent of Darwin's involvement with "metaphysics" during this period or indicate the use it was to him.

without distortion. Beginning anywhere else involves one too readily in lesser or extraneous issues. Such an interpretation is an absolute requirement for the period under discussion, 1837-1839, when Darwin accomplished the greater part of the theoretical work on which his reputation rests. But in choosing to address Darwin chiefly as a theorist, one must face the fact that neither his contemporaries nor the majority of historians have taken so straightforward an approach. Periodically, Darwin's theoretical abilities have even been denied – one recalls here Jacques Barzun's sorry characterization of Darwin as a "poor joiner of ideas."[3] Now in order for Darwin to be addressed as a theorist, it must be agreed that he possessed theoretical abilities. The most obvious means of assuaging doubts on this score is to point to the ultimate adoption of his theoretical position by scientists. With more grace, one can also demonstrate Darwin's theoretical acuity by analyzing his methodological consistency[4] or by describing the structure of his finished theory.[5] Yet defending Darwin's theoretical abilities by these means satisfies the modern reader's doubts without accounting for past misconceptions concerning the theoretical nature of Darwin's contributions. Even now, one speaks more easily of the theory of evolution through natural selection than of Charles Darwin, *theorist*.

At this point one must confront the reality that if Darwin has not received full credit as a theorist, this has been due less to insufficient respect for his ability than to the fact that he did not choose to present himself in so clear a light. Occasionally, he described his work in terms which in retrospect seem disingenuous to the point of falsehood, as, for example, when he observed in his autobiography that "My first notebook was opened in July 1837. I worked on true Baconian principles, and without any theory collected facts on a wholesale scale."[6] In such

3. Jacques Barzun, *Darwin, Marx, and Wagner: Critique of a Heritage*, 2nd ed. (Garden City, N.Y.: Anchor Books, 1958), p. 74.

4. For a persuasive presentation of Darwin's consistency in formulating and testing hypotheses, see Michael Ghiselin, *The Triumph of the Darwinian Method* (Berkeley: University of California Press, 1969). For a different treatment of Darwin's method, see Martin Rudwick, "Darwin and Glen Roy: A 'Great Failure' in Scientific Method?" *Stud. Hist. Phil. Sci., 5*, no. 2 (1974), 97-185. Rudwick is also concerned with methodological consistency, but finds that consistency in repeated characteristic styles of argument rather than reliance on a standard method.

5. See, for example, Michael Ruse, "Charles Darwin's Theory of Evolution: An Analysis," *J. Hist. Biol., 8*, no. 2 (Fall 1975), 219-241.

6. Barlow, ed., *Autobiography*, p. 119. For more positive expressions of Darwin's attitude toward theoretical work, see the *Autobiography*, pp. 140–141, and his letters to Henry Fawcett (Sept. 18, 1861) and John Scott (June 6, 1863) in Francis Darwin, ed., *More Letters of Charles Darwin*, 2 vols. (New York: D. Appleton, 1903), I, 195, and II, 323.

statements Darwin cast himself in the role of the disinterested scientific investigator working without theoretical direction. At other times Darwin was more equivocal, but rarely did he represent himself plainly as a theorist. Clearly, his own ambivalent portrayal of his role in science contradicts that which we have assigned him. The question then becomes whether one should take this conflict seriously, assuming that it did exist, or dismiss it as of minor importance.

The answer to this question depends on what one sees as problematic in Darwin's work overall. If one views Darwin as a single individual struggling against the prejudices of a hostile public, then his primary presentation of himself as a disinterested observer is true enough in the largest sense, and the question of the theoretical nature of his contribution assumes secondary importance. But if one sees Darwin in a social setting, as one of a group of men engaged in common activities and working toward common ends, then his reluctance to claim the role of the theorist poses a serious difficulty. Was Darwin unaware of the nature of his own contribution? Was he only modest? Or was he consciously misrepresenting himself?

One can pursue these questions indirectly by identifying the nature of the exterior world which governed Darwin's presentation of himself and his work. Taking this indirect tack requires one important assumption: that, where he could, Darwin chose to present himself and his work in a manner in which he and it would be comprehended and accepted. One assumes, then, that Darwin was a self-conscious actor. That done, one can hope to appreciate how Darwin's pattern of work – on man in this case – was affected by the intellectual setting in which he operated.

The chief determinant of Darwin's presentation of himself was the expectations of the audience he wished to address. That audience was composed of individuals who, like himself, were committed to pursuing science seriously, rigorously, and continuously, in short, as a career. Without the simple certification offered by university science degrees, it was an audience for which entrance requirements were uncertain. And without clear entrance requirements, the boundaries of the audience fluctuated. Indeed, part of the appeal for the historian in studying this period in science stems from the diversity in career patterns produced by this uncertainty.[7] But for all the uncertainty about its boundaries, the

7. For an example of the difficulty in determining who should be counted as within science, see the discussion of William Webster, an interesting unknown, in Sandra Herbert, "The Place of Man in the Development of Darwin's Theory of Transmutation, Part I to July 1837," *J. Hist. Biol.*, *7*, no. 2 (Fall 1974), 249-257.

scientific audience in the 1830's was a real group with powerful advantages to confer. Internally, the benefits were chiefly practical ones of communication, for being recognized as a member gave one easy access to other members. Darwin's quick and brilliant disposition of his collections, discussed in Part I of this article, is a good instance of the uses to which the group might be put with respect to internal communication. Externally, the advantages, though less immediate, were equally valuable, since the specialized audience served to mediate between the individual practitioner (or scientist, if one can use the term slightly anachronistically) and the general public. This mediation might be one of encircling the individual with respect: the royal chartering of scientific societies contributed to this end. It might be one of gaining leverage: small scientific societies, linked to the Royal Society of London, spoke with an authority all out of proportion to the number of members on their rolls. Or it might entail showing strength by combination: the activities of the British Association for the Advancement of Science are noteworthy here. Whatever the form of mediation, however, the result was to create a restricted audience for science which interposed itself between the individual practitioner and the general public. It was this audience to which Darwin addressed himself.[8]

Of his contemporaries, Darwin was one of the most extreme in this respect. Just as one can define the scientific community broadly or narrowly, so one can expand or restrict the definition of the scientific audience. The audience (and community) which Darwin recognized was narrowly defined. At first this may appear a contentious claim since the natural history tradition, in which Darwin's *Journal of Researches* stands, was very broad, and since, in later life, Darwin had much to do with breeders whose work lay outside of science proper. Yet closer inspection of his record of publication suggests that he preferred the more select audience when given a choice – for example the Geological Society of London over the British Association for the Advancement of Science. Furthermore, he rarely addressed himself to the public on scientific subjects. Where a Lyell or a Huxley engaged in public lectures, Darwin was silent. Indeed, he was uncomfortable with colleagues who went too easily to the public and uncharacteristically harsh with those who willingly converted scientific reputation to public standing. Such is the meaning of his sharp words against Charles Lyell and Roderick

8. It should be pointed out that this characterization of the scientific audience in England during the 1830's is consistent with that drawn by J. B. Morrell in "Individualism and the Structure of British Science in 1830," *Historical Studies in the Physical Sciences, III* (Princeton: Princeton University Press, 1971), 183-204.

Murchison in his autobiography.[9] Darwin's motivation for this extreme caution in approaching the public can only be surmised. Personal modesty, satisfaction with inherited social status, sensitivity to the potentially controversial nature of the subject matter, and family recollections of the erratic public reception of Erasmus Darwin's works all may have figured.[10] Whatever the cause, the effect was to leave Darwin open to the criteria set by a narrowly defined scientific audience and closed to those of any other.

In adopting the term "audience" to describe Darwin's presentation of himself, one must exercise some caution. Describing relations in science using terminology drawn from the stage can obviously be done more readily for popular science, where the theatrical metaphor can be imposed with less strain, than for the restricted tradition of established

9. Barlow, ed., *Autobiography,* pp. 101, 103. There are cases where Darwin did take a public position on public issues. Early in his career he cosigned a letter with Robert Fitzroy, captain of the *Beagle,* defending missionary activities in Tahiti. See their joint contribution in the *South African Christian Recorder, 2,* no. 4 (Sept. 1836), cited in Nora Barlow, ed., *Charles Darwin and the Voyage of the Beagle* (New York: Philosophical Library, 1946), p. 131. Later in his career, he was somewhat more free with the use of his name. For example, he actively and publicly opposed the anti-vivisectionists. On this see Richard D. French, *Anti-Vivisection and Medical Science in Victorian Society* (Princeton: Princeton University Press, 1975). But usually Darwin enschewed the public forum, relying instead on the internal development of science to serve as the instrument of public enlightenment. He made this point very clearly in 1880 when he denied an author permission to dedicate a book to him which dealt with the subjects of science and religion: "Moreover though I am a strong advocate for free thought on all subjects, yet it appears to me (whether rightly or wrongly) that direct arguments against Christianity and theism produce hardly any effect on the public, and freedom of thought is best promoted by the ["gradual" added] illumination of ["the" deleted, "men's" added] minds, which follow from the advance of science. It has, therefore, been always my object to avoid writing on religion, and I have confined myself to science" (letter of October 13, 1880). The recipient of the letter, once thought to be Karl Marx, has been shown to be Edward Aveling. For a transcription of the letter, see Lewis S. Feuer, "Is the 'Darwin-Marx Correspondence' Authentic?" *Annals of Science, 32,* (1975), pp. 2-3. For a reconstruction of the correspondence and a re-examination of authorship see Feuer, *ibid.*, pp. 3-10; also a group of complementary articles by Feuer, P. Thomas Carroll, Thaddeus J. Trenn, and Ralph Colp, Jr. published under the heading "On the Darwin-Marx Correspondence" in *Ann. Sci., 33,* no. 4 (1976), 383-394. These scholars have authenticated the first Darwin-Marx letter of October 1, 1873, wherein Darwin thanked Marx for a copy of the first volume of *Das Kapital,* and have positively identified Edward Aveling as the recipient of the letter quoted above.

10. Erasmus Darwin's works, popular when published, came under severe attack in the conservative mood which swept England following the French Revolution. This reversal of opinion is discussed in Norton Garfinkle, "Science and Religion in England, 1790-1800: The Critical Response to the Work of Erasmus Darwin," *J. Hist. Ideas, 16* (June 1955), 376-388. Anticipating public sensitivities, Erasmus at one time considered publishing

science.[11] Yet the increasing professionalization and specialization of science during the nineteenth century created an interesting situation from a theatrical perspective, in that all members of the most select scientific audience came to double as performers. Under one guise an individual practitioner was a performer meeting the expectations of a critical audience; under another guise this same individual served as a member of that audience, judging the performance of others. Borrowing an analogy drawn by Steven Shapin,[12] one can compare the reciprocity between performer and audience in professional science to that between

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anonymously, though not for political reasons. While completing *The Botanic Garden*, part 2, he wrote to his publisher, "I would not have my name affixed to this work on any account, as I think it would be injurious to me in medical practice, as it has been to all other physicians who have published poetry" (Erasmus Darwin to J. Johnson, May 23, 1784, Manuscripts Department, University of Virginia Library). But whatever his worry over the reception of his work, it did not occur to him to seek cover in the newly organizing societies devoted to natural history. Their botany was not his botany, despite his advocacy of the Linnean system, and they met in London besides. On his complete lack of interest in the Linnean Society, see A. T. Gage, *A History of the Linnean Society of London* (London: Taylor and Francis, 1938), p. 19.

11. Recent works which consider the broad tradition of science from the point of view of its audience include: Terry Parssinen, "Mesmeric Performers and Their Audience," paper delivered at the History of Science Society meeting in Atlanta, December 30, 1975; Steven Shapin, "The Audience for Science in Eighteenth Century Edinburgh," *Hist. Sci.*, 7 (1974), 95-121; Shapin, "Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh," *Ann. Sci., 32.* (1975), 219-243; and Arnold Thackray, "Natural Knowledge inCultural Context: The Manchester Model," *Amer. Hist. Rev., 79* (June 1974), 672-709. For an exemplary contribution which considers a single actor in an elite tradition from a similar perspective, see Richard W. Burkhardt, Jr., "Lamarck, Evolution, and the Politics of Science," *J. Hist. Biol., 3,* no. 2 (Fall 1970), 275-298. How one characterizes the scientific audience is, of course, a difficult question. For suggestions on this topic, see Arnold Thackray and Steven Shapin, "Prosopography as a Research Tool in the History of Science: The British Scientific Community, 1700-1900," *Hist. Sci., 7* (1974), 1-28. Unlike Thackray and Shapin, however, I have considered the question of scientific audience not as a numerical one (for which prosopography is a method of obvious utility), but as one of human action. From this perspective, the theatrical metaphor, from which the concept of audience derives, is a rich one indeed. For its most adaptable expansion, see Erving Goffman, *The Presentation of Self in Everyday Life* (New York: Doubleday, 1959).

12. Steven Shapin, "The Audience for Science in Eighteenth Century Edinburgh," p. 96: "If another metaphor is required, it would be that of charades, where performers and audience can reverse roles and are engaged in an activity with common goals, rather than, for example, the cinema, where the audience merely holds the options of attending or not; applauding, hissing or remaining silent." While the audience of Shapin's concern were the landed classes of Edinburgh who patronized (and influenced) the doings of science in that city, the metaphor fits just as well, perhaps better, the situation of reciprocal influence and mutual dependence found within professional science.

players in a game of charades: in each the performer relies on the audience for interpretation, while the audience looks to the performer for new material on which rests the continuation of the game. In science, and in the game, the performer is directed by the situation away from free self-expression and toward that which meets his audience's capacity for comprehension and in science at least, for approval. But here the analogy fails, for while the rules for expression are fixed in the game, they are fluid in science, especially in a period of change like the early nineteenth century. Further, while the audience in charades is clearly marked, the audience for professional science (and here the historical period must be specified) is less so. Yet if the analogy is incomplete in these two respects – the fixity of the rules and the delineation of the audience – it is still suggestive, for the basic requirement of the situation, the fit between performer and audience, remains throughout. What I should like to do now is to refine the analogy in such a way that it will allow me to explain Darwin's reluctance to present himself as a theorist.

The changing relation between scientific audience and performer as it affected Darwin's presentation of himself was primarily the result of two developments on the English scene: the emergence of philosophy of science as a discipline[13] and the recent founding of specialized societies devoted to the study of botany, geology, and zoology.[14] Apart from their effect on content, these developments altered social forms for expression within science. That is, the presence and activities of philosophers of science and specialized societies served, though often indirectly, to set standards on such secondary matters as the organization of scientific papers, the definition of evidence, the nature of proof, and th like. One such formal social aspect affected by the work of the philosophers and the societies was the public and professional understanding of what constituted a correct scientific posture. By scientific posture is meant the manner in which a practioner of science presents himself to his audience, including how he characterizes his function within the scientific enter-

13. For a discussion of the relationship of the new discipline to Darwin's work see David Hull, "Charles Darwin and Nineteenth-Century Philosophies of Science," in Ronald N. Giere and Richard S. Westfalls, eds., *Foundations of Scientific Method: The Nineteenth Century* (Bloomington: Indiana University Press, 1973) pp. 115-132; and Alvar Ellegård, *Darwin and the General Reader: The Reception of Darwin's Theory of Evolution in the British Periodical Press, 1859-1872* (Göteborg: Universitets Årsskrift 1958), chap. 9.

14. The Linnean Society of London was founded in 1788, the Geological Society of London in 1807, and the Zoological Society of London in 1825.

prise.[15] Assuming a correct posture is thus a means by which the scientific performer establishes trust between himself and his audience. Yet, unfortunately for those in natural history who were theoretically inclined, the accepted scientific posture in the 1830's was not that of the theorist. Thus the theoretical Darwin of the late 1830's was forced to mask his own interest under the guise of being a more ordinary practitioner of science than he indeed was. As an apt performer, he presented himself, and his work, in conformity with the expectations of his audience. Those responsible for the situation which required this misrepresentation were not hostile to science. On the contrary, they were those very philosophers of science and specialists who, looking backward to what they saw as a period of excessive speculation, saw their own cautionary attitude toward theory as salutary and conducive to orderly growth in science. Their attitudes require elucidation.

The opinions of the philosophers of science in England during the 1830's, as they pertain to the performance of Charles Darwin, can adequately be represented by those of one man - William Whewell.[16] Whewell is important for his place in his own discipline and for his characterization of the scientific process at a time when men who were ambitious to advance that process required guidance. While Whewell, a philosopher, did not recommend a correct scientific posture, his views can be inferred from his general discussion of the elements of scientific knowledge. Whewell had a Kantian appreciation of the mind as a constituent element in the creation of knowledge and a corresponding respect for the role of theory in science. Yet, rather surprisingly, his positive attitude toward theory did not support the creation of a permanent place for the theorist. The reasons "theory" did not translate into "theorist" with ease come from both the historical and the systematic sides of his philosophy. On the historical side, the obstacle derived

15. For a treatment of the larger question of the scientist's role within society, see Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs, N.J.: Prentice-Hall, 1971).

16. John Herschel is the only other candidate whom one might want to consider. But Whewell was aligned with Herschel in general approach and far more expansive. Michael Ruse fairly treated them together in "Darwin's Debt to Philosophy: An Examination of the Influence of the Philosophical Ideas of John F. W. Herschel and William Whewell on the Development of Charles Darwin's Theory of Evolution," *Stud. Hist. Phil. Sci. 6*, no. 2 (1975), 159-181. Ruse discusses Darwin's contacts with Herschel and Whewell in detail and suggests that Darwin's search for fundamental laws governing species was motivated by his reading of their works. While I am not opposed to crediting Herschel and Whewell with great influence over Darwin, I believe that the influence was not always positive, as, for example, with respect to Darwin's characterization of the nature of his own activity.

from Whewell's developmental image of the growth of science. According to Whewell, different orders of mental activity appropriately characterized successive periods in the history of each science.[17] In everyday language, concern with facts (hence empirical work) characterized the early stages of growth of a science, concern with fundamental laws (hence theorizing) its mature stage. Because of this implicit periodization of activity, there could be no continuous theoretical function appropriately exercised throughout the history of a science. Moreover, Whewell was explicit concerning geology, the one science he shared with Darwin: geology had not developed to the stage where general theory was justified.[18] In arriving at this conclusion, Whewell constructed somewhat arbitrary analytical categories, but used them consistently to make his point.[19] From the historical side, then, Whewell's work did not underwrite the role of the theorist, and certainly not in geology.

On the abstract philosophical level, the deeper reasons for this

17. For Whewell each science developed sequentially in three stages: a period of preliminary exploration, an "inductive epoch" where the central idea of a science emerged, and a period of consolidation. While men's minds were active during all these periods (speculation being a permanently human characteristic), general or deductive reasoning was appropriate only after a science had passed through its inductive epoch. Out-of-sequence deduction led to what Whewell called "school philosophy," which halted the progress of the science. William Whewell, *History of the Inductive Sciences,* 3 vols. (London: John W. Parker, 1837), I, 12-13, 15-16, 18-19. See also Walter F. Cannon, "William Whewell, F.R.S., Part II; Contributions to Science and Learning," *Notes and Records Roy. Soc. London, 19,* no. 2 (1964), 176-191.

18. Whewell, *History of the Inductive Sciences,* II, 621-622. "It has been stated, that when the Geological Society of London was formed, their professed object was to multiply and record observations, and patiently to await the result at some future time; and their favourite maxim was, it is added, that the time was not yet come for a general system of geology. This was a wise and philosophical temper, and a due appreciation of their position. And even now, their task is not yet finished; their mission is not yet accomplished. They have still much to do in the way of collecting facts; and in entering upon the exact estimation of causes, they have only just thrown open the door of a vast labyrinth, which it may employ many generations to traverse, but which they must needs explore, before they can penetrate to the oracular chamber of Truth." Whewell expressed the same thought another way when he remarked that physical (i.e., in his terms theoretical) geology had not yet found its Newon (ibid., p. 596).

19. Whewell separated geology into three aspects: (1) descriptive or phenomenal geology, (2) geological dynamics, and (3) theoretical or physical geology. Whewell did not consider geology to have arrived at the third stage. Under this heading, he discussed unacceptable general theory, which, in keeping with his developmental schema, he labeled premature rather than false.

conclusion become more apparent.[20] First, the essential problem existed of translating between philosophical terms and what amounted to occupational descriptions. "Theory" referred to an intellectual construct, "theorist" to a human actor. To proceed from "theory" to "theorist" required that a distinction be made between the activity of theory making and other scientific activities, for example, the finding of facts or, conceivably, the performing of experiments. Such distinctions Whewell was reluctant to make. On the contrary, he regarded the activities of fact finding and theory making (his standard division) as flowing into each other, just as, in his view, the categories of fact and theory flowed into each other. For Whewell what distinguished fact from theory was not its source but its certainty. Hence he could remark aphoristically, "A true Theory is a fact, a Fact is a true theory."[21] If fact and theory were seen as continuous with each other, as Whewell's remark suggests, the pursuit of one could not be meaningfully separated from the pursuit of the other. Thus Whewell's notion of fact and theory, on which much of his philosophy of science depended, dit not lend itself to a distinction in social terms between the role of the theorist and some other role.

An added feature of Whewell's notion of theory which impeded its translation into occupational terms was his association of the arrival at theory with the act of discovery. The *History of the Inductive Sciences* was written around discoveries in the theoretical realm, Newton's arrival at the theory of universal gravitation being the "greatest discovery ever made."[22] Whewell recognized the emphasis by titling the third part of his systematic work in the philosophy of science *On the Philosophy of Discovery*.[23] While this use of discovery to describe the

20. Whewell began publishing systematic works in the philosophy of science beginning in the 1840's, too late to have affected Darwin in the period under discussion. I mention points drawn from these works only as they explain positions taken in the *History of the Inductive Sciences.* Even here reference to Whewell's philosophical works is highly selective. In the interests of economy, I have not addressed directly the major issue of Whewell's inductionism, though I believe it would support my general line of argument if fully treated.

21. William Whewell, "On the Fundamental Antithesis of Philosophy" (read Feb. 5, 1844, *Trans. Camb. Phil. Soc., 8,* pp. 170-181, in Robert E. Butts, ed., *William Whewell's Theory of Scientific Method* (Pittsburgh: University of Pittsburgh Press, 1968), p. 59.

22. Whewell, *History of the Inductive Sciences,* II, 180.

23. William Whewell, *On the Philosophy of Discovery; Chapters Historical and Critical Including the Completion of the Third Edition of the Philosophy of the Inductive Sciences* (London: John W. Parker & Son, 1860). Robert E. Butts has characterized Whewell's theory of induction, the central feature of his system, "as a kind of process of discovery rather than of proof or argument." See Butts, ed., *William Whewell's Theory of Scientific Method,* p. 20.

arrival at true theory was consonant with Whewell's appreciation of the role of the active intellect in the apprehension of truth, it did not translate well into a description of daily activity. One could hardly describe oneself, either then or now, as a "discoverer" without being thought presumptuous. In any case, for Whewell one became a discoverer (and by implication a theorist) only after the event. Theory was something one had more than something one did. Whewell was thus in the position of defending theory while using the term in such a way as to imply the impropriety or inutility of its self-conscious pursuit. Because of this ambivalence, Whewell's philosophy in both its historical and systematic aspects inhibited the translation of the notion of theory into social terms.

An equally qualified attitude toward the theorist existed in the professional society with which Whewell and Darwin were most closely associated, the Geological Society of London. In the Society the permanent existence of theoretical activity was not denied, but the Society did what it could to place the construction of theory outside its official program of research. Justification for this position derived from the orthodox understanding within the Society of the historical role played by theory in the development of the science. According to the common wisdom, geology had been afflicted with a superabundance of theory until the early years of the century, when under the leadership of the Society, the subject had been reoriented toward the cooperative collection of fact.[24] These collected facts were to form the basis for the future establishment of the subject on solid theoretical grounds.[25] In the

24. For an excellent account of the Society's definition of itself as a repository for the cooperative collection of geological facts, see Martin Rudwick, "The Foundation of the Geological Society of London: Its Scheme for Co-operative Research and Its Struggle for Independence," *Brit. J. Hist. Sci., 1,* no. 4 (1963), 333-336. For the governing statement by the Society on its goals, see "Preface," *Trans. Geol. Soc. London, 1* (1811), v-ix. I have not used the term Baconian to describe the Society's intentions in order to avoid the question of the actual views of Bacon with respect to theory. If, however, one means by Baconian simply the organized gathering of relevant facts, the term fits the program of the Society quite nicely.

25. To wit, the opinion of W. D. Conybeare on the subject of the relationship of fossil species of identical formations in different parts of the world: "To India and to Australia, however, it is that we must look, no less than to America, with full confidence that we shall speedily thence obtain sufficient evidence on all these fundamental questions to afford us a basis of induction sufficiently extensive and firm to enable us, at no distant period, steadily to lay the foundation, and securely to raise the superstructure of an enduring and general geological theory." W. D. Conybeare, "Report of the Progress, Actual State, and Ulterior Prospects of Geological Science," *Rep. Brit. Assoc. Adv. Sci.* (York, 1831; Oxford, 1832), p. 413.

meantime, individuals were free to express their own opinions on theoretical matters, being clear that they, not the Society, were responsible for them.[26] In this simple view of things, the permanently valuable collective production of the Society was observational and descriptive in nature, the transitory individual contribution theoretical. The lesson in this story for the young geologist was to avoid general theory while building up the Society's supply of usable fact. This, at any rate, is how Darwin read the situation.[27] Obviously this advice did not enhance the establishment of a rolé for the theorist within the Society, for as long as theory (even more large-scale or general theory) was regarded as an individual effort, it did not have the same standing within the organization as the empirical researches, which were regarded as part of a collective effort. The role of the theorist thus did not receive official sanction in relation to the Society's definition of its own work.

26. The instructions on this point in the first volume of the Society's *Transactions* are significant: "In the present imperfect state of this science, it cannot be supposed that the Society should attempt to decide upon the merits of the different theories of the earth that have been proposed. In the communications, therefore, which are now submitted to the public, every latitude has been allowed to authors, with regard to their theoretical inferences from the observations which they record; it being understood, according to the rule of Literary and Philosophical Societies in general, that the writers alone are responsible for the facts and opinions, which their papers may contain." "Preface," *Trans. Geol. Soc. London, 1* (1811), viii-ix. Elsewhere in the Preface, facts are represented to be the common property of the Society, in effect making only opinion an individual possession.

27. Darwin was aware that opinion in the Society leaned against the elaboration of general theory. For example, at the end of a long set of notes, probably written in 1837, on the subject of his theory of the earth's crustal movement, he remarked rather defensively that, "Those who do not think we are bound to endeavor to account for the structure of the earth by what they see, they will think this argument based on slight grounds, but I hope others will at least grant that it is necessary to offer some solution of so great a difficulty, [which] has been overlooked." Darwin MSS, vol. 42, University Library, Cambridge.

With respect to the Society's attitude toward theory, it is also interesting to note how one of Darwin's papers was treated by the Society when it was submitted for publication in the *Transactions*. Adam Sedgwick in reviewing the paper treated its theoretical component with respect but reminded the author in an offhand way, as though it were general knowledge, that theoretical opinions were the property of the individual. Sedgwick's comment reads: "The concluding or theoretical part is not all clearly brought out, and might be reconsidered by the author with some advantage: Not with any view of altering his theoretical opinions (for he only is responsible for them) but for the purpose of making them more definite and univocal." Referees Reports, Geological Society of London Archives, Burlington House. Darwin's paper appeared in print as "On the Connexion of Certain Volcanic Phaenomena, and on the Formation of Mountain-Chains and the Effects of Continental Elevations," *Trans. Geol. Soc. London, 5* (1840), 601-632.

Of course, the historical view which made theoretical labors seem an individual option was mistaken. Clearly, many of the questions being asked in the 1830's pertaining to the collection of facts had been generated by theoretical considerations.[28] In emphasizing the power of fact to undermine theory, the common wisdom had ignored the complementary power of theory to elicit fact. And as if in response to the actual inclination toward theory in geology during the 1830's, practical adjustments were made to the common wisdom. First, the false notion of the possibility of purely empirical research was attacked by a leading member of the Society, himself the author of a highly theoretical treatise.[29] Second, de facto professional recognition accrued to theoretical investigations in a wide range of areas so long as they appeared within the context of empirical researches. The geologists had not rid themselves of "theories of the earth" (see note 26), but they had legislated how they might be presented. Thus in the 1830's the practical situation allowed the necessary latitude for the expression of theoretical views, and Darwin, like others, was free to publish within the limits of the rules.[30] But a de facto license to express theoretical opinion

28. William Conybeare admitted as much by defining the "fundamental facts" of geology as those bearing a relation to certain "questions at the very root of any sound geological theory." See his "Report on the Progress, Actual State, and Ulterior Prospects of Geological Sciences," pp. 375, 376.

29. Criticizing those who sought to confine geology to factual description, Charles Lyell remarked in a lecture at King's College in 1833, "But they could not . . . disconnect the ordinary language which they made use of from theoretical views, and hence there was a manifest inconsistency between their professions and their practice, [and] they added. . . an additional source of prejudice - a determination to have no theory." Quoted in Martin Rudwick, "Charles Lyell, F.R.S. (1797-1875) and His London Lectures on Geology, 1832-1833," *Notes and Records Roy. Soc. London, 29* (1975), 252. In 1833 Lyell was well situated to defend a protheoretical position in the Society since the first two volumes of his *Principles of Geology*, a theory-laden work, had met with universal praise, if not acceptance, when published in 1830 (vol. I) and 1832 (vol. II). See Charles Lyell, *Principles of Geology*, 3 vols. (London: John Murray, 1830-1833). Also, Lyell had tacitly challenged the relegation of theory to individual opinion when he put himself forward in a lecture at King's as presenting a synthesis of current theoretical opinion. (See Rudwick, p. 241.)

30. For Darwin this meant publishing his theory of the earth's crustal movement piecemeal in a series of articles oriented around individual empirical researches rather than as a single coherent argument. The main articles in which the theory was developed were: "Observations of Proofs of Recent Elevation on the Coast of Chili, Made during the Survey of His Majesty's ship Beagle, Commanded by Capt. Fitzroy, R. N.," *Proc. Geol. Soc. London, 2* (1833-1838), 446-449; "On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations," *Proc. Geol. Soc. London, 2* (1833-1838), 552-554; "On the connextion of certain volcanic phaenomena, and on the formation of mountain-chains and volcanos, as the effects of

without censure did not automatically bring professional recognition for the theorist. As long as theoretical activity was officially regarded as an individual pursuit, ancillary to the real work of the Society, the theorist's role in the progress of the discipline could not be fully recognized. Thus while the Geological Society of London provided a forum for the expression of relevant theory, it did not simultaneously create a role for the theorist. Within the ranks of the Society one was permitted to do theory, but not licensed to present oneself as a theorist.

Even less hospitable to the theorist were the organizations devoted to the pursuit of botany and zoology. The most important of these organizations for Darwin were the Linnean Society of London and the Zoological Society of London. In the contemporary context it would, of course, have made no sense for members of these groups to distinguish sharply between those who concentrated on the theoretical, that is, on the systematic, side of their subject from those who collected, dissected, observed, compared, or described. As with most sciences of the period, the work to be done in botany and zoology did not require, and would not have supported, a sharp division of labor with respect to the articulation of theory. Among the sciences as they were pursued in England during the 1830's, only an astronomer could regard himself as choosing between the theoretical and the descriptive aspects of his subject.[31] Even in physics, theory and experiment remained intertwined

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continental elevations," *Proc. Geol. Soc. London, 2* (1833-1838), 654-660; and "Observations on the Parallel Roads of Glen Roy, and of Other Parts of Lochaber, with an Attempt to Prove That They Are of Marine Origin," *Phil. Trans. Roy. Soc.*, 1839, pp. 39-81.

While my characterization of this style of theory presentation as "piecemeal" is intentionally negative (for the reason that it disguised the importance of the theory in generating the individual researches), it is interesting that Martin Rudwick has seen this approach in a more positive light. Indeed, he has suggested that it was *just* this "worked example" format, where theory appears imbedded in concrete researches, which allowed what is sometimes termed the Lyellian revolution in geology. He writes, "If there is a sense in which Lyell's geology embodied a new 'paradigm' for the science, it must be sought not so much in his large-scale theorizing - much of which was rejected both at the time and later - nor even his rhetorical advocacy of the elusive principle of uniformity, but rather in his presentation of a series of persuasive 'exemplars' which convinced others of the efficacy of actualistic explanations in geology." See Rudwick, "Charles Lyell, F.R.S. (1797-1875)," pp. 256-257. Carrying these thoughts a bit further, one can also see how considerably the "exemplars" format differed from the deductive argumentation of the *Origin of Species*, leaving one with yet another explanation for Lyell's resistance to the work.

31. Thomas Young recognized a modern division of labor in astronomy when he divided the discipline into theoretical (mathematical) and descriptive components. See Thomas Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 2 vols. (London: Joseph Johnson, 1807), I, 487-488. I am grateful to Stephen Brush for this reference.

through most of the century.[32] So one cannot look to any division of labor among practitioners to guarantee a place for the theorist in either zoology or botany. Still, even without a division of labor, the theoretically inclined practitioner may be recognized. Thus far were the geologists willing to go in extending to the individual the right to hold theoretical positions. The situation in English zoology and botany was far different. In these areas, no official space was granted to the theoretician, but for the very substantive reason that the role of theory itself was heavily circumscribed. Indeed, what one finds in English zoology and botany of the period is a suspicion of theoretical activity which exceeded the internal requirements of the subject. In a general way, this suspicion characterized English botany and zoology from the 1780's, when the first professional society was founded, through 1859 and the publication of the *Origin of Species.* At a rhetorical level, this suspicion is easy to detect, for it permeated the literature. It was there in 1791 when James Edward Smith, first president of the Linnean Society, ridiculed Leeuwenhoek and Buffon for their theoretical excesses,[33] and it was still there in 1862 when George Bentham, then president of the Society, characterized Darwin's theory as "those speculations on the origin of species."[34] This anti-theoretical attitude on the part of English

32. The differentiation between theoretical and experimental physics occurred in the last third of the nineteenth century. For a characterization of the personnel in the new discipline of theoretical physics, see Paul Foreman, John L. Heilbron, and Spencer Weart, "Physics *circa* 1900: Personnel, Funding, and Productivity of the Academic Establishments," in Russell McCormmach, ed., *Historical Studies in the Physical Sciences, V* (Princeton: Princeton University Press, 1975), 30-33. Obviously, such differentiation within a field, as occurred in physics, promotes recognition of the theorist's role. However, since the differentiation within physics may be seen as a transposition of an earlier division between mathematically and experimentally oriented fields, exactly this sort of division cannot be expected to obtain in fields where nonmathematical theory predominates. For a discussion of the founding of modern physics with reference to earlier traditions in the physical sciences, see Thomas S. Kuhn, "Mathematical vs. Experimental Traditions in the Development of Physical Science," *J. Interdisciplinary Hist. 7,* no. 1 (Summer 1976), 1-31.

33. James Edward Smith, "Introductory Discourse on the Rise and Progress of Natural History," *Trans. Linn. Soc. London, 1* (1791), 1-55. For Smith, Leeuwenhoek's errors on generation might serve as a "*momento* to future theorists" and "the jest of philosophers for many ages to come" (p. 20), while Buffon's hypotheses were "for the most part, the essence of futility" (p. 47).

34. George Bentham, "Anniversary Address," *Proc. Linn. Soc. London, 4* (1859-1864), lxxxi. For a full discussion of the reception of Darwin's theory within the setting of professional societies, see Frederick Burkhardt, "England and Scotland: The Learned Societies," in Thomas F. Glick, ed., *The Comparative Reception of Darwinism* (Austin: University of Texas Press, 1974), pp. 32-74, especially pp. 47-53 on the Linnean Society. Following the pattern set by Thomas Bell, the previous president of the Linnean Society,

zoologists and botanists affected the ways in which these fields were allowed to develop. While I cannot establish this claim fully, I would like to suggest, briefly and tentatively, how these fields restricted the growth of theory within their domain, and to indicate, by one example, how such restrictions might close off areas of study, thus altering the course of the development of the science, at least temporarily.

If one surveys the titles of the articles published in the official journals of the Linnean Society of London and the Zoological Society of London, patterns emerge which suggest the nature of the limitations placed on the pursuit of theoretical questions.[35] Two features stand out from a survey of these titles. First, with the exception of one class of articles, most papers were titled so as to emphasize the centrality of a physical object or set of objects. Where general implications of the research were made, they were contained in the text of the article. Rarely does one find an author titling his paper, as William Yarrell did, "On the *Laws* that regulate the Changes of Plumage in Bird" (italics mine).[36] The

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Bentham refused to allow discussion of evolutionary theory at the Society's meetings. Since the theory had received its initial public presentation at a meeting of the Society (on July 1, 1858), Bentham's refusal suggests the considerable extent to which the leadership of the Society was prepared to go to prevent theoretical issues from becoming integral to the Society's business. It should be emphasized that this refusal was a principled one and not based on any animus toward Darwin or Wallace. Indeed, as Burkhardt points out, Bentham praised Darwin's concrete researches, discussed the state of opinion regarding evolutionary theory in his presidential addresses, and eventually became convinced himself of the mutability of species.

35. Journals included in this survey are the *Transactions of the Linnean Society of London* for the years 1791-1859, the *Proceedings* of the same society for the years 1838-1859, the *Zoological Journal* for the years 1824-1834, the *Proceedings of the Zoological Society of London* for the years 1830-1859, and the *Transactions* of the same society for the years 1835-1866. I have included the *Zoological Journal* in my survey because it was in effect the predecessor of the journals published by the Zoological Society of London. On the relationship between the *Zoological Journal* and the Zoological Society of London, see John Bastin, "The First Prospectus of the Zoological Society of London: New Light on the Society's Origins," *J. Soc. Bibliog. Nat. Hist., 5,* no. 5 (1970), 369-388, esp. 369-370.

Surveying article titles in specialized journals gives one only a rough sense of the content and lines of development in a field. But titles do indicate what the author, or the editor, saw as the essential point of the article from his own perspective and that of the select readership of the journal. The way in which articles are titled thus provides one with a means for gauging the expectations of the audience and for inferring how these expectations might have been met by prospective contributors.

36. William Yarrell, "On the Laws that regulate the Changes of Plumage in Birds," *Proc. Zool. Soc. London 2* (1833-1835), 9-10, 56. This paper was published in full under the more qualified title "Observations on the Laws which appear to influence the Assumption and Changes of Plumage in Birds" in *Trans. Linn. Soc. London, 1* (1835), 13-19.

only exception to this rule are papers dealing with classification, where abstract questions were foremost. Apart from these papers, emphasis in titles remained with the object. Also, the notion of what an object was, its definition, its boundaries, remained remarkably constant over half a century. Recognized objects included any taxonomic grouping, the individual organism, its anatomy or physiology, or, with less frequency, its behavior and place in the natural economy. Either extinct or extant forms could be considered. With these clear rules of definition, the object became the natural focus for the title of a paper. Reference to a complete list of Darwin's publications in these journals will substantiate the point.[37] A second, related pattern suggested by the titles of the articles from these botanical and zoological journals is the remarkable consistency in the kinds of research done over the period extending from the founding of the journals through the 1850's. While a complete enumeration of the categories of research in English botany and zoology during these years is beyond the scope of this paper, it is interesting to note how much research was carried out within the categories announced in the initial issues of the journals. For example, nearly all contributions appearing in the *Proceedings* and the *Transactions* of the Zoological Society through the 1850's fall under one or another of the headings listed by the *Zoological Journal* in its first call for papers. Those headings were: "Zoological Classification – Comparative Anatomy – particular Classes, Families, Genera, and Species – Animal Chemistry – Palaeontography and Nomenclature."[38] The perpetuation

37. For Darwin's contributions to the major scientific journals consult the *Royal Society Catalogue of Scientific Papers* (19 vols.). One paper not titled to emphasize an object was Darwin's joint communication with Alfred Russel Wallace to the Linnean Society, "On the Tendency of Species to Form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection," *Proc. Linn. Soc. London 3* (1855-1859), 45-62. While one may argue that the communication was titled so as to place it under the heading of papers on taxonomy (and it is interesting from this point of view that the familiar and acceptable terms "species" and "varieties" appear twice in the title and that the notion of the descent of species does not appear at all), no prudential wording of the title could disguise the exceptional nature of the contribution.

38. "Introduction," *Zool. J., 1* (1824-1825), vi. The comparable piece for botany, heavily influenced by the Linnean outlook of its author, is James Edward Smith, "Introductory Discourse on the Rise and Progress of Natural History," *Trans. Linn. Soc. London, 1* (1791), 1-56, see esp. 51-55.

Again, on zoology, I am taking the *Zoological Journal* to be the effectual predecessor of the publications of the Zoological Society. The initial volumes of the official publications of the Society do not contain a formal call for papers. The prospectus for the Zoological Society, widely circulated and printed in the *Zoological Journal, 2* (1825-1826), 284-288, proposed a research program combining scientific interests similar to those described in the "Introduction" to the *Zoological Journal* with an ambitious and thoroughly

of these categories of research over a thirty-year period was an important feature of English zoology. The maintenance of this pattern allowed the subject of zoology, and in similar fashion, botany, to grow in an orderly and assured manner. The object world, as defined by the English botanists and zoologists, was more completely known in the 1850's than it had been in 1791 and 1825. Clearly, the official goals of the societies were being met.

Yet ordered growth was purchased at a price. That price was the subordination of leads whose pursuit would have subverted the methods and conceptual apparatus of the fields as they were then constituted. It was as though the societies desired to fulfill their original goals before setting new ones. Limiting the theoretical expansion of the fields served this end, for by its nature theory transforms as it adds. An antitheoretical posture protected the definition of the field – its notion of an object (that is, its unit of analysis) and its categories of research. By maintaining this posture, English botanists and zoologists were exercising a powerful control on the development of their fields.

To assert as much suggests that with a different attitude toward theory English botany and zoology could have been other than they were, a hypothetical case but an important one to make. I offer only one example as a sketch of an argument in its favor, that of the differential exploitation of Robert Brown's 1831 identification of the cell nucleus by German and English botanists. As is well known, Brown's work was the catalyst for the final successful development of cell theory on the continent.[39] Yet Brown's discovery did not serve to unite English and continental research efforts. They remained complementary and overlapping, but distinct traditions. Now, looking at Brown himself, we may ask why he did not pursue the implications of his own discovery, thus joining the two traditions by continuing to contribute to both.

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utilitarian scheme to introduce new breeds of domestic animals into Britain. The practical aspects of the program, inviting cooperation between science and what amounted to a rationalized agriculture, were only very partly implemented by the Society. For more details on various versions of the prospectus see Bastin, "The First Prospectus of the Zoological Society of London."

39. Mathias Schleiden's critical paper "Beiträge zur Phytogenesis" (1838), which established cell theory for plants, depended directly on Brown's observations of the cell nucleus. For a brief introduction to the history of cell theory see the *Dictionary of Scientific Biography* entries on Schleiden and on Theodor Schwann, who extended the theory to the animal world. See also William Coleman, *Biology in the Nineteenth Century: Problems of Form, Function, and Transformation* (New York: John Wiley, 1971), pp. 23-24.

Clearly, he had the theoretical abilities to do so (witness his work on "Brownian" movement as well as on the nucleus), and he continued to be well informed of continental developments. Yet he did not choose to pursue cell theory himself. Why?

His failure to do so has usually been associated in a general way with his personal repugnance to theorizing, an attitude enforced by a shyness so extreme that he was once said to object to reading aloud in public.[40] But explaining very much by Brown's idiosyncrasies ignores the consistency of his action with the established practices of English botanists with respect to the development of their field. For Brown to have developed his insights on the cell nucleus further he would have had to submerge his concern with objects, as they were then identified by English botanists, in favor of new units of analysis. Moreover, in pursuing cell theory in the manner of the Germans, who held traditional interests in morphological questions, he would have surrendered control over the definition of research interests to the internal requirements of a new theoretical construct. This Brown would not do. Instead, he continued to work within the English definition of the field. Indeed, his collected works exhibit just those patterns which characterized English botany generally, that is, a precise understanding of what objects were of interest and a recognition of a limited and relatively stable number of research categories. (On the last score, Brown's corpus is particularly impressive for its nearly even distribution of papers across three recognized research areas - geographical distribution, anatomy and physiology, and classification.)[41] In sum, Brown's publications indicate that he was, just as his colleagues judged him to be, the model of British professional practice in botany. And here is the key point: his antitheoretical posture, his refusal to pursue theoretical points wherever they might lead, was but another aspect of his professionalism. Thus despite his obvious theoretical gifts, well documented by the contributions he

40. On Brown's dislike of speculation, see Barlow, ed., *Autobiography,* pp. 84, 103. On his reluctance to speak in public, see Bishop Samuel Goodenough as quoted in A. T. Gage, *A History of the Linnean Society of London,* p. 31. For a more complete view of Brown, which emphasizes his theoretical abilities, see the *Dictionary of Scientific Biography* entry by W. T. Stearn.

41. Brown's researches were grouped under the following headings in the standard edition of his collected works: Part I, Geographico-Botanical Memoirs; Part II, Structural and Physiological Memoirs; Part III, Systematic Memoirs; and Part IV, Contributions to Systematic Works. See John J. Bennett, ed., *The Miscellaneous Botanical Works of Robert Brown,* 2 vols. (London: Robert Hardwick for the Ray Society, 1866-1867).

made to two traditions of research, he was not primarily a theorist. His tradition did not authorize him to be. The traditions of English botany, and in similar fashion English zoology, were, as the career of Robert Brown indicates, restrictive of theoretical developments in specifiable respects. Thus restricted, the traditions could not authorize a role for the theorist.

I have now identified the elements necessary to explain why Darwin did not present himself to the world as a theorist. First among those elements is the fact that Darwin's presentation of himself was elicited by the expectations of the audience he wished to address. For Darwin that audience was wholly scientific in the narrowest construction of the term. The second element involves identifying the expectations of the scientific audience as it was constituted in England during the 1830's. At a general level, the expectations of the scientific audience were expressed by philosophers of science, particularly William Whewell. At a particular level, expectations concerning scientific conduct arose from the formal policies and informal practices of specialized scientific audiences. For Darwin the most important of these audiences were formed by the professional societies organized around geology, zoology, and botany. The third element is the nature of the expectations themselves. And here, as I showed, neither the general nor the specialized scientific audiences expected a serious contributor to their activities to present himself as a theorist. Whewell's respect for theory in science could not be translated into social categories; the geologists officially regarded theory making as an individual rather than a social endeavor; and the zoologists and botanists restricted the place of theorizing even more severely. In sum, no audience which Darwin chose to address licensed him to present himself as a theorist.

Thus, keenly aware of the expectations of his audience, Darwin did not present himself as a theorist. Aside from brief hints which appear to us in letters, Darwin did not reveal to his contemporaries the scope of his theoretical ambitions or accomplishments during the 1830's. Because of this, he could fairly be accused of disingenuousness. But before criticizing him, one must appreciate his motives. For Darwin, accepting the social forms provided by the scientific audience eased his entry into their company. The sacrifice of complete self-expression, even if he had been inclined to it, must have seemed a small price to pay (although an inconvenient one for historians) in exchange for the respect of his colleagues. And in this he was right, for ultimately his good standing with the scientific audience gained a fair hearing for all of his published works. The fate of a Jean-Baptiste Lamarck or a Robert Chambers was

not to be his. Darwin's attention to the expectations of a carefully chosen audience had earned him that much.

I have next to consider how Darwin's regard for correct forms affected the publication of his theoretical views. This will entail a discussion of the overall development of his transmutationist ideas. But before moving on to a detailed treatment of the 1837-1839 period, I must acknowledge the omission of any mention of man up to this point in the argument. That I could come this far in describing the terms of Darwin's activities in science without referring to man is significant in itself. It speaks for the fact that Darwin did not present himself before an audience whose primary concern was man. Since this is true, it renders moot the question of audience expectations. But one must then ask whether there existed a suitable audience for Darwin to address. While this is a large and complicated question, a few remarks may indicate the lack of parallels between the scientific audience for the study of man and other specialized audiences discussed thus far.

Very simply, there did not exist a scientific group devoted to the study of man in the same way in which the Geological Society of London was devoted to the study of the earth. The organization of study in geology illustrated the enormous practical value of specialization, for in the space of about fifty years a relatively small number of individuals (about 800 names appear on the list of the Geological Society for March 1, 1839) were instrumental in transforming a subject. The nearest counterpart to the situation in geology would be that of specialized societies devoted to various aspects of the study of man, as, for example, anthropology, psychology, and sociology. However, societies devoted to the pursuit of these now established social sciences were embryonic, if they existed at all, in the late 1830's.[42] The only important new scientific society in London devoted to some aspect of the study of man which met regularly enough to be listed in the weekly *Athenaeum* was the Statistical Society, founded in 1834. The British Association for the Advancement of Science, whose roster of subjects provides a clue to the intellectual terrain, also had a section devoted to statistics. Beyond this,

42. Anthropology was defined in England as a field in the late 1830's, psychology in the 1840's, and sociology, depending on whether one considers systematic treatment, which Herbert Spencer gave the subject, or ongoing formal organization, either in the last quarter of the nineteenth century or the first decade of the twentieth. For a discussion of these fields, see George W. Stocking, Jr., "What's in a Name? The Origins of the Royal Anthropological Institute (1837-71)," *Man*, n.s., *5* (1971), 369-390; Leonard S. Hearnshaw, *A Short History of British Psychology, 1840-1940* (New York: Barnes and Noble, 1964); and G. Duncan Mitchell, *A Hundred Years of Sociology* (London: Gerald Duckworth, 1968).

scientific societies organized around the study of man were largely medical, and there were a number of these. But while medicine had been the traditional center of scientific studies concerning man, it was too much settled on anatomy and physiology to accommodate new interests on an even footing with the old. And while other traditional scientific fields, like zoology, might occasionally extend themselves to include a piece on man (as, for example, the article by John Oliver French on human instincts in the first volume of the *Zoological Journal*), their attention was only partial. In sum, there was no one obvious scientific forum for discussions concerning man. The constraints on public expression this lack of a bounded authoritative audience would impose on Darwin will become clear when Darwin's reflections on man are compared with his record of publication.

2. THE NOTEBOOKS

"From my return to England Oct. 2, 1836 to my marriage Jan. 29, 1839: These two years and three months were the most active ones which I ever spent." So wrote Darwin in his autobiography.[43] "Active" is the key word in Darwin's sentence, and by it he meant professionally active. While Darwin had been guided by professional requirements during the *Beagle* voyage, indeed much more so than is usually recognized, he had been physically isolated from his peers. On his return, that isolation ended, and he was drawn into a round of activities. His days in London were directed toward two ends: caring for his collections, as described in Part I of this article, and participating in the work of the Geological Society, serving as a member of its Council (and later, from 1838 to home. Within months of his arrival he was presenting papers to the Society, serving as a member of its Council (and later, from 1838 to 1841, as one of its secretaries), and in other ways contributing to the managing of its affairs. He was also reading in preparation for writing a comprehensive work on his geological studies in South America, and looking forward to beginning new field work where he might apply what he had learned in South America to the English scene. His work in Scotland on the origin of the parallel roads of Glen Roy was designed with this latter goal in mind.[44] For his remarkable performance in all these professionally sanctioned activities he received immediate recognition, culminating in his election to the Royal Society of London in

43. Barlow, ed., *Autobiography*, p. 82.
44. See Rudwick, "Darwin and Glen Roy," p. 118.

1839. But along with these acceptable efforts, Darwin was pursuing another course during this "most active" period in his career. He was engaged in theoretical work which, for the greater part, was not sanctioned by professional norms. Thus, in a sense, he was leading two lives. In the public eye where he spent his days, he was the busy professional whose calendar was filled with meetings and consultations. In private, when at leisure, "unwell," or during summers (and often at Maer, the home of his Wedgwood cousins), he was his own, more theoretical man. It is therefore necessary to uncover the private Darwin, the Darwin of penciled notebooks and excised pages, to trace the development of his theoretical views.

The sources for interpreting Darwin's theoretical work from the 1836-1839 period consist of contemporaneously published works, including the *Journal of Researches* and the articles published in professional journals; other contemporaneous writing, mainly notes, letters, and marginalia, unpublished during Darwin's lifetime; and his later writings as they cast light on the period. Of the entire group the most important source is a set of eight notebooks from the period. These are the R.N., or Red Notebook, and seven notebooks labeled A, B, C, D, E, M, and N.[45] Other unpublished writings from the period add to the understanding of Darwin's work without having the importance of any one of the interlocking set of notebooks.[46] Of course, none of the sources can be used without interpretive canons. Obviously, published sources pose the

45. The Red Notebook, discussed in Part I of this article, is among the manuscripts at Down House, the Darwin Memorial in Kent. I would like to express my appreciation to Jon Hodge, who provided the evidence for the full name of the notebook. I am presently editing the Red Notebook and notebook A for publication. Notebooks A through N are among the Darwin manuscripts at the University Library, Cambridge. Of these notebooks B, C, D, and E appear as "Darwin's Notebooks on Transmutation of Species," Parts I-IV, ed. Gavin de Beer, "Addenda and Corrigenda," ed. Gavin de Beer and M. J. Rowlands; Part VI [excised pages] ed. Gavin de Beer, M. J. Rowlands, and B. M. Skaramovsky, *Bull. Brit. Mus. (Nat. Hist.), Historical Series, 2,* nos. 2-6, and *3*, no. 5 (1960-1967). De Beer's Parts I. II, III, and IV correspond to Darwin's B, C, D, and E. The transcription and photocopies of notebooks M and N which I used in preparing this article were done by Miss B. M. Skramovsky, formerly of the British Museum, to whom I would like to express my gratitude. Notebooks M and N, transcribed and annotated by Paul H. Barrett, are now available in Howard E. Gruber and Paul H. Barrett, *Darwin on Man: A Psychological Study of Scientific Creativity* (New York: E. P. Dutton, 1974). In most cases of conflict I have adopted Barrett's reading of the text. Citations of all notebook entries will be made to the notebook by name and to the page number in Darwin's pagination. Excised pages will be signified by a lower-case *e* after the page number.

46. For a catalog and brief description of the major collection of Darwin manuscripts, see *Handlist of Darwin Papers at the University Library, Cambridge* (Cambridge:

least problem, since they were meant to be read in the form in which they appear. One caveat only need be added, that they must be considered in relation to the audience for which they were intended.

Interpreting notebooks which were kept for Darwin's use alone poses the quite different problem of reading a text which was not formed as an argument. Entries in the notebooks are short, careless of traditional elements of style, and frequently obscure. Also they are often cast in the form of commentaries on texts so that every line in the notebook cannot be presumed to be equally Darwin's own. Last, even neighboring entries may point to contradictory conclusions, a reflection of Darwin's testing of many possible solutions to any given problem. How, then, should the notebooks be read? A useful general rule is to consider the notebooks thematically. This can be done by tracing key words and concepts through the eight notebooks.[47] A corollary to this rule is to avoid placing the weight of interpretation on any single passage, except where the interpretation is corroborated by evidence outside the text. When these rules are followed, as they have been in this essay, the danger of arbitrary interpretation, though never absent, is considerably diminished.

The first conclusion to be drawn from study of Darwin's work on species in the eight notebooks is respect for the remarkable unity of his efforts. This unity derives from the consistency of his intentions, which were theoretical, that is to say, explanatory, and from the clearly defined shape of the period as a whole. On the first score, Darwin's theoretical impulse is self-evident from his having "got a theory by which to work" during this time.[48] But an appreciation of his work as a day-to-day activity gains if one considers its cumulative nature. As the most recent commentators have pointed out, Darwin's attack on the species question proceeded in stages.[49] The exact stages identified by the authors are

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Cambridge University Press, 1960). While the manuscript collection at Down House is not cataloged, most of the material there pertains to the *Beagle* voyage proper. Exceptions are the Red Notebook and a notebook labeled "St. Helena Model," containing notes, mainly of a practical nature, on a variety of topics.

47. The method of content analysis has been applied by George J. Grinnell to a thematic study of the originally published (that is, unexcised) portions of notebooks B, C, D, and E. See George J. Grinnell, "The Darwin Case: A Computer Analysis of Scientific Creativity," Ph.D. diss. University of California, Berkeley, 1969, chap. 2. In addition, a useful tool for identifying themes in the notebooks is the computer printout of vocubulary from notebooks B, C, D, and E prepared by Paul H. Barrett of Michigan State University and available directly from him.

48. Barlow, ed., *Autobiography*, p.120.

49. The following authors who have examined at least four notebooks (B, C, D, and E) in detail have each concluded that Darwin made a series of attempts to provide a

not at issue here. What is instructive from the point of view of characterizing the notebooks as a whole is that four scholars, working independently, or nearly so, have recorded this phenomenon. In itself, the very existence of these stages, each one built on the other, signifies the self-feeding propulsive quality characteristic of theoretical work. Moreover, it is clear from the notebooks that the process of devising explanations was controlled by Darwin himself. None of the commentators who have concentrated on the 1837-1839 period writes chiefly in terms of influences. Lyell, Lamarck, Malthus (the list is long) all appear in recent accounts, but as authors whose opinions were solicited and judged by a single mind. Darwin's direction over the process remained paramount. In this light, Darwin's originality as a theoretician is not seen to derive from his novelty on any set number of points but from his skill in negotiating a way, or ways, through an enormous number of possibilities – some drawn by him, some by others – to satisfactory conclusions. This independence of judgment, combined with the cumulative nature of the process, marked the notebooks with a unified theoretical style.

The second source of unity underlying the eight notebooks is temporal. As a group, they are marked off from everything before and after them, and within the group, they follow each other in sequence. Prior to this set of notebooks, Darwin was engaged in field work connected with the voyage; near their completion he turned to a new set of still unresolved issues. In the interim, he was concerned with following out those theoretical insights first declared in the Red Notebook. This he did to his own satisfaction in the two-year period from mid-1837 to mid-1839, when notebooks A through E, M, and N were filled.

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mechanism for species change once he had converted to a transmutationist position. George Grinnell identified three successive theories: (1) geographical isolation, (2) habits changing structure, and (3) the domestic breeding model. This ordering, which emerged in his dissertation (Berkeley, 1969), was reaffirmed in "The Rise and Fall of Darwin's First Theory of Transmutation," *J. Hist. Biol.*, *7*, no. 2 (Fall 1974), 272. Camille Limoges saw Darwin's work in the 1837-1839 period as moving from a biogeographical model to one based on natural selection, the two united by a common interest in adaption. See Limoges, *La sélection naturelle* (Paris: Presses Universitaires de France, 1970). Sandra Herbert also recognized two major stages, the geologico-geographical model based on the distribution of species through space and time and the model based on natural selection. See Sandra Herbert, "The Logic of Darwin's Discovery," Ph. D. diss. Brandeis University, 1968. Writing shortly after the others, Howard E. Gruber identified three separate theories of species change: (1) monads, (2) perpetual becoming, and (3) natural selection. See Gruber and Barrett, *Darwin on Man*, pp. 103-105.

Since certain physical features of the notebooks suggest their intellectual character, I shall begin by describing their appearance. They are small in size, running on an average 6″ × 4½″, leatherbound, and contain upward of seventy-five leaves of good-quality paper. Of the eight notebooks, only two pairs (D and M; E and N) are identical, a telling fact for the members of each pair. As a group, the eight stand as the successors to the miscellany of pocket-sized notebooks which Darwin carried with him on the voyage.[50] These field notebooks, kept by Darwin on his person, were used for the recording of on-site observations. Thus the order of entries in the field notebooks reflects what was immediately before his eyes. This immediacy is quite apparent from the sketchiness of most entries. (Full geological and zoological notes were made at leisure on larger-sized paper. These formal notes are contained in volumes 30-38 of the Darwin manuscript collection at the University Library, Cambridge.)

In overall appearance and function, the Red Notebook forms the transition between the field notebooks and the lettered sequence A through E, M, and N. Like the field notebooks, it contains notes on some contemporaneous events, but in the main it is composed of reading notes and theoretical remarks, and in that it is like the later notebooks. The mixed nature of the entries reflects the fact that Darwin was no longer primarily engaged in field work (the rate of field observations declined sharply after the Galápagos stopover), but in new projects. However, like the field notebooks, the Red Notebook contains observations on both geological and zoological subjects (appropriate in a notebook to be used in writing the *Journal of Researches*), and as in the field notebooks, entries in the Red Notebook are often sketchy.

But in addition to being a transitional notebook, the Red Notebook is also a pivotal one, for it was in this notebook that Darwin announced to himself the extent of his theoretical ambitions. In succeeding notebooks he pursued those ambitions. I have made that point with respect to species in Part I of this article. But for the interdependence of the eight notebooks to be appreciated, the point needs to be made for geology as well. I will make it here briefly, suggesting only the nature of Darwin's

50. Eighteen notebooks, numbered by an unknown cataloger and characterized as from the *Beagle* voyage, are stored at Down House. Of these, notebook "1" is entirely excised and bears a London address, notebooks "2" (the Red Notebook) and "5" ("St. Helena Model") are mainly of post-voyage date, and the others are genuine field notebooks from the voyage. In addition, there are at Down House six notebooks, bound in two sets of three, listing specimens collected during the voyage. For selections from all of these notebooks see Barlow, ed., *Charles Darwin and the Voyage of the "Beagle."*

ambition as a geological theorist. The entire subject of Darwin's contributions as a geologist requires separate treatment elsewhere.

Darwin's goal in geology was to discover laws governing the movement of the earth's crust. Many of his individual researches were directed toward that end; indeed, they were subsidiary to it. His theory, founded on vertical crustal movement, had very partial success, accounting adequately for the formation of coral reefs but failing in other important applications. Yet, whatever the theory's ultimate fate, Darwin's intention to build a geological theory around the mechanism of vertical crustal movement is conveyed explicitly in the Red Notebook. The following passage suggests the extent of Darwin's ambition in that direction. Its sketchy character is typical of his notebook style:

> Humboldts fragmens
>
> Read geology of N. America. India. - rememb[er]ing S. Africa. Australia. Oceanic Isles. Geology of whole world will turn out simple. -
>
> Fortunate for this science that Europe was its birth place. - Some general reflections might be introduced on great size of ocean; especially Pacifick, insignificant islets - general movements of the earth; - Scarcity of Organic remains. - Unequal distribution of Volcanic action, Australia S. Africa - on one side S. America on the other: The extreme frequency of soft materials being consolidated; one inclines to belief all strata of Europe formed near coast. Humboldts quotation of instability of ground at present day - applied by me geologically to vertical movements.[51]

This passage, which refers to Alexander von Humboldt's *Fragmens de géologie et de climatologie asiatiques* (1831), revolves around the sentence "Geology of whole world will turn out simple." The enormity of the claim asserted in this sentence is striking. It suggests the role Darwin believed an adequate understanding of crustal movement would play in future geological theory. As a phrase, it conveys the extent of Darwin's individual researches. But for my immediate purpose it is important only in that it occurs in the very notebook which also contains Darwin's first expression of a transmutationist position.

51. Red Notebook, pp. 72-73. The reference to Europe suggests Darwin's belief that its geology was more complicated than that of South America, owing to differences in crustal movements between the two continents.

When the Red Notebook wil filled, sometime toward the end of spring 1837, more thought on both geology and species was clearly in order. However, with the first draft of the *Journal of Researches* out of the way, there was no longer any practical reason for the two subjects to be considered together, and each was assigned a new notebook of its own - geology to notebook A and species theory to notebook B. Expressed visually, the first step in Darwin's differentiation of subject matter is suggested by Fig. 1.

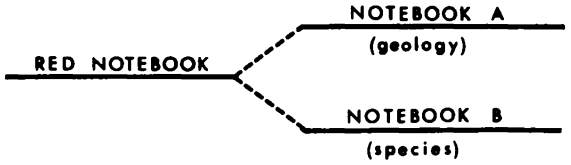

Fig. 1. The relationship of the Red Notebook to notebooks A and B. Solid lines represent the notebooks, broken lines the generative connection between the Red Notebook and notebooks A and B.

This diagram indicates the generative relationship between the Red Notebook and notebooks A and B. Not every aspect of this relationship is known, as, for example, the exact date on which each notebook was opened.[52] But the shift in intention suggested by the new division of note taking is clear. Whereas the field notebooks were tied in a direct way to observation and where the Red Notebook was associated with a discrete project, notebooks A and B were tied to no itinerary, and with B particularly, no project save the pursuit of theoretical issues. With the opening of these two notebooks, Darwin's work as a theorist became explicit.

In the next two years, Darwin pursued the theoretical issues he had engaged in the Red Notebook. Notebook A, which represents only a fraction of Darwin's work in geology in 1837, filled slowly. Notebook B, which contains nearly all his work on species, filled by the end of

52. For the sake of convenience and pending further evidence, I shall date the opening of the Red Notebook to June 1836 and of notebooks A and B to July 1837. The June 1836 date for the opening of the Red Notebook derives from references it contains to points on the *Beagle's* itinerary. The July 1837 date for the opening of notebook A is conjecture on my part. The first dateable reference of relevance in the notebook occurs on p. 15e and is to the August 1837 issue of *L'Institut*. Since notebook A filled fairly evenly and slowly, overall at the rate of less than 10 pages a month, the August date on p. 15e is consistent with a July date for the opening of the notebook. For notebook B the opening date of July 1837 is secured by Darwin's word. (See Gavin de Beer, "Darwin's Journal," *Bull. Brit. Mus. (Nat. Hist.), Historical Series, 2*, no. 1 [1959], 7.)

February 1838.[53] In notebook C, Darwin continued work begun in notebook B. Then, in July 1838, after a year of work on the transmutation hypothesis, Darwin again divided his labors. When notebook C was filled in mid-summer 1838, he opened two notebooks in its place, notebook D devoted to the species question in general, and notebook M devoted to the subsidiary subjects of man, mental activity, and behavior. When notebooks D and M were filled at the end of September 1838, their places were taken by notebooks E and N, respectively. The division of subject matter suggested by the opening of notebooks M and N is shown in Fig. 2.

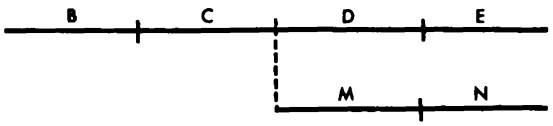

Fig. 2. The relationship of notebooks B, C, D, and E to notebooks M and N. Solid lines represent the notebooks, the broken line the generative connection between them.

In a similar fashion, though less marked since it did not involve the opening of a new notebook, Darwin began a separate section of notebook D, beginning at page 152, devoted to the subject of generation. Taken altogether, the eight notebooks developed from 1837 on in the fashion shown in Fig. 3. This diagram indicates in a schematic way the intellectual relationships among the notebooks within the group. It suggests how questions bred questions to a point where as many as four different lines of thought were being pursued simultaneously. Likewise, ending of the series of notebooks speaks to a diminution of efforts, a finish line.

The end came in the following manner. With the reading of Malthus, recorded in notebook D, Darwin had come to a turning point. Armed with what he was to call "my Malthusian views" (E, p. 136), he believed he could justify a transmutationist position. This freed him to concentrate on the subject of generation, with its attendant issues of variation

53. The exact date of completion of notebook B is in doubt. Darwin referred in his heading for the notebook (probably added in 1844 when he was arranging his papers) that he had completed it at the beginning of the month. In fact, the notebook was completed somewhat later, for p. 235 refers to the February 24 issue of the *Athenaeum*. Since the notebook ran to another 29 pages of text, after p. 235, it was probably completed no earlier than the end of the month. The enthusiastic burst of entries toward the close of the notebook would also agree with the notation for February 25 in "Darwin's Journal" (p. 8) that he "also speculated much about 'Existence of Species' " about that time.

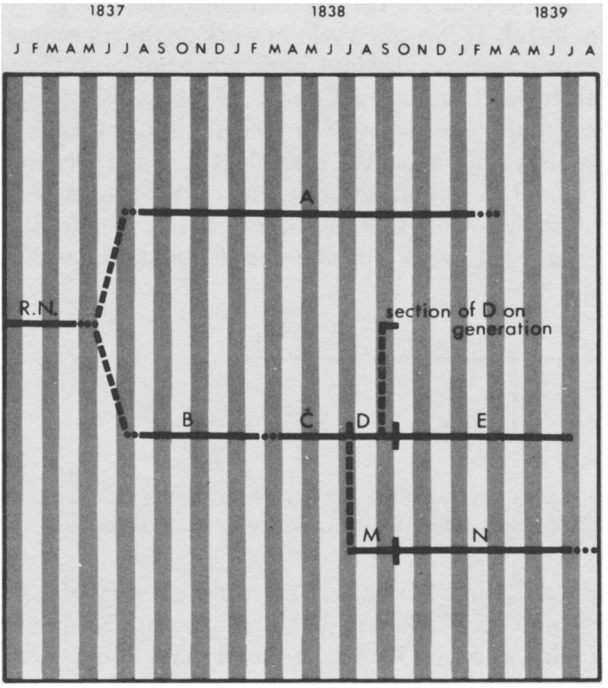

Fig. 3. Eight Darwin notebooks from 1837 to 1839. Solid lines represent the notebooks, dotted lines uncertainties in dating, and broken lines divisions of subject matter among members of the set.

and inheritance. He had already singled the subject out for special consideration in notebook D, where it had become an increasingly prominent center of attention. But sometime in the last quarter of 1838 or early 1839, he set out on a new program of research which in its departure from the notebook format signaled the close of the period represented by the eight notebooks. This new program of research entailed a systematic survey of the opinions of those who had most experience with generation, namely, breeders and horticulturists. For animals he had printed a questionnaire[54] which he circulated among

54. See Charles Darwin, *Questions about the Breeding of Animals* [1840], with an introduction by Gavin de Beer (London: Society for the Bibliography of Natural History, 1968). Correcting de Beer's dating, R. B. Freeman and P. J. Gautrey have shown that the pamphlet may have been composed and printed as early as the last quarter of 1838 and was first circulated sometime between January 1, 1839, and May 6, 1839, most likely earlier in that period. See Freeman and Gautrey, "Darwin's *Questions about the Breeding of*

breeders. For plants he made extensive inquiries to William Herbert,[55] a well-known and published horticulturist, and to others, including J. S. Henslow. The impact of this new program of research on the course of Darwin's species work was striking, for it made the work of breeders central to his theory. Thereafter, the creation of new domestic varieties by selective breeding became the observable analog for the hidden process of species formation in nature. Moreover, this analogy formed the core of the draft which constituted Darwin's first formal argument for this theory. This draft appears to have been composed sometime in the latter half of 1839.[56] With the existence of this draft, the first known expression by Darwin of a coherent point-by-point argument for transmutation, the period of theoretical exploration represented by the notebooks was over.

From the point of view of chronological distinctness, then, as well as of style, the eight notebooks belong together. This unity, aesthetically as well as intellectually pleasing, reflects the work of a single author working out solutions on his own, for himself, in private. Yet despite the brilliant singularity of Darwin's effort, his theoretical work from the 1836-1839 period still bore a relation, albeit graded and complex, to his professional life. The most obvious sign of this relation is the division of subject matter Darwin observed in his notekeeping.[57] Where possible, the filing system set up by his system of parallel notebooks reflected what were then conventional lines between fields. The relationship between notebook A and geology was simple and direct; there existed a one-to-one correspondence between the professional definition of the field and Darwin's own interest. The relationship between notebooks B,

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Animals, with a Note on *Queries about Expression,*" *J. Soc. Bibliog. Nat. Hist., 5,* no. 3 (1969), 220.

55. I am grateful to David Kohn for allowing me to read an important series of letters between Darwin and William Herbert from this period. Herbert's first reply to Darwin's inquiries was dated April 5, 1839, which suggests that Darwin was gathering information on plants and animals simultaneously.

56. Peter J. Vorzimmer, "An Early Darwin Manuscript: The 'Outline and Draft of 1839,'" *J. Hist. Biol., 8,* no. 2 (Fall 1975), 191-217. See particularly p. 216, which may be compared with notebook E, p. 118, for the key outline for what became Darwin's finished argument. When the date of this manuscript is fully established, it will be of great help in reconciling Darwin's various and conflicting statements on the importance of artificial selection to him in forming his theory.

57. The division of labor in the notebooks was clearly intentional. Where Darwin wrote in one notebook what belonged in another, he noted as much, for example, in D,p. 25, which reads "Vide p. 63. Notebook M," or N, p. 73, "V. [iz] E. p. 125. wrong entry."

C, D, and E and professional fields was less simple. Obviously, transmutation, the subject of these notebooks, transgressed disciplinary boundaries. Yet since most citations in the four notebooks were to the professional literature in the separate fields of zoology, botany, entomology and so on, there was a clear bond between the four notebooks and scientific fields as they were then defined.

More problematic is the relationship between the M and N notebooks and professional interests. The question arises even as one tries to settle on a description of the subject of the two notebooks. Reading only these notebooks, one might identifiy one or more of the topics listed in the headings Darwin later gave the notebooks as their subject matter: for notebook M, "Metaphysics on Morals and Speculations on Expression," and for notebook N, "Metaphysics & Expression." Or one might select another prominent theme from the notebooks to describe them.[58] But reading notebooks M and N in the context of the other seven notebooks in the set leads one to characterize them as transmutation notebooks, like notebooks B, C, D, and E, but with a more specialized subject. The nature of that subject can be described in either of two ways. In the context of the whole series of notebooks, notebooks M and N represent Darwin's chosen approach to the question of man's place in nature. From that point of view, the subject of the two notebooks is man. Yet, from another point of view, equally valid, the subject of the two notebooks is mental activity in relation to behavior, a subject in its own right within the context of the entire transmutationist theory. How these two topics – man, and mental activity in relation to behavior – related to each other will be discussed in the next section of this essay. For the moment we need only discuss how poorly either topic aligned with a professionally drawn area of inquiry in science. "Man," was studied under a variety of contexts, preponderantly nonscientific, "mental activity" was the province of epistemology, and "behavior" fell, depending on the subject, to ethics, politics, zoology, botany, and entomology. Thus the M and N notebooks were further removed from bounded areas of inquiry in science than the other notebooks in the series. As notebooks, M and N did not stand for a field. The consequences of this situation for the public appearance of work from these two notebooks were profound.

Darwin's pattern of publication, like his division of labor in note taking, reflected his perception of professionally drawn boundaries

58. Charles Swisher has identified instinct as the subject of the notebooks. See his "Charles Darwin on the Origins of Behavior," *Bull. Hist. Med., 41,* no. 1 (1967), 25.

between scientific fields. As I showed in section 1, there did not exist a role for the theorist in science generally. This fact prevented Darwin from publishing his theoretical work from the 1836-1839 period as a single piece, which, given its logical coherence, he could have done. Rather, he published various portions of his theoretical work as they met the expectations of specialized scientific audiences. This principle explains the content and timing of publications based on theoretical work from the notebooks.

Again the field of geology provides the straightforward case. Its audience was well formed, and their views were cogently expressed in the governing documents of the Geological Society of London. As I have shown, the Society permitted and individual to make explicitly theoretical statements. Darwin took advantage of this toleration. If we discount unfinished work, his theoretical opinion is fully expressed in his published work in a form acceptable to the Society. Aside from the odd critical or expansive remark, little is lost in the transition from private to public sphere. Indeed, much is gained, for Darwin's early geological articles have greater cogency than his notes, with no loss of urgency or excitement. (For a list of the most important articles see note 30.) Moreover, ideas moved quickly from private notebook to printed page. Darwin's heading of notebook A tells the story: "Feb 24th 1839 As far as p. 140 – abstracted as far as concerns 'Geolog Observat on Volcanic Islands & Coral Formation.' " (*The Structure and Distribution of Coral Reefs* appeared in 1842, *Geological Observations on the Volcanic Islands* in 1844.) Thus Darwin's geological theorizing, as represented by notebook A, came before its public both quickly and completely.

The audience for transmutation was more difficult to locate. The theory was known to be unacceptable before the most general scientific audience. In England it had been rejected most recently by Charles Lyell, whose refutation of the theory was given added currency by William Whewell.[59] Moreover, those professional bodies which held immediate jurisdiction over the subject of living species, namely the Zoological Society of London and the Linnean Society of London, could not have accomodated transmutation even on a formal level, for it violated their strictures against theory. Darwin was thus faced with the knowledge that there was no suitable body for him to address. He reacted with outrage and caution. Inwardly, he sustained himself with the thought of eventual triumph. Outwardly, he remained silent, or said

59. See Lyell, *Principles of Geology* (1st ed.), II, chaps. 1-4, and Whewell, *History of the Inductive Sciences*, III, 573-576.

so little that even Lyell, his closest colleague, could not have known his conclusions.[60] And he waited, for he would not present himself inappropriately. Finally, in 1844, he arranged for the posthumous publication of his theory in the event of his sudden death. The opening sentences of his testamentary letter show how essential he considered an audience to be: "I have just finished my sketch of my species theory. If, as I believe, my theory in time be accepted even by one competent judge, it will be a considerable step in science."[61] He was prophetic in his remark, for eventually he had one competent judge, not drawn from the company of those cultivated for the purpose in the late 1840's and 1850's (particularly Joseph Hooker), but appearing in the person of Alfred Russel Wallace.[62] From that point on the story is well known. It need only be emphasized that twenty years had been required for an audience to form which, even then, was comprised of two codiscoverers, each the audience for the other, supported by a band of colleagues of one of the pair carrying the joint announcement of the theory before a scientific body. A meager audience in numbers, but one that satisfied Darwin's requirements for presentation of his work. No other explanation is needed for the twenty-year interval between the transmutation notebooks and the *Origin of Species.*

In contrast to the situation in geology and zoology, the scientific audience for the material contained in the M and N notebooks simply did not exist, even potentially. Darwin knew very well that no contemporary scientific body would sanction the utterly naturalistic view of man he had come to. He could also see no scientific audience which

60. See Darwin's enthusiastic but unforthcoming remarks on his species work in his letter to Charles Lyell of September 13, 1838, in Francis Darwin, ed., *The Life and Letters of Charles Darwin,* 2 vols. (New York: Basic Books, 1959), I, 377.

61. Charles Darwin to Mrs. Emma Darwin, July 5, 1844, in F. Darwin, ed., *Life and Letters*, I, 377. Also see the letter to T. H. Huxley of November 25, 1859, where Darwin emphasized his reliance on the opinions of others (in this case Hooker, Lyell, and Huxley) for judging his work, ibid II, 27-28.

62. I am not suggesting that Darwin would never have published without Wallace. I am merely observing the circumstances under which he did. On this I may add two related points, first that Wallace was part of the potential audience for a transmutationist theory from 1855 on, by virtue of his well-read paper "On the Law Which Has Regulated the Introduction of New Species" (*Ann. and Mag. Nat. Hist., 16,* 2nd ser. [1855], 184-196) and, second, that before Wallace's communication of 1858 Darwin was contemplating publishing his theory as speaking to one side of the species question rather than as an assertion of the true state of affairs. See F. Darwin, ed., *Life and Letters*, I, 394, 400, 406. Minimally, then, one must credit Wallace creating the audience for the book Darwin actually wrote.

might accommodate other themes from the notebooks. Thus, following his rule, he did not reveal himself. In this case, however, Darwin guarded himself on so many levels and over so long a period of time that full self-expression eventually became unthinkable. It would have required too much undoing.

The primary mask was assumed deliberately. Very consciously, he drained his species argument of references to man. No discussion of human origins appears in any draft. Further, when discussing mental activity and behavior, subjects he had developed most powerfully in relation to man, Darwin turned to non human examples to make his points. Even then, habit and instinct figured less prominently in successive drafts of the theory.[63] With these changes, the contents of M and N were all but unrecognizable in the formal exposition of the theory. It is thus not surprising that when Gavin de Beer published Darwin's notebooks on transmutation, he included only B, C, D, and E in the series. Notebooks M and N were left behind, transcribed but unpublished.

This obscurity of relation between notes and argument, which permitted de Beer's error, resulted from the misleading impression Darwin created of the genealogy of his theory. It too was a mask. To take only the most obvious point, Darwin almost certainly read Malthus not "for amusement,"[64] as his published autobiography suggests, but in the course of pursuing those subjects for which the M and N notebooks had been opened. If this is true, the turning point in Darwin's development of a theory of transmutation came at the intersection of two lines of thought, the one zoological and botanical, represented by notebooks B through E, the other philosophical in a general sense and represented by notebooks M and N. Darwin's debt to philosophy, and related areas

63. In the drafts of the transmutation theory written in 1839 [?], 1842, and 1844, habit is treated in the first section of the argument under the heading of variation. See Vorzimmer, "An Early Darwin Manuscript: The 'Outline and Draft of 1839,' " pp. 208-212; see also Charles Darwin and Alfred Russel Wallace, *Evolution by Natural Selection* (Cambridge: Cambridge University Press, 1958), pp. 54-58 (1842 version) and pp. 136-143 (1844 version). In the extant portion of the "long" version of the *Origin*, as well as the *Origin* itself, the related subject of instincts is treated primarily in the latter part of the argument as a difficulty to be overcome rather than as a supporting element in the whole scheme. Even with that, the subject was presented in reduced form in the *Origin*. For the story of the delayed publication of the original chapter on instinct from the "long" version of the *Origin*, see R. C. Stauffer, ed., *Charles Darwin's Natural Selection* (Cambridge: Cambridge University Press, 1975), pp. 463-466.

64. Barlow, ed., *Autobiography*, p. 120.

of social and political thought thus appears substantial, and his presentation of himself only as a natural scientist incomplete.

Darwin's presentation of himself was lacking at the emotional level as well. In fairness to him, one must allow that personal experience is for the most part inexpressible within the bounds of scientific communication. However, Darwin was more closed than most in conveying his sense of his work to his colleagues. For obvious reasons, he could not announce his conversion to a transmutationist hypothesis to Lyell. Similarly, he could not tell Lyell, and if not Lyell then no one, of his growing excitement about the hypothesis. In 1838, for example, when he was at the peak of his form, making theoretical advances almost daily, he was addressing only his notebooks. Thus, despite his intense activity in the public affairs of science during this same period, on the level of his deepest involvement he was working alone. On no subject was this more true than that of man.

These, then, were the masks: silence on the subject of man and mental activity, incomplete recounting of the genealogy of the theory, and suppression of personal experience. They remained in place until the published version of the theory received a favorable hearing. Then, "in order that no honorable man should accuse me of concealing my views,"[65] Darwin let the masks fall, though late in life and incompletely. He broke his silence on man with *The Descent of Man* (1871) and on mental activity and behavior with *The Expression of Emotions in Man and Animals* (1872). Along with related smaller publications (including *On the Movement and Habits of Climbing Plants*, 1865), these volumes represented the completion of work taken up in the M and N notebooks. Since these later works were published on the strength of an already formed reputation, with less regard for the requirements of specific audiences, they are relatively pure representations of Darwin's views. Thus Darwin eventually did speak his mind on the offensive subjects.

Reticence, however, had not been his only disguise, and others slipped away less easily. With respect to crediting philosophy or social theory with a role in the creation of his theory, Darwin faced a dilemma. Having taken care to present himself as a geologist and zoologist, he could not reveal himself some thirty years later to have been something else as well. It was more convenient to minimize his early involvement with metaphysics ("I was not at [all] well fitted for such studies")[66] than to revise a consistently drawn public identity. Yet to his credit, Darwin did

65. Ibid., p. 130.
66. Ibid., pp. 84-85.

arrange to leave behind him a record of his own experience. In 1876, when he was sixty-seven, he began an autobiography, chiefly to explain the relation between his life and his published work. Presumably, the autobiographical form allowed him to reveal himself more fully than he had in life. And indeed as a document it is at points remarkably candid, especially in the evidence it provides for correlating his loss of religious faith with his gain in confidence in his species theory. Yet overall the autobiography followed the unwritten rule on public statements within the British Civil Service: "Nothing may be said which is not true: but it is as unnecessary as it is sometimes undesirable, even in the public interest, to say everything relevant which is true; and the facts may be given in any order."[67] Moreover, as a gesture the autobiography was characteristically made, for it was directed not to the general public but to the audience whose approval Darwin most required, in this case his family. Darwin's act left transmission of the work, along with all his manuscripts, to their discretion. The children would decide what the German editor should know.[68] Darwin had thus assured that a select audience stood between himself and posterity. Again he had provided that his true opinion be known, but at the appropriate time and in the appropriate forum.

Darwin's ability to judge audiences permitted the remarkably graceful reception of the *Origin* within science. Yet although successful, his strategy was not without cost to himself, to science, and to the public's

67. H. E. Dale, *The Higher Civil Service of Great Britain* (Oxford: Oxford University Press, 1941), p. 105, as quoted in Goffman, *The Presentation of Self in Everyday Life*, pp. 62-63.

68. Barlow, ed., *Autobiography*, p. 21. The original excisions from the autobiography were determined by family discussions in the spring of 1885 when Francis Darwin was considering the work for inclusion in the *Life and Letters*. Three positions were staked out on the issue. Emma, Darwin's wife, favored publication with omissions; George and Francis, two sons standing for science, favored complete publication; and Henrietta, an exceedingly proper and idiosyncratic daughter, argued for withholding the document entirely. Surprisingly, it was Henrietta who made some of the more interesting additions to what was a very heated family argument. In opposing her mother's position, for example, she touched on a point close to the center of this essay when she wrote her brother, "I never thought the idea of cutting out sentences was a practicable one and I do not think that cutting out short sentences is honest – that kind of alteration of a man's writing gets very near to the point of deceiving the reader." (Henrietta Darwin Litchfield to Francis Darwin, April 9, 1885, Darwin MSS, Black Box, University Library, Cambridge). As the senior party to the discussion, Emma Darwin won out, and the autobiography was published with the rawer remarks on religion and man excised. Appropriately, these excisions were restored by Nora Barlow, a Darwin grandchild. Her unexpurgated edition of the autobiography appeared in 1958.

perception of both. On the subject of man, costs ran highest. Here Darwin had worked out his views in private, without benefit of a contemporary audience. By the time he could speak freely, his emotional ties to the subject were gone, leaving him uncharacteristically cool to the sensivities of others. Lyell, with his hesitations, went unreassured. Moreover, since Darwin had worked out his conclusions on man in isolation from his peers, his positive contributions were not readily absorbed when published. Wallace, for example, complained of gratuitous assumptions in *The Expression of Emotions in Man and Animals*[69] and the work did indeed appear a free-standing monument to Darwin's genius until its relatively recent integration into the field of ethology. Conversely, other fields dealing with man, such as neurophysiology, a prominent science at mid-century, were left outside the argument for transmutation, "sequestered,"[70] as Robert M. Young has put it, from genuine intellectual contact with a theory to which they were logically allied. Thus the cost for science of privately kept knowledge was high. The integration of the human sciences into general evolutionary theory was left to future generations.

The discrepancy between private and public knowledge also led to some widely circulated misunderstandings about the impact of Darwin's work on the social sciences. For example, until recently the *Origin* had commonly been credited with a primary role in forming the new discipline of anthropology. In *Evolution and Society: A Study in Victorian Social Theory,* J. W. Burrow demonstrated the falseness of the common view, arguing that the *Origin* came too late to affect the basic intellectual orientation of the discipline.[71] Yet while Burrow is certainly right in the main, his view must be qualified. As Gay Weber has shown, anthropological thinking during the first half of the nineteenth century was dominated by just those sorts of biological questions – on race, anatomy, and the like – which we know interested Darwin in the 1830's.[72] Thus Darwin's work and the nascent science of anthropology now appear parallel and related developments. Only Darwin's pattern of publication prevented this relation from being appreciated earlier.

69. Alfred Russel Wallace, review of *The Expression of Emotions in Man and Animals, Quart. J. Sci.* (January 1873), 117-118.

70. Robert M. Young, "The Role of Psychology in the Nineteenth-Century Evolutionary Debate," in Mary Henle, Julian Jaynes, and John J. Sullivan, eds., *Historical Conceptions of Psychology* (New York: Springer, 1973), p. 187.

71. J. W. Burrow, *Evolution and Society: A Study in Victorian Social Theory* (Cambridge: Cambridge University Press, 1966).

72. Gay Weber, "Science and Society in Nineteenth Century Anthropology," *Hist. Sci., 11* (1974), 264.

Public understanding of Darwin's views on human society has been muddled for the same reason. From his writings it is not altogether clear what arrangement of human society Darwin believed natural. One based on competition? On cooperation? Or some other element? Here again Darwin's exteme caution in choosing and addressing an audience promoted the confusion. Even after fame had guaranteed him a public audience, he would not address it on all subjects. His inaction left the field to others. The divergence in application of his writings to society on both academic and practical questions was thus a cost of his strategy of self-presentation. Those, like Wallace, who were explicit in expressing their social views publicly were not penalized in the same way.

Yet the matter did not end there. Interested readers could not content themselves with Darwin's silence. Instead they pressed to learn his true opinion, often to enlist him on the side of one or another view of society.[73] However, insofar as their inquiries rested on historical reconstructions of the origin of his theory, they were doomed to fail or, at best, to succeed very partially. As we have seen, Darwin routinely deferred to the sensitivities of those audiences he chose to address. Only recently, with the availability of his manuscript, could his private views be known in detail. One can now show, for example, that Darwin came to intellectual maturity in a world not yet fully industrial, where slavery remained a major political issue. Similarly, one could show him pulled later in life by the concerns of a society both industrial and imperial. Yet even when the course of Darwin's private opinion is fully described, it must not be given undue weight in interpreting his legacy. If his works were read differently in a variety of political and disciplinary contexts, that fact must remain part of the record. Knowledge is, after all, a shared commodity. The Darwin as he was to himself must be held against, not substituted for, the Darwin as he was to others. What ambiguity Darwin

73. An example of such an attempt made recently on behalf of anthropology is Derek Freeman, "The Evolutionary Theories of Charles Darwin and Herbert Spencer," *Current Anthropology, 15,* no. 3 (1974), 211-235. In order to make Darwin not at all a social scientist, Freeman minimizes Malthus's contribution to Darwin's theory. In contrast, Marvin Harris, Freeman's opponent, makes Darwin too much a social scientist by not recognizing the variety and independence of intellectual settings in which he operated. See Marvin Harris, *The Rise of Anthropological Theory* (New York: Thomas Y. Crowell, 1968), chap. 5. A similar uncertainty over how much to regard Darwin as a social scientist exists in an otherwise informative article by James Allen Rogers, "Darwinism and Social Darwinism," *J. Hist. Ideas, 33,* no. 2 (1972), 265-280. Like Freeman, Rogers minimizes Darwin's debt to Malthus (pp. 275-276), which leads him to separate Darwin from Spencer and the Social Darwinists on spurious grounds (p. 280).

left in his wake must be charged to that very solicitude for audiences which enabled him to succeed. It was a cost of his strategy.

I began this section by contrasting Darwin's public and private lives in science during the years 1836-1839. I then showed how his privately pursued theoretical work developed over the period, until such time as he was pursuing several lines of inquiry simultaneously. Finally, I described how each line of inquiry reached the public separately, according to Darwin's perception of the requirements of disparate audiences. What remains to be shown now is how the line of inquiry which included man figured in the development of the entire schema.

3. MAN
Notebook B

The primary fact to be learned on the subject of man from Darwin's first full notebook on transmutation is that the subject held no terrors for him. Unlike nearly all the other principals in the English discussion on transmutation, Darwin came to the issue unalarmed at its implications for man. Why he differed from the others is a matter of conjecture, though the liberality of his family's views provides one obvious explanation. What is incontrovertible, however, is his initial lack of emotional engagement with the subject. From July 1837 when notebook B was opened and for six or seven months thereafter, man was discussed only as the subject pertained to the general question of species change. The transmutation of species, not the evolution of man, was clearly Darwin's concern. However, later, from February 1838 on, and for reasons central to the development of the theory, the subject of man became a leitmotif in the notebooks. To show how this happened, we must look at the two ways in which Darwin first went about integrating the subject into his theory.

The initial fitting of transmutationist theory to man pertained to geography. In the course of keeping notebook B, Darwin had to decide whether his own model of species change, which at that time was based on geographical isolation, was consistent with the majority view on the human species held by English zoologists and geologists, that is, that man was a single species of recent historical origin, divided into geographically separate races. After due reflection, recorded in notebook B, Darwin sided with the majority. He could agree that man was a single species on the usual basis of the fertility of interracial crossings. (He did, however, make inquiries on such unions.)[74] He found no fault

74. See B, pp. 32-34, 68-69e, 179-182.

with relating the worldwide distribution of the species to its attainment of reason and the arts (B, pp. 231-232, 244). He could also recognize human races as natural units. Indeed, since his theory derived from an understanding of geographical races as incipient species, he was entirely comfortable with the notion of race. And, finally, he saw no reason to doubt the universal conclusion that the human species was of recent origin in geological terms. Rather, he believed his theory added authority to the belief, for were man a geologically ancient species, more differentiation would probably have occurred among the races.[75] So on dating the emergence of man, as on the other points mentioned, Darwin saw only complementarities between transmutationist theory and the standard view of man as a biological species.

Conflict between transmutationist theory and the contemporary scientific consensus on man came in the second way in which Darwin tried to integrate the two. The point at issue was not man himself but his relative standing with respect to other species. Here the difficulty occurred largely because the standard scientific opinion held a fundamental contradiction concerning the ranking of species, which Darwin's probing soon exposed. As it stood for Darwin, the common agreement centered on the belief that the human species was qualitatively different from and higher than other species by virtue of possession of reason. It is well known that Charles Lyell, a keen anti progressionist, had come over to the progressionist side on just this one point. What made this scientific consensus peculiarly pressured, however, was that in maintaining it, English scientists found themselves aligned with the traditional Western view, which most shared, that man was, in some sense, the end of creation. Any change in the scientific view therefore threatened what was a nearly universal and highly satisfactory alignment of traditional and scientific, not to mention popular, opinion. When Darwin felt compelled to withdraw from this alignment, which he did in the course of keeping notebook B, he knew he had broken a trust. In his own mind, he had displaced the human species from its accustomed place in nature. From that point on, man became an issue on its own merits in the transmutation notebooks.[76]

75. B, p. 244. Darwin positioned himself nicely between the monogenists and the polygenists on this matter. With the addition of new evidence he could move either way.

76. As a rough index to Darwin's new interest in the subject, we may note that the word "man" appears about forty times in notebook B, about eighty times in notebook C. Even within notebook B, the increase is marked, for nearly half of the references to man occur in the last third of the notebook.

The question of the proper ranking of species arose as Darwin was considering the notion of progressive development. This term, by which Lyell had characterized the transmutationist beliefs of Lamarck,[77] represented at least two claims about species which Darwin, as a transmutationist, had either to accept or to deny. The first was the not necessarily transmutationist notion that lower species had preceded higher species in the earth's history. Progressionism of this sort had both traditional underpinnings, in that it recalled the notion of "the great chain of being" described in the classic work of that title by Arthur O. Lovejoy, and contemporary paleontological corroboration.[78] Also, progressionism had special appeal because it added substance to the traditional placing of man as the ultimate creation. As Darwin put it:

> Progressive development gives final cause for enormous periods anterior to man. Difficult for man to be unprejudiced about self, but considering power, extending range, reason and futurity, it does as yet appear cl....[79]

Yet Darwin did not remain satisfied with the implications of this view for long. Progressionism, at least on a rhetorical level, soon struck him as untenable, for it presumed a too simple ranking of organisms. Upon reflection, Darwin realized that all contemporary taxonomic systems recognized major divisions in the living world which required that the terms "higher" and "lower" be used only with respect to standards relevant to each grouping of organisms. Once he realized this, Darwin reconsidered the traditional ranking of the human species. His change of mind is recorded in the following key passage from notebook B:

> It is absurd to talk of one animal being higher than another. - We considert those, where the (cerebral structure intellectual faculties) most developed, as highest. - A bee doubtless would where the instincts were.[80]

77. See Lyell, *Principles of Geology,* II (1832), chap. 1.

78. See Martin J. S. Rudwick, *The Meaning of Fossils: Episodes in the History of Paleontology* (New York: Macdonald and American Elsevier, 1972), chap. 3.

79. B, p. 49. The remainder of this statement is unfortunately excised. The sense of it is, however, reasonably clear from the existing fragment.

80. B, p. 74. In this particular entry, Darwin's bracketing of physical and mental attributes - cerebral structure/ intellectual faculties - is also interesting since it is the first indication of attention in the notebooks to the mind-body problem.

Now in distinguishing the scales against which comparisons of species were to be made, Darwin was in no way original. He was simply appropriating a conceptual refinement which had already been drawn in science but was not universally observed in language. But in being more precise with his own language, he was able to articulate and then discard the view that made man the highest form in nature (B, p. 252). In the new view, man was the highest species in a scientific sense only on specified counts.

Of course, the case for progressionism did not rest solely with the ranking of species. Darwin had also to consider its relevance to the interpretation of the geological record. Notebook B contains an abundance of speculation on the subject. This is not surprising, since progressionism was the most contested issue of the day in geology, which it had become when Charles Lyell dared challenge it by way of his *Principles*. Very briefly, what stands out from Darwin's speculations on progressionism is his independence of judgment. From Lyell he took his central interpretive principle that geological strata form an imperfect record of the history of life on the planet (B, p. 201e). There could be no thought, therefore, of ever learning all the passages between forms. But Darwin also adopted two key tenets of the progressionists, namely, that the earth was a cooling body (B, p. 15) and that successive geological ages had produced increasingly more complex forms of life (B, pp. 19-20, 202e, 204-207). He was then led to consider what for transmutationist-minded progressionists was the last point: the notion of progressive *development* or, roughly, the belief that "the simplest cannot help becoming more complicated" (B, p. 18). Obviously, this conclusion had implications for the interpretation of man, who might be thought to be among the final products of such development.

Developmentalism as a model for transmutation had attractive features. It made the production of species seem close to more familiar generative processes in nature. Moreover, some facts fitted it nicely. For example, species extinction, that great anomaly in the traditional view, could be treated as a kind of dying analogous to deaths elsewhere in nature. But the overriding advantage of the developmental mode for the purposes of explanation was that it made appearance of new species seem but a more spectacular form of natural growth: species followed one another as surely as stages of growth in an embryo. As Darwin put it with respect to man: "If all men were dead, then monkey make men. – Man makes angels –" (B, p. 169). In the developmental model, then, man was one stage of the entire process of growth in nature. In a certain sense, everything still could be related to him.

Yet however compatible the developmental ideal was with respect to transmutation (and Geoffroy St. Hilaire was its partisan in France during the 1830's), Darwin rejected it in the course of keeping notebook B. He decided instead in favor of a more externally driven, accidental, and reticulate model. Why he did so is a matter of conjecture. Lyell's ridicule of the developmentalist concept of monads (which Darwin himself used for a time) was no doubt cautionary.[81] But probably more important was the fact that Darwin's own first model for species change – geographical isolation – was nondevelopmentalist. On its own, the mechanism of geographical isolation was compatible with the view that speciation was caused by changing external conditions more than by internal laws of growth. By the end of notebook B, Darwin was confirmed in this view. He thereupon rejected his earlier developmentalist view of human phylogeny, quoted above, in favor of the view that the human species was the accidental, not the inevitable, outcome of a long process of differentiation among species. He wrote: "Without *two* species will generate common kind, which is not probable, then monkeys will never produce man, but both monkeys and man may produce species" (B, pp. 214-215). Or, as he said a bit later in the notebook: "Fish never become a man" (B, p. 227).

Thus far had Darwin come by the end of notebook B. It was a long way indeed. In the space of eight months, from July 1837 to February 1838, he had settled the main parameters of how he would treat the human species. What is more, he had done so without leaving the broad tradition of natural history. Let me review his main conclusions: Darwin held that man was a geologically recent species divided into geographical races. He did not doubt that man was rational, but he rejected the notion that man was the end of creation, either in the sense of being its highest form or in the sense of being an inevitable product of the evolutionary process. Man, like other species, was accidental in his particulars. Were he destroyed, new forms would take his place in the economy of nature. What those forms might be was not predictable given the number of elements operating in the system.

Making man fully part of nature, and of his theory, satisfied Darwin immensely. He rejoiced that he found himself "netted together" with the animals, "our fellow brethren in pain, disease, death, suffering and famine, our slaves in the most laborious works, our companions in our amusements" (B, p. 232). What is more, he knew that successfully integrating another element into his theory would strengthen its simplic-

81. See Lyell, *Principles of Geology*, II (1832), 12-14.

ity and hence its persuasiveness.[82] By the close of notebook B, then, Darwin felt optimistic about the general prospects of his theory and intrigued with their relevance for man. He had by no means answered all questions. That which purportedly separated man from other animals – reason – he had not analyzed. Nor, having chosen an externally driven mechanism to explain speciation, did he regard himself as having explained functional adaptation. But he was pleased as he surveyed the road ahead of him. The following passage from near the end of the notebook displays his optimism:

> With belief of transmutation and geographical grouping we are led to endeavor to discover *causes* of changes, – the manner of adaptation (wish of parents??), instinct and structure become full of speculation and line of observation. – My theory would give zest to recent and fossil Comparative Anatomy; it would lead to study of instincts, heredity and mind heredity, whole [of] metaphysics. –[83]

Having accommodated the contemporary scientific view of man to his theory, he was now prepared to go beyond it. He opened notebook C delighted by the prospect.

Notebook C

Notebook C, filled from March to mid-July 1838, continued on the subject of man where notebook B had left off. The most immediately striking change in the new notebook is in tone. Whereas Darwin was customarily tentative and probing in his speculations on man in notebook B – though the mood shifted noticeably in its last third – he was exuberant, assertive, and boastful in notebook C. In a tone of raging self-confidence, he ridiculed the obscurantism of philosophers, exclaiming "Metaphysics!!" of a book by James Mackintosh and dismissing out of hand the claims of that more present figure William Whewell (C, pp. 55-56, 218). He even turned on his closest intellectual associate, instructing himself that "Lyell's Principles must be abstracted & answered."[84] And for the first time, he looked at religion critically.

82. On this topic see B, pp. 101-102.

83. B, pp. 227-228. For his complete summation of his anticipated work, see B, pp. 224-229.

84. C, p. 39. From here on Darwin regularly countered Lyell's arguments against transmutation in his own copies of successive editions of the *Principles of Geology*. Unfortunately, since Darwin always had the *Principles ready at hand, dating any of his*

Where he had previously avoided the subject, or made the familiar case that the glory of God was enhanced by the expansion of the domain of natural law, he now exclaimed: "Love of deity effect of organization, oh you materialist!" (C, p. 166). This was also the first occasion in the notebooks where Darwin described himself as a materialist. Doubtless his gradual disaffection from Christianity was well under way by this time.[85]

The source of Darwin's new level of self-confidence was his growing satisfaction with the explanatory powers of his theory. Not only was the material he was gathering from systematic reading fitting nicely into his scheme, but his survey of the literature revealed no serious competitors. Thus encouraged, he took the offensive. One of his first points of attack was the view of human reason held by his peers, a view he had taken on faith in notebook B. The majority of his peers drew a sharp line between man and animals with respect to mental attributes, while allowing a continuity of form on the physical level. In February Whewell had taken this approach once again in the course of reporting discoveries of fossil monkeys to his fellow geologists.[86] Had Darwin retained Whewell's view, however, he would have had to leave the question of the origin of

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marginal comments in the various editions is an uncertain business, a circumstance which has led to some scholarly disagreement over their proper dating. In my opinion, none of the external evidence brought forward thus far is sufficient to date precisely the important marginalia in Darwin's copy of the fifth edition of the *Principles,* published in 1837. Judging from the substance of these marginalia, however, I would place them in the period under discussion and would regard them as Darwin's "answer" to Lyell. During this time, Darwin was extremely self-confident, assertive on the subject of human orgins, but still working with a theory founded on geographical isolation rather than on Malthusian population pressure. These views also characterize Darwin's remarks in his copy of the fifth edition of the *Principles.* The latest I would date these marginalia is the end of September 1838, when Darwin did have Malthus to work from. For more on this subject, as well as direct quotations of Darwin's marginal commentary with respect to Lyell's view on man, see Part 1 of this article, pp. 257-258.

85. See Barlow, ed., *Autobiography,* pp. 85-87. In discussing the change in Darwin's religious views, one should be careful to observe his own distinction (p. 87) between his loss of belief in traditional Christianity, which occurred in the 1837-1839 period, and his later speculations about the nature and existence of God. Darwin's work on transmutation prompted his departure from traditional religion; it did not thereby make him an atheist.

86. William Whewell, presidential address, delivered February 16, 1838, *Proc. Geol. Soc. London 2* (1833-1838), 641-642. In a similar vein, see Lyell, *Principles of Geology* (1st ed., 1830), I, 155-156; and Edward Blyth, "On the Psychological Distinctions between Man and All Other Animals; and the Consequent Diversity of Human Influence over the Inferior Ranks of Creation, from Any Mutual and Reciprocal Influence Exercisd among the Latter," *Mag. Nat. Hist.,* n.s., *1* (1837), 1-9, 77-85, 131-141.

man's moral character and intelligence outside the scope of his theory – just as Whewell had proposed leaving these questions outside the scope of geology. This Darwin was unwilling to do on grounds of principle. To him such exclusions were inconsistent with the tenets of inductionism and suggestive of supernatural orders of explanation (C, p. 56).

Darwin's counterproposal was to assert a continuity between man and animals on all points (C, pp. 76-77). The gradation between physical form which all had observed would be presumed to obtain for mental attributes as well. Because of the principle of gradation, Darwin would assert that man was separated from the animals by a "hiatus" rather than a "saltus" (C, p. 154). To explain the steps in the gradation, Darwin invoked the notion of replacement of function, that is, that less definite mental activity (reason) replaced more definite mental activity (instinct) as one ascended the scale (C, pp. 77-78). (I use the word "scale" here intentionally since both gradation and replacement are notions which have a scalar aspect to them, in keeping with their eighteenth-century origins.)

A few comments are in order with respect to Darwin's aberrant views on reason. First, in taking the position that he did, Darwin was restating claims made by his grandfather Erasmus Darwin. Like Charles, Erasmus had argued that nature was graded and self-generative in an evolutionary sense and that reason was one of its principal "productions," common to lower as well as higher forms.[87] Without question, Charles was indebted to Erasmus for these opinions. What is even more interesting is that the very indebtedness to the opinions of a family member gave Charles the license to break with his peers on the sensitive subject of human reason. In aligning himself with Erasmus and his circle, Charles could be both a traditionalist – for he was returning to views of his fathers – and yet a radical. The result of all this for English science was that evolutionary theory with respect to the mind simply leapfrogged generations. Second, to a greater extent than Erasmus, Charles Darwin used the term reason in a zoological sense. All his field experience had taught him that the daily operation of the organism in its environment was the critical consideration. Therefore, he identified reason in animals (and mental activity in plants) by observing their actions. For Darwin an act was rational if it showed foresight and was not the uniform (and hence instinctive) response of the organism to

87. Erasmus Darwin, *The Temple of Nature* (London: J. Johnson, 1803). See, for example, canto I, "Production of Life," and canto III, "Progress of the Mind." The point could be made from Erasmus's other works as well.

similar situations.[88] Epistemological questions were secondary when they were considered at all. Last, as in all his theorizing, Darwin understood that his assertions concerning reason, and mental phenomena generally, would require empirical demonstration.

After reason, the next issue to be raised in notebook C with relevance for man was adaptation. The notion of progressive development which accounted for adaptation had been discarded in notebook B. The surviving mechanism of geographical isolation explained only speciation. Darwin was thus left to begin again on the subject of adaptation. He chose to start his inquiry at the point where organisms appeared to him most malleable, that is, with behavior. Now in taking this tack, Darwin knew he was on dangerous ground, since Lamarck had gone the route before him. Darwin thought, however, that he might avoid the fate that met Lamarck if he could rid himself of the least tenable aspect of the Lamarckian system. The unacceptable element in the system was what he took to be Larmarck's assumption that organisms act willfully or freely in filling their needs (C, p. 63). Otherwise, Darwin embraced Lamarck's explanation for the origin of functional adaptiation. Since this is not generally understood, the point requires emphasis, and this is most easily provided by quotation. Here, for example, is Darwin on the subject of the origin of webfootedness in birds: "a bird can swim without being web footed yet with much practice and led on by circumstance it becomes web footed" (C, p. 173). Or on the link between behavioral and structural change: "Hereditary ambling horses (if not looked as instinctive) then must be owing to hereditary ambling horses (if not looked as instinctive) then must be owing to hereditary power of muscles - Then we SEE structure gained by habit" (C, p. 163). Or again: "According to my views, habits give structure,... habits precede structure,... habitual instincts precede structure.–" (C, p. 199). In short, in the course of keeping notebook C Darwin became a Lamarckian.

Now Darwin began to study behavior in earnest. As a naturalist, he had always been extraordinarily careful to observe the habits of animals, but with his new understanding of adaptation the subject had taken on theoretical interest. As the origin of novelty in the organic system, behavior was the center of the Lamarckian scheme. Darwin was confident that he recognized the adaptive elements of behavior and their importance to the life of the organism. But he felt less certain of the manner in which behavior originates or becomes habitual. He was also

88. Because of the operational nature of this definition, it was often difficult to tell whether an act should be judged a rational or an instinctive one. Darwin cited the instance of a tit lark placing withered grass over a spied-on nest as being just such a difficult case (C, p. 189).

curious about how habits become hereditary. The following passage indicates the range of topics he was considering:

> Reflect much over my view of particular instinct being memory transmitted without consciousness, a most possible thing see man walking in sleep. – an action becomes habitual is probably first stage, an habitual action implies want of consciousness will therefore may be called instinctive. – But why do some actions become hereditary instinctive not others. – We even see they must be done often to be habitual or of great importance to cause long memory, – structure is only gained slowly. Therefore it can only be those actions which *many* successive generations are impelled to do in same way. – The improvement of reason implies diversity therefore would banish individual but general ones might yet be transmitted. – Memory springing up after long intervals of forgetfulness, – after sleep strong analogies with memory in offspring. or simply structure in brain people senses recollecting things utterly forgotten – Some association in such cases recall the idea it is scarcely more wonderful that it should be remembered in next generation. (C, pp. 171-172)

Especially noteworthy in this passage are the references to man. While Darwin could study behavior in any species, only in his own could he also consider the mental states of the organism. It was this newfound interest in mental processes which led Darwin to look outside natural history for information on man. His inquiries on the subject led to the opening of notebook M. Ultimately, of course, his interest in the human mind, like his interest in the general topics of habit, instinct, memory, and the heritability of mental phenomena, referred back to his Lamarckian understanding of the origin of adaptation.

Before leaving notebook C, we must also note that in it Darwin became concerned with finding subjects suitable for the empirical study of the theoretical issues he had raised. Thus he began to look for human subjects suitable for studying the relationship between mind and behavior. The experimental subject closest at hand was obviously himself. He therefore began to analyze his own behavior, particularly where it displayed a large measure of unconscious motivation. Here is one observation of this sort:

> Case of habit: I kept my tea in right hand side for some months, then when that was finished kept it in left, but I always for a week took of

> [f] cover of right side though my hand would sometimes vibrate seeing no tea brought back memory–old habit of putting tea in pot, made me go to tea chest almost unconsciously. (C, p. 217)

Similarly, he was interested in recalling his own earliest memories[89] for he saw there was a likely connection between memory and the implantation of new patterns of behavior. In addition to himself, Darwin also contemplated using other experimental subjects. He was interested primarily in those individuals who might be expected to have more habitual or instinctual behavior than normal. The inventory of subjects he considered with this in mind is instructive. Among those listed were: infants and children,[90] savages and foreign tribes (C, pp. 72e, 91e), the insane (C, p. 255), and sleepwalkers and those suffering fits (C, pp. 211-212). As it turned out, Darwin did not exploit all of these opportunities. But it is interesting to note that he held his list of possible experimental subjects in common with other nineteenth century figures.

Also on the empirical side, it is important to note that Darwin went to great effort to provide evidence for his assertion that the mental difference between man and animals was entirely one of degree. To do this, he required a system whose operation he could document where human and animal minds expressed themselves in identical or commensurable terms. This he found in the emotional realm. If he could show that man and at least some other animals possessed a similar system of emotional expression, he could substantiate his claim that there existed a gradation between mental phenomena in man and animal. With that in mind, Darwin set out to study the expressive behavior of the higher primates. He visited the London zoo for this purpose and spent time observing the orangutans and baboons (C, pp. 79, 154). His observations there convinced him that this was an "important" way of viewing the subject (C, p. 243). He knew he was not the first to think so (he was quite aware of the contributions of Charles

89. C, p. 242. In this entry Darwin recalls his first memory, which was of a holiday at the sea. He later expanded his account of his earliest memories in an autobiographical deposition; see n. 94.

90. See C, p. 70, and C, p. 236, on the instinct of sucking. Darwin began to make occasional observations on infants in the course of keeping notebook M, but his only full-scale study of infant behavior was done in 1840, following the birth of his first child. As with all his works on man, it was not published until much later. See Charles Darwin, "A Biographical Sketch of an Infant," *Mind: A Quarterly Review of Psychology and Philosophy, 2,* no. 7 (July 1877), 285-294.

Bell and his own grandfather on the subject),[91] but his reasons for desiring this sort of comparative study were his own. Eventually, in *The Expression of Emotions in Man and Animals* (1872), he was able to extend his observations and establish his position in a thoroughly systematic and empirical fashion. This work was not, of course, a complete confirmation of his claims respecting the continuity between man and animals since it did not touch on reason per se. But to Darwin's contemporaries the moral, and hence the emotional, faculty was of equal standing with the rational in distinguishing man from other species. So in demonstrating a commensurability between man and animals with respect to the expression of emotions, Darwin was addressing the issue of man's place in nature at what was an appropriate point for his contemporaries.

In sum, then, the major themes emerging from notebook C which pertain to man are: Darwin's overweening confidence in his theory, his assignment of reason to animals, his Lamarckian focus on behavior in relation to adaptation, and his interest in the empirical demonstration of his ideas. For the sake of completeness, I should mention that Darwin also considered man from other points of view in the notebook. Brief comments appear on human phylogeny (C, pp. 174, 234), racial inequalities (a circumstance he found at variance with the Christian imperative to brotherhood) (C, pp. 196, 204, 217), sexual differences (C, p. 178), and the place of man in creation (C, pp. 196-197). The last datable entry in the notebook is to an issue of *L'Institut* published on July 19, 1838. Notebooks D and M were opened on July 15.

Notebooks D and M

The two and one half months from July 15 to the end of September 1838 when notebooks D and M were filled was the most intense period in Darwin's career as a theoretician. He worked faster, more easily, and on a greater variety of questions then than at any other time in his career. During these months, he also came to see what place he might fill in the history of science. This perception was sealed by his apprehension in late September of a new explanatory principle derived from Malthus. Because of the importance of this period in Darwin's life, a few remarks on its overall significance are in order. These remarks pertain to Darwin's sense of himself during this period, his exploration of new fields, and the result of his exploration of his theory as a whole.

91. C, p. 243. The relevant works are Charles Bell, *Essay on the Anatomy and Philosophy of Expressions*, 2nd ed. (London: John Murray, 1824), and Erasmus Darwin, *Zoonomia; or the Laws of Organic Life*, 2 vols. (London: J. Johnson, 1794-1796).

A growing selfawareness

During the summer of 1838 Darwin became conscious of himself as a being in time, that is, as an individual freed for certain kinds of action by opportunities present to him in the moment. The most obvious sign of Darwin's new attitude toward himself was his effort to record and reconstruct his own experience. Starting on July 15, when he opened notebooks D and M he dated a large portion of his notebook entries (see Table 1). Previously, he had dated none. While he did not record a reason for the change, he probably did it to make his notes more useful to him in the future. He was beginning to regard his entire career as of a piece and wanted his daily work to be recoverable. In a similar show of selfconsciousness, Darwin also opened a new notebook in August which he styled his "Journal."[92] In it he listed all the significant events in his life up to that point, including what was to become the standard summary of his conversion to transmutationism.[93] Thereafter he used the "Journal" to record the important events in his private and professional life, and it served him as his main guide when writing his full autobiography in 1876. His first autobiography, however, also dates from the period under discussion. In August 1838, he set down a 1700 word account of his life from the date of his earliest memory when he was four to the year 1820, when he was eleven. The majority of these early recollections (guilt over stolen fruit, thoughts of a mother, the slip of a knife) remind one of Augustine or Proust. Yet a few sentences in the account suggest a feeling for nature – "the shady green road (where I saw a snake)" – combined with rampant curiosity – "the desire I had of being able to know something about every pebble in front of the Hall door" – which mark it as Darwin's own.[94] Whatever experimental use Darwin imagined for this early autobiography, writing it, like dating notes or beginning a lifetime diary, signified his emerging awareness of his own place in time.

Another way Darwin showed a new sense of himself was in strengthening his family ties. During these months, he sought out the opinions of his father, Robert, and his father's father, Erasmus. He also decided to form his own family. What is more, all this took place "at home" as he shuttled between the Darwins at Shrewsbury and the Wedgwoods, his

92. De Beer, ed., "Darwin's Journal." The cover of the original notebook held at University Library, Cambridge, is inscribed "Journal/Charles Darwin/August 1838."

93. Ibid., p. 7

94. "An Autobiographical Fragment," in *More Letters*, 1, 5, 3. The manuscript at University Library, Cambridge, is titled "Life written August 1838."

Table 1. Dated entries in notebooks D and M; they illustrate Darwin's new interest in dating his notes and also indicate the rate which notebooks were filled.

Date	Page in Notebook D	Date	Page in Notebook M
		July 15	1
		July 22	43
July 23	4		
		August 7	62
		August 12	81
		August 12	82
		August 15	82
August 16	36	August 16	83
		August 16	84
August 17	37	August 17	89
		August 19	89
		August 21	92
		August 23	99
		August 24	104
August 25	48		
		August 26	108
August 27	49		
August 29	50	August 29	111
		August 30	117
September 1	54	September 1	127
September 2	57		
		September 3	130
		September 4	128
September 8	70	September 8	132
September 9	70		
September 11	95		
September 11	152		
September 13	99	September 13	141
September 14	107		
September 17	113		
September 19	116		
		September 21	143
September 23	118	September 23	144
September 25	126		
September 25	134		
September 25	163		
September 28	134		
September 29	136		

mother's family, at Maer. Notebooks D and M were opened at Shrewsbury, and Darwins and Wedgwoods figure prominently in them. In particular, Charles's notes on conversations with his father during the week of July 15-22 occupy most of the first forty-three pages of notebook M. With respect to marriage per se Darwin's decision was reached after the most utilitarian calculation. Like a Benthamite accountant, he arrived at his decision by toting up in two columns the points for and against marriage, finally yielding to the prospects of a "nice soft wife" who, if things worked out, whould be "better than an angel and had money." But, significantly, the "angel" he chose was Emma Wedgwood.[95] Charles proposed to her in November and they were married in January. Their fathers' gifts, including a promise of annual income from the Wedgwoods, granted them financial independence. Thus when Darwin celebrated his thirtieth birthday in February 1839, he could contemplate a future carefully arranged within a familiar society.

What difference this change in life made to Darwin's scientific career must now be considered. The irreversible effect of marriage had, of course, been anticipated, that is, that children and responsibility would take the place of geological travel and freedom. Yet at a more profound level, marriage for Darwin was a slightly belated sign of a shift between promise and fulfillment in his work as a theoretician. This shift occurred precisely at the time of his reinvolvement with his family. It was aided by this reinvolvement in three ways. First, Darwin's conscious identification with his family afforded him a model of success at the level of operation of the general culture. This point requires emphasis since an overdrawn parallel exists in the minds of many between Erasmus Darwin and Jean Lamarck, who remained a controversial figure throughout the period. In contrast to that of the unfortunate Lamarck, the reputation of Erasmus Darwin had risen sharply by the 1830's. Indeed, Charles could pride himself that his grandfather's 'prophetic spirit' was the talk of London, citing his distinguished contemporary Thomas Babington Macaulay, the historian, for the opinion.[96] Second, the wide interests and abilities of the Darwin-Wedgwood circle gave Charles access to areas outside his initially narrow professional concerns. As I shall show in a moment, this access was of extraordinary

95. From the appendix "This Is the Question" in Barlow, ed., *Autobiography*, pp. 233-234. For the letters between Charles and Emma, see Henrietta Litchfield, ed., *Emma Darwin: A Century of Family Letters*, 2 vols. (London: John Murray, 1915).

96. Charles to Catherine Darwin, undated but 1837 or 1838. Darwin MSS, vol. 92, University Library, Cambridge.

importance to the development of his theory on species. And, finally, the philosophical predilections of his Wedgwood kin, their Unitarianism and rather phlegmatic rationalism, fortified his own claims as a scientist respecting secondary causation. In this sense, one can say that when Darwin expressed repugnance to the notion of the special creation of species with the epithet "bad taste," he was but sounding Wedgwood antipathy toward trinitarian Christianity in a different key.[97] On all these counts, then – the presence of a successful 'prophetic' model, access to new fields, the provision of a satisfactory and supporting cosmology – Darwin's conscious reassociation with his family gave him an identity which set him apart from other members of his scientific fraternity. Many of Darwin's colleagues were well established in the general culture, but few of them had entered it at the same point.

Exploration of new fields

Until he opened notebook M, Darwin had not ventured beyond the broad reaches of natural history. Rather surprisingly, even his new view of man had been drawn from its domain. But when Darwin became interested in the subject of adaptation and behavior, he crossed the boundary of natural history into other areas. This point can be demonstrated by examining his initial reading list for the period. The disparate nature of the entries on the list is clear at a glance:[98]

~~Rays Wisdom of~~ Nothing	
Lisiansky's Voyage round world. 1803-6	
Lyell's Elements of Geology	
Gibbons life of himself	
Humes do, with correspond. with Rousseau	"Hume": M-104, 155; N-101, 184; vol. 91,
Miss Martineau How to observe	M-75, 76, 142, 151
Mayo Philosophy of Art of Living	D-49; M-11, 110, 114, 116, 126; N-31

97. D, p. 37. See also M, p. 154e.

98. The list is taken from C, p. 270. Darwin's entries appear in the left-hand column, notations of references to these works in his notebooks in the right-hand column. "Vol. 91" refers to the location of a set of Darwin's reading notes at University Library, Cambridge. The list first appeared in print in Sydney Smith, "The Origin of the '*Origin*,'" *Adv. Sci., 16,* no. 64 (1959), 395.

Several of Walter Savage Landor's Imaginary Conversations – very poor		
Sir T. Browne's Religio Medici.		D-54; M-126; N-184
Lyell Book III. There are many marginal notes.		
Mitchell's Australia.		M-132; D-71, 77, 99
Walter Scott's Life 1 & 2nd & 3rd Volumes		M-26, 129; N-19, 46
Abercrombie on the Intellectual Powers		D-56; M-114, 141; vol. 91
Hunter's Animal Economy edited by Owen. read several papers – all that bear on any of my subjects		D-many refs.: M-44, 147
Elie de Beaumont's IV Vol. of Memoirs on Geolog of France= on ~~Etna almost reread the previous volume~~ & C. Prevost on l'Ile Julie		
Waterton's Essays on Natural History	Octob. 2nd	N-1; D-148
Transaction of Royal Irish Academy	-- do.	D-164; M-145; N-6, 9, 10
Lavater's Physiognomy		
Malthus on Population		D-134, 135; E-136; N-10; vol. 91
W. Earl's Eastern Seas.	October 12th	E-18, 19, 182

The compiler of this list was clearly reading for more than one purpose. Darwin admitted as much when he noted with respect to Hunter that he should read "all [papers] that bear on any of my subjects." If, however, one eliminates the works which one might expect a naturalist to consult – works on geology, zoology, and travel – one arrives at another list which reflects an identifiable complex of subjects. This list includes literary works (Gibbon, Landor, Scott), medical works, including, loosely, all those by physicians (Abercrombie, Browne, Lavater, Mayo), and works in philosophy and political economy (Hume, Malthus, Martineau).[99]

99. Exact citation of works from the list would be misleadingly specific in most cases since Darwin did not read every work on the list (though he may have read others by the same author), and of those he did read the exact edition consulted is not always known. Full names of authors and brief titles of the works listed are as follows: Edward Gibbon, *Autobiography;* Walter Savage Landor, *Imaginary Conversations*; J. G. Lockhart,

The disparate nature of these subjects presents the question of how Darwin went about compiling his list. It does not seem that one book led him directly to another; nor, in the absence of a formal discipline of psychology, was there anything like a standard reading list for him to draw on. Moreover, with the possible exception of Johann Caspar Lavater, the authors listed were commonly known in literate circles. Darwin could have come across any of their works at the Athenaeum, the London club to which Lyell had proposed him and where he did in fact do much of his reading, particularly of periodicals. But apart from works of literature, to which any of his contemporaries might have referred him, the combination of subjects represented by these books suggests a more obvious instrument of selection, which is that in choosing these books Darwin was acting on advice from members of his family. Medicine, after all, was the profession of his father and his grandfather, as well as a field where he had received preliminary training. Moreover, medicine was tied to philosophy within the family: Erasmus Darwin's theory of disease derived in part from the work of David Hartley. Notebook M contains numerous references which suggest that the associationist psychology of Hartley, with its accompanying empiricism, remained an oral tradition within the Darwin household. Charles's father, Robert Waring Darwin, maintained the medical side of the tradition, Charles' brother, Erasmus, nicknamed "Philos," the philosophical. The name of David Hume on Charles's reading list thus reflects a family taste for empiricism in philosophy. In political economy, there was a similar correspondence between Darwin's reading list and the interests of his extended family. The Wedgwood pottery works at Etruria were among England's leading new industries, and members of the family were entirely conversant with publicists for the new order. In this connection, it is also interesting to note that Charles's brother, Erasmus, was then a warm friend and frequent companion of Harriet Martineau, who, among her other accomplishments, was known for her popularization of Malthus. In sum, then, it would appear that when Charles Darwin started to study human behavior he looked to other Darwins and Wedgwoods for advice. Their traditional pursuits and

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ed.,*Memoirs of the Life of Sir Walter Scott, Bart.;* John Abercrombie, *Inquiries concerning the Intellectual Powers and the Investigation of Truth;* Sir Thomas Browne, *Religio Medici;* Johann Casper Lavater, *Essays on Physiognomy for the Promotion of the Knowledge and the Love of Mankind*, trans. Thomas Holcroft; Herbert Mayo, *The Philosophy of Living;* David Hume, *My Own Life* and *Private Correspondence of David Hume* (1820); Thomas Malthus, *An Essay on the Principle of Population;* Harriet Martineau, *How to Observe Morals and Manners.*

tastes directed his reading. Medicine, associationist psychology, and the new political economy triumphed over theology, continental philosophy, history, law, and whatever other subjects might pertain to human behavior.

By now Darwin was reading on half a dozen subjects. Nothing better suggests the complexity in his intellectual life during these months than his contemporary entries in his "Journal":[100]

"Journal" page 26	"Journal" page 27
1838 reached Shrewsbury July 13th July 29th Set out for Maer	Very idle at Shrewsbury, some notes from my Father & opened note book connected with metaphysical enquiries.
August 1st London. Began paper on Glen Roy and finished it.	August. Read a good deal of various amusing books & paid some attention to Metaphysical subjects.
September 6th Finished paper on Glen Roy one of the most difficult & instructive tasks I was ever employed on.	All September read a good deal on many subjects; thought much about religion.
Sept. 14th Frittered these foregoing days away on working on Transmutation theories & correcting Glen Roy. Began Crater of Elevation theory.	
October 5th Began Coral Paper requires much reading. 25th Went to Windsor for two days rest, glorious weather, delightful.	Beginning of October do [ditto].

100. De Beer, ed., "Darwin's Journal," p. 8. In the original manuscript, the items listed in the left-hand column appear on p. 26, the items in the right-hand column on p. 27.

As one can see from the two columns, Darwin divided his authorized work as a geologist from his "frittering" on transmutation theories. This was his way of managing his affairs. He was to himself primarily a geologist in private as in public. Throughout notebook M, he presents himself that way: "I a geologist" or, when speaking of his speculations on the effects of changes in sea level, "this greatest mental effort of which I am capable –" (M, pp. 40, 90). Yet for all his private deference to his public persona, he did not hesitate to enter new fields. Indeed, he was extraordinarily good at bearing the sorts of tensions involved in pursuing more than one subject at a time. This was so even where his inquiries were unauthorized by professional convention and offensive to public taste. For example, during the very months when he was working hardest on the Glen Roy article, which he anticipated would bring him lasting recognition as a geologist, he was observing sexual behavior in himself and others.[101] Moreover, he did all this without losing that simplicity of manner which led one privileged but truthful observer to describe him as "the most open, transparent man I ever saw."[102] What one can conclude from this is unclear, except that Darwin was a man of complex interests and simple manners and that somehow the two seem to have been related. Clearly, he moved from field to field with unusual ease.

Significance of Darwin's exploration of new fields

In discussing the significance of Darwin's entry into new fields, one must distinguish immediately between *anticipated* and unanticipated results. Darwin *anticipated* that he would fill out his behavioral explanation of adaptation with material drawn from areas beyond the limits of natural history. This did happen. But it was the *unanticipated* effect of his new pursuits which mattered in the long run, for it was in the course of his expanded program of reading that he came on Thomas Malthus's *Essay on the Principles of Population*[103] As I have described

101. To wit: "Case of Shrewsbury gentleman, unnatural union with turkey cock, was *restrained* by remonstrances with him" (M, p. 18). Or: "We need not feel so much surprise at male animals smelling vagina of females. - when it is recollected that smell of one's own pud is not disagree[able]" (M, p. 85), "Pud" derives from the Latin *pudenda*, the private parts. See Gruber and Barrett, *Darwin on Man*, p. 317.

102. Emma Wedgwood to Jesse Sismondi, November 15, 1838, in Litchfield ed., *Emma Darwin*, II, 6.

103. See D, pp. 134e-135e, dated September 28, 1838. Editors of this passage have identified the key sentence as being from Thomas R. Malthus, *An Essay on the Principle of Population*, 6th ed., 2 vols. (London: John Murray, 1826), I, 6.

elsewhere, Darwin gained from Malthus an understanding of intraspecific competition around which he built his theory of natural selection.[104] Selection quickly became the primary locus of his theory on species. Thus Darwin's entrance into new fields altered his theory in a direction and to an extent which he could not have imagined beforehand. Once Darwin had natural selection in hand, he could relegate the question of the source of variation to a secondary position in his entire theory. This allowed him to publish a theory of evolution without simultaneously advancing a theory of genetics. Thus because of the enormous effect of Malthus on Darwin's work, biology remains permanently indebted to the field of political economy, as it does to the ability and willingness of certain individuals like Darwin to transgress the boundaries between fields.

Now if Darwin's intentions were as I have described them, then he certainly did not read Malthus's *Essay* "for amusement," as he said in his autobiography.[105] Only when one recalls that Darwin described time spent on transmutation theory as "frittered away" does his self-depreciation with respect to his reading of Malthus seem comprehensible, if vastly disingenuous. But so that the point may be certain, it should be emphasized that there is no serious reason to doubt that Darwin read Malthus in the context of reading other works in philosophy and political economy. It should be noted, for example, that immediately upon gaining an appreciation of Malthusian principles of population, and this on page 6 of the first volume of the *Essay,* Darwin went on to finish the work, assimilating Malthus's observations on human behavior to his own. For example, in notes dated October 2, 1838, Darwin cited the views of both Malthus and Adam Smith in considering the adaptive value of strong and presumably ancient emotions.[106] Similarly, later that week Darwin noted that Malthus had explained in an analytical fashion why chasity was regarded as a virtue in women (N, p. 10). Darwin also reminds himself to check Malthus's views on "passions of mankind, as being really useful to them" before developing his own view of the "origin of evil passions" (N, pp. 10-11). Thus, quite in the manner expected, Darwin found Malthus's views on human behavior

104. See Sandra Herbert, "Darwin, Malthus, and Selection," *J. Hist. Biol. 4,* no. 1 (Spring 1971), 209-217.

105. Barlow, ed., *Autobiography,* p. 120.

106. Notes dated October 2, 1838. Darwin MSS, University Library, Cambridge. vol. 91. Cited by Barrett and Gruber, *Darwin on Man*, p. 390. The word which Barrett and Gruber read as "[named?]" has been read by Peter J. Gautrey as "married," which appears to me more consistent with the sense of the passage.

absorbing. The fact that he received an unexpected insight from the first few pages of the *Essay* did not deter him from reading the entire work with his original end in view.

Since Darwin read Malthus just as he finished notebook M, the notebook itself does not record that encounter. It contains observations of the more anticipated variety. Still, the notebook is so rich thematically that it is difficult to describe in a balanced way. Historians will read through it and observe its Lamarckian impulse. Scientists will see in it the outlines of an evolutionary psychology. Other readers will fasten on other themes, not least those of a self-relevatory nature. Yet without touching on every theme from the notebook, it is possible to suggest a formula which relates each to the whole. The formula is drawn from Darwin's contemporary statement about his method. It reads: "The line of argument often pursued throughout my theory is to establish a point as a probability by induction, & to apply it as hypothesis to other points, & see whether it will solve them" (D, p. 117). What came of this method was a structure of inference whereby points radiated from each other rather than forming an ascending series. This structure did not resemble the inductive pyramids invoked by Adam Sedgwick.[107] Rather it was like a net with each point having connections in more than one direction. The weight of the theory was thus distributed throughout its structure. With this structure, particular points could be discarded or bonds between dissolved or rearranged without destroying the argument as a whole.

The point of departure in notebook M was the Lamarckian notion of adaptation. Darwin could have enhanced its probability in any number of ways. In hindsight, the roads not taken are most obvious. There is nothing in the notebook about a physical conduit for inheritance. Nor is there anything explaining the transfer of behavioral characteristics from the individual to the species. Indeed, the line between individual and species is not clearly drawn. But genetics aside, the kinds of inquiries Darwin made in the notebook are on the mark. For example, being keen to show that the mind does affect the body, he recorded a number of case histories from his father which show as much. Similarly, he collected examples suggesting that supposedly rational human beings behave habitually or instinctually on all sorts of occasions. And he did examine what seemed to him difficult requirements of the Lamarckian scheme.

107. [Adam Sedgwick], "Objections to Mr. Darwins Theory of the Origin of Species" [1860], in David L. Hull, *Darwin and His Critics* (Cambridge, Mass.: Harvard University Press, 1973), p. 160.

For example, he saw that it required him to believe that the mind operated on at least two levels. On the conscious level, the mind was a *tabula rasa*. On the unconscious level, it was disposed toward more habitual or instinctive action. Further, he saw that the mind must operate on these two levels simultaneously. To convince himself that this was so, he recorded instances where the split between conscious and unconscious modes was clearly marked. He expected splits to be most observable among individuals in extreme conditions - the very young, the very old, the insane. Hence many of the case histories he took from his father the week of July 15 pertain to insanity. For a similar reason, he sought to analyze borderline states of consciousness such as dreams (M, pp. 111-113, 143-144) or states, like the playing of music from memory (M, p. 8), where unconscious knowledge was acted on. Finally, by midway through the notebook, he was satisfied that his presumption was factual. Moreover, he concluded from his empirical inquiries that the unconscious mind was not necessarily accessible to conscious recall and manipulation. He termed the conscious-unconscious split "double consciousness" or, in associationist language, the coexistence of separate trains of thought. He noted of it:

> The possibility of two quite separate trains going on in the mind as in double consciousness may really explain what habit is. In the *habitual* train of thought one idea calls up other & the consciousness of double individual is not awakened. - (M, p. 83e)

Understanding this sort of mental layering filled out the Lamarckian scheme. To his own mind, it increased its probability.

Once it was established as a probability, Darwin applied his Lamarckian understanding of behavior to other points. With it he could assign a meaning to the inheritance of mental characteristics across generations - why "I [am] a Darwin & take after my Father in heraldic principle" (M, p. 83). He could interpret the uneven distribution of ability in a similar manner. Here is a particularly striking example on this score:

> My father has somewhere heard (Hunter?) that pulse of new born babies of labouring classes are slower than those of Gentlefolks, & that peculiarities of form in trades (as sailor, tailor, blacksmiths?) are likewise hereditary, and therefore that their children have some little advantage in these trades.- (M, pp. 44-45)

As a Lamarckian, Darwin could also explain the differential in learning ability between young and old. Children, he observed, learn more readily than adults, which is entirely predictable if one considers the Lamarckian principle that only what one knows before producing children can affect the next generation (M, pp. 82, 104). On a cultural level, Darwin used the Lamarckian hypothesis to account for the paradoxical nature of man's conduct: "Evil passions" like anger and revenge were once preservative and hence ingrained in man's nature, while "good" or cooperative inclinations are adaptations to the more recent requirements of civilized life (M, pp. 123, 125). (As a practical corollary to these views, Darwin advocated universal education, which he believed would further the implantation of virtue in the race. [C, p. 220].) Finally, Darwin sought to multiply the links between his understanding of behavior and points already established in his theory. For example, he noted a complementarity between his understanding of expression and his goals in classification: recognition of differences in behavior, as between the New and Old World monkeys (M, pp. 142-143), would be required for the construction of a system of classification based on phylogeny. In sum, then, as of the end of notebook M, the Lamarckian notion of adaptation, and its complementary understanding of behavior, occupied an important, even central, place in Darwin's theory. It seemed probable and it fitted well with other points in the theory.

In the course of keeping notebook M, Darwin also began to look at the entire theoretical network he had created from what might succinctly be characterized as a "metatheoretical" point of view. The most obvious sign of his new perspective was his decisive alignment of his theory with the general philosophical principles of materialism and determinism. For example, he argued in the following passage in favor of the materialist position that mental phenomena were sufficiently accounted for by the action of physical circumstances. He did so in unmistakably associationist language:

> Hensleigh [Wedgwood] says to say *Brain* per se thinks is nonsense; yet who will venture to say germ within egg cannot think – as well as animal born with instinctive knowledge. – but if so, yet this knowledge acquired by senses, then thinking consists of sensation of images before your eyes, or ears (language here means of exciting association) or of memory of such sensation, and memory is repetition of whatever takes place in brain when sensation is perceived. – (M, pp. 61e-62e)

The second issue on which Darwin asserted a philosophical preference was determinism. Here he took the traditional position that free will was illusory (M, pp. 27, 126). And he had recourse to the traditional materialist position in explaining the illusion: "I verily believe free will and chance are synonymous.–" (M, p. 31). This left him to account for novelty by means of the mere conjunction of previous thoughts:

> When a thought is thought very often it becomes habitual & involuntary, – that is involuntary memory, as in sleep. – a new thought arises? compounded of the involuntary thoughts. – (M, p. 46)

As mentioned earlier, such extreme associationism allowed Darwin to fend off the voluntarism which had destroyed Lamarck. Now, on neither the mind–brain nor the will–determinism issue was Darwin breaking new ground. Rather he was announcing what philosophical positions were consistent with his theory.

In a second "metatheoretical" effort, Darwin sought to place his theory within the setting of human knowledge generally. The most obvious setting was historical. Here Darwin found congenial the views of August Comte, whose classic work the *Cours de philosophie positive* he had read a review of in August.[108] Darwin explicitly identified the order of explanation represented by his theory with Comte's final stage of knowledge, namely, the scientific or "positive" stage as opposed to the theological or metaphysical stages (M, p. 70). Then Darwin went a step further. He imagined what a third-stage theory could do to reconcile disputes from what appeared to him more primitive orders of explanation. This occurred to him particularly with respect to philosophy. For example, in ethics there had long been contention between the utilitarians and those who believed in an innate moral sense. Darwin observed that evolutionary theory encompassed both schools:

> Two classes of moralists: one says our rule of life is what *will* produce the greatest happiness. – The other says we have a moral sense. – But my view unites both & shows these to be almost identical. What *has* provided the greatest good or rather what was necessary for good at all *is* the instinctive moral sense.[109]

108. M, p. 81. See [David Brewster], review of Auguste Comte's *Cours de philosophie positive, Edinburgh Rev., 67* (July 1838), 271-308.

109. Darwin MSS, University Library, Cambridge, vol. 91. Notes dated October 2, 1838.

Similarly, in epistemology he believed his work united the rationalist and empiricist positions. His own understanding of adaptation was exemplary in this respect, for it relied equally on the rationalist belief in innate ideas and on the sort of rank empiricism which posited the infinite capacity of the individual to interpret new experience. Nowhere is his interest in subsuming philosophical traditions under his own theory better marked than in the following passage:

> Plato. . . says in Phaedo that our "*necessary ideas*" arise from the preexistence of the soul, as not derivable from experience. – read monkeys for preexistence.–[110]

However presumptuous his remarks, they indicate a new stage in his own development. By the end of notebook M, and quite apart from Thomas Malthus, he had clearly transcended his initial interest in species to place the theory of evolution in a more generous framework.

Notebooks E and N

Like notebooks D and M, notebooks E and N were paired. The first pair was opened July 15, 1838, the second pair October 2, 1838. The two pairs differed primarily in the level of interest Darwin showed in them. Notebooks D and M were kept in a period of increasing excitement with their subject matter; notebooks E and N in the period of relative detachment which followed the reading of Malthus. Darwin's change of heart is apparent in the change in the rate the notebooks were filled. Of the first pair, notebook D was filled at the rate of 2.28 pages a day, notebook M at a rate of 1.99 pages a day. In contrast, notebook E to the last dated entry on page 174 was filled at the rate of .65 pages a day, notebook N to the last dated entry on page 97 at a rate of only .33 pages a day.[111] The slow filling of notebooks E and N signaled the ending of the series. The tone of the entries in these notebooks also changed. There are fewer defiant remarks, fewer exclamation points. The tone is subdued, thoughtful, precise. As one might expect, these changes did not occur

110. M, p. 128. Barrett, *Darwin on Man*, reads "imaginary ideas" for Skramovsky's "necessary ideas." Having consulted the original text, which shows a clear double *s* in the middle of the first word, I prefer Skramovsky's reading.

111. The figure for notebook N may be somewhat low since a number of notes in Vol. 91 of the Darwin MSS date from this period. Even so, by including these notes the total number of pages on the subject of man written during this period would not be doubled.

overnight. The high spirits and hard work of the late summer carried through the month of October. During that month, Darwin made entries in notebooks E and N at the rate of about a page a day. After that the rate of note taking in notebook E fell off gradually, in notebook N more abruptly (see Table 2).

As a whole, notebook N seems both a continuation of notebook M and its terminus. Without question, Darwin carried his program of reading over from one notebook to the next. Thus the reading list for notebook M reproduced in the previous section ends with references to works read after the opening of notebook N, as, for example, the book whose title headed the first page of the notebook, Charles Waterton's *Essays on Natural History* (N, p. 1). Moreover, on the primary reading list for notebook N, which begins in notebook C on page 269, one finds the same distribution of subject matter which characterized notebook M. Similarly, in the notebook one finds the same presence of family members, both as subjects of observation and as contributors to the discussion. There is also continuity in content between the two notebooks. Nearly all the old issues reappear in some form, and no important new ones enter. Indeed, the near identity of themes between the notebooks is itself a sign of closure. In notebook N, Darwin is easing himself out of the concerns which compelled him to open notebook M. He consolidates some work, refining its definition. Other work he abandons or postpones indefinitely. Still other questions are given rough answers which would do until he, or someone else, would flesh them out. What one is left with, then, is a sense of ending, of completion.

Nowhere is this sense of ending more apparent than in Darwin's handling of the subject of behavior. His chief effort was now directed toward ordering his conceptual scheme. For example, he went to some length to distinguish the mental *faculty* which would allow a bird to find its direction among the stars from the mental *instinct* which impelled it to migrate in the first place (N, pp. 76-80). Similarly, he was keen to provide definitions for "habit," "hereditary habit," and "instinct." The problem was to assign each to a separate domain. He decided in favor of defining instinct as whatever behavior was universal to the species: "We know not how stonge henge [*sic*] raised, yet not instinct, but if all men placed stone in same position, it would be instinct" (N, p. 77). Habit, then, became whatever pattern of repeated behavior was characteristic of the individual, and hereditary habit the transmission of that pattern from generation to generation. This sequence was, as he admitted, "almost identical" with Lamarck's (N, p. 91).

In the process of establishing a set of definitions, however, Darwin

Table 2. Dated entries in notebooks E and N.

Date	Page in Notebook E	Date	Page in Notebook N
		October 2	1
		October 3	4
October 4	4		
		October 8	13
October 10	9		
October 12	19		
October 13	20		
October 14	22		
October 16	23		
October 19	25	October 19	18
October 25	31		
October 26	32		
		October 27	26
		October 30	33
November 1	37		
November 7	55		
		November 20	37
		November 27	41
		November 28	45
December 2	59		
December 16	69		
December 21	85		
December 25	86		
		December 27	56
January 6	89	January 6	59
		January 21	60
January 29	98		
		February 12	62
February 24	98		
March 5	100		
March 6	104		
March 9	106		
March 11	112		
March 12	114		
March 16	119	March 16	63
		March 16	64
March 20	122		
April 3	129	April 3	75
April 6	133		
April 12	136		
May 4	141		
May 27	162		
May 29	165		
June 18	168		
June 26	174		
		July 20	97 [pages 128-182 blank]

was provoked to a second thought. In an unrepresentative but interesting passage, he departed from his usual line of reasoning to consider the possibility that chance rahter than behavior might be the origin of novelty in the system:

> An habitual action must some way affect the brain in a manner which can be transmitted. – this is analogous to a blacksmith having children with strong arms. – The other principle of those children which *chance*? produced with strong arms, outliving the weaker ones may be applicable to the formation of instincts independently of habits. – the limits of these two actions either on forms or brain very hard to define. (N, pp. 42-43)

The passage neatly defined the poles between which Darwin's opinion oscillated in the next forty years. By no means was he discarding his plausible construct of the passage from habit to hereditary habit to instinct to structure across generations. Rather he was observing that it was "very hard to define" the "limits" of those two actions (chance or habit) on "forms or brain."

A project he did abandon decisively was that of assimilating traditional philosophy into an evolutionary scheme. Some of what I termed his metatheoretical interests remained. For example, he heartily endorsed John Herschel's defense of secondary causation (E, p. 59) and, drawing on Comte, was pleased to be able to classify contemporary zoology as "purely theological" (N, p. 19e). Yet he gave up his effort to subsume traditional philosophy under the rubric of his own theory. Why he did so is a matter of interest. His later explanation that he was not "well fitted"[112] for metaphysics will hardly do, especially when as late as the opening of notebook N he claimed his studies would provide a "stable foundation" for the subject (N, p. 5). At the very least, one must say that he dropped the subject too soon. In October 1838 he had been at philosophy only two and a half months, hardly enough time to penetrate the subject. Furthermore, his own vocabulary and conceptual scheme were so indebted to a broadly empiricist tradition, for which the name of Francis Bacon was emblematic, that it was impossible for Darwin to break with that tradition, even where he had cause to. Consider, for example, his comments on a review article on Samuel Taylor Coleridge:

> P. 267 says the great division amongst metaphysicians – the school of Locke, Bentham & Hartley, & the school of Kanto to Coleridge is

112. Barlow, ed., *Autobiography,* p. 85.

> regarding the souurces of knowledge - whether "anything can be the object of our knowledge except our experience." - is this not almost a question whether we have any instincts, or rather the amount of our instincts - surely in animals according to usual definition, there is much knowledge without experience, so there *may* be in men - which the reviewer seems to doubt.[113]

Clearly, Darwin would have liked to side with Kant and Coleridge on the question of knowledge prior to experience. But just as clearly he felt unable to. The fact that the anonymous reviewer was the young J.S. Mill did not help, for though respectful toward Kant and Coleridge, Mill read them with the skepticism of one educated in another tradition. And yet - and here one feels the isolation of disciplines - even with the most impartial reviews at hand, Darwin could not have subsumed philosophy into his scheme without knowing a great deal more than he did about the history of its distinctions. Thus, probably because he recognized his ignorance and was unwilling to carry the matter further, he abandoned his claims.

Yet Darwin did not leave behind all the vaster claims of notebook M. In particular, he remained committed to the belief that human behavior generally would be explained under the terms of his theory. He no longer presumed that he would do the major part of the work of explanation himself. Indeed, after a rash of notes on the familiar subject of the expression of emotions, he had to remind himself to read a certain author "if I follow up this subject" (N, p. 103). But whatever his plans for research, his interest in human behavior continued. Notebook N is filled with observations on the subject. Characteristically, many of them concern quite small details. For example, he seems to have taken genuine delight in noting the pleasure children have in "skulking about in shrubbery, when other people are about." He explained such behavior as the "hereditary remains of savages state" (N, p. 66). He also considered the origin of more general systems of human behavior, such as language: "Probably, language commenced in some necessary connexion between things & voice, as roaring for lion" (N, p. 20e). With respect to morals, some rules of behavior posed little problem under his theory. The taboos against suicide and homosexuality were readily understandable as being opposed to basic instincts affecting survival of the species (N, p. 99). Other rules which seemed to disadvantage the

113. Darwin MSS, University Library, Cambridge, vol. 91. See [J. S. Mill], review of works by and about Samual Taylor Coleridge, *London and Westminster Rev., 23,* 2 (March 1840), 257-302.

survival of the species seemed more troublesome. Observe in the following example how intent Darwin was to show that the general moral injunction toward self-sacrifice in certain situations was not destructive overall because the obligation did not fall at any given moment on the *entire* species:

> A Dog may hesitate to jump in to save his master's life, - if he meditated on this, it would be conscience. A man might not do so even to save a friend, or wife. - yet he would ever repent, & wished he had lost his life in doing so. - nor would he regret having acquired this sense of right (whether wholly instinctive as in the dog, or chiefly habitual as in man), for it added much to the happiness of his life, & the chance of so dreadful a consequence to each man is small. (N, pp. 46-47)

With remarks such as these, we have come to questions of current scientific interest. That Darwin should have sketched answers for them should surprise no one familiar with the comprehensive nature of his imagination.

We can now end our story. When Darwin's entries in notebook N trailed off during the summer of 1839, he was already engaged in new projects. By that time, the network of points which made up his species theory had shifted considerably. Still at the center of the theory were the key notions of speciation and adaptation. But now, after Malthus, adaptation was understood primarily in terms of selection rather than behavior. Thus the main subject of the M and N notebooks moved from the center of the theory to its periphery. While at the center, the subject had dominated Darwin's attention. Indeed, for the brief period from mid-July to October 1838, one can agree with Robert M. Young that "in many respects evolutionary theory... was applied psychology."[114] However, having got more than he expected from his brief incursion into the social sciences and philosophy. Darwin let the subject of behavior drift to the outer regions of his theory. There it remained, and for reasons not purely intellectual, until the end of his career.

114. Robert M. Young, "The Role of Psychology in the Nineteenth-Century Evolutionary Debate," p. 192.

CONCLUSION

The place of man in Darwin's development of a theory of transmutation has been obscured by his manner of disclosure. Comparing the 1837-1839 period to his entire career as a theorist suggests that it was Darwin's practice to present himself and his work only before the most select scientific audiences, and then in accordance with their expectations. The negative implications of this rule for his publication on man are clear enough: finding no general invitation in science to publish as a theorist and no contemporary scientific audience for the sorts of inquiries he was making on man, he was silent, at least until such time as he could publish on the strength of reputation alone. Now, with the availability of manuscripts from the early period, what was once hidden stands revealed. It is clear from Darwin's notebooks that man played a dual role in the formation of his theory: as a zoological species to be incorporated into the theory and as the primary vehicle for the study of behavior. On the first score, integrating man into the theory provoked Darwin to break with the traditional view of man's place in nature and to reject a major element in the scientific notion of progressive development. On the second score, the study of behavior led Darwin outside natural history and thence, unexpectedly, to Malthus and natural selection.

One is left with the certainty that the subject of man was a central element in Darwin's formulation of his species theory. To an extent, then, the public judgment of Darwin was right all along, for the public had always sensed that Darwin spoke to a larger audience than that formed around science. On the basis of new evidence, we can add that Darwin drew from that larger audience as well. There are of course ironies to this conclusion: that Darwin the professional drew so heavily from fields where he was the amateur, that as a transparent man his inner life should prove so at odds with the manner in which he presented himself, and that his arrival at a strong sense of himself – the revolutionary "I" of his notebooks – should occur just as he stepped beyond science to engage the general culture. But when one considers the inherent difficulties of Darwin's subject and the magnitude of his claims respecting man, these ironies are perhaps not surprising at all but those of a kind which might be anticipated.