Hist. Sci., xxii (1984)

HOW DID DARWIN ARRIVE AT HIS THEORY? THE SECONDARY LITERATURE TO 1982

David R. Oldroyd
University of New South Wales

This article surveys the voluminous literature to 1982 on the problem of how Darwin arrived at his theory. The amount of writing on this single topic is truly immense and part of my task has been simply to provide a summary guide to the huge literature. It is hoped that this will, in itself, be of interest and use to students, and also to historians of science who are not Darwin specialists. In addition, in making my survey, I have sought to form an overview of the form of development of a particular specialty in a metascience (i.e., historiography of science), comparing the features thus revealed with the developmental features that usually characterize particular areas of scientific enquiry — such as the 'S'-shaped 'logistic' curves that are the stock-in-trade of writers in scientometrics.

To this end, I have attempted to identify all the writings that have, in my judgement, made *original* contributions to the question of the origin of Darwin's theory; and on this basis I have sought to trace the lines of information and the more important interpretations to be found in the texts, drawing attention to the changes, in time, of historiographical concerns, techniques and perspectives. Such a survey enables one to see how consensus is, or is not, reached by historians of science. But we cannot say that by 1982 there was anything like unanimity of opinion on our topic. Clearly, the problem has been, and is, one of considerable difficulty.

As indicated, this survey has sought to identify all original contributions to the problem of how Darwin obtained his theory. It includes, therefore, Darwin's *Autobiography*, and his published notebooks, where these include accompanying interpretative essays. Textbooks and most of Darwin's biographies are excluded, for generally these do not contain any *new* information bearing on my inquiry. I have, however, included some essays on Darwin's 'precursors', since I judge that in many cases such writings were inspired by the desire to throw light on the question of how Darwin reached his theory. Doctoral dissertations have been excluded, but most of their results have subsequently appeared in print. For papers presented at history of science congresses, I have listed them according to the conference dates, rather than the dates of publication of the proceedings. My survey runs from 1859 — when, in his *Origin*, Darwin gave just a hint as to how and when he arrived at his theory — to the centenary year, 1982. I have, in addition, included a few papers from the

0073-2753/84/2204-0325/$2.50 © 1984 Science History Publications Ltd

first issue of the 1983 volume of the *Journal of the history of the behavioral sciences*, which can be regarded as belonging to the 1982 'festivities'. My compilation is, I believe, essentially complete with respect to writings in the English language. It also contains some material from other languages. There are probably a few gaps here, but not sufficient to undermine the general picture that I have drawn as a result of my literature survey. Reprinted articles are excluded.

Thus, the 'primary' literature dealing with the question 'How did Darwin arrive at his theory?' is represented by Figures 1 and 2. We should also notice a paper by F. B. Churchill,[177] which provides a survey of the biographies of Darwin, showing how changes in historiographical methods and concerns over the years have manifested themselves in these works. The present paper is intended to complement Churchill's study.

Inspection of Figure 1 immediately reveals that the growth of the 'Darwin Industry' displays marked perturbations, corresponding principally with the Darwin 'festivities' of 1909 and 1959.[178] However, despite such perturbations, a steady overall growth in the 'Industry' is clearly manifest, accompanying the growth of the professional field of history of science. Thus the bibliometric curves for this metascience are similar in kind to the corresponding curves that have been published for particular scientific specialties.[179] For science, such curves customarily display 'irregularities' after empirical or theoretical findings of particular importance are published. For the 'Darwin Industry', corresponding phenomena seem to have been the publication of Darwin manuscripts (notably the *Life and letters* and the "Transmutation notebooks") and the impulse to historical research generated by the aforementioned Darwin 'festivities'. Thus Figure 2 suggests that the whole tempo of Darwin research was accelerated by the events associated with the centenary of *The origin*. A possible further acceleration in the 1970s may be linked with the establishment of the *Journal of the history of biology*, an event itself a mark of professionalization in studies in the history of biology.

Inspection of Figure 2 does not offer any indication that the 'Darwin Industry' is beginning to exhaust itself; and if one were to count pages, rather than numbers of publications, the literature for the period 1978-82 would show an increase even greater than that revealed in Figures 1 and 2. Moreover, one can discern (not from the graphs, of course) a significant qualitative increase in the rigour and sophistication of recent publications. Not only has there been a professionalization of the history of science but also there is now intense specialization within this new discipline, to the extent that a few workers are now virtually full-time scholars of the Darwin letters and notebooks. So the earlier 'hagiographical' enterprise, suggested by the 'perturbations' of 1909 and 1959, is no longer so much in evidence. In line with this, the period encompassing 1982 does *not* seem to reveal an unexpectedly large number of publications, but

maintains the growth pattern established in the last thirty years. There will, no doubt, be some dropping back after 1982, for some 'celebratory' publications of that year will not be repeated. Later writings are likely to be almost entirely 'professional'. Already, by 1982, it was not easy for the non-specialist to make any discernible impact on the 'Darwin Industry', whereas in 1959 (say) this was by no means the case.

In considering "the origin of *The origin*", one of the most enduring features of the debate has been the estimate of the role of 'external' causes, notably social and economic circumstances in nineteenth century Britain, and economic theory as mediated by Malthus — a hare which Darwin himself started running in the famous passage in his *Autobiography*.[180] Long ago, on reading *The origin*, Marx wrote: "It is remarkable how Darwin recognises amongst beasts and plants his English society with its division of labour, competition, opening up of new markets, inventions, and the Malthusian struggle for existence."[181] This notion was echoed by the German historian of biology, Emanuel Rádl, who held that "Darwin merely transferred the prevailing English political ideas and applied them to nature".[182] A figure of the British establishment, J. M. Keynes, could see things in a similar light, when he spoke of the survival-of-the-fittest principle as "a vast generalisation of the Ricardian economics".[183] But broadly speaking the 'externalist' (or 'contextualist') view has been favoured by leftward-leaning historians of science. The resulting political dimension to the scholarly enquiries has thus added considerable interest to the whole historiographical enterprise. I shall endeavour to say something about this in what follows. But first let us examine some of the early writings on 'the question'.

Before the publication of Darwin's *Autobiography* and his *Life and letters* in 1887, Darwin's biographers had little information available to them. Grant Allen, for example, writing in 1885, merely cited a letter of Darwin, published in Haeckel's *History of creation*,[184] which referred to the importance of the domestic selection analogy and the reading of Malthus. Otherwise, Allen had to fall back on the skimpy information available in *The origin* and what could be read into *The journal of researches*.

The first major consideration of the origin of Darwin's views was offered by T. H. Huxley in an obituary published six years after Darwin's death.[185] (Presumably, Huxley had been waiting for the issue of the *The life and letters* in order to furnish material for his study.) According to Huxley, Darwin collected data on the *Beagle* and applied the uniformitarian principle of Lyell to the study of geological phenomena. Darwin was greatly impressed by the discovery of the extinct giant mammalian remains in South America and by the distributions of birds and tortoises on the Galapagos Islands. But his evolutionary ideas were not fully established until 1837, after his return to England, when he was reconsidering his collected specimens. He began collecting facts wholesale, and considered how artificial selection produced new varieties. Finally, he found the

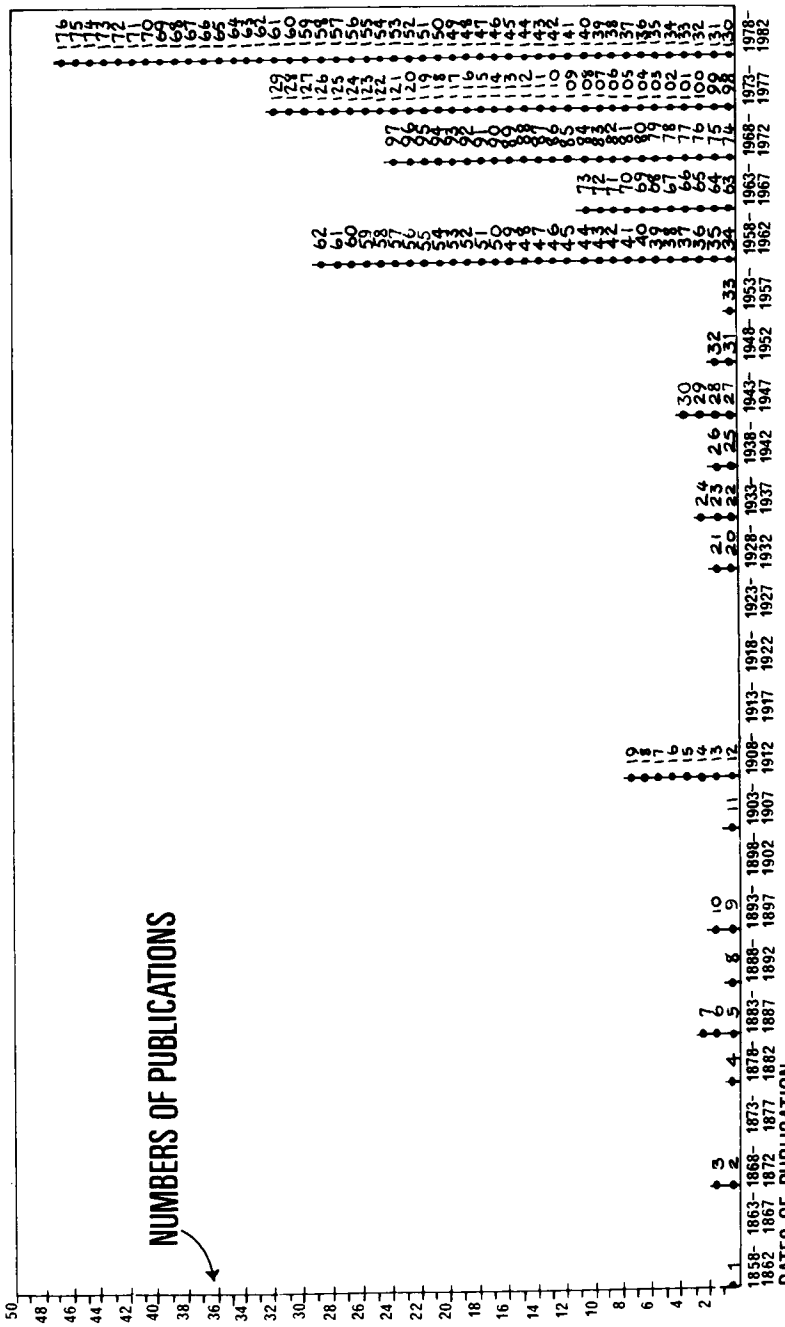

NUMBERS OF PUBLICATIONS
DATES OF PUBLICATION
1858-1862
1863-1867
1868-1872
1873-1877
1878-1882
1883-1887
1888-1892
1893-1897
1898-1902
1903-1907
1908-1912
1913-1917
1918-1922
1923-1927
1928-1932
1933-1937
1938-1942
1943-1947
1948-1952
1953-1957
1958-1962
1963-1967
1968-1972
1973-1977
1978-1982

FIG. 1.

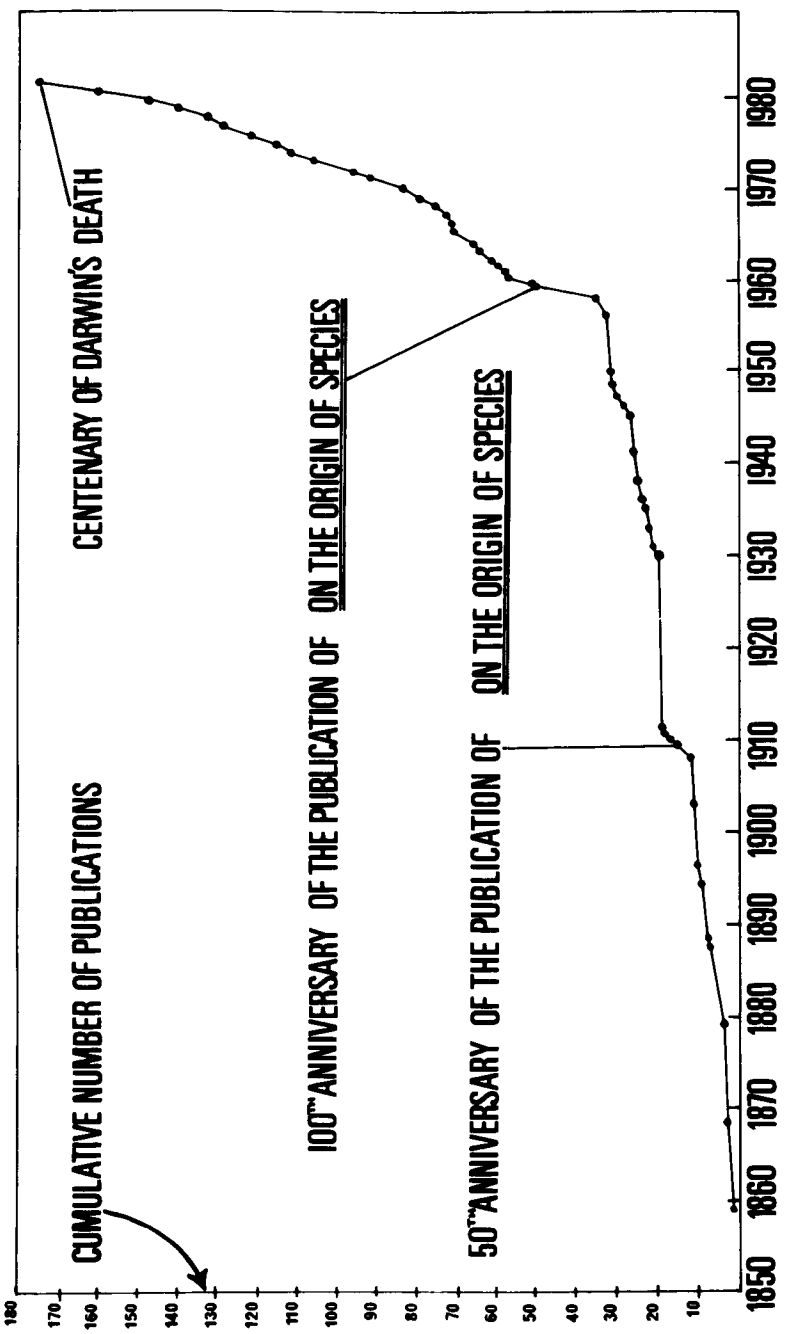

CUMULATIVE NUMBER OF PUBLICATIONS
CENTENARY OF DARWIN'S DEATH
100TH ANNIVERSARY OF THE PUBLICATION OF ON THE ORIGIN OF SPECIES
50TH ANNIVERSARY OF THE PUBLICATION OF ON THE ORIGIN OF SPECIES
180
170
160
150
140
130
120
110
100
90
80
70
60
50
40
30
20
10
1850
1860
1870
1880
1890
1900
1910
1920
1930
1940
1950
1960
1970
1980

FIG. 2.

key to natural selection in his 'accidental' reading of Malthus. Huxley went on to tell the now well-known story of barnacles, the sketches of 1842 and 1844, the Asa Gray letter, the Wallace communication, and the eventual publication of *The origin*. He made no mention of the principle of divergence. His account was based chiefly on information available in *The life and letters*,[186] notably Francis Darwin's quotation from his father's "Pocket book" for 1837: "In July opened first note-book on Transmutation of Species. Had been greatly struck from about the month of previous March on character of South American fossils, and species on Galapagos Archipelago."[187] What is remarkable about Huxley's account is the extent to which it has stood the test of time. The historiographical effort of nearly a century has not really overthrown the simple account of the matter that Huxley proposed. However, the huge amount of detail that has subsequently been accumulated now allows us to form some conception of Darwin's actual creative thought processes. In the nineteenth century, such matters remained well hidden, and Huxley did not seek to uncover them.

As his contribution to the Darwin celebrations of 1909, Francis Darwin published the *Essays* of 1842 and 1844, with a valuable introduction. The younger Darwin had the advantage of ready access to the riches of the Darwin archival material, including the celebrated notebooks on species transmutation.[188] Francis noted that in 1837 his father envisaged an analogy between species and individuals: individuals come into being and die — why not species also? Here the son glimpsed the father's efforts towards generating a new theory, and the analogy to which Francis drew attention has been the subject of much subsequent interest. Francis was inclined to see the adumbration of the evolutionary idea occurring during the voyage. So, he suggested: "[T]he evolutionary current in my father's thoughts ... continued to increase in force from 1832 onwards,[189] being specially reinforced at the Galapagos in 1835 and again in 1837 when he was overhauling the results, mental and material, of his Travels."[190] Thus Francis Darwin was willing to allow some weight to the opinion of the geologist, J. W. Judd, who saw the early observations of the giant fossils as the beginning of Darwin's transmutation theory.[191] But Francis also located Darwin's discovery partly in his post-voyage sojourn in London (1837). He further suggested that his father's observations on ostriches on the Pampas had done something to suggest an 'evolutionary' theory. He contended that the theory of natural selection would have been reached without the benefit of reading Malthus.[192] Francis Darwin displayed some quite sophisticated historiographical techniques in order to give a correct dating to the pencil sketch of 1842, rejecting the date of 1839, which was suggested by a letter to Wallace of 1859, where Darwin referred to his work on the theory of evolution by natural selection "written in 1839, now just twenty years ago".[193] But already the historiographical tradition of looking for an *evolutionary* theory, rather than a 'transmutation' theory, in Darwin's notebooks was being established.[194]

In 1935, Darwin's grand-daughter, Nora Barlow, discussed the manuscripts of Darwin's ornithological notes, prepared, she thought, either while he was in the Galapagos just a hundred years before (a further instance of research apparently stimulated by a centenary), or in the closing months of the *Beagle* voyage, while the vessel was on its return to Britain.[195] The passage in the notes referred to the birds being similar to, but different from, the mainland types in Chile. Darwin noted further that the Spaniards could recognize which islands the different giant tortoises came from. He referred also to the "constant asserted difference between the wolf-like Fox of East and West Falkland Islands", and concluded with the subsequently oft-quoted words: "if there is the slightest foundation for these remarks, the zoology of Archipelagoes will be well worth examining; for such facts would undermine the stability of species."[196] Thus Barlow was apparently inclined to defer the beginning of Darwin's transmutationist ideas to the later part of the *Beagle* voyage, rather than 1832, as Judd had suggested, or to the period after Darwin's return to England in 1836.

1936 and 1938 saw the publication of two interesting papers by Thomas Cowles (of Berkeley, California) and Alexander Sandow (of the Department of Biology, Washington Square College, New York).[197] On the basis of remarks in Darwin's *Autobiography*, Cowles suggested that a transfer of concepts had occurred between the social sciences (represented by Malthus) and the natural sciences (biology). The struggle for existence idea had subsequently been transferred back to the social sciences in works such as Walter Bagehot's *Physics and politics*,[198] and then (possibly) back yet again into Darwin's *Descent of man*, where Bagehot is mentioned with approval several times. I do not know whether this perception was Cowles's own first-hand thought; nor do I know anything of his political predilections. But his notion of an interplay of ideas between evolutionary and social theory had been formulated long before by Engels.[199] And later writers of the Left, such as R. M. Young, have also placed particular emphasis on the interaction of biological and social thought in nineteenth century culture.[200]

Cowles's interpretation appears well suited to recent sociology-of-knowledge theories, such as one finds in David Bloor's *Knowledge and social imagery*.[201] According to the historiographical programme associated with such theorists, one would argue that the 'context' provided by the *laissez-faire* economy of nineteenth century Britain provided the framework and model for Darwin's intellectual construction of the natural selection theory, and guided his thinking when more than one contending line of reasoning might be pursued.

Sandow's article did not break much new ground, but it usefully emphasized the magnitude of social struggle in Europe since the Industrial Revolution and asserted that the economic activities of capitalism played a role in the genesis of evolutionary theory. However, apart from the pioneering essays of Cowles and Sandow, most Darwin studies until the late '60s and the '70s continued to

explore the 'internalist' features of the origin of Darwin's theory.

In 1959, some twenty publications appeared that related to our topic. Two authors (Crombie[202] and Feibleman[203]) began to look at the philosophical presuppositions of Darwin's work. But a major concern amongst the centenary writers — doubtless reflecting the interests of the scientist-historians, still in the ascendancy amongst historians of science in the '50s — was with matters of priority, and possible precursors of Darwin's thought.[204] In particular, Loren Eiseley claimed that Darwin's Calcutta correspondent, Edward Blyth, might have furnished him with the essentials of his theory.[205] And Cyril Darlington made a rather scathing attack against Darwin,[206] castigating him for his failure to give due credit to forerunners such as William Wells, William Lawrence and James Prichard. These men did indeed adumbrate the principle of natural selection, and Lawrence had some ideas on sexual selection. Subsequent opinion has it, however, that they were working within the tradition of *anthropology*, rather than natural history, and were not seeking to develop a general theory of evolution for all living organisms. These early statements of the principle of natural selection, arising within the context of *human* evolution particularly, do, however, give support to the idea that the *social* 'frame' provided an important element for the construction of the transmutation theory. Moreover, Herbert Spencer arrived at a theory of human progress arising from population pressures seven years before the publication of *The origin.*[207] And in all Wallace's accounts of how he arrived at his theory he stressed that he shifted his thinking from mankind to a consideration of animals at the moment when he grasped his theory.[208] In his later work, Eiseley continued to uphold Blyth as an important source of Darwin's ideas,[209] in effect, accusing Darwin of plagiarism. But commentators on Eiseley's work have not been disposed to accept his arguments.[210]

An interesting suggestion was first put forward in 1959 by Ernst Mayr,[211] who has developed the idea in greater detail in subsequent publications.[212] Mayr emphasized what he calls the 'population' character of Darwin's thinking, and his moving away from an 'essentialist' or 'typological' stance. That is to say, Darwin is claimed to have contemplated creatures of a similar type as a kind of 'spectrum'. Natural selection would act on all, but with greater or less effect according to the nature and extent of the variations present within a group (or 'population') of organisms. Thus, contrary to what was sometimes supposed,[213] Darwin's theory did not (according to Mayr) envisage the production of occasional specially-favoured variants that would then mate with the rest of the species. And thinking in 'population' terms, Darwin was at variance with the traditional 'essentialist' or 'archetypal' notion of species. *His* species had no fixed boundaries, either in time or space. Mayr's views were taken up by philosophers of biology such as D. L. Hull,[214] who have seen Darwin as a major figure in Western thought by reason of his contribution to the supposed destruction of

essentialist metaphysics. However, Mayr has recently been the subject of censure for his claimed historiographical anachronism.[215] It would appear that Darwin's thinking during the *Beagle* voyage, and shortly after, when he was in the process of theory construction, was not significantly characterized by populational thinking.

In 1960, R. C. Stauffer began to examine the role of ecology in Darwin's thinking;[216] and this point was subsequently explored more fully by writers such as Peter Vorzimmer[217] and Frank Egerton.[218] The period of the centenary of *The origin* also yielded some early results from the examination of the Darwin archives at Down House and Cambridge, and the gradual publication of much of this archival material. In a preliminary examination of Darwin's "Transmutation notebooks",[219] and the annotations in the margins of the books that Darwin read at the period of these notebooks (1837-38), Sydney Smith concluded that Darwin must have had his theory at least by March 1837, for he referred to "my theory" in his marginalia to the fifth edition of Lyell's *Principles*, which he is known to have read that month.[220] On the other hand, Darwin's copy of the second volume of the first edition, which he read in Montevideo, contains no marginal protest against Lyell's arguments *against* species transmutation. So, while he might have had transmutationist *thoughts* while on the voyage, Darwin did not, Smith argued, arrive at the natural selection *theory*. In Smith's view, this was only reached after Darwin's return to England — but before his reading of Malthus. Interestingly, Smith showed that the Notebooks reveal that Darwin apparently read Blyth, Prichard and Lawrence *after* he had reached his theory. So the suggestions of plagiarism made by Eiseley and Darlington were considerably diminished in significance.

In 1961, Sir Gavin de Beer, who was chiefly responsible for the publication of the "Transmutation notebooks" by the British Museum, gave an important ("Wilkins") lecture at the Royal Society, in which he sought to settle once and for all how Darwin reached his theory.[221] De Beer thought that the *idea* of transmutation was conceived on the voyage, because of (a) the discovered extinct giant mammals related to modern kinds; (b) the variation in ostrich (rhea) types encountered as Darwin travelled southwards in South America; and (c) the similarity of the Galapagos fauna to those of the mainland, but with differences between one island and the next. De Beer thought, following Nora Barlow, that the "Ornithological notes" were written in the closing stages of the voyage, and that they did suggest a belief in a transmutationist view. Then, considering the "Transmutation notebooks", he remarked that Darwin began by referring back to his grandfather's *Zoonomia*, and that he considered the question of sexual reproduction being associated with variation. Darwin then wondered why there should, nevertheless, be differences in a freely-interbreeding population. The answer was *geographical isolation*, which would place a limit on the blending effects of crossings. Here, de Beer suggested, Darwin's thinking was very likely

indebted to Alexander von Humboldt (whom Darwin took as a model for his work as an explorer) or the ageing geologist Leopold von Buch, who in his studies of the Canary Isles had noticed how varieties seemed to become species on different islands. De Beer believed that Darwin was in full possession of his theory by July 1837.

De Beer's views on the influence of Malthus are interesting and important. However, in 1961, when he gave his Wilkins Lecture, not all of the "Transmutation notebooks" had been found.[222] Darwin stated in his *Autobiography* that he read Malthus in October 1838. But consideration of passages in the parts of the "Notebooks" known to de Beer in 1961 suggested to him that Darwin was in possession of the elements of his theory of natural selection before his reading of Malthus. However, when the excized fragments of the Notebooks were discovered (and subsequently published) it became apparent that Darwin actually read Malthus on 28 September 1838, finishing his reading on 3 October. Also, it became evident to de Beer that Malthus *had* meant something to Darwin. For Malthus suggested to Darwin how population *pressure* would produce a "wedging" effect, driving variants into different niches, thereby encouraging speciation.[223] So according to de Beer (in 1964), natural selection became a kind of creative process for Darwin, rather than one that merely eliminated unadapted forms.[224] Even so, the same year de Beer claimed[225] that the principles of adaptation and ecological niches were present in the passage on ostriches in the first "Transmutation notebook" of 1837. All he would admit was that Malthus provided "one tooth for the cogwheels of his argument",[226] by emphasizing the *inevitability* of mortality for many members of the human race. And even in his *New scientist* article of 1964, when de Beer seemed most willing to concede significance to Darwin's reading of Malthus, he suggested that all Darwin really got was a "banal slogan":[227] geometrical versus arithmetical rates of increase.

It is interesting to consider what may have lain behind de Beer's reluctance to concede much significance to Malthus. I suggest there may have been two reasons. First, de Beer was an undoubted exponent of the 'great-man' theory of history, and an inductivist to boot. This is evident in his *Annals* (1964) paper, "Other men's shoulders", where there is extended discussion of the creative "flash of genius" and the "mystery of the creative processes of the mind",[228] and a likening of the creative activity of the scientist to that of the poet. A second reason seems to have been de Beer's distaste for Malthus's political and economic views. De Beer regarded him as a "champion of the old order"[229] in his opposition to 'liberals' such as Godwin and Cordorcet. It may be noted that no less than J. M. Keynes believed that Malthus's economic understanding was more astute than that of Ricardo, and that if nineteenth century economic theory had followed Malthus rather than Ricardo the world might have become a better place.[230] But this has never been the orthodox view, and, rightly or

wrongly, de Beer did not regard Malthus as being on the side of the angels. So it may be that this influenced his examination of the Darwin/Malthus relationship. We may say also that de Beer's interpretation generally tended to follow those of the Darwin family. We have seen above, for example, that Francis Darwin believed that his father could have reached his theory without the help of reading Malthus. No doubt the Darwins were inclined to see their patriarch as an archetypal 'great man'. De Beer seems to have approved of this.

The later 1960s saw the publication of further studies of Darwin's ecological views, and, following Eiseley's earlier suggestion, an examination of the influence of Alexander von Humboldt on Darwin's thinking. Eiseley[231] and Théodoridès[232] pointed out that Humboldt's *Narrative of travels*, which Darwin read and admired as a young man, contained a version of the principle of divergence. But as will be pointed out below, Dov Ospovat has recently suggested that Darwin arrived at this principle over time, and perhaps repeatedly so, only coming to it fully as late as 1856.[233]

Be this as it may, according to Egerton (1970)[234] Humboldt provided a kind of general intellectual inspiration for Darwin's explorations. He, as well as Lyell, showed Darwin how and what to observe, particularly in matters of demography. More recently, Susan Cannon has referred to "Humboldtian science" as a kind of generic term.[235] And she has represented Darwin's work as a good illustration of this sort of approach. But unlike Darwin, Humboldt never saw the implications of demographic studies for a theory of biological transmutation or evolution.

In a paper read in 1965,[236] Sydney Smith discussed Darwin's barnacle studies (which he undertook after 1844). These have sometimes been thought of as Darwin's personal hair shirt — his own self-inflicted self-instruction in systematic zoology, undertaken to make himself a 'professional' zoologist. But Smith claimed that Darwin's barnacle work was an intellectual response to problems raised by W. S. McLeay's "quinary system" for biological classification,[237] notably the question as to whether barnacles are or are not transitional forms between echinoids and crustacea. Smith, I have heard, believes that an understanding of Darwin's barnacle work provides the key to an understanding of Darwin's whole career, but he has published nothing further on the subject. The role of Darwin's barnacle studies has recently been clarified by Janet Browne.[238]

In 1969, scholars returned once again to consider the role of Malthus, it being thought that de Beer had not really resolved the problem satisfactorily. The matter was discussed in very different ways by Peter Vorzimmer[239] and Robert Young.[240] Vorzimmer emphasized that Darwin was consistently influenced by the Lyellian principle of using the present as the key to the past. Thus, the 'present' for Darwin was represented by animal breeders' selection procedures to produce new varieties. So the question became: "What is the analogue in nature

of artificial selection?" According to Vorzimmer, the answer was provided by Malthus, the struggle for existence arising from population pressure. Thus Vorzimmer, who did not accept that Darwin was a "convinced" mutationist when he opened the "Transmutation notebooks" in July 1837,[241] suggested that *before* the reading of Malthus Darwin was concerned with the phenomena of adaptive change (which might somehow become permanent), and was looking for 'present' evidence for variations and modifications (e.g., in the work of breeders). *After* Malthus, Darwin became acutely aware of the role of struggle for existence in the process of natural selection, and hence how this might produce species change. Thus, for Vorzimmer the reading of Malthus was indeed a key event in the establishment of Darwin's theory.[242] But Vorzimmer treated the matter from an 'internalist' historiographical perspective. He did not represent Malthus as the 'spokesperson', so to speak, of the socio-economic order.

By contrast, this was more or less the view taken by Young, painting with the much broader brush of 'externalist' historiography.[243] Contrary to de Beer (but like Vorzimmer), Young saw Malthus as being of central importance in Darwin's thinking. Paley's *Natural theology* (1802) had stressed perfect adaptation; Malthus stressed conflict. Darwin showed how conflict could yield adaptation. Hence, for Young, Darwin's work allowed a continuation of old-style natural theology. Moreover, Malthus might be said to be 'legitimating' the idea of a law of struggle for Darwin, while providing him with the essential analogy he needed to move from artificial to natural selection.[244] In a later paper, Young saw the legitimation process running in the opposite direction as well, with Darwin's theory, based upon individual differences, giving respectability to the notion of division of labour.[245]

Young's attempts to display what he regards as the *essential* interconnections between the social context of science and science itself have proved both stimulating and rewarding.[246] But they depend upon the results obtained by the 'internalist' historiographers, seeking to establish the exact role of Malthus in Darwin's thought. The publication of the excized pages from the "Transmutation notebooks" in 1961 and 1967 provided scholars with fresh data, and new interpretations were soon forthcoming.

An important study, making use of the newly available material, was that of the French Canadian scholar, Camille Limoges.[247] Like Young, but viewing the matter from a somewhat different perspective, Limoges was particularly concerned with the relationship between Darwin's thinking and contemporary British natural theology. According to the Paley tradition, organisms displayed a divinely organized perfect adaptation to their environment. But Limoges discerned evidence for Darwin rejecting this in his first "Transmutation notebook" ("Notebook B"), and according to Limoges's view, *this* was the essential step for the establishment of Darwin's evolutionary theory. Chance,

not design, would lie at the heart of the dynamic process of evolutionary adaptation. However, until reading Malthus Darwin did not, according to Limoges, have an adequate understanding of how adaptation came about. So although Darwin was a transformist from the time of the first Notebook, the only mechanism for transformism that he could adduce before the Malthus reading was that of geographical isolation. Limoges maintained that Darwin remained a non-transformist throughout the *Beagle* voyage. He suggested that historians who thought otherwise had been imposing their *own* transformist views onto notes and other material from the voyage. In particular, Limoges believed that Nora Barlow's interpretation of the "Ornithological notes" was incorrect.[248] These, he maintained, were written shortly *after* the voyage, not during its closing months. They would have been drawn up when he was preparing material for the ornithological section of the *Zoology of the voyage of H.M.S. Beagle*. Limoges's study was widely acclaimed at the time of its publication, but some of his interpretations relating to the early versions of transmutation theory in the "Transmutation notebooks" have subsequently been challenged by David Kohn.[249]

In further examination of the published "Transmutation notebooks", and the light that they could throw on the role of Malthus in Darwin's thinking, the American Darwin scholar, Sandra Herbert, pointed out in 1971[250] that in editing the third Notebook (="D") in 1960 de Beer had supposed that the whole book had been written *before* the reading of Malthus. Consequently, a passage towards the end of the "Notebook", which seemed to indicate that Darwin had the theory in his possession, had been used by de Beer as evidence *against* the influence of Malthus. But with the discovery of the excized pages, this particular passage was revealed to be 'post-Malthus'. So the de Beer view of the role of Malthus (as stated back in 1960) had to be revised. Knowing, in 1971, the true date for Darwin's reading of Malthus, Herbert examined the 'pre-Malthus' notes and concluded that they did not reveal Darwin as having a sufficiently clear idea of artificial selection to have enabled him to expect to find some process of natural selection as a mechanism for transmutation. Rather, suggested Herbert, the discovery of natural selection with the help of Malthus made the domestic-selection analogy clearer. Malthus demonstrated the notion of "wedging" to Darwin, from which natural selection and transmutation followed.

Thus the work of an 'internalist' scholar, Sandra Herbert, yielded information useful to the wider claims of the 'externalists' such as Young. It is perhaps not surprising, therefore, that a paper by Barry Gale appeared a year after that of Herbert with the title: "Darwin and the concept of struggle: A study in the extrascientific origins of scientific ideas".[251] The author described how the notion of 'struggle' provided a major intellectual assumption of Darwin's age. But he didn't really show (apart from the well-known link via Malthus) how the competitive ethos of Victorian society actually connected up with Darwin's

theory.

Several major studies relating to our theme were published in 1974. Besides Darwin's four "Transmutation notebooks", ("B", "C", "D", and "E"[252]), previously mentioned, there are also the two labelled "M" and "N", in which Darwin wrote notes on "metaphysics" and "morals". These were begun in July 1838, just after Darwin had completed "Notebook C". Notebooks "M" and "N" were published by Paul Barrett in 1974,[253] along with a detailed commentary by Howard Gruber, who also offered an elaborate reconstruction of Darwin's thought processes during the period when he was building up his transmutation theory in the "Transmutation notebooks".

In Gruber and Barrett's *Darwin on man*, the reader is introduced to a new account of Darwin's thinking, with reference to a claimed "theory of monads", found in "Notebook 1" (="B"). This 'theory' appears in Darwin's reflections on the rheas of Patagonia, in which he envisaged species appearing by a kind of spontaneous generation process, and then dying out again when their time-span was finished. Thus, according to Gruber's reading of Darwin's notes, simple living particles ("monads") were supposedly constantly springing into life from inanimate matter, being produced by natural forces.[254] They differentiated, matured, reproduced, and eventually died, also somehow giving rise to new species in the process, in a branching fashion. The life of a species, with its 'birth', 'maturity', and eventual 'death', was supposedly analogous to the life of an individual. And when a monadic series eventually terminated, all the species to which it had given rise would terminate likewise.

Darwin soon gave up the "monad hypothesis", but, Gruber argued, it left its mark in the branching-tree and the branching-coral models that one finds figured in "Notebook B". A species lives on, Darwin then supposed, if it can change to produce another species. Otherwise extinction occurs. So Darwin was led to look for the causes of change. He considered particularly the phenomena of variation, and hence of sexual reproduction. And from examination of hybridization (a cause of variation) he was led to consider animal breeding, and hence artificial selection. This led to the search for the natural analogue of artificial selection: and hence Darwin was led to formulate his theory of natural selection. This, in summary, is the route that (according to Gruber) Darwin took, from the opening of his first "Transmutation notebook" (July 1837) to his achievement of the theory, culminating with the reading of Malthus in September 1838.

It must be acknowledged, however, that the weight given to the few enigmatic references to "monads" in "Notebook B" does seem rather great. And some later commentators such as David Kohn have not been prepared to go along with Gruber's reconstruction.[255] But Gruber made the suggestion that the principle of divergence[256] was implicit in the very early versions of Darwin's theory — the tree model. This may seem curious, given that Darwin himself regarded the

divergence principle as the missing element of his theory, only discovered in the 1850s after the barnacle work was completed. So perhaps Darwin 'discovered' the principle twice. Or in its early form — merely the notion of branching, and detached from an explanation in terms of wedging and natural selection — Darwin didn't really regard it as the 'principle of divergence' proper.[257]

Gruber also maintained that there were ideas of natural and artificial selection, and superfecundity, in the "Transmutation notebooks" some time before the reading of Malthus. Indeed, one finds a vast number of topics alluded to in the notes, and the period of the actual reading of Malthus seemed (in Gruber's view) to have been more like an elevated plateau than a peak of achievement. There was no sign of high excitement in the notes for 28 September 1838. On the other hand, Gruber acknowledged that Darwin probably did not see natural selection as a *creative* force until his reading of Malthus.

As to "Notebook M", Gruber saw there ample evidence for Darwin's humanism, notably in his interest in the anti-slavery movement, and in his seeing humans as a unified species. For example, the Fuegians who went to England and were returned during Darwin's voyage seemed to show a high degree of educability. This raised for Darwin the question of whether habits could become inherited. And so we see a high degree of 'Lamarckian' thinking in the "Transmutation notebooks", seemingly generated by Darwin's interest in man at that time.

Gruber's study performed valuable service by seeking to delve into Darwin's mental processes, both conscious and unconscious, during the extended period of construction of his theory. His concern with metaphysical and moral questions during the period immediately preceding the final arrival at the natural selection theory (assumed to correspond with the Malthus reading) was seen as specially significant. Gruber sought to display the social context of Darwin's theorizing by considering his education and intellectual background, and the consequent fears he felt about the materialist tendency of his thinking and the social unacceptability of a non-teleological theory of biological transmutation. A private fear of persecution on Darwin's part was revealed, which would, of course, mesh with his later retiring disposition and apparent psychosomatic illness. Gruber laid particular emphasis on a dream of Darwin, just one week before he read Malthus, in which a hanged man came to life and then made jokes about not having run away, but having faced death like a hero. Here indeed one obtains insight into Darwin's mental condition in his period of intense intellectual activity associated with the "Transmutation notebooks". However, Gruber was not inclined to see Darwin's creative activity functioning solely at some unconscious and unfathomable level. He sought to display the apparent systematic way in which Darwin reconstructed his world view and developed his theory concomitantly.

In 1974 there also appeared the first part of an important paper by Sandra

Herbert, entitled "The place of man in the development of Darwin's theory of transmutation".[258] The second half of this (1977), which we shall discuss below, involved a further close consideration of Notebooks "M" and "N". Part I introduced to Darwin scholars yet another Darwin notebook, bearing the legend "RN".[259] This was formerly thought to denote "Rough Notebook", but it is now considered to mean "Red Notebook" by reason of the colour of the notes' binding. Herbert regarded "RN" as antedating Notebooks "A" to "E", and presenting the first intimations of Darwin's transmutationist speculations. She subsequently suggested that it was begun about June 1836, that is, in the closing months of the *Beagle* voyage.[260]

As we have seen, Limoges had queried Nora Barlow's dating of the "Ornithological notes" (with their transmutationist hints), and Herbert too was inclined to regard them as belonging to the post-voyage period, being written for the 'professional' London audience. Herbert further maintained that one could also find important transmutationist remarks in "RN", which was evidently written partly on the voyage and partly in England (as indicated by a change in the direction of the handwriting on the pages). The transmutationist passages come from the later section of the "Notebook". Here, in discussing the ostriches of the Pampas, Darwin suggested that just as one kind apparently changed abruptly into another kind in *space*, so, historically, there might have been an abrupt change from one kind to another in *time*. So he was envisaging saltatory, *not* gradual, changes in species. Herbert dated the remarks of Darwin to the early months of 1837, when he was preparing his *Journal of researches* for publication. She suggested that Darwin's discussions with the London experts, particularly Owen, might have provided the stimulus for a radical shift in Darwin's views. A date of March 1837 for this agrees, of course, with Darwin's own statement on the matter.[261]

Further reference to "RN" revealed to Herbert how Darwin contemplated two species as having a common origin; but also species could form and die away rather like individual organisms. This had some analogy with Lyell's theory of species change, which I have represented diagrammatically elsewhere,[262] except that Darwin seemed to contemplate an abrupt change in time, of one species into another. The relationship between the ideas of Darwin and Lyell has recently been examined in minute detail by M. J. S. Hodge, whose work will be discussed below.

Another interesting essay published in 1974 was George Grinnell's "The rise and fall of Darwin's first theory of transmutation".[263] Grinnell observed that while on his voyage Darwin had had a dispute with Captain Fitzroy about the Galapagos finches. Darwin thought them varieties, while Fitzroy thought them "specially created" species. On his return to London, in his discussions with the ornithologist John Gould, Darwin became convinced that the birds from the different islands were indeed different species. Hence he came to think of species

as produced by geographical isolation. Then he wondered how creatures might reach oceanic islands. He contemplated floating tortoise eggs, land bridges, and even a theory of continental drift. But this last speculation was not pursued, and Darwin hypothesized that the Galapagos fauna were *relics*, left cut off on islands after land subsidence. These speculations occurred in the "Ornithological notes", dated by Grinnell, contrary to Nora Barlow's view, as March 1837, thus paralleling the "Red notebook", where Sandra Herbert had previously descried the first intimations of the transmutation concept.

In 1975, Peter Vorzimmer drew attention to some further notes, which he published,[264] suggesting that they were an early version of Darwin's 1842 "Sketch" of his theory, datable to 1839. However, it has recently been shown by Kohn, Stauffer and Smith[265] that these notes were in fact a sketch of the first chapter of Darwin's "Essay" of 1844, in which case they fall outside the period in which Darwin first constructed his transmutation theory. So I shall not consider them further here.

In 1975, after some adumbrations in earlier essays, Michael Ruse drew attention to the role of the philosophies of science of Darwin's contemporaries Herschel and Whewell in the formulation of his theories.[266] As is well known,[267] Herschel[268] argued for a hypothetico-deductive model for science and a hierarchy of laws of increasing generality. He thought that science should be concerned with the identification of 'true causes' (*verae causae*) — not just good correlations between observed phenomena. Also, a theory should explain phenomena over and above those that were used in its formulation. Whewell's philosophy was published in full after the period when Darwin was first formulating his theory of species formation by natural selection. But the main elements of Whewell's view of science were quite well known in the 1830s through various reviews, and Darwin had been acquainted with Whewell when he was a student at Cambridge. A good theory, in Whewell's view, should display a "consilience of inductions".

Ruse, then, drew attention to the ample evidence within Darwin's letters and notes that he was an adherent of a Herschelian/Whewellian philosophy of science, with a particular emphasis on the virtues of hypothetico-deductivism. Also, according to Ruse, Darwin sought to formulate the high-level laws of struggle and variation. The Darwinian theory was, of course, notable for its powerful consilience of inductions, explaining simultaneously the geological record, classification, embryological development, geographical distribution, and so on.

Ruse saw Darwin's shift from artificial selection to natural selection as a kind of hypothetico-deductive move. Darwin was examining breeders' pamphlets in the first half of 1838 (as shown in "Notebook C"), and these suggested that the effects that could be produced by artificial selection were finite. But when he read Malthus he came to recognize the enormous effect of population pressures;

so the negative features of the domestic-animal analogy could be discounted. Darwin could see how 'nature' selected — or what was the natural analogue of the selection procedures of the animal breeders. Thus Malthus provided a much-needed basis for quantitative reasoning whereby one could 'deduce' the struggle for existence. So, according to Ruse, Malthus showed Darwin how he could locate the notions of struggle and selection in a "hypothetically-deductively organized network of laws".[269] Ruse further argued that the wedge analogy — which Darwin consistently used from 28 September 1838 to the first edition of *The origin* — was for Darwin a kind of *force*. In artificial selection the breeder exerts a kind of 'force' on an animal population; and there is an analogous force in nature — natural selection. Hence, with his analogy between artificial and natural selection, Darwin thought he had found a *vera causa*, as Herschel and Whewell recommended. Consequently, the new theory was eminently acceptable to Darwin, for it conformed to his view of what a scientific theory should look like. As Herschel required, a *vera causa* could be identified by virtue of the fact that a suitable analogical argument could be constructed. So we can see (Ruse argued) why Darwin regarded his theory as satisfactory; and we may even see how his understanding of the nature of scientific methodology may have directed his thinking. Ruse's interpretation was given, with somewhat different emphasis, in another paper published in 1975.[270]

We should note, however, as did W. F. Cannon in an article published in 1976,[271] that Darwin was also conducting a kind of private intellectual debate with Whewell in the "Transmutation notebooks". The two were opposed on a host of issues, such as uniformitarianism and materialism. Even so, Darwin was in accord with Whewell over the general nature of scientific method, and, in particular, the worth of a good consilience of inductions.

It has been described above how various 'internalist' historians have found reason to attribute significance to Darwin's reading of Malthus for the establishment of the natural selection principle, by reason of Darwin's own testimony and the evidence of the "Transmutation notebooks". Complementing this, an 'externalist' such as R. M. Young has argued that there was one very general debate going on in Darwin's day. As Young put it, there was a "common context"[272] to biological and social theorizing that can be seen interacting directly in the Darwin/Malthus episode. So historians of science of both 'internalist' and 'externalist' persuasions could find common ground here.

However, Young's view was questioned in 1976 in an interesting paper by Peter Bowler.[273] Taking up a point previously adumbrated by Sandra Herbert,[274] Bowler suggested that there were for Darwin's consideration two kinds of 'struggle' for existence: (a) intraspecies struggle, as when two dogs fight for a bone; and (b) interspecies struggle, or the struggle of organisms with the environment, as when a plant 'struggles' for life at the edge of a desert. Now, argued Bowler, Malthus's doctrine was evidently concerned with "struggle (b)".

As a result, one is faced with a problem, for Darwin's theory was clearly based upon "struggle (a)". How, then, was it that Darwin was assisted by reading Malthus? For de Beer, of course, there wouldn't be a problem, since for him Malthus was virtually superfluous to Darwin's argument. But from all that has been said above, one can hardly dismiss Malthus's influence so lightly. So one such as Young *is* posed a problem by Bowler, if we grant Bowler's contention that "struggle (b)" (Malthusian) does not imply "struggle (a)", needed by Darwin's theory.

Bowler himself saw the link between Malthus and Darwin as holding for primitive, but not for civilized, man. In Malthus's text, primitive man was seen to be engaged in both "struggle (a)" and "struggle (b)". Consequently, Darwin could supposedly argue from Malthus's text to "struggle (a)" in the animal kingdom, whereas (according to Bowler) Malthus did not see the rich as victors over the poor in a "struggle (a)" kind of situation. Indeed, the rich would help the poor in a sense, not by philanthropy but by their provision of technology to increase agricultural production. Thus, in the tradition of Malthus's own economic theory and natural theology, the different components of society were seen as mutually interdependent. Accordingly, Malthus found virtues in *laissez-faire* capitalism; but this meant something different to him from what it meant in the later nineteenth century: it involved a kind of harmony of human interests, rather than some kind of 'dog-versus-dog' struggle of "type (a)".

In this way, Bowler sought to rebut those who might wish to claim that *laissez-faire* capitalism was itself causally responsible for the origination of Darwin's theory — via Malthus.[275] For Bowler, it was Darwin's special achievement to find the notion of "struggle (a)" within Malthus's text, and use it as the basis of a theory of evolutionary progress and advancement. It was not to be found there as an obvious literary manifestation of capitalism. After Darwin, the notion of the virtues of "struggle (a)" became widespread in the community. And so we find the great flourish of Social Darwinism in the later nineteenth century.

I do not know whether Bowler may have been moved to undertake his analysis in order to try to undermine a historiographical claim from what he regarded as a politically suspect source. Perhaps not, since at the end of his paper[276] he did hint that Darwin's theory might somehow be a "reflection of society as it really was", even if Darwin did not reach his theoretical destination through finding the notion of "struggle (a)" explicitly in a text on political economy — i.e., Malthus's *Essay*. Unfortunately, Bowler did not elaborate on this hint. But it would seem to have some plausibility. After all, as has been pointed out by Gale,[277] many of the contemporary commentators on Malthus saw him as depicting a 'dog-eat-dog' world. And by the time Darwin read Malthus, *laissez-faire* capitalism had begun to take on its characteristic nineteenth century appearance. The old eighteenth century facade of a harmony of interests between men in different stations was badly cracked. In any case, despite all the

subtlety of Bowler's argument, and his careful distinction between "struggle (a)" and "struggle (b)", Darwin *may* have turned his thoughts to the economic sphere as he examined Malthus's text. And even if he did not do so in a conscious manner, and was simply impressed by the extent of struggle in the human condition that Malthus revealed, it was nevertheless the case that Malthus's text was one in social and economic theory; and consequently social and economic theory had *some* direct role in the construction of Darwin's biological hypothesis. I would add further that there are other ways of classifying the notion of 'struggle' than that suggested by Bowler. Edward Manier, for example, has offered a significantly different taxonomy.[278]

We must now look at the second part of Sandra Herbert's extended study of the place of man in the development of Darwin's theory, which she published in 1977.[279] Here she discussed the social circumstances in which Darwin worked in London on his return from the *Beagle* voyage. The intellectual climate was not conducive to theorizing, so Darwin did not present himself publicly as a theorist.[280] His theorizing went on privately in the "Transmutation notebooks", and particularly in "M" and "N" which paralleled "D" and "E", "D" being the book in which the reading of Malthus was recorded.

"Notebook B" reveals Darwin as quite content to view man as part of the general animal kingdom. In "Notebook C" he revealed himself as a materialist, willing to regard "love of the deity [as] effect of organization".[281] He saw man grading into animals, but with a "hiatus" (not a "*saltus*") between them due to man's special mental powers. Also in "Notebook C" we find Darwin thinking in terms of what is today called 'simulated Lamarckism' (or the 'Baldwin effect') — namely behavioural change preceding (and hence 'causing') slow morphological change over many generations.[282] This led Darwin to examine behaviour, and thence to study mental phenomena. Hence he was stimulated to open "Notebook M" on 15 July 1838, simultaneously with the critically important "Notebook D".

Thus we find Darwin's reading changing significantly at this period, with a new interest in 'humanist' works — such as the writings of Hartley, Browne, Comte and, of course, Malthus. In particular, Darwin began a general preliminary exploration of mental phenomena — memory, dreams, and the coexistence of separate trains of thought. He seems to have been exploring the 'Lamarckian' aspects of his theory — the way in which ideas might be related to new behaviour patterns and hence to subsequent biological change. In the process, he adopted an associationist philosophy/psychology.[283] Darwin further speculated about morals and the rationalist/empiricist dispute in 'evolutionary' terms.

After "Notebook D" and "M" came "E" and "N" — subsequent to the reading of Malthus. There was a decided change in tone. The pace of notetaking was slower, and the period of intense creative theoretical activity was evidently

coming to a close. Darwin tried to establish satisfactory definitions for 'habit', 'hereditary habit' and 'instinct'. Human behaviour was to be explained by biologists in 'evolutionary' terms. After Malthus, transmutation was seen chiefly in terms of selection rather than behaviour. But while Darwin's conceptual excitement was at its height, 'evolutionary' theory seemed almost to be a kind of applied psychology.

Thus spake Sandra Herbert in 1977. To be sure, if one looks through the "Transmutation notebooks", the overwhelming impression they give is of Darwin's concern with the phenomena of natural history. Even so, I think Herbert's case stands. The role of man in Darwin's thinking was amply demonstrated by her study. And so we have further evidence, in a general way, for the importance of 'social imagery' in scientific theory construction.

From all that has been said above, it might be thought that the role of Malthus in Darwin's thinking had already received sufficient attention. But 1977 and '78 saw two major studies, by Sylvan Schweber[284] and Edward Manier[285] respectively, that added very considerably to our understanding of the matter. Schweber sought to identify the books that Darwin was reading in the period immediately prior to the Malthus episode. We find that these included a long review of Comte's *Cours de philosophie positive* by David Brewster, Dugald Stewart's *Life of Adam Smith*, and an analysis of the work of the Belgian statistician, Adolphe Quetelet, which actually stated Malthus's population principle. So Darwin was indeed, as he said in his *Autobiography*, "well prepared" when he read Malthus "for amusement". And the 'humanist' direction of his reading is made evident by this list.

According to Schweber, Darwin's interest in variation and artificial selection may have been stimulated by his contact with the ideas of Comte. The positivist philosopher required a scientist to have empirical backing for his ideas. Hence Darwin turned to the animal breeders' pamphlets. Then, when he was thinking about variations and population, he naturally turned to Quetelet, the statistician/demographer. Quetelet probably led Darwin on to Malthus.

In a certain sense, Schweber only saw Malthus as a kind of 'catalyst' for Darwin's thinking, as did the internalist historian, de Beer, years before. However, according to Schweber, it was within the writings of economists/philosophers and demographers that Darwin found the help he needed for his theory construction. (The earlier "Notebooks", with all their numerous references to the literature of natural history, reveal that Darwin had not then 'cracked' his problem.) Also, it was in the economics of Adam Smith that Darwin found the notion of society being made up of free *individuals*, whose several activities produced the social formation. This would have provided the impulse for the 'populational thinking', so important in Darwin's thinking according to Ernst Mayr. On the other hand, it would not mesh very well with Bowler's notion that the idea of "struggle (a)" was incompatible with a

balanced, ordered, harmonious universe as understood by the ideals of the Enlightenment. Be that as it may, Schweber's paper gave additional support to Young's thesis — that there was a "common context" for social and biological thought in Darwin's day. For it was remarkable that some of the chief immediate stimuli to Darwin's thinking were provided by writings in social science, philosophy of science, economics and statistics. Schweber's ideas were approved by the leftward-leaning Stephen Gould.[286]

I turn now to consider the important monograph by Edward Manier, *The young Darwin and his cultural circle*,[287] which explored in greater depth some of the ideas adumbrated in Schweber's article. There is insufficient space here to do justice to Manier's work, but some indication of his interests, methods and conclusions will be given. As its subtitle indicates, Manier was concerned to identify the "influences which helped shape the language and logic of the first drafts of ... [Darwin's] theory of natural selection". To this end, he examined in detail the evidence as to what texts Darwin read as a young man, and the intellectual interests that seemed to have worked upon him. For this purpose, Manier made counts of the authors cited in Darwin's notebooks and early manuscripts; and hence he sought to identify Darwin's 'cultural circle'.[288] He then examined the ideas of these men, and investigated how they might have contributed to the shaping of Darwin's world view. For example, for the poet Wordsworth, whom Darwin mentioned with pride in his *Autobiography* as having read when a young man,[289] Manier identified passages in *The excursion* that have some evolutionary suggestiveness:

> ...The vast Frame
> Of social nature changes evermore
> Her organs and her members, with decay
> Restless, and restless generation, powers
> And functions dying and produced at need,—
> And by this law the mighty whole subsists:
> With an ascent and progress in the main....[290]

By building up a considerable collection of like examples, Manier was able to give a plausible depiction of the leading elements of Darwin's intellectual background; and hence he could make comprehensible how Darwin was able to make his remarkable intellectual reconstruction in the 'evolutionary' theory that he synthesized.

As we may readily anticipate, Manier addressed particularly the role of Malthus in Darwin's theory construction, seeking to display how Darwin actually made use of Malthus's work — moving from the notion of population pressure to differential survival or natural selection. In fact, Manier argued, Darwin did *not* derive the selection (or 'pruning') principle directly from Malthus: it is not present in Malthus's text. Further, in Manier's view, the

notions of 'struggle' in Darwin and Malthus were significantly different. For Darwin, it meant "an effort to overcome a difficulty", but might also encompass differential survival, differential reproductive success, and struggle involving ecological relationships and dependences. For Malthus, it simply meant a "zero-sum game for a *scarce* resource":[291] if some men acquired more of a finite resource, others had to make do with less. Even so, Manier believed that Darwin could have been deeply influenced by Malthus: he could perfectly well use Malthus's idea of struggle for his own purposes and develop it analogically in his own way. Thus Manier gave cautious support to the idea that economic theories impinged on Darwin via Malthus.

In 1978, we find ourselves returned once again to the Galapagos Islands, in a paper by M. J. Kottler,[292] to consider Darwin's views on species. It will be recalled, from Grinnell's paper,[293] that Darwin had a controversy with Fitzroy as to whether the different finches were varieties or species. The received view was that Darwin thought they were varieties and only recognized them as species when he got back to London and discussed the specimens with John Gould. Then, believing the birds were *bona fide* separate species, Darwin was driven to think in transmutationist terms — *after* the voyage.

However, Kottler's examination of the relevant texts suggested that this wasn't entirely satisfactory, for Darwin had noticed differences between mocking birds (thrushes) by himself, from one island to the next. And after the Vice-Governor of the penal colony pointed out the obvious differences in the tortoises of the different islands, Darwin began to collect the finches with more care, taking note of which islands they came from. However, while this seems to show that Darwin realized that his Galapagos observations were in some way theoretically important, it does not prove that a '*eureka*' effect occurred there and then. It is quite compatible with the suggestion that it was Gould who convinced Darwin that the finches were genuine species; and from this flowed Darwin's conversion to transmutationism. As will be shown below, this contention has subsequently been supported by the researches of Sulloway. However, the investigations of Hodge do not present precisely the same interpretation. We shall examine the views of Sulloway and Hodge below, but first let us look at David Kohn's careful examination of the "Red notebook" and the "Transmutation notebooks".[294]

Kohn discussed first the passages in the "Red notebook" where Darwin was considering the relationship between different species of ostriches and llamas in South America. Darwin observed two distinct types of ostriches (or rheas) living in distinct but overlapping regions. Also, there was evidence of extinct camel-like creatures (*Macrauchenia*) living in the same region as the related modern llama. Maybe, he speculated, the two cases were somehow analogous. Perhaps, as one type of ostrich apparently turned into another in space ("at one blow"), so the *Macrauchenia* might have changed into the llama in time ("*per saltum*").

Such changes were required, Darwin speculated (drawing on suggestions to be found in Lyell's *Principles*, themselves derived from the Italian geologist Brocchi[295]), because species would eventually exhaust themselves, so to speak. Just as individuals are born and die, so too may species. So it would appear, on this view, that Darwin came to entertain saltatory species transmutation by contemplating the observational evidence of ostriches and llamas, with the help of theoretical explanations derived from Lyell/Brocchi. Darwin did not, of course, offer any suggestion as to *how* a species could 'jump' to form another type when it grew old and came to the end of its tether, so to speak. A little further on in the "Red notebook", Darwin speculated whether two closely related species might somehow have common parents. So the curious notion of species simply changing "*per saltum*" did not last very long, and we soon have a more clearly recognizable Darwinian position. However, Kohn saw the thinking revealed in the "Red notebook" as indicative of Darwin's first change to a transmutationist view, occurring in the context of thinking about the problem of adaptation — that is, species seemed to die out of 'old age' rather than maladaptation. All this, for Kohn, occurred in the spring of 1837.

Turning to the "Transmutation notebooks" proper, Kohn described how Darwin developed an early theory which sought to show how species could adapt to changing conditions by means of the variability inherent in the process of sexual reproduction, this variation supposedly being spread through a species according to the changing conditions of the environment. The variability would be limited, however, by the blending inherent in the sexual process of reproduction — blending giving the apparent constancy of a species. But then, thinking about island populations again, Darwin supposed that geographical isolation could also be a means of containment such that the sexual mechanism could produce change. So he could contemplate speciation by accumulation of change over time ('straight-line' transmutation) and speciation by isolation as on islands. These two processes were then conceived as acting in concert to produce the various tree models figured in "Notebook B".[296] Yet at this stage Darwin was still a long way from the notion of natural selection proper. He envisaged a kind of ('eighteenth century') balance of nature. As some species were formed, old ones had to disappear. His subsequent wedging hypothesis was something entirely different.

Kohn saw little merit in Gruber's idea of Darwin having a 'monad' theory. Darwin did mention the idea of 'monads' giving rise to groups of organisms and then dying out; but he promptly rejected the idea. It seemed, on Kohn's view, to have had little importance for Darwin. Following Herbert,[297] Kohn noted particularly that in the "Transmutation notebooks" Darwin used the idea of behavioural change preceding adaptive structural changes.

As for the role of artificial selection, the 'traditional' view (e.g., that of Ruse) was (or is) that Darwin was examining breeders' pamphlets just before the

Malthus episode. And Malthus made clear to Darwin what was the natural analogue of the breeders' selection or 'picking' process. Yet in fact, Kohn reiterated, in his "Notebooks" Darwin was *rejecting* the analogy between wild and domestic creatures. Domestic animals were, he thought, "monstrosities", incapable of surviving in nature. To be sure, Darwin *was* reading the breeders' pamphlets. But there is no indication in the "Notebooks" of him suddenly deploying the domestic/wild analogy after the Malthus reading. This analogy was only constructed afterwards, therefore, and was, of course, made much of in *The origin*.

For Kohn, then, the importance of Malthus was the way he changed Darwin's attitude towards the balance of nature. Beforehand, the 'balance of nature' could be seen in terms of a Paleyesque design. Afterwards, Darwin's thinking shifted to a balance of nature arising from dynamic processes, in turn due to intra-species struggle. But the balance was achieved only as a result of the war of nature — by "wedges" being pounded into cracks. Malthus utterly destroyed for Darwin the optimistic benign world of William Paley.

Most of Kohn's analysis has not been undermined by subsequent researches, but in 1982 Frank Sulloway threw doubt on the priority of the ostrich/llama case for Darwin's first transmutation ideas. And in the same year Jonathan Hodge, examining the relationship between Darwin's thinking and that of Lyell, suggested that it was necessary to push forward the period of the first transmutation speculations from 1837 to that of the late voyage. Also, Hodge allowed more significance to Darwin's "monad hypothesis" than did Kohn.

In order to date the "Ornithological notes" more securely, Sulloway[298] examined Darwin's mis-spellings, to see which years he was mis-spelling which words. Studying the "Notes", then, and comparing these with manuscripts of known date, Sulloway concluded that the "Notes" fell into the period November 1835-September 1836, thus suggesting that they were indeed written in the closing months of the voyage, as Nora Barlow had suggested long before,[299] but contrary to the opinion of Sandra Herbert.[300] Sulloway claimed that the 'voyage' dating was confirmed by comparison with the format of other specimen catalogues, which by internal evidence were clearly written during the voyage. However, while Darwin wrote that the facts about the mocking birds and the tortoises tended to "undermine the stability of species", he did also say "I must suspect they are only varieties".[301] So the "Ornithological notes", whatever their date, did not, Sulloway contended, necessarily signal a major theoretical shift on Darwin's part. Moreover, Sulloway showed that at the time of his Galapagos visit Darwin scarcely had the opportunity to observe the different tortoises on the different islands, and in fact he made little of the Vice-Governor's assertion that the Spanish inhabitants could tell which island a particular species came from. Indeed, the *Beagle* took thirty tortoises on board for provisions, and the shells were thrown overboard after the animals were eaten. It seems unlikely,

therefore, that at that time Darwin considered them specially significant. He kept two young ones as pets, but they were insufficiently differentiated to provide any useful evidence when he returned to England, and the tortoises were really only sorted out in 1838 when he met the French herpetologist Gabriel Bibron.

There was similar confusion with the finches.[302] In his *Darwin's finches* (1947),[303] David Lack implied that Darwin began to sort his finches (as well as tortoises, mocking-birds and certain plants) island by island, as soon as he heard the Vice-Governor's testimony about the tortoises. But Sulloway showed that in fact Darwin reconstructed his finch distributions only after his return to England, borrowing specimens from Fitzroy and other shipmates for the purpose. Moreover, some of the reasoning was circular. Unmarked specimens were allotted locations on the assumption of the transmutationist ideas that Darwin had by then developed.[304] All this may explain why Darwin never mentioned the finches in *The origin*.

As we know, back in Britain Darwin had his specimens farmed out to experts, and they were described at various meetings by authorities such as Owen and Gould.[305] Sulloway found and described in detail a piece of paper, dated by internal evidence to early 1837, recording a discussion at the Zoological Society between Darwin and Gould. It was *then*, Sulloway claimed, that Darwin was made aware of the expert opinion that the birds on the different islands represented distinct species. This would mesh with the well-known remark in Darwin's "Journal" that he was struck with the character of the South American fossils and the Galapagos species in the month of March before he opened his first "Transmutation notebook".[306]

Sulloway then turned to deal with the "Red notebook", seeking to refine Sandra Herbert's dating for the notes on the ostrich (rhea), which gave intimations of transmutationist ideas. Herbert had placed these notes somewhere between January and July, 1837. The ostrich case was more difficult than that of the finches, for the two different forms overlapped, rather than (for example) occurring on separate islands. Modern zoologists would suppose that the two forms evolved in geographical isolation and subsequently mingled a bit. But Darwin didn't take this view. Rather, as previously noted,[307] he gave a 'saltationist' view, one species supposedly being formed from the other "*per saltum*".[308] According to Herbert,[309] such passages (on rheas) in the "Red notebook" marked Darwin's earliest transmutationist hint. However, from the picture as reconstructed by Sulloway, it would seem rather unlikely that Darwin could have thought of the rheas the way he did without previously having been primed by his thinking about geographical isolation in the Galapagos, stimulated by his discussions with Gould. But this was not a problem for Sulloway. The transmutationist segments of the "Red notebook" come just after Darwin's meeting with Gould. And by internal evidence Sulloway placed them

in the last two weeks of March 1837, which is compatible with Herbert's dating.

The conclusion reached by Sulloway and Herbert, then, was that it was the facts of biogeography,[310] amassed during the *Beagle* voyage, and then evaluated by expert taxonomists and comparative anatomists, that drove Darwin to the idea of transmutation — though not a worked-out theory to explain it, of course. The idea of transmutation, it seems, occurred in March 1837, as Darwin himself had said, long ago.

When I read Sulloway's paper, I was convinced that he had at last got to the bottom of the problem of when Darwin became a transmutationist. But in 1982 the matter was not, in fact, resolved so tidily, for an important study by M. J. S. Hodge was also published,[311] which argued once again for Darwin's first conversion to the transmutationist hypothesis during the course of the *Beagle* voyage. So we must also examine Hodge's detailed work with the attention that it deserves.

As mentioned above, Hodge was particularly concerned to determine the exact role of Lyell's work in Darwin's theory construction. It is, of course, well known that Lyell's influence was profound, and that after the *Beagle* voyage they became, in time, intimate friends and correspondents. However, the precise role of Lyell in Darwin's early intellectual construction work had not been sufficiently thoroughly scrutinized before the publication of Hodge's essay. Hodge, it should be noted, thought it an historiographically inappropriate question to ask when Darwin first "believed in evolution".[312] For one thing, it was not an 'all-or-nothing' matter, but a complex and extended process of theory construction. And for another, Darwin's early work was concerned with the processes of species change, and then with the question of "the origin of species". To deploy the word evolution here is likely to lead us rapidly to historiographical anachronism and solecism.[313]

According to Lyell's theory, the Earth's crust was constantly moving up and down in different places, bringing constant local fluctuations in environmental conditions.[314] These would cause species extinctions from time to time; likewise there would supposedly be creation of new forms from time to time. The actual mechanism for species creation was not spelled out by Lyell, but by considering the adaptations of different organisms he did seek to explain why particular types would appear in particular places, for they would be suitably adapted to those places. However, by examination of Darwin's notes of 1835, Hodge showed that Darwin came to reject Lyell's account of extinction, for the field evidence suggested that the giant mammals had become extinct without any obvious change of conditions that might have caused their extinction.[315] So perhaps, Darwin thought, species died out when they became, so to speak, 'exhausted' — just as plants, continuously grafted from a single stock, eventually die out.[316] Perhaps there could be a finite number of individual 'germs' propagable in an animal species, just as there was (apparently) a finite number

of buds propagable from a single grafted plant bud.

As to the formation of new species, and the relationship of species to their particular geographical regions, Darwin considered carefully the very large quantity of data that he assembled in his South American travels. But early in 1835 he was still disposed to accept Lyell's views of the matter. And in Darwin's notebooks for the period in the Andes and the Galapagos, Hodge detected no indications of change to a new theory of biogeography. But by the time of the "Ornithological notes" and the "Red notebook" (late voyage and back in England), Darwin was writing words that suggested a transmutationist hypothesis. So Hodge tentatively situated the change in Darwin's view of biogeography — from a Lyellian to a transmutationist position — in Australia where Darwin would have been pondering his South American observations, and making new ones in the arid continent. (However, Hodge was not able to provide any direct documentary support for this proposal.)

So, on Hodge's interpretation, by the time that Darwin was writing about the Galapagos fauna in the "Ornithological notes" of the late voyage, he was doing so in terms of a transmutationist view, not Lyellian adaptation. For example, the Galapagos are deficient in indigenous insects, and also deficient in insect-eating birds. This was explained by assuming that the islands were inaccessible to insects. Birds *did* reach the island; and then changed in the adverse conditions to produce new indigenous species. By contrast, the insects never got to the Galapagos to be able to do the same thing. This, according to Hodge, was Darwin's way of seeing the problem, by the end of the voyage. Likewise, the overlapping rhea species could be construed as being types that had migrated and transmuted. However, while it must be acknowledged that Hodge did an admirable job in effecting a reconstruction of Darwin's thinking during the voyage, rightly relating his thinking closely to that of Lyell, there is doubt in my mind as to whether the case for *species* transmutation was established. For the passage in the "Ornithological notes" where Darwin spoke of the Galapagos birds must be recalled: "I must suspect they are only varieties."[317]

Considering, then, the "Transmutation notebooks", Hodge showed[318] that early in "Notebook B" Darwin was entertaining *two* modes of species change: either sudden, with changing environmental circumstances; or gradual, as a result of geographical isolation. Both of these had to be encompassed by Darwin's theorizing. Like Kohn, Hodge saw this period of Darwin's intellectual development as being particularly concerned with the idea of variation arising from sexual reproduction, which had a kind of teleological role — to produce new species from old.[319] However, Hodge saw more in Darwin's references to monads (or "monucles") that did Kohn. For Hodge, the monad hypothesis was important for the understanding of the tree diagrams near the beginning of "Notebook B", which although often reproduced in textbooks, etc., had scarcely been subjected to close exegetic scrutiny before Hodge's work.

Darwin (like Lamarck) needed to explain the presence on Earth of simple forms, accompanying complex types. This could be done if one contemplated the successive spontaneous production of simplest life forms (monads), which could, however, develop their potentialities (so to speak) to produce a definite number of different types — all of which would eventually die out according to the Brocchi principle of species senescence. A monad, and all its issue, could be regarded as one very large life-span, with birth, youth, maturity, old age and death. And it would branch like a tree — like the offspring from the head of a family. Some (like mammals) might proceed quickly in their development; others (like molluscs) would proceed more slowly. Such a model enabled Darwin to arrive at the well-known "coral of life" diagram of "Notebook B".[320] However, Darwin quickly discarded the monad-theory basis of the tree model, while retaining the notion of a branching tree. In his subsequent diagram,[321] we have species deaths and births approximately in balance, with some 'twigs' on the tree terminating (dying) and new ones budding out (being born). The resultant pattern could more or less account for Darwin's taxonomic, biogeographical and palaeontological data. All this was, of course, developed independently of any natural selection principle, and with the third tree model the monad hypothesis was discarded. But the short-lived monad theory did, on Hodge's view, leave an important and permanent mark on the shape of Darwin's eventual theory.

It would appear from the foregoing that Darwin had some kind of principle of divergence from very early on in the development of his theory. Yet, according to his *Autobiography* Darwin had difficulty in accounting for divergence satisfactorily, and did so only long after the rest of his theory was established. However, the divergence implicit in the tree diagrams of "Notebook B" was aetiologically very different from that eventually made public in 1859, which was intimately linked with the notions of population pressure ('wedging') and natural selection. Even so, it seems likely that there was carry-over of the notion of branching from the early to the later work and Darwin's mature understanding of divergence. So to complete this survey, I shall now look at some of the chief secondary sources on the question of how Darwin arrived at the principle of divergence.

There have been some insinuations and accusations in the literature[322] that Darwin filched the divergence principle from Wallace. But this is wholly at odds with Darwin's own report of how he arrived at the principle. In his *Autobiography* Darwin asserted that he was driving in his carriage one day when the idea suddenly came to him as to the causal mechanism of divergence.[323] And now that we have Dov Ospovat's reconstruction of how Darwin arrived at the principle,[324] the accusations against Darwin on this matter can, I think, be discounted.

We have just been examining some of Darwin's early ideas on branching,

found in "Notebook B". However, Ospovat pointed out that the idea of branching was commonplace in early nineteenth century natural histories. (Indeed tree metaphors have been commonplace throughout human history.[325]) But perhaps more important for Darwin's thinking on this matter was his growing interest, after 1838, in embryology, to which particular attention was drawn by Chambers's *Vestiges* of 1844.[326]

Contemplating the phenomena of embryonic growth, Darwin noted that as an embryo develops it becomes more and more differentiated from other forms of embryos. And he supposed that this differentiation process might continue after birth. His investigation of pigeons showed that there was a gradual divergence of form, after hatching, into different varieties. This suggested that variation occurred chiefly later in life. Also (an idea carried over into Darwin's theory of inheritance, via pangenes, in his *Variation*[327]), he supposed that inheritance manifests itself at corresponding ages in parents and offspring — e.g., the onset of grey hair or baldness.

So branching seemed to be a kind of process that went on even between conception and adulthood. Between 1846 and 1858, Darwin wrote many, presently unpublished, notes on the tendency of embryos to diverge. He accepted from embryologists like von Baer and Milne Edwards, the principle that the more different two animal forms are, so much the further back in embryonic development must one go to find similarities between them. Thus embryology could provide a systematic basis for taxonomy — an important prop to Darwin's argument as eventually presented in *The origin*.

However, it seems that Darwin finally approached his principle of divergence from taxonomy rather than embryology. He was interested in so-called 'botanical arithmetic' — notably the question of why it is that large genera are the most successful; or why families with many genera and species also have many specimens, being 'successes' in the evolutionary struggle for existence.

Ospovat showed that Darwin's notes for 1847 reveal him as recognizing that something more than accidental dispersal and natural selection is required for divergence. In 1856, when writing *Natural selection*, Darwin evidently possessed the principle.[328] Between these dates we have the massive works on barnacles, published in the early 1850s. In 1848, Darwin recorded how he was struck that there seemed to be no limit to the endless breaking up of taxonomic groupings in time. In 1854, he wrote a note remarking that large genera tend to be widely spread, hence encountering many habitats, and therefore tending to diverge. Thus "the most diverse forms can best succeed".[329] This was almost the principle of divergence proper, but not quite, for Darwin restricted it to the circumstances of organisms encountering new conditions.

So, taking the matter further, Darwin began to study geographical distributions once again. Consulting a wide range of botanical texts, he made calculations to determine whether large genera in fact had wider ranges than

smaller genera. His evidence seemed to suggest that this was so, but it didn't really prove his hypothesis that "the most diverse forms can best succeed". So how could he show that large genera are growing genera; or the converse? To do so, he hypothesized that large genera are characterized by many 'fine' or 'transitional' species. (By contrast, a small genus like that of the platypus might have only one extant species.) And by the spring of 1856 Darwin had collected data which seemed sufficient to support this hypothesis.[330] The previous year he had been collecting pasture plants to substantiate the claim, later made in *The origin*, about divergence of species within a piece of turf. He was supported on this by an article he found in *The gardener's chronicle* for 1836 on the growth of mixed and unmixed wheats.[331]

Two further important notes from Darwin's manuscripts were mentioned by Ospovat. In January 1855 Darwin wrote that he had been led to his principle of divergence "by looking at a heath thickly clothed by heath, and a fertile meadow, both crowded, yet one cannot doubt more life supported in second than in first; and hence (in part) more animals are supported. This is not final cause, but mere results from struggle."[332] Ospovat related this to the 'carriage episode' in the *Autobiography*.

But there was a further note, dated 23 September 1856, in which Darwin wrote:

> The advantage in each group becoming as different as possible, may be compared to the fact that by division of ... labour most people can be supported in each country — Not only do the individuals of each group strive one against the other, but each group itself with all its members, some more numerous, some less, are struggling against all other groups, as indeed follows from each individual struggling.[333]

Ospovat saw this passage as marking a shift in Darwin's thinking: to the view that divergence *always* occurs, even in groups living in the same regions in essentially the same conditions. Also, we notice a link back to social concerns, such as may appeal to the 'externalist' historian. For Darwin here explicitly mentions the analogy with the economists' principle of division of labour. And he reiterated it in *Natural selection*.[334]

So perhaps, once again, we should look at Darwin's social milieu for the inspiration of his theory. This was done in an interesting paper by Schweber (1980).[335] In *Natural selection*, in discussing the principle of divergence, Darwin made specific reference to a volume by the French naturalist, Henri Milne Edwards, published in 1851,[336] which Darwin read in 1852. Milne Edwards himself referred to the 'physiological' division of labour, but he also explicitly stated that he had reached his idea by thinking about the writings of political economists.

According to Schweber, there are early indications of the divergence principle

in, for example, the 1842 "Sketch". However, he claimed, as the natural selection principle came to be seen more and more by Darwin as a universal explanatory principle in natural history, he lost sight of the divergence principle for a time. But then it came back to him when he read Milne Edwards in 1852. Actually, Darwin was perfectly aware of Adam Smith's principle of the division of labour before he read Milne Edwards, just as he was aware of what Malthus had to say before he read Malthus. But, in Schweber's view, things fell into place for Darwin after reading Milne Edwards — just as they did after the Malthus episode.

In fact, according to Schweber, one can find some intimations of the divergence principle in the "Transmutation notebooks" — a claim with which our foregoing discussions would lead us to agree. But further, in Schweber's view, the statistical discussions on biogeography that one finds in the "Notebooks" very likely derived from Utilitarian political economy. The principle of maximization of organic matter can, for example, be compared with the Benthamite principles of maximization of output and of happiness.

We have said that Darwin read Milne Edwards in 1852. But it may be that he did this earlier for his barnacle work in the 1840s, and possibly even on the *Beagle* voyage, for the ship carried a copy of Milne Edwards's *Dictionnaire classique d'histoire naturelle* (1827), where the following passage occurs:

> The body of these animals [polyps] can be compared to a workshop where each worker is employed in executing similar labours, and where, consequently, their number influence the sum total, but not the nature of the result. In effect, each portion of the body can smell, contract, move, nourish itself, and reproduce into a new body....
>
> The diverse parts of the animal economy all compete towards the same goal, but each in a manner appropriate to it, and the more the faculties of the organism are numerous and developed, the greater the diversity of structure and the division of labour ... are furthered.[337]

So there seems reason to see Milne Edwards as the vehicle whereby a principle of economics entered Darwin's thinking, and thereby played *some* role in his arrival at the principle of divergence. In Schweber's view, this was particularly congenial to Darwin, who could thereby give the impression that his science was untainted with political ideology. For Milne Edwards had already conveniently metamorphosed Smith's principle of "division of physiological labour" into the biological notion of "physiological division of labour". It was, for Darwin, a tactical advantage to refer to the writings of an eminent zoologist, rather than a political economist.

But quite apart from Milne Edwards, Schweber clearly displayed the Utilitarian background to Darwin's thought, as revealed by his reading and his family connections. In 1840, Darwin read J. R. McCulloch's *Principles of*

political economy,[338] which discussed *territorial* division of labour — for example, different regions concentrating on different kinds of commercial activity. Thus, improvements in communication facilitated trade and wealth. (Darwin agreed and invested successfully in railways and canals.)

Darwin also read in 1847 the work of the Swiss economist Sismondi (to whom he was related by marriage), which expressed a repudiation of the 'cut-throat' economic views of some of the British political economists.[339] Darwin marked the book "Poor", which suggests that it was the British image of nineteenth century capitalism that he favoured.

Needless to say, all these matters were not revealed in *The origin*, and although Darwin 'took on board' the principle of "division of labour" he appears to have done so through the mediation of a reputable zoologist, Milne Edwards. The case was rather different for Malthus, who was explicitly mentioned in *The origin*. For Malthus's principle itself rested on a biological law; therefore Darwin felt able to use it as the basis of a biological argument. As for Milne Edwards himself, he said that he got his idea from J. B. Say of Say's Law fame;[340] and Say apparently got some of his ideas from the botanist A. P. de Candolle, who likened struggle for existence among living organisms to warfare,[341] and to whom both Lyell and Darwin made several references.

It may be noted that Ospovat reported[342] that Schweber's article was written without the advantage of access to the manuscripts of the Darwin archive at Cambridge. But this in itself does not, of course, nullify an 'externalist' thesis in relation to Darwin's route to the principle of divergence, as argued by Schweber. And Ospovat himself, in his concluding chapter, rightly regarded "the development of Darwin's theory as a social process". In summary, then, it seems that both the 'internalist' and 'externalist' historians have had something important to say respecting Darwin's principle of divergence. In one sense, Darwin appears to have had the *idea* of divergence from the early stages of his transformationist doctrines. And evidently he pondered deeply over the problem for many years, during which time ideas gleaned from political economy formed a leaven in his mind. But when the final understanding came — when he saw how the 'wedging principle' and natural selection could account satisfactorily for divergence — it does seem this time to have been more like a classic '*eureka*' experience, such as that described so well by Poincaré.[343] Eventually the long-examined pieces of Darwin's jigsaw suddenly fell into place — if we are to believe his own account, as given in his *Autobiography*.[344]

What conclusions and prognostications can be drawn from the foregoing enquiry? One interesting feature, to my mind, is the general accuracy of Darwin's own various versions of the matter. He never wrote a detailed, systematic account of the origin of his ideas. These were just fragmentary remarks in letters and various publications, notably his *Autobiography*. But now that historians have reconstructed so much of the detail of his life and his thinking, it is

remarkable how closely what Darwin wrote matches with the story, as reconstructed from the Darwin manuscripts. For example, there now seems to be general agreement that Malthus *did* perform a very important service for Darwin, enabling him to conceptualize just how the struggle for existence might bring about species change through 'wedging'. Likewise, his letter to Zacharias of 1877[345] spoke of doubts about the permanence of species flitting across his mind during the *Beagle* voyage, but that when he was preparing his journal for publication on his return to England he "saw how many facts indicated the common descent of species". This is compatible with the account of Darwin's thought-processes on the voyage, as reconstructed by Hodge, and the results of the investigations of Sulloway, Herbert and others in relation to the post-voyage period. Even the famous (and oft-maligned) passage in the *Autobiography*, where Darwin said of his "Transmutation notebooks" that he "worked on true Baconian principles, and without any theory collected facts on a wholesale scale",[346] seems to me to be broadly compatible with what we now know of the "Notebooks". Certainly, Darwin had no *fixed* theory, and he drew in a great deal of information somewhat like a jackdaw — which is not to say that his thinking was unstructured or incoherent. The way he suddenly shifted his attention with the opening of "Notebook M" is an indication that he was seeking to widen the net to capture the theory that was proving so elusive — collecting information that might not appear at first sight to be so very relevant to the enquiry. This is Baconian, using the term loosely. One doesn't have to draw up tables of 'essence and presence' etc. before the epithet 'Baconian' is applicable. However, it must be acknowledged that Darwin's 'fact-gathering' was structured by a succession of tentative theories or hypotheses, so his words "without any theory" do appear today as somewhat misleading.

It must further be noted that, as has been shown in Tallmage,[347] in the composition of his *Journal of researches* Darwin systematically altered the course of events for the *Beagle* voyage, seemingly for literary effect — to make it appear as if his explorations yielded one progressive unfolding of experience and understanding. Also, in his Introduction to *Variation* (1868), Darwin depicted himself in the Galapagos as having ideas that we now think he did not reach until two years later.[348] And a great deal of the difficulty that has attended the reconstruction of the events leading to the formulation of Darwin's theory lies in the fact that he said virtually nothing in print about his early attempts to construct a transmutationist theory *before* the natural selection hypothesis was achieved. However, it is now held in some quarters[349] that Darwin's early attempts at theorizing provided an obstruction to his achievement of the selection hypothesis. So historians have been at a considerable disadvantage as a result of Darwin's public reticence concerning the early phases of his thinking.

The enquiry into the origin of Darwin's theory has been the subject of some valuable 'philosophical' reconstructions;[350] and also the concern of some

biologists with interests to pursue — or less charitably with axes to grind.[351] But chiefly it has been a matter of empirical enquiry — with the written records left by Darwin and others forming the principal basis for work. This can be seen from the impulse given to the 'Darwin Industry' by the publication of Darwin's *Letters* and his "Transmutation notebooks". Some of those earlier publications are now being revised, and only now is the archival record itself being reconstructed in published form in a manner adequate to meet the needs of modern scholarship.[352] The full publication of Darwin's "Theoretical notebooks" for the period 1836-44 has been announced,[353] and likewise the future publication of *The collected letters of Charles Darwin* is promised.[354] The issue of such material will, I doubt not, sustain the 'Darwin Industry' for decades to come. As Figure 2 shows, there is no indication that the end of the growth period for the literature on the origin of Darwin's theory is in sight. One might hazard that it will last another twenty years or so at least, or until every word that Darwin ever wrote on every scrap of paper is finally in print. But long before then, 'new questions' may begin to be asked; and of course it is tempting to ask what those might be.

An interesting line of enquiry, that is just being opened up by Evelleen Richards,[355] concerns Darwin's personal relations with his family. His illness has already received much attention,[356] but in my view such study is unlikely to throw much further light on the origin of Darwin's theory, thinking of the matter from the perspective of Darwin's psychological characteristics.

However, it is possible that when the complete Darwin archive is in print we shall have further information that will enable scholars to follow Gruber's lead and explore more thoroughly the psychological aspects of Darwin's process of theory construction. But it must be acknowledged that psychological reconstructions are fraught with difficulty. An attempted Freudian analysis of Darwin's discussion of the Galapagos tortoises that I encountered appeared quite ludicrous,[357] and I was not clear whether it was or was not intended as a joke. I am uncertain, therefore, as to how much further progress will be made with the reconstruction of Darwin's psychic processes — especially when one seeks to probe the realm of the unconscious.

In a field of scholarship as active as the Darwin Industry, it is impossible to be sure what will happen next, or to say with certainty what the lines of future research may be. My task, therefore, has been to give as clear a picture as possible of what had been achieved by 1982, and the manner and sequence in which this was done.

What emerges is a process that can be likened to the composite painting of a three-dimensional picture — a picture, that is, of Darwin's gradual construction of his theory. Year by year, details have been added to the representation, and the whole has gradually been brought into sharper focus. The 'internal' features of the history may be likened to the foreground of the picture; the 'external'

features to the background. Both are needed to supply a total representation. Neither the 'internalist' nor the 'externalist' features can be represented successfully without knowledge of work going on on other parts of the picture.

According to the history of (the segment of) the Darwin Industry presented in this paper, it has not been found necessary to erase many of the 'artists'' (i.e., historians') contributions, though sometimes 'objects' that were initially painted large (e.g., in Judd's depiction of the role of the extinct giant mammals in South America) have subsequently been diminished in size. The 'size' of Malthus in the composition has fluctuated considerably over the years, and he has been shifted around between background and foreground. Presently, he seems to be represented as a large figure, linking foreground and background. An area being worked on assiduously at the moment is that depicting Darwin's thinking on problems of biogeography, and I anticipate that further emphasis will be placed on this aspect of the composition. Areas that seem to require further artistic attention, being painted only lightly by 1982, are those depicting Darwin's work on invertebrate zoology (especially barnacles) and sexual selection.

At the time of writing, it is clear that the composition is incomplete, and I am aware that the forthcoming volumes edited by David Kohn[358] — themselves a belated outcome of the 1982 "Darwinfest" — are likely to reveal gaps or blemishes in the composite picture that I have sought to construct on the basis of the secondary literature to 1982. It should also be remembered that historians are trying to produce a kind of 'motion picture', rather than a 'still life'.

A point that must be emphasized is that even now, after all the efforts of the Darwin Industry in recent years, we *still* cannot say precisely how Darwin arrived at his theory. This is, in itself, an indication of the difficulty that attends historical researches, where experimental methods of enquiry cannot be deployed. However, the main lines of the story are now becoming clear. For example, Darwin (as he said himself) probably became a convinced transmutationist in early 1837. Considerations of biogeography were of paramount importance in this. His early monad theory led him to think of tree models and to an early notion of divergence (not the same as that which crystallized in his mind in the 1850s). His reading on man, and 'humanist' works, led him to Malthus, who *did* play a critical role in emphasizing the significance of population pressures. The analogy between artificial selection and natural selection was not, it seems, quite so important for Darwin's thinking as he suggested in his own accounts of the origin of his theory.

In considering possible futures for the Darwin Industry, one can see from Figure 2 that the historical research on the origin of Darwin's theory will not be completed for several years. Present studies of the Darwin manuscripts are, however, becoming increasingly esoteric, with consideration of different coloured inks and the like. And one may have a fear that when the Darwin archive is eventually published in full, the Industry may degenerate into an arid

scholasticism. However, such a prospect does not seem very plausible, for science is itself a dynamic entity, constantly generating new historical problems. So we are not likely to see a state of affairs developing such as one finds in Classics, where, deprived of nutriment in the form of new textual material for exegesis, scholars become obsessed with trivia and descend to the frequent employment of *ad hominem* arguments. Rather, I believe, when the Darwin Industry has brought its logistic curve to a smooth plateau, there will be no shortage of other topics awaiting consideration. And these will, I doubt not, soon command the attention of the present Darwin *aficionados*. Of course, philosophers of history tell us that history is "a continuous process of interaction between the historian and the facts",[359] so that changing historians produce changing histories. However, on the very narrow question of how Darwin arrived at his theory, I think we may expect to see a general consensus eventually emerge, even if that consensus does not correspond to some ultimate truth about the matter in question. As to the wider import of what Darwin discovered and how he did it, there will probably never be final agreement.

REFERENCES

1. C. R. Darwin, *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life* (London, 1859).
2. C. R. Darwin, *The variation of animals and plants under domestication...* (2 vols, London, 1868), i, Introduction.
3. E. Haeckel, *The history of creation of the development of the Earth and its inhabitants by the action of natural causes: A popular exposition of the doctrine of evolution in general, and of that of Darwin, Goethe, and Lamarck in particular* (London, 1876; first German edn, Berlin, 1868).
4. S. Butler, *Evolution old and new: or, the theories of Buffon, Dr. Erasmus Darwin, and Lamarck, as compared with that of Mr. Charles Darwin* (London, 1879).
5. G. Allen, *Charles Darwin* (London, 1887).
6. G. T. Bettany, *Life of Charles Darwin* (London, 1887).
7. F. Darwin (ed.), *The life and letters of Charles Darwin, including an autobiographical chapter* (3 vols, London, 1887).
8. T. H. Huxley, "Obituary notices of fellows deceased [Charles Darwin]", *Proceedings of the Royal Society of London*, xliv (1888), i-xxv.
9. H. F. Osborne, *From the Greeks to Darwin: An outline of the development of the evolution idea* (New York and London, 1894).
10. E. B. Poulton, *Charles Darwin and the theory of natural selection* (New York, 1896).
11. F. Darwin and A. C. Seward (eds), *More letters of Charles Darwin: A record of his work in a series of hitherto unpublished letters* (2 vols, London, 1903).
12. *The Darwin-Wallace Celebration held on Thursday, 1st July, 1908, by the Linnean Society of London* (London, 1908). (This contains some reflections by Wallace on the events surrounding the establishment of the Darwin/Wallace theory.)
13. E. Rádl, *Geschichte der biologische Theorien...* (2 vols, Leipzig, 1905-9; English edn, Oxford, 1930).
14. E. B. Poulton, *Charles Darwin and the origin of species* (London, 1909).

15. F. Darwin (ed.), *The foundations of the origin of species: Two essays written in 1842 and 1844* (Cambridge, 1909).
16. J. W. Judd, "Darwin and geology", in A. C. Seward (ed.), *Darwin and modern science* (Cambridge, 1910), 337-84.
17. J. W. Judd, *The coming of evolution* (Cambridge, 1910).
18. H. M. Vickers, "An apparently hitherto unnoticed 'anticipation' of the theory of natural selection", *Nature*, lxxxv (1910-11), 510-11.
19. J. W. Judd, "Charles Darwin's earliest doubts concerning the immutability of species", *Nature*, lxxxviii (1911-12), 8-12.
20. G. Sarton, "Discovery of the theory of natural selection", *Isis*, xiv (1930), 133-54.
21. J. Rostand, *L'évolution des espèces: Histoire des idées transformistes* (Paris, 1931).
22. N. Barlow (ed.), *Charles Darwin's diary of the voyage of H.M.S. "Beagle"* (Cambridge, 1933).
23. N. Barlow, "Charles Darwin and the Galapagos Islands", *Nature*, cxxxvi (1935), 391 and 534-5.
24. T. Cowles, "Malthus, Darwin and Bagehot: A study in the transference of a concept", *Isis*, xxxi (1936), 341-8.
25. A. Sandow, "Social factors in the origin of Darwinism", *Quarterly review of biology*, xiii (1938), 315-26.
26. C. Zirkle, "Natural selection before the 'Origin of species'", *Proceedings of the American Philosophical Society*, lxxxiv (1941), 71-125.
27. N. Barlow (ed.), *Charles Darwin and the voyage of the Beagle* (London, 1945).
28. R. H. Shryock, "The strange case of Wells' theory of natural selection (1813): Some comments on the dissemination of scientific ideas", in M. F. Ashley Montagu (ed.), *Studies and essays in the history of science and learning offered in homage to George Sarton on the occasion of his sixtieth birthday 31 August 1944* (New York, 1946), 197-207.
29. J. S. Huxley, "Charles Darwin: The decisive years", *Nature*, clvii (1946), 536-8.
30. D. Lack, *Darwin's finches: An essay on the general biological theory of evolution* (Cambridge, 1947).
31. P. H. Jespersen, "Charles Darwin and Dr. Grant", *Lychnos*, xi (1948-49), 159-67.
32. P. Tasch, "Darwin and the forgotten Mr. Lonsdale", *Geological magazine*, lxxxvii (1950), 292-6.
33. L. C. Eiseley, "Charles Darwin", *Scientific American*, clxciv (1956), 62-72.
34. G. de Beer, *Charles Darwin: Lecture on a master mind* (London, 1958).
35. G. de Beer, "The Darwin-Wallace centenary", *Endeavour*, xvii (1958), 61-76.
36. L. C. Eiseley, *Darwin's century: Evolution and the men who discovered it* (New York, 1958).
37. C. D. Darlington, "The origin of Darwinism", *Scientific American*, cc (1959), 60-66.
38. P. H. Barrett, "From Darwin's unpublished notebooks", *The centennial review of arts and sciences*, iii (1959), 376-90.
39. L. C. Eiseley, "Charles Darwin, Edward Blyth and the theory of natural selection", *Proceedings of the American Philosophical Society*, ciii (1959), 94-158.
40. P. J. Darlington, "Darwin and zoogeography", *Proceedings of the American Philosophical Society*, ciii (1959), 307-19.
41. A. C. Crombie, "Darwin's scientific method", *Actes du IX^e Congrès International d'Histoire des Sciences, Barcelona, Madrid, 1-7 Septembre 1959*, i (Barcelona, n.d.), 354-62.
42. J. K. Feibleman, "Darwin and scientific method", *Tulane studies in philosophy*, viii (1959), 3-14.
43. G. de Beer (ed.), "Darwin's journal", *Bulletin of the British Museum (Natural History) historical series*, ii (1959), 1-21.
44. E. Mayr, "Darwin and evolutionary theory in biology", in B. Meggers (ed.), *Evolution and anthropology: A centennial appraisal* (Washington, 1959), 3-12.
45. S. L. Sobol, "Evolusionnaya koncepcia Charlza Darvina na piervyh etapah eyo

formirovanya", *Annaly biologii*, i (1959), 13-34 (in Russian with English summary).
46. E. M. Kondratiuk, "Ch. Darwin-Tvorets' materialistichnoi teorii rozvitku organichnogo svitu", *Ukrain'skyi botanichnyi zhurnal*, xvi (1959), 3-14 (in Ukrainian). (I am grateful to Barry Lewis for furnishing me with a translation of a section of this paper.)
47. T. Dobzhansky, "Blyth, Darwin, and natural selection", *American naturalist*, xciii (1959), 204-6.
48. R. C. Stauffer, "'On the origin of species': An unpublished version", *Science*, cxxx (1959), 1449-56.
49. G. Himmelfarb, *Darwin and the Darwinian Revolution* (London, 1959).
50. S. Smith, "The origin of the 'Origin'", *Impulse*, xi (1959), 2-14.
51. S. Smith, "Evolution: Two books and some marginalia", *Victorian studies*, iii (1959-60), 109-14.
52. S. Smith, "The origin of 'The origin' as discerned from Charles Darwin's notebooks and his annotations in the books he read between 1837 and 1842", *Advancement of science*, xvi (1960), 391-401.
53. G. Hardin, "The competitive exclusion principle", *Science*, cxxxi (1960), 1292-7.
54. R. C. Stauffer, "Ecology in the long manuscript version of Darwin's *Origin of species* and Linnaeus' *Oeconomy of nature*", *Proceedings of the American Philosophical Society*, civ (1960), 235-41.
55. "Darwin's notebooks on transmutation of species, Part I", edited with an introduction and notes by Sir Gavin de Beer, *Bulletin of the British Museum (Natural History) historical series*, ii (1960), 23-73.
56. *Idem*, "Part II", *ibid.*, 75-118.
57. *Idem*, "Part III", *ibid.*, 119-50.
58. *Idem*, "Part IV", *ibid.*, 151-83.
59. "Darwin's notebooks on transmutation of species: Addenda and corrigenda", edited with notes by Sir Gavin de Beer and M. J. Rowlands, *Bulletin of the British Museum (Natural History) historical series*, ii (1961), 185-200.
60. G. de Beer, "The origins of Darwin's ideas on evolution and natural selection", *Proceedings of the Royal Society of London*, series B: *Biological sciences*, clv (1961-62), 321-38.
61. H. E. Gruber and V. Gruber, "The eye of reason: Darwin's development during the *Beagle* voyage", *Isis*, liii (1962), 186-200.
62. L. Croizat, *Space, time, form: The biological synthesis* (Caracas, 1962), ch. 8.
63. "Darwin's ornithological notes", edited with an introduction, notes and appendix by Nora Barlow, *Bulletin of the British Museum (Natural History) historical series*, ii (1963), 201-78.
64. P. Vorzimmer, "Charles Darwin and blending inheritance", *Isis*, liv (1963), 371-90.
65. G. de Beer, *Charles Darwin: Evolution by natural selection* (London, 1963).
66. G. de Beer, "How Darwin came by his theory of natural selection", *New scientist*, xxi (1964), 216-18.
67. G. de Beer, "Other men's shoulders", *Annals of science*, xx (1964), 303-22.
68. J. Théodoridès, "Humboldt et Darwin", *Actes du XIe Congrès International d'Histoire des Sciences ... 24-31 Août 1965*, v (Warsaw, 1968), 87-92.
69. S. Smith, "The Darwin collection at Cambridge with one example of its use: Charles Darwin and *Cirripedes*", *Actes du XIe Congrès International d'Histoire des Sciences 24-31 Août 1965*, v (Warsaw, 1968), 96-100.
70. P. J. Vorzimmer, "Charles Darwin's ecology and its influence upon his theory", *Isis*, lvi (1965), 148-55.
71. L. C. Eiseley, "Darwin, Coleridge, and the theory of unconscious creation", *Daedalus*, xciv (1965), 588-602.
72. H. E. Gruber, "Pensée créatrice et vitesse du changement adaptif: Le développement de la

pensée de Darwin', in F. Bresson and M. de Montmollin (eds), *Psychologie et épistémologie génétiques: Thèmes Piagétiens* (Paris, 1966), 407-21.
73. "Darwin's notebooks on transmutation of species. Part VI: Pages excised by Darwin", edited by Sir Gavin de Beer, M. J. Rowlands and B. M. Skramovsky, *Bulletin of the British Museum (Natural History) historical series*, iii (1967), 129-76.
74. F. N. Egerton, "The concept of competition in nature before Darwin", *XII^e Congrès international d'Histoire des Sciences Paris 1968: Actes*, viii (Paris, 1971), 41-46.
75. C. Limoges, "Darwin, Milne-Edwards et le principe de divergence", *ibid.*, 112-15.
76. F. N. Egerton, "Studies of animal population from Lamarck to Darwin", *Journal of the history of biology*, i (1968), 225-59.
77. R. M. Young, "Malthus and the evolutionists: The common context of biological and social theory", *Past and present*, xliii (1969), 109-45.
78. P. J. Vorzimmer, "Darwin's *Questions about the breeding of animals* (1839)", *Journal of the history of biology*, ii (1969), 269-81.
79. P. J. Vorzimmer, "Darwin, Malthus and the theory of natural selection", *Journal of the history of ideas*, xxx (1969), 527-42.
80. M. T. Ghiselin, *The triumph of the Darwinian method* (Berkeley, Los Angeles and London, 1969).
81. C. Limoges, *La sélection naturelle: Étude sur la première constitution d'un concept (1837-1859)* (Paris, 1970).
82. C. Limoges, "Darwinism et adaptation", *Revue des questions scientifiques*, xxxi (1970), 353-74.
83. F. N. Egerton, "Humboldt, Darwin, and population", *Journal of the history of biology*, iii (1970), 325-60.
84. G. de Beer, "The evolution of Charles Darwin [review of Ghiselin's *Triumph of the Darwinian method*]", *New York review of books*, xv (1970), 31-35.
85. M. T. Ghiselin, "The individual in the Darwinian Revolution", *New literary history*, iii (1971), 113-34.
86. R. M. Young, "Evolutionary biology and ideology: Then and now", *Science studies*, i (1971), 177-206.
87. R. M. Young, "Darwin's metaphor: Does nature select?", *The monist*, lv (1971), 442-503.
88. S. Herbert, "Darwin, Malthus and selection", *Journal of the history of biology*, iv (1971), 209-17.
89. F. N. Egerton, "Darwin's analysis of biological competition", *Proceedings of XIIIth International Congress of the History of Science Moscow August 18-24, 1971*, ix (Moscow, 1974), 71-77.
90. K. D. Wells, "Sir William Lawrence (1783-1867): A study of pre-Darwinian ideas on heredity and variation", *Journal of the history of biology*, iv (1971), 319-62.
91. E. Mayr, "Open problems of Darwin research [essay review of reprint of Darwin's *Life and letters*]", *Studies in history and philosophy of science*, ii (1971), 273-80.
92. F. N. Egerton, "Darwin's method or methods? [essay review of Ghiselin's *Triumph of the Darwinian method*]", *Studies in history and philosophy of science*, ii (1971), 281-6.
93. B. G. Beddall, "Wallace, Darwin and Edward Blyth: Further notes on the development of evolution theory", *Journal of the history of biology*, v (1972), 153-8.
94. E. Mayr, "The nature of the Darwinian Revolution", *Science*, clxxvi (1972), 981-9.
95. B. G. Gale, "Darwin and the concept of a struggle for existence: A study in the extrascientific origins of scientific ideas", *Isis*, lxiii (1972), 321-44.
96. R. M. Young, "Darwin and the division of labour", *The listener*, lxxxviii (1972), 202-5.
97. W. B. Huntley, "David Hume and Charles Darwin", *Journal of the history of ideas*, xxxiii (1972), 457-70.

98. K. D. Wells, "William Charles Wells and the races of man", *Isis*, lxiv (1973), 215-25.
99. B. G. Beddall, "'Notes for Mr. Darwin': Letters to Charles Darwin from Edward Blyth at Calcutta: a study in the process of discovery", *Journal of the history of biology*, vi (1973), 69-95.
100. K. D. Wells, "The historical context of natural selection: The case of Patrick Matthew", *Journal of the history of biology*, vi (1973), 225-58.
101. A. Sibatani, "Antiscience as expressed by a professional scientist: A non-European viewpoint", in *Modern science and moral values: Proceedings of the Second International Conference on the Unity of the Sciences, Tokyo, November 18-21, 1973* (Tokyo, 1973), 553-68. (This paper is not chiefly concerned with the origin of Darwin's theory, but it contains the claim, and the Conference discussion thereof, that the British navy was one of the 'causal factors' leading to Darwin's discoveries.)
102. F. N. Egerton, "Changing concepts of the balance of nature", *Quarterly review of biology*, xlviii (1973), 322-50.
103. M. Ruse, "The nature of scientific models: Formal *v* material analogy", *Philosophy of the social sciences*, iii (1973), 63-80.
104. M. Ruse, "The value of analogical models in science", *Dialogue*, xii (1973), 246-53.
105. D. L. Hull, "Charles Darwin and nineteenth-century philosophies of science", in R. N. Giere and R. S. Westfall (eds), *Foundations of scientific method: The nineteenth century* (Bloomington, Indiana and London, 1973), 114-32.
106. R. J. Wood, "Robert Bakewell (1725-1795) pioneer animal breeder and his influence on Darwin", *Folia Mendeliana*, viii (1973), 231-42.
107. R. M. Young, "The historiographic and ideological contexts of the nineteenth-century debate on man's place in nature", in M. Teich and R. Young (eds), *Changing perspectives in the history of science: Essays in honour of Joseph Needham* (London, 1973), 344-438.
108. P. J. Bowler, "Darwin's concept of variation", *Journal of the history of medicine and allied sciences*, xxiv (1974), 196-212.
109. S. Herbert, "The place of man in the development of Darwin's theory of transmutation. Part I: To July 1837", *Journal of the history of biology*, vii (1974), 217-58.
110. G. Grinnell, "The rise and fall of Darwin's first theory of transmutation", *Journal of the history of biology*, vii (1974), 259-73.
111. J. S. Schwartz, "Charles Darwin's debt to Malthus and Edward Blyth", *Journal of the history of biology*, vii (1974), 301-18.
112. H. E. Gruber and P. H. Barrett, *Darwin on man: A psychological study of scientific creativity ... together with Darwin's early and unpublished notebooks* (New York, 1974).
113. P. J. Vorzimmer, "An early Darwin manuscript: The 'Outline and draft of 1839'", *Journal of the history of biology*, viii (1975), 191-217.
114. M. Ruse, "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", *Studies in history and philosophy of science*, vi (1975), 159-81.
115. M. Ruse, "Charles Darwin and artificial selection", *Journal of the history of ideas*, xxxvi (1975), 339-50.
116. J. C. Greene, "Reflections on the progress of Darwin studies", *Journal of the history of biology*, viii (1975), 243-73.
117. D. R. Biggins, "Biology and ideology", *Science education*, lx (1976), 567-78.
118. W. F. Cannon, "The Whewell-Darwin controversy", *Journal of the Geological Society of London*, cxxxii (1976), 377-84.
119. E. Eng, "The confrontation between reason and imagination: The example of Darwin", *Diogenes*, no. 95 (1976), 58-67.

120. P. J. Bowler, "Darwin, Malthus and the concept of struggle", *Journal of the history of ideas*, xxxvii (1976), 631-50.
121. M. T. Ghiselin, "Two Darwins [Erasmus and Charles]: History versus criticism", *Journal of the history of biology*, ix (1976), 121-32.
122. F. N. Egerton III, "Darwin's early reading of Lamarck", *Isis*, lxvii (1976), 452-6.
123. P. J. Vorzimmer, "The Darwin reading notebooks (1838-1860)", *Journal of the history of biology*, x (1977), 107-53.
124. M. Allan, *Darwin and his flowers: The key to natural selection* (London, 1977).
125. R. Colp, "Charles Darwin and the Galapagos", *New York State journal of medicine*, lxxvii (1977), 262-7.
126. E. Mayr, "Darwin and natural selection: How Darwin may have discovered his highly unconventional theory", *American scientist*, lxv (1977), 321-8.
127. S. S. Schweber, "The origin of the *Origin* revisited", *Journal of the history of biology*, x (1977), 229-316.
128. P. R. Thagard, "Darwin and Whewell", *Studies in history and philosophy of science*, viii (1977), 353-6.
129. S. Herbert, "The place of man in the development of Darwin's theory of transmutation. Part II", *Journal of the history of biology*, x (1977), 155-227.
130. M. J. Kottler, "Charles Darwin's biological species concept and theory of geographic speciation: The transmutation notebooks", *Annals of science*, xxxv (1978), 275-98.
131. S. S. Schweber, "The genesis of natural selection — 1838: Some further insights", *Bioscience*, xxviii (1978), 321-6.
132. J. Browne, "The Charles Darwin-Joseph Hooker correspondence: An analysis of manuscript resources and their use in biography", *Journal of the Society for the Bibliography of Natural History*, viii (1978), 351-66.
133. E. Manier, *The young Darwin and his cultural circle: A study of influences which helped shape the language and logic of the first drafts of the theory of natural selection* (Dordrecht and Boston, 1978).
134. S. S. Schweber, "The young Darwin", *Journal of the history of biology*, xii (1979), 175-92.
135. D. Ospovat, "Darwin after Malthus", *Journal of the history of biology*, xii (1979), 211-30.
136. R. D. Keynes, "Darwin and the *Beagle*", *Proceedings of the American Philosophical Society*, cxxiii (1979), 323-35.
137. S. A. Kleiner, "Feyerabend, Galileo and Darwin: How to make the best out of what you have — or think you can get", *Studies in history and philosophy of science*, x (1979), 285-309.
138. R. Colp, "Charles Darwin's vision of organic nature: 'A force like a hundred thousand wedges'", *New York State journal of medicine*, lxxix (1979), 622-9.
139. F. J. Sulloway, "Geographical isolation in Darwin's thinking: The vicissitudes of a crucial idea", *Studies in history of biology*, iii (1979), 23-65.
140. M. Ruse, *The Darwinian revolution: Science red in tooth and claw* (Chicago and London, 1979).
141. S. J. Gould, "Darwin's deceptive memories", *New scientist*, lxxxv (1980), 577-9.
142. R. Colp, "'I was born a naturalist': Darwin's 1838 notes about himself", *Journal of the history of medicine and allied sciences*, xxxv (1980), 8-39.
143. "The Red Notebook of Charles Darwin", edited with an introduction and notes by Sandra Herbert, *Bulletin of the British Museum (Natural History) historical series*, vii (1980), 1-164.
144. D. Kohn, "Theories to work by: Rejected theories, reproduction, and Darwin's path to natural selection", *Studies in history of biology*, iv (1980), 67-170.
145. P. H. Barrett, "Preface" to *Metaphysics, materialism, and the evolution of mind: Early writings of Charles Darwin*, transcribed and edited by Paul H. Barrett with a commentary by

Howard E. Gruber (Chicago, 1980), xv-xxiv.
146. E. Manier, "History, philosophy and sociology of biology: A family romance", *Studies in history and philosophy of science*, xi (1980), 1-24.
147. J. Browne, "Darwin's botanical arithmetic and the 'principle of divergence', 1854-1858", *Journal of the history of biology*, xiii (1980), 53-89.
148. J. Tallmage, "From chronicle to quest: The shaping of Darwin's 'Voyage of the Beagle'", *Victorian studies*, xxiii (1980), 325-46.
149. S. S. Schweber, "Darwin and the political economists: Divergence of character", *Journal of the history of biology*, xiii (1980), 195-289.
150. H. E. Gruber, "The evolving systems approach to creative scientific work: Charles Darwin's early thought", in T. Nickles (ed.), *Scientific discovery: Case studies* (Dordrecht, Boston and London, 1980), 113-30.
151. M. Ruse, "Ought philosophers consider scientific discovery? A Darwin case-study", *Scientific discovery* (ref. 150), 131-49.
152. E. Manier, "Darwin's language and logic", *Studies in history and philosophy of science*, xi (1980), 305-23.
153. D. Ospovat, "God and natural selection: The Darwinian idea of design", *Journal of the history of biology*, xiii (1980), 169-94.
154. S. A. Kleiner, "Problem solving and discovery in the growth of Darwin's theories of evolution", *Synthèse*, xlvii (1981), 119-62.
155. R. A. Richardson, "Biogeography and the genesis of Darwin's ideas of transmutation", *Journal of the history of biology*, xiv (1981), 1-41.
156. J. J. Parodiz, *Darwin in the New World* (Leiden, 1981).
157. J. Secord, "Nature's fancy: Charles Darwin and the breeding of pigeons", *Isis*, lxx (1981), 163-86.
158. S. Sheets-Peyenson, "Darwin's data: His reading of natural history journals, 1837-1842", *Journal of the history of biology*, xiv (1981), 231-48.
159. D. Kohn, "On the origin of the principle of diversity", *Science*, ccxiii (1981), 1105-8 (review of A. C. Brackman, *A delicate arrangement: The strange case of Charles Darwin and Alfred Russel Wallace* (New York, 1980)).
160. D. Ospovat, *The development of Darwin's theory: Natural history, natural theology, and natural selection, 1838-1859* (Cambridge, 1981).
161. P. Brent, *Charles Darwin: A man of enlarged curiosity* (London, 1981).
162. F. J. Sulloway, "Darwin and his finches: The evolution of a legend", *Journal of the history of biology*, xv (1982), 1-53.
163. K. S. Thomson and S. P. Rachootin, "Turning points in Darwin's life", *Biological journal of the Linnean Society*, xvii (1982), 23-37.
164. M. J. S. Rudwick, "Charles Darwin in London: The integration of public and private science", *Isis*, lxxiii (1982), 186-206.
165. M. T. Ghiselin, "The intellectual path to natural selection", *New scientist*, xciv (1982), 156-9.
166. K. H. Parshall, "Varieties as incipient species: Darwin's numerical analysis", *Journal of the history of biology*, xv (1982), 191-214.
167. J. Beatty, "What's in a word? Coming to terms in the Darwinian Revolution", *Journal of the history of biology*, xv (1982), 215-39.
168. J. Browne, "Essay review: New developments in Darwin studies?", *Journal of the history of biology*, xv (1982), 275-80.
169. F. J. Sulloway, "Darwin's conversion: The *Beagle* voyage and its aftermath", *Journal of the history of biology*, xv (1982), 325-96.
170. F. J. Sulloway, "The *Beagle* collections of Darwin's finches (geospizinae)", *Bulletin of the British Museum (Natural History) zoology series*, xliii (1982), 49-94.

171. D. Kohn, S. Smith and R. C. Stauffer, "New light on *The foundations of the origin of species*: A reconstruction of the archival record", *Journal of the history of biology*, xv (1982), 419-42.
172. E. Mayr, *The growth of biological thought: Diversity, evolution, and inheritance* (Cambridge, Mass., and London, 1982).
173. B. G. Gale, *Evolution without evidence: Charles Darwin and The origin of species* (Brighton, 1982).
174. M. J. S. Hodge, "Darwin and the laws of the animate part of the terrestrial system (1835-1837): On the Lyellian origins of his zoonomical explanatory programme", *Studies in history of biology*, vi (1982), 1-106. (This paper is marked "copyright 1983".)
175. H. E. Gruber, "History and creative work: From the most ordinary to the most exalted", *Journal of the history of the behavioral sciences*, xix (1983), 4-14.
176. R. T. Keegan and H. E. Gruber, "Love, death, and continuity in Darwin's thinking", *Journal of the history of the behavioral sciences*, xix (1983), 15-30.
177. F. B. Churchill, "Darwin and the historian", *Biological journal of the Linnean Society*, xviii (1982), 45-68.
178. A few major items arising from the 1982 centenary have not appeared at the time of writing (1983). This 'time-lag' has therefore partially suppressed the visible effect of the centenary on the graphs of Figures 1 and 2.
179. See, for example, D. Crane, *Invisible colleges: Diffusion of knowledge in scientific communities* (Chicago, 1972), 171ff.
180. F. Darwin (ed.), *op. cit.* (ref. 7), i, 83.
181. K. Marx, "Letter to F. Engels", 18 June 1862, in K. Marx and F. Engels, *Selected correspondence 1846-1895* (3rd edn, Moscow, 1975), 119-20.
182. Rádl, *op. cit.* (ref. 13, 1930), 18.
183. J. M. Keynes, "The end of laissez-faire", pamphlet published by Hogarth Press, July 1926, based on the Sydney Ball Lecture, Oxford, November 1924, in *The collected writings of John Maynard Keynes*, ix: *Essays in persuasion* (London and Basingstoke, 1972), 272-94, p. 276.
184. Allen, *op. cit.* (ref. 5), 67, citing Haeckel, *op. cit.* (ref. 3), 137-8.
185. Huxley, *op. cit.* (ref. 8).
186. F. Darwin (ed.), *op. cit.* (ref. 7), i, 276. (This was subsequently republished in "Darwin's journal" (ref. 43), 7.)
187. This contained an 'expurgated' version of Darwin's *Autobiography*. For the full version, see N. Barlow (ed.), *The autobiography of Charles Darwin 1809-1882: With original omissions restored...* (London, 1958).
188. See refs 55 to 58.
189. Darwin excavated the giant South American mammalian fossils in 1832.
190. F. Darwin (ed.), *op. cit.* (ref. 15), xiv.
191. Judd, *op. cit.* (refs 16, 17 and 19).
192. F. Darwin, *op. cit.* (ref. 15), xvi.
193. F. Darwin (ed.), *op. cit.* (ref. 11), ii, 146.
194. Darwin himself spoke of "transmutation", not "evolution" and it is something of an anachronism to ascribe the term 'evolution' to him. The word 'evolution' has numerous connotations for us, notably the idea of progress, and to suppose that Darwin was, on the *Beagle* and through the years 1836-38, searching for an evolutionary theory, as understood today, can be misleading. In his early thinking about species change, he envisaged sudden alterations in time — something that is quite foreign to most modern 'evolutionary' theory. Darwin did not use the word 'evolution' in *The origin*, though Lyell did so in his *Principles* in relation to Lamarck's transmutation theory. For discussion of the etymology of the term 'evolution', see P. J. Bowler, "The changing meaning of 'evolution'", *Journal of the history*

of ideas, xxxvi (1975), 95-114.

195. Barlow, *op. cit.* (ref. 23).
196. *Ibid.*, 391. See also [C. R. Darwin], *op. cit.* (ref. 63), 262.
197. See refs 24 and 25.
198. W. Bagehot, *Physics and politics: or, thoughts on the application of the principle of "natural selection" and "inheritance" to political society* (London, 1872).
199. F. Engels, "Letter to P. L. Lavrov", 12 November 1875, in Marx and Engels, *op. cit.* (ref. 181), 283-5.
200. See refs 65, 77, 86 and 107. Writing in 1959 (ref. 46), the Ukrainian, E. M. Kondratiuk, maintained that the nineteenth century natural sciences developed as a result of the combined productive output of the advanced capitalist countries. This was associated with colonial expansion; and so Darwin's *Beagle* voyage, with its intellectual product, the transmutation theory, was intimately associated with capitalist expansion. However, S. I. Sobol (ref. 45), also writing in 1959, wished to deny that Malthus provided Darwin with anything of significance for the construction of his theory. Sir Gavin de Beer has pointed out that some Russians have been unwilling to credit the achievement of Darwin (of whom they approved) to Malthus (whom they regarded as a reactionary). See G. de Beer, "200 years of Malthus: The gloomy cleric's gift to Darwin", *New scientist*, xxix (1966), 423-4.
201. D. Bloor, *Knowledge and social imagery* (London, 1976).
202. Crombie, *op. cit.* (ref. 41).
203. Feibleman, *op. cit.* (ref. 42).
204. This had previously exercised the attention of C. Zirkle in his very thorough survey of possible precursors, published in 1941 (see ref. 26).
205. Eiseley, *op. cit.* (ref. 39).
206. Darlington, *op. cit.* (ref. 37).
207. H. Spencer, "Art. IV. — A theory of population, deduced from the general law of animal fertility", *Westminster review*, lvii (1852), 468-501. (This has sometimes been represented as an anticipation of Darwinian natural selection theory, but of limited scope, being propounded in terms of human population pressures. But Spencer seems to have been supposing that population pressure would be a stimulus to self-help, and so to intellectual progress. He envisaged an eventual cessation of the whole process, when man was fully adapted to his new civilized condition, and limited his own fertility.)
208. See H. L. McKinney, *Wallace and natural selection* (New Haven and London, 1972), 342.
209. L. C. Eiseley's several papers on this matter were published posthumously as *Darwin and the mysterious Mr. X: New light on the evolutionists* (New York, 1979).
210. See, for example, the review of J. S. Schwartz, *Isis*, lxxi (1980), 517; also refs 93 and 111.
211. Mayr, *op. cit.* (ref. 44).
212. See refs 126 and 172.
213. For example, by Fleeming Jenkin, in his celebrated review of *The origin*, which is known to have caused Darwin to think deeply about the matter. See [F. Jenkin], "The origin of species", *North British review*, xlvi (1867), 277-318.
214. D. L. Hull, "The metaphysics of evolution", *The British journal for the history of science*, iii (1967), 309-37; *idem*, *Darwin and his critics: The reception of Darwin's theory of evolution by the scientific community* (Cambridge, Mass., 1973), 67-77.
215. Hodge, *op. cit.* (ref. 174), 94.
216. Stauffer, *op. cit.* (ref. 54).
217. Vorzimmer, *op. cit.* (ref. 70).
218. Egerton, *op. cit.* (ref. 76).
219. I.e., the subsequently published material referred to in refs 55 to 58 above.
220. Smith, *op. cit.* (ref. 52).

221. De Beer, *op. cit.* (ref. 60). (De Beer had previously discussed the matter in a lecture on Darwin at the British Academy (ref. 34).)
222. As is well known, Darwin used to cut out the most important bits and use them for writing up his major published work.
223. The 'wedge' metaphor was of great importance to Darwin. It persisted until the sixth edition of *On the origin of species.* On the wedging metaphor, see Colp, *op. cit.* (ref. 138).
224. De Beer, *op. cit.* (ref. 66), 218.
225. De Beer, *op. cit.* (ref. 67), 308.
226. *Ibid.*, 309.
227. De Beer, *op. cit.* (ref. 66), 218.
228. De Beer, *op. cit.* (ref. 67), 321.
229. De Beer, *op. cit.* (ref. 60), 329.
230. J. M. Keynes, *The collected writings...*, x: *Essays in biography* (London, 1972), 100-1.
231. Eiseley, *op. cit.* (ref. 36), 183.
232. Théodoridès, *op. cit.* (ref. 68).
233. *Cf.* ref. 344.
234. Egerton, *op. cit.* (ref. 83).
235. S. F. Cannon, *Science in the early Victorian period* (New York, 1978), ch. 3.
236. Smith, *op. cit.* (ref. 83).
237. W. S. McLeay, *Horae entomologicae: or essays on the annulose animals* (London, 1819-21).
238. See ref. 344.
239. Vorzimmer, *op. cit.* (ref. 79).
240. Young, *op. cit.* (ref. 77).
241. Vorzimmer drew particular attention to a letter written to Otto Zacharias in 1877 in which Darwin stated that: "I did not become convinced that species were mutable until, I think, two or three years had elapsed [since the opening of the "Transmutation notebooks" in July, 1837]" (F. Darwin and Seward (eds), *op. cit.* (ref. 11), i, 367). But this letter, of Darwin's old age, is of doubtful historical significance. Maybe, at that late date, Darwin didn't regard himself as a 'convinced' transmutationist until his theory was well established on the principle of natural selection. That is to say, biological transmutation was not a 'fact' for Darwin until it was satisfactorily situated within a biological theory.
242. One must comment, however, that Darwin himself demonstrated no '*eureka*' effect in the (excized portions of the) "Transmutation notebooks". And it is now known that Darwin had a fair idea of the contents of Malthus's book before he read it. It was, after all, common knowledge in Darwin's day. However, this does not prove that Darwin received no essential stimulus from Malthus.
243. Young, *op. cit.* (ref. 77).
244. *Ibid.*, 130.
245. Young, *op. cit.* (ref. 96).
246. We have seen above that Engels, long before, saw a two-way traffic of ideas here between natural history and the social world.
247. Limoges, *op. cit.* (ref. 81).
248. *Ibid.*, 13.
249. Kohn, *op. cit.* (ref. 144). See also below.
250. Herbert, *op. cit.* (ref. 88).
251. Gale, *op. cit.* (ref. 95).
252. "Notebook A" is concerned with geological matters and at the time of writing (1983) is still unpublished, though its publication has been announced by Sandra Herbert.
253. Gruber and Barrett, *op. cit.* (ref. 112), 236-381.
254. *Ibid.*, 136. (The idea had some analogy to the doctrines of Lamarck. See D. R. Oldroyd,

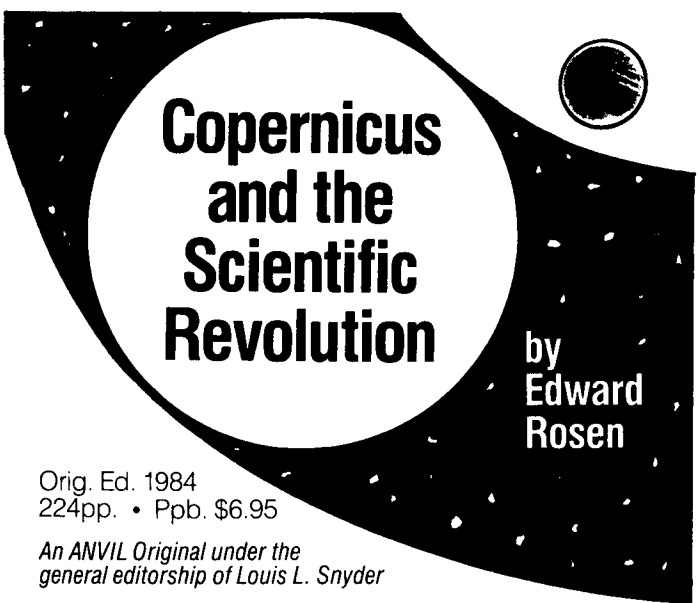
Darwinian impacts: An introduction to the Darwinian Revolution (Kensington, Milton Keynes and Atlantic Highlands, 1980), 34.)

255. Kohn, *op. cit.* (ref. 144). However, other commentators such as Kleiner (ref. 154) and Hodge (ref. 174) have found merit in Gruber's emphasis on the importance of Darwin's "monad theory".
256. Gruber and Barrett, *op. cit.* (ref. 112), 117.
257. Later investigators of Darwin's work on divergence have been inclined to see significant differences between the fully-fledged principle and the early idea of branching. See ref. 344.
258. Herbert, *op. cit.* (ref. 109).
259. This has now been published by Herbert. See ref. 143.
260. Herbert, *op. cit.* (ref. 143), 17.
261. See ref. 186.
262. Oldroyd, *op. cit.* (ref. 254), 45.
263. Grinnell, *op. cit.* (ref. 110).
264. Vorzimmer, *op. cit.* (ref. 113).
265. Kohn *et al.*, *op. cit.* (ref. 171).
266. Ruse, *op. cit.* (ref. 114).
267. See, for example, J. Losee, *An introduction to the philosophy of science* (London, 1972), 115-20.
268. J. F. W. Herschel, *Preliminary discourse on the study of natural philosophy* (London, 1830).
269. Ruse, *op. cit.* (ref. 114), 171.
270. Ruse, *op. cit.* (ref. 115).
271. Cannon, *op. cit.* (ref. 115).
272. Young, *op. cit.* (ref. 77).
273. Bowler, *op. cit.* (ref. 120).
274. Herbert, *op. cit.* (ref. 88), 217.
275. Neither Young nor Gale put the matter in quite such simplistic terms. It has, however, been a widely-held view amongst writers of the Left. For example, J. D. Bernal, in his *Science in history* (3rd edn, 1965), described Darwin's theory of natural selection as a "reflection of the free competition of the full capitalist era" (iv, 1233). As we saw above, such a view was proposed by Marx soon after the publication of *The origin*. It has attracted many adherents since.
276. Bowler, *op. cit.* (ref. 120), 649.
277. Gale, *op. cit.* (ref. 95), 338.
278. Manier, *op. cit.* (ref. 133), 82.
279. Herbert, *op. cit.* (ref. 129).
280. He did so more, however, in geology than in zoology. His professional standing was higher in geology. See also Rudwick, *op. cit.* (ref. 164).
281. [C. R. Darwin], *op. cit.* (ref. 55), 101.
282. For my didactic exposition of this, see my *op. cit.* (ref. 254), 184-5.
283. R. M. Young has regarded associationist psychology as an essential background assumption to the participants in the nineteenth century evolutionary debate. See R. M. Young, "The role of psychology in the nineteenth-century evolutionary debate", in M. Henle, J. Jaynes and J. J. Sullivan (eds), *Historical conceptions of psychology* (New York, 1973), 180-204.
284. Schweber, *op. cit.* (ref. 127).
285. Manier, *op. cit.* (ref. 133).
286. Gould, *op. cit.* (ref. 141).
287. Manier, *op. cit.* (ref. 133).
288. This included men such as Lamarck or Adam Smith who were dead at the time that Darwin was notetaking; and also men such as Lyell, who became Darwin's intimate friend.

289. F. Darwin (ed.), *op. cit.* (ref. 7), i, 69.
290. Manier, *op. cit.* (ref. 133), citing Wordsworth's *The excursion*, Book VII, ll. 999-1005.
291. Manier, *op. cit.* (ref. 133), 82-83.
292. Kottler, *op. cit.* (ref. 130).
293. Grinnell, *op. cit.* (ref. 110).
294. Kohn, *op. cit.* (ref. 144).
295. C. Lyell, *Principles of geology, being an attempt to explain the former changes of the Earth's surface by reference to causes now in operation...*, ii (London, 1832), 129. (While stating Brocchi's hypothesis, Lyell himself rejected it.)
296. [C. R. Darwin], *op. cit.* (ref. 55), 44, 46.
297. See above at ref. 250.
298. Sulloway, *op. cit.* (ref. 169). See also 162 and 170.
299. Barlow, *op. cit.* (ref. 23).
300. Herbert, *op. cit.* (ref. 109).
301. [C. R. Darwin], *op. cit.* (ref. 63), 262.
302. See Sulloway, *op. cit.* (ref. 162), and *op. cit.* (ref. 170).
303. Lack, *op. cit.* (ref. 30), 115. (However, Lack did state that "observations on ... [the finches] were obscured by his [Darwin's] unfortunate mixing of specimens...".)
304. Indeed, since some of the type specimens in the British Museum were given wrong Galapagos locations, there was at one time some argument amongst zoologists as to whether a little evolutionary change had occurred since Darwin's visit to the islands!
305. The career of Darwin, on his return to London, in relation to the city's scientific 'experts', is most ably represented graphically by Rudwick, *op. cit.* (ref. 164).
306. De Beer, *op. cit.* (ref. 43), 7.
307. When discussing the ostriches of the Pampas, see above.
308. Herbert, *op. cit.* (ref. 143), 66.
309. *Ibid.*, 7.
310. The significance of biogeography was reaffirmed by R. A. Richardson in his 1981 paper (*op. cit.*, ref. 155).
311. Hodge, *op. cit.* (ref. 174).
312. *Ibid.*, 5.
313. See also ref. 194.
314. See D. Ospovat, "Lyell's theory of climate", *Journal of the history of biology*, x (1977), 317-39.
315. This point had been adumbrated by Kohn, but without such detailed consideration of Darwin's field evidence.
316. This can happen where chimeras are formed between the host and the grafted plant tissue; and over several generations the host plant tissue may gradually 'swamp' that of the grafted buds.
317. Barlow (ed.), *op. cit.* (ref. 63), 262.
318. Hodge, *op. cit.* (ref. 174), 60.
319. *Ibid.*, 70. (Hodge, it should be noted, did not regard the opening of "Notebook B" — which is headed ZOONOMIA — as a "Transmutation notebook", but as a "Zoonomical sketch", i.e. one seeking to determine the "laws of life".)
320. [C. R. Darwin], *op. cit.* (ref. 55), 44.
321. *Ibid.*, 46.
322. McKinney, *op. cit.* (ref. 208), 141. The case against Darwin has been stated more forthrightly in A. C. Brackman, *A delicate arrangement: The strange case of Charles Darwin and Alfred Wallace* (New York, 1980). But Brackman's case seems to have been effectively demolished. Kohn, *op. cit.* (ref. 159).
323. F. Darwin (ed.), *op. cit.* (ref. 7), i, 84.

324. Ospovat, *op. cit.* (ref. 160).
325. See R. Cook, *The tree of life: Symbol of the centre* (London, 1974).
326. [R. Chambers], *Vestiges of the natural history of creation* (London, 1844).
327. C. R. Darwin, *The variation of animals and plants under domestication* (2 vols, London, 1868), ii, 75-80.
328. R. C. Stauffer (ed.), *Charles Darwin's Natural selection, being the second part of his big species book written from 1856 to 1858* (Cambridge, 1975), 233-4.
329. Ospovat, *op. cit.* (ref. 160), 176 (citing a manuscript from the Cambridge Darwin archives).
330. It has recently been shown (Parshall, *op. cit.* (ref. 166)) that the data that Darwin collected did not provide a satisfactory warrant for his conclusions. But he *thought* that they did.
331. Stauffer (ed.), *op. cit.* (ref. 328), 229.
332. Ospovat, *op. cit.* (ref. 160), 180-1.
333. *Ibid.*, 181.
334. Stauffer (ed.), *op. cit.* (ref. 326), 233.
335. Schweber, *op. cit.* (ref. 149).
336. H. Milne Edwards, *Introduction à la zoologie générale* (Paris, 1851).
337. Schweber, *op. cit.* (ref. 149), 250-1, translating from the article "Organisation" in Milne Edwards's *Dictionnaire.*
338. J. R. McCulloch, *The principles of political economy with a sketch of the rise and progress of the science* (2nd edn, London, 1830).
339. J. C. L. de Sismondi, *Political economy and the philosophy of government* (London, 1847).
340. Schweber, *op. cit.* (ref. 149), 286.
341. *Ibid.* See also A. P. de Candolle "Géographie botanique", *Dictionnaire des sciences naturelles*, xviii (Paris, 1820), 359-422, p. 384.
342. Ospovat, *op. cit.* (ref. 160), 267. (Ospovat made the same comment in relation to the work of Limoges, who had also written on the relationship between Darwin's thought and that of Milne Edwards.)
343. J. H. Poincaré, *Science and method* (New York, n.d.), 46-63.
344. This paper has concerned itself with the secondary literature to 1982. But we may notice here an important subsequent publication: J. Browne, *The secular ark: Studies in the history of biogeography* (New Haven and London, 1983). Following Ospovat, Browne has argued that a substantial change occurred in Darwin's theory between the *Essay* of 1844 and the *Origin.* In the earlier text, Darwin supposed that variation was largely a *product* of changing environmental circumstances, which, according to Lyellian geology, could also supposedly account for geographical distributions through hypothetical land bridges, etc. But his lengthy study of barnacles convinced him of the widespread occurrence of intraspecies variation. Responding to this, he engaged in his studies on "botanical arithmetic", which led to the "principle of divergence proper". This would give a plausible account of the significance of the barnacle work for Darwin's thinking. His later theorizing and experimentation involved consideration of mechanisms for the dispersal of living organisms other than along pathways provided by land bridges, etc. Browne has emphasized that from the first construction of his theory Wallace always assumed the ubiquity of spontaneous variation. It was only brought home to Darwin by his barnacle work. (Earlier, Ghiselin (*op. cit.* (ref. 80), ch. 5) laid emphasis on Darwin testing his 'evolutionary' theory by means of the barnacle work. The observed gradations of form would certainly have offered evidence favourable to the theory.)
345. See ref. 241.
346. F. Darwin, *op. cit.* (ref. 7), i, 83.
347. Tallmage, *op. cit.* (ref. 148).
348. C. R. Darwin, *op. cit.* (ref. 2), 9-10.

349. Kohn, *op. cit.* (ref. 144), 149-54.
350. See, for example, refs 104 and 154.
351. On this, see the remarks of Hodge, *op. cit.* (ref. 174), 94.
352. See, for example, Kohn *et al.*, *op. cit.* (ref. 171).
353. *British Museum (Natural History) book catalogue 1984* (London, n.d.), 18-19. (The editors are announced as: P. H. Barrett, P. J. Gautrey, S. Herbert, D. Kohn, and S. Smith.)
354. See Herbert, *op. cit.* (ref. 143), 29. (The editors are stated to be: F. Burkhardt, S. Smith, D. Kohn, and W. Montgomery.)
355. E. Richards, "Darwin and the descent of woman", in D. Oldroyd and I. Langham (eds), *The wider domain of evolutionary thought* (Dordrecht, London and Boston, 1983), 57-111.
356. See R. Colp, *To be an invalid: The illness of Charles Darwin* (Chicago and London, 1977).
357. J. Franzosa, "Darwin and Melville: Why a tortoise?", *American imago*, xxxiii (1976), 361-79.
358. D. Kohn (ed.), *The Darwinian heritage* (forthcoming, Princeton University Press).
359. E. H. Carr, *What is history?* (Harmondsworth, 1964), 30.



The wisest philosophers and the ablest astronomers agreed with the age-old belief that the earth was motionless. But the competing astronomical systems based on a stationary earth had to resort to tricky complications. The desire to preserve the basic principle of uniform motion impelled Copernicus to proclaim the earth's true cosmic status as a natural satellite of the sun.

This book presents concisely and fascinatingly the author's intensive investigations of everything that Copernicus wrote and was written about him from his own time to ours. This volume is a fresh view of Copernicus in relation to the Scientific Revolution which since his time has grown to dominate the world in which we live.

KRIEGER PUBLISHING CO., INC.
P.O. Box 9542, Melbourne, FL 32902-9542 • U.S.A.
When ordering, please add $3.00 for first book ($1.00 each additional) to cover shipping.