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Increasingly often in recent critiques of books and articles in the history of science an author is accused of having written whig history. Sometimes specific items are mentioned that are said to justify this label, but sometimes it would seem simply to document that the critic does not like the writings of the author. No two people seem to use the term in exactly the same sense, nor would any two historians entirely agree exactly what is whiggish. "One person's whig history is another's revisionism."[1] As the label whiggish was more and more frequently applied, some historians became so fearful of the whig epithet that they decided it was better not to make any interpretation or evaluation of the past than to be called a whig. This meant, of course, as has been claimed, a return to "the Baconian inductive method, which attempts to investigate phenomena with an observant but empty mind."[2] With that method historiography would become a deadly, purely descriptive exercise of reporting facts. Furthermore, the use of the term whiggish has brought such an unpleasant tone into many book reviews that a critical analysis of its meaning and justification would seem appropriate.

The expression "whig interpretation of history" was proposed by the historian H. Butterfield to characterize the habit of some English constitutional historians of seeing their subject as a progressive broadening of human rights, in which good, "forward-looking" liberals were continuously struggling with the backward-looking conservatives.[3] More broadly, the term was applied by Butterfield to any interpretation of history that "studies the past with reference to the present." In that sense, it was applied by him later to the kind of history of science in which past science and scientists were judged in the light of modern

[1] Peter Bowler, "The Whig Interpretation of Geology," *Biology and Philosophy*, 3 (1988), 100.

[2] E. Harrison, "Whigs, Prigs and Historians of Science," *Nature*, 329 (1987), 213-14.

[3] H. Butterfield, *The Whig Interpretation of History* (London, 1931).

knowledge.[4] A consideration of the present in studies of the past has also been called "presentism."[5]

My own conclusion is that Butterfield was ill advised in his literal transfer of the whig label from political history to history of science. It was based on the erroneous assumption that a sequence of theory changes in science is of the same nature as a sequence of political changes. Actually the two kinds of changes are in many respects very different from each other. In political changes succeeding governments often have diametrically opposed objectives and ideologies, while in a succession of theories dealing with the same scientific problem each step benefits from the new insights acquired by the preceding step and builds on it. Galileo, indeed, had a superior understanding of physics than the Greeks, Newton than Galileo, and Einstein and modern physicists than Newton. The same is true for the sequence Linnaeus-Lamarck-Darwin-modern evolutionary biology or, for that matter, for any historical sequence of scientific theories. For this reason the historiography of science proceeds by necessity in many respects very differently from political historiography. This is most clearly recognized by those who write developmental history of science.

In his listing of the errors of whiggish historiography Butterfield presents a most heterogeneous assortment. There are some (e.g., selectiveness, teleological assumptions) to which the label whiggish might be applied specifically (see below). Others are well known faults of historiography against which good historians have warned from times immemorial.[6] Bias against some theory or author is the worst and most common fault. The two best known earlier histories of biology, those of Radl (1907) and Nordenskiöld (1928), were both so biased against Darwinism that neither author even tried to give an adequate account. Darwinians, before the last twenty-five years or so, behaved on the whole not much better in their reporting on Lamarck. The evaluation of the biometricians by the Mendelians is another example. The histories of the post-Darwinian conflict between science and religion by Draper and White were thoroughly biased. Chauvinism either by nationality or field is a particular form of such bias. In the history of geology the work of Continental geologists was neglected or even maligned by British authors in order to exaggerate the merits of Hutton and Lyell in the development of geological thought.[7] Early accounts of the evolutionary synthesis, in which all the credit for the synthesis was given to genetics, are other examples. Several historians were biased against Cuvier and Richard Owen because they disliked them as persons. Priorities are often neglected or falsified, either for chauvinistic reasons or simply owing to ignorance. I remember a Cold Spring Harbor conference in the 1950s when a young geneticist proudly claimed that genetics had brought population thinking into systematics, when a glance at the literature could have shown him easily that the reverse direction is historical reality. By suppressing the contributions of

[4] H. Butterfield, *The Origins of Modern Science* (New York, 1957).

[5] G. W. Stocking, Jr., "On the limits of 'presentism' and 'historicism' in the history of the behavioral sciences," *Race, Culture, and Evolution* (New York, 1968), 1-12; D. L. Hull, "In defense of presentism," *History and Theory*, 18 (1979), 1-15.

[6] Hull, see note 5 above.

[7] M. Greene, *Geology in the Nineteenth Century* (Ithaca, N. Y., 1983).

earlier scientists or schools a historian may completely falsify the actual course by which a given field (and its concepts) developed.

Where Butterfield's Whig label is perhaps justified is where modern hindsight is used to make unfair value judgments about earlier authors. Any author must be evaluated in terms of the intellectual milieu of his time. For instance, there is no justification for criticizing Lamarck for having accepted the inheritance of acquired characters. What else could he have done when everybody else in his time accepted this mode of inheritance, a theory that was not seriously challenged until 1883? It is equally whiggish to criticize Darwin for having accepted pangenesis, which at that time was the only feasible explanation for the effects of use and disuse which at that time were generally accepted as an established fact. Obviously, then, it is entirely wrong to assume that all retrospective history has to be done in a whiggish manner.

The worst cases of bias are those where a historian completely falsifies the past. This may be illustrated by the claim of a cladist that the word phylogeny meant for Haeckel exclusively the branching pattern of the phylogeny, when Haeckel in fact had expressly included in his definition all other aspects of phylogeny (degree of divergence, etc.).

Whenever there is a scientific controversy, the views of the losing side are almost invariably later misrepresented by the victors. Examples are the treatment of Buffon by the Linnaeans, of Lamarck by the Cuvierians, of Linnaeus by the Darwinians . . . and so forth. . . . Almost always those who held an erroneous theory had seemingly valid reasons for doing so. They were trying to emphasize something that was neglected by their opponents. The preformationists, for instance, attempted to stress something which was later resurrected as the genetic program. The biometricians upheld Darwin's views of gradual evolution against the saltationism of the Mendelians.[8]

To omit correct components of an otherwise erroneous theory is a falsification of history.

In former years the history of science was mostly studied by scientists, particularly senior scientists. There was no such thing as a profession of history of science. The situation is now drastically different. "In the past twenty-five years or so, study of the history of science has changed out of all recognition. In a word it has been 'professionalized.' "[9] Most of these professional historians of science have received their training in the humanities or social sciences. Some recent historians of science have adopted the classical attitude expressed in the well known saying of Leopold von Ranke that the historian should "show how it really has been." Such history-writing is strictly descriptive, avoiding all value judgments and to a large part even comparisons. It assumes that there is only one legitimate way of doing historiography, that of describing each period in great detail. This should include a full account of all competing theories, no matter how irrelevant they were to the subsequent history of the field. It carefully describes the entire milieu of the period including the social and economic situation. In other words, such historians attempt to present "the total picture".

[8] Ernst Mayr, *The Growth of Biological Thought* (Cambridge, Mass., 1982), 11, 12.
[9] C. Russell, "Whigs and Professionals," *Nature*, 308 (1984), 777-78.

They attempt to justify their love for detail by claiming that the conscientious recording of "family and social lives, travels, accomplishments, publications and awards ... are the meat of historical explanation."[10] As one critic of this descriptive type of history writing has complained (with respect to a recent history of geology), "not a hint of contemporary relevance intrudes into the historical narrative as ... the geology of the 1830s is put under a 'historical microscope.' If you stay just at the level of crude phenomenological description, you have practically built in an anti-theoretical bent to your narrative." And one can describe the ideology of such historians of science by saying that it consists of "the dogma that one must eschew entirely anything with a hint of whiggish theorizing, or even any kind of seeing of the past through the eyes of the present. There is an overriding insistence that one must stay as close as possible to the documents."[11]

It is curious that this approach should have been revived in the field of the history of science when in political historiography Ranke's recommendation of simply describing everything as it had been has long since been abandoned as the ideal of history writing.

Most scientists have had considerable interest in the history of science. This is not surprising, because "science without its history is like a man without a memory."[9] The interest of the scientist, however, is quite specific and in many respects different from that of the historian trained in the humanities. The foremost interest of the modern scientist-historiographer is the development of ideas, from their origin through all their permutations up to the present day. The reason for this interest is that it is impossible to understand many of the current controversies and prevailing concepts without studying their history. The recent histories by Stresemann, Lenoir, M. Greene, and R. Laudan are splendid examples of this genre of historiography.[12] W. and M. Kneale in their developmental history of logic state "our primary purpose has been to record the first appearances of those ideas which seem to us most important in the logic of our own day."[13] In the preface of my *Growth of Biological Thought* I stated clearly: "This volume is not, and this must be stressed, a history of biology.... [T]he emphasis is on the background and the development of the ideas dominating modern biology; in other words, it is a developmental, not a purely descriptive, history. Such a treatment justifies, indeed necessitates, the neglect of certain temporary developments in biology that left no impact on the subsequent history of ideas." Sloan, a professional historian, understood this fully. For a developmental historian, he says, "the history of science functions primarily as a tool for concept analysis and clarification.... The aim is not historical completeness, but conceptual clarification." He concludes that such

[10] S. C. McCluskey, "Historians, Whigs, and Progress," *Nature*, 330 (1987), 598.

[11] M. Ruse, Booknotes, *Biology and Philosophy*, 2 (1987), 377-81.

[12] E. Stresemann, *Ornithology: From Aristotle to the Present* (Cambridge, Mass., 1975); T. Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology* (Dordrecht, 1982); R. Laudan, *From Mineralogy to Geology: The Foundations of a Science, 1650-1830* (Chicago, 1987); M. Greene, see note 7 above.

[13] W. Kneale and M. Neale, *The Development of Logic* (Oxford, 1982).

an approach "is neither whig history in the sense intended by Butterfield . . . nor is it illegitimate."[14]

What a scientist is most interested in when doing historical studies is to illuminate or reconstruct the pathway of the currently prevailing ideas of science. This includes not only the origin of each new idea (background, causal contributions) but also a study of all subsequent modifications as well as the determination of who was responsible for them. If geology is sometimes described as "the study of the present in order to reconstruct the past," then developmental historiography of science can be described as "the study of those aspects of the past that help our understanding of the science of the present." Ideally, what a scientist would like to do for every concept in science is what Lovejoy did for the concepts of plenitude and the Great Chain of Being. His approach was guided by a strong emphasis on the vertical component of history.[15]

An emphasis on this component, however, does not mean that the historian's account has to become finalistic. Butterfield quite rightly criticizes the tendency among political historians of the nineteenth century to describe the "present as the inevitable outcome of a triumphant historical process" or "the tendency . . . to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the past." This approach to historiography is, of course, an application of transformational evolutionism to history, combined with a strong belief in teleology. Scientific progress, by contrast, is obeying the principles of Darwinian variational evolution which has no teleological components. For a scientist who adheres to Darwinism there is nothing wrong or unscientific in following the evolution, and usually progress, of a scientific idea. Those who object to this procedure do so because they do not understand the interplay between variation and selection, which is as active in the history of ideas as it is in organic nature. This has nothing to do with teleology or a naive belief in an intrinsic drive toward progress. Inevitably it includes a treatment of false starts and of competing theories. Yet it does not necessitate exploring every long forgotten blind alley in the development of science. However, it must make use of our modern understanding of particular scientific concepts or problems in order to be able to explain the reasons for the difficulties of former periods.

Two major criticisms have been raised by the anti-whigs against developmental historiography. One was stated by Butterfield in the words "it is part and parcel of the whig interpretation of history that it studies the past with reference to the present," as if there was anything wrong with this. By considering such an approach as objectionable, Butterfield demonstrated that he did not understand the objectives of developmental history. Developmental history is impossible (and would be utterly vacuous) if retrospection were not done. Obscure former controversies simply cannot be fully understood without the superior modern insights into the problems. As Hull has said rightly, if we are not prepared to interpret the past in terms of the present, why should we care about the past?[16] Admittedly, to write interpretive history is a far more demanding task than to write a purely descriptive one. It requires a careful study

[14] P. R. Sloan, Essay review, *Journal of the History of Biology*, 18 (1985), 145-53.

[15] A. O. Lovejoy, *The Great Chain of Being* (Cambridge, Mass., 1936).

[16] See note 5 above.

of the succession of Zeitgeists throughout the history of science, or at least throughout the historic period dealt with by the author. It requires a considerable amount of knowledge of philosophy, as Ruse has quite correctly pointed out. It requires an analysis of that part of the intellectual and ideological environment of each period that had an impact on the development of scientific thinking. To excuse oneself from undertaking such an interpretative analysis merely by saying that such a treatment would be whiggish will find little credibility.

It is necessary to reject specifically certain criticisms by showing that developmental historiography is definitely not whiggish, opposing claims of the anti-whigs notwithstanding. One cannot do developmental history if one is not guided in the study of the past by an understanding of the present. Historiography is based on observation, and its most productive method is comparison, just as it is in all observational sciences. It is not in the least objectionable when Lamarck's framework of concepts is compared to that of modern evolutionary biology, when it is pointed out where it differs from the current views and what particular commitments were responsible for the failure of Lamarck's evolutionary paradigm.

There is a curious misconception among the anti-whigs that a retrospective analysis is incompatible with a study of each time period on its own merits. Consequently, they praise "the superiority of an approach which attempts to reconstruct in all its aspects the problems faced by earlier thinkers *rather than* [italics mine] judging the past with the benefit of hindsight."[17] As if it were a question of either-or. Good developmental history always deals with both aspects.

Furthermore, there is no reason why errors of earlier authors should not be pointed out. This has nothing to do with whiggishness because contemporary authors also criticize each other when in disagreement. Criticism and the endeavor to find errors is the soul of good science. Why should it become whiggish when a historian makes the same critical points as the contemporaries?

A major error of the anti-whigs is to reject selectiveness. Historians of ideas must be selective and always have been so. Lovejoy in his superb *The Great Chain of Being* provides a splendid illustration. In each period he singled out only those developments that had a bearing on the great idea of the *Scala Naturae*. He omitted everything else at that time level. He would have had to fill numerous volumes if he had not been selective when following up the basic theme of his work from the ancients to the end of the eighteenth century. He never hesitated to break complex systems "into their component elements, into what may be called their unit ideas," select those he needed for his story, and discard the others. He was selective, but neither biased nor finalistic. On the basis of the criteria of some recent anti-whigs Lovejoy was a super-whig owing to this selectiveness.

A scientist when doing developmental history follows Lovejoy's example by tracing an idea or controversy back to its sources without being deflected by nonessentials. Yet, while concentrating on the concept or argument in which he is interested, the scientist devotes sufficient attention to the context that is needed to cast additional light. But there is nothing wrong with omitting something that is irrelevant. The anti-whigs have so far failed to produce a single

[17] C. B. Wilde, "Whig history," W. F. Bynum, E. J. Browne, R. Porter (eds.), *Dictionary of the History of Science* (1981), 445-46.

case where such a neglect of a blind alley had led to a misinterpretation. I share Ruse's disdain for the wastefulness of chasing after irrelevance.[18] It is ludicrous to blame it on whiggishness that "the amount of literature on Newton and Darwin ... is vastly greater than that on their opponents."[19] There is no justification whatsoever for giving as much space to the anti-evolutionary arguments of a Koelliker, Agassiz, or von Baer as to Darwin. It is simply that it was Newton and Darwin who had the decisive impact on the subsequent development of their sciences and therefore deserve all the attention which they receive.

Let me illustrate with a few examples how unfair much of the criticism of the anti-whigs is. Being best acquainted with the subject matter, I take some cases of my own writings. Bowler accuses me of whiggishness by having "ignored [in my *Growth of Biological Thought*] the extensive network of non-Darwinian thinking in the late nineteenth century."[20] Actually I devoted almost fifty pages to it in which I also cited much of the enormous secondary literature (e. g. Kellogg) dealing with these developments.[21] The effect of orthogenesis, saltationism, and Lamarckism on the development of our modern thinking was primarily to delay its acceptance. So far as I know, it had little constructive impact. To be sure, the anti-Darwinian theories had an overwhelming influence on cultural anthropology, sociology, psychology, and the humanities, as Bowler points out correctly. However, it is not necessarily the task of developmental historiography to study such transferences.[22] In another example, Bynum accuses me of "whiggishness" because I "dismissed" Robert Chambers as "an ignorant layperson."[23] Is this evaluation justified? Let us look at this case more closely. In order to be fair to Chambers I had gone so far as buying my own copy of the *Vestiges*. I studied the work very carefully and devoted four full pages to it in my *Growth of Biological Thought*, surely major attention in a volume dealing with all the history of ideas in all nonfunctional biology from the Greeks to the present. I gave Chambers a most sympathetic hearing, pointing out that he "displays an amount of common sense in his consideration of the evidence that is sadly lacking in the writings of the contemporary antievolutionists." I pointed out that "it was he who saw the forest where all the great British scientists of the period (except for the nonpublishing Darwin) only saw the trees." I finally concluded that it had been so easy for his opponents to demolish Chambers because he made so many horrendous factual errors, not surprisingly so because, after all, he was not a professional but "an ignorant layperson." But how can one say I "dismiss" an author to whom I had paid so much attention and whom I had treated with such sympathy? It is this irresponsible use of the term whiggish

[18] See note 11 above.

[19] See note 17 above.

[20] P. J. Bowler, *The Non-Darwinian Revolution* (Baltimore, 1988).

[21] V. L. Kellogg, *Darwinism Today* (New York, 1907); Mayr, see note 8 above, 501-50.

[22] Ernst Mayr, "The Myth of the non-Darwinian Revolution," *Biology and Philosophy*, 5 (1990), 85-92.

[23] W. F. Bynum, "On the Written Authority of Ernst Mayr," *Nature*, 317 (1985), 585.

by the opponents of developmental history, which has nearly destroyed the usefulness of the term.

The two other manifestations of whiggishness Bynum ascribes to me are my reputed failure to give sufficient credit to the achievements of Erasmus Darwin and Richard Owen. Erasmus Darwin is a highly controversial figure whose stature is still uncertain, even after the recent work of A. J. Cain and M. McNeil. Evaluations have ranged from the one extreme of considering him "just a medical crackpot, given to writing tediously long poems in ludicrously bad verse, noteworthy only because of his grandson's fame"[24] to rating him a major pioneer of evolutionism. I actually analyzed him rather carefully (I own a copy of *Zoonomia*) but finally concluded that "there is no justification for a detailed presentation of his thought [for reasons stated there] . . . [and because it] had remarkably little impact on subsequent developments."[25] The situation is different with Richard Owen who is indeed an important figure, more so than acknowledged in most historical accounts. I gave only a short abstract of his contributions because I was planning a full treatment of Richard Owen in the story of the history of morphology in Vol. II of my *Growth of Biological Thought* (never completed).

Popper aptly described the pathway of scientific progress as conjectures and refutations. At any particular period of time there are frequently a number of competing conjectures concerning some unsolved problem. Usually one of these conjectures leads to the next step in our understanding, while the others are refuted or at least only poorly supported. It is only common sense for a historian to devote most attention to that particular conjecture that turned out to have had the greatest "fitness," that is, which had the greatest subsequent impact.

Finally, developmental history must not only be comparative and selective, it must also be historical.[26] Strictly horizontal historiography, which reports in loving detail the happenings of only a single moment of time, is singularly unrevealing. It fails to communicate the spirit of searching and experimenting that is such a characteristic element of science.

The accusation that developmental history ignores everything but the main line is demonstrably unfair. In all good developmental histories known to me I find an adequate treatment of the intellectual and cultural context. "Failed" scientists are always treated appropriately, even though not in anywhere near the same detail as those of their contemporaries who contributed significantly to the subsequent development of their field.

What can we consider as the outcome of this analysis? With Ruse I conclude that it is by no means wrong to look at the past on the basis of an understanding of the present.[27] As Hull has said so rightly "a knowledge of the present is absolutely crucial for the historian. . . . From his position in the present the historian must use all evidence and tools available to him in reconstructing the past, even if this knowledge was unavailable to the people in the period under

[24] *Fide* M. Ruse, "Booknotes," *Biology and Philosophy*, 3 (1988), 404.

[25] See note 8 above.

[26] D. R. Oldroyd, "Historicism and the Rise of Historical Geology," *History of Science*, 17 (1979), 191-213, 227-57.

[27] See note 11 above.

investigation."[28] To be sure, the historian must avoid the well known faults of bias, chauvinism, falsifications of priority, and finalistic interpretations, but this is true for any kind of historiography, developmental or not. On the other hand, selectivity is a necessity in developmental historiography. Also, the historian must be permitted to make evaluations when writing intellectual history, as Lovejoy has demonstrated so beautifully. A history that does not evaluate but merely records facts and presents documents is anti-intellectual—it is prig-history, as Harrison has called it.[29] Finally, I feel the pejorative label "whiggishness" has been used increasingly in such an irresponsible and often completely unjustified manner, that one might want to hope that it will disappear altogether from the literature of scientific historiography. If used at all, it should be applied only to genuine cases of whiggishness and not to developmental historiography.[30]

Museum of Comparative Zoology, Harvard University.

[28] See note 5 above.

[29] See note 2 above.

[30] I thank Robert K. Merton, I. Bernard Cohen, and Frank J. Sulloway for some very useful constructive comments on an earlier version.

[Footnotes]

[5] **In Defense of Presentism**
David L. Hull
History and Theory, Vol. 18, No. 1. (Feb., 1979), pp. 1-15.
Stable URL:
http://links.jstor.org/sici?sici=0018-2656%28197902%2918%3A1%3C1%3AIDOP%3E2.0.CO%3B2-U