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IN DEFENSE OF PRESENTISM

DAVID L. HULL

L. Pearce Williams was so irate over Joseph Agassi's book on Michael Faraday and the section on Faraday in a book by William Berkson that he was moved to entitle his review "Should Philosophers Be Allowed to Write History?"¹ Williams answers the question posed in the title of his review with a resounding "NO!" He complains that philosophers are prone to scandalous carelessness in transcribing quotations and to inaccurate descriptions, not to mention some highly questionable interpretations. They are more interested in plausible connections between ideas than in actual connections, in what *they* would have thought in the circumstances rather than in what the people concerned *actually* thought. But worst of all, philosophers tend to use history of science to illustrate their own views on the nature of science, rather than treating it inductively. Popperians such as Agassi and Berkson view histories of science as good places to introduce conjectures to be refuted by later workers. Imre Lakatos has even gone so far as to state that history of science should be written as it *should* have taken place, given a particular philosophy of science, rather than as it actually did take place: "One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the footnotes* how actual history 'misbehaved' in the light of its rational reconstruction."²

Once Lakatos' position is translated out of the controversial idiom which he invariably prefers, it does not sound so radical and ahistorical. Even so, Williams would surely object. "History," according to Williams, "is an inductive science."³ I happen to agree with most of Williams' historiographical preferences. I too value accurate quotations, citations, and de-

1. L. Pearce Williams, "Should Philosophers Be Allowed to Write History?", *The British Journal for the Philosophy of Science* 26 (1975), 241-253; Joseph Agassi, *Faraday as a Natural Philosopher* (Chicago and London, 1972); William Berkson, *Fields of Force* (London, 1974); Williams has since tempered his condemnation of Berkson but not Agassi, "Reply to Agassi and Berkson," *The British Journal for the Philosophy of Science* 29 (1978), 252.

2. Imre Lakatos, "History of Science and Its Rational Reconstruction" in *PSA 1970*, ed. R. Buck and R. Cohen (Dordrecht, Holland, 1971), 107.

3. Williams, 253.

scriptions even when I myself fall short. However, an inductivist philosophy of history is no less a philosophy of history because it is inductivist and widely shared by other historians. Williams is caught up in the sort of self-referential snarl so dear to the hearts of philosophers. It is unlikely that Williams himself gathered his principles of historiography inductively from an extensive examination of past histories of science; rather he brings his preferences to his study of past science. His historical work will be influenced as surely by his principles of good historiography as the works of the Popperians are by theirs. Williams may have no general philosophy of science. He certainly does not recommend an inductivist philosophy to physicists and biologists. But he does hold an inductivist philosophy of history.

Should philosophers be allowed to write history? Should *historians*? In spite of excesses on both sides, I am forced to answer both questions with a resounding "YES!" If the philosophical views which an historian holds have any influence on the history he writes, and I cannot see how such influence can be avoided, then philosophers have something to contribute to history. Conversely, if the subject matter of philosophy of science is science, then historians surely have much to contribute to philosophy of science. Furthermore, for any philosopher attempting to develop an evolutionary analysis of science, the temporal dimension to the units of conceptual evolution becomes crucial. Historians have more to contribute to philosophy than just examples. In conceptual evolution, actual connections between ideas are what count, not "logical" connections.⁴ In general, I find the academic divisions characteristic of universities all too real. Scientists, historians, sociologists, and even philosophers help to increase our understanding of science. Each of these fields has its own goals, techniques, and standards. Some consideration must be shown by people trying to work in more than one field to the indigenous mores of the different disciplines. But sympathy is also called for. "Allowing" or not "allowing" someone to write history is incompatible with free inquiry.

I. PRESENTISM

Present-day historians frequently criticize their predecessors for a variety of poor historiographic practices, especially for a cluster of interrelated faults commonly labeled "presentism." In an early criticism of what he termed the Whig interpretation of history, Herbert Butterfield complained of "the tendency in many historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story

4. Karl Popper, *Objective Knowledge* (Oxford, 1972); Stephen Toulmin, *Human Understanding* (Princeton, N.J., 1972); Larry Laudan, *Progress and Its Problems* (Berkeley, 1977).

which is the ratification if not the glorification of the present.”⁵ Comparable observations apply to history of science. For example, Stephen G. Brush points out the tendency of early historians of science to “judge every scientist by the extent of his contribution toward the establishment of modern theories. Such an interpretation looks at the past in terms of present ideas and values, rather than trying to understand the complete context of problems and preconceptions with which the earlier scientist himself had to work.”⁶

In this paper I intend to argue that certain forms of presentism are both undesirable and eliminable. It is certainly a mistake to think that by “gemmule” Darwin meant the same thing that present-day geneticists mean by “gene” or to criticize him for not holding the modern conception. Nor could anything introduce greater distortion into an historical narrative than assuming that the agents concerned viewed the empirical world the way we do today. However, I also intend to defend certain forms of presentism in history of science as necessary evils and still others as perfectly legitimate. I realize that my defending presentism in any form is likely to prove as popular among historians as my saying a few words at a meeting of the Parent-Teacher Association on behalf of child molesters, but I think that the faults of presentism are not as transparent as they might at first appear.

For example, one of Thomas Kuhn’s maxims for the “new internal historiography” is that the “historian should set aside the science that he knows. His science should be learned from the textbooks and journals of the period he studies, and he should master these and the indigenous traditions they display before grappling with innovators whose discoveries or inventions changed the direction of scientific advance.”⁷ However, Kuhn prefaces the preceding maxim with the remark that the historian should set aside the science that he knows only “insofar as possible” and “it is never entirely so, nor could history be written if it were.”⁸ Similarly, Murray G. Murphey notes that historians “are now calling for a historicist approach which will seek to understand the past in its own terms, not in terms of its relation to later events which happen to interest certain investigators.”⁹ He then asks, “But what does it mean to understand the past in its own terms? It means, I think, to seek for an understanding of past phenomena in terms of the system of thought and action of which they were a part. Of course, this system is in part our construction, as is any theory, and it must usually

5. Herbert Butterfield, *The Whig Interpretation of History* (London, 1931), v.

6. Stephen G. Brush, “Should the History of Science Be Rated X?”, *Science* 183 (1974), 1169.

7. Thomas Kuhn, “The History of Science,” *International Encyclopedia of the Social Sciences* 14 (1968), 76.

8. *Idem.*

9. Murray G. Murphey, *Our Knowledge of the Historical Past* (Indianapolis and New York, 1973), 120.

contain recently discovered principles of which the members of past societies were unaware.”¹⁰

If an historian knows no science of his own period, then he certainly cannot read it back into the past or pay undue attention to precursors. In practice, of course, such abject ignorance of the present is impossible, even for historians and philosophers of science, but what of Kuhn’s remark that history could not be written if it were and Murphey’s claim that understanding the past in its own terms is compatible with referring to principles unknown at the time? Practicing historians might well reply that historiographers like Kuhn and Murphey are manufacturing problems where none exist. Historians know what they mean when they claim to write of the past in its own terms even if historiographers do not. On occasion an historian might slip, allowing his narrative to be distorted by his own perspective, but the goal is clear.

I am of two minds on this issue. Many of the points which I make in this paper may seem too obvious to warrant mentioning. One way to evaluate a philosophical exposition is the extent to which it accords with the practices and intuitions of those actually working in the field under investigation. The more frequently my reader exclaims, “Of course, everyone knows that!” the better I like it. But philosophy is also prescriptive. On occasion the reader is likely to be moved to exclaim, “Rubbish! Only a philosopher could talk such nonsense.” In such circumstances, the only thing that a philosopher can do is to explain his position as best he can and to show how, when one thinks about it, it makes admirably good sense. In this paper I discuss three sorts of presentism, reading present-day meanings, principles of reasoning, and empirical knowledge back into earlier periods. I argue that in all three cases knowledge of present-day language, logic, and science is necessary not only for investigating the past but also for communicating the results of these investigations to the historian’s contemporaries. I also limit myself to presentism in the history of science. Whether comparable observations apply with equal force to history at large must remain, for the purpose of this paper, a moot question. However, I happen to think that they do.

II. RECONSTRUCTION VERSUS COMMUNICATION

A distinction which should prove helpful in the succeeding sections of this paper is between the methods which historians use to find out about the past and the means which they use to communicate these findings to their contemporaries. In rare cases the historian can use current scientific theories to infer the occurrence of a past event; for instance, the dating of a

10. *Idem.*

document which mentions a lunar eclipse of the sun by means of current data and the principles of celestial mechanics. In doing so, he is hardly contravening any principles of historiography. He is not claiming that the people at the time understood eclipses the way we do or possessed our principles of celestial mechanics. He is simply using the science available to him to infer the past. The historian is lodged squarely in the present. He must use the theories, methods, and data available to him in reconstructing the past or use nothing at all. Knowledge of the present becomes even more important when the historian turns from trying to discover what happened in the past to writing up his findings. Histories are written not only *by* people and *about* people but also *for* people. The people about whom a history is written lived in the past, but the historian and his readers live in the present. No purpose is served by pretending otherwise. The very fact that the historian shares knowledge of the present with his readers is what allows him to communicate successfully with them. Any historian completely ignorant of the present could not begin to discover what happened in the past. After all, his evidence is all in the present. He would be at an equally grave disadvantage in writing history for people living in the present. As Maurice Mandelbaum has observed: "a historian knows something about the nature of his own society through having grown up in it, and he will have learned through its culture something about its past; furthermore, in any society in which there is *inquiry* into the past, a historian will also know something about societies other than his own, and about their pasts."¹¹ As erroneous as the historian's knowledge of the present may be, as much distortion as it may introduce into his work, it is also absolutely necessary.

One often hears that history must be rewritten for every generation, as if this observation implied that each generation necessarily reads its own prejudices into the period under investigation. As true (or false) as this assumption may be, the need to rewrite history has other sources as well. Sometimes later historians have access to data which was unavailable to earlier workers, but more importantly they will be writing for a different audience with different life experiences. Communication is a relation. If either partner in the relation changes, the relation itself changes. The two-member relation of a contemporary historian studying the past becomes a three-member relation when the historian attempts to explain an earlier period to people living in the present. Successive histories of the same period may differ markedly, not because the period under investigation has changed, not because historians necessarily introduce their own concerns and prejudices into their work (though they may), but because

11. Maurice Mandelbaum, *The Anatomy of Historical Knowledge* (Baltimore and London, 1977), 113.

the readers of these histories are different. A history of the Peloponnesian War written for a Victorian schoolboy must differ markedly from one written for students today. This point does not depend on the audience existing at different times. Differences in culture is what matters. Contemporary cultures can differ as markedly from each other as those that exist serially in time.

III. THE RELATIVITY OF MEANING

Periodically philosophers have mused about how nice it would be to have an ideal language in which everything could be said with absolute precision. Historians, however, are presented with real languages in which the same word can mean very many things to different people and these meanings can change through time. Although natural languages are far from total chaos, they also pose serious problems for anyone who wishes to understand and to be understood. An historian will have been raised in a particular subculture speaking the variant of his language peculiar to that subculture. If one wishes to learn about a contemporary subculture and its language, one can always become part of it and learn through participation. Of course, it might be impossible to eliminate totally one's previous experience. Some immigrants always view their new surroundings through the eyes of their original homes, but at least there are non-linguistic ways of reducing the discrepancies. Such non-linguistic experience is all but unavailable to the historian studying an earlier period. By and large the best he can do is to immerse himself in the records currently available, and the most informative records tend to be linguistic. Whether the historian's subjects speak a foreign language or an earlier version of his own language, the historian begins by translating their utterances into his own idiom. Little by little, however, he may develop the ability to think in the language of the period under investigation. But how can he be sure that he has completely annulled the distortions which his own culture and language are likely to introduce into his understanding? He has caught himself often enough in the past. How can he be sure that anachronisms will not continue to infect his investigations?

One message of recent philosophy for all empirical investigators, including historians, is that absolute certainty is not possible. The only thing that one can hope to do is to decrease the likelihood of error, and there are numerous ways to do that. One is to read the works of other historians, especially historians from other cultures and times. Each historian brings with him his own set of biases, but it is much easier to detect the biases of others than one's own. To the extent that the biases of different historians are themselves different, they tend to be mutually corrective. Another way

to decrease the bias that one's own conceptual scheme can introduce is by studying more than one area. I might be so indoctrinated in contemporary evolutionary theory that I dismiss nineteenth-century notions of saltative evolution as supernatural. However, I might not be so totally committed to the conceptual outlook of contemporary geology. Reading the uniformitarian debate in geology might liberate my understanding of biological evolution. That increase in understanding might in turn improve my original understanding of geology. As "illogical" as the bootstrap effect may be, the resulting increase in understanding is nonetheless real.

The point I wish to emphasize is that one way of eliminating the biases introduced into our understanding of past science by our knowledge of current science is *not* by ignoring the fact that we do understand certain areas of present-day science. If an historian knows anything about the science of his day, and it is difficult to see how that can be avoided, he would be wise to become very clear about his views so that he does not allow them to color his reading of early science. For example, one often hears that Darwin held a "blending" theory of inheritance in contrast to our modern "particulate" theories, and that he thought that the variations operative in the evolutionary process were "continuous" rather than the small, discrete mutations which we now take to be the ultimate source of variation. Such claims are hopelessly misleading, not because Darwin's views are being contrasted with modern ideas but because the two are being confused. The historian has been taken in by a century of scientific propaganda.

Another problem posed by the relativity of meaning is that the historian may well be able to neutralize his own preconceptions by long years of study, but his readers will not have had the benefit of this same extensive experience. After eliminating as best he can his own misunderstandings of the past, the historian must make sure that his readers do not fall into the same traps. One might be tempted to try to avoid the difficulties posed by translating from one language into another by using the language of the period under investigation; for instance, by writing about classical Greek science in classical Greek. Such a maneuver has all the advantages of publishing a book as its own translation or, as Lewis Carroll remarked, attempting to use a country as its own map:

"What do you consider the *largest* map that would be really useful?"

"About six inches to the mile."

"Only *six inches!*" exclaimed Mein Herr. "We very soon got to six *yards* to the mile. Then we tried a *hundred* yards to the mile. And then came the grandest idea of all! We actually made a map of the country, on the scale of a *mile to the mile!*"

"Have you used it much?" I enquired.

"It has never been spread out yet," said Mein Herr: "The farmers objected: they

said it would cover the whole country, and shut out the sunlight! So we now use the country itself, as its own map, and I assure you it does nearly as well."¹²

I have heard historians complain of calling anyone a biologist prior to Lamarck's coining the term "biologie" in 1802. I can see the point of not confusing the sort of thing which Aristotle did with the activities of a Lamarck, Darwin, or Pasteur, but I cannot see how refusing to term Aristotle a "biologist" will help in the least. The gap between Pasteur and Lamarck is even greater than that between Lamarck and Aristotle. Current usage departs even more radically from Lamarck's original usage. To be sure, Darwin did not use the term "evolution" in the *Origin of Species*. At the time it referred roughly to what we now call ontogenetic development. But as Michael Ruse has stated quite forcefully, it now means *evolution*, and "we today have a perfect right to use our own language."¹³ If use of one's own language in writing about the past is presentism, then presentism is a necessary element in good historiography.

One way to overcome the difficulties introduced by the relativity of meaning is to use the closest contemporary term while warning the reader of the relevant differences. Another is to introduce the term actually used at the time but with an appropriate explanation. The point to notice is that on both strategies, the same explanation is given, and it can be given only in full knowledge of the differences and similarities between the culture which is being written *about* and the culture being written *for*. For example, it is difficult to see how one could get a present-day reader to understand Darwin's views on inheritance and evolution without introducing such anachronistic terms as "genotype" and "phenotype." Genotypically, Darwin's theory of inheritance was nearly as particulate as Mendel's theory and even more particulate in certain respects than current views. After all, Darwin's gemmules were tiny, discrete particles. On Darwin's view, phenotypic traits tended to blend because of the variable number and kind of gemmules which he believed contributed to the formation of any one trait. Finally, Darwin neither said nor believed that variation was "continuous." He thought that the variations operative in evolution were slight, small, and insensibly fine in contrast to sports. The notion of continuous variation was introduced much later in the dispute between the biometricians and the Mendelians.

Writing history requires translation, whether the languages concerned are two different contemporary languages, such as German and English, or different stages in the development of the same language. I can read *Beowulf* with no greater ease than *Buddenbrooks*. And, as Sir Karl Popper has argued, translation requires interpretation:

12. Charles Lutwidge Dodgson, *Sylvie and Bruno Concluded* in *The Complete Works of Lewis Carroll* (London, 1939), 556-557.

13. Michael Ruse, *The Darwinian Revolution* (Chicago and London, forthcoming), preface.

Everybody who has done some translating, and who has thought about it, knows that there is no such thing as a grammatically correct and also almost literal translation of any interesting text. Every good translation is an *interpretation* of the original text; and I would even go so far as to say that every good translation of a nontrivial text must be a theoretical reconstruction. Thus it will even incorporate bits of a commentary. Every good translation must be, at the same time, close *and* free.¹⁴

IV. THE RELATIVITY OF REASONING

As historians of philosophy have pointed out, our systems of logic, like natural languages, have changed through the years. Certain sorts of arguments which were prohibited in Aristotelian logic are now considered to be perfectly valid, and vice versa. Aristotle's philosophy of science was condemned by Bacon, and we in turn condemn his extreme inductivism. In a review of Adolf Grünbaum's *Philosophical Problems of Space and Time* (1973), Arthur Miller complains that Grünbaum's "analysis is carried out ahistorically, that is, exclusively in terms of the philosophy of science circa 1973."¹⁵ There is an obvious point to Miller's criticism. If Grünbaum supposes that the scientists under scrutiny accepted the same philosophy of science which he himself accepts, then he is mistaken. If Grünbaum fails to mention differences in the principles of reasoning accepted in the period under investigation and those prevalent today, he is omitting important information. However, Miller also seems to be criticizing Grünbaum for using present-day standards of good reasoning in his own analysis.

When an historian begins to study a period, should he himself use the methods of good reasoning accepted at the time under investigation or his own? Put this bluntly, the answer is painfully obvious. In the initial stages of inquiry, an historian cannot use the standards accepted at the time because he has no way of knowing what they are. More importantly, even as he comes to understand these earlier principles of reasoning, he would be wrong to substitute them for his own. Doing so would mean replacing principles which he and his fellow historians take to be correct with those which we now take to be mistaken. More serious problems arise when historians attempt to *evaluate* the reasoning used by earlier scientists. Many historians maintain that such evaluations have no place in history. Historians should record what happened and nothing else. Such historiographic dicta are easier to enunciate than to defend. Present-day scientists make mistakes, equivocate on the meanings of terms, claim that certain

14. Karl Popper, *Unended Quest* (La Salle, Ill., 1976), 23.

15. Arthur I. Miller, "Review of Adolf Grünbaum, 'Philosophical Problems of Space and Time' (Dordrecht, Holland, 1973)," *Isis* 66 (1975), 590-594; Adolf Grünbaum, "Remarks on Miller's Review of 'Philosophical Problems of Space and Time'," *Isis* 68 (1977), 447-448; Arthur I. Miller, "Reply by Arthur I. Miller," *Isis* 68 (1977), 449-450.

conclusions follow from their premises when they do not, and so on. Their contemporaries feel perfectly free to point out these mistakes. However, once science slips into the past and becomes the province of the historian, all such evaluations must suddenly cease.

As always there is some point to such prohibitions. Mutual criticism among scientists living at the same time results in the improvement of science. For example, there was some point to William Hopkins' scolding Darwin for his sloppy methodology.¹⁶ It might have forced Darwin to improve his practice or explain why there was nothing wrong with it. This debate, of course, would have taken place in the context of nineteenth-century views on the subject. The same justification does not apply to later criticisms by such historians as Nordenskiöld and Himmelfarb.¹⁷ There is no way that they can hope to improve Darwin's methodology by their criticisms. Instead the point of their criticisms seems to be to cast doubt on contemporary versions of evolutionary theory by attacking Darwin. In doing so, they are using (possibly abusing) history in the service of a scientific cause. They are masquerading science as history.

However, prohibitions about importing present-day principles of good reasoning into the history of science can be carried too far. For example, the explicitly stated philosophies of science in the first half of the nineteenth century in Great Britain were extremely empirical and inductivist. In this context, many of Darwin's contemporaries claimed that his theory was logically unacceptable. It was one mass of conjectures, unsubstantiated assertions, and leaps of faith. Darwin and his defenders claimed otherwise. Can an historian say nothing more? When Darwin's practice is compared to the inductivist standards of a John Stuart Mill, it comes up wanting. But this is only part of the story. In general, the actual practice of other nineteenth-century scientists fared no better. In addition, then as now, scientists had too much sense to swallow methodological pronouncements without a bit of salt. After studying Mill, Whewell, Newton, and Darwin, an historian is forced to conclude that Mill and Whewell distorted the work of these scientists to fit their own purposes. Does proper historiographic method preclude an historian from *saying* so? I hope not.

The problem of self-reference discussed earlier is only magnified when at issue are the principles of good reasoning. We are not all biologists or historians, but we are all "reasoners." No matter what hat an investigator

16. William Hopkins, "Physical Theories of the Phenomena of Life," *Fraser's Magazine* 61 (1860), 739-752, 62 (1860), 74-90; reprinted in D. L. Hull, *Darwin and His Critics: The Reception of Darwin's Theory of Evolution by the Scientific Community* (Cambridge, Mass., 1973), 229-272.

17. Erick Nordenskiöld, *The History of Biology* [1920-1924], transl. Leonard Bucknall Eyre (New York, 1928); Gertrude Himmelfarb, *Darwin and the Darwinian Revolution* (London, 1959).

is wearing at any one moment, there is one hat which he can never take off. No matter what else an historian may be doing, he is using certain principles of reasoning, and his use implies at least tacit acceptance. Although divine revelation was once thought to be a legitimate way to arrive at conclusions about the world in which we live, no historian today would justify his findings in these terms, any more than he would use the "logic" exhibited in the writings of a mystic in his own investigations of that mystic. However, the very fact that a contemporary historian would never cite Scripture in support of his discussion of the debate over evolutionary theory places him squarely in the naturalistic camp, and naturalism was one of the chief points at issue. If an historian actually thinks his principles of investigation are superior to those of the people whom he is studying, if his use of these principles implies that he thinks they are superior, what is the point of his not honestly *saying* so? Of course Grünbaum uses philosophic principles circa 1973 in his analysis. What other principles should he use?

When we turn from the methods by which an historian attempts to discover what went on in the past to his communicating these findings to his contemporaries, the problem of discrepancies between the principles of reasoning used in the past and today arises once again. If an historian sets out without comment a line of reasoning which on today's standards is clearly fallacious, his readers have every right to feel puzzled. Similarly, presented with a scientist's failing to draw what seems like an extremely obvious conclusion from a set of premises, a modern reader might also be justifiably puzzled. One possible source of the puzzlement is that the scientists concerned did not share with us our current principles of logic. For example, many of the peculiarities of Aristotle's laws of motion make sense once we are told that Aristotle's syllogistic logic is applicable only to existent things — and the void is the absence of everything. Of course, there is always the possibility that the scientist made a mistake, given the standards of his day, or else failed to see the obvious. But in many cases the question arises only because of differences between our principles of reasoning and those accepted in earlier periods. Having noticed such discrepancies, the historian would only increase confusion if all he did was pass over them without comment.

V. THE RELATIVITY OF TRUTH

One of the chief faults which modern historians find with the histories written by their predecessors, especially those written by scientists emeriti, is the tendency to read current empirical knowledge back into the past, describing, for example, the origin of vaccination as if everyone concerned understood the nature of viruses and contagious diseases. Even worse is

the condemnation of early scientists for not knowing what we know today. However, sometimes present-day historians in their historiographical asides sound as if they think that truth is relative. Throughout the history of civilization, most people believed that the earth was flat. Today most people believe that it is roughly spherical. Unlikely though it may be with respect to this overly simple example, we too could be wrong. The problem is how to describe this state of affairs in full recognition of the fallibility of human knowledge and the variability of human discourse.

One solution is to equate truth with belief. Saying that something is true is equivalent on this view with saying that the people in question believed it. On one extreme interpretation, such claims imply that the earth changed its shape as people changed their beliefs on the subject. The earth used to be flat, and now it is roughly spherical. The absurdity of this interpretation can be avoided by refusing to talk about the shape of the earth in the first place and to refer only to people's beliefs. People used to believe that the earth was flat; now they believe that it is roughly spherical. The actual shape of the earth is irrelevant to the historian. As attractive as this maneuver may seem, it accomplishes nothing because the next question is whether these people actually held the beliefs being attributed to them. Beliefs about the shape of the earth are just as much part of the empirical world as the shape of the earth — and no easier to discover.

Regardless of what they may say on the subject, the actions of historians belie any belief in the relativity of truth. Did Marx write to Darwin asking to dedicate *Das Kapital* to him? No historian would accept as evidence in this dispute the unsupported beliefs of his fellow historians, regardless of how widely or how deeply they were held. At times historians say things which make it sound as if they believed that truth is relative. To be sure, the beliefs of the people living in a society in some sense "define their reality." But reality has a way of forcing itself on us independent of our beliefs. Whether or not historians are right to do so, they act as if there were more to history than the beliefs of historians. The presentation of relevant data and cogent arguments are what really count. Perhaps historical claims, like all empirical claims, will never attain absolute certainty. Perhaps the relativity of meaning will always interfere with our attempts to describe the empirical world precisely. No doubt our understanding of rationality will continue to change through time. But there is a difference between belief and rationally justified belief. If historians are willing to make this distinction for themselves — and they are — they should be willing to make it for others as well.

Because early historians of science unfairly lampooned such figures as Aristotle, Lamarck, and Gall for holding some of the views they held, modern historians are led to argue that no mention should be made of discrepancies between the empirical beliefs of the scientists under investi-

gation and our own. One should set out a scientist's ideas and leave it at that. Once again, there is considerable point to this maxim. In order to understand Aristotle's physiological system, it is important to know that he thought that the brain cooled the blood. That we now take this belief to be mistaken is not very important. However, this maxim itself can be carried too far. We can learn a great deal about a scientist by the mistakes he makes. For example, Aristotle himself seems to have dissected many of the organisms which he described. In general, he was a very good observer. When his descriptions depart too radically from the organisms as we now know them, the historian has the right to suspect that Aristotle did not make these dissections but is passing on second-hand information. Similarly, we know today that several of the physical differences which Aristotle notes between men and women do not exist. That Aristotle thought they did tells us something of the attitudes in his society about the sexes.

Historians use our knowledge of the present to reconstruct the past. They could not do otherwise. All the evidence they have available to them exists in the present. Historians are also well aware that certain features of the world in which we live change through time. The earth has cooled, continents have drifted, mountain ranges have worn away, jungles have come and gone, species have become extinct, human societies have arisen and disappeared. One task of the historian is to chronicle these changes. But he also believes, and must believe if he is to reconstruct the past on the basis of the records surviving to the present, that the processes producing these changes are sufficiently stable through time. Darwin, for example, believed that certain traits could be explained only in terms of the inheritance of acquired characteristics. That is no surprise. The belief was common at the time. The evidence seemed to support it, and it fit nicely into his theory of inheritance. That we now take such beliefs to be mistaken is irrelevant. However, what if Darwin had mentioned that snow is black? Instead of mindlessly registering that Darwin believed that snow is black, the historian would surely try to find out if Darwin actually held such a belief and if so, why. Darwin was not prone to make such patent observational mistakes. The source of the historian's puzzlement is that he knows that snow is white now and that we have every reason to believe that it was just as white in Darwin's day — regardless of the beliefs of the people at the time. Similarly, Christians have traditionally maintained that Mary was a virgin when she gave birth to Jesus. The historian must surely take note of these beliefs, but he is also warranted in looking for the biological father. He has every right to believe that virgin births were no more common two thousand years ago than they are today.¹⁸

18. In his review of Joe D. Burchfield's *Lord Kelvin and the Age of the Earth* (London and Basingstoke, 1975), Edward Bullard discusses the story which Burchfield tells and then concludes:

Historians use their knowledge of the present in reconstructing the past. They also use it in writing for readers who are likely to share this knowledge with them. A modern reader is just as likely to be shocked at Darwin's claiming that snow is black as the historian. Some explanation is surely called for. Present-day readers are likely to view a belief in Lamarckian modes of inheritance as not only mistaken but also unscientific. Mistaken, though justified, it surely was. Unscientific it was not. Which of Darwin's beliefs the historian decides to expand upon in this connection is not determined by which turned out to be correct and which faulty, given current knowledge, but by which are likely to puzzle the modern reader and which not. Because the only science a present-day reader is likely to know is the science contained in contemporary textbooks, the historian is well advised to pay attention to similarities and differences between this science and the science under discussion. For example, the first time that a person reads Darwin commenting on three-to-one ratios in inheritance, he is likely to jump to the conclusion that Darwin had just stumbled on the key to Mendelian genetics. Why this conclusion is mistaken takes some explaining. Ignoring the likelihood that this is exactly the sort of mistake that someone knowing modern Mendelian genetics is prone to make can hardly be good historiography.¹⁹

VI. CONCLUSION

In this paper I have been concerned to show that knowledge of the present is absolutely crucial for the historian, both in reconstructing the past and in

In spite of the careful account of all these things some readers may feel a little deprived. An historian must study the past in its own terms. He must not ask 'What was Henry VIII's attitude to women's lib?'. This viewpoint has been adopted also by historians of science, and in some degree, is clearly necessary. But should one go the whole way? The author of this book consistently refrains from saying that anything is right or wrong, silly or unjustified. If Kelvin assumes that the Earth was initially at 1,500° C, the author reports that he said it and that it was a more or less arbitrary choice, but he does not add that it matters very little what figure is taken. On the other hand no one would guess that Kelvin's assumption of the absence of convection within the Earth was crucial and that the attacks on it were well based. The history of science is different from other kinds of history; there is the additional fact that some things are correct and some wrong *sub specie aeternitatis*.

I share Bullard's feelings of deprivation, but I do not think that these feelings are limited just to readers of histories of science.

19. The major sort of relativity which I do not discuss in this paper is the relativity of morals. Strangely enough, historians who would never dream of criticizing an early scientist for not having present-day conceptions, for failing to adhere to those principles of good reasoning which we now hold, or for making what we take to be factual errors feel perfectly free to sneer at early scientists for not living up to the historian's own moral standards. Denigrating Darwin for believing that a previous insemination of a mare can affect later pregnancies is bad historiography; condemning him as a racist is somehow perfectly all right. But Darwin's attitudes on racial matters were as "enlightened" by nineteenth-century standards as were his ideas on heredity.

explaining it to his readers. 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 investigation. He then must communicate these findings to his contemporaries. Even the most compulsive historian is likely to know more about his own age than the one he is studying. His reader is sure to. Warnings about “presentism” are designed to prevent our knowledge of the present from distorting our knowledge of the past. Perhaps the possible abuses of prohibitions against presentism detailed in this paper are exaggerated. I am afraid they are not. The two commonest responses which I have received to this paper are that no historian has ever held the historiographic principles which I discuss — they are straw men — and that every right-thinking historian does.

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[Footnotes]

¹ **Review: Should Philosophers Be Allowed to Write History?**

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Faraday as a Natural Philosopher by Joseph Agassi

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³ **Review: Should Philosophers Be Allowed to Write History?**

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⁶ **Should the History of Science Be Rated X?**

Stephen G. Brush

Science, New Series, Vol. 183, No. 4130. (Mar. 22, 1974), pp. 1164-1172.

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¹⁵ **Review: [Untitled]**

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