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Stephen G. Brush

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Engraved for the Universal Magazine.



*For J. Hinton at the King's Arms in Newgate Street.*

**One can be present-minded without being whiggish. One need not avoid noting that Robert Boyle, for example, misunderstood the physics of his vacuum apparatus. See page 220. Photograph courtesy of the Chemical Heritage Foundation.**

# Scientists as Historians

By Stephen G. Brush\*

“SHOULD SCIENTISTS write history of science?”<sup>1</sup> Surely this question would sound bizarre to those who are not professional historians of science. They would be more likely to ask: “Should anyone who is *not* a scientist write history of science?” The idea that anyone would write about the history of science without having at least acquired advanced training in science, if not significant research experience, strikes most people—and especially scientists—as absurd.

The short answer to both questions is yes. Scientists should write history of science if they are willing to acquire the skills and background knowledge of the historian of science; and nonscientist historians should write history of science if they are willing to learn enough science to understand what they are going to write about.

The real issue is the proposition that historians and scientists, even if they share the same knowledge and skills, have distinctively different ways of writing about the history of science. History written by scientists has been called, by Allen Debus and others, “science-history.”<sup>2</sup> Is it different from, or an identifiable version of, “history of science”? Is there any good reason for imposing a single style or viewpoint on all writings about the history of science?

During the 1960s and 1970s, as historians of science struggled to establish an academic discipline within history rather than as an adjunct to science, the most popular way to stigmatize science-history was the disdainful phrase “the whig interpretation of the history of science.”<sup>3</sup> Did historians of science strive so hard to be antiwhig that they slipped into a dogmatic “contextualism” that has been outgrown

\* Department of History and Institute for Physical Science & Technology, University of Maryland, College Park, Maryland 20742.

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<sup>1</sup> This was the assigned topic for the paper presented at the Conference on Critical Problems and Research Frontiers.

<sup>2</sup> Allen G. Debus, “The Relationship of Science-History to the History of Science,” *Journal of Chemical Education*, 1971, 48:804–805.

<sup>3</sup> For accounts of the development of the history of science discipline in the United States, portrayed in part as liberation from the domination of scientists and as an assertion of the professional independence of the historian, see Arnold Thackray and Robert K. Merton, “On Discipline Building: The Paradoxes of George Sarton,” *Isis*, 1972, 63:473–495; Thackray, “The Pre-History of an Academic Discipline: The Study of the History of Science in the United States, 1891–1941,” *Minerva*, 1980, 18:448–473; Frederick Gregory, “The Historical Investigation of Science in North America,” *Zeitschrift für Allgemeine Wissenschaftstheorie*, 1985, 16:151–165; and Nathan Reingold, “History of Science Today, 1: Uniformity as Hidden Diversity: History of Science in the United States, 1920–1940,” *British Journal for the History of Science*, 1986, 19:243–262.

by other historians? Have they committed themselves to an untenable belief in the objective reality and knowability of past events? Conversely, has antiwhiggism led historians to exclude themselves from any role in contemporary affairs where they might be able to use their professional knowledge?

While it is easy enough to find contemporary examples of the whig interpretation, that label is no longer a very useful club to beat the scientists with, for reasons to be examined below. Instead, the current fashion is to stress the social construction of scientific theories and concepts, and to deny that scientists are actually discovering the truth about the world, or that their efforts to do so have any moral or epistemological superiority to those of pseudoscientists, humanists, and theologians. The historian no longer assumes that scientific research is an admirable activity and scientific progress a benefit to society, whereas the scientist, by definition, thinks science is worth doing.

Readers unfamiliar with recent publications in science studies may not believe that any reputable scholar actually holds such extreme views, and might even dismiss the abstract "historian" in the last sentence as a straw person. Fortunately, Paul Forman has published an essay that beautifully describes the distinction between the putative historian's and scientist's views, and has clearly declared his allegiance to the former. He also identifies a specific document—Max Dresden's biography of H. A. Kramers—as an example of science-history, although we do not of course have to accept that document as being representative of the genre.<sup>4</sup> Nevertheless, Forman has greatly advanced the discussion of the issue. In his terms, historians of science should aim at *independence*—should be neutral or skeptical observers rather than partisans of science, in contrast to the scientists, whose emotional attachment to the *transcendence* of science disqualifies them from being suitably objective about it.

Whatever may be its defects from Forman's viewpoint, Dresden's book certainly illustrates some of the *advantages* of science-history. In particular, Dresden's technical knowledge allows him to make a detailed (and not always favorable) evaluation of Kramers's contributions to science, while at the same time his own experience in doing research enables him to understand and empathize with the emotional state of a researcher. Neither is impossible for nonscientist historians, but both are increasingly rare. To what extent can our understanding of the history of science be advanced by putting ourselves in the place of and reliving, physically and psychologically, the experiences of a scientist? Apparently Forman would say, not at all.

One of the critical problems, not just in the history of science but in the philosophy and sociology of science, is the extent to which scientific facts are socially constructed rather than discovered. Unfortunately, social construction has all too often been simply asserted, not demonstrated. Stephen Cole stated in a recent book that scholars who claim that social processes influence the cognitive content of science have failed to produce a single example in which this alleged causal influence is demonstrated by the standards of sociological research. In reviewing this book

<sup>4</sup> Paul Forman, "Independence, Not Transcendence, for the Historian of Science," *Isis*, 1991, 82: 71–86; and Max Dresden, *H. A. Kramers: Between Tradition and Revolution* (New York: Springer-Verlag, 1987).

Steven Shapin, a leader of the social construction school, quoted that statement but could not refute it with any specific examples.<sup>5</sup>

One way to test an assertion about social construction would be to try to reproduce the calculation or experiment, to see if nature or mathematics yields the reported result. In doing so, one is behaving as a scientist—or at least one must enlist the expertise of a scientist to perform the test. Of course it cannot be assumed that the modern replication yields a true fact about nature, and that the original result must be wrong if it is different; I suggest only that replication can provide additional relevant evidence.

### I. FROM WHIG TO PRIG?

The phrase “whig interpretation of the history of science” was apparently introduced by George W. Stocking, Jr., as an explicit application of Herbert Butterfield’s characterization of writings on British political history. Stocking quoted Butterfield’s definition: “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 which is the ratification if not the glorification of the present.”<sup>6</sup>

Stocking identified the whig interpretation as a version of *presentism*—the term then being used by historians for the general tendency to see the past in terms of the present—and contrasted it to *historicism*, which he defined as “commitment to the understanding of the past for its own sake.” While admitting that even professional historians cannot completely avoid presentism, Stocking suggested that scientists are more likely to judge the history of science in terms of present-day science:

There is a sort of implicit whiggish presentism virtually built into the history of science. . . . However disillusioned we may be with the idea of progress in other areas, however sophisticated in the newer philosophy of science, most of us take it for granted that the development of science is a cumulative ever-upward progress in rationality. Indeed, George Sarton, long-time doyen of historians of science, described his study as “the only history which can illustrate the progress of mankind” because “the acquisition and systematization of positive knowledge are the only human activities which are truly cumulative and progressive.”

Stocking also asserted that Thomas S. Kuhn’s *Structure of Scientific Revolutions* marked the beginning of a trend away from whiggism in the history of science, even though it was “imperfectly historicist in its focus on the inner development of science” to the neglect of external factors.<sup>7</sup>

<sup>5</sup> Stephen Cole, *Making Science: Between Nature and Society* (Cambridge, Mass.: Harvard Univ. Press, 1992), p. 81; and Steven Shapin, “Mertonian Concessions” (rev. of Cole), *Science*, 1993, 259:839–841.

<sup>6</sup> George W. Stocking, Jr., “On the Limits of ‘Presentism’ and ‘Historicism’ in the Historiography of the Behavioral Sciences,” *Journal of the History of the Behavioral Sciences*, 1965, 1:211–218; and Herbert Butterfield, *The Whig Interpretation of History* (London: Bell, 1931), p. v. See also W. F. Bynum, E. J. Browne, and Roy Porter, eds., *Dictionary of the History of Science* (Princeton, N.J.: Princeton Univ. Press, 1981), article “Whig History,” pp. 445–446.

<sup>7</sup> *Ibid.*, pp. 213, 214; and Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Univ. Chicago Press, 1962; 2nd ed., 1970).

In 1968 Kuhn defined the “maxims of the new internal historiography” which had for the previous thirty years “increasingly guided the best interpretive scholarship in the history of science”:

Insofar as possible (it is never entirely so, nor could history be written if it were), 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. . . . Dealing with innovators, the historian should try to think as they did. . . . [T]he historian should ask what his subject thought he had discovered and what he took the basis of that discovery to be. And in this process of reconstruction the historian should pay particular attention to his subject’s apparent errors, not for their own sake but because they reveal far more of the mind at work than do the passages in which a scientist seems to record a result or an argument that modern science still retains.<sup>8</sup>

Kuhn is himself a former scientist, with a Ph.D. in physics and at least two research publications in solid-state theory.<sup>9</sup> Yet he seems to imply that this background is a handicap in understanding the physics of earlier centuries. But without this background, how would he distinguish “his subject’s apparent errors” from what “modern science still retains”?

Other historians of science shared Kuhn’s view. Robert Fox, now a professor at Oxford, wrote in 1970:

A knowledge of science, which is most frequently cited as a fundamental requirement for the historian of science, is in reality no more than an auxiliary tool in a study whose basic skills are those of the historian. . . . [F]or most work in the history of science, whether at the undergraduate or postgraduate level, [a background in science] is emphatically not essential; in fact, as I shall argue, such a background can even be positively harmful.<sup>10</sup>

There is a curious echo here of the claim by Robert Livingstone Schuyler, criticizing present-minded historians:

I submit that there is no reason at all why the historian, considered as such, should be concerned with what is going on around him. If, for example, a historian of the medieval English parliament knew nothing of the history of parliament since, say, 1689, would he for that reason be in a less favorable position, for investigating its beginnings than if his head were full of nineteenth-century reform bills and the present-day cabinet system? I think not. Perhaps he would be in a more favorable position, for he would be less likely to be led astray by modern developments.<sup>11</sup>

Butterfield’s demand that the historian study the past for its own sake without reference to the present has proved hard to satisfy. In fact Butterfield himself did not

<sup>8</sup> Thomas S. Kuhn, “History of Science,” *International Encyclopedia of the Social Sciences* (New York: Macmillan, 1968), Vol. 14, pp. 74–83, on pp. 76–77.

<sup>9</sup> Thomas S. Kuhn, “An Application of the W. K. B. Method to the Cohesive Energy of Univalent Metals,” *Physical Review*, 1950, series 2, 79:515–519; and Kuhn and J. H. Van Vleck, “A Simplified Method of Computing the Cohesive Energies of Monovalent Metals,” *ibid.*, pp. 382–388.

<sup>10</sup> Robert Fox, “The History of Science,” in *History: An Introduction for the Intending Student*, ed. H. J. Perkin (London: Routledge & Kegan Paul, 1970), pp. 173–186, on p. 175.

<sup>11</sup> Robert Livingstone Schuyler, “Some Historical Idols,” *Political Science Quarterly*, 1932, 47: 1–18, on p. 16. I owe this reference to Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (New York: Cambridge Univ. Press, 1988), p. 273, who says Schuyler “had been much impressed” with Butterfield’s 1931 book.

consistently obey it. In 1944 he admitted that the whig interpretation is here to stay, and it is futile to go on attacking it:

Those who, perhaps in the misguided austerity of youth, wish to drive out that whig interpretation, (that particular thesis which controls our abridgment of English history,) are sweeping a room which humanly speaking cannot long remain empty. They are opening the door for seven devils which, precisely because they are newcomers, are bound to be worse than the first. We, on the other hand, will not dream of wishing it away, but will rejoice in an interpretation of the past which has grown up with us, has grown up with the history itself, and has helped to make the history. . . . [W]e will celebrate this whig inheritance of ours with a robust but regulated pride.<sup>12</sup>

As for history of science, it has been noted by several readers that Butterfield's own book on the history of modern science is as whiggish as anything from the pen of George Sarton. While stressing the importance of understanding earlier theories and the context in which they were replaced by modern ones, Butterfield insists that the events of the 1940s helped historians to see the true significance of the Scientific Revolution of the seventeenth century more clearly than was possible at the beginning of the twentieth century.<sup>13</sup>

Although general historians and historians of science uncritically accepted Butterfield's original thesis for three or four decades, it gradually became clear that it suffers from severe defects. These may be summarized under two headings: first, it is impossible in principle to write a nonpresentist history of anything; second, nonpresentist history is not even a desirable ideal toward which to strive. Moreover, it seems perverse to deny that science *has*, in most respects, made substantial progress in understanding how nature works, whatever one may think of the uses to which that understanding has been put.

Before discussing these objections, we must decide: What is "nonwhiggish history" and what should we call it? Butterfield asserted that "there is not anything worth the name of 'the tory interpretation of English history,'"<sup>14</sup> although in the history of science one might give the label "tory interpretation" to the view that the quality of science has declined since a putative golden age (see, e.g., the writings of Clifford Truesdell). Studying the past for its own sake used to be called *antiquarianism*, but that word has acquired a negative connotation. Similarly, the term *historicism* has been so abused that it no longer has a generally accepted coherent meaning. Edward Harrison suggests that the extreme antiwhig historian should be called a "prig," denoting the "narrow-minded superiority" of those who make a virtue of their ignorance about modern science.<sup>15</sup>

For lack of a better word, *contextualism* seems to have emerged as a positive label for the doctrine that one should study the ideas and theories of a period in terms of the scientific knowledge and general culture of that period, without regard to what came afterwards. (Some historians would also limit their context to a local

<sup>12</sup> Herbert Butterfield, *The Englishman and His History* (Cambridge: Cambridge Univ. Press, 1994), pp. 3–4.

<sup>13</sup> Herbert Butterfield, *The Origins of Modern Science (1300–1800)* (1949), rev. ed. (New York: Macmillan, 1957), p. 148.

<sup>14</sup> Butterfield, *Englishman* (cit. n. 12), p. 2.

<sup>15</sup> Edward Harrison, "Whigs, Prigs, and Historians of Science," *Nature*, 1987, 329:213–224. See comments by S. C. McCluskey and P. J. Bowler, "Historians, Whigs, and Progress," *Nature*, 1987, 330:598.

community). Progress in historiography is to be measured by the extent to which the historian is liberated from "constraints imposed on inquiry by presuppositions about the past" stemming from knowledge of the present.<sup>16</sup> The contents of the new journal *Science in Context* may be taken to exemplify one currently popular version of contextualist history of science.<sup>17</sup>

It is also essential to distinguish between whiggism and the more general term *present-mindedness*, or *presentism* for short. The present-minded historian asks *questions* about the past inspired by concerns of the present; the whig historian gives *answers* that are distorted by those concerns. One can reject whiggism without completely rejecting presentism.<sup>18</sup> Thus it is presentist to ask why Gregor Mendel's work was ignored by his contemporaries (a historian writing before 1899 would have had no reason to ask that); one can see in the historiography of genetics a shift from whiggish to nonwhiggish answers to that question, yet the question itself is still considered legitimate.

Let us reserve the term *priggism* for the view that the historian should be exclusively contextual. The prig historian would insist that if a crucial theorem in Isaac Newton's *Principia* rests on a mathematically fallacious derivation but no one caught the error during Newton's lifetime, it is improper to point that out now; if Robert Boyle's conclusion about the transmission of sound at low pressures is vitiated by his misunderstanding of the physics of his vacuum apparatus, it is unacceptably whiggish to say so; and if Mendel's contemporaries ignored him, so should we! All that matters, for the prig historian, is what scientists thought to be true or important at the time. There are probably few if any prig historians of science; this is simply the position implied by taking seriously some of the more extreme antiwhig statements.

The first objection can then be stated thus: There is no unique and consistent way to define the context of an event in the history of science, just as there is no unique and consistent way to define the context of a text in literary analysis.<sup>19</sup> The choice of what context to consider has to be made by the individual historian, and there is no objective basis for preferring one choice to another. Contextualism assumes there is an objective past that can be uncovered by the historian, and an objective historical description that, once properly written, will not have to be rewritten by future historians.<sup>20</sup> But since the dogma that events in the present have an objective existence

<sup>16</sup> Kieran Egan, "Progress in Historiography," *Clio*, 1979, 8(2):195–228, on p. 221.

<sup>17</sup> See the review of *Science in Context* by John Hendry, *Brit. J. Hist. Sci.*, 1989, 22:102–105. Hendry also states that sociologically grounded works such as those of Martin Rudwick, Andrew Pickering, and especially Simon Schaffer and Steven Shapin, at present constitute the mainstream of the history of science.

<sup>18</sup> In developing this distinction (though these authors do not define terms quite as I do), I have been influenced by the arguments of David Hull, "In Defense of Presentism," *History and Theory*, 1979, 18:1–15; Loren Graham, "Why Can't History Dance Contemporary Ballet? or, Whig History and the Evils of Contemporary Dance," *Science, Technology & Human Values*, 1981, 34:3–6; Helge Kragh, *An Introduction to the Historiography of Science* (New York: Cambridge Univ. Press, 1987); Adrian Wilson and T. G. Ashplant, "Whig History and Present-Centred History," *Historical Journal*, 1988, 31:1–16; Ashplant and Wilson, "Present-centred History and the Problem of Historical Knowledge," *ibid.*, pp. 253–274; and Ernst Mayr, "When Is Historiography Whiggish?" *Journal of the History of Ideas*, 1990, 51:301–309.

<sup>19</sup> See David Harlan, "Intellectual History and the Return of Literature," *American Historical Review*, 1989, 94:581–609.

<sup>20</sup> This is not a logical consequence of contextualism, and indeed one could just as well argue, especially in view of the diversity of contexts invoked by historians, that contextualism problematizes the notion of a unique objective past (I owe this point to George Stocking). Yet almost every individ-



independent of the observer has been resoundingly refuted (at least for events involving individual atoms) by modern physics, *a fortiori* the objective existence of events in the past, which in any case can be investigated only through relics surviving in the present, cannot be taken for granted. It is the observer (including the historian) who confers reality on the past as well as the present.<sup>21</sup>

For readers not convinced by the argument from physics, a linguistic proof may be more persuasive. Gary Hardcastle uses William V. O. Quine's thesis of the indeterminacy of translation to show that since any historical interpretation of a document or event involves "translation," based on some presentist assumption, there is no single correct answer to historical questions about science.<sup>22</sup>

Many commentators on the whig interpretation have pointed out that it is impossible to avoid presentism completely, since the very selection of topics to be studied is based on some present-minded preconception. Why do many more historians choose to study the history of astronomy than of astrology? Of Newton's theory than of Descartes's? Of Charles Darwin's theory than of Jean-Baptiste de Lamarck's? Having made a present-minded choice of topic, and of questions to ask about that topic, one can still try to avoid whiggish answers.

It is very difficult to carry through a complete contextualist criticism of a whiggish history without at some point invoking whiggish arguments. A recent example is David Leveson's indictment, on grounds of whiggism, of Claude Allègre's book on the development of plate tectonics. In discussing Allègre's evaluation of objections to Alfred Wegener's theory in 1926, Leveson complains that Allègre dismisses several objections which "are still considered valid. No one now proposes that sial drifts through sima, or that in doing so mountains are thrown up. Pleistocene moraines are not correlated across the Atlantic."<sup>23</sup>

A more interesting example is Kuhn's book on the origin of quantum theory.<sup>24</sup> He attacks the widely held view that Max Planck introduced the quantum hypothesis in 1900, arguing that this view arose from reading Planck's paper in the light of later developments. If we read the 1900 paper in the context of research by Planck and others before 1900, we can see that he did not really postulate a physical discontinuity in energy in that paper. But to make his argument plausible, Kuhn presents a detailed technical analysis that relies in part on twentieth-century understandings of statistical concepts, including a critique of Ludwig Boltzmann's derivations that uses standards of rigor rarely if ever applied by physicists in the nineteenth century.<sup>24</sup> Kuhn's book is a brilliantly successful contribution to the history of science, somewhat present-minded but not whiggish, and very much dependent on his own

usual historian tends to write as if the chosen context is unproblematic. This is noted explicitly by Novick, *That Noble Dream* (cit. n. 11).

<sup>21</sup> John Archibald Wheeler, "Frontiers of Time," in *Problems in the Foundations of Physics*, ed. N. Toraldo di Francia (Amsterdam: North-Holland, 1979), pp. 395–497.

<sup>22</sup> Gary L. Hardcastle, "Presentism and the Indeterminacy of Translation," *Studies in History and Philosophy of Science*, 1991, 22:321–345.

<sup>23</sup> David J. Leveson, "Whiggism and Its Sources in Allègre's *The Behavior of the Earth*," *Earth Sciences History*, 1991, 10:29–37, on p. 30; and Claude Allègre, *The Behavior of the Earth: Continental and Seafloor Mobility* (Cambridge, Mass.: Harvard Univ. Press, 1988). See also D. R. Oldroyd, "Sir Archibald Geikie (1835–1924), Geologist, Romantic Aesthete, and Historian of Geology: The Problem of Whig Historiography of Science," *Annals of Science*, 1980, 37:441–462.

<sup>24</sup> T. S. Kuhn, *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (New York: Oxford Univ. Press, 1978), pp. 43–70, 100–101, 107.

expertise as a theoretical physicist. Ironically, it has received little recognition from other historians of science, in part because of the influence of Kuhn's own historiographic writings, which helped to make this kind of technical analysis unfashionable.

I turn to the second objection: presentism is not only inescapable but may even be desirable. Here we can begin with Butterfield's view: "History is not the study of origins; rather it is the analysis of all the mediations by which the past was turned into our present." Butterfield did not object to asking present-minded questions like "How did religious liberty arise" but rather to the assumption that one should try to identify and praise a particular person or persons in answer to the question, "To whom do we owe our religious liberty?" So he does not really disagree with A. Rupert Hall, who argues that questions of the former kind, which are now considered whiggish, are perfectly legitimate ones for the historian to consider:

The most obvious of all historical questions is: "How did we arrive at the condition we are now in?" To argue that historical study cannot answer that question with finality [is] quite true but . . . also irrelevant if history is to be written. The question is put, and the questioner will find an answer somewhere; if academic historians are silent he or she will seek an answer in other ways. For if there is an academic supply of historical, literary and philosophical reading, there is also a non-academic or general demand for historical, literary and philosophical writing. . . . [I]t is surely deplorable for the intellectual health of a society if the two are totally out of step, and especially if the claims for the intellectual purity, rigour and rectitude of the academic supply are elevated to such extremes that the general demand goes entirely unrecognized by those best qualified to satisfy it. Academia does not exist solely for the sake of delighting and gratifying itself.<sup>25</sup>

But there are also academic justifications for abandoning extreme contextualism or priggism. Some historians have always accepted the thesis that the significance of a person or event depends at least partly on how it affects and is perceived by later generations. Some of the most exciting historical research in recent years started by taking a second look at the past with consciousness raised by late-twentieth-century sensitivity to the concerns of women, minorities, and the "common people." In particular, historians of science have asked fascinating questions about the "gender" of science, completely overlooked by earlier contextual studies.<sup>26</sup>

Advocates of "postmodernism" suggest that contextualism has become a sterile dogma, and that literary analysts, anthropologists, or hermeneutic historians like Hans-Georg Gadamer offer a more enlightened conception, involving the active participation of the historian in creating history. Michel Foucault insisted that "historical descriptions are necessarily ordered by the present state of knowledge." A typical postmodernist formulation is that of F. R. Ankersmit: "The essence of the past is not, or does not lie in, the essence of the past. It is the scraps, the slips of the tongue, the *Fehlleistungen* of the past, the rare moments when the past "let itself go," where we discover what is really of importance to us." According to Ankersmit, social history is still futilely seeking the essence of history, looking for the trunk of the

<sup>25</sup> Butterfield, *Whig Interpretation of History* (cit. n. 6), on pp. 46–47; and A. Rupert Hall, "On Whiggism," *History of Science*, 1983, 21:45–59, on p. 54.

<sup>26</sup> See, e.g., Carolyn Merchant, "Isis' Consciousness Raised," *Isis*, 1982, 73:398–409; Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven: Yale Univ. Press, 1985); and Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge, Mass.: Harvard Univ. Press, 1989).

tree; the postmodernist looks instead for the leaves—even though, now that it is autumn for Western historiography, the leaves are blowing off the tree. Their importance is not the place they once had on the tree but “the pattern we can form from them *now*.” Thus we should “let go” of the historical context. “History here is no longer the reconstruction of what has happened to us in the various phases of our lives, but a continuous playing with the memory of this. The memory has priority over what is remembered. . . . The time has come when we should *think* about the past, rather than *investigate* it.”<sup>27</sup>

This extravagant thesis provoked a revealing reply from Perez Zagorin. Postmodernist history must be rejected because it abandons even the pretense of objectivity; it fails to retain history’s distinction between fact and fiction: “The historical work presents itself as consisting, to a great degree, of facts and true or probable statements about the past.”<sup>28</sup>

I would not argue that postmodernism offers a satisfactory or even a coherent alternative to contextualism. Indeed, I have no strong objection to contextualism itself as one approach among several. I simply reject its claims to a monopoly over historical writing. The recent criticisms of contextualism do not show that it should be abandoned, but may serve the useful function of forcing historians of science to rethink and reformulate their assumptions, even if for no other reason than to avoid appearing hopelessly out of date to their colleagues in the humanities.

Liberation from the contextualist straitjacket could also allow historians of science to remedy one of the major shortcomings of our profession: failure to meet the widespread demand for broad synthetic overviews of the subject suitable for students and the public. This failure might be blamed on Butterfield’s call for detailed studies of special problems as the only legitimate form of professional historical writing and his denigration of surveys as misleading “abridgments”—although he himself produced the successful survey of the history of science noted above.<sup>29</sup> To adapt Albert Einstein’s famous advice: If you want to find out what methods historians use, don’t listen to their words, fix your attention on their deeds!<sup>30</sup>

Finally, if historians of science can get over their fear of presentism, they could participate more freely in the discussion of contemporary problems, where politicians and others pontificate about the “lessons of history.” Forman notes that Hunter Dupree, a highly respected historian of American science, was not reluctant to offer history-based advice on science policy. Lewis Pyenson reminds us that when our discipline was founded, one of its major goals was supposed to be “clarity to act in the present on the basis of an understanding of the past.” In particular, Loren Graham suggested that historians of science could play a useful role on advisory boards

<sup>27</sup> Michel Foucault, *The Archaeology of Knowledge* (New York: Pantheon, 1972), on p. 5; and F. R. Ankersmit, “Historiography and Postmodernism,” *Hist. Theory*, 1989, 28:137–153, on pp. 148, 150, 152. See also Dominick LaCapra, *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, N.Y.: Cornell Univ. Press, 1983); Harlan, “Intellectual History” (cit. n. 19); and Gabrielle M. Spiegel, “History, Historicism, and the Social Logic of the Text in the Middle Ages,” *Speculum*, 1990, 65:59–86.

<sup>28</sup> Perez Zagorin, “Historiography and Postmodernism: Reconsiderations,” *Hist. Theory*, 1990, 29:263–274 on p. 272. See also Ankersmit, “Reply to Professor Zagorin,” *ibid.*, pp. 275–296.

<sup>29</sup> Butterfield, *Whig Interpretation of History* (cit. n. 6); the survey is Butterfield, *Origins* (cit. n. 13). See also Hall, “On Whiggism” (cit. n. 25); and C Hakfoort, “The Missing Syntheses in the Historiography of Science,” *Hist. Sci.*, 1991, 29:207–216.

<sup>30</sup> Albert Einstein, *On the Method of Theoretical Physics* (Oxford: Clarendon Press, 1933).

such as those dealing with medical ethics. In fact a historian, Nicholas Steneck, was recently appointed chair of a Public Health Service Advisory Committee on Scientific Integrity.<sup>31</sup>

## II. HISTORY OF SCIENCE OR HISTORY OF SCIENTISTS?

In 1966 Hunter Dupree announced to the American Historical Association that the field of the history of American science had "found itself." It had done so, he claimed, because historians, following the advice of Arthur M. Schlesinger, Sr., had recognized the central role of science in recent American history and had refused to let their ignorance of science prevent them from writing about it. Rejecting Sarton's demand that historians should be "deeply familiar with at least one branch of today's science" and "should have a more superficial acquaintance with various other branches," Dupree argued that they could dispense with such scientific training, since they are "not interested in the subject matter of the sciences per se . . . the history of science properly does not concern itself with the things of science: the plants, the animals, the molecules, the atoms, the ether, the quanta, or even the law or the equation." Their subject was the scientists and their relation to society: "The only object of study in the history of science is *Homo sapiens*, and since a scientist without communication is hard to conceive of, it is *Homo sapiens* in a social context that is the sole object of the historian's study of science."<sup>32</sup>

When Dupree said he was "not interested in the subject matter of the sciences," he meant not *professionally concerned* in his capacity as a historian; many of his works show that he does have an interest, in the sense of *curiosity*, in scientific matters. But some younger historians are "not interested" in a more literal sense—and not only indifferent but hostile. David Knight says that he and his colleagues are "tired of meeting the assumption that they must be interested in how things turned out; as though the student of the Austro-Hungarian Empire should necessarily be concerned about local government in present-day Budapest." For them, science is no longer the heroic enterprise it had seemed in the nineteenth century; now it has an "ambiguous" reputation. In a culture where there is widespread "alarm and unease about science, . . . there is no reason why its historian should also be its apologist."<sup>33</sup> Skepticism about the moral stature (and epistemological authority) of science pervades the comments of a dozen British historians invited to address the question "What Is the History of Science?" in the magazine *History Today*.<sup>34</sup>

Is alienation from science a prerequisite for objective study of its history? Anglo-American historians of science will not usually say that in print, but a Japanese

<sup>31</sup> Paul Forman, "1990 Sarton Medal Citation," *Isis*, 1991, 82:281–283; Lewis Pyenson, "What Is the Good of History of Science?" *Hist. Sci.*, 1989, 27:353–389; Graham, "Why Can't History Dance Contemporary Ballet?" (cit. n. 18); and David L. Wheeler, "NIH Director Seeks Changes in Office of Scientific Integrity," *Chronicle of Higher Education*, 24 July 1991, pp. A1, A6.

<sup>32</sup> This paragraph is adapted from an earlier article: Stephen G. Brush, "Looking Up: The Rise of Astronomy in America," *American Studies*, 1979, 20(2):41–61. For the Dupree and Sarton quotations see A. Hunter Dupree, "The History of American Science—A Field Finds Itself," *Amer. Hist. Rev.*, 1966, 71:863–874, on pp. 865, 869. For Schlesinger's advice see Schlesinger, "An American Historian Looks at Science and Technology," *Isis*, 1946, 36:162–166.

<sup>33</sup> David M. Knight, "The History of Science in Britain: A Personal View," *Zeitschrift für Allgemeine Wissenschaftstheorie*, 1984, 15:343–353, on p. 344.

<sup>34</sup> See, e.g., Roger Cooter, "What Is the History of Science?" *History Today*, April 1985, No. 35, pp. 32–33, and following articles.

scholar, Shigeru Nakayama, has frankly admitted that for him and his colleagues history of science is “a subject for the frustrated.” A person who is content with the way science is done may not be “a good critical historian of science” but will only write “a self-congratulatory narrative” or become “a dull, bureaucratic archivist”; those who reject the norms of the scientific community can borrow the authority of history to support their critique of that community.<sup>35</sup>

But this creates a dilemma for a historian who needs the cooperation of scientists. According to Joan Bromberg, “historians of modern physics have pusillanimity as an occupational hazard. We depend upon the good graces of the scientists we treat for our raw material—the interviews, access to archival and personal collections, even, occasionally, our funding. The temptation to treat scientists with kid gloves is enormous.”<sup>36</sup>

This temptation is resisted by Paul Forman, who proclaims independence from science as crucial for the historian. Forman argues that scientists relinquish moral responsibility for their work by embracing the “transcendence” of science, but historians must not do so. The historian should not accept the scientist’s judgment as to what is true or important, based on appeal to a transcendent reality, but instead should undertake an independent analysis based on “mundane factors and human actors.” When scientists write history, they celebrate the success of science, but this “always involves some degree of mendacity” which “must inevitably prove damaging to historiographic practice.” Since, Forman asserts, scientific knowledge is socially constructed, the historian “must focus either on social problems of science or on science as a social problem.”<sup>37</sup>

Forman had earlier carried his attack on science-history into the enemy camp with a slashing critique of Jagdish Mehra’s work in the widely read journal *Science*. Reviewing the first four volumes of *Historical Development of Quantum Theory*, written by Mehra with Helmut Rechenberg, Forman accused Mehra of “intellectual poverty, pompous pretension, depreciation of the quantity and significance of the extant historical writing in the field.” Like those “without experience in historical research,” Mehra (according to Forman) overestimates the value of recollections of the major actors. He assumes “that only one who was there . . . could know the true but secret history of his field.” The historian, in contrast to the scientist, wants independence from the judgments of the participants. Forman faults Mehra and Rechenberg for using few of the many extant letters that could provide a more accurate record, for being uncritical of their sources and failing to give enough documentation for them, and for using the work of other historians without adequate credit.<sup>38</sup>

<sup>35</sup> Nakayama Shigeru, “The History of Science as a Subject for the Frustrated,” in *Science and Society in Modern Japan: Selected Historical Sources*, ed. Nakayama et al. (Tokyo: Univ. Tokyo Press, 1974), pp. 3–16 (previously published as “The Externalist Orientation of Japanese Historians of Science,” *Japanese Studies in the History of Science*, 1972, 11:1–10). See also Morris Fraser Low, “The History of Japanese Physics and the Rise of the Scientist-Historian,” review of *Proceedings of the Japan–USA Collaborative Workshops on the History of Particle Theory in Japan, 1935–1960: Japan–USA Collaboration, Second Phase, Historia Scientiarum*, 1989, 36:117–120.

<sup>36</sup> Joan Bromberg, “When Physicists Served Fascism,” *Science and Nature*, 1982, No. 5, pp. 143–145, on p. 145.

<sup>37</sup> Forman, “Independence, Not Transcendence” (cit. n. 4), p. 83, attributing the phrases and some of the arguments to Jerome Ravetz, Sal Restivo, and Robert Merton.

<sup>38</sup> Paul Forman, “A Venture in Writing History,” *Science*, 1983, 220:824–827, reviewing Jagdish Mehra and Helmut Rechenberg, *The Historical Development of Quantum Theory*, 5 vols. (New York: Springer-Verlag, 1982–1987).

Forman is not simply attacking a particular piece of science-history that he considers badly done; he makes it clear that he considers these shortcomings to be characteristic of *most* history written by scientists: "For scientists history is not the field upon which they wrestle for truth, but principally their field of celebration and self-congratulation. . . . History written in celebration of heroic ages and agents can scarcely avoid becoming propaganda pandering to the scientists' *amour propre*. To this danger Mehra has wholly succumbed." Forman concluded by scolding those who, just because they were "pleased and flattered" by the attention of Mehra and Rechenberg, supported their project—an insult which drew an indignant response from one of those supporters, Ilya Prigogine.<sup>39</sup>

Scientists have been more polite and less vociferous in their criticisms of non-scientists who write history of science. In 1950 Einstein told Robert Shankland: "Nearly all historians of science are philologists and do not comprehend what physicists are aiming at, how they thought and wrestled with their problems. Even most of the work on Galileo is poorly done."<sup>40</sup> At a 1967 conference of historians and physicists, all the physicists shook their heads in disagreement when a historian suggested that in the 1930s social factors rather than scientific merit determined access to accelerators.<sup>41</sup> Commenting on works by historians, scientists occasionally lament the absence or inaccuracy of the description of science and the (to them) inappropriate emphasis on personalities and social and institutional history. An engineer put it more strongly: reading a biography that fails to explain its subject's technical achievements is like "a biography of Babe Ruth written for people who have never seen a game and who aren't interested in learning much about the rules."<sup>42</sup>

The strongest criticism of writings by nonscientist historians has come from other nonscientist historians, especially Charles Gillispie. Delivering the Sarton Lecture at the annual meeting of AAAS in 1980, Gillispie questioned the profession's shift away from analysis of the substance of science toward discussion of cultural influences. The text of this lecture has not been published but Gillispie supplied the following abstract:

<sup>39</sup> Forman, "Venture"; and Ilya Prigogine, "History of Quantum Theory," *Science*, 1983, 221:604. Prigogine did review the Mehra-Rechenberg books quite favorably, in *Foundations of Physics*, 1984, 14:275–277, and 1987, 17:1131–1136. In his second review he remarked that they show "the grand forward march" of quantum mechanics and inform the reader about "a turning point in the history of mankind." Another scientist also praised the work and was especially pleased by the use of interviews, which had been turned into "accounts that ring very true"; see Nicholas Kemmer, "Jump in Quantum History," *Nature*, 1988, 332:745–746.

<sup>40</sup> R. S. Shankland, "Conversations with Albert Einstein" *American Journal of Physics*, 1963, 31: 47–57.

<sup>41</sup> Charles Weiner, ed., *Exploring the History of Nuclear Physics* (New York: American Institute of Physics, 1972), p. 47.

<sup>42</sup> M. Granger Morgan, "The Wizard of Schenectady: An Unresolved Portrait," *American Scientist*, 1993, 81:182–183, on p. 182. Cf. Eugene S. Rochow, letter to the editor, *Isis*, 1989, 80:664; A. G. W. Cameron, review of *Nucleus: The History of Atomic Energy of Canada Limited*, by Robert Bothwell, *Physics Today*, 1989, 42(5):78–79; and G. W. Wetherill, "Asteroid Paradox," *Science*, 1990, 247: 1386–1387. Historians of science may resent criticism of their work on such grounds: see, e.g., Geoffrey Cantor, letter to the editor, *British Society for the History of Science Newsletter*, Sept. 1991, pp. 30–31. For similar complaints by nonscientist historians see "Science in Medicine," *Osiris*, 1985, 1:37–58, on pp. 37–38, 48–49; Leonard Wilson, "Medical History without Medicine," *Journal of the History of Medicine*, 1980, 35:5–7; I. B. Cohen, review of *The Launching of American Science, 1846–1876*, by Robert Bruce, *Nature*, 1987, 329:209–210; R. W. Home, review of *Entstehung des modernen Systems wissenschaftlicher Disziplinen: Physik in Deutschland, 1740–1890*, by Rudolf Stichweh, *Social Studies of Science*, 1990, 20:761–763; and Alan E. Shapiro, review of *Reappraisals*

In the last decade scientists have been subjected to a barrage of summonses to every sort of responsibility—ethical, environmental, economic, technological, social, political, military, and whatnot. In writings on the history of science, emphasis on the effects of science has produced such a shift in discourse that a large proportion of scholarship is now addressed to institutional and social factors accompanying the development of science rather than to technical and intellectual context. The change in fashion is in keeping with a widespread politicization and socialization of historical scholarship in general, and indeed of all scholarship—whether for good or for ill, the future may reveal more clearly than the present. In any case, the picture of science is drawn in political and social brush strokes. It conveys little of what science has achieved in the way of knowledge or even technique, and scientists are treated as actors in the political and social process like anybody else. The Sarton lecturer has no doubt that there is much to be learned from all this, but does sometimes feel concerned lest matters go too far and the baby drown in the bath. The lecture will be a modest plea to scientists to pay attention to what historians—and other social scientists—are making of their enterprise, and will venture to urge upon them yet another responsibility: that of exercising a measure of vigilance, at least over the references to technical matters that even the most externally minded commentators cannot altogether avoid, and perhaps more largely over the impression that is conveyed to the public of the experience of being a scientist.<sup>43</sup>

The lecture was reported in *Science* under the inflammatory headline, “History of Science Losing Its Science,” with the opening sentence: “Once a highly respected field that focused on the conceptual evolution of scientific ideas, the history of science is losing its grip on science, leaning heavily on social history, and dabbling with shoddy scholarship.” That report provoked a letter from the historian Robert Kohler, who defended the standards of the profession as being different from those of previous generations but not inferior; he did not directly address the issue of competence or interest of historians in dealing with the technical aspects of science.<sup>44</sup>

Another historian, Nathan Reingold, discussed the controversy at greater length a year later, expressing his surprise at “the firm support of a few historians in private conversations” for Gillispie’s position but insisting that “a majority of the teachers and graduate students in the field have scientific backgrounds which inevitably influence their work.” While conceding that the proportion of internalist work in history of science might have declined from more than 80 percent in the period 1955–1960 to less than two thirds, he suggested that part of the increase in nontechnical writings was generated by economists, sociologists, philosophers, and so forth, rather than by professional historians of science. More significantly, after describing the “emerging consensus in the history of science” as reflecting “influences from history, the social sciences, and the philosophy of science,” Reingold cited as the single best example of this consensus a book, *Scientists in Power*, written by Spencer Weart—a historian of science who took a doctorate in astronomy.<sup>45</sup>

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*of the Scientific Revolution*, ed. David C. Lindberg and Robert S. Westman, *Science*, 1990, 250:1600–1601.

<sup>43</sup> Charles C. Gillispie, abstract for “Is the Inwardness of Science Extraneous to Its History?” Sarton Lecture at Annual Meeting of American Association for the Advancement of Science, 1980 (unpublished).

<sup>44</sup> William J. Broad, “History of Science Losing Its Science,” *Science*, 1980, 207:389; and Robert Kohler, “History of Science: Perceptions,” *Science*, 1980, 207:934–935. See also Gillispie, “History of Science: Perceptions,” *ibid.*, p. 934.

<sup>45</sup> Nathan Reingold, “Science, Scientists, and Historians of Science,” *Hist. Sci.*, 1981, 19:274–283, quoting from pp. 274, 276, 280.

What Einstein wanted from the history of science, and what at least one reader of Reingold's paper found missing there, was an understanding of "the intense desire of research scientists to produce new basic discoveries"—an "intense excitement in the workings of nature" which takes priority over any future consequences of the discoveries.<sup>46</sup> That does not mean that historians must approve of the values of the scientist or share in what Forman calls the "transcendence" of science. It does mean that there is a place in our discipline for *some* historians who have enough personal experience in scientific research to describe what it feels like. As Gerald Holton interprets Einstein's statement, we need "a particular kind of historical sense, one that largely intuits how a scientist may have proceeded, even in the absence of 'the real facts' about the creative phase."<sup>47</sup>

### III. DISCOVERY VERSUS CONSTRUCTION

The transcendence of scientific research relies on the implicit assumption that one is objectively uncovering true facts about the world, not just making them up. The independence of historical research includes the freedom to argue that facts as well as theoretical concepts are subjectively constructed with the help of philosophical preconceptions, social interests, and rhetorical devices. Scientists, like historians or anyone else, may be dismayed if such "construction" arguments undermine the credibility of their work. But it is also scientists, acting as or in cooperation with historians, who have a great deal to say about the validity of construction arguments.

For an earlier generation of historians of science, the most famous construction argument was Alexandre Koyré's claim that Galileo did not actually perform the experiments he mentioned in his dialogues, but instead described how they *must* come out on the basis of his Platonic presuppositions about how nature works. Koyré supported his claim by asserting that if Galileo had really done those experiments with the apparatus available to him, he would have obtained different results, or at least results not as accurate as he stated. But Koyré did not do the experiments either, relying on his own presuppositions about how nature works as well as about how Galileo worked. In the 1960s the discussion moved to a different plane when Thomas Settle and other historians of science with competence in physics tried to reproduce these experiments with the kinds of apparatus used in the seventeenth century. As a result of this contribution from science history, we now have a much better understanding of the role of philosophical preconceptions in Galileo's work, and the extent to which his results might have been constructed rather than discovered—apparently much less than Koyré thought.<sup>48</sup>

Science history has also played a role in the historiography of John Dalton's chem-

<sup>46</sup> M. A. B. Whitaker, "Science, Scientists, and History of Science," *Hist. Sci.*, 1984, 22:421–424.

<sup>47</sup> Gerald Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein*, rev. ed. (Cambridge, Mass.: Harvard Univ. Press, 1988), p. 346.

<sup>48</sup> Alexandre Koyré, *Études galiléennes* (Paris: Hermann, 1939); Koyré, *Metaphysics and Measurement* (Cambridge, Mass.: Harvard Univ. Press, 1968); Thomas Settle, "An Experiment in the History of Science," *Science*, 1961, 133:19–23; Settle, "Galileo and Early Experimentation," in *Springs of Scientific Creativity*, ed. R. Aris et al. (Minneapolis: Univ. Minnesota Press, 1983), pp. 3–20; James MacLachlan, "A Test of an 'Imaginary' Experiment of Galileo's," *Isis*, 1973, 64:374–379; MacLachlan, "Galileo's Experiments with Pendulums: Real and Imaginary," *Ann. Sci.*, 1976, 33:173–185; and Stillman Drake, "Galileo's Experimental Confirmation of Horizontal Inertia: Unpublished Manuscripts (Galileo Gleanings XXII)," *Isis*, 1973, 64:291–305.



ical atomic theory. J. R. Partington and Leonard Nash both tried to replicate one of Dalton's experiments; both concluded that since the results were not as reported by Dalton, they must have been adjusted later to illustrate the theory rather than providing the inspiration for it as Dalton claimed.<sup>49</sup> The issue here is not so much whether Dalton really constructed his experimental facts from his theory, but whether that issue can even be discussed by an historian who does not have some competence in chemistry.

Recently it has been suggested that Robert Brown did not actually observe the kind of particle-movement now named after him.<sup>50</sup> Again, the technical expertise of the scientist is needed to test this historical hypothesis.

It is taken for granted that professional historians can read original sources in the language in which they are written. But what about mathematics, the language of science? Ivor Grattan-Guinness points out that mathematics is simply ignored in most current writing on the history of science.<sup>51</sup> By default, historians of science have relinquished to practitioners not only the history of mathematics but topics like the development of general relativity, even though, as John Stachel puts it, "the skills of trained historians of science are badly needed, not least in the formulation of the right questions to be answered by future research."<sup>52</sup>

Even in the history of biology some mathematical competence is needed. A vexing question in Mendel scholarship is whether the venerable abbé "cooked" the data he reported in support of his theory. The question is asked only because a scientist, R. A. Fisher, pointed out that the data are, from a statistical viewpoint, too good to be true. Obviously a mathematician-historian like B. L. van der Waerden will have an advantage over most historians of biology in discussing this question.<sup>53</sup>

A more general problem for any attempt to write a social history of science is that social influences on the detailed process of research can be discerned only if one understands the process itself at a technical level.<sup>54</sup> The most successful and convincing studies of the social aspects of science are precisely those whose authors are willing to immerse themselves in the life of the laboratory, not as an anthropologist visiting from an alien culture but as a participant-observer. Whether or not the historian has originally been trained as a scientist, she or he needs to acquire some knowledge of how research was done by the scientists being studied. Those who lack this knowledge can still do useful work on other aspects of the history of science (as Schlesinger and Dupree argue) but cannot claim to provide a *complete* description of the scientific enterprise.

<sup>49</sup> J. R. Partington, "The Origins of the Atomic Theory," *Ann. Sci.*, 1939, 4:245-281; and Leonard K. Nash, "The Origin of Dalton's Chemical Atomic Theory," *Isis*, 1956, 47:101-116.

<sup>50</sup> Daniel Deutsch, "Did Robert Brown Observe Brownian Motion: Probably Not," *Bulletin of the American Physical Society*, 1991, 36:1374; and John Rennie, "A Small Disturbance," *Scientific American*, 1991, 265(2):20.

<sup>51</sup> I. Grattan-Guinness, "Does History of Science Treat of the History of Science? The Case of Mathematics," *Hist. Sci.*, 1990, 28:149-173.

<sup>52</sup> John Stachel, introduction to *Einstein and the History of General Relativity*, ed. D. Howard and J. Stachel (Boston: Birkhäuser, 1989), pp. 1-4. See also B. Bertotti *et al.*, eds., *Modern Cosmology in Retrospect* (Cambridge: Cambridge Univ. Press), pp. xi-xx.

<sup>53</sup> R. A. Fisher, "Has Mendel's Work Been Rediscovered?" *Ann. Sci.*, 1936, 1:115-137; Joan Fisher Box, *R. A. Fisher: The Life of a Scientist* (New York: Wiley, 1978), pp. 296-300; and B. L. van der Waerden, "Mendel's Experiments," *Centaurus*, 1968, 12:275-288.

<sup>54</sup> F. L. Holmes, "The Fine Structure of Scientific Creativity," *Hist. Sci.*, 1981, 19:60-70; and Holmes, *Lavoisier and the Chemistry of Life* (Madison: Univ. Wisconsin Press, 1985), pp. 501-502.

## IV. CONCLUDING REMARKS

There is no doubt that much historical writing by scientists is whiggish or merely celebratory. Some scientists acknowledge this criticism but publish their memoirs anyway, leaving it to the reader to decide what has historical value.<sup>55</sup> Autobiographies, interviews, and efforts to preserve documents are all useful contributions that scientists can make to history without becoming historians themselves.<sup>56</sup> There is a vast amount of “amateur” writing on history of science which professional historians treat as evidence of how scientists perceive their own enterprise and how they justify it to the public, rather than as historical scholarship. But the issue here is the attitude of professional historians of science toward those scientists who have taken seriously the criticisms of science history noted above, and have made a conscious effort to appreciate (and to some extent adopt) the methods and attitudes of the historian. The ultimate justification for the participation of scientists in writing the history of science must rest on the quality of their *best* work. That work should not be denigrated if it starts from presentist questions, employs skills and knowledge from modern science, and ignores the current fad for social and institutional history. Nor should one overlook the fact that some historians with solid credentials in science have become leaders in social and contextual analysis.

In the past, science historians like Partington, Pierre Duhem, and Joseph Needham pioneered in mapping large regions of our subject—chemistry, the Middle Ages, and China, respectively. Without their efforts to build on, our knowledge of those regions would now be much less advanced than it is.

To assess the more recent contributions of historians who have earned doctorates in the sciences, one can use (with some caution) the membership directory of the History of Science Society and the indexes to its journals. Anyone may join the History of Science Society (by paying the dues), but only those papers that satisfy the scholarly criteria of the discipline can get published in *Isis* or *Osiris*. For example, among those who hold a doctorate in physics we find the following major contributors to the history of physics: Geoffrey Cantor, Allan Franklin, Peter Galison, Gerald Holton, Martin J. Klein, Thomas S. Kuhn, Arthur I. Miller, Andy Pickering, Sylvan S. Schweber, Daniel Siegel, Roger Stuewer, and M. Norton Wise. This list could easily be enlarged by including others with doctorates in physics who did not indicate that in the directory entry, and several who have published monographs or articles in other history of science journals.<sup>57</sup> But even as it stands, this list already includes the authors of a substantial portion of the best work (by any standards) in the history of modern physics; their publications at the same time display a striking diversity of styles and approaches. Probably a similar statement could be made by someone familiar with recent scholarship in the history of biology, geology, or any other science.

I would not argue that science historians who have published in history of science journals share any single historiographic perspective, or even that all of them make

<sup>55</sup> Walter H. Stockmayer, “When Polymer Science Looked Easy,” *Annual Review of Physical Chemistry*, 1984, 35:1–21.

<sup>56</sup> E. N. Hiebert, review of *Fifty Years of Neutron Diffraction*, ed. G. E. Bacon, *Archives Internationales d’Histoire des Science*, 1990, 40:129–130.

<sup>57</sup> P. Thomas Carroll, ed., *Guide to the History of Science*, 8th ed. (Philadelphia: History of Science Society, 1992).

use of their scientific training in their historical research. But at least some of them can remedy one of the shortcomings of contextualism as it is often practiced: I mean the tendency to focus so strongly on what happened at one particular time, and on the events *before* that time, that one overlooks the impact of the events on the *subsequent* development of science. Daniel Siegel writes, in connection with the analysis of Maxwell's electromagnetic theory:

The fantasy has been entertained of having the history of science written by individuals who do not know the modern theories, and who will therefore be able to enter into the spirit of past theories unprejudiced. Such individuals, however, would be ill situated to interpret the theories of the past for audiences acquainted with the modern theories, and would be ill equipped to discern patterns of development leading from past to present. It is just as well, then, that there are historians of science who are conversant with modern science.<sup>58</sup>

Siegel's goal is to understand Maxwell's theory in its nineteenth-century context, but in a more detailed way that can be connected with later developments.

To summarize: scientists have much to contribute to the history of science, and there are certain kinds of important questions that can be discussed only by those who have considerable technical background. Holmes has pointed out that "context" by itself is only a background that situates a foreground:

The study of what is variously referred to as the "intellectual" history of science, the "internal dynamic," or the "cognitive" side of intellectual development is as fresh and new, as underdeveloped, as urgently in need of more concentrated, penetrating analysis as is the study of the "social dimension." . . . [T]he study of these subjects should remain at the heart of the discipline of the history of science; for it is only through a profound understanding of these subjects that we can know *what it is* that the various contexts surround.<sup>59</sup>

In this enterprise those scientists who are willing to learn historical methods and study original sources have a continuing and essential role to play.

<sup>58</sup> Daniel Siegel, "The Origin of the Displacement Current," *Historical Studies in the Physical and Biological Sciences*, 1986, 17:99–146, on p. 101.

<sup>59</sup> Holmes, "Fine Structure" (cit. n. 54), on p. 60.

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#### <sup>3</sup> **On Discipline Building: The Paradoxes of George Sarton**

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#### <sup>4</sup> **Independence, Not Transcendence, for the Historian of Science**

Paul Forman

*Isis*, Vol. 82, No. 1. (Mar., 1991), pp. 71-86.

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### <sup>18</sup> **In Defense of Presentism**

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<sup>28</sup> **Historiography and Postmodernism: Reconsiderations**

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<sup>42</sup> **Letters to the Editor**

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