ARNOLD THACKRAY

Science: Has Its Present Past a Future?

Before you study the history, study the historian . . . Before you study the historian, study his historical and social environment.

E. H. CARR, *What Is History?*

The fact is that the historian projects into history the interests and the scale of values of his own time . . .

ALEXANDRE KOYRE, *Scientific Change*

To judge from the pages of *Isis*, the atom has not yet been split. The *Annals of Science* apparently do not include secret research of any kind. Those advances in chemical and biological understanding which now allow plague and disease to be spread around the world at will find no place in the *Archives Internationales d'Histoire des Sciences*. *History of Science* (an annual avowedly devoted to the highlighting of "outstanding historical problems") displays no curiosity over the industrial and military pressures which have shaped and sustained the unparalleled scientific activity of the last three decades. The situation is no different if one turns to any of the dozen or so other journals now serving the history of science.

It is not only that the supremely important scientific revolution through which we have all lived seemingly invites no curiosity. *Perhaps more critical is the fact that historians of science have signally failed to make the pressures and perplexities of this revolution the springboard of their inquiries into other periods and problems.* As a newly established professional discipline, the history of science is undeniably born out of the tensions and aspirations engendered by modern science. How paradoxical then that such tensions and aspirations apparently hold no interest for the recently legitimized practitioners of this discipline. Yet it would be a mistake to suppose that the present world does not deeply, and perhaps perversely, affect the historiographic assumptions of this new profession. After all, withdrawal is every bit as much a response as engagement.

AUTHOR'S NOTE: I owe thanks to many colleagues for their generous response to, and thoughtful criticism of, earlier versions of this paper. Though my debts are far wider, I particular wish to acknowledge the help so freely afforded by Yehuda Elkana, Mary Hesse, Russell McCormmach, Bruce Mazlish, Everett Mendelsohn, Jerome Ravetz, and Charles Rosenberg. I am also grateful to the National Science Foundation for partial support of this work.

The historiographic assumptions that have now characterized Western history of science for a generation are well known and easily accessible.[1] At the same time these assumptions are curiously devoid of historical analysis. To begin that analysis, and to explore the historical, sociological, and intellectual roots of prevailing presuppositions, is one purpose of this present essay. To consider newer and still neglected alternatives, another. Historians of science have so far seemed averse to any sustained debate over historiography and ideology. It is the writer's firm conviction that such debate, and the reordered priorities it will induce, are urgently needed.[2]

II

It is not necessary here to invoke the sacred names of Tannery, Duhem, and Sarton, still less of such "precursor" historians of science as Delambre, Whewell, and Kopp. Instead it seems more fruitful to concentrate on the 1930's. That was truly a golden decade for innovation and debate within the history of science, and the discipline still lives within its shadow. In the thirties previous analyses (often antiquarian in intent and amateur in ap-

[1] These assumptions are best displayed in the essays dating from 1943 to 1960, now collected in Alexandre Koyré, *Metaphysics and Measurement: Essays in the Scientific Revolution* (London: Chapman and Hall, 1968). See also such widely read texts as Herbert Butterfield, *The Origins of Modern Science, 1300–1800* (London: Bell, 1957), Charles C. Gillispie, *The Edge of Objectivity* (Princeton, N.J.: Princeton University Press, 1960), and A. Rupert Hall, *From Galileo to Newton, 1630–1720* (London: Collins, 1963).

[2] The early 1960's showed a still unfulfilled promise of critical thought on historiographic issues. At a 1961 symposium Henry Guerlac voiced a timely and unheeded warning of the distortions inherent in "the newer history of science with its strong flavour of idealism and super-rationalism." His remarks, and a spirited response by Koyré, appear in *Scientific Change*, ed. A. C. Crombie (London: Heinemann, 1963), pp. 797–812 and 847–857. A more extended defense of "the newer history of science" also appeared in 1963, in A. R. Hall's "Merton Revisited, or Science and Society in the Seventeenth Century," *History of Science*, vol. 2, pp. 1–16. The same year also saw publication of Joseph Agassi's supporting tract "Toward an Historiography of Science," *History and Theory*, supplement 2. Since that time there has been only limited sniping in the book review sections of learned journals. The solitary exception is the historiographic commentary embedded in T. S. Kuhn's survey of "The History of Science" in *The International Encyclopedia of the Social Sciences*, ed. David L. Sills (New York: Macmillan, 1968). Mention must also be made of A. R. Hall's "Can the History of Science Be History?" *British Journal for the History of Science*, 4 (1969), 207–220, which appeared as this present essay went to press. Hall's courteous and temperate response to this "friendly critic" well displays his own very different historiographic position.

proach) were replaced by what may now be seen as three types of protoprofessionalism.[3]

The first of these was Marxian in form. In an age of economic depression and looming fascism it is not surprising that Marxian analyses held the center of the stage. Equally reasonably, England was the locus of both the major discussions and the major historical concern. Professor B. Hessen's classic 1931 paper on "The Social and Economic Roots of Newton's *Principia*" was read in London at the Second International Congress of the History of Science. The paper brilliantly exemplified the way in which economic determinism could contribute to rewriting the history of science. Spurred both by this example and by contemporary events, a loosely linked group of English scientists set to work to write those Marxist histories that would illuminate the previous growth and present condition of the scientific estate. J. D. Bernal, J. G. Crowther, Lancelot Hogben, and Joseph Needham come immediately to mind as members of this group.[4] Lacking younger disciples, for reasons we shall shortly explore, this English coterie has yet remained highly productive, if remarkably captive to its youthful ideas and pointedly ignored by present-day professional historians of science.

The basic position of the Marxist writers may be simply stated. In Hessen's words: "The method of production of material existence conditions the social, political and intellectual process of the life of society." Interpreted in a weak way, such a statement would be hard to quarrel with. But the Marxist interpretation was anything but weak. Hessen's own essay abounds with assertions like "The struggle of the university, and non-university science serving the needs of the rising bourgeoisie, was a reflection in the ideological realm of the class struggle between the bourgeoisie and feudalism." Or again, "Science flourished step by step with the development and flourishing of the bourgeoisie. In order to develop its industry, the bourgeoisie needed science . . ."[5] Such simplistic sloganeering weakens the writing of this whole school, including Bernal's otherwise impressive *Science in History*. Published almost a quarter century after Hessen, *Science in History* represents a last, late flowering of the Marxist tradition. It bravely insists that "Greek science reflects the rise and decline of the money-dominated, slave-owning iron age society. The long interval of the Middle Ages marked the growth and instability of feudal subsistence economy with little use for science. It was not until the bonds of feudal order were broken by the rise of the bourgeoisie that science could advance. . . . The phases of the evolution of modern science mark the successive crises of capitalist economy,"[6] etc. Sufficient to say that by the time Bernal's work was published the relevance of such analyses had long been overtaken by changes in the political form and climate of the Western world.

The second type of protoprofessionalism was both more complex and less influential. Its *locus classicus* is R. K. Merton's 1938 monograph on "Science, Technology and Society in Seventeenth-Century England." Though indebted to Marxist canons and concerns in many ways, this monograph was quite separate in methodology and intent. An extensive historicosociological investigation, it stands unparalleled and unpursued to this present day. The reasons for this neglect have been partially analyzed by Professor Hall. His judgment that Merton's work "represented the culmination of an established tradition, not the beginning of a new one,"[7] is not the least provocative of his many sallies. Even so one must admit that few historians of science have assayed the sociological inquiries that Merton envisaged. Weber's "Protestant ethic," Hessen's economic determinism, and the sociologist's statistical method have as a combination proved singularly unappealing to the new profession, while sociology itself has been passing through a prolonged antihistorical phase.

[3] As Agassi neglects to his cost, and Kuhn is careful to stress, the writing of histories of particular sciences has an exceedingly long and involved history. No adequate account of this history exists: the best introduction is Kuhn's "History" article. Here it is unnecessary to reach back beyond the 1930's, in which "amateur" activity reached a new peak and the embryonic professional discipline received enduring prenatal impressions.

[4] Other associates included C. H. Waddington, J. B. S. Haldane, the scientist manqué C. P. Snow, the anthropologist V. Gordon Childe, and the classicist Benjamin Farrington. Neither a more complete enumeration of the group nor a listing of their works is possible here. No more is it possible to discuss the extent of, and shifts in, the Marxism of the various members. For this, the analytical study now being conducted by Mr. Gary Werskey of Harvard and Edinburgh universities must be awaited. However, some insight into the period, and the influence of Hessen, may be gained from such works as J. G. Crowther, *British Scientists of the Nineteenth Century* (London: K. Paul, Trench, Trubner, 1935).

[5] Quoted from p. 170 of *Science at the Cross Roads* (London, 1931?). This fascinating period piece, which owns neither editor nor publication date, contains 11 papers presented by the Soviet delegation to the Second International Congress of the History of Science and Technology, held in London in 1931. On Hessen, see the remarks in David Joravsky, *Soviet Marxism and Natural Science, 1917–1932* (New York: Columbia University Press, 1961), *passim*.

[6] John D. Bernal, *Science in History* (London: Watts, 1954), p. xi. The revisions Bernal has made in successive editions of this work would constitute an interesting study.

[7] "Merton Revisited," p. 1. Merton's study was published as pp. 360–632 of volume 4 of *Osiris*. Its long-overdue separate publication is now announced.

The third influence of the thirties came out of a particular tradition in the history of philosophy, in its Gallic form. Distinct from Marxism and Merton as much in its fate as its approach, the work of Alexandre Koyré has captured the imagination of Western historians of science. If this assertion requires demonstration, it is only necessary to point to Professor Cohen's discussion of "the magistral influence" of Koyré, or to the remarks of Professors Clagett and Westfall. Clagett sees Koyré as having an "extraordinary influence on a generation of American scholars." In addition he stresses that "our students . . . were urged to take Koyré's studies as models," and notes "how dependent on his friendship and scholarship American historians of science had become [by the late 1950's]," and how they "sought him out repeatedly for advice and conversation." Going even further, Westfall asserts that "no single work has done more to shape the history of science as it is now practiced than Koyré's *Etudes galiléennes*." Indeed Westfall himself admits being "molded by them to the extent that I cannot see them objectively."[8]

Koyré and his followers have sought to stress at the same time both the autonomy of, and the conceptual shifts in, the developing pattern of scientific thought. Their conviction has been that (to quote Professor Hall) "the intellectual change is one whose explanation must be sought in the history of the intellect." Why this position should have proved so enticing invites investigation.

As spokesman for the group, Hall attributes the appeal of Koyré to the way he made "peculiarly his own" the "analysis of the scientific revolution as a phenomenon of intellectual history." Even so, Hall feels constrained to add that "other factors" aided this appeal.[9] Since it is not self-evident that Koyré's stress on the intellectual history of science surpasses in cogency that of such earlier writers in the same tradition as Lovejoy, Metzger, Dampier-Whetham, Burtt, or Meyerson,[10] to name only the most obvious, it may be rewarding to pause and explicitly discuss those "other factors" which have contributed to Koyré's wide and enduring influence.

[8] For the remarks of Marshall Clagett and I. Bernard Cohen see *Isis*, 57 (1966), 157–166. In saying of the *Etudes Galiléennes* that "more than any other work it has been responsible for the new history of science," Cohen both anticipated Westfall and illustrated the remarkable degree of unanimity among American historians of science. For R. S. Westfall's remarks, see *Science*, 162 (1968), 553.

[9] "Merton Revisited," pp. 10, 11.

[10] See, for example, Arthur O. Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge, Mass.: Harvard University Press, 1936), Hélène Metzger, *Newton, Stahl, Boerhaave et la Doctrine Chimique* (Paris: F. Alcan, 1930), William C. D. Dampier-Whetham, *A History of Science and Its Relations with Philosophy

III

The history of science emerged as a professional discipline in the Western world in the 1950's, primarily in the United States. North America could boast perhaps five professional historians of science in 1950, twenty-five in 1960, and probably a hundred and twenty-five by the time this article appears in print.[11] The fifties was thus the crucial decade for defining standards, agreeing on methods, enrolling students, and creating a discipline. It was also the decade of the H bomb, the Cold War, Senator Joseph McCarthy, loyalty oaths, militant anticommunism, and the "silent generation" of students. There were therefore unusually complex political, ideological, social, and professional factors at work in the shaping of this new discipline.

The emerging profession of history of science had obvious and urgent need of effective tools, and of analytical methods possessing demonstrated worth. While Merton's work suggested one approach, it lay in no clearly developed tradition of historical writing about science. In contrast, the English Marxists did offer an articulated body of knowledge, but one that had failed to generate a body of contributions of comparable quality in the more pragmatic climate of America, even in the 1930's. Most of the English writers had also failed to refine, broaden, and develop their methods with the passage of time. The notable exception of Joseph Needham illustrates how content the majority were with traditional themes, an unsubtle use of Marxist ideology, and broadly popular rather than serious scholarly endeavor. Well attuned to the mood of the thirties, their vulgarizing work was poor preparation for the postwar world and the debut of a new profession. What their writing did generate was a stage setting for Koyré in the careful and restrained critique of the historian G. N. Clark, and the important original researches of his student Rupert Hall.[12]

and Religion* (Cambridge: Cambridge University Press, 1929), Edwin A. Burtt, *The Metaphysical Foundations of Modern Physical Science: A Historical and Critical Essay* (New York: Harcourt, Brace, 1925), Emile Meyerson, *Identity and Reality*, trans. Kate Loewenberg (London: G. Allen and Unwin, 1930; original French edition, Paris, 1908).

[11] These figures are impressionistic estimates, but derive some support from D. J. de Solla Price, "Who's Who in the History of Science," *Technology and Society*, 5 (1969), 52–55, and "A Guide to Graduate Study and Research in the History of Science and Medicine," *Isis*, 58 (1967), 385–395. Throughout this present article, developments within the English-speaking world have been run together. A more careful differentiation of parallel and conflicting currents in Britain and the United States is obviously needed, but scarcely profitable within the limits of this paper.

[12] See G. N. Clark, *Science and Social Welfare in the Age of Newton* (Oxford: Clarendon Press, 1937), and A. Rupert Hall, *Ballistics in the Seventeenth Century* (Cam-

Within a few years the movement to professionalism was dominant, and its favored means discovered. The sophisticated style, steadily widening output, impeccable scholarly credentials, effortless command of the textual sources, and uncompromising idealism of Koyré all carried exciting implications for the emerging profession. His work thus served as a more than welcome model around which to organize the history of science. Unashamed Marxism, the only viable alternative analytic tool, labored under all too obvious handicaps in both England and America.

More subtle factors were also important. A social history of science, whether patterned on Marx or Merton, might well demand primarily *historical* training of its professional practitioners, and see its role as contributing to historical debate. But a *discipline* is created and defined by exclusion and special expertise, however rhetorically desirable cultural bridges and a widened scholarly embrace may seem. The history of science has drawn its first generation of both professional practitioners and graduate students almost exclusively from the sciences, rather than from history. It would be interesting to explore the reasons for this, but here it is the fact itself that matters.

Ex-scientists could scarcely claim that their particular expertise (their *discipline*) lay in some special grasp of historical method, or some broader understanding of the historical context that escaped the history Ph.D. Matters were obviously different when it came to their ability to comprehend and critically evaluate the *scientific concepts* of past science. Here a scientific training was of obvious relevance. Who better to understand the physics of the seventeenth or the chemistry of the eighteenth century than the ex-physicist or chemist? Who better also to stress theoretical and idealistic aspects of the history of science than the former natural scientist, the new and zealous convert to conceptual studies. That the history of science was often taught to potential, and read by actual, scientists, merely served to reinforce already powerful trends.[13] The accident of timing which made

bridge: Cambridge University Press, 1952). Hall's book was the outgrowth of a 1949 Cambridge Ph.D. thesis. While Needham's work extended the Marxist analysis in one direction, highly original and important, though unfairly neglected, studies in another direction were undertaken in the United States by Edgar Zilsel: see, for example, his "The Genesis of the Concept of Scientific Progress," *Journal of the History of Ideas*, 6 (1945), 325–349.

[13] Revealing comments on the teaching situation and the current operating philosophies of historians of science may be found in the papers and discussions of section 8 of *Critical Problems in the History of Science*, ed. Marshall Clagett (Madison: University of Wisconsin Press, 1959) and section 26 of *Scientific Change*, ed. A. C. Crombie (London: Heinemann, 1963). See also the articles of A. C. Crombie and G. Buchdahl

the 1950's the decade of professionalization thus happily coincided with the widespread intellectual and social reaction against all things Marxian, and the growing awareness of the alternative explanatory model offered by Koyré.

Perhaps too, the "loss of innocence of science" played a part. After Hiroshima, the technological consequences of "pure" scientific research no longer seemed such unmixed blessings. The triumphant progress of Western science was no longer quite so triumphant and perhaps—disturbing thought—not even progress. One natural consequence was a renewal of interest in past and golden days, days in which science was no more (or less) than pure and autonomous thought, unstained by the pressures of technology and politics. How soothing too to hear that "the science of our epoch, like that of the Greeks, is essentially *theoria*, a search for the truth . . . an inherent and autonomous . . . development."[14]

Even Professor Hall would seem implicitly to admit the validity of this analysis. Though he argues that "the intellectual change is one whose explanation must be sought in the history of the intellect" when scientific research is at issue, this criterion apparently does not apply to the discipline of Clio. Discussing the current vogue for intellectual rather than social approaches to the history of science, he notes that "the historical evolution of this situation is of historical significance too." Does he therefore seek the explanation of this intellectual change "in the history of the intellect"? Not at all. Instead Hall argues that social approaches are at a discount because of "a certain revulsion from the treatment of scientists as puppets," a situation in which we are all "guiltily involved," and one we cannot review "without passion."[15] That an internalist should seek the explanation of the evolution of historical thought in such social and psychological terms, while vigorously insisting on intellectual autonomy for scientific ideas alone, is at least worth noting.

The foregoing analysis is not in any way intended to deny the obvious quality and intellectual power of the work associated with Professors Clag-

in *History of Science*, 1 (1962), 57–66. The alliance formed between the history and the philosophy of science, each small discipline seeking to professionalize and enlarge, also worked to aid the identification of *science* with *scientific thought*, so common throughout this period. This whole development deserves more detailed treatment, but here can only be summarily noted—though see the exchange between R. H. Shryock and H. Dingle in *Proceedings of the American Philosophical Society*, 99 (1955), 327–354, and H. Dingle, "History of Science and the Sociology of Science," *Scientific Monthly*, 82 (1956), 107–111.

[14] Koyré in *Scientific Change*, p. 856.

[15] "Merton Revisited," p. 15.

ett, Cohen, Crombie, Gillispie, Guerlac, Hall, Kuhn, Westfall, and their many students. They have not only created a discipline, but added immeasurably to our understanding of the science of the past. The prevailing standard of work in the history of science is indeed impressive. To realize this, it is only necessary to compare any recent issue of *Isis* with one from three decades back, or to contemplate the ever-escalating sophistication of research on Isaac Newton, over the last quarter century. The very necessary insistence on the intellectual validity *in its own terms* of previous scientific thought has been salutary. But it is not sufficient. As any analysis of recent research, any sustained conversation with younger scholars, any serious inquiry among present graduate students will reveal, the discipline of the history of science is now seriously distorted, and out of temper with the times.

The atomic and hydrogen bombs have long since lost their power to shock. For those who have come of age since 1960, even antiballistic missiles, chemical defoliants, and riot-control gases seem merely part of the accepted order, as inescapable as rain or sun. And from the computer, through the moon rocket, to the Xerox machine, the more peaceful technological abilities of modern science are everywhere displayed, and preach their silent sermon. Concepts such as "the free world" and "the Communist bloc" are now revealed for the simplistic slogans they always were, though peace on earth does not appear to be the inevitable outcome. Joseph McCarthy has been replaced by Eugene, who has vanished in his turn. Indicative of the shifting mood is the way that societies for social responsibility in science are now in vogue.[16] It is against this background that the future of the history of science invites fresh thought.

IV

Historians of science are fortunately peculiarly liberated from those degenerate scientisms which still dog some other areas of history. Prolonged exposure to the fads and fashions of past scientific thought has taught them that inductivism and positivism are at best philosophic and methodological prescriptions, not unyielding statements about the only way that truth may be attained. The desire to "tell it like it is" (or "wie es eigentlich

[16] Or consider the way responsible scientists are themselves calling for a new image of science: e.g., "Science can no longer be content to present itself as an activity independent of the rest of society, governed by its own rules and directed by the inner dynamic of its own processes." Robert S. Morison, "Science and Social Attitudes," *Science*, 165 (1969), 156.

gewesen," if you will) thus holds little lure. Lord Acton's vision of "ultimate history" has correspondingly small appeal. It is characteristic that in the recent and contrasting accounts of the nature of history given by two distinguished Cambridge historians, it should be E. H. Carr, the relativist, who draws heavily on modern work in the history and philosophy of science to support his position, and Geoffrey Elton, the old-style inductivist, who shows the traditional historian's disdain for this new discipline.[17]

If the historian of science knows too much about the relativity of scientific thought to be lured by any quasi-absolutist view of history, itself deriving from outmoded ideas about science, where may he turn instead? The intellectually, emotionally, and professionally satisfying response of the fifties and sixties lay in "the method of conceptual analysis, based on the model set before us by Koyré." Obviously one might continue and extend this approach. Koyré's own researches (and those of many of his disciples) focus exclusively on the period before 1700—and it is not, after all, self-evident that scientific change ended there. However, Koyré's method, with its stress on the interrelations of scientific, philosophical, and theological ideas, its demand for the complete mastery of a body of textual material, and its inevitable focus on the thoughts of great men, was perhaps mainly suited to the embryonic science of the sixteenth and seventeenth centuries. The few self-conscious attempts to extend its coverage into the nineteenth century have not been remarkable for their success.[18]

Koyré himself freely admitted that "history is always being renewed," and that "nothing changes more often and more quickly than the immutable past." He also acknowledged that his own stress on Platonism and idealism was "nothing else than a reaction against the attempts to interpret . . . modern science . . . as a promotion of arts and crafts, as an extension of technology, as an *ancilla praxi*."[19] It would therefore seem that to continue wholly preoccupied with conceptual analysis (or to return to Marxism, its polar opposite) would be to project into the seventies an approach more suited to the particular tensions of the 1930's. It is of course possible to argue that this approach *is the* method for the history of sci-

[17] See Edward H. Carr, *What Is History?* (London: Macmillan, 1961), and G. R. Elton, *The Practice of History* (London: Methuen, 1967), *passim*.

[18] The most interesting attempt is undoubtedly C. C. Gillispie's "Elements of Physical Idealism," in *Mélanges Alexandre Koyré* (Paris: Hermann, 1964), II, 206–224. On the proper method of approaching more recent science, see also the exchange between Professors T. S. Kuhn and L. P. Williams in *British Journal for the Philosophy of Science*, 18 (1967), 148–161.

[19] Quoted from *Scientific Change*, p. 852.

ence, and thus has a unique and timeless validity. But as the quotation above illustrates, Koyré himself would have had little patience with such an argument, whatever some more committed practitioners may feel.

What then are the possible alternatives? Admitting that conjecture and refutation lie at the base of all history, and that the discipline is (among other things) fundamentally committed to the search for a believable future, the present author wishes to appeal to the twin virtues of nonconformity and catholicity. The nonconformity lies in a very modest suggestion. It is that *concern about the future which reflects the present pressures and problems in science must be the basis for a far greater fraction of the historical enterprise.* We must face and accept the challenges posed by the multivarious tensions and aspirations which modern science has loosed into our world, and make these tensions the driving force of creative new research. Catholicity is also necessary, because as the tensions and aspirations are many-faceted, so must be the resulting history. This catholicity will embrace a variety of techniques, techniques that neither require nor allow of exhaustive enumeration here. Even so, four well-tried approaches from other areas of history would seem particularly relevant in the search for a comprehensible and usable past.

V

Perhaps above all an approach that embraces the sociology of knowledge is needed. A major weakness of the internalist position lies in its assumption that ideas can meaningfully be divorced from institutions. This is not to suggest that institutions make ideas, any more than incubators create eggs. However, the institutional context is often crucial in determining which ideas are adopted and flourish, and thus in creating regional and national styles in science. Dr. P. M. Rattansi's study of the physician-apothecary clash in Restoration England is one obvious example. Another is available in the writer's work on the political, and Professor Manuel's on the social, context in which the Royal Society became a vehicle for the creation of a British Newtonian tradition. Professor Kuhn's *Structure of Scientific Revolutions* suggests a variety of other ways in which the institutional background may be of the greatest significance, as do the essays of Joseph Ben-David. More generally still, the writings of Karl Mannheim explore the importance of a historical sociology of knowledge for any full understanding of our intellectual heritage. The work of Professor Namier suggests rather different ways to penetrate behind the outer label to the inner

reality.[20] His techniques are now so well assimilated by the political historians that none would use the terms "Whig" and "Tory" as sufficient explanatory categories in themselves. Yet the denotation "Cartesian," "Newtonian," "Positivist," or "Romantic" is still too often made the complete explanation of a scientific group and its activities. The discussion of the social, political, and moral pressures covered by such labels has yet to begin. In this context it is revealing to note that Namier's archfoe, while unable to vanquish him on their common ground, yet enjoys a major reputation among historians of science for an internalist study of the evolution of scientific thought.[21]

If the sociology of knowledge is one area that the history of science has much both to learn from and to contribute to, role theory is another. From Znaniecki's pioneering study of *The Social Role of the Man of Knowledge* on, there is an extensive and important literature available. Though its insights have so far been ignored by historians of science, this literature has been ably exploited by historians of supposedly more traditional bent.[22]

The creation of modern science may be viewed in many complementary ways. One may if one wishes focus on a seventeenth-century conceptual revolution, or on the associated appearance of wholly new institutional forms. One might also usefully consider the emergence of, and shifts in, the social role of the man of science. The differences in ideology and social function between, say, Paracelsus, Robert Boyle, John Dalton, and R. B. Woodward are profound. Their investigation would illuminate some of the most critical phases in the growth of modern science. Again we are

[20] See, for example, P. M. Rattansi, "The Helmontian-Galenist Controversy in Restoration England," *Ambix*, 12 (1964), 1–23; A. Thackray, " 'The Business of Experimental Philosophy'—The Early Newtonian Group at the Royal Society," *Actes du XII^e Congres International d'Histoire des Sciences* (Paris, in press); Frank Manuel, *A Portrait of Isaac Newton* (Cambridge, Mass.: Harvard University Press, 1968), chapter 13; J. Ben-David, "Scientific Productivity and Academic Organization in Nineteenth-Century Medicine," *American Sociological Review*, 25 (1960), 828–843; Karl Mannheim, *Ideology and Utopia: An Introduction to the Sociology of Knowledge*, trans. Louis Wirth and Edward Shils (New York: Harcourt, Brace, 1936); Lewis B. Namier, *The Structure of Politics at the Accession of George III* (London: Macmillan, 1929); A. Thackray, *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Mass.: Harvard University Press, 1970).

[21] See Herbert Butterfield, *George III and the Historians* (London: Collins, 1957), and J. M. Price, "Sir Lewis Namier and His Critics," *Journal of British Studies*, 1 (1961), 71–93.

[22] Florian Znaniecki, *The Social Role of the Man of Knowledge* (New York: Columbia University Press, 1940); and, for example, Thomas C. Cochran, *Railroad Leaders, 1840–1890: The Business Mind in Action* (Cambridge, Mass.: Harvard University Press, 1953). See also Michael P. Banton, *Roles: An Introduction to the Study of Social Relations* (London: Tavistock, 1965).

confronted with a paradox. One of the most obvious and central themes in the history of science is the changing social role of the practitioner. From virtuoso to natural philosopher to scientist to, say, physical biochemist, these changes mirror profound shifts in the organization, content, and social function of scientific knowledge. Yet there is little literature that deals with the history of *science as a profession*.[23] Merton's pioneer work aside, almost nothing is known about the recruitment of men of science, the effect of the "Ph.D. machine," and the emergence and differential sociology of the various scientific disciplines. It is of course possible to argue that the evolution of, say, biochemical thought owes nothing to the competition and interaction between physicians, apothecaries, agricultural chemists, and "pure" research scientists. Possible but, one hopes, increasingly difficult.

A third neglected approach of crucial importance is through the interaction of material culture and intellectual forms—here, more specifically, of technology and science. The Marxist view of this interaction is encapsulated in Hessen's remark (quoting Engels) that "when after the dark night of the middle ages science again began to develop at a marvellous speed, industry was responsible." Not surprisingly such a dogmatic and extreme statement called forth its polar opposite. We have already noted Koyré's insistence that science "is essentially *theoria*, a search for the truth" and that it has "an inherent and autonomous development." It is time to move away from such barren antitheses. The division between "internal" and "external" history may accurately reflect much current work. However, to seek for "a demarcation of their respective fields of application with some degree of accuracy," as Professor Hall has urged,[24] would be to elevate a polemical division to the status of a methodological principle.

It would seem more fruitful to search for that *via media* which avoids the extreme formulations of either side. That material and intellectual cul-

[23] Though see E. Mendelsohn, "The Emergence of Science as a Profession in Nineteenth Century Europe," in *The Management of Scientists*, ed. Karl Hill (Boston: Beacon, 1964), pp. 3–48; J. Ben-David, "The Scientific Role: The Conditions of Its Establishment in Europe," *Minerva*, 4 (1965), 15–54, and "Social Factors in the Origins of a New Science: The Case of Psychology," *American Sociological Review*, 31 (1966), 451–465. Also useful to, but usually neglected by, the historian of science are the directly sociological discussions of modern science as a profession: e.g., Warren O. Hagstrom, *The Scientific Community* (New York: Basic Books, 1965), and Norman Storer, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966).

[24] "Merton Revisited," p. 15.

ture profoundly influence each other would seem a truism to the anthropologist. It is thus ironic that historians of man's greatest intellectual adventure so continually ignore this obvious theme. Studies of the first industrial revolution—I mean that of the eighteenth not the sixteenth century—offer a particularly apposite field. Between 1760 and 1880 the industrial bases of the Western world were transformed. The same period witnessed a clearly related event in the replacement of natural philosophy by positivistic science. Yet studies of the manifold and subtle interconnections of these two fundamental changes in Western culture have scarcely begun. Professor Guerlac's perceptive study of eighteenth-century French chemical technology reveals one possible approach. That it was promptly dubbed "un peu Marxiste" again reveals those fruitless antagonisms we must now renounce.[25]

My fourth field is the most irony-laden of all. If contemporary historians of science are agreed on one thing, it is that the success of science, its very intellectual power, its cutting *Edge of Objectivity* lies in the ability to quantify. One might naively suppose that those recruited from the ranks of science would, of all historians, prove the most eager to employ this demonstrated weapon. The reality is far otherwise. The impact of railroads on American history has been subject to masterly quantification. Every accepted and hallowed judgment of political history is now at the mercy of the computing behaviorist historians. Social mobility, family structure, the rise of the gentry: all are now moving out of the realm of easy and unsupported generalization into that of exact measurement.[26] The same cannot be said of any area within the history of science.

Quantification itself is not the *goal* of history, any more than of science. But in both cases it does allow a finer grasp, an easier handling, a more meaningful discussion of what can be known. The whole subject of quantification is now treated with considerable sophistication by general historians, as Professor Aydelotte's essay reveals.[27] Yet the matter is rarely

[25] See H. E. Guerlac, "Some French Antecedents of the Chemical Revolution," *Chymia*, 5 (1958), 73–112; and *Scientific Change*, p. 810.

[26] See, for example, R. W. Fogel, *Railroads and American Economic Growth: Essays in Econometric History* (Baltimore: Johns Hopkins Press, 1964); S. P. Hays, "The Social Analysis of American Political History," *Political Science Quarterly*, 80 (1965), 373–394; Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth Century City* (Cambridge, Mass.: Harvard University Press, 1964); *An Introduction to English Historical Demography from the Sixteenth to the Nineteenth Century*, ed. Edward A. Wrigley (London: Weidenfeld and Nicolson, 1966); Lawrence Stone, *The Crisis of the Aristocracy* (Oxford: Clarendon Press, 1965).

[27] "Quantification in History," *American Historical Review*, 71 (1966), 803–825.

broached by historians of science. Professor Price's somewhat differently conceived studies are the solitary exception that proves the rule. If we knew how many and who were the scientists of seventeenth-century England or nineteenth-century Germany, our discussions would be more informed. Education, religious affiliation, social class, regional location, occupation, and research interests are all susceptible of quantification and, with the aid of the digital computer, comparison and correlation. Such information would not of itself constitute the history of science. It would, however, remove much of the mist and murk that obscures current discussion. For example, we would be able accurately to specify the importance of Protestant Dissent and manufacturing interests among eighteenth-century English natural philosophers. Are Joseph Priestley and the Lunar Society really characteristic, or merely glamorous exceptions that obscure the rule? To answer this question would not explain the course of late-eighteenth-century science, but it would go far toward defining what any adequate explanation must encompass. At present, for want of hard statistics, we wallow in an impressionistic sea of unsupported generalizations.

VI

Enough of these laments. The argument is almost at an end. Lest it be misunderstood, the writer must again make clear that he values and is himself fundamentally indebted to that "method of conceptual analysis" advocated by Koyré. The untangling of past ideas will always be an important part of the task of the historian of science. The thesis of this paper is not that such activity is worthless. The point is rather that its present dominance is a reflection of past ideological positions no longer relevant, that it too narrowly restricts the range of discussion, and that it commands an unhealthy hegemony over the field. The urgent need is for a far greater diversity in methods of attack and forms of inquiry. This need parallels that for a greater concern with the major issues of today. War and peace, the use and misuse of research, the interrelations of science and technology, the effects of secrecy and military objectives, the reciprocal responsibilities of the scientist and society, the funding and control of science, problems of morale and status within the scientific enterprise: these must be the motivating pressures behind new historical research, aimed as much at the seventeenth and nineteenth as at the twentieth century.

The result of altered perspectives and priorities may not be that "history of systematized positive knowledge" for which Sarton yearned, nor that

rethinking past thoughts of great men so popular at present. But in a world where the past, like so much else, is not what it used to be, fresh approaches may provide a firmer base from which to contemplate the present, and debate alternative futures. This search for a projectable past would now seem incumbent on the historian of science. To refuse the call and continue convinced that familiar problems and by now traditional methods are sufficient would be to condemn the discipline to the status of an esoteric luxury. Such a luxury would of course continue to be savored behind closed doors by the select few, while outside science and Western society continue what may yet prove the dance of death. The historian may rightly reject the call to direct social involvement. Equally, he should resist the lure of a past attractive only by its denial of present problems.

In conclusion, it may be appropriate to repeat Koyré's remark that "nothing changes more often and more quickly than the immutable past." The question before the historian of science is in what ways, and why, he can best aid that unceasing change.

COMMENT BY LAURENS LAUDAN

And, although in the hundred and twenty years or so, during which this ambition to imitate Science in its methods rather than its spirit has now dominated social studies, it has contributed scarcely anything to our understanding of social phenomena, not only does it continue to confuse and discredit the work of the social disciplines, but demands for further attempts in this direction are still presented to us as the latest revolutionary innovations which, if adopted, will secure rapid undreamed of progress.

HAYEK, *The Counter-Revolution of Science*

I am not quite sure how best to formulate my reactions to Professor Thackray's interesting and provocative paper. In substantial agreement with many of the *specific* proposals he makes, I am nonetheless rather uneasy about the *general* direction in which he takes the argument. Indeed, our differences at the general level are as great as our agreements over points of detail. By and large, we tend to agree at the level of truisms. Thackray wants pluralism and diversity in the writing of the history of science; so do I. He thinks it misleading to view the history of science entirely as the evolution of disembodied ideas. Again, I suspect we all nod our heads in enthusiastic support. He thinks historians of science should know something about how to handle statistical data. I believe he would find few living historians of science to play the role of *advocatus diaboli* against him. He insists that the historian projects his own interests into history. That point has surely been commonplace for every historian since the turn-of-the-century debates between Meyer, Weber, and Rickert![1] Like Agassi and Kuhn before him, Thackray, in parts of his paper, has chosen to do battle against an already vanquished enemy,[2] and as a result he often seems (mixing the metaphor) to be preaching to the converted. But Thackray's paper does not consist entirely of innocuous reflections on the historian's craft. On the contrary, there are two general theses in his paper which are far from tautological, and it is those which I should like to discuss briefly here.

Where I disagree most fundamentally with Thackray is on the question of the *value* of, and the *urgency* for, historiographical debate of the kind he hopes to provoke and intensify. Let me explain why. A historian always begins with a particular problem or set of problems. They may range from "Why was Faraday uneasy about action at a distance?" to "What factors influenced the growth of agricultural chemistry from 1800 to 1850?" The nature of the particular problem will usually indicate that certain factors are likely to be operative in the given situation, and that certain strategies are more likely to lead to a coherent solution to the historical puzzle. Clearly some questions and problems are amenable to quantitative analysis or a role-theoretical approach, while the resolution of others will depend almost completely on textual exegesis. More complex problems will require a more subtle combination of several such approaches. What we should avoid is dissipating our limited energies needlessly in pompous and protracted debates about the *general* nature of the history of science. Unless one is prepared to defend the highly dubious thesis that all scientific developments depend on the same sort of influences and pressures, then it is clearly foolish to argue that all (or even most) historical problems can be analyzed in the same way or in terms of the same categories of narration. If Thackray thinks the acceptance of Newtonian theory in England was a function of a power struggle within the Royal Society, then let us discuss that particular claim on its own merits. If a disciple of Hessen wants to treat Newton as a pawn of the English mercantile class, then we can discuss that argument. If, like Professor Frank Manuel, someone wishes to "explain" Newtonian action at a distance in terms of Newton's Freudian frustrations, we may even try to take that claim seriously. But I am convinced that an abstract discussion about historiography is going to leave

[1] See especially E. Meyer, *Zur Theorie und Methodik der Geschichte* (Berlin, 1902), pp. 37ff, or M. Weber's *Gesammelte Aufsätze zur Wissenschaftslehre* (Leipzig, 1922).

[2] Where Agassi's straw man was the "inductivist," and Kuhn's the "internalist," Thackray invents the "intellectualist."

all the interesting questions unresolved. It is also very likely to be counterproductive, for the validity of an intellectual, sociological, role-theoretical or psychological approach can only be determined by, and for, individual cases.

For the same reasons that it has generally been useless (and sometimes harmful) for the members of a scientific profession to worry themselves over general questions of method rather than getting on with their work,[3] we as historians must not allow ourselves to be diverted from the task at hand by a heated debate, however enticing, "over historiography and ideology." In the final analysis, the only important testimony to the soundness of a sociological or an intellectual approach to the history of science is that it gives plausible answers to interesting and important questions. It is a sad fact about the sociology of science that, Merton's work notwithstanding, it has failed to produce a signal contribution to the field which could be ranked with the books of (say) Koyré, Burtt, or Duhem. In lieu of giving us the genuine product, sociologically oriented historians of science have lately tried to move the debate into the arena of the philosophy and logic of history. What they have not been able to establish convincingly in the particular case, they have sought to establish as a *general* characteristic of the evolution of science.

If all this seems philistinic, I plead guilty to being a philistine in these matters. My excuse is that we are in a field with very many interesting problems and with far too few bodies (and perhaps still fewer minds) to solve them. It would be nothing short of retrograde for us to declare a moratorium on our researches in order to go through the motions of an insoluble general debate about the kinds of forces affecting scientific change. So, while Thackray views with dismay the fact that historians of science have not taken up the challenge to historiographical debate issued by Agassi, Kuhn, and Hall, I interpret their disinclination as a reassuring sign of widespread common sense among the practitioners of the subject.

There is a second aspect of Thackray's essay which I want to touch on briefly. Thackray claims that the historian's researches ought to be governed by considerations of contemporary relevance. We live, he insists, in a real world with genuine social problems growing out of science and its products. We have before us, to continue the paraphrase, two choices: either we can pursue historical investigations relevant to those problems or

[3] Witness the stalemates in psychology in the 1920's and 1930's whenever methodological questions became uppermost.

we can ignore them altogether. I find this argument nothing short of invidious, and embodying the kind of rhetoric and fuzzy-mindedness typical of the new right (otherwise known as "the new left"). Thackray's sloganistic "withdrawal is every bit as much a response as engagement" is but another version of the simplistic chant "if you're not part of the solution . . . you're part of the problem."

I am not so bold as Professor Thackray to attempt to speculate on the psychological and political motives which drew our colleagues Gillispie, Guerlac, Cohen, and others into the history of scientific ideas. Perhaps, as he suggests, they were afraid of McCarthy and the House Un-American Activities Committee. The point is that I do not really care very much what their motives might have been. A treatise on "the genetic fallacy" is not required to show that the history they have written stands more or less on its own, and that its importance and validity clearly must be assessed independently of whatever lurking traumas and fears might have been imbedded in their psyches. Fortunately, history is sufficiently empirical that *we generally can* (in spite of Professors Carr and Thackray) *assess the history without assessing the historian.* By the same token, the sociological history of science must needs be assessed by historical standards. All the good intentions in the world, leavened with heaps of relevance, will not justify such history unless it is sound history, for relevance is not a historical desideratum. We decide whether Merton's work is cogent and valid, not by asking what light it sheds on the National Science Foundation, but by asking what light it sheds on the early Royal Society.

There are several classic difficulties with the nebulous demand for relevance, even assuming that it is right to take that demand seriously. How, for instance, is the historian to interpret this demand? It might well happen that a study of the early years of the British Association is more "relevant" to modern problems than a history of the National Research Council since the last war. For all we know, a study of the financial support given to French scientific expeditions in the eighteenth century will be of greater relevance than an investigation of the science-policy-shaping practices of the National Science Foundation. How does one know in advance whether any given investigation is going to be (or is even likely to be) more relevant than another? Thackray's formula, identifying relevance with temporal proximity to the present day, is just too simpleminded to be taken seriously. Moreover, overt preoccupation with relevance normally tends to make for bad history, because this puts constraints on the

historian, which should not be there, and which cause him to cast about for contemporary analogies, however farfetched. I think there is general agreement, for instance, that Professor Santillana's classic *The Crime of Galileo* was weakened by Santillana's penchant for casting Galileo in the role of Oppenheimer, fighting a seventeenth-century McCarthy. To write the book as he did certainly made it more relevant (for the 1950's). But it made it much less interesting and less reliable as a piece of history. This is but one of many cases one could cite to illustrate the Orwellian consequences of Thackray's plea for a "search for a . . . *usable* past."

Of course, it would be a piece of good fortune if one could manage to write sound history which was relevant as well. Nor is this necessarily as difficult as having one's cake and eating it too. But even if we concede that relevance, though not necessary, is nice when you can get it, I am not sure why this has any bearing on the historiographical dispute between the intellectualist historians of science and their detractors. Indeed, I believe there is an important sense of relevance according to which the intellectual history of science is crucially relevant, and it is a serious oversight on Thackray's part to have missed, or at least ignored, the kind of relevance to which I refer.

Anyone familiar with contemporary science will concede that there are many problems in the science of our day. Some of them, as Thackray observes, are related to the obviously moral difficulties which science poses for modern man. Others, no less significant in magnitude, are problems of a conceptual character. The scandals in quantum mechanics are well known, and the recent work of Dicke and others in relativity theory similarly suggests that the choice between classical mechanics and relativity theory is not as clear-cut as it once seemed. It is not implausible to think, as Jammer, Feyerabend, and Hanson (among others) have suggested, that a careful analysis of the history of science—especially the history of physics—may well shed considerable light on many of these thorny conceptual problems, perhaps even indicating possible ways out of certain theoretical impasses in which contemporary scientists find themselves. Thus, insofar as it is possible that historical studies will illuminate the intellectual difficulties in which modern scientists find themselves, the history of science is clearly relevant to modern concerns.

If the historian feels strongly about moral and social issues of the day (as one certainly hopes he will in times like these), then he has every obligation to let his views be known, by direct social involvement if necessary. But he is under no moral obligation to prostitute his intellectual interests by making them subordinate to a sense of social mission and an exaggerated sense of the importance and significance of the research he does produce. It would be a disaster if the profession as a whole were to accept the sophistical argument which concludes that *socially* relevant research is a moral imperative. It is neither immoral nor escapist to believe that there are important historical problems about medieval or seventeenth-century science, and to believe that those problems are completely irrelevant to modern *social* concerns.

Thackray suggests that a view like the one I am defending renders history of science an "esoteric luxury." The fact is that history of science (like most aspects of intellectual life) *is* a luxury; and I think Thackray would be hard-pressed to show how it could be otherwise. Indeed, it is in part precisely because it is a luxury that we must work to preserve and to cultivate it. To abandon intellectual luxuries is, after all, to compromise civilization. To act as if history of science is (or might become) more than a luxury is dangerous in the extreme; for it is likely to arouse expectations which, if not satisfied, will turn against the subject and undermine it. If there are those who attack the history of science because it is a luxury, we should face them here and now.

I have said more than I intended to say. By way of summary conclusion, let me reiterate my conviction that the debate about historiography of science, precisely because it is concerned with the general, is going to be hopelessly inconclusive. What we should be doing (and what we should be encouraging our students to do) is tackling significant but specific issues concerned with the evolution of science, bringing to bear as many techniques as we can master and as the problems require.

REPLY BY ARNOLD THACKRAY

An adequate reply to Professor Laudan is not possible here, for "art is long, and time is fleeting." Even so, I would like to correct some trivial misunderstandings and make two brief clarifications, in the hope of keeping the issues sharp.

I do *not* want to debate "the *general* nature of the history of science" (though, as I trust is apparent, I do hope for widespread discussion of ways of studying past science); I was *not* offering psychological and political explanations of individual actions (though I was essaying a first delineation

of factors affecting the developing pattern of a newly emerging discipline); I did *not* (I trust) identify relevance with "temporal proximity to the present day" (though I do think historians should study recent science, and that relevance has the same benefits and dangers here as in the seventeenth century).

More generally, let me challenge Professor Laudan's belief that historians simply seek "plausible answers to interesting and important questions." Unless he is an inductivist, which I doubt, Professor Laudan presumably admits some general criteria for deciding which questions are interesting and important. Similarly, those "particular problems" with which his historians so innocently begin are not God-given but selected from the unending range of possible problems by the implicit or explicit application of general criteria. It is of course a well-worn debating technique to hide one's own preferred assumptions behind the label "objective" (or even "obvious") while attacking one's opponent not for his facts or his logic but for his temerity in starting from general propositions. This present discussion deserves better, especially from so able a philosopher of science.

Finally, let me deny that I asked for a history "governed by" relevance. Yet when one searches in vain among the scholarly articles of the past decade for material which illuminates our present social dilemmas, it does not seem entirely rash and extreme to ask for some changes in emphasis. I began by quoting Koyré, because he clearly realized that "the interests and the scale of values of his own time" shaped his work. Free discussion and critical debate will make present interests and values our ready servants: a conspiracy of silence serves only to perpetuate the unacknowledged rule of the interests and values of yesteryear.