Book Reviews

Mertonian Theses

The Sociology of Science. Theoretical and Empirical Investigations. ROBERT K. MERTON. University of Chicago Press, Chicago, 1973. xxxii, 606 pp. $12.50.

Collected in this volume are many papers, some 22 of them in the present arrangement, that Robert Merton has addressed to problems in or near the sociology of science over the last 40 years. Norman W. Storer has made the selection and provided an informative introduction and comments. He has also had the convenient idea of grouping the investigations, not all of which appear under the original titles, into five broad categories according to whether they concern the sociology of knowledge, the sociology of scientific knowledge, the normative structure of science, the reward system of science, or the processes of evaluation in science.

It may be appropriate in this instance to begin the discussion with an apology for being the one to have written it, for I am neither a sociologist nor a scientist but a historian, and must leave evaluation of Merton's work as sociology to the professional journals. What the editors of *Science* asked to have set out, however, is the interest it may hold for scientific readers, and I felt privileged to accept the commission for a reason they may not have known, which is that historians of science have learned more about scientists from Merton than from any other sociologist. We appreciate the extensive and accurately documented use he makes of the historical literature of science, both primary and secondary, and stand in awe of his knowledge of our subject.

Having thus begun somewhat personally, perhaps I may be permitted an anecdote to illustrate the unexpectedness of the kind of thing we have learned. Some years ago, probably in early 1958, Merton sent me an offprint of what I have since found to be the most eye-opening single piece that he has written, his presidential address to the American Sociological Association on "Priorities in Scientific Discovery." It starts by noting (pp. 286–287) "the great frequency with which the history of science is punctuated by disputes, often by sordid disputes, over priority of discovery." As I read on, dismay overtook amusement at the parade of eminent scientists arguing and frequently quarreling with each other, not over what the truth was, but over who had it first, Newton or Leibniz, Newton or Hooke, Cavendish or Watt or Lavoisier, Adams or LeVerrier, Jenner or Pearson or Rabaut, Freud or Janet. Sometimes the great men themselves abstained from contending in the lists of professional recognition for title to their intellectual property only to have their claims championed by disciples or compatriots. All too clearly the particular instances that Merton adduced in a number of variations on the theme of intellectual possessiveness could have been multiplied almost indefinitely.

In a note of acknowledgment to Merton, I wrote that, though it seemed surprising that the phenomenon was so nearly universal an accompaniment to scientific discovery, I did wonder whether the matter wasn't a bit trivial. I don't believe I also said "unworthy" but recollect that such a dark thought was in my mind. Only a few years later, when I began to study and teach materials in the social and institutional as well as the more traditional internal and intellectual history of science, did I come to take the full thrust of what he had in fact said, and said clearly and convincingly. It was that such behavior occurs in service to social norms; that norms arise in the life of real communities governing the conduct of their members; that the phrase "scientific community" is, therefore, no mere manner of speaking about some shared pleasure in the study of nature but refers to an effective social entity; and that, within its membership, which is bounded professionally and not geographically, two main sets of norms constrain behavior and do so in ways that conflict, the one enjoining selflessness in the advancement of knowledge, and the other ambition for professional reputation, which in science accrues from originality in discovery and from that alone. The analysis exhibits the scientific community to be one wherein the dynamics derive from the competition for honor even as the dynamics of the classical economic community do from the competition for profit, and neither of those statements is in any way incompatible with agreeing that the competitors characteristically like their work and choose it for that reason.

Merton replied a little stiffly though politely to my note, and only now on reading other essays do I learn, and suppose I should feel comforted, that my initial resistance was not mere obtuseness, but an instance of a methodological fallacy against which he warns, the supposition that the social importance of a phenomenon is a measure of its sociological significance (pp. 59–60). I find further, and less comfortably, that the instinct to trivialize what is demonstrably significant is a signpost familiar to sociologists, and that it points to distaste for the facts and hence to wishfulness and the substitution of sentiment for analysis (p. 384). The episode (out of modesty I should like to say trivial but, such is Merton's way with a norm, no longer dare do so) brings home to me the distinctive feature of his touch, which is to situate behavior, most often intellectual behavior, in its sociological context, but without thereby robbing it of individuality.

Some ten years later Merton welcomed the appearance of James Watson's *The Double Helix* (1968) for the epitome it gave of the inwardness of a scientific investigation into a strategic problem, and for the confirmation it afforded of the competitiveness among scientists entailed by the premium on being first with a solution. In his appreciation of Watson's candor, Merton dismisses the squeamish reaction that it shows contemporary science to have been corrupted by the scale, pressure, and contagion of a world that is too much with us. In fact it has been ever thus and ever an illusion, fostered by the myth of lonely, leisurely, disinterested contemplation of objects of simple curiosity, that science in olden times—say prior to World War II—was somehow better and purer than life. Indeed, what with the increasing prevalence of research in groups and teams, and perhaps also with the imposition of institutionally induced civilities, the occurrence of multiple discovery, though no less characteristic of science than in the past, has produced relatively fewer priority disputes in recent times than in its heroic ages.

Extending his investigation in the

immediate sequel to the "Priorities" paper, Merton advanced the startling hypothesis (p. 356) that "the pattern of independent multiple discoveries in science is in principle the dominant pattern rather than a subsidiary one. It is the singletons—discoveries made only once—that are the residual cases, requiring special explanation." Moreover, the special explanations that he adduces virtually explain away the common notion of the unique and individual discovery in science. In every case of an apparent singleton that he has examined, Merton detects an example of rediscovery of something not fully seized, or else of work unpublished, suspended, or forestalled, and thus potentially when not actually duplicated. Lest the case here seem a little forced, he points out tellingly that scientists habitually live in the fear that such will happen, knowing it in their hearts to be the common fate.

A historian for his part must acknowledge that the argument, even when pushed to the extreme, makes sense of his vaguer feeling that the creations of science pertain to the scientist in a manner different from the relation of the work of art to the artist: that there is a sense in which the problem finds its scientists and that we would thus have had the law of gravity and the laws of motion even without Newton, but would not have had Hamlet without Shakespeare. Consistently enough, Merton supports his hypothesis sociologically by empirical evidence rather than by cognitive or logical considerations. He and his associate, Harriet Zuckerman, have inventoried 264 cases of discovery and found 179 doublets, 51 triplets, 17 quadruplets, six quintuplets, eight sextuplets, one septuplet, and two nonaries.

What, then, of the role of scientific genius? For it is attractive that Merton never denies the reality or importance of greatness in the gifted person or dodges it in the study of faceless aggregates. On the contrary, he dispels the notion that social explanation of a process derogates from the single man's part in it. What he finds, also empirically, is that scientists commonly reckoned to have been great participated in an altogether larger number of multiple discoveries than their lesser colleagues. They did a lot more science and were connected professionally with a much larger number of other scientists. A detailed study of much of Kelvin's published work detects some 32 instances of multiple discovery in-

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volving 30 other scientists. A scientific genius is measurably great, then, in the sense (though not necessarily only in this sense) that his contributions are equivalent to those of a considerable number of lesser lights.

That these findings should have been unwelcome to many scientists is not surprising. To have some conflict in inner values exposed to the inspection of others is never exactly pleasant, however salutary, and though Merton did not mean to be insulting, the spectacle that he spreads upon the record flatters only the very few who were self-denying about their claims to priority. No one likes to have his behavior labeled, the less so if it be aptly done, and Merton calls the elation (in part illusion) of the moment of discovery the Eureka syndrome (p. 401). He goes to some lengths to explore the deviant or repressive behavior which the premium on originality produces—plagiarism or falsification in the extreme instances, which are rare in science; cryptomnesia in the common instances of an investigator who forgets, not only that someone else had the fine idea before, but often that he himself did at some earlier stage. To study all this frankly would be healthy, Merton argues in a paper on the advantages to be anticipated from further study of the phenomenon of multiple discovery. More concretely, such investigation could be expected to bear on the problem of creativeness in science, on the role of the scientific milieu, on the comparative methodology of science, on the relation of research establishments to society at large, on the psychology of science, and finally on planning and science policy. On the last score Merton advances the intriguing possibility that multiple research and discovery may not in fact be wasteful and redundant if the problems are important, but may instead be functional, in that repetition and reinforcement will affect the situation more strongly than would single statements.

Among the other papers collected in this volume are a chapter, "The Puritan Spur to Science," from the doctoral dissertation that Merton completed in 1935—*Science, Technology and Society in Seventeenth-Century England* (in *Osiris Studies*, 1938)—and the preface that he composed for the reprinting of that monograph in 1970. Not many a thesis furnishes fuel for a controversy lasting as long as its author's career, much less bidding (as this one is beginning to do) for immortality. It was Merton's contention, advanced at a time when science and religion were supposed to be categorically antithetical each to the other, that on the contrary the Puritan values of diligence, rationality, practicality, asceticism, self-denial, civic spirit, and service to God through the work of this world (for this purpose the study of His works in nature) had the strong tendency of stimulating and validating the scientific enterprise among the generation that founded the Royal Society. In effect the argument utilized the categories of Max Weber's famous *The Protestant Ethic and the Spirit of Capitalism*, where they served the analysis of the religious legitimation of capitalism, and transferred them to an analysis of the religious legitimation of science.

Merton's treatment of the Puritan ethos rests upon the same kind of evidence as did Weber's, an essentially psychological insight into literary sources and biography. It touched us on the quick at first reading in much the same way, at least it did those of us who still recognize our own springs of action in Puritan modes of behavior. It was also open to the same kind of objection on the part of critics of a more literal or Catholic turn of mind than Weber, Merton, or we who remain persuaded that there is something important here that has yet to be fully made out. For clearly there are difficulties, and grave ones. Economic enterprise for profit has flourished in many a milieu untouched by Puritanism, and a theory of capitalism that requires excluding Florentine bankers has its problems. By a similar token, it is very easy to find notable Roman Catholics among European scientists of the 17th century, and the embarrassment may be complemented by citing eminently antiscientific statements on the part of certain Puritan divines.

If it is correct that some sort of intuitive recognition (or resistance) was responsible for the initial and continuing interest in Merton's argument for a stimulating effect of Puritanism on science, then the inability of scholarship either to settle the question or drop it may arise from a discrepancy between that fact and the way in which the case was presented. For Merton made it depend on a statistical analysis of the membership of the Royal Society in its early days. His contention that the persons who predominated scientifically in that body were in significant degree of a Puritan persuasion in religion has not, in my judgment, withstood criticism based on chronology, counting, and biography. During the interval of more than 30 years between the publication and the reissue of his monograph, Merton kept largely silent about the question, preoccupied as he was with the other investigations reported in this book, and even more so with sociology at large. But now that he has answered his critics, the preface (pp. 173–190) in which he does so fails to meet their case. The trouble begins with the fancy that his own book written long before is by somebody else. There are passages in other writings in which Merton likes to make a point by doing sociology right out there in the open before the reader's very eyes, and usually the device is self-deprecating and entertaining in effect, but here it distracts attention from the issue. Neither will it do to refute Lewis Feuer's *The Scientific Intellectuals* (1963) as if that specious book were representative of the skeptics, much less worthy of them. Not much more pertinent is his disclaimer of having argued that Puritanism was an indispensable incentive to scientific work, for I doubt that he has often or seriously been charged with so simplistic a statement. Most generally, however, he rejects the identification of his Ph.D. thesis with the "Merton thesis" on Puritanism and science (though his own emphasis on eponymy in the "Priorities" paper should have convinced him that it is not called that for nothing), and observes a little plaintively that his monograph concerns the whole cultural, social, and economic context of science and technology, and that it adduces much evidence about the importance of mining, navigation, and military needs in the early work of the Royal Society, devoting more pages to these matters than to Puritanism.

That is true, but what Merton does not reckon with—though he recognizes it—is that precisely the question of Puritanism in science served to keep the interest in his monograph alive. In a way, he has himself justified the concentration of his critics on the subject, even if inadvertently. For it is the topic of the most interesting of the few essays on sociology of science that he included in his earlier collection, *Social Theory and Social Structure* (third edition, 1968). Moreover, Storer has chosen the chapter on Puritanism, and not those on economic or military in-

volvement of science, for incorporation in the present work.

Perhaps, therefore, Puritanism in science came home to Merton himself as well as others, and I trust it will not be inconsistent with the profound respect I feel for the whole corpus of his work to say that in treating this question, as perhaps elsewhere on occasion, he sometimes seems to me to misjudge the location of his own greatest strengths. He often emphasizes that sociological research is an empirical undertaking, and though I am not sure he ever says that it is itself science, such is certainly the implication. The reader is often reassured that careful counting has gone on behind the scenes (I have no doubt it has) and that hypotheses are ever being tempered by evidence (I have no doubt they are). It is not there, however, it is rather in his insight into motivation and behavior, individual and collective, that Merton is at his best and deepest. The important quality he brings to his work is psychological acumen, not quantitative rigor, and that makes it a work which is humane, exciting, and inceptive rather than decisive and conclusive. Notice how the latest papers in several lines that he has opened to inquiry end by enumerating the hares he has started rather than the results he has reached. Notice also that the various topics he has pursued in the sociology of science concern the internal functioning of the scientific community rather than its relations with the external structure of society at large, whither an innately statistical and quantitative forte might more naturally carry a sociologist. Perhaps that is why he did not catch (or set) fire over the military and economic associations of science in the 17th century even though he did write about them dutifully.

As for the Weber-Merton analysis of Puritanism itself, carrying it further has been frustrated in part by the feebleness of the statistical approach. For one thing, the numbers involved in counting bluenoses on the science side are very small. For another, the form in which the question has been discussed has required that Fellows of the Royal Society be called Puritans or not, and this at a time when Puritanism was already a century old and had permeated (as it still does) the values of people whose religion was pallid or even different. Another sort of analysis is what is needed, one which Merton would be very well qualified to give but has not given. It would be a combined psychology and sociology of the comparative social dynamics of Puritanism, capitalism, and science, and would be independent of whether particular capitalists and scientists were themselves Puritanical in the religious part of their lives, or whether particular Puritans were capitalistic or scientific in the economic or intellectual parts of their lives. Then the intersections and interactions in which Puritanism, capitalism, and science have reinforced each other in their thrust to change their worlds might become more manifest. As a historian, I am convinced that they did. I am convinced that all three pertain to the forward march of history, to the forces that have modernized society, and that all worked corrosively against the complex of Catholic, feudal, and scholastic forces—if that is the appropriate trinity of traditionalism—wedded to preserving the past rather than to making the future.

To study that would require enlarging the boundaries of the problem. At the other end of Merton's sociological range, however, a methodological restriction obtains which also impedes this particular inquiry, though in a different way. In practice, and maybe in principle, his is a sociology of scientists, taking no account of the content of their work as a factor in the social and institutional relations among them. He makes no distinction, to cite the example that is important here, between the mathematical and the experimental, or, better, between what Thomas S. Kuhn has called the classical and the Baconian sciences in the 17th century (it is not quite the same distinction, but no matter). In the article on history of science in the new *Encyclopedia of the Social Sciences* (1968), and also in the 1973 Sarton lecture before the AAAS, Kuhn has suggested that if one were to concentrate on the latter sciences, and consider how largely Baconian were the collective interests of the Royal Society, the association of Puritanism with the initial impetus and early activity might be made a good deal stronger. I agree, for the economic and technological topics that Merton discussed and wants noticed would thereby be integrated into the argument, which might then turn on the sociology of the Puritan aftermath rather than the theology of Puritan belief. It would be more convincing for the change in focus.

So much for the parts of Merton's work that a historian is best qualified to discuss. I hope it is not parochial to suggest that scientific readers not already familiar with his writings might wish to begin with those papers and issues, and then go back to the first two sections of the book, where the emphasis is more on sociology itself than on its object in knowledge or science. They will thus have become accustomed to his mode of analysis in relation to their own affairs, and will find it no less illuminating in this more arcane area. One of his recurrent preoccupations has been the reason for so studied a neglect of science on the part of other sociologists. He does not himself claim credit for having finally reversed the situation (though in fact no one has a better right to it). Circumstances have done that, and in a manner that fulfills one of the shrewdest of his predictions, which was that interest in the subject would develop when and only when science itself came to be regarded as a social problem or a source of social problems. For difficulties, strains, dysfunctions, and dangers are what attract the interest of social scientists, not mere importance or success.

A second set of concerns is the degeneration of scientific or cognitive disagreement or conflict into the political and sectarian strife of schools and factions, wherein the question ceases to be "Is that right?" and becomes "Why did he say that?" An important paper, "The Perspectives of Insiders and Outsiders," was largely inspired by the recent proliferation of scholarly and sociological enterprises linked to the aspirations of blacks, women, and other segments of society that feel impeded or abused. Merton discusses the matter from the standpoint of whether it is necessary to belong to such a group in order to have knowledge of it, and concludes with an injunction to openness and tolerance all the more welcome for being sociological.

Grouped in the last section are certain recent investigations that touch scientists in their actual careers more closely than any of the foregoing. Merton there brings under scrutiny the procedures by which recognition is awarded and work evaluated together with the equity of the results, and related to that, considers the effects of age and seniority in the life of the scientific community. It may be that his sense of whimsy is sometimes a little like James Reston's in his occasional Sunday column, a serious man jesting

about a recalcitrant subject. Merton calls his paper on the reward system "The Matthew Effect in Science," the allusion being to the statement in the first Gospel, "For unto every one that hath shall be given, and he shall have abundance. . . ." Recognition is the common coin in which scientific rewards are paid, and as in other communities, treasure is unevenly distributed. It is the already prominent scientist whose name becomes associated with projects on which he works with junior colleagues, not the younger people who need the credit. Such collaboration is to their advantage in another way, however, since the notice that the findings attract will also depend on the fame of a name and not just on their importance, though on that too. For beginners the value of working with outstanding people is no mere matter of public relations. The guidance and example, Merton does not hesitate to say the character, of eminent scientists are instrumental in increasing the productivity and effectiveness of those fortunate enough to study with them, particularly in their identification and choice of problems. A significant proportion of Nobel laureates have been trained under older Nobel laureates, whose influence in science becomes then a function of their standing and not mainly of the research that won them the prize.

A more extensive paper, this one written with Harriet Zuckerman, inquires into the refereeing system in scientific publication, an aspect of the institutionalization of science that is coeval with the earliest societies and journals. Comparison with the humanities and social sciences exhibits a very low rate of rejection in science compared with humanistic disciplines; the harder the subject the lower the rate. (That is not his terminology. Always preferring to make a sociological statement, he says of the softer subjects: "This suggests that these fields of learning are not greatly institutionalized in the reasonably precise sense that editors and referees on the one side and would-be contributors on the other almost always share norms of what constitutes adequate scholarship" [p. 472]). It should, at any rate, prove startling to my historical colleagues to learn that leading journals in our field reject 90 percent of the manuscripts submitted to them compared with 24 percent rejected by physics journals. An intensive study of the archives of the *Physical Review* reveals that the judgment of referees is not significantly affected by the age, standing, or institutional affiliation of referees or contributors, and permits the conclusion that physics is well served by the system.

The final paper investigates another factor in science about which much myth and gossip have clustered but little research. Also written with Zuckerman, it is called "Age, Aging, and Age Structure in Science." Among the questions considered is that most famous or infamous one, on which views change as scientists grow older, whether indeed scientific invention is a secretion if not a secret reserved to youth. But I think it will be consistent with the main purpose of this article, which is to draw the attention of scientists to the interest Merton's work holds for them, not to give that answer away, nor even to say what he makes of the question. I shall observe only that this, his most recent paper in the sociology of science, is evidence that one sociologist, in what he will not mind my calling his maturity, need fear no weakening of his powers to see what is deep in things commonly mistaken for obvious, and to make the best and most humane of good sense out of the most unexpected of problems.

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Celebrating a Quinquecentennial

Nicholas Copernicus: Complete Works. I, The Manuscript of "On the Revolutions," Facsimile. Polish Academy of Science, Warsaw-Cracow, 1972 (also to be published by Macmillan, London). xxv, 439 pp. + plates.

Symposium on Copernicus. Papers from a meeting, April 1973. American Philosophical Society, Philadelphia, 1974. xii, 140 pp., illus. Paper, $1. Proceedings of the American Philosophical Society, Vol. 117, No. 6 (pp. 413–552).

Nicolaus Copernicus. An Essay on His Life and Work. FRED HOYLE. Harper and Row, New York, 1973. xii, 94 pp., illus. $5.95.

Nicolaus Copernicus and His Epoch. JAN ADAMCZEWSKI. Scribner, New York, 1974. 164 pp., illus. $7.95.

The Scientific World of Copernicus. On the Occasion of the 500th Anniversary of His Birth, 1473–1973. BARBARA BIEŃKOWSKA, Ed. Translated from the Polish by Christina Cenkalska. Reidel, Boston, 1973. xii, 144 pp., illus. + plates. $19.50.

The Reception of Copernicus' Heliocentric Theory. Proceedings of a symposium, Toruń, Poland, 1973. JERZY DOBRZYCKI, Ed. Reidel, Boston, 1973. 368 pp., illus. + plates. $24.

Vistas in Astronomy. Vol. 17, Copernicus Yesterday and Today. Proceedings of a conference, Washington, D.C., 1972. ARTHUR BEER and K. AA. STRAND, Eds. Pergamon, New York. Scheduled for publication 1 September 1974. About $38.

Anniversary celebrations have become an increasingly popular sport for scientists as well as historians of science. The past decade marked the 400th year since the death of Vesalius (in 1564), the birth of Galileo (also in 1564), and the birth of Kepler (in 1571); the centenary of the birth of George Ellery Hale (in 1864); and the 300th year since the *annus mirabile* when Isaac Newton created so many wonderful things in Woolsthorpe, whence he had gone to escape the plague that had closed Cambridge University.

But of all these occasions, none was so widely observed as the Copernican quinquecentennial in 1973. The reasons are clear—never before have scientists actually celebrated a 500th birthday for one of their own, not to mention the fact that Nicholas Copernicus, a great international figure and symbolic founder of modern science, is also a favorite son of Poland, a brave nation not generously endowed with illustrious men.

Not surprisingly, some of the finest pieces from the great outpouring of publications and symposia originated in Poland, where the astronomer was accorded a particularly high priority. Foremost among them is the Polish Academy of Science's *Nicholas Copernicus: Complete Works*, an ambitious three-volume project planned in separate English, Polish, and Latin editions. The first volume contains a handsome color facsimile of the original manuscript of Copernicus's *De revolutionibus* ("On the Revolutions"). Perhaps the most priceless artifact of the scientific Renaissance, the autograph manuscript has almost miraculously survived the