the mark. And it stimulates a conviction that American science and business will continue to ignore this subject at their peril.

Laurie Brown and R. Yoshida deserve generous praise for presenting this book to English-speaking readers.

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References and Notes

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A Venture in Writing History

The Historical Development of Quantum Theory. JAGDISH MEHRA and HELMUT RECHENBERG. Vols. 1–4. Vol. 1, *The Quantum Theory of Planck, Einstein, Bohr and Sommerfeld: Its Foundation and the Rise of Its Difficulties, 1900–1925*. Part 1, xlviii pp. + pp. 1–372. $33.80. Part 2, vi pp. + pp. 373–878. $36. Vol. 2, *The Discovery of Quantum Mechanics, 1925*. vi, 356 pp. $32. Vol. 3, *The Formulation of Matrix Mechanics and Its Modifications, 1925–1926*. viii, 334 pp. $32. Vol. 4, Parts 1 and 2, *The Fundamental Equations of Quantum Mechanics, 1925–1926* and *The Reception of the New Quantum Mechanics, 1925–1926*. viii, 322 pp. $38. Springer-Verlag, New York, 1982.

Scientists will be disposed to regard this work—which promises to reach nine volumes—as one of great importance, nay, "one of the most significant scientific works ever published" (J. Gribbin in *The New Scientist*, 24 March). They will be badly mistaken.

The distribution of writings on the history of physics has been emphatically bimodal, concentrated upon the 17th and 18th centuries and upon the first third of the 20th century. Works dealing with this latter period, insofar as they are not biographical, again show a decided doublet structure, being concentrated upon relativity and upon quantum theory, especially as it developed in conjunction with problems of atomic physics. The contributors to this literature have been, on the one hand, physicists with historical interests, and, on the other hand, professed historians of physics, with some few individuals seeking to maintain full standing in both camps. By and large the physicists write book-length histories of the whole field, based largely upon the published scientific literature, whereas the historians write narrower and closer studies of particular problems, usually at but article length. Though many aspects of the history of atomic physics and quantum theory before and after 1925 still await close inspection, the number of such special studies is already considerable. Indeed, a recent listing of the *Literature on the History of Physics in the 20th Century* (Office for History of Science and Technology, University of California, Berkeley, 1981) runs 500 pages.

Now comes a physicist who, as he tells us, has since his postdoctoral studies in the early 1950's pointed his steps toward the full and true history of the quantum theory. Over 25 years Mehra sought out every notable theoretical physicist active before his own time—some 100 are paraded in the preface.

> During this long period my collection of notes and transcripts of tape recordings of conversations, discussions and interviews had become quite large. It was supplemented [note what supplements what] by copies of all the relevant original papers, unpublished manuscripts and notebooks, and letters exchanged between the principal quantum physicists. . . . Thus, there resulted vast materials related to the historical development of quantum theory.

Having gotten in his possession "all" the sources, Mehra's only problem was to turn them into history. Here, 30 pages into the 50-page preface signed by Mehra alone, his collaborator, Rechenberg, is introduced—to meet the task of ordering Mehra's "vast materials," filing them in 39 folders "according to specific problem areas" and preparing notes and outlines for Mehra's use in the writing. The 2000 pages under review, distributed over four volumes bound as five, apparently contain the contents of the first 23 of these folders; the remaining 16 are to fill another four or five volumes.

The two tomes constituting volume 1 encompass nearly half these 2000 pages. They are devoted to the quantum theory prior to the creation of quantum mechanics and are arranged topically, with many names and papers cited. However, quantum mechanics itself, in the authors' view, was the work of just six "heroes": Werner Heisenberg, Max Born, Pascual Jordan, Wolfgang Pauli, P. A. M. Dirac, and Erwin Schrödinger. They did it all, "while the others stayed aside and watched their endeavours." Accordingly, volume 2 is Heisenberg's, from his entrance into Arnold Sommerfeld's seminar in 1920 to his revolutionary invention in the summer of 1925. Volume 3 describes the elaboration of Heisenberg's ideas into a matrix mechanics by Born, Jordan, and Pauli, with three-quarters of the volume being devoted to just three papers written in the latter half of 1925. Volume 4 is in two parts bound as one. It is chiefly Dirac's, part 1 being his intellectual biography through the spring of 1926. Part 2, the last 60 pages of the volume, is a hodgepodge headed "The Reception of the New Quantum Mechanics, 1925–1926." This subject, intrinsically far larger, and historically not less important, than the process of discovery here treated so fulsomely, is, impossibly, addressed before Schrödinger's wave mechanics, which is to receive "epic" treatment in volumes yet unpublished.

The coverage being briefly indicated,

> One may ask how our work relates to or compares with the other accounts of the history of quantum theory. The depth and scope of our work are different from any attempted thus far in the field: we bring in all the physical, mathematical and human details to provide the reader a complete account of the old quantum theory and the discovery and development of quantum mechanics. . . . We are aware of the fact that several accounts dealing with certain parts of the story we cover already exist in print. . . . Our aim, however, goes much beyond such works; we want to give the full story of all significant problems and their interplay.

The quotation is in every respect characteristic for Mehra's work: intellectual poverty, pompous pretension, depreciation of the quantity and significance of the extant historical writing in the field. Obviously, as the work is five times longer than any other on the subject, it

must be in *some* sense of greater depth and scope. Yet Mehra does not give any definition of his opus, any explanation of his intent, beyond the claim that it includes everything, does everything, supersedes everything. Though the authors do in fact make all too liberal use of some prior studies, far more numerous are the valuable studies they ignore. I will deal with these issues below; but first the intellectual failings of this misguided enterprise.

What is history? What is the task of the historian? Just as the scientist, through empirical investigations and conceptual constructions, seeks to reveal and then to order the phenomena of the natural world, so also the historian, again by empirical inquiries and mental constructs, seeks to reveal and order the universe of human action, to reconstruct the complex yet integral reality through which the historical actors acted. The written product of such endeavours is history. The principal intellectual task of the historian, his principal means for structuring the past, is organization of his "vast materials." Thus in reviewing a historical work one pays particular attention to interpretative questions. How, one asks, has the author circumscribed his subject, periodized the historical development, defined the features that characterize his historical stages, explained the transition from one to the next? Are his materials vast enough, and has he culled them with sufficient discrimination? Finally, has he presented his material in a form that manifests the structure he has given to the past and persuades the critical reader of its verisimilitude?

That is history; that is what history must be. However, that is not to say history is the only legitimate form of historical publication. The requirements of the scientist who finds that the forest of historical interpretation interferes with his view of the fabled oaks and old chestnut trees may be served by collections of reprints of papers of acknowledged importance, or the collected correspondence of some central figure. Such publications fit well with the prejudice—so well exemplified by Mehra's work—that there are only a relatively small number of important papers in any field, and an even smaller number of "principal actors and heroes." But *history* written from that point of view, as Mehra's is, becomes the more ahistorical the more "detailed and meticulous" its reportage and analysis. A historian who claims, as Mehra does, to have treated all aspects of the historical development of quantum theory "in a deeply human context," and yet describes not the forest but only a few of its largest trees, is misrepresenting his work, even if the reportage is reliable and the analyses acute. This is not merely because the human context is much broader than those few papers and that limited range of interactions, but equally because the more closely one examines those papers and interactions the more necessary is an articulated conception of the scientific milieu within which they arose. In short, what may be just passable in a one- or two-volume history of quantum theory becomes monstrous in an eight- or ten-volume history.

History is *not* one damn thing after another. But these tomes are. What structure they show we owe, by Mehra's elaborate prefatory account, to Rechenberg. Unfortunately that framework remains too weak to bear serious discussion. The authors are so far from recognizing the task of history, such conceptions of the stages and the factors in the development of atomic physics and quantum theory as they offer are so lame, that any criticism of their historical interpretations would miss the mark:

> In Copenhagen, Heisenberg grasped very well, perhaps better than anyone else at Bohr's Institute, the trend of the time: that the direction of physics in fall 1924 was "to get clearer and clearer feelings about how nature worked" (Heisenberg, Conversations, p. 134).

Thus readers desirous of understanding the historical development of quantum theory are thrown by the authors (just as Heisenberg would throw the quantum theorists) wholly upon their powers of intuition. Again and again, the authors start their story over, traversing and retraversing, zigging and zagging, repeatedly passing through the same historical and scientific territory. Never does their account advance along a broad historical front, never do we get a synthetic view of all that bore at any given time on any particular theoretical issue. The best that can be said of their way of proceeding is that the authors thereby provide a proof by exhaustion that certain issues—for example, the use of half-integral quantum numbers—were unceasing preoccupations of the period.

Mehra and Rechenberg have shown us nothing of the pattern woven on Goethe's rushing loom of time. What however of the promised fullness and accuracy of historical detail? As I signaled above, Mehra regards the recollections of the principal actors as being of particular value as sources of such information. This is the common supposition of those without experience in historical research. It reflects not only a gross overestimation of the amplitude, pertinence, and reliability of human memory—in this case three to five decades after the events in question—but equally a failure to distinguish between history and the limited subjective experience of individual historical actors. Neglect of this distinction is buttressed by the scientist's counterpoised convictions that, on the one hand, the *essential* history of his field is incorporated in its conceptual structure—the physicists' history of quantum theory has changed little in its outlines from Rubinowicz's work in the late 1920's to Mehra's work today—and, on the other hand, that only one who was there, one of his own clan who stood very close to the center of the storm, could know the true but secret history of his field. Such convictions remain compatible because, although that secret history, available only during the lifetimes of those who were there, is regarded as true, it is also regarded as inessential.

This, the scientist's view, diverges doubly from that of the historian, for whom historical truth is of the very essence and temporal distance is welcome as affording independence—in particular, independence from the judgments of the participating physicists, who so quickly upon the discovery of quantum mechanics laid down what were thenceforth regarded as the essentials of its historical development. In truth, however, though recollection may add vividness and color, it cannot reliably be used except as embellishment of a picture delineated by written sources from the period. For the period and developments covered by these volumes, the existing correspondence among participating physicists is so extensive and revealing as to render the recollected record very nearly negligible. Of these tens of thousands of extant letters Mehra and Rechenberg have used only a minute fraction. Rather they have larded their pages with quotations from memoir and recollection, quotations whose insignificance as evidence is not wholly obscured by their verbosity and repetitiousness.

Here, as everywhere, the work fails because of the authors' utter lack of criticism—criticism of their sources, criticism of their own work. From volume to volume, paragraph to paragraph, line to line, their statements contradict one another and the historical facts presented—or not presented. Take, say, p. 213 of volume 1. Mehra and Rechenberg begin by asserting Arnold Sommerfeld's interest in the theory of the Zeeman effect (the effect of a magnetic field on spectral lines), citing papers Sommerfeld

wrote on this problem in 1913 and 1914. They then quote from a congratulatory postcard Sommerfeld directed to Niels Bohr on 4 September 1913, omitting however the final two lines, in which Sommerfeld expresses his wish to apply Bohr's theory to the Zeeman effect—the only part of the card that makes any mention of the Zeeman effect. (Some 243 pages later Mehra and Rechenberg will quote those last two lines, mistranslating Sommerfeld's past subjunctive, expressing a polite wish, with an English past subjunctive, thus creating a nonsensical reference to a wish in the past.) Our authors state that "at the end of his letter [sic] Sommerfeld asked Bohr whether he planned to calculate the Zeeman effect from his model" but make no mention of Sommerfeld's expressed wish to do so. Rather they assert a few lines later that "in any case, Sommerfeld did not concern himself with the application of Bohr's atomic model to the problem of the Zeeman effect." However, another three lines and we're told that Sommerfeld "soon . . . became deeply involved in the problem of atomic constitution and in Bohr's theory; especially, he announced a course of lectures on '*Zeemaneffekt and Spektrallinien*' . . . for the winter semester 1914–1915."

Although the contradictions follow one another with more than usual rapidity in the example given, it is in every other respect characteristic. Not less than dubious historical methods, it is subservient, uncritical attitudes that vitiate this work. The authors' reportage is already unreliable because it is so largely guided by the self-imposed obligation to justify traditional attributions of scientific importance. They are fully capable of giving lengthy descriptions of quite misleading or useless scientific "results"—for example, the rules formulated by Carl Runge (1907) and Ernst Back (1921) for the Zeeman effect—treating them as solid additions to the body of physics. In Mehra and Rechenberg's historiography there are no misapprehensions, no blind alleys; "the rational description of nature converges to one and the same scheme no matter what the starting point."

Among the responsibilities falling upon the authors of any scholarly work, and most especially such as claim to be definitive, is the documentation of their facts and the disclosure of their sources. Mehra and Rechenberg, however, especially when relying upon word of mouth, go on for paragraphs describing their heroes' exploits without any indication of their sources. In the case of unpublished correspondence and other documents quoted, it stands somewhat better, for these are nearly always cited; but one who wished to consult the originals would hunt in vain through text and notes for their locations. In stunning contrast with their reticence in respect of unpublished sources, Mehra and Rechenberg have padded the backs of their volumes with redundant bibliographies of published sources. More than 200 pages are filled with reference lists giving citations, in senselessly elaborate forms, of scientific papers of which the greater number are merely referred to in passing in the text, while various historical works cited in the footnotes are omitted.

Hopeless as a work of reference on grounds of reliability alone, these volumes also lack subject indexes, or even analytical tables of contents, and are thus extremely difficult of access, all the more so as a given piece of work may be discussed (albeit repetitiously) in several places. Whatever help the "author index" might have offered here is largely defeated by Mehra's name-dropping. When the number of entries for an individual climbs above half a hundred—as it does in volume 1 alone for Bohr, Boltzmann, Born, Debye, Ehrenfest, and so on—an index has long ceased to be of any use. (Perhaps it was in an effort to alleviate this problem that many page references were simply omitted.)

Exasperating as these deficiences of research and writing are, they are not the most serious of the authors' delicts. As I intimated above, while Mehra and Rechenberg rely most heavily upon their own inadequate efforts for historical synthesis, they are at various points all too deeply indebted to previous historical studies. These debts are not, however, to the historical insights of the literature on which they lean; for insights our authors have no concern. Rather, they have lifted from previous writers the fruits of historical spadework, profiting from the sources these writers identify and their organization of them, retelling the stories they have told, often in very nearly the same words. At the outset, as in discussions of Planck and Bohr, the fact, though not the extent, of indebtedness is acknowledged in footnotes. However, as Mehra and Rechenberg proceed, the citations of the works from which they are borrowing wholesale become fewer, tardier, more obscure, and sometimes wholly absent (as in vol. 1, pp. 364–372 and pp. 459–472; vol. 2, pp. 31–32; and vol. 4, pp. 238–239).

Mehra and Rechenberg's use of the work of Helge Kragh on the discovery of hafnium is a flagrant case. At the end of an eight-page narrative of this dramatic success of Bohr's theory of the periodic system, Mehra and Rechenberg finally cite the author from whom they've taken it: one reference, hidden at the end of a footnote, to a publication by Kragh not centrally concerned with this episode, and couched in a manner that suggests that they owed him only a small point of detail. Borrowings without any acknowledgment whatsoever seem to be limited to doctoral dissertations, as though the authors' rule were that the less widely known their source, the less need to acknowledge their debt.

In science and scholarship alike, the use of others' work without acknowledgment is regarded as reprehensible. In the present case this shades gradually over into another delict not generally taken very seriously by scientists: failure to cite, and to take into account, the work of those who have previously traversed the same historical ground. Indeed, ignoring the "secondary" literature is presumably sanctioned since practiced by many physicists writing on the history of physics. (Thus A. Pais, "Max Born's statistical interpretation of quantum mechanics," *Science* **213**, 1193–98 [17 Dec. 1982], to take an example from recent pages of this journal.) This causes less surprise when one recognizes that for scientists history is not the field upon which they wrestle for truth, but principally their field of celebration and self-congratulation. In such celebratory sagas the citation is a laurel wreath. Obviously the historian with his secondary writings has no claim to a place in the august company of heroes of science.

However intelligible, this circumstance is not without its dangers, both to history and to science. Representations of reality that are not in the service of truth are commonly self-serving. 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, assuring us that by careful checking he has determined that "none of the quantum physicists had made false claims for himself" and this "may be attributed to the impeccable leadership of such men as Planck, Rutherford, Einstein, Bohr and Sommerfeld." Although not more than a half-dozen times in their 2000 pages do Mehra and Rechenberg deign to take issue (impotently) with previous writers on the history of quantum theory, they reach (with presumably unconscious irony) the height of their indignant scorn in rejecting an imputation that Planck had not fully acknowledged one of his intellectual debts.

Within science ethical standards are held inflexibly, uncompromisingly. Scientists rightly fear that were these bulwarks even slightly eroded the resulting suspicion and cynicism would threaten the entire enterprise. But, precisely because these standards are held so rigidly, the realm within which they apply is defined very narrowly; the conduct of historical scholarship is largely excluded. However, with the formation of a Division of the History of Physics within the American Physical Society, and the recognition of history as a legitimate research specialty, supported and encouraged by the society, this line of demarcation cannot hold. The danger is less that one day an eminent historian may become president of the APS than that a physicist of deficient moral character may.

Who, we must finally ask, is responsible for this work? Primarily Mehra, of course. And so he has been held to this point. But "vast materials" are not collected, and 2000 pages—for starters—are not written, printed, bound, and distributed without substantial encouragement and support. I venture that no reputable American press would have published this work in its present form; it would have been regarded as impossible on stylistic grounds alone. Springer-Verlag, however, has in this as in other cases evidently left the setting of editorial standards for its historical publications to the scientists in whom it has chosen, on grounds other than competence in that regard, to place its confidence.

A particularly heavy responsibility for this work must also fall upon those who, being pleased and flattered by the attentions and representations of its authors, gave it support and encouragement, morally and materially. Doubtless many of Mehra's numerous patrons among the nestors of theoretical physics will now in reading his 50-page preface be a bit chagrined to discover how promiscuous their protégé has been. But will they, and the many other individuals and institutions of the Euro-American physics community that have supported Mehra's work and promoted his career, now recognize that they bear a responsibility for this deplorable product that cannot be evaded by pleading ignorance of the canons and literature of history? In all such cases the fault is as the irresponsibility of the act.

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Jerzy Neyman

Neyman—From Life. CONSTANCE REID. Springer-Verlag, New York, 1982, vi, 298 pp. + plates. $19.80.

"For his participation in the Uprising of 1863, [Neyman's grandfather] was burned alive in his house, his lands confiscated, and all his sons except [Neyman's father] sentenced to exile in Siberia." Not quite the typical beginning of a statistician's life story, but Jerzy Neyman's life always tended toward the extraordinary. The first or second greatest statistician of this century, a triple émigré who was born in Russia, claimed to be Polish, did his best work in England, and felt most at home in Berkeley, and a participant, possibly victorious, in the fiercest intellectual battle of 20th-century mathematical history, Neyman is a tempting biographical target.

Fortunately for us, this ripe subject has fallen into experienced hands. Constance Reid, well known for her biographies of Hilbert and Courant, pursued the awesomely vigorous Neyman during the last three years of his life, 1978–81, and wound up knowing more about the great man than he himself did.

Several stories blend skillfully in the narrative: the personal life story of political turmoil and poverty in early 1900's central Europe, which threaten to end Neyman's career before it begins (he didn't publish his first paper until he was 30); the escape to England, the triumphant collaboration with Egon Pearson, which in a few years, 1930–38, generated the dominant theory of modern mathematical statistics, and the building of the great Berkeley statistics department in the years 1938–54; and most of all the unending battle with Sir Ronald Fisher, also the first or second greatest statistician of this century, and the undisputed villain of this narrative.

In 1914 in an obscure university 400 miles south of Moscow, 20-year-old Jerzy Neyman read Lebesgue's *Leçons sur l'intégration* while the Russian army disintegrated to the west. Probability and statistics were rough subjects then, interesting but not well understood mathematically, in a state similar to the current situation of computer science. Lebesgue's book, in the hands of Kolmogorov, produced a fully satisfactory mathematical basis for probability. It also launched Neyman on the road toward mathematizing statistics. Kolmogorov, however, didn't have to deal with Ronald Fisher. Neyman's 25-year debate with Fisher is, quite properly, the crux of Reid's biography. The book's major success is its vivid rendering of this argument, both in personal and intellectual terms, which I will try to summarize here.

While Neyman read Lebesgue, the 24-year-old Fisher, working in the relative tranquillity of England, began his spectacular dual career in statistics and genetics. Considered the world's leading mathematical geneticist, Fisher was even better as a statistician. His approach to statistics was an attempt to extend classical logical inference to the problems of statistical induction. Here is a typical Fisherian result: in sampling from a Gaussian distribution with known variance, all possible information about its unknown mean is contained in the average of the sample. This simple principle, "sufficiency," eluded both Gauss and Laplace.

Fisher's theory of maximum likelihood estimation replaced the method of moments developed by Karl Pearson, Egon's father. Karl Pearson responded to his younger rival with unmitigated hostility, keeping his work out of *Biometrika*, the leading statistics journal, and keeping Fisher himself out of a university chair. By the time Neyman arrived as a student in 1925, England, the birthplace of modern statistics, was a bitterly split camp. After K. Pearson's death Fisher wrote of him, "If peevish intolerance of free opinion in others is a sign of senility, it is one which he had developed at an early age."

Fisher proceeded to develop the same peevish intolerance for Neyman. It is a mark of Neyman's prowess that he became the prime target of Fisher's jealousy. At first all went smoothly. Neyman venerated the slightly older Fisher, who responded with paternal approval. The 1933 Neyman-Pearson paper, containing the famous lemma on optimum hypothesis tests, and the 1934 Neyman paper introducing confidence intervals, are written in a spirit of clarifying and extending Fisher's seminal ideas on likelihood.