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.

More quickly than anyone else, including the authors, Fisher began to perceive the subversive nature of these ideas to his own preeminence. Consider the Neyman-Pearson lemma. In a few short lines the problem of hypothesis testing, which had developed in a two-century swirl of confusing methodology, was reduced to a clean mathematical statement with an elegant *optimal* solution. Similarly, confidence intervals offered a mathematically clear answer to the problem Fisher had addressed with his semimystical fiducial theory. By 1935 Fisher was writing, "Were it not for the persistent efforts which Dr. Neyman and Dr. Pearson had made to treat what they speak of as problems of estimation, by means merely of tests of significance [that is, the theory of confidence intervals], he had no doubt that Dr. Neyman would not have been in any danger of falling into the series of misunderstandings which his paper revealed." This opinion was as inaccurate as it was ungenerous.

By 1938, Neyman felt unwelcome enough in England to consider a position with the mathematics department of the University of California at Berkeley. Gilbert Evans, the skillful chairman of the department, dreamed of "California as a place for a really outstanding statistician, if possible at the level of R. A. Fisher himself." Fisher visited Berkeley in 1936, but the visit was not a success. (Even Oppenheimer found him excessively egotistical.) Neyman was offered the job and accepted. The substitution of Neyman for Fisher changed the course of American statistics.

Neyman's temperament was the opposite of Fisher's and K. Pearson's. He surrounded himself with the best young people he could find, encouraged their work in every possible way (including emergency financing from his own pocket), and took genuine pleasure in their successes. The men and women Neyman promoted and praised were good enough to shift the center of world statistics to the United States.

The shift was more than geographical. Statistics in the United States became strict mathematical statistics, developed in the optimality tradition of Neyman. Neyman at Berkeley and his brilliant disciple Abraham Wald at Columbia were the twin centers of this development. "Decision theory" was the name given to the new systemization of statistical thought. Berkeley became, in the words of the English statistician D. G. Kendall, "the most important and largest statistical center in the world." Neyman organized a series of Berkeley symposiums on mathematical statistics, which generated tremendous international interest and symposium volumes comprising strings of famous papers. (Typically, Neyman always extended invitations to Fisher, who did not attend.)

Drawing of Jerzy Neyman by his wife, Olga Solodovnikova Neyman. [From *Neyman—From Life*]

Reid makes just one major error in tracing this complicated intellectual development, but it is a serious error that deserves discussion. My guess is that most of her readers will conclude that Neyman won the battle and that the war is over. In fact Fisherian ideas have remained dominant in England and show resurgent vigor in the rest of the statistical world, including America.

Why aren't statisticians completely satisfied with the mathematically optimal solutions of Neyman, Pearson, and Wald? One reason is that only very simple problems can be optimally solved. Fisher painted with a rougher but broader brush, which covered more of the problems statisticians face in practice.

A more profound disagreement concerns the relationship of optimality to correctness. Fisher intended his theory to provide logically correct conclusions in statistical inference, just as ordinary logic provides correct deductions in nonstatistical problems. The Neyman-Pearson-Wald theory is neutral on the problem of correctness. Optimal solutions are produced, but the statistician is not instructed which situation to optimize. Fisher's work was far from totally successful. Dissatisfaction with its vagueness and outright contradictions was a driving force behind the much more precise formulation Neyman provided and set the stage for Neyman's immense influence on statistical thinking. Nevertheless, there is a growing consensus that decision theory by itself misses some important part of statistical inference.

Here is a simple example. Suppose the statistician observes independent Gaussian observations with unknown mean μ and known variance 1. The experiment is set up so that with probability .90 one hundred observations are made, while with probability .10 only nine observations are made. In either case, all the information about the unknown mean value μ is in the sample average $\bar{x}$. Fisher's theory says that the *correct* 95-percent confidence interval for μ is $\bar{x} \pm 1.96/\sqrt{100}$ if one hundred observations are made, and $\bar{x} \pm 1.96/\sqrt{9}$ if nine observations are made. In other words we use the 95-percent interval appropriate for the sample size actually observed.

We can state this situation as an optimality problem: minimize the average interval length subject to covering the true value of μ with probability .95. But then Fisher's solution, which is in fact everyone's preferred answer, is not optimum. For example, using the interval $\bar{x} \pm 2.00/\sqrt{100}$ if one hundred observations are made and $\bar{x} \pm 1.70/\sqrt{9}$ if nine observations are made gives coverage probability .95 and shorter expected length. It is easy to find the optimum solution here, but the solution isn't interesting because this particular optimality criterion misses part of the real problem. We need a more relevant optimality criterion, which is not automatically provided by the Neyman-Pearson-Wald theory.

Saying that Neyman didn't win the war doesn't mean that Fisher did. The situation is something of a standoff, with both approaches, and the older Bayesian theory as well, showing strengths and weaknesses. What is not in doubt is that Neyman's work profoundly altered the course of statistical theory. Anyone seriously interested in statistics has to study Neyman.

Anyone interested, seriously or not, in 20th-century mathematical history will get good value from Constance Reid's book. Neyman was that rare combination, a man of extraordinary talent and also extraordinary temperament. Both talent and temperament are drawn beautifully in this fine biography.

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