dently not meant to be taken completely seriously, but I think he goes too far when he accuses Gauss of circular reasoning. I find no foundation for that in what Gauss actually wrote. From the assumption that the arithmetic mean of repeated observations of a single quantity is the maximum likelihood estimate, he derives the normal distribution, and from that the more general principle of least squares for getting maximum likelihood estimates from observations on a number of related quantities. He asserts that the latter principle should be considered to be just as valid as the principle of the arithmetic mean; he does *not* close the circle by claiming that this in turn justifies the principle of the arithmetic mean. (My use of the term "maximum likelihood" is anachronistic; I am not claiming that Gauss had an exact equivalent of the modern notion clearly in mind.)

Gauss argument involves some tacit assumptions, and one can debate the extent to which he was aware of them; the argument is loose by modern standards, but it is not absurd. The extent to which he claimed to be giving a rigorous proof is not clear to me. (Neither Gauss (1809) nor his later extensive exposition of the method of least squares, which appeared in 1821 and 1823 with a supplement in 1826 (Volume 4 of his collected works, pp. 1–93) is at all a treatise in pure mathematics.) He clearly considered the method of least squares to be of great practical value. He was also interested in justifying it philosophically, but he explicitly stated that it cannot be singled out as the only reasonable method on purely theoretical grounds (see Section 186 of Gauss (1809) and Section 6 of the work on least squares mentioned above.)

Professor Le Cam's dissatisfaction with the performance of the hypothetical referee of Gauss (1809) brings to mind one more conversation with Feller, when he expressed some preference for the old days before the present refereeing system. An author who submitted inferior work for publication then ran this risk of damage to his reputation if it appeared. Of course the old system only worked well when the research community was smaller, the volume of publication was much less, and many papers actually got read by at least a few experts, not just counted by deans evaluating candidates for promotion. Gauss' reputation does not seem to have suffered from either the original publication in 1809 or its reprinting over a century and a half later.

I also wonder whether the influence of Gauss' work among nonmathematicians (astronomers, physicists, surveyors) may have played some role (along with the law of eponymy) in attaching his name to the distribution that bears it.

Comment

J. L. Doob

Le Cam's interesting account can be described with only slight exaggeration as a history of (nonrigorous) early research in probability, of probability texts written by mathematicians ignorant of the subject, and finally of frequently clumsy research published before the writers had digested their own work or consulted that of others. Is such the history of all probability research? Of all mathematical research? Should trade secrets be disclosed?

The following quotations are relevant. Lévy, who plays an important role in Le Cam's account, once remarked to me that reading other mathematicians' research gave him actual physical pain. A well known nonprobabilist mathematician remarked to me that the first time a mathematician publishes a research result the treatment is likely to be both wrong and unreadable; the second time the treatment will be correct but obscure; finally, a third treatment may be both correct and clear.

As a partial explanation of the second remark, and of the sometimes unseemly haste to publish, it must be acknowledged that no matter how much a mathematician admires his own work, the writing of it may finally make it so loathesome to his sight that he hastens to send it off for premature publication.

Influential on the nature and speed of probability research is the fact that probabilists, until about the last 30 years, have labored under the psychological disadvantage that their field was not considered a mathematical discipline by their colleagues, who for one thing did not understand why standard mathematical nomenclature was insufficient, why old concepts had to be rechristened "random variables" and "expectations." Moreover, probability books were full of nonmathematical concepts: dice, gambling houses, Peter, and Paul.

Even as late as the 1930s it was not quite obvious to some probabilists, and it was certainly a matter of

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