was no requirement that *every* new report on an already marketed drug be submitted to the FDA. Once the NDA became effective, additional data on the drug were not filed unless they represented unexpected and serious new hazards. Since Lowinger's survey covered the period 1954 to 1966, a number of his reports may have failed to reach the FDA because of the above considerations. Without further information, it is impossible to ascertain whether all, some, or none of the cited failures to report were really delinquencies. This, in turn, vitiates the final recommendation that investigators be required to assume the additional burden of sending reports directly to the FDA.

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Statistical Uncertainties

Most of us in the business of statistical consulting would gladly display bumper stickers proclaiming "WIPE OUT $A \pm B$." Eisenhart's article ("Expression of the uncertainties of final results," 14 June, p. 1201) is a much-needed contribution to this campaign. However, his nearly all-inclusive use of the term *uncertainty*, even with the modifying words, gives the reporting scientist what amounts to a cop-out phrase. Wouldn't it be better to use such established statistical concepts as *estimated standard deviation, confidence limits, sample size, degrees of freedom,* and *tolerance limits*?

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Eisenhart leaves very little uncertainty as to proper statement of a physical quantity. I only wish that someone would equally clarify some matters of biometrics. For example, the statement, "The average height above ground of an ear of American corn is 1.5 ± .5 meters," certainly does not mean that we are unable to measure corn within a meter. It could mean that one half the corn will ear at between 1 and 2 meters, or it could refer to the standard deviation.

The statement, "Hybrid corn type X ears at 1.5 ± .1 meter," could mean

that one half of this superior hybrid ears out at between 1.4 and 1.6 meters. Again, this is distinctly a statement of a property of the corn, not of my precision. A second name and number is needed to describe the uncertainty of determination of an inherent dispersion about a mean. For instance, I could come forth with the true but idiot statement that the average weekly wage is 1.986 ± .02 dollars per hour, where the .02 represents not variance in wage but a disagreement among the findings of several pollsters in the same area.

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I welcome Tietjen's and Moore's support in my campaign to wipe out or at least curb "$a \pm b$." They complain, however, that my "nearly all-inclusive use of the term *uncertainty* . . . gives the reporting scientist what amounts to a cop-out phrase." The aim of "my" recommendations was, of course, not to aid "copping out"; but rather to foster separate, adequate, and lucid reporting of the random and systematic components of uncertainty, as a first step in the direction of clear and adequate reporting of details needed for later critical evaluation.

Some scientists do elect to take the easy way out. They state only their personal estimate of the overall uncertainty, without details of the various components thereof and how they were combined. The consequence is "loss of information through over-simplification" (*1*). Such brevity may expedite a paper's publication, but at the risk of its joining the useless literature explosion (*2*). My colleagues in the National Bureau of Standards Office of Standard Reference Data find that an appalling fraction of the literature in any specific field contains data not worthy of critical evaluation. Estimates of the faulty fraction range from 50 to over 90 percent (*3*). The three principal reasons for failure appear to be: the experimental work was done incorrectly; the sources of uncertainty were not analyzed; or the work was not reported in sufficient detail to permit evaluation.

With regard to the five "established statistical concepts" cited by Tietjen and Moore, three of these are mentioned explicitly, and nothing in our recommendations precludes the use of *confidence limits* or *tolerance limits*, when appropriate. They were not in-

troduced because careful discussion of the operational difference in their meanings, and of their unsatisfactoriness when based entirely on small samples, would have diverted attention from the general theme.

The situation in biometrics pointed out by Foster certainly calls for avoidance of "$a \pm b$" without qualification. As a former biometrician, my feeling is that the needs of biometrics will usually be met by the recommendations given for the case of "systematic error negligible, imprecision not negligible" provided that any "standard deviation" reported is clearly identified as relating either to the *standard deviation of the population* of animals, plants, and so forth, or to the *standard error of the (reported) mean*; and in the latter case it is made clear whether it connotes sampling variation, measurement error, or a combination of both.

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1. H. H. Ku, *Meas. Data* **2**, 72 (July–August 1968).
2. L. M. Branscomb, *Sci. Res.* **3**, 49 (1968).
3. *Status Report–National Standard Reference System, April 1968*, Technical Note 448 (National Bureau of Standards, Washington, D.C., 1968), p. 1.

Can Intelligence Be Measured?

Frank A. Meier (Letters, 27 Sept.) says "Whether or not there are racial differences in intelligence is a legitimate subject for scientific investigation; no one is disputing that fact."

Some *are* disputing that fact. Intelligence has yet to be defined in demonstrable, verifiable, scientific terms which are susceptible to public agreement. In fact, all the so-called research concerning "intelligence" seems to be manipulating a reciprocal measure of accumulated ignorance more than any measure of inherent capability. At least, a child who fails to recognize a noun in a sentence and therefore later does not capitalize personal pronouns, the experts deem "low in intelligence" . . . The word "intelligence" has been a verbal booby trap for centuries. Better forget it.

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