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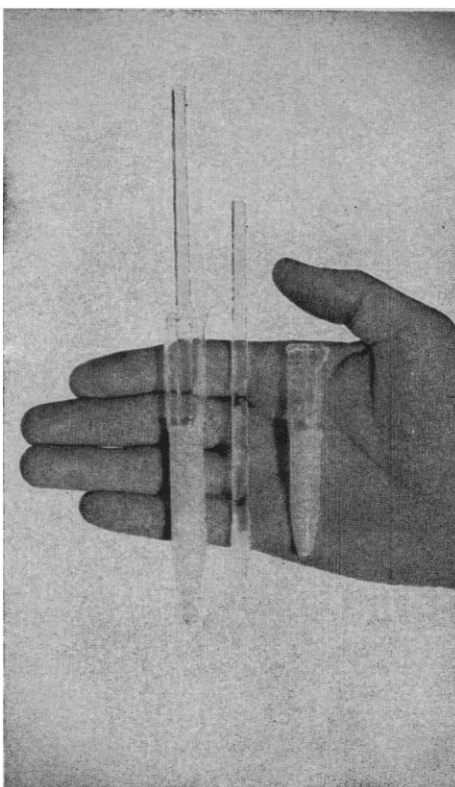
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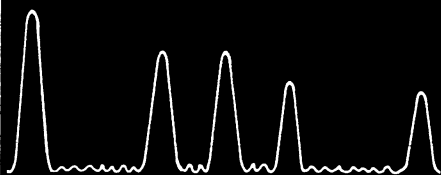


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Second Printing July 1960

AAAS Symposium Volume No. 52

EVOLUTION OF NERVOUS CONTROL FROM PRIMITIVE ORGANISMS TO MAN

Editor: Allan D. Bass

1959, 240 pp. \$5.75, AAAS members' prepaid orders \$5.00

From a review in the *Psychiatric Quarterly*, January 1960:

This book is another in the superb series of monographs put out by the American Association for the Advancement of Science. . . . The text is actually a very readable review of some of the major research going on in various phases of neuropsychiatry.

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viewer's remark that the statement was so unacceptable that it had to be rewritten, the truth may be ascertained by any reader who cares to compare the first statement, mainly written by social scientists, with the second statement, mainly written by physical anthropologists and geneticists. The difference is as between Tweedledum and Tweedledee.

ASHLEY MONTAGU

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Princeton, New Jersey

I am sorry if I misrepresented Montagu's role in the preparation of the first UNESCO Statement on Race; I was reflecting what I feel to be the opinion of many physical anthropologists. Since it is only natural that he would be modest on this score, perhaps others connected with the project will set the record straight.

In the final sentence of his letter Montagu uses a literary reference to say that the second UNESCO Statement on Race differs only insignificantly from the first. This alleged equality is supposed to prove that the first statement was acceptable and did not need rewriting. Why then was it necessary to go to all the trouble of preparing a second statement? And why does Montagu take up space in his textbook with two "identical" statements? Here it is pertinent to point out that Comas, who was a member of the first committee, includes in his textbook *not* the first statement but the second. Why has he, a renowned physical anthropologist, abandoned his own committee's statement, unless he now feels that it is unacceptable?

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Smithsonian Institution,
Washington, D.C.

Authors as Indexers

John R. Clark's letter [*Science* 133, 1040 (7 Apr. 1961)], suggesting that authors are best qualified to index their own books, misses the same point—of some moment for scientists and for scholarship as a whole—that is overlooked by the advocates of bibliographical machines.

Actually, of course, authors are seldom qualified to do indexing. Only occasionally can they do half as well as an experienced professional indexer. The fact that authors or publishers, or both, are frequently unwilling to pay a professional illustrates nicely their underestimation of the problems involved.

Aside from such general considerations as the special nature of indexing technique and the fact that some specialists cannot write intelligible prose without help, authors nearly always

have special slants or blind spots that make it difficult for them to do a good index. An individual may be the authority on the content of his particular book, but it does *not* follow that he has a good enough over-all, objective view of the whole field to which his book is a contribution to do a competent piece of indexing. This is particularly true in those fields where, for various reasons, terminology is not stabilized.

The point overlooked by both Clark and the gadgeteers is that research, writing, publishing, indexing, literature searching, and criticism are all parts of an extremely complex, ever-changing social process in which more judgments and decisions, involving values as well as subject-matter technicalities, must be made than any one person can handle.

HENRY BLACK

*Bibliographical Services,
New York, New York*

Statistical Evidence

Warren Weaver's statement ["The disparagement of statistical evidence," *Science* 132, 1859 (1960)] that "statistical evidence is, in essentially all non-trivial cases, the only sort of evidence we can possibly have" seems to me to be as "wholly unwarranted" as is the practice he very properly condemns—namely, the "automatic discarding of evidence because it is statistical." Statistical evidence is usually very useful, sometimes essential, but there certainly are many kinds of useful evidence that are not statistical in the usual meaning of that term. Our belief that the earth is not flat, that it revolves once in 24 hours, and that it completes an orbit around the sun in a year is not based on statistical evidence. Neither does our acceptance of the theory of evolution, or of a dozen other theories that might be mentioned, depend to any great degree on statistical evidence, although these theories may be and often are supported by such evidence.

The difficulty is partly semantic. Some writers seem to regard as statistical practically any method of dealing with quantitative data, but usually the term implies frequency distributions, standard errors, analysis of variance, correlation coefficients, and so on. Presumably it is these latter that Weaver had in mind. If so, he certainly must realize, on second thought, that failure to use these techniques does not automatically negate the usefulness of quantitative data. Mendel, for example, did not use them, and yet he revolutionized our ideas of heredity.

Weaver is not the first to imply or state that statistical methods are essen-

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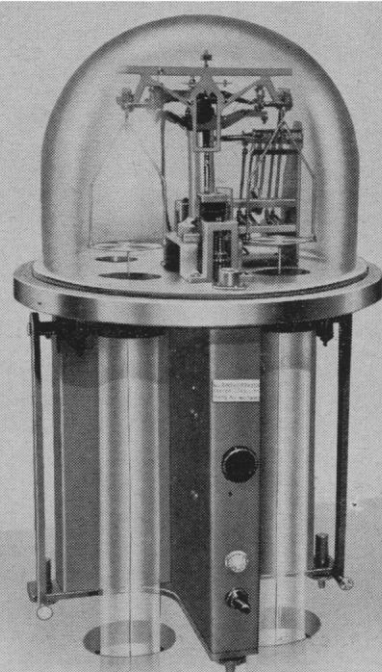
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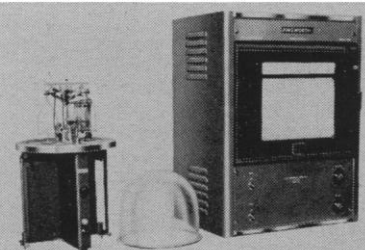
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