LETTERS

History of Science: Perceptions

Several statements in the report of my lecture on history of science at the AAAS annual meeting (News and Comment, 25 Jan., p. 389) require correction. A slip of memory evidently led me to call Otto Hahn's collaborator Strassner instead of Strassmann. More important, the organizer of the colloquium "Do scientists have blood on their hands?," who was not known to me, has since persuaded me that my perception of the tenor of the principal presentation was unduly affected by the provocative choice of words for the title from Robert Oppenheimer's famous statement to Truman.

The presentation was by a serious historian specializing in the political role of science in the early atomic age; and I was at fault in stating that no one present was knowledgeable about the technical aspects of nuclear weapons. Actually, several physicists and others knowledgeable about these issues were in the room, and it was inappropriate to cite the occasion to illustrate the proposition that judgments about the political morality of decisions to develop and employ atomic weapons have too often been uninformed with respect to the precise technical prospects at critical junctures.

The final point concerns the reporting of my lecture. I did not intend to leave the impression that personality has no place in the history of science. My view is the contrary, and I believe I observed that even scientists, when they take any interest at all in the history of science, are likely to fasten on minor matters of gossip or scandal instead of on content.

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One would never guess from Gillispie's lecture at the AAAS annual meeting or from William J. Broad's account of it that the history of science is in a period of intellectual excitement and growth unmatched since the 1930's. Historians of science are reaching out to new problems and methods. They are learning ways of analyzing the creative process and the diffusion of ideas as social processes. With historians of technology and medicine they are analyzing the two-way interaction between basic research and practice. Joined by recruits from general history and the social sciences, historians of science are finding wider audiences in these allied disciplines. Especially for those who, like myself,

came to history from careers in science, the past decade has been one of enormous intellectual refreshment and progress. The history of science is flourishing and growing in an otherwise depressed academic market.

Of what, then, is Gillispie complaining? He alleges that standards of scholarship are declining. I disagree. There are *different* standards now from those of a generation past; but not inferior standards—quite the contrary. There is just no question that standards of scholarship, sophistication in the use of archives, and standards of intellectual significance are much higher now than they were a decade ago; and they continue to improve, markedly among younger historians. The "decline of standards" is an old trick. A century ago the defenders of compulsory Greek cried "declining standards" to prevent the invasion of college curricula by the experimental sciences. This kind of argument may be good politics, but it is not good policy or good history.

Gillispie warns that the new historians of science are undermining the authority and public support of science by talking about scientist-entrepreneurs and scientist-politicians. I think the real danger is misplaced idealism. Can we really doubt in 1980 that the health of science depends on scientists' entrepreneurial and political skills? Is it wise to base public support for science on a false image of scientists as apolitical, isolated intellects and truth-seekers? To do so is to court disaster, for when the inevitable disillusionment comes it will indeed breed disrespect and cynicism. Historians and sociologists of science must contribute to an honest and realistic picture of the scientific enterprise as a social institution, not different in any fundamental way from other economic, cultural, or political institutions. To counsel historians to put scientists back in an imagined ivory tower is a great disservice both to the history of science and to science itself.

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Occupational Lead Exposure and Cancer

Recent issues of *Science* have contained comments (*1*) on the role of occupational and environmental factors in cancer causation and of epidemiology in identifying such associations. In light of this interest, we present here a reevaluation of data previously interpreted as supporting the noncarcinogenicity in humans of lead, one of the most ubiquitous substances in the environment.

In 1975, Cooper and Gaffey (*2*) reported on a cohort of 7032 men employed from 1946 through 1970 for one or more years in lead production facilities or battery plants. The stated objective of the study was to determine the mortality patterns of "individuals whose levels of lead absorption were below those associated with plumbism, but above those regarded as normal in the general population." Data on actual airborne lead concentrations were reported not to be available. Employment histories of cohort members were obtained from company records. Vital status was determined through December 1970 for all but 2 percent of the smelter workers and 5 percent of the battery plant workers. For 18 smelter workers and 71 battery plant workers who had died, but for whom death certificates were not obtained, the distribution of individual causes of death was assumed to be the same as for individuals whose certificates had been obtained. Expected numbers of deaths were determined on the basis of rates from the U.S. male population. Standardized mortality ratios (SMR's) were calculated as 100 times the ratio of observed to expected deaths. Statistical significance of the SMR was determined by first calculating the standard error (S.E.) of each SMR with the technique developed by Chin Long Chiang (*3*). If an SMR deviated from 100 by more than

$$z_{(1-\alpha/2)} \times \text{S.E.}$$

it was interpreted as significant at the 100α percent level.

The SMR for all causes was 107 for smelter workers and 99 for battery workers. According to Cooper and Gaffey (*2*), deaths from all malignant neoplasms were excessive in smelter workers (69 observed versus 54.95 expected, $P < .05$), but not in battery plant workers (186 observed versus 180.34 expected). An excessive, although not statistically significant, number of deaths resulting from cancer of the digestive organs and of the respiratory system were reported among both smelter and battery plant workers.

In the study by Cooper and Gaffey it appears to us that there are errors in the way they determined statistical significance. First, according to Armitage (*4*) the formula for the S.E. of SMR should read $\text{S.E.} = \sqrt{100 \times \text{SMR/expected}}$, rather than $\text{S.E.} = 100 \times \text{SMR/expected}$,