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 $\text{S.E.} = 100 \times \text{SMR/expected}$,

Table 1. Expected and observed deaths resulting from specified malignant neoplasms for lead smelter and battery plant workers and levels of significance by type of statistical analysis according to one-tailed tests.

Cause of death (ICD* Code)	Number of deaths		SMR†	Probability		
	Observed	Expected		Poisson‡	This analysis§	Cooper and Gaffey‖
Lead smelter workers						
All malignant neoplasms (ICD Code Nos. 140–205)	69	54.95	133	< .02	< .01	< .02
Cancer of the digestive organs, peritoneum (ICD Code Nos. 150–159)	25	17.63	150	< .03	< .02	< .05
Cancer of the respiratory system (ICD Code Nos. 160–164)	22	15.76	148	< .05	< .03	> .05
Battery plant workers						
All malignant neoplasms (ICD Code Nos. 140–205)	186	180.34	111	> .05	> .05	> .05
Cancer of the digestive organs, peritoneum (ICD Code Nos. 150–159)	70	61.48	123	< .05	< .04	> .05
Cancer of the respiratory system (ICD Code Nos. 160–164)	61	49.51	132	< .03	< .02	< .03

*International Classification of Diseases. †SMR values were corrected by Cooper and Gaffey for missing death certificates under the assumption that distribution of causes of death was the same in missing certificates as in those that were obtained. ‡Observed deaths were recalculated as follows: adjusted observed deaths = (given SMR/100) × expected deaths. §Given $z = (\text{SMR} - 100)\sqrt{\text{expected}}/100$. ‖Given $z = (\text{SMR} - 100)/\sqrt{100 \times \text{SMR/expected}}$.

when SMR = (observed/expected) × 100. Second, even for a two-tailed test the corresponding z value for 1 percent level of significance is 2.576 instead of 1.96, as stated by Cooper and Gaffey (*2*). Third, although the formula (S.E. = $\sqrt{100 \times \text{SMR/expected}}$) is appropriate for drawing confidence intervals, it should not be used as a test of significance, as Cooper and Gaffey did. Since they were presumably interested in whether SMR's for lead workers were equal to or greater than 100, the S.E. for the test of significance relative to an SMR of 100 should be S.E. = $100\sqrt{1/\text{expected}}$ as shown by Armitage (*4*). Consequently, the test statistics should be

$$z = \frac{\text{SMR} - 100}{100\sqrt{1/\text{expected}}} = (\text{SMR} - 100) \times \frac{\sqrt{\text{expected}}}{100}$$

Fourth, since the objective of this type of study is to determine whether the mortality of the lead workers is excessive as compared to that of men not occupationally exposed to lead, a one-tailed test would be more appropriate than a two-tailed test.

The apparent errors stated above substantially alter the results and interpretation of Cooper and Gaffey's study pertaining to cancer mortality. Table 1 shows the P values obtained by different statistical analyses.

For smelter workers, a significant excess ($P < .05$) of mortality is demonstrated for all three causes of death indicated in Table 1 regardless of the type of statistical analysis used. The only exception to this trend is that the analysis used by Cooper and Gaffey indicated a level of significance at $P < .06$ for cancer of the respiratory system.

For battery plant workers, the number of excess deaths from all malignant neoplasms is not statistically significant. However, the number of excess deaths from respiratory system cancer is significant regardless of the type of statistical analysis used. The number of excess deaths from cancers of the digestive organs and of the peritoneum is also significant when two of the three types of analysis are used, the only exception being the Cooper and Gaffey method, which results in a P value of .052.

Reanalysis of Cooper and Gaffey's data demonstrates a significant excess of mortality from two categories of cancer among workers exposed to lead. The magnitude of the risk of cancer among the lead-exposed workers in Cooper and Gaffey's study may still be underestimated, as the latency period for 59 percent of the smelter workers and 36 percent of the battery plant workers was less than 20 years. The International Agency for Research on Cancer (IARC) has judged latency periods 20 years or longer to be relatively more sensitive in the identification of a carcinogenic risk by epidemiological means (*5*). Numerous studies have shown less total mortality and mortality from cancer (in comparison to the expected mortality in the general population) in industries known to be associated with excessive cancer risk. Failure to analyze data by latency categories would presumably result in a dilution of findings in studies of any disease characterized by long latency.

Observation of a significant excess of cancer in two independent populations exposed to lead in two different industrial settings lends credibility to the suggestion that lead is an etiological factor. Further indications that lead plays a role in this excess of cancer in humans come from laboratory studies demonstrating in vitro malignant cell transformations (*6*) and from previously conducted carcinogenesis bioassays demonstrating oncogenicity in experimental animals (*7*).

The study by Cooper and Gaffey (*2*) clearly demonstrates the need for use of appropriate epidemiological methodology and statistical analyses and for a full presentation of data in a format that permits an assessment of latency. Concerns about appropriate techniques for handling data in epidemiological studies were suggested several years ago at a workshop sponsored by the IARC (*5*) and more recently by the Epidemiology Work Group of the Interagency Regulatory Liaison Group (*8*). We hope these recommendations can be put into practice in any further analyses of data from this study or in any epidemiological study bearing on the carcinogenicity of occupational or environmental factors.

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