LETTERS

Irked by the IRS

At the AAAS annual meeting in Boston in 1976, I described the scientific journal as "an endangered species" (*1*). After reciting a long litany of grave problems that were besetting the journals, I managed a weak smile and concluded with the hope that the journals would somehow escape extinction. It then seemed that the problems, economic in nature, might possibly be resolved.

Unfortunately, the problems look worse today than they did then. Printing and production costs continue to escalate at an alarming rate, at the same time that our principal (library) market is increasingly impoverished. To further unbalance this equation, the Postal Service is increasing second-class mail rates at a dizzying pace, and the new Copyright Act, by requiring *individual* transfer of copyright from author to publisher, is giving publishers an administrative (and expensive) nightmare.

And now, poised to administer the coup de grace, the Internal Revenue Service (IRS) rears its unlovely head. The frontal attack by IRS on six of our major scientific and engineering societies, as described by John Walsh (News and Comment, 23 June, p. 1369), may ultimately turn out to be life-threatening to at least some of our journals (and perhaps societies). A threat by IRS to change an organization's status from 501(c)(3) to 501(c)(6) is worrisome; their threat to revoke entirely the tax-exempt status of the American Chemical Society (ACS) and the American Institute of Physics is frightening.

As to our scientific journals, the ominous position taken by IRS in finding fault with the "practice of setting nonmember subscription rates for ACS publications higher than for members" goes right to the jugular of almost all society journals. The practice of providing journals to members as "part of dues" or through some similar mechanism is almost universal among scientific societies. And it always has been. For the IRS to suddenly find something sinister or illegal in this basic function of scientific societies is incredible.

In fact, "cheap" prices to members also means "cheap" (although higher) prices to nonmembers. A major reason that journals published by societies are "cheap" (compared with commercial journals), even for nonmembers, is that the mass distribution to members translates to relatively low unit printing costs, making the cost reasonably low to all subscribers.

Many society journals already pay taxes on journal advertising, as "unrelated business income." Walsh is misleading in indicating that c(6) organizations pay taxes on journal advertising revenue, whereas c(3) organizations do not. Both types of nonprofit organizations must pay taxes on ad revenue when it is classified as unrelated business income.

Perhaps it is possible that ACS will be able to convince the IRS that a reasonable number of tax dollars will continue to flow into the U.S. Treasury if societies are allowed to continue with their longstanding and reasonably successful pricing policies. If, instead, societies must price their own members out of the market, society journals could well move from the "endangered" list to the "extinct" list; and Uncle Sam, as well as scientists everywhere, will be left empty-handed.

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References

1. R. A. Day, *Am. Soc. Microbiol. News* **42**, 288 (1976).

Beryllium: Carcinogenicity Studies

Both government and independent scientists involved in research leading to the identification of beryllium as a carcinogen in humans have had numerous inquiries concerning the accuracy of Deborah Shapley's article "Occupational cancer: Government challenged in beryllium proceeding" (News and Comment, 2 Dec. 1977, p. 898). In view of the misleading nature of that article, we feel a factual response is necessary.

In the article, concern is expressed about fair play in the conduct of epidemiological studies and government regulatory processes. With regard to this issue, it should be recognized that, in 1975, the beryllium industry and its consultants proposed (*1*) that past studies of workers exposed to beryllium be updated and that additional studies of several working populations exposed to beryllium be initiated. After those proposals were made, the National Institute for Occupational Safety and Health (NIOSH) independently undertook and completed one of the recommended studies, an update (*2*)—referred to as "Bayliss III"—of a previous study of mortality among employees of a beryllium production plant in Reading, Pennsylvania. NIOSH also completed a study (*3*) of mortality among subjects in the U.S. Beryllium Case Registry. Concurrently, Mancuso (*4*) updated his previous study of cancer mortality among workers from two beryllium production facilities. In contrast, during this same time period, and despite their recommendations, industry and its consultants neither sponsored nor undertook a single epidemiological study.

Although other studies are briefly mentioned, discussion in the *Science* article is restricted to the NIOSH update of the study of workers at the Reading beryllium production facility. The important results of the other epidemiological studies (*3–5*) also indicated an increased risk of lung cancer mortality among subjects exposed to beryllium. Using data from the Social Security Administration, Mancuso (*4*) found an increased risk of lung cancer mortality among workers occupationally exposed to beryllium at two production facilities, a Kawecki Berylco Industries, Inc. (KBI) facility in Pennsylvania and a Brush Wellman Inc. (BW) facility in Ohio. Similarly, NIOSH (*3*) found an excess of lung cancer mortality in a subcohort of individuals entered in the Beryllium Case Registry with a diagnosis of prior beryllium-related pneumonitis or bronchitis. These individuals had had short-term exposure to beryllium, an observation consistent with findings in the NIOSH study of KBI workers (*2*), the recent Mancuso study of KBI workers and BW workers (*4*), and an earlier Mancuso study of workers who had previously had beryllium-related pneumonitis (*5*). Also, the results of these epidemiological studies are consistent in general with numerous animal bioassay studies (*6*) demonstrating that beryllium is carcinogenic by several routes of administration and in many species, and specifically with animal bioassay results (*7*) demonstrating induction of lung cancer in 51 percent of the exposed animals by a single dose of beryllium oxide. The significance of the positive findings of these particular studies are not mentioned in the news article.

It is stated in the article that "in the early 1970's, few people paid much attention to the carcinogenic potential of beryllium to humans, particularly since the only two well-known studies of the subject . . . found no unusual incidence of lung cancer." This statement does not acknowledge the existence of Mancuso's 1970 study (*5*), the beryllium industry's awareness of that study, and its desire for NIOSH to refute the findings of the study, as verbally expressed in 1973 (*8*). The statement also does not acknowledge the many shortcomings of the previous NIOSH epidemiological studies of populations exposed to beryllium—referred to as "Bayliss I" (*9*) and "Bayliss II" (*10*). Some of these shortcomings

were stated in the Bayliss I manuscript (*9*, p. 94), verbally repeated to the beryllium industry in 1973 (*8*), and published in 1975 (*11*). One of the major shortcomings of these first two Bayliss studies was that the latency period allowed for was not long enough for beryllium-induced cancers to become clinically manifest.

The continued reluctance of industry and its consultants to accept the carcinogenicity of beryllium is reminiscent of the past denial of beryllium toxicity. In this regard, the *Science* article states that, since the 1940's, when the beryllium industry came of age, it was known that beryllium is toxic to humans, causing a disease known as berylliosis. However, in 1943, the U.S. Public Health Service concluded that "beryllium itself is not harmful" (*12*). In 1949, an article entitled, "Beryllium's toxicity is largely myth," denied that beryllium alloys were toxic to humans (*13*). Two years later, the following statement in *Lancet* further perpetuated the myth of the nontoxicity of beryllium: "To charge such an admirable metal with having poisonous properties is about as distasteful as accusing a trusted butler of stealing the family plate" (*14*). Thus, even though beryllium was introduced into American commerce in 1931, its general toxicity to humans was still being denied in the 1950's.

It is stated in the *Science* article that, since the 1940's, industry has had to limit beryllium exposure to 2 micrograms per cubic meter. However, it was not until 1949 that the Atomic Energy Commission recommended a 2 $\mu g/m^3$ guideline (*15*). In 1971, this guideline was adopted by the Occupational Safety and Health Administration (OSHA) as an official standard (*16*). At the recent OSHA public hearing on beryllium, representatives of the primary production segment of the beryllium industry admitted that areas of their plants were not in compliance with the 2 $\mu g/m^3$ health standard (*17*). Furthermore, during the period 1968–1972, environmental sampling by NIOSH (*18*), the Massachusetts Institute of Technology (*19*), the Pennsylvania Department of Environmental Resources (*20*), and OSHA (*21*) revealed airborne concentrations of beryllium in the primary production industry facilities that grossly exceeded 2 $\mu g/m^3$, and in several operations exceeded 1000 $\mu g/m^3$ (*19, 20*). Thus, evidence clearly indicates that guidelines recommended by the government approximately 30 years ago (which have been the legal OSHA standard since 1971) are still not being adhered to by the beryllium production industry.

With regard to government transmission of data bearing on the human carcinogenicity of beryllium, it is stated in the *Science* article that representatives of BW began asking NIOSH for results of the Bayliss III study after having learned of its existence "partly" from a Cleveland, Ohio, newspaper. Clearly, industry had knowledge of this ongoing study as early as 1973 (*8*), through contact with NIOSH officials who were trying to clarify the data previously submitted by industry and upon which the Bayliss I and II studies had been based. Moreover, a freedom of information (FOI) request from the Cleveland newspaper was only responded to after copies of the preliminary study results had been released to management and labor representatives of the beryllium production facility under study. BW was stated to have made requests for the underlying data throughout 1976. Actually, between December 1975 and August 1977, NIOSH received a total of nine FOI requests from BW and KBI. All were fulfilled but one, which was denied on the basis of unwarranted invasion of personal privacy.

It is stated in the *Science* article that, during the OSHA public hearing on beryllium, NIOSH continued to hand over information at the last minute and then only in response to formal FOI requests. This statement does not acknowledge the frequency and magnitude of industry's requests and their impact on NIOSH during the critical period of preparing reports of study results and testimony for presentation at the OSHA hearing. In the interest of a balanced viewpoint regarding the government's release of data, it should be noted that well over 500 hours of clerical personnel time were required to search for requested information. This does not include numerous hours of staff time required for copying and collating, computer programmer staff time for data processing, and professional staff time for overseeing the deletion of personal identifiers, all of which was at the expense of undertaking research on other potential occupational hazards. This latter type of staff time cannot be charged to an initiator of an FOI request.

With regard to specific study results, industry consultants Brian MacMahon and H. Daniel Roth have criticized the NIOSH Bayliss III study for failing to use lung cancer death rates specific for Reading, Pennsylvania, "an old industrial town" having a lung cancer mortality rate higher than that in both the surrounding county and the United States as a whole. These consultants failed to acknowledge data submitted by NIOSH during the OSHA beryllium hearing which clearly indicated that a majority of the working population at the KBI facility did not reside in Reading city proper (*22*). Thus, the use of Reading city-wide rates would have been scientifically inappropriate. Furthermore, the NIOSH use of lung cancer mortality rates of white males in the United States to compute expected deaths led to an *underestimate* of the true risk in the study cohort. This is so because the average annual age-adjusted lung cancer mortality rate for the past 20 years in Berks County has been significantly *lower* than that of the U.S. white male population.

Consultants to the beryllium industry are reported to have testified that the NIOSH study was inconsistent with a theory of carcinogenesis, in that it failed to exhibit a dose-response relation as measured in terms of duration of employment and lung cancer mortality. It must be recognized, however, that duration of employment, in the absence of detailed information on the environmental concentrations of beryllium, may not be a valid measure of total beryllium exposure. For example, individuals exposed to high concentrations over a shorter period of time could have a total lung burden of beryllium as great or greater than individuals exposed over a longer period of time to lower concentrations. In support of this are observations that the body does not readily clear beryllium, as demonstrated by the detection of elevated concentrations of beryllium in body tissue more than 20 years after termination of occupational exposure (*23*). Also, among the 3055 individuals comprising the study cohort, only 519 had accumulated five or more years of employment. Among those 519 study cohort members, only 200 (632 person-years at risk) were observed 25 or more years after onset of employment, a time period stated by the International Agency for Research on Cancer to be associated with a relatively greater sensitivity for the detection of an occupational cancer risk (*24*). On the basis of sample size alone, one could not detect a risk of mortality from lung cancer that is five times greater than average among individuals having this duration of employment and latency classification (*25*). Thus, the inability of the NIOSH study to demonstrate a "dose-response relation" in terms of duration of employment must be viewed as a function of a small sample size. Consultants to industry acknowledged this fact during the OSHA public hearing on beryllium (*26*). These consultants now appear to overlook the fact that the NIOSH study also provides no data to *refute* a dose-response relation.

According to the *Science* article, Roth claimed that the excess of lung cancer mortality among workers exposed to beryllium could be explained by correcting for cigarette smoking. This conjecture is in conflict with analyses of data from a 1967–68 Public Health Service survey of smoking habits among beryllium workers at the study facility (*2*). NIOSH presented data at the OSHA hearing demonstrating that a higher percentage of workers exposed to beryllium had never smoked cigarettes (27.2 percent) when contrasted with the U.S. white male population (24.7 percent). Whereas a lower percentage of workers exposed to beryllium were found to be current cigarette smokers (50.4 percent versus 54.7 percent), a higher percentage of those beryllium workers who smoked cigarettes were found to smoke more than one pack daily (21.4 percent versus 15.3 percent). This distribution of smoking habits among workers at the beryllium production facility under study was of a magnitude to increase the lung cancer risk by only 14 percent in the absence of beryllium exposure. However, in the group with the greatest latency period (25 or more years since initial employment), the lung cancer risk was increased by 85 percent. Thus, NIOSH presented data indicating that cigarette smoking per se could not have accounted for the increased risk of lung cancer among the study cohort.

The *Science* article states that the beryllium companies would like to see "another study made of a separate cohort of beryllium workers." In view of the lack of positive action by industry and its consultants in the past, we, as public health scientists, can only interpret the "new" recommendation by industry as a tactic to delay government regulatory decision-making. In view of the demonstrated findings of increased lung cancer mortality in four independently ascertained data sets of populations exposed to beryllium, together with results of carcinogenesis bioassay studies, we also view the request for additional data collection in the context of a continued and needless exposure of workers to a proved human carcinogen.

Finally, as government and independent researchers, we accept responsibility for the objective conduct and evaluation of epidemiological studies. At the same time, as scientists with a public trust for protecting the health of workers, we expect a similar degree of objectivity on the part of all investigators, including those from industry and its consultants. In like manner, we would hope that representatives of the press, particularly those involved in "scientific" is-

around, maybe Jean Marx will, after all, see the day when a few dinosaurs are "rounded up and studied directly."

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Yale's Discontinued Department

The article " 'New wave in academia' wipes out department at Yale" (News and Comment, 17 Mar., p. 1189) contains several inaccuracies which should be put straight. The initiative for discontinuing the Department of the History of Science and Medicine at Yale did *not* originate in the School of Medicine. The fourth professor was George (not Charles) Rosen. At no time did the medical school "want to deploy the vacant professorship in a subject such as the ethics of medicine." In fact, the School of Medicine is currently engaged in a search for an individual to fill a senior faculty position at the rank of professor or associate professor as the head of a Section of the History of Medicine.

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Sex Differentials in Salaries: Faults in Analysis of Covariance

Much criticism has been leveled against the use of covariance procedures to adjust for known differences among populations in order to test for hypothesized differences among them. The criticisms offered by Woodward and Goldstein (9 Sept. 1977, p. 1096) of the research on "communication deviance in the families of schizophrenics" apply, with minor variations, to the work of Bayer and Astin (23 May 1975, p. 796) relative to salary differences between men and women on university faculties.

The major conclusion of Bayer and Astin is that men are paid more than women of similar academic rank, departmental affiliation (*1*), number of publications, and so forth. Variables such as number of publications are, however, fallible indicators of constructs, and being fallible they control incompletely for the target construct, research productivity. As a result, one cannot infer a salary differential because of sex from the analysis offered by Bayer and Astin.

Instead of reiterating criticisms of analysis of covariance offered by Woodward and Goldstein and many others, I refer the reader to Fig. 1, which is derived from Bayer and Astin's data (*2*). In that figure it is evident that there are sex differences in both number of publications and salary. However, inspection of this figure, and other figures (*3*) based on the means of other homogeneous groupings in the same population, indicates that there is no systematic salary differential attributable to sex per se. Although in some of these groups men seem to have been paid more than women, the opposite seems true equally often.

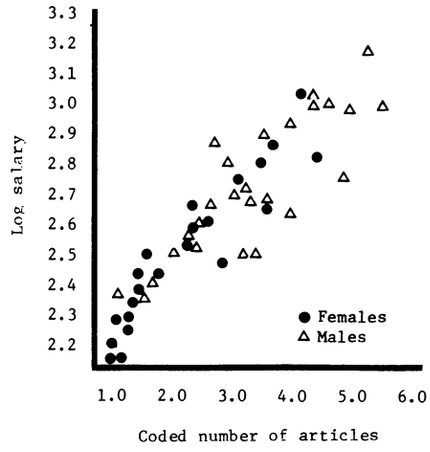

Fig. 1. Mean log salary plotted against mean number of articles for groups homogeneous with respect to sex, rank, and departmental affiliation (*3*). For each group $N \geq 25$.

In many respects this way of looking at the data is also vulnerable to criticism. No claim is made from this analysis that a sex differential in salary is not present. It seems reasonable to suppose, however, that one should be able to detect a sizable differential from inspection of such plots. On the other hand, the covariance analysis used by Bayer and Astin is known to be biased in the direction the results indicate: that is, the group higher on the fallible covariate will tend to appear disproportionately higher on the variate (when the variate and the covariate are positively correlated) even when there would be no such disproportionate difference if an infallible covariate were used.

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References and Notes

1. Departmental affiliation is a grouping of faculty according to kind of department: business, education, biology, physical sciences, social sciences, fine arts, humanities, health.
2. I thank Bayer and Astin for making these available.
3. Tables upon which Fig. 1 and the other figures are based are available from the author.
4. Supported by contract NIE-C-74-0115 from the National Institute of Education.