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Letters to the Editor

I wish to give two cheers for Ellen Silbergeld's editorial "The Fullness of Time in Epidemiology" in the March/April 1987 issue of the *Archives of Environmental Health*. The real importance of her principal thesis is diminished by her unfortunate misuse of the Brown and Jones study results, and by her demonstration of her tenuous mastery of the principles of epidemiology.

In the former case she has told part of the truth. She gives the impression that the only noteworthy finding of the initial study was an excess mortality from stomach cancer based on a "small number of deaths," leaving us to wonder what was found when the study was updated. She gives the further impression that an excess of liver and biliary cancer was first detected in Brown's update of the study.

The whole truth is that an excess of liver and biliary cancer was found in the first study and was widely mentioned. The update confirmed the excess, and confirmed the fact that it was inversely related to latency and duration of exposure. The "small number of deaths" from stomach cancer was, in fact, a single death, was not even mentioned by the authors, and disappeared in the update.

There is no doubt that updating the Brown and Jones study was a desirable and productive effort which confirmed the ambiguity of the original findings. However, Silbergeld's selective citations of the results is an attempt

to wring from them an emerging indictment which is not there.

Silbergeld's epidemiologic pronouncements seem to assume that short follow-up implies short latency. This is not true. She also states that some studies are often terminated "on the basis that sufficient time has elapsed." Since her initial pronouncement is wrong, terminating a study early would eliminate long latent periods only if the study were designed so as to make this happen. More important, the cheapest and most expedient termination date for a study is the most recent date for which complete information can be obtained. I challenge Silbergeld to name one example of a study which has done otherwise.

The bottom line of all of the above is that an important and relevant point was used, to its detriment, as a springboard to launch (a) misstatements about the results of a particular update and (b) pronouncements about epidemiology and epidemiology studies that are simply not true.

Naive readers will swallow the baby with the bath water, to mix a metaphor. Careful and suspicious readers will find the misstatements and may as a result reject the main thesis also. I do not want either of these things to happen.

William R. Gaffey, Ph.D.
Epidemiology Director

To the Editor:

This letter is response to the letter received from Dr. William Gaffey concerning my editorial, "The Fullness of Time in Epidemiology" which appeared in the March/April 1987 issue of *Archives*. The purpose of the editorial was not to continue the prolonged scientific and political debate on the carcinogenicity of PCBs, but rather to discuss the problems in environmental/occupational epidemiology which are associated with incomplete data. As the editorial states, the best solution to these problems is exemplified by the methods used in the paper by Malcer and Gemme. In the absence of mechanisms which can exploit computerized linkages between disease and exposure registries, I commended the effort by Brown to reinvestigate the occupational cohort exposed to PCBs. The editorial did not imply that the follow-up study found radically different results from the first Brown study, but rather that the effect of time was to increase the number of observations available for analysis. The consequence of this was to provide further resolution of the associations observed between occupational exposure and cancer. As stated by Brown and Jones, in their original paper (*Arch. Environ Health* 36: 120, 1981), the observation of excess mortality for rectal and liver cancer was not statistically significant (contrary to the assertion in Gaffey's letter); moreover, to quote Brown and Jones: "Because a relatively small number of deaths were observed, conclusions drawn from the results of this study are tentative." This is exactly the point of the editorial:

that time by itself can reduce the tentativeness of a conclusion.

Gaffey has misunderstood my comments on follow-up and latency. In fact, my concerns are that short follow-up times are inappropriate for diseases with long latency just because the full expression of the pathophysiologic process may require more time than has elapsed between exposure and compilation of results. Obviously, a study can only be done at the time at which it is done, and not at some time in the future; however, the point of the editorial is that when follow-up times are short, and when the number of observations is low, the investigator should as a matter of professional obligation undertake to revisit the cohort in the future. No experimental study would be considered acceptable if only 10% of the total possible observations were in fact available for analysis; epidemiology does not enjoy the advantages of the laboratory, but it can use the resolving power of time, particularly in the absence of the power of linkage analysis. It is a shame that Gaffey did not see his way to endorsing the importance of the kind of epidemiological investigations proposed by NIOSH which have been suppressed by OMB. This is more critical than raising somewhat tangential critiques of my editorial.

Ellen K. Silbergeld, Ph.D.
Environmental Defense Fund