LETTERS

Occupational Health Research: Weill Replies

Sheldon W. Samuels' letter (17 Nov., p. 694) in response to Nicholas Wade's article of 8 September (News and Comment, p. 892) would not necessitate a reply were it not for the several departures from fact that should be brought to the attention of *Science* readers. Contrary to Samuels' characterization, I found Wade's article to be an accurate and lucid account of the circumstances underlying the regrettable "NCI [National Cancer Institute] disinvitation."

Samuels asserts that after the NCI action I remained on the planning committee for the conference on lung cancer surveillance. In fact, I never served on this planning committee, either before or after the disinvitation; never received a program of this meeting; and was never asked to attend in any capacity. Furthermore, the "regret" over this incident expressed by NCI officials to others was never communicated directly to me.

Second, in spite of Samuels' disclaimer, he appears to remain confused or uninformed about my role in the Occupational Safety and Health Administration (OSHA) cotton dust standard public hearings. A simple check with the Department of Labor (may I suggest Grover Wrenn, director of Health Standards for OSHA) would have revealed that I was specifically asked to give testimony and consultative assistance on (i) our study of cotton textile workers, (ii) our study of workers engaged in cottonseed oil processing, and (iii) standards and minimum criteria for the performance of lung function testing in workers. Samuels is in error in claiming that I was asked only to provide testimony on the last item, and he continues to ignore the fact that, in my testimony, I supported the OSHA standard for cotton dust exposures in textile mills (contrary to the position of the textile industry); this can be easily checked in the public record.

In addition to misstatements, Samuels' letter contains quotes out of context. In this case the quotes are two portions of my statement submitted to OSHA on the proposed asbestos standard of 1975. When the entire statement is read, these quotes are seen to be parts of a discussion of the two major alternative approaches to setting exposure standards based on dose-response data. Is a discussion of the options available within a strategy of disease prevention unacceptable to Samuels? There is increasing concern that the benefits which can be obtained by the setting of occupational health standards be supportable by scientific evidence (of the type which has led to recognition of carcinogens in the workplace). It is clear that, with limited expenditures possible for the reduction of environmental contaminants in the workplace, maximizing health benefits for workers requires that these dollars be spent where they will have demonstrable beneficial results. There is no paucity of responsible scientific and public thought in support of this thesis.

I must confess that I lose the substance of Samuels' discussion when he talks of our "experiments" on working populations. If he means by this the use of standard x-ray techniques and pulmonary function testing methods in the study of workers exposed to hazardous inhalants in many industrial settings throughout the country for the purpose of generating data on dose-response relationships, then we plead guilty! The application of epidemiologic methods for studying exposed populations in order to detect the determinants and distribution of disease forms the basis of effective occupational health standards. Samuels should be reminded that *we* do not *expose* the workers to the particular inhalant being investigated but rather direct our efforts toward the early detection of an adverse respiratory health response. If health surveillance by means of radiography and pulmonary function testing constitutes unacceptable "human experiments," then I suggest we have no common ground for dialogue. It should be pointed out that these methods are mandated by OSHA for workers exposed to some respiratory hazards (for example, asbestos). Does Samuels condemn this practice also?

More than three-quarters of the research support of our unit comes from federal funds (the National Institutes of Health and the National Institute for Occupational Safety and Health). The National Heart, Lung, and Blood Institute and the Quebec Asbestos Mining Association (QAMA) did not *match* funds; they independently have funded various phases of our research on asbestos health effects. Samuels implies that support for specific studies from industry, such as the American Textile Manufacturers Institute for the textile study and QAMA, which in part has supported our asbestos work, in some way produces a fatal blow to our objectivity and credibility. I vigorously reject this concept and firmly contend that the segment of our society which is in the best position to modify an injurious occupational envi-

ronment is management, which has not only the *right* but the *responsibility* to seek and support competent investigation and consultation from the academic community. Indeed, they should be considered remiss (frequently the case in past years) if they do not make it possible to generate objective, unbiased data from studies of the occupational environment and health status of their workers. The polarization engendered by the thoughts expressed by Samuels will serve to accomplish exactly the opposite. Credibility of scientific data and conclusions resulting from industry-supported research or, for that matter, research supported by any other segment of society should be judged by the integrity of the investigators and by peer review of the study design, collection of data, analyses, and interpretation of results. We have always welcomed such critical review on the merits of our research, for which personal attacks based on "perceptions" are no substitute.

Although Samuels makes sweeping comments concerning how my colleagues and I are perceived by various public interest groups, the eloquent statement by J. M. Calhoon (Letters, 17 Nov., p. 694) of the Marine Engineers' Beneficial Association which follows Samuels' letter makes it clear that Samuels does not even speak for the labor movement. I suggest that he redirect his energies toward substantive activities which will promote the health of the American worker and abandon his philosophy that all scientists working in this area must be identified with one camp or the other. I also suggest that he visit our unit at Tulane in order that he might better know his "adversary"; I predict that if he came with an open mind, he would be convinced of the dedication and credibility of the scientists working in our multidisciplinary group. In any case, we will not be intimidated or discouraged from continuing our investigations, which we hope will, as before, be with the full cooperation and support of workers, management, and government research and regulatory agencies.

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Lovins' Data Source

Amory B. Lovins (Letters, 22 Sept., p. 1077) gives the impression that the "Bechtel data base" is the foundation for his arithmetic on capital costs of conventional energy technologies and, implicitly, his conclusions on the comparative economic advantages of soft energy paths. The purpose of this letter is to record my reservations about the methods employed by Lovins in his use of this data base.

Lovins refers to the Bechtel data base in 6 of the 19 citations in his letter and, in a previous reference (*1*), cited it as "probably the most detailed, authoritative, and up-to-date [data] available." Statements such as "In fact, they are Bechtel's data . . ." (referring to his assumptions on the capital costs of nuclear-electric systems) and "derived from the Bechtel data base" (regarding his assumptions for cost of 1980's U.S. frontier oil and gas) are used to authenticate his numerical analysis.

In all these citations, Lovins is referring to the data base of the Bechtel Energy Supply Planning Model (ESPM), which was developed initially by Bechtel under contract to the National Science Foundation (*2*). As codeveloper of the model and principal investigator responsible for its development since April 1975, I am familiar with the characteristics of the model and data base and with Lovins' use of the data base in his stream-of-numbers logic. In my judgment his data and conclusions bear little relation to the ESPM data base with which he purports to have started.

My principal criticisms, which I will elaborate in more detail below, are four:

- *Use*. He stretches the use of the data base for purposes which go beyond its design objectives.
- *Consistency*. He makes selective use of the data base and thereby loses consistency across technology comparisons.
- *Extrapolation*. He adds various factors not part of the data base to the basic model data.
- *Currency*. He continues to base his calculations on information published in 1975 despite the availability of extensive literature documenting updates of the ESPM since that time.

1) *Use*. The ESPM was designed to provide a tool for calculating the magnitude and timing of resources (capital, labor, materials, equipment, land, and water) required to implement alternative energy development programs. This required the development of resource data on the requirements of individual energy supply and transportation facilities. We recognized that this facility resource data base might, in addition to serving the specific needs of the ESPM for which it was designed, be useful as a starting point for other research efforts.

However, as stated in the model's