

ES&T LETTERS

Prevention of significant deterioration

Dear Sir: While I found Myron Uman and John Middleton's article "Implementing the PSD Program" (*ES&T*, Vol. 15, No. 9, 1981, pp. 1000-1005) generally accurate and useful, there is one statement I believe is false. The authors state "using any of these statistical measures of compliance need not lead to a change in the level of protection afforded by the existing system since knowledge of the statistical behavior of the atmosphere will suggest what new number (that may be exceeded more frequently) corresponds to the same level of air quality as the current number (which may be exceeded only once)." While this statement is true for any single facility and associated receptors, there is no single number in the form of a revised increment that can provide air quality protection for a variety of source-receptor configurations.

I am a participant in the Los Alamos National Laboratory's Four Corners Regional Study, performed for the National Commission on Air Quality, and we calculated annual average concentrations, highest fifth percentile, highest, and second highest concentrations. The ratio of the second highest to either the fifth percentile or the annual average was quite variable. For Class II areas the ratio of the highest, second highest 24-hour average to the highest fifth percentile varied between 1.15 and 4.2. The ratio of the highest, second highest 24-hour average for Class I areas to the highest fifth percentile ranged from 2.3 to 14.5. The ratio of the highest, second highest 24-hour average to the annual average ranged from 12 to 85. Furthermore, as one would expect, as more sources were added to a region the ratio of the highest, second highest to the highest fifth percentile or annual average decreased. For example, for one Class I area the ratio went from 7 to 2.3 as more sources were added.

It is also not evident to me that with the current collection of models the highest fifth percentile or annual average can be predicted with greater fractional accuracy than the highest, second highest. For isolated point sources it is rarely possible to test annual average predictions because concentrations are below instrument

thresholds most of the time. What little data I have seen does not suggest that the highest fifth percentile can be predicted with an error as low as the 10-40% found for a highest, second highest. Model results tend to show relatively steep slopes near the fifth and 10th highest percentile levels but somewhat flatter slopes near the highest, second highest. It seems to me that a great deal more study would be required to justify deleting the short-term increments in favor of those defined on another basis. It is also evident that a change to a more frequent criterion will not result in equivalent protection for most source receptor configurations.

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Inhalable particulate matter

Dear Sir: Thank you for sending me a copy of the *ES&T* article on particulate matter by Bette Hileman (*ES&T*, Vol. 15, No. 9, 1981, pp. 983). I had previously read it and found it impressive. It has good balance between science and news and is very well written in terms of readability and presentation of data. While there are some areas in which my interpretation would differ from that of the author, it is in general a superb example of how one should present issues of this type to your readers.

I look forward to more articles of this type in *ES&T*.

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The lead debate: a response

Dear Sir: Dr. Hammond's concern (*ES&T*, Vol. 16, No. 3, 1982, p. 145A) that I believe his research is tainted because he accepts industry support would be eased if he rereads my letter (*ES&T*, Vol. 15, No. 11, 1981, p. 1250) with greater care. Nothing can be found there that would allow him to draw such a conclusion.

In my letter I pointed out quite clearly that Dr. Jerome Cole of the

International Lead-Zinc Research Organization (ILZRO) quoted an ILZRO-supported literature review in support of an ILZRO-supported investigation that found no lead-related health effects, but that Cole omitted mention that both authors were the beneficiaries of lead industry sponsorship. I also suggested that Dr. Cole's comments would be taken more seriously if he were able to supply other studies or commentaries than those underwritten by his organization in support of his position.

Nowhere did I mention Hammond's name or research, and his spirited—if misplaced—defense puzzles me. I do believe that the subject of scientific quality and objectivity in relation to the source of sponsorship is one which needs thorough and objective study and discussion, rather than anecdotal assertions. Such a systematic inquiry should be welcomed by all scientists, including Dr. Hammond and me.

Dr. Hammond presumes to know what I think. If so, I am at a disadvantage. There is nothing in my pediatric or psychiatric training that has prepared me to read minds. If his education as a toxicologist has supplied that gift, I would be grateful for the name of the course.

Herbert L. Needleman, M.D.
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The lead debate continues

Dear Sir: I do not wish to enter the specific debate raised by Mr. Budiansky's report regarding the scientific validity of the several studies on the effect of low-level lead exposure on children (*ES&T*, Vol. 15, No. 3, 1981, p. 243). I wish, however, to react strongly against a suggestion made by Professor Needleman that studies supported by ILZRO are invalid (*ES&T*, Vol. 15, No. 11, 1981, p. 1250). This is an insult to many scientists who have worked with industry funds. As the head of a laboratory who has received financial support from ILZRO, I can categorically state that ILZRO has never attempted to influence directly or indirectly the design of a study and the interpretation of the collected data even when the conclu-

sions might be detrimental to the nonferrous metal industry. As proof of my statement, I advise Professor Needleman to examine critically several papers published by my laboratory.

I think that a scientist does disservice to the scientific community by having recourse to nonscientific allegations to refute the conclusions of studies performed by others.

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Dear Sir: Dr. H. L. Needleman's letter (*ES&T*, Vol. 15, No. 11, 1981, p. 1250) about effects of lead seems to imply that industry-sponsored studies and reviews are not dependable. This was disturbing since I am currently working on a review of metals in marine organisms, to be published by the International Lead-Zinc Research Organization, Inc. It would be dismaying if I thought that this review would be automatically considered biased. I expect to make interpretations and draw conclusions freely. Indeed, the only requests from the industrial organization were to write a thorough critical review of world literature, and to base it on a reading of original articles, not second-hand quotations of them.

Similarly, in a two-year research project sponsored by oil companies, I found little evidence of "industry bias." The sponsors were committed at the beginning to publish the results on sublethal toxicity of refinery effluents, which could have been damaging to their interests. We had a lot of fun doing the research, and found the sponsors open and easy to work with. On the other hand, I have experienced government suppression of results that might not be "reassuring to the public."

Having worked on problems of water pollution for 28 years in government and university, I doubt that I am completely naïve in this matter. Of course, there are strongly differing points of view and some sorry tales. However, the bulk of my experience is that industry generally wants to get on with things in an effective and straightforward manner. Dr. Needleman's inference may not be entirely fair.

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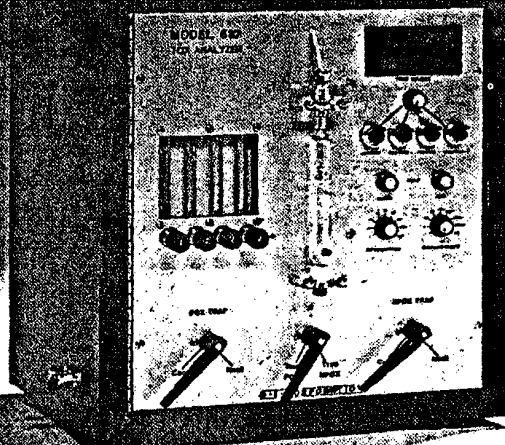
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