

DEPARTMENT OF PUBLIC HEALTH

2151 BERKELEY WAY
BERKELEY 94704

April 27, 1967

Robert A. Kehoe, M.D.
Kettering Laboratory
University of Cincinnati
Eden and Bethesda Avenues
Cincinnati, Ohio 45219

Dear Bob:

Thank you very much for your letter of March 22 and for the enclosures. I don't know how such busy Californians could think of getting so involved in the problems of Pennsylvania. However, since we are involved I would be pleased to give you my views on the matters which you have brought up.

I was, of course, aware of the likelihood that you would disagree with my views of what should be a prudent public health policy. It is, indeed, the question of the logical treatment of the variation of environmental and biological phenomena that is the main cause of our disagreement. Surely if one could only draw conclusions when all variables are controlled then we would never draw any conclusions, for we can never be sure that all variables have been controlled. It is always necessary to make some assumptions about the variables which are not controlled--for instance, that their effect is too small, or will balance out. The assumptions I made, that certain uncontrolled variables are randomly fluctuating, are quite reasonable and in fact are the same assumptions which are made whenever standard deviations are calculated. That the differences we are examining may be smaller than the differences produced by uncontrolled variables makes these effects harder to detect but does not detract from their reality; the mathematical techniques available to today's researchers are quite capable of discovering these differences and of estimating their magnitude with reasonable precision.

In your letter to Mr. Sussman (at the top of page 3) you call attention to the likelihood that variations of the order of 0.02 to 0.04 milligram per liter of urine or per 100 grams of whole blood commonly occur in the studies of individuals. If I understand you correctly you are saying that the effect of atmospheric exposure can be studied only if it is larger than these individual variations. If this is your intent then this ignores well-developed statistical and epidemiological methodology which has long been in use for over a hundred years. To take an extreme example, the death rate for an individual can take on only one of two values, either 0 or 100,000 per 100,000; that is, the individual either lives or dies. The individual variations are very large, yet this does not invalidate comparisons of death rates among populations where the differences in death rates may be rather small.

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I think your understanding of epidemiology is somewhat less extensive than your understanding of many of the other relevant disciplines; your implication that clinical significance is important has neither been alleged nor as far as I am concerned, demonstrated. We are not looking for additional cases of disease; we are looking for increases in the storage of a potentially hazardous substance. I fully agree that the absorption of lead from the alimentary tract is inconstant from day to day and from person to person but this is no reason to deduce that all differences observed between populations are due to differences in alimentary absorption, unless and until the differences in alimentary ingestion can be shown to be responsible for the differences in blood lead among these populations, and I don't believe you have shown this.

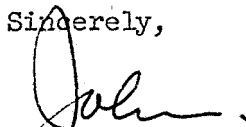
I have serious doubts on one point about which you seem certain, mainly that the lead ingested with food and beverages is responsible for the larger proportion of lead found in the blood of persons in groups of a general population. I believe that there are populations for which this is not the case. Indeed, your own very careful studies indicate that the subjects in your exposure chambers in Cincinnati retained approximately 20 micrograms of lead per day from respiratory exposure, whereas the amount presented to the body's metabolic pool by the orally ingested lead was of the order of 30 micrograms per day. Surely there are some populations with more than a fifty percent higher exposure to atmospheric lead than the non-occupationally exposed people in Cincinnati.

I was quite surprised in reading your testimony before the Pennsylvania Commission to see that your estimates on the proportion of retention of lead in the body were based on rather generalized theoretical considerations instead of on your own excellent laboratory studies. I find this a source of some concern since I have been frequently guided by your views on the importance of using just such measurements as are necessary.

In closing, may I again tell you how grateful I am for your candid discussion of this complicated problem and for the wealth of data which you have so kindly made available to us. I hope that you will not hesitate to re-examine some of the matters which I have raised. I would be very pleased to discuss them with you at your convenience. I assure you that any differences of interpretation of the facts in this complex matter cannot conceal my admiration for the contributions which you have made to this field.

With warmest personal regards,

Sincerely,



John R. Goldsmith, M.D., Chief
Environmental Hazards Evaluation Unit

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*P.S. The Department's report is
being sent under separate cover
JRG.*