

December 22, 1971

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New York, N. Y. 10017

Dear Jerry:

I have intended, for several days, to write to you and to tell you of a decision I have arrived at very recently, which relates somewhat to our recent conversation. Your letter about the Kettering Abstracts arrived in this morning's mail and has prompted me not to delay longer in reporting my decision. I want you to know my intentions, but also to be fully aware that I can change my mind, if necessary.

I refer in the last sentence above to my willingness to do anything I can to aid in dealing with the problem of lead in gasoline. It is my understanding that the experimental work in Albany is now to be extended to deal with the level of 5 micrograms of lead per cubic meter of air. For a variety of reasons, among which their latest failure to establish the level of the concentration of lead in the air, to which their men were subjected, looms fairly prominently. I tend to have my fingers crossed as to the significance of their work, and the extent of its acceptance as a valid expression of a standard or near standard. I do not mean by this that I am indifferent about their work. Rather, I am prepared to examine it with some skepticism.

In this situation, the completion of the work which I began, by dealing with 5 micrograms per cubic meter of air, on the basis of the previous design of the experiment, would, I think (I hope not with undue optimism), be invaluable. I would not like to appear to be unwilling to undertake the experiment. I would set it up as quickly as I could obtain a suitable subject (or perhaps, 2 subjects investigated in parallel). On the other hand, I have no wish to do this unless it is considered to be desirable, for the experiment being performed by the group at Albany is a better experiment if it is carried out properly. Because of this fact, I have stepped aside willingly to investigate other aspects of occupational exposure. But I have not lost interest in doing whatever I can to obtain the answer to this important question.

Now for my decision, with respect to my immediate future. I have been disturbed by the fact that I have been unable, thus far, to devote myself and my time, intently and to the exclusion of most other professional or scientific activities, to the study and preparation for publication of the work done over the period of the past twenty or thirty years. I propose to drop additional experimental work entirely in 1972 and to work on publication. Bernie Sobel at Baton Rouge is willing to do a lot of statistical work for and with me, and this is an opportunity I shall not find easily later. I plan to take a batch of material with me to San Francisco, and to get to work on it, and to continue in the same work on our return to Cincinnati. This work, with some additionally scheduled interruptions, as the professional meetings in the United States, the International Congress on Occupational Health in Buenos Aires in September, will

give me plenty to do, and will enable me to go on later, with additional work in publication, which can be so planned as to give me some free time, without stopping the progress of the publication. (I have two monographs in mind, one of which, at least, should be completed within a finite period of time.)

After 1972, I should like to begin anew, the study of certain lead compounds with the idea of obtaining (for the first time, really) some quantitative relationships between the several compounds, as to their absorability from the respiratory tract. I would like also to study the effects of several specific compounds, as, for example, the *sulfide*, sulfide, the silicate, and perhaps the chromate, on the rate of absorption from the alimentary tract. You have commented previously, to the effect that this is a costly type of experimentation. I admit this freely, but on the other hand, it is nothing short of remarkable that this has not been done in a definitive manner, in relation to industrial standards as to threshold values, long ago. Incidentally, the alimentary experiments can now be done in comparatively brief periods of time. We have most of the information that is needed in relation to prolonged exposure, and the most urgent need of the moment, is that which concerns the relative rates of absorption.

I shall not be troubling you further in 1972 for funds with which to continue the experimental work. If I am lucky enough to make real progress in assembling my experimental data and finding out what they mean, I shall return to you in 1973 - that is provided also I survive and remain fit enough to work - with a proposal. I shall provide you with a progress report on the last experiment, as well as a more comprehensive report of our work on the tolerable level of lead in the air. This is the first thing I have in mind to do, and while I caution you not to expect a miracle, I shall get this done in due course.

With kindest regards, and especially good wishes for the Holiday Season and for the New Year which will come shortly.

Sincerely,

Robert A. Kehoe, M.D.
Professor Emeritus of
Occupational Medicine

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enc: Check for subscription to Kettering Abstracts on Lead for 1972.

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