May 1, 1969

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Dear George:

I am grateful to you for your comments on the paper prepared for publication in the Journal of APCA, and I shall take them carefully into account in making such final revisions as may seem appropriate. I've set it aside for reexamination later.

I won't discuss them with you in any detail at this time, but I think I should indicate my immediate reactions to certain of your suggestions.

I do not react favorably to your suggestion that I include references to John Goldsmith's writings in this review. In the first place, his paper for delivery at that time will say what he has said previously, and I would not like to use my position in the program to attack what he will have said. I may comment on certain aspects of his paper but not before the fact. I disagree with him very sharply indeed, and much more vigorously than I shall indicate. But I do not wish to offend him for personal reasons, and I think I would inevitably offend him if I were to say what I think of his ideas on this subject. The long article to which you refer in your letter is literally filled with errors of fact and of judgment. I wrote a long and detailed criticism of it for my own satisfaction, in the form of a letter, and then decided that I would not send it. In addition, my criticism of his brief article in Science was toned far down before I sent it in. I know you thought this article well written. I agree that the words were well arranged, but the idea expressed was far afield. Of course the absorption of lead by the two subjects, as indicated by their seeming excretory response (urinary), fitted his curve. That proves or even suggests nothing as to the source of the lead absorbed. It related only to a generic physiological pattern. Furthermore, it was blatantly presumptuous of him, although his self-assurance was based on ignorance, to believe that he could interpret the results of this experiment more realistically than could I, with years of experience behind me in watching the behavior of experimental subjects. However, one cannot make such comments, even by inuendo, in a document for publication which must err, if at all, on the side of both courtesy and understatement.

I omitted much discussion of pseudo-controverial matters in this paper, in order to get a modest, straightforward statement of my views, and the evidence in support of them, bringing in important, recent contributions to our knowledge. I had to combat the idea of some vague type of subclinical injury induced by lead, because it had become a much too convenient form of mindlessness in relation to this matter. I had also to bring some critique to bear upon the paper of Schroeder and Tipton (essentially Schroeder's analysis of Isabel Tipton's data), because it, in a sense, misrepresented the facts, unintentionally, I'm sure. However, what I undertook to do was, quietly, to take over the subject, and I did not wish to introduce any

is so, or that it will so appear.

One other aspect of your comments puzzles me a bit. This concerns the 20 micrograms per day on the output side of the balance experiments involving Subjects I.F. and S.W. This to me seems a simple matter. Whether the quantity is 20 or 10 micrograms per day is of little consequence. One is dealing here, I think, with a physiological fact in which the output is systematically greater than the intake over the period of months. This can hardly be artefact, but neither can it be accepted as quantitatively precise. It is simple physiological logic to assume that it is due to a factor which could not be included in the experimental design. In fact, there are several features of both intake and output which could not be measured - for example, the output of lead in sweat which occurred no doubt, and the intake represented in the introduction of articles into the mouth which were neither food nor beverages. Try as one may, without constant critical supervision of subjects, certain other factors enter in on one side or the other. To recognize this and admit it is not to vitiate the essentials of the experiment, but rather to render the quantitative information somewhat deficient. This is, of course, why I have never claimed precision for these observations, but rather have emphasized the elucidation of a pattern which can hardly be invalid, considering the certainly small size of the uncontrolled variables. Certainly the quantitative relationships of respiratory behavior will have to be determined by direct measurement of these phenomena by methods which are suited precisely to the problems at issue.

But enough of this. I had not intended to discuss these matters with you at this time, but it is just as well, perhaps. As I have said, I'll reconsider these details when I reexamine the manuscript.

I'm terribly sorry to learn of Elsie's misfortune. I do hope that this subsides. Please give her my best wishes. I realize that such wishes are of little help, but the wish is there nevertheless. To these, add my best regards for yourself.

Sincerely,

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

RAK:wp

P.S. One word on the A.L.A. and my reasons for caution and brevity in discussing it. I learned in Amsterdam that there are several questions here on which there is not full agreement, probably because of variability in the methods of observation and in the groups observed. The one thing I stated appears to be true. Our own observations, which can be trusted, are not sufficient to depend upon.

RAK