

August 11, 1942.

Mr. Philip Drinker,
Harvard University School of Public Health,
55 Shattuck Street,
Boston, Mass.

Dear Mr. Drinker:

At long last, I am forwarding to you a series of papers representing the subject matter of the symposium on lead poisoning at the Cincinnati convention. The delay in getting it together has been largely my own, but I dare say I need not offer explanations.

I realize that you have not committed yourself to the acceptance of these papers for publication, and yet I think when you see them and go over them you will feel that you should take them. However, there are one or two points to which I wish to make reference, and there is one or which I want your suggestions. The latter concerns the paper of John Buxell. I believe you will agree with me that this is not a good paper. On the other hand, it is essentially in the form in which it was given in Cincinnati. The paper was a disappointment to me at that time, and I am no better pleased with it now. Mr. Buxell's letter to me enclosing it said that it was in the form for insertion in the symposium. I doubted that and took the liberty of editing it, with the idea that I would communicate with him before submitting it for publication. On second thought, I decided to submit the matter to you, sending you the original paper and the somewhat edited version. You are at liberty to take any action you like, but I should appreciate your communicating your intentions to me in order that I may maintain properly courteous relationships with Mr. Buxell. I have attached pencil notations to this material.

My second comment applies to the paper by Sanders. After pondering your suggestions for the presentation of the data, Sanders has concluded that he prefers to present the charts as he originally prepared them with some slight reduction in their number. I must confess that I am in agreement with him on this question, since, I believe, a similar and more abbreviated method of presentation involves rather arbitrary and unjustified assumptions. The irregularities in the curves

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might be assumed to be sampling errors, but this is by no means certain, and both Sanders and I are very dubious of that explanation. There is also the difficulty of choosing the level which shall be considered "normal". We believe that any use of this arbitrary point will produce confusion and will indicate a much greater certainty than exists concerning this physiologic phenomenon. Accordingly, we both feel that the presentation of these data has been handled in a reasonably satisfactory manner, and we hope you will agree. If you do not, and if you wish to reduce the number of figures, we shall do our best to be guided by your suggestions.

With respect to the other papers, I have no important comments. My own, or rather the one which is headed by my name, is a running discussion which may not be adequately supported at every point by the data. On the other hand, the paper that I am preparing to fully present the data of these studies is too bulky to be considered in this connection, and since some of the observations are still in progress, I am put to the necessity of covering the more important points without presenting the detailed data. I am at some loss to know how to present the complete work, for it is voluminous, and it can not be reduced to the size of an ordinary paper. It is of considerable practical importance, however, to get this carefully checked information into a place where it can be used, and that was my purpose in presenting it as I did at the Cincinnati meeting.

I shall be out of my office for the next ten days, and I dare say you will not have time to come to definite conclusions on this matter before the end of that time. On the other hand, I should be glad to have your decision and your comments on this material at your early convenience.

With kindest personal regards,

Cordially yours,

Robert A. Kehoe, M. D.

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