MEDICAL DEPARTMENT
OCCUPATIONAL MEDICAL SERVICE

CAMBRIDGE 39, MASSACHUSETTS

December 29, 1965

Robert A. Kehoe, M.D.
The Kettering Laboratory
University of Cincinnati
Cincinnati 19, Ohio

Dear Bob:

Holiday greetings to you, too!

Let me answer your letter of December 23rd as follows:

I was quoting Chisholm as to the behavior of lead poisoning when infection has been added in children. I did this because Dr. Chisholm had left, and he had made this statement so clearly and I had made a note of it. Since you have written him, he will be able to answer for himself.

Have I missed something in the work by you and your group describing the behavior of lead and calcium during infection or alcoholic bouts, as mentioned on page 2? I would very much appreciate references to such data.

Finally, under the terms of the NIH grant that we have here at MIT it will be possible for us to try to make firsthand observations of the behavior of lead and calcium during infection in a child about whose lead burden we have reasonably good knowledge.

Do let me hear from you.

As ever yours,

Harriet L. Hardy, M.D.

HLH:hsn

ICE 01097

December 23, 1965

Harriet L. Hardy, M.D.
Medical Department
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Dear Harriet:

You will not be surprised, I dare say, to know that I was not pleased by some of the things that were said and done at the Symposium on Lead in Washington, D.C. I shall not go into these matters beyond the statement that the atmosphere was, in some respects, reminiscent of a conference that was called by Surgeon General Cummings in May of 1924, and that I seemed to be suspected of making statements that went far beyond the facts elicited by clinical and experimental evidence. Since I am a stickler for evidence, I could not swallow this with equanimity. I reacted more than I like to admit to Harry Heiman's distortion (unintentionally, I believe) of my remarks, and to his transparent personal remarks.

All of this is by way of commenting about your statement, which may have been in accord with Dr. Chisolm's that acute infections are serious complications in childhood plumbism. He may have meant that such infections altered, seriously, the distribution of lead in the body, but he didn't actually say this. You did say this later, however, in commenting on a statement of mine reported by the co-chairman.

I did not consider that this was especially relevant to the problem of the effects of lead in general environment, and I did not make a reply. I intended to talk to Dr. Chisolm about his experience, and to write to you later, as I am now doing. He got away before I had a chance to talk with him, and I have also written to him.

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What I said was that we had been unable to find any evidence of significant changes in the metabolism of lead in response to alcoholic bouts, general anaesthetics, certain infections, or acute disturbances in the calcium and acid-base metabolism. I said further that we had observed such changes only in response to the administration of certain chelating agents, and that I believed, from the observations of others, that the dissolution of the skeleton permitted the mobilization of lead therefrom.

I am not in the habit of making statements of fact without evidence, and I can assure you that what I said is backed up by experience and critical observation, some of which has been published. What I would like to have, from you, Chisolm or anyone else, is evidence to show that there is a significant change in the distribution of lead in the tissues, body fluids, or excreta of persons with either "normal" or grossly abnormal body burden of lead, under the influence of acute infections. I have seen a fairly large group of men of this type through an epidemic of influenza without any sign of change in the rate of their excretion of lead. We have other similar data.

I have do doubt whatever, that children with lead poisoning at any stage - i.e., at the onset, or in apparent convalescence - are affected adversely by acute infection. That is not the point. Has it been found during or after their illness, or at necropsy, that the distribution of lead has been altered? I have not seen any such evidence, and I believe that the view that there has been such an effect is an assumption based on the experimental and clinical observations and interpretations of Aub and his associates. I am entirely willing to be convinced that I am wrong, but it will take evidence, not argument - evidence based on the application of the methods of analytical investigation that are now available.

Indidentally, I had and have no quarrel with Joe Aub. His hypothesis in relation to the mobilization of lead by various factors was an ingenious one. Unfortunately, it was based largely on the study of the fecal lead output of experimental animals and patients, at a time when the methods of analysis were not up to dealing with ordinary specimens of urine or blood. The conclusions were incorrect because the basic physiologic concept on which they were founded was

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incorrect. Lead is excreted in the feces under certain conditions, but these conditions can not be visualized without knowing the content of lead in the food and beverages consumed - that is, without "balance" observations. It is now certain that, while there is an association between the metabolism of lead and that of calcium under certain unusual conditions (one of which I mentioned above), the two metals act independently, as a rule, and may go in opposite metabolic directions at one and the same time. If you doubt this, examine the data of our experiments on human subjects. There are other data indicative of the same facts.

I have written to Dr. Chisolm to make sure of the content and purport of his statement. I have no doubt as to his clinical competence, and I want to know his views and their background. I also want to know how and to what extent he has followed the physiological behavior of his patients. As to their clinical behavior, I would trust his observations. It is to be remembered that our pediatricians, at the Children's Hospital, have gone along with us for many years in the investigation of a number of aspects of this problem, and while I have great respect for Chisolm, I have had a considerable background or experience in this matter.

I hope you will not feel this letter is written in wrath. I feel certain that you believed what you said. I doubt very much that you intended to accuse me of drawing unwarranted conclusions. I even doubt that you have considered critically and carefully, the evidence on which my statement was based. Furthermore I also doubt that you have the evidence that will demonstrate my mistake, if I am mistaken. If you have, I shall apologize in sack cloth and ashes, but I must have evidence, not an opinion based on clinical observation.

And now after all this, let me wish you a Merry Christmas.

Sincerely yours,

Robert A. Kehoe, M.D.

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