

November 8, 1968

Henry A. Schroeder, M.D.
Professor of Physiology
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9 Belmont Avenue
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Dear Doctor Schroeder:

I hasten to reply to your nice letter of November 6, for the reason that I shall be going abroad shortly for two weeks, during which various things will accumulate on my desk and cause me to seem negligent of my correspondence.

I would like very much to meet with you as you suggest, and I shall make every effort to do so next spring or summer, as our mutual convenience will permit. If something comes up around that time to suggest a suitable opportunity, let me know, for I understand something of your difficulties.

I see no special virtue in dealing with our differences of opinion, such as they may be, through correspondence, since it will be both more agreeable and more effective to talk of these directly. There are, however, one or two points in the descriptive physiology of lead, on which I would like to comment, just by way of avoiding controversy. I have seen Dr. Tipton's data - not all, perhaps, but a considerable proportion - and my objection to regarding them as an expression of the "normal" pattern of the distribution of lead in the tissues of persons whose absorption of lead has no unusual source or sources, is that the deceased persons have not, I believe, been selected carefully to avoid the effects of unusual types of exposure. The opportunities for such exposure are numerous in terms of occupations, manual skills of one type or another, unusual family settings, and a host of other conditions too numerous to list. It can be argued, of course, that these features are part of the environment of our time, and so they are, but most of them are avoidable and preventable, in contrast with basic environmental conditions in modern community life in the United States.

An example will serve to indicate my meaning. Lionel Tompsett published data years ago and demonstrated that the subjects of his analyses fell into an age and analytical distribution corresponding to that of your interpretation of Dr. Tipton's data. However, many of the values were high enough to indicate an unusual rate of absorption of lead, and when I wrote to him about this, he agreed fully that his was an unusual population in which cases of lead poisoning had been occurring from time to time over the years. The factor consisted in lead-lined tanks from collecting rain water in the attics of houses in an area of Glasgow (in which his hospital was situated). Our balance experiments show that when the usual levels of continuous lead intake and absorption are increased sufficiently, accumulation occurs for a long time (probably indefinitely) at a constant rate. They also show, I believe, (the evidence being indirect but fairly impressive), that when the exposure is limited to that involved in the consumption of food and beverages and in the inhalation of the surrounding atmosphere of communities generally, the rate of accumulation is too small to be detected within the limits of "normal" variation.

As to the significance of the concentration of lead in the blood in relation to a persistent environmental situation, one can claim, I believe, that the evidence is strongly indicative of a stable pattern in the distribution of lead in the soft tissues

of the body, of which the blood (as a cellular tissue) is one. Admittedly, what has gone into the less accessible portion of the skeleton (metabolically speaking) at one time or another, or through a minute (?) addition thereto during the years of a lifetime, enters slowly into the in and out of the skeletal stream (as a participant in the metabolic process). This is why the analytical examination of cadavers, toward a conclusion on the issue raised here, must be based on a detailed lifetime history which tends to exclude periods of unusual exposure to lead.

I do not choose here to comment on the experiments with animals except to point out that the experimental addition of lead to their dietary and other environmental exposure to lead fulfill exactly my objection to the induced situation. The question is, I believe, how do they get along under the conditions which are "normal" to them, in the present environment of men and animals?

I share fully your feeling about the issuance of pronouncements, even if they were entirely realistic, by the personnel of organizations within which there is a conflict of interest. I suppose that in our society, with its compulsions toward communication, such activities are unavoidable, but they do not impress me as being prudent or desirable.

You mention the Danielson report, with some degree of favor, and to this I must react. I have met and talked with this man and have examined the situation which he is discussing with considerable care. His knowledge of the matters at issue is exceedingly sketchy, and his approach to the whole subject lacks objectivity. In my view it is just as bad, in fact, perhaps not in effect, to be on the side of virtue for the wrong reasons - that is, for emotional and prejudicial reasons, without evidence - as it is to be biased in the opposite direction. In the long run, we shall not find it possible to deal with the complicated doings of the society of our time, to its ultimate advantage, until we learn to be fully objective in our intellectual operations, while yet remaining cautious and compassionate persons.

Sincerely yours,

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

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November 6, 1968

Dr. Robert A. Kehoe
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Dear Dr. Kehoe:

I do not know just what Isabel Tipton had in mind, but I would most welcome an opportunity to meet you and to discuss our mutual interests and such differences in conclusions and opinions as may exist. Because I suffer from muscular dystrophy, I no longer travel much, and because my physician told me a few days ago to cut down on my activities, I am going south in December and will remain until April. Therefore, such a meeting would of necessity have to be here and next spring or summer. I hope that it will be possible.

Let me say that in no way do I regard myself as an expert on lead. I regard you as the Expert, just as I think of Cotzias on manganese, Hardy on beryllium, Scheinberg and Schubert on copper, Mertz on chromium and Underwood on molybdenum. I have the highest regard for your careful, meticulous and well-planned studies and have frequently referred to them. I keep a copy-- very ragged and brown-- of your 1940 works with Cholak and Story in my top drawer. I fully accept your tolerable limits for dietary lead. I have the highest respect for you as a scientist, and what you have done.

However, I have the privilege of having access to data which you do not have on your own subject. On these data I must make some conclusions, and if they differ from yours which were made without these data, they do so in all honesty and wholly impersonally. When better data become available to others, their conclusions may differ from mine.

These data are: 1. Isabel Tipton's analyses of human tissues, part of which I collected for her. They show, when grouped in decades, an unmistakable increase in the mean lead content of a number of tissues as age increases, up to the fifth decade, or in some samples longer. Therefore, I draw the conclusion that the population analyzed was not in lead balance, thus differing with your conclusion, based on less complete data, that another population, probably similar, was.

2. When we feed lead at 5 ppm to rats and mice for their

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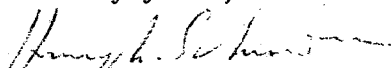
lifetimes, they do not do well. They grow well when young. When they are old, they lose their hair and much weight. They do not resist infections well. When we analyze their tissues, we find the lead concentrations within Tipton's human ranges. Therefore, I draw the conclusion, admittedly tenuously, that lead at present human tissue concentrations is not too healthy for other mammals, i.e. is toxic in some way. You do not have these data.

I also differ with you and others on one matter of opinion. This is my opinion and that is all it is. As a physiologist I do not believe that serum or plasma levels reflect body burdens of any intracellular cation and I just as firmly believe that they do reflect body burdens of extracellular cations. I may be wrong--and I have been wrong before--but this opinion is what reason tells me. From our own studies, I draw no conclusions on what hair reflects.

There is one other point. When I read a publication such as "Facts on Lead in the Air" put out by the Lead Industries Committee, I react emotionally and strongly. When I read reports such as the Danielson report, from Sweden, on present lead burdens, I am inclined to agree with them. This again is opinionated of me--but natural.

Hoping that we may meet and discuss our common interests, and wishing you continued success in your retirement, I remain

Sincerely yours,


Henry A. Schroeder, M.D.
Professor of Physiology

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cc: Dr. I.H.Tipton