May 9, 1949

Dr. A. M. W. Hursh,
Chief Medical Examiner,
The Pennsylvania Railroad Voluntary Relief Department,
15 North 32nd Street,
Philadelphia 4, Pennsylvania.

Dear Doctor Hursh:

In finally replying to your letters, and in commenting on the bulletin which you sent me, I feel very apologetic for my tardiness. However, the fact of the matter is that this is only one of many things that I have been wholly unable to touch for some weeks, because of engagements, teaching responsibilities and an accumulation of duties that have been wholly beyond my capacity. I finally had an opportunity to read the article. I can only comment briefly, since if I were to go into the detailed criticism of this publication, I would have to present data and discussion which would be much more extensive than the article itself. Obviously a proper criticism would have to be documented, and this I cannot do in a letter. Therefore, I can only say that, in my opinion, practically the entire article is incorrect. There is no part of it which is in agreement with our experimental data and the conclusions drawn from them over the period of years. I believe that Doctor Smith's general ideas on this subject are the result of their failure to use adequate methods of investigation. Moreover, I believe that the most of the variations in their data, with respect to the alleged lead content of serum, are within the limits of the error of their analytical methods.

Just to demonstrate what I mean, we are on the point of publishing an article on the lead content of the blood plasma of human beings, under a wide variety of conditions, ranging from normal individuals with no occupational lead exposure to sick individuals following extensive lead exposure. Included among the latter, are a group of patients treated with BAL with very dramatic effects upon the lead concentration in the blood and urine. After extensive and very careful studies of this type, with blood samples taken and promptly separated into cells and plasma so as to avoid any possibility of hemolysis, or of any other chemical change that might result from allowing a sample to stand, we find no variation in the lead content of the plasma of the human blood under any normal, clinical or experimental conditions. That is to say, the variation was so slight, and is so well within the limits of analytical accuracy, that we have no evidence whatever that the plasma is ever elevated in its lead content. Smith and his associates, have dealt with clotted blood, and have analyzed the serum and the clot. I do not know how long they allow the samples to stand, but I do not regard this as satisfactory technique, since during this period the blood can and does undergo certain chemical changes which might well alter the distri-

bution of lead between the cells and the plasma. I regard this whole matter as artifact, and I may say in that regard that I have reason to feel that we employ the most precise methods of analysis, and that we take the most care to avoid contamination, that can be achieved at this time. We know the size of our analytical error, and therefore we make no suppositions about physiologic phenomena based on analytical variations per se.

With reference to the problem of deleading, and I can only repeat what I said on adequate evidence, that we have been unable to find the slightest indication that persons can be deleaded by the administration of acids and the other therapeutic agents that have been suggested. Everything that happens here, except in the case of the administration of BAL, is within the limits of physiologic variability. We should be in better position to answer this question than anyone else, since we have carried out balance experiments at very great expense of time and money under a variety of conditions. It would be utterly impossible to prove one's way in this matter by resorting to the analysis of blood samples alone. The claims made in the article, therefore, are rather silly and dogmatic, since it is apparent that this group has not had the means of making the observations that would prove or disprove their point. They have been relying for years on a dubious method of approach to the problems involved.

I do not know Doctor Smith, but he sounds to me like a very dogmatic young man who knows much too little about the whole problem and who, therefore, can speak very freely. It would be very interesting to know the detailed clinical facts on the cases illustrated on page 7. I regard the analytical data on these cases as almost wholly irrelevant. I have never seen a case of encephalopathy precipitated by any type of therapy, and I do not believe that it ever occurs. I strongly suspect that we are dealing here with biochemical information interpreted very glibly without adequate clinical knowledge.

I consider it most unfortunate that such a bulletin as this would have been issued from an official source, since it confuses the facts in a number of ways, and contributes nothing useful to our information and our techniques in the control of occupational disease.

I return your copy to you. We shall make an effort to obtain a copy of this bulletin, but if you should happen to have an extra copy, I should be very grateful to you for sending it to me.

Sincerely yours,

Robert A. Kehoe, M. D.

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