

October 18, 1971

J. Julian Chisolm, Jr., M.D.
Baltimore City Hospitals
4940 Eastern Avenue
Baltimore, Maryland 21224

Dear Doctor Chisolm:

I am pleased that you took time to write to me, following your recent visit to the Kettering Laboratory, and let me say that I was very happy with your visit, with the opportunity to show you some of our facilities, and to talk with you about our mutual interests. It was not primarily an expression of my kindness, but rather of a very sincere interest in, and respect for, you and your work. I have been greatly concerned about the community problem of lead poisoning in children, and while I am not much concerned, in this connection, about the fall-out of lead upon the ground, streets and other surfaces in our cities, I am prepared to recognize the fact of its occurrence, and to alter my views of its importance if there should be any hint of its effects.

I have been pretty much out of contact with the problem of lead poisoning among children in Cincinnati since Dr. Hugo Smith left the Children's Hospital. I gather that things are going on, but I have no part in them. This, I think, is a bit unfortunate. Certainly it is for me, and I suspect that some of the people involved will have to repeat some of our experience of the past before they go much further. Perhaps, I shall learn to accept such things with equanimity, but my active professional life has not equipped me with the lack of concern which is required to dissociate myself from this tragic problem. For this reason, if there were no others (and such is not the case), I shall continue to be interested in your activities and observations. Among these, the behavior of the porphyrin in the blood strikes me as being of considerable importance, and partly for the reason which you have pointed out, namely that the substances in the blood, especially if they are bound, are not so subject to wide physiological variation as are those which occur in the urine. Primarily, however, the hemoglobin is an exceedingly significant feature of the human organism, and any factor which affects it, for better or worse, is of great significance in human physiology. I shall not have the access to people which you have in your hospital, but I shall certainly be very much interested in your observations.

We do have ready access to necropsy material, and I have given every possible assistance and encouragement to Pfitzer and Gross to make use of this material. Pfitzer (Sc.D.) is a sound scientist and an excellent teacher, and I am very glad to have Dr. Gross (Ph.D.) in close association with him in this and, hopefully, in other work.

I would like to have the work in which I have been engaged (not only on lead) continued and extended, and such a pair (certainly Pfitzer and some physician) may well be the means of arranging this so as not to incur any great lapse in the work. As you are aware, perhaps, other members of the staff (Petering, Ph.D., Klevay, M.D., Sc.D., Michaelson, Ph.D., Stemmer, M.D. and other lesser figures) are investigating other aspects of the behavior of trace metals, and I would like to make as sure as I can, that the entire field of investigation continues in depth and strength. I avoid intruding in any way

October 18, 1971

into the matters which are in the province of my good friend, Raymond Suskind, M.D., who is now the Director of the Department of Environmental Health and of the Kettering Laboratory, but I have a free hand in following and developing further my own interests, so long as I do not make assumptions - financial and otherwise - which impinge automatically upon the responsibilities of the Director. Nothing has ever been said about such matters, but I know all too well the administrative problems of this Department to be willing to augment the difficulties of the Director. In this multi-disciplinary field of Environmental (and Occupational) Health, in both research and the training of graduate students, the many problems of industry and the community, are difficult to handle properly, but having had a start in it in 1947, we are finding our way and, I hope, to some extent leading the way. At any rate, we have the understanding assistance of a group of persons, who, in their own fields of engineering and science, appear to know their way about.

The results of this work are being gathered systematically, and assembled in a form suitable for several lines of study. No such study has been made as yet, however, since the data must be such as to yield some answers to some open questions. As you have noted, the data available elsewhere have a great deal of variability and some of the conclusions reached by Schroeder, for example, are simply insufficiently supported by the evidence. They may be correct, but they may also be wide of the mark. Pat Barry's conclusions are not so shaky, for he and Mossman have been more cautious and less opinionated in advance of the evidence required to substantiate extensive views. I have worked with both of them, and I know that they are anxious to learn, and not trying to prove anything. This latter motive has botched up the conclusions of a number of "would be" experts. Somewhat more modesty is required of the scientist. Schroeder, for some reason which I cannot fathom, seems to have a chip on his shoulder.

But I must not allow myself to become garrulous, and I must get back to a paper I am writing for a "festschrift" in honor of Alice Hamilton, which will appear in February in the Journal of Occupational Medicine.

Best regards.

Sincerely yours,

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

RAK:wb

KE 0020208