UNIVERSITY OF CINCINNATI
COLLEGE OF MEDICINE
DEPARTMENT OF ENVIRONMENTAL HEALTH

KETTERING LABORATORY
EDEN AND BETHESDA AVENUES
CINCINNATI, OHIO 45219

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Dr. Ralph E. Yodaiken
Department of Pathology
Cincinnati General Hospital

Dear Doctor Yodaiken:

I have found this article interesting, and I am impressed by the amount of hard work that has been done. On that account, and because of my own lack of experience with electron microscopy, I have considerable reluctance to critize the work that has been done. Yet I find myself dissatisfied with the methods and with the lack relevance of the results to physiological or pathogenic mechanisms. I shall try to give you my reasons for this state of mind, and I'll also suggest certain approaches that should be made if indeed they are possible of accomplishment.

First, I must point out that the blood which you are examining has an appreciable quantity of lead in it before any has been added. Almost all of it is in some kind of bond with materials which are either on the surface of the erythrocyte or within its substance. Under ordinary conditions in man (not so well worked out in the case of various species of animals), this amounts to 20 to 30 micrograms of lead per 100 grams of whole blood, and the concentration ranges upward to 600, 700 and even as high as 800 micrograms per 100 grams of whole blood under varying conditions of exposure; still all but quite low concentrations are in (or on) the erythrocytes. It is said, on poor evidence, I think, that most of it, if not all of it, is on the surface, but no one really knows how or with what material. This is what I would like to know, and it does not seem to me that the concentrations of lead which have been employed in the experiments as described, can be likened, in their location in or on the erythrocytes, to those which occur under the conditions of life. The very fact that they are visible both on the surface and within the erythrocyte, while that which was (or is) there originally is not visible, indicates an important difference.

I wonder what could be done with lower concentrations, comparable to those which occur normally or naturally under occupational conditions, to establish where they go. Until some such thing can be achieved, I find it difficult to see just what significance can be attached to the reported observations.

I must confess that I am not familiar with the paper of Mortensen and Kellogg, on the "biological saturation" of the erythrocytes with lead. I've been much too busy to keep up with the literature, and only now, in my retirement, do I have an opportunity to explore the material uncovered by our excellent bibliographic staff. I hope to achieve this, in connection with a comprehensive examination of our own data, within the next two years. (However, I'll look up this article promptly, for it has an important bearing on some matters in which I am concerned at this time.)

I would hope that some type of "vital staining" of fresh tissue would prove to be feasible, in an investigation of this type. I find it hard to believe that

the results of the manipulation of dead tissue, including the use of fixatives, can reveal much about the in vivo mechanisms, except by comparison (or contrast) with those made on living tissue. This may be an expression of inexperience, and I would like to be shown.

I have no suggestions for the publication of the paper, except to express my opinion that it would seem to fit into a publication that is dealing with histopathological observations per se. Only if it could reveal something of the site of absorbed lead in the erythrocytes under the conditions of life would it be of immediate interest otherwise.

If you wish to discuss this matter with me further, I shall be glad to have you do so. I am returning it with this note only because I shall be out of town for the remainder of this week, and will soon be going to another meeting. I would not like to delay my response much longer.

Sincerely yours,

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

RAK:wp

enclosure