

April 18, 1952

Dr. Herman D. Pocock, Jr.
Willard Storage Battery Company
246-286 E. 131st Street
Cleveland 1, Ohio

Dear Doctor Pocock:

Your letter of March 10 has gone unanswered much too long, but I have had to be out of my office much of the time for the past two weeks, and consequently my correspondence has accumulated instead of being dealt with. I regret this, particularly in relation to questions of the type you have raised, since I should like to be able to deal with such matters promptly.

The matter of the significance of the determination of porphyrins in the urine is not too easily disposed of in a letter. The problem is one of such significance, that we began to investigate it systematically about two years ago, and during this time we have collected data systematically on normal individuals and on individuals exposed to lead in the lead trades as well as individuals who are the victims of lead poisoning. We have not had a large series of cases of lead poisoning recently, and therefore our information is not as comprehensive as I should wish. However, we have learned a number of things, and they have not made us any more enthusiastic than we were in the beginning about the use of this test. I should not wish you to think me dogmatic in the expression of my point of view, but I think that the following statements are justified.

First of all, there is no specific relationship between the urinary porphyrin and exposure to lead. A variety of situations result in the appearance of significant quantities of coproporphyrin in the urine. Moreover, there is no quantitative relationship between the extent of the porphyrinuria and the absorption of lead. It seems likely, and this is the principal reason we are interested in the problem, that the appearance of excessive quantities of porphyrin in the urine may be an expression of early intoxication. Whether this is the case or not, has not been proved by our own observations, but for physiologic reasons it seems probable. Thus it may be found to have the advantage of giving advance notice before intoxication has become severe. I am not sure that this is the case, but this is our best hope for the use of this technique.

Because of the nonspecificity of this test, and because it has no quantitative relationship to lead absorption, it does not impress me as being useful now or likely in the future, in connection with observations on men under continuing lead exposure.

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I doubt if it is as useful as rather precise methods of determining, quantitatively, the extent of the stippling of the erythrocytes. It may be found to be equivalent, but there is not adequate evidence on the point at present, and I would not accept it as at all likely. Certainly stippling is not quantitatively related to the extent of lead absorption, and if the porphyrin test is even less revealing in this regard, it will prove, I think, to be a very poor crutch. Actually I do not regard either of these methods as a satisfactory means of following persons who are exposed to hazardous or potentially hazardous degrees of lead absorption. There is only one method available by which we can determine the extent of the hazard of workmen exposed to lead in the lead trades. That is by means of analyses of the blood and/or urine, which will show us the extent to which these men have absorbed lead. Considering the variability of men, with respect to the time at which symptoms will develop and the severity of the symptoms which may develop, I do not like to depend on nonspecific and nonquantitative methods in this regard. Moreover I am quite frank to say that I do not believe that industry is justified, at present, in the conduct of dangerous industrial operations, unless it is in position to follow lead absorption by the presently available methods. Hematologic observations and the determinations of porphyrin will simply not do this. It has been quite clearly proved that they will not, and therefore, as physicians, we should not be placed in the position of having to depend upon them, since they are simply not good enough. Because of the difficulties and the expense of carrying out adequate numbers of lead determinations, we are undertaking to find short-cuts and less troublesome procedures for the determination of these facts. What is required, obviously, is a specific method of examination which will yield quantitative results directly related to the order of severity of the exposure to lead. This is the only means at present by which we can measure the risk to individual men, with an adequate degree of precision. I am being quite frank in saying that I would not accept the professional responsibility for a group of men working under conditions which I know to be hazardous, unless I had the means of following their lead excretion and measuring the extent of their hazard as it develops. This is the only way, I am sure, by which we can prevent the development of disabling cases of intoxication, under conditions in which exposure is hard to control and is, therefore, definitely hazardous.

Since you have asked my opinion on the article of Doctor Brooks and on the methods which he has used, I do not mind telling you that I do not agree with Doctor Brooks and I know perfectly well that his organization is resorting to analyses of the blood, in those cases in which they feel that they are dealing with considerable risk. I should also like to say, that the methods of analysis for coproporphyrins which are being used at the present time are generally fraught with considerable risk of inaccuracy.

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The best that one can say about them is that you may hope that they will reveal gross departures from the normal limits of urinary porphyrin. This is useful in the diagnosis of cases of illness, as contributory evidence, particularly when one cannot do lead determinations. Otherwise, I think they have very little advantage.

I gather that you are not altogether satisfied with the use of hematologic observations as a measure of medical control. In my opinion, you will find that you are dropping one method in order to use another which will not be one bit better. I think you will recognize from these remarks that I am not very enthusiastic about the usefulness of this test in preventive medicine.

I should not wish to discourage you from the attempt experimentally to make observations of this sort. Therefore, if you wish to go ahead with these observations, I would be pleased to send you a description of the methods which we are employing at the present time with some critique from our analysts on the method used by Doctor Brooks. I am not an expert in analytical procedures myself, and I always rely in these matters on competent chemists to test the methods and to select those which are the most satisfactory.

Sincerely yours,

Robert A. Kehoe, M. D.

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