

March 24, 1950

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Dear Doctor Orr:

I should like to express my appreciation of your courtesy on Wednesday, in talking with me about your patient, [REDACTED], and in letting me see [REDACTED]. It was particularly good of you to interrupt your busy day and to discommode yourself at your lunch hour. I felt and still feel apologetic, ^{but} in view of my responsibilities, it seemed necessary to get from you at first hand the basis on which you had arrived at the diagnosis in this case.

I have considered the possibility of obtaining more information by the further study of Mr. [REDACTED] that would throw light on his illness, and there are two matters in which it seems to me that I might make suggestions. I would like to make certain of the present facts with respect to his lead metabolism. Therefore, I would like to have duplicate samples of blood and two samples of urine, the latter to be taken some hours apart or perhaps on successive days. Accordingly I am sending containers for the collection of the samples together with instructions for the collection of both, and I would appreciate it very much indeed if you would see to it that the samples are obtained with meticulous care to avoid the possibility of their contamination. The instructions are specific and detailed and should be adequate for the purpose.

My other suggestion relates to the possibility of establishing, at some later date, a more satisfactory and better substantiated clinical diagnosis. Mr. [REDACTED] may recover completely or he may show progressive or retrograde changes of some characteristic type. I certainly hope the former will be the case, but inasmuch as the latter is a possibility, I should like very much to know what happens to him subsequently. If, therefore, it were possible to maintain contact with him and to follow up his case through you, it would be most useful to me. Since 1926 I have undertaken to obtain complete records on every case of actual or alleged tetraethyl lead poisoning that has occurred in the United States (and elsewhere as well, so far as possible). When it is possible to deal with the matter on a strictly medical basis without threat of medico-legal complications, I should like to be able to obtain records, or at least all of the pertinent information in the records, of this case from the hospitals at both Chattanooga and Nashville. I have tried to gather all of the facts as far as they relate to the hazards of this industry in all parts of

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the world, so far as that has been possible, and I have attempted to make this the center for all of the experience and published information on the subject. Consequently, I would like to maintain our records as nearly perfect as possible.

If you should wish to follow through on your idea of undertaking to "de-lead" this patient, and thereby to try to demonstrate that he has absorbed significant and unusual quantities of lead, I should be glad to cooperate with you by having the necessary analyses carried out in this Laboratory. In fact, if you wish to do this, I will suggest a means of collecting samples of food, feces, urine and blood so that you can have the data of approximately balanced observations for an interval of time. At an earlier time I would have advised against this procedure as being dangerous, particularly in relation to a case of suspected lead encephalopathy. Having established the fact that "demobilization" of lead does not occur, even under the most rigid limitation of the calcium intake and the most extensive shift in the acid base equilibrium that can be tolerated, I do not fear the procedure. I would not anticipate that it would do your patient any good, and therefore I would not advise it, but on the other hand, I do not think it will hurt him, and I should be willing to assist you in carrying out such observations, especially in view of your skepticism with respect to my statements. Incidentally our observations on patients, with respect to "mobilization" and "demobilization" of lead, and on concurrent symptomatology, have not been published for the lack of time in recent years to assemble the data of a rather large series of systematic investigations on which we embarked some years ago. I see no means by which this work can be published except in monograph form, and I've been too busy with the day's work to do more than outline such a monograph. Thus our recent publications have been fragmentary, and I am at some considerable disadvantage, since statements made on unpublished evidence may well be regarded as ill founded. The evidence, however, is available, and in due course I shall cite some of it in a report on certain aspects of this situation.

Having expressed my tentative opinion on this case and on the evidence thus far adduced toward a diagnosis, I shall not repeat it here. I shall investigate such aspects of the matter as are still open to some question and shall undertake to uncover any facts that might conceivably be pertinent and open to direct inquiry. After I obtain all the information I can get, I'll write a summary of my own opinion, for such use as it may be for the record and for the professional people concerned.

Cordially yours,

Robert A. Kehoe, M. D.

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P. S. An article has just come to my attention in the March number of The American Journal of the Medical Sciences to which I commend your attention. It is that of Proctor and Kahn, on "Disodium Hydrogen Phosphate Therapy in Lead Poisoning."

It illustrates a number of the errors of critical judgment and of inexperience that have characterized most of the experimental work of this type. (1) It ignores the variability of the excretory response of individuals from day to day based on variation in water output and on other factors not too clearly elucidated but important. Such variation is the greater, the larger the quantities of lead available in the tissues. (2) It fails to take into account the variability in the intake of lead from day to day by "controls" in their food and beverages. (3) It fails to include observations on untreated individuals to show the order of magnitude of the decrease in the urinary lead excretion following discontinuance from exposure, without any kind of therapy.

In other words, this work has failed completely to include the control observations necessary to interpret highly variable physiological processes. It will be noted that no analyses were made on the blood.

I call your attention to this for the reason that no one who has undertaken to understand and to elucidate any other aspect of the mineral metabolism would accept such results and conclusions as these as having any validity. The authors are right when they say that "this form of therapy merits further investigation." Actually all they have said that has any positive significance is that the administration of this medicament causes clinical improvement. This may or may not be true, since the usual case of lead intoxication improves, in varying degree of promptness, when exposure to lead is brought to an end.

The only evidence on this subject that is worthy of serious consideration is that obtained when the lead intake, and ^{the} output in the urine and feces are studied simultaneously coincident with observations on the changes in the lead concentration in the blood, all of these findings being interpreted ^{the} carefully developed background of corresponding observations on untreated patients.

R.A.K.

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