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Dear Dr. Flinn:

Let me apologize first for my delay in replying to your letter of December 30. I returned only a few days ago from a trip out of the country, and since that time have had to be out of my Laboratory continuously. As a consequence, my correspondence has had to wait until I could get to it. I hope the delay will have caused you no great inconvenience.

I am glad to give you such opinion as I can on the cases recorded in the attachments to your letter. Inasmuch as you did not ask that they be returned, I am keeping the records, since they are rather interesting exhibits. If you want them returned, please let me know.

With reference to the case of Anthony Candella, I have reviewed the record carefully, and it is a little difficult for me to come to a precise conclusion. As you have pointed out, the analytical data would indicate that the lead excretion is within normal limits. The lead concentration of 0.05 mgs. per 100 c.c. of blood is just at the upper edge of the normal, and in view of the history of exposure up to a very short time before the analysis was made, this would be accepted as sound evidence that there has been no recent significant lead absorption. The low value in the urine would seem to confirm this conclusion. If we can accept the validity of the analytical results, it is then quite clear that this man did not have lead poisoning. Any effort to explain the results on any other basis than analytical error could not weaken this conclusion, since the results, if accurate, show conclusively that there has been no real occupational exposure to lead. The nature of the illness on the other hand, sounds very much like lead poisoning, and the findings on the blood would seem to be suggestive clinical evidence of lead absorption. Moreover, the lead line, if such was actually present, confirms the conclusion that there has been significant lead absorption. Unfortunately, the lead analyses necessitate the opposite conclusion.

In this dilemma, it is perfectly apparent either that the analyses were made inaccurately or that the clinical observations

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were incorrect. I find it difficult to express an opinion as to which of these is the most likely. I have met Dr. Cotter, and for your private ear, I do not mind saying that in my opinion his clinical observations are very sketchy and are likely to be untrustworthy. I would judge from his experience that he ought to be able to do lead analyses, but other men have made serious blunders in such work, especially when it is turned over to laboratory technicians, and his general attitude in the only case in which I came in contact with him was such as to make me skeptical. In this I may be unjust, but I was certainly not impressed. In any case, I have no doubt about this situation. Either Dr. Cotter decided this was a case of lead poisoning and made the facts as to the history and the clinical examination fit this conclusion, or else his analytical data were incorrect. Such analytical results simply cannot be explained on the argument that during this period lead was being absorbed but not excreted. If lead was present in any considerable quantity in the body, it had to be present in abnormal concentration in the blood.

You raise the question as to the relationship between the lead concentration in the blood and that in the urine. It is not unusual to find discrepancies of this order of magnitude between the blood and the urine, although concentrations of lead in the urine as low as is reported here are quite unusual, unless the urine is extremely dilute. Such concentrations may be seen in spot samples of urine voided after a large water intake and therefore quite dilute. They are unusual even in normal individuals, and this makes me distrustful of the analytical results. In any case, the analytical error of the method can scarcely be less than 5%, and is probably greater than 10% for values of this order of magnitude, and therefore the discrepancy between the blood and the urine may not be as great as it would seem. I cannot but wonder whether Dr. Cotter uses some method of precipitating the lead out of the urine (either by means of ammonia or by means of an oxalate. If so, this might well explain his extremely low result for both of these methods are unreliable in our experience, and unless one is dealing with fresh urine, they are always to be viewed with some skepticism.) The fact that analyses were made by one of the modifications of the Dithizone method is in itself of no significance. There are several modifications of this general procedure that are extremely sensitive and specific. There are others that are neither specific nor sufficiently sensitive. The method is very tricky in inexperienced hands, and it must be carefully and skillfully executed.

In the case of this same man, the examination by Dr. Reznikoff indicates a somewhat different state of affairs. The examination is somewhat later, and it is quite apparent that there could have been some change in the clinical picture as well as

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in the laboratory findings. Unfortunately Dr. Reznikoff's analyses were made following the period of three days of ammonium chloride therapy and low calcium intake. This, in my opinion, makes it less easy to interpret the urinary findings. In my experience, the general tendency of this therapy is to reduce the lead concentration in the urine because of the factor of diuresis. (It is true that diuresis does not always occur and that even when it does occur, it usually does not last throughout the period of therapy. However, this procedure always beclouds the issue to some degree and, in my opinion, the determination of the status of lead absorption by this means is unsound.) Despite what I have said, the finding of 0.13 mgs. lead per liter tends to indicate that this man had had some lead absorption. Granting that the method of analysis was sufficiently precise, a lead concentration of this magnitude following some time after the discontinuance of exposure would tend to show that the lead exposure had been significant. However, the statement in the conclusion that the normal urinary content is 0.10 mgs. per liter rather tends to detract from this conclusion. If Dr. Reznikoff's normal mean value is of this order of magnitude, then his method is not sufficiently exact, for the mean value of normal individuals is under 0.04 mgs. per liter when all possibility of contamination in the collection of the sample and in its analysis, has been eliminated.

On the whole, despite contradictory evidence in the two physical examinations and in the two sets of analyses, I am disposed to feel on the clinical grounds that this man probably had lead intoxication and that the original analyses made by Dr. Cotter were inaccurate. It is most unfortunate for this man and for the establishment of the facts at this time, that the handling of this case initially was somewhat bungled.

The second case, that of George Petruska, is a more difficult one to deal with. I have no way of telling whether the history of lead exposure is exact. If it is, one would feel that this man had had some opportunity for significant lead exposure. However, there is nothing in the entire clinical record that would justify the conclusion that he had had any significant lead exposure with the single exception that it is stated that there was a large amount of "basophilic stippling." No quantitative figure is given, and therefore the statement is not as definite as one would wish. The analytical findings in this case, if accepted at their face value, clearly indicate that this man had had no recent occupational lead exposure. The figures are well within the normal range and the result on the urine is so low that it is almost certainly incorrect. It is very rare, in the most dilute urine to obtain a result like this even in wholly unexposed persons. I must confess, I have no possible way of visualizing the workings of Dr. Cotter's mind. The only conclusion that could possibly be justified on the basis of his record is that this man had had no lead exposure in his occupation for many

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months, yet he states blandly that there is increased lead in the blood and in his diagnosis speaks of "lead storage," whatever that may mean. Having all the evidence before him, he still maintains that lead is a factor in this man's illness and discusses in his report of December 1, "the serious error" that has been made in taking the man off a high calcium diet. As to the wrist weakness and the wrist drop which Dr. Cotter appears to feel is impending, I am not convinced. If he were on the point of developing a wrist drop, it would scarcely be influenced greatly by the therapy which he suggests.

I cannot escape the feeling that this man Cotter is a dangerous man in any relationship to industrial medicine. He seems to have little clinical judgment and his laboratory interpretations are a strange mixture of fact and fancy. With it all he is very self-assured, speaking with an authority which I am sure his experience and knowledge does not justify. It is in fact the authoritative voice either of a medical novice or of a charlatan. Obviously, I should not speak so directly and frankly except to a medical colleague. I think it is important in this field to separate the ignorant from the experienced person and especially to separate the honest from the dishonest. I am not sure where to put this man, but I do know that he belongs with the goats and not with the sheep. There has been so much confusion in this field that it is time we were beginning to get it clarified a little. We shall probably never be able to do this until the uninformed and designing adventurers in the field have been discredited.

From my point of view the worst aspect of this kind of blundering is not so much that an industry is penalized unjustly as that a man who has a just claim for compensation may lose his claim. Any claimant deserves to have his case studied carefully, honestly, and competently, since it is only in this way that he can obtain justice. Likewise, every employer is entitled to have the facts concerning his employees studied in an entirely unbiased and competent professional manner. I only differentiate the two situations practically for the reason that the employer can take the gaff a little more easily than the employee.

I should be very much interested in knowing what occurs further in connection with these cases if you should care to tell me.

Cordially yours,

Robert A. Kehoe, M. D.

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