

CONSOLIDATED EDISON COMPANY OF NEW YORK, Inc.
AND
AFFILIATED COMPANIES

4 IRVING PLACE
NEW YORK, N.Y.

J. J. WITTMER, M. D. .
DIRECTOR, PERSONNEL DEPARTMENT

N. E. ECKELBERRY, M. D.
ASSISTANT MEDICAL DIRECTOR

R. C. KIMBALL, M. D.
ASSISTANT MEDICAL DIRECTOR

January 31, 1940

Robert A Kehoe, MD
College of Medicine
Eden Avenue
Cincinnati, Ohio

Re [REDACTED]
Con Tel & Elec Sub Co
Transportation Department
(Assistant Stockman)

Dear Doctor Kehoe

[REDACTED] #17
Sub Co
Chauffeur - Class A
(Stockman)

Subsequent to our conversation on January 22, 1940, I have reviewed the whole problem presented by the two above captioned employees with the proper parties in the Consolidated Telegraph & Electric Subway Company and I have obtained approval to request you and your laboratory to repeat the laboratory tests on these two men as regards determining the lead content of the blood and urine and also to request your laboratory to make an investigation of the conditions at the garage and repair shop of the Consolidated Telegraph & Electric Subway Company located at 491 Walton Avenue., New York City. As per our conversation, this investigation of the conditions at the garage and repair shop is primarily to determine whether men working in the stockroom particularly between the hours of 7 and 9 AM are subjected to a sufficient exposure to lead to account for an abnormal absorption of lead which might account for lead intoxication.

I am enclosing herewith a resume of our clinical data and laboratory reports on these two men which, of course, must be considered as confidential.

All reports of the tests and reports of the investigation of the garage and repair shop should be referred directly to me at this office. It is my understanding that there will be no charge made to us for these tests and investigation inasmuch as a public health problem is involved. If I am incorrect in this assumption, please advise me before proceeding with any tests.

Containers, needles, instructions, etc., should be sent directly to me and I shall see that the instructions in procuring the samples are followed in detail.

Very truly yours

N. E. Eckelberry

NEE/emd
Attachments

February 1, 1940

Dr. Niel E. Eckelberry
Consolidated Telegraph and Electrical Subway Co.
4 Irving Place
New York City
New York

Dear Doctor Eckelberry:-

I have been delayed by the pressure of other work in making a few comments on the cases which we discussed some days ago in New York. I realized that owing to the brevity of the time available, and because of our preoccupation with the significance of the lead exposure as a factor in the diagnostic problems of these men, I had neglected to give adequate consideration to certain points in the differential diagnosis in these cases. I think I might profitably state my opinion on clinical grounds, point out the reasons for a certain skepticism as to the accuracy or adequacy of the analytical results, and then briefly mention one or two points in relation to the insignificance of the type of lead exposure which was under consideration in these cases. First, I believe it is quite well recognized that there are many possible causes for retro-bulbar neuritis. Lead is mentioned only rarely in this connection. Very few cases actually have been seen. Of these a still smaller number have definitely been convicted as due to lead. Whether such neuritis due to lead can be or is likely to be unilateral has certainly not been clearly demonstrated. I can not state from experience whether it is more likely to be unilateral for the reason that I have never seen a demonstrable case of retro-bulbar neuritis due to lead. I have seen several cases of retinal hemorrhage attributable to lead, but in each case the lesions have been bilateral. It is my feeling from my knowledge of the subject, that lesions of the eye due to lead are much more likely to involve certain vascular disturbances with hemorrhage than are some of the other causative agents to which retro-bulbar neuritis has been attributed. It seems quite possible that fleeting visual disturbances can result from local vascular spasm and this might conceivably be the mechanism in lead poisoning. Certainly, however, most of the cases of vascular disturbances reported in the literature have been of a bilateral type.

Considering the rarity of retro-bulbar neuritis due to lead, it would seem almost incredible if two

such cases were to occur at the same time in the same place, and from the same cause. Accordingly I was not greatly surprised to find that the pathological picture in these two cases was appreciably different. In the one instance, that of Connor, I could not but feel that we are dealing with an exudative process which could scarcely be explained as the effect of lead absorption.

Further considering the rarity of this condition in relation to lead, it would seem to me that the diagnosis of lead poisoning could only be arrived at after the very careful exclusion of the numerous other factors such as various drugs, infections etc. I did not inquire into this aspect of the matter in specific detail but I should be very much interested in knowing the extent to which other factors, such as the use of tobacco, for example, had been eliminated. In this connection, the conviction of lead as the causative agent in terms of a history of lead exposure, would have to be made, I think, on the basis of definite evidence that the type of lead exposure in question had been capable of producing at least some of the manifestations of lead poisoning in other people. Lead encephalopathy and lead neuritis are generally speaking a manifestation of lead exposure and lead absorption of considerable magnitude. So far as I know, the condition has never been reported and has never been confirmed, except in connection with occupational exposure or accidental non-occupational exposure of serious magnitude. In this instance, therefore, the history of an exposure to conditions which hitherto have never been known to produce a single case of even the mildest lead poisoning, is not a history of significant lead exposure and can not be regarded as a contributing factor to the diagnosis.

I realize that in making the above statement, I am ignoring your tentative conclusion that the opportunities for exposure here were somewhat unique, and also that I am not taking into account the more significant evidence obtained from the analyses of blood and urine which indicated that these men had absorbed abnormal quantities of lead. Continuing to ignore these points for the moment, however, I wish to return to my statement that the neurological lesions resulting from lead absorption are the result of serious exposure and not mild or questionable lead exposure. One would expect, therefore, if such lesions were due to lead, that the analytical results obtained in the blood or urine would not be in the middle or doubtful zone of values, but would be highly elevated and would continue to be elevated for prolonged periods of time. Even under these conditions, I should question the diagnosis in view of the fact that the neuritic lesions in these men had been unassociated with retinal hemorrhage, had been unilateral in character, had continued to improve fairly rapidly, and had, moreover, been unassociated with other adequate evidences of active lead intoxication.

Turning now to the analytical data.
I wish to record the opinions which I expressed to you in my

conversation that I could not but question the validity of the analytical data. There are several reasons for my skepticism. One arises from my knowledge of the fact that the quantities of lead which are being dealt with in such analyses are so minute as to be among the most delicate of all chemical procedures. I believe we work in this laboratory with a somewhat greater care against contamination than almost any other persons who are working in this field. This is not the result of a desire on our part to work with a refinement which is far beyond the need of the situation. It is rather the result of rather bitter experience with the difficulties and pitfalls associated with the handling of materials for analysis and the conduct of the analyses themselves. Even under the conditions under which we work, we occasionally find that a sample is contaminated either in the process of collection or in the process of analysis. We find it necessary to exclude the soot and dirt in the atmosphere around the laboratory by filtering the air entering the laboratory. It is also necessary to exclude from our analytical laboratories all lead-containing materials and compounds in so far as this is humanly possible. We find it necessary in order to obtain accurate analytical results on small samples to exercise the most meticulous care continually by purification of all reagents, by the most rigid supervision of our glass ware, and by the adoption of the most uniform technique in every operation. Even then in our investigative work we analyse samples by parallel methods of analysis in order to avoid the difficulties that result from occasional contamination. That is to say, we use both the spectrographic and dithizone methods of analysis, and in the case of the latter method, we have reduced our blank to something under one gamma. This blank, as a matter of fact, is of the greatest importance since if it is appreciable, or if it is of any considerable magnitude, it introduces the possibility of serious error into the analyses. Despite all of these precautions, I should never be content to trust the analytical evidence in a diagnosis unless we have a series of results which confirm each other. By this I mean that in every instance we obtain either a blood sample and a urine sample simultaneously or we obtain a series of urine samples which give us the same picture. If the blood and urine samples are in disagreement with each other, as they sometimes are, we repeat the observations since we have found from extensive experience with adults that the two results should be in substantial agreement in respect to their deviation from the normal values.

I recite the above points not to argue but merely to show what reservations have to be attached to the analytical data in these cases. With this idea in mind, I note certain discrepancies in the analytical data which have been obtained in these two cases which, although they are within the limits of possibility, are not the accepted results. In the

case of Connor, the results reported on December 7th were 0.10+ per 900 cc of urine, and 0.17- per 100 cc of blood. I suspect such results as being wrong at once on their face since if the blood was at this level, I should expect the urine to be even higher. I may say parenthetically that I suspect these results, at least the blood result, as being incorrect for the reason that I can not conceive of any combination of circumstances in a garage which would produce that level of lead concentration in the blood. In the case of Drum, the values obtained on the blood and urine are more consistent. On the other hand there is one striking factor which makes me somewhat skeptical. It is reported that on November 28th the urine and blood values respectively were 0.14+ and 0.12+, whereas only a week later, the urine was 0.077 and the blood was 0.062. Again these are not impossible findings but they represent rather sizeable drop, particularly in the blood, in so short a time.

You will probably think it strange that I am so disposed to be critical of these findings. I can assure you, however, that this is not being nor does it arise from an unwillingness to accept the results of other persons. In all frankness, however, I can say that I found from experience that the analytical results obtained on these materials in most laboratories are highly variable and highly inaccurate. There are only a few people who have had the training and experience that is necessary to appreciate the opportunities for error which exist in this type of analysis. Indeed it is only after a considerable experience that one is willing to admit that this is a most difficult procedure, and that it often gives rise to misleading results. Unfortunately the tendency of the available methods which are used commonly at the present time is to yield high rather than low results.

I think you will be considerably interested in the general type of information which has been obtained with reference to the exposure of garage mechanics, filling station attendants and other workers. Accordingly I am sending you a reprint which gives the data on one of our latest studies in this field. The surprising thing about this whole matter of lead absorption and lead excretion is the uniformity of the rate of excretion of a considerable number of individuals under the same type of exposure. One would expect a much greater variability than occurs. It is because of the tendency to uniformity of excretory response of a large number of individuals under the same type of exposure that I am convinced of the adequacy of this type of information as a measure of the magnitude of exposure. It is on the basis of such observations carried out on a large number of men under a wide variety of working conditions in relation to the use of gasoline containing tetraethyl lead, that I am quite convinced that there is no circumstance or combinations of circumstances which can produce a hygienically significant lead exposure as a consequence of this type of fuel. I am quite convinced by both the use of

negative and positive evidence that so long as the lead content of automobile gasoline does not exceed the present levels, there will not be a magnitude of lead exposure from this source which will or can result in lead intoxication. It is for this reason that I am so very keen on the study of your cases, and on the investigation of the conditions which are presumed to have brought them about. In the first instance I believe it is almost imperative that we establish beyond any doubt the presence or absence of lead in significant quantities in the blood and excreta of these patients. If the finding of abnormal quantities of lead is confirmed, then I am quite confident, regardless of the diagnosis in these cases, that we shall have to look carefully for some source of lead exposure which is not derived from the exhaust gases of automobiles. Despite my virtual certainty on this point, I should be quite willing to approach the whole matter with an open mind, and to determine the facts in the case of the garage in question.

The above rather lengthy letter is in large measure a re-statement of the point of view which I expressed to you in our discussion. It has occurred to me that you might like to have it in writing. Therefore I have gone into some detail to make my attitude entirely clear.

Please accept my thanks for the privilege of having discussed these cases with you.

Cordially yours,

RAK:is

Robert A. Kehoe, M.D.

P.S. I neglected to mention the microscopic evidence obtained in the blood, for the reason that it is not especially helpful. One case showed no stippling, which is to some degree useful as negative evidence, while the other was not at a significant diagnostic level of stippling. In the case of lesions of this degree of severity due to lead, one would expect unambiguous elevations of the number of stippled erythrocytes. RAK.

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