June 22, 1971

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Dear Doctor Beritić:

I write at this time to explain, to you, the many difficulties which have prevented me from getting the papers of the Amsterdam Conference into print for such a long time. I want also to make clear why it is that not all of the papers, and none of the discussion has been published. Included in the latter, is the reason that one of your papers - the one on lead colic and blood lead - has been published, while the other has not.

First, let me say that the Archives of Environmental Health does not publish the Proceedings of meetings in which it has had no hand in so arranging that all of the papers are to be wholly original and composed of material that has not been published previously. Moreover, the discussions must be prepared beforehand, so as not to be spontaneous, or else they are subject to editorial modification. You will recognize, therefore, that the organization of this Conference was not such as to provide such satisfactory material. After its submission to the Chief Editor, it was unavoidable that it should be reviewed by the Chief Editor, the Editorial Board, and other reviewers, for criticism and comment. For these reasons, plus the critical action which I was called upon by the Chief Editor to take, a number of the papers did not measure up to the requirements of the Archives, while I, in person, had to advise against the inclusion of a few which were not relevant to the objectives of the Conference. Moreover, several of the papers came to my attention as having been published in full elsewhere. These, of course, in accordance with the fixed policies of the Archives, had to be eliminated. They will be included only by citation of references.

This, I think, will acquaint you with the situation. Thus, it came about that I had to edit all of the papers which had been adjudged acceptable, if edited properly. The editing has amounted to a fairly large job which only just now has been completed. Those which were, so to speak, translated by their authors from their native languages, into English, had to be put into proper linguistic form - that is, rewritten - and the curves, photographs and other figures had to be redone to suit the form required by the Archives, and the references, also, had to be dealt with similarly. You will see what I mean, when you see your own paper in its final form.

All of the above requirements have taken time, and, unfortunately, I have been handicapped by other obligations and also by some degree of disability. (I was overtaken by a labyrinthitis in February of 1970, which made it impossible for me to work consistently, for a time. It subsided considerably by September 1970, but I still have to drop my work when I get tired, especially when my eyes warn me that they are being overworked.)

In the case of your two papers, the one relating to colic, as I have indicated above, has been prepared for publication, and is in the hands of the Chief Editor. The other was regarded by a reviewer (for the Archives) as unsuitable for publication, and I had to accept his appraisal. This need not prevent its publication elsewhere. The reviewer, however, considered it irrelevant to the objectives of the Conference, and as being an expression of opinion, that is, a theoretical interpretation of certain aspects of lead poisoning in a manner which could not be proved or documented. As a matter of fact, all of the papers which were concerned with hypothetical concepts of the nature of the disease were dealt with in the same manner, by other reviewers, and these decisions could not be combatted by either personal or professional arguments.

I should say to you, further, in view of my comments in your edited paper, that I would quarrel with your conclusion about the irrelevance of the concentration of lead in the blood to the existence of lead poisoning, at only one point. You, no doubt, have seen lesions of lead poisoning, in men whose blood lead was below 80 micrograms per 100 milliliters of whole blood. I do not doubt your observation. I have seen such events myself. The issue, however, here, does not negate, as some persons would hold, the diagnosis of lead poisoning. But the latter is not made by finding 80 or more micrograms of lead in 100 ml. of blood of a patient. The diagnosis is made clinically, as I have always maintained, and as you are aware. The analytical findings may be compatible with such a diagnosis and so help to confirm it, because they may indicate that the patient has had sufficient exposure to justify such a diagnosis, if made on the basis of a suitable history or other clinical findings. But if the patient is not seen reasonably promptly, the excretion of lead from the body, plus its distribution int the dense part of the skeleton, lowers the concentration in the blood (and urine) so tha this finding is not helpful in portraying the magnitude of the (one time) exposure. We see this frequently in controversies in which analytical evidence was not obtained early enough. Then one has to make his diagnosis from the history and the residual physical and laboratory findings (if any). On the other hand, I have said that, at the onset of lead poisoning, which means, in occupational medicine, that the workman was overtaken by lead poisoning during the period of work or just after it (not days or weeks or even months after the termination of his exposure, but during the period when he was under the observation of the industrial physician). The same is true of a child, who may not have reached the hospital, where he is being studied, until days or weeks after the illness overtook him, when someone woke up to the fact and sent him into the hospital. It must be remembered that most of these children belong to poor people, in poor housing, and that they do not get to the hospital until they have had convulsions, or some other dramatic form of the illness. Thus, most of them, but not all, have had severe exposure, and high values in blood and urine, which persist and actually increase during this type of environmental exposure. Some, who get out of this exposure by moving, or by some other means, and who fail to recover promptly, are seen by the pediatrician, long after the illness first appeared. This, I am sure, is the reason why Montcrief in England, found sick children with residual lead poisoning whom he had no chance to see at the time of onset.

The value of the figure which I have proposed, is that it is the least level of lead in the blood that I have found in persons who have contracted lead poisoning while still engaged in work - I do not mean during the day while they were at work, but during the period of their employment. This then is, in my experience, the first warning of danger, and the signal for removal from employment, until such time as the lead in the body decreases to a safe level. Meanwhile, it is essential that the excessive exposure which produced this result be reduced to a satisfactory level. By this means, lead poisoning can be (and has been) effectively prevented in industrial

populations. This is not a theory of lead poisoning, but a fact of preventive medicine, which works. It has been worked out factually in many years of responsible experience, and there is no doubt of its truth as a pragmatic fact of life. We have seen more than twenty years of utter freedom from the occurrence of lead poisoning in dangerous types of occupational exposure, and we have learned how to give workmen the benefit of full protection. It may be that the level of 80 micrograms is higher than it should be considering the methods of analysis which are often employed, but it is satisfactory - or has been up to the present, when these methods are as precise as those which we use. If we should find that there are persons who become ill at a lower level, we shall follow the lower figure in dealing with workmen in industrial plants. Cramer has said that the figure employed in Sweden is 70 micrograms; with this, I would not quarrel. But, as a matter of fact, in dealing with thousands of people in some 50 industrial plants, 80 micrograms has been low enough, while in plants insufficiently controlled, most of the cases of poisoning occur at 100 micrograms or above. We have every reason to believe, at present, that the threshold of 80 micrograms is satisfactory for the prevention of poisoning in the most susceptible person who is likely to be employed in industry in this country.

I'm sorry to be so long in saying this, but what I have said previously, is so frequently misinterpreted, that it is necessary to state the case with somewhat excessive speech. I am sure that the application of this principle, along with the environmental control that will fit it, will remove lead poisoning from industry. I am sure, only because it has been accomplished in industries which will follow proper advice, under a type of medical and hygienic supervision, that is reauired to know what is going on at all times.

I have had the pleasure of taling with one of your countrywomen, Mrs. Zarka Kerin, who has left one of her papers with me to edit and prepare for publication in the Archives of Environmental Health. I presume you know her and know that she is working in the project concerned with the group investigating the background and the problems associated with the mine and smelter in your northern mountains along the Meza River.

It has been a pleasure to talk with her, and I am very glad to be able to assist her in some ways.

With kind regards and the hope of seeing you in your own country in the not too distant future.

Sincerely yours,

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

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