

May 27, 1970

Mr. Francis S.L. Williamson
Editor of BioScience
American Institute of Biological Science
3900 Wisconsin Avenue, N.W.
Washington, D.C. 20016

Dear Mr. Williamson:

You have given me a very unpleasant and somewhat dismaying task, in sending to me, for review, the manuscript of the paper of Paul P. Craig, Ph.D., entitled "Lead in the Biosphere," etc., etc. I greatly deplore anything which has any of the odor of repression, and in this situation, in which I am, in a way, one of the voices, if not the principal voice, in a very considerable controversy at present, I dislike to be involved in a decision of policy.

The paper is an extension of the several papers put together by C.C. Patterson, Ph.D., of the California Institute of Technology, one of which - a principal one - I urged for publication in the Archives of Environmental Health, so as to get it out in the open. As to the technical papers of Patterson, et al., nothing I might say of a detrimental nature would be important or appropriate. Much of the work has been admirable. His entrance into the purely biological and, especially, the clinical area has been unfortunate and essentially evangelical and illogical in character. This, I am forced to say, is the atmosphere of the present paper, in which almost all of the misinterpretations and misjudgments of Dr. Patterson and others, in the literature, have been restated, along with many truths and half-truths. Let me give a few illustrations, for I have neither the time nor the inclination to review, critically, the entire paper, although I have examined it carefully.

1. Dr. Craig speaks with seeming authority about the "critical level for brain damage in children as substantially below that in adults." This statement could only be derived by devious indirection, since there is no "level" in the brain itself, or in the blood, or in the body as a whole, which can be quantitatively or even approximately associated with brain damage from lead. The sentence has a very ominous sound, but it is sound and fury, not fact. The statement which follows it, in the abstract, about what we do not know about "chronic exposure" to lead, and encephalopathy, has also an awesome sound, but it has absolutely no meaning or basis in knowledge or experience. The quantities of lead involved in childhood plumbism and encephalopathy are generally so enormously different from those involved in exposure to the ambient air as to render 1/4 or 1/2 of such amounts as entirely beyond the order of magnitude of our consideration.

2. Dr. Craig states categorically that febrile diseases and metabolic disturbances result in the mobilization of lead, with presumably disastrous consequences. The hypothesis on which this statement is made was discredited more than two

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decades ago, although, of course, it is still in the literature, while its rebuttal is not so easily found by the novice because the evidence was presented quietly.

I could try your patience by example after example of this type throughout the paper, but I will desist. The difficulty is that Craig, like Patterson, has ventured into a field of review in which he has not had the experience, training or detailed knowledge required to understand the very nature of the controversies, much less the judgment to appraise the meaning and the weight of the evidence, or the points of view, on one side or the other of this complex matter. The essential question in this matter was not raised, seriously, with respect to lead, in earlier times - that is, the question of the potentially harmful effect of the absorption of lead in the small amounts which exist in the human environment, which, until our work in 1925 et seq., were not known, and it is not likely to be answered by amateurs with ever such good intentions.

I do not think this paper will contribute a single fact of value, or that it can create a way of thinking which will lead to the truth. Consequently, I do not think it worth publication. Dr. Craig's opinion on a basic physiological problem is not of importance, while his opinion on a basic problem or concept of physics would deserve respect. His opinion of me, derived, no doubt, from Patterson, is still less pertinent.

I could wish that this way of approach to this and other environmental issues of the physiological type could be discouraged. I doubt that it can be. If you refuse to publish it, it will appear somewhere else, in all likelihood. You, however, and your Journal, will not be involved in such an approach. I advise against the acceptance of this paper. It is not needed, and it will not be useful in enabling our community to come to a satisfactory answer. For your private ear, I think this issue will be decided without benefit of knowledge, but for political reasons.

Cordially yours,

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

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