

Lead Industries Association

420 LEXINGTON AVENUE

NEW YORK 17, N.Y.

CLINTON H. CRANE, PRESIDENT
H. C. BROWNELL, VICE PRESIDENT
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SECRETARY-TREASURER

MANFRED BOWDITCH
DIRECTOR OF HEALTH AND SAFETY

June 9, 1948

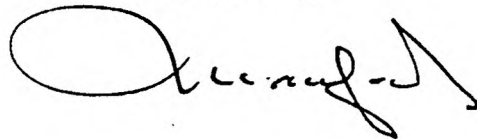
Robert A. Kehoe, M.D.
Director, Kettering Laboratory
College of Medicine
Eden Avenue
Cincinnati 19, Ohio

Dear Bob:

I have noted the abstract of a paper by Ryder Cholak and Kehoe on "Influence of Dithiopropanol (BAL) on Human Lead Metabolism."

If reprints of this are available, I would greatly appreciate your having one sent to me.

Sincerely yours,



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DIRECTOR OF HEALTH AND SAFETY

July 8, 1948

Robert A. Kehoe, M.D.
Kettering Laboratory of Applied Physiology
College of Medicine
Eden Avenue
Cincinnati 19, Ohio

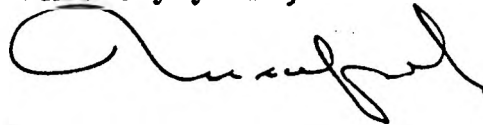
Dear Bob:

Thank you for your letter of July 6. I had a hunch that I would get a reply from Cincinnati and what I got is remarkably in line with what was expected. I flatter myself that I understand what you are talking about (perhaps, as we speak of effective temperature, we should coin some such phrase as "effective toxicity"). I am in substantial agreement with most of what you say.

Quite apart, however, from the good, bad or indifferent jobs that various industries are doing, I cannot feel that such discussion is unprofitable. In general, the consensus of opinion seems to be that we should look forward to a differentiation in MACs such as I have suggested, but that further studies must be a preliminary. I wanted to start some thinking along these lines and have apparently done so.

If you are interested, drop in when you are next in these parts and I'll show you the rest of the correspondence.

Sincerely yours,



July 26. Telephoned and asked to use my letter as a basis for discussion - the personal comments and the last sentence of which M.D. disapproves

RBK



KE 03425

July 6, 1948

Mr. Manfred Bowditch,
Lead Industries Association,
420 Lexington Avenue,
New York City 17.

Dear Manfred:

I am glad to comment on your letter of July 1 and its enclosures, and what I shall say is precisely what I should be prepared to say publicly, at the meeting of 2-37 or elsewhere.

First, there can be no doubt that the practical hazards of occupational lead exposure vary tremendously with the chemical composition and physical state of the compound or compounds suspended in the air breathed by workman. I have never said or thought anything to the contrary. It is obvious that galena, suspended in the air in connection with mining operations, presents a quite different problem from that of the same concentration of metallic lead and/or lead oxide, distributed in the atmosphere of a smelter or foundry. One is substantially harmless. The other in the concentration of 1.5 mg. per 10 cubic meters is dangerous for prolonged periods of exposure (in excess of one to two years). This is not wholly a question of solubility, but is in part a matter of state or subdivision and shape of particle. No doubt the solubility and chemical reactivity, (i.e. nature of the chemical compound,) are important factors of the practical hazard of lead compounds, but the physical factors also influence the matter tremendously.

Second, when we speak of toxicity, we must be sure whereof we are speaking. Fairhall, with his customary difficulties of cerebration and personality, confuses the issue, not by misquoting me, but by failing to understand what either he or I is talking about. Toxicity, in the toxicological laboratory, may have meanings utterly unrelated, or only indirectly related, to practical hazard. When I spoke (correctly) of the toxicity of tetraethyl lead, I was talking about the lethal properties of a material that was known to have been absorbed, and about the effects upon the functions of tissues incident to the presence therein of a known quantity of lead. I did not undertake to say what its final form or chemical combustion in the tissues might be. My purpose in the article cited was only to point out that when one administers lead chloride or lead acetate (dissolved in water) intravenously, the mortality among experimental animals corresponds to that which results from the intravenous injection of a corresponding quantity of lead as lead tetraethyl. I did not say, and in fact, did not discuss, in this relationship, the practical industrial hazards of tetraethyl lead. Fairhall knows full well that I have stressed the seriousness of these hazards, their mechanisms, and their control. He simply cannot bring himself ever to be rational, sensible or relevant in any matter that involves me and my doings and sayings in relation to lead. That is, of course, understandable, but it is childish and

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also dangerous. But to resume the discussion, toxicity, in relation to a cell or a collection of cells in the body, is one thing. The potential toxicity of a material outside the body, on the skin, in the alimentary tract or even in the respiratory tubes and air cells, is quite a different matter, since any evidence of toxicity depends upon the ability of the material to gain access to the tissues. From then on what happens will depend upon where it goes and how it is disposed of, but, if it does not get in, it exerts no practical threat. The general biologist knows this and has long since pointed out that there is no necessary relationship between the toxicity of a substance for unicellular organisms, and that for complex forms of animal life. I repeat, therefore, let us know what we are talking about.

Third, there are very few sound clinical and experimental data, in relation to the various compounds and subdivisions of compounds represented individually or collectively in industrial environments, whereby we can set tolerances. I and my associates have the most precise and best correlated data available on this matter, I am sure, in relation to a number of situations, but with one exception, namely, that of exposure to tetraethyl lead, they are not qualitatively and quantitatively adequate for the purpose. We regard this problem as requiring extensive investigation, and we have planned a program of human experimentation along this line, comparable to our work on the ingestion of lead. Until this is done by ourselves and/or others on a comprehensive scale in the laboratory or in industry, tolerances cannot be set with much better background than now exists.

Fourth, a standard not related to the environment, but to the extent of the lead absorption, could be set up, with every assurance that it will stick indefinitely. This cannot be done in relation to stippling or to quantitative estimates of basophilic material in the blood, for these physiologic responses are not dependent primarily or solely upon lead absorption. There is no point whatever in adding confusion to the existing confusion by promoting reliance upon any such procedure. One will have to measure by appropriate specific analytical means the extent of the absorption of lead, as shown by the lead concentration in certain body fluids. When this is done, no important consideration need be given to the nature of the exposure or to the compound involved. Danger, or the lack of it, can be expressed adequately in terms of the concentration of the element. In relation to both of these matters (i.e. basophilic material in the blood, and lead concentration in blood, urine, etc.) the data are available to prove my statements, and therefore no serious question of fact exists. Here we return to the problem of toxicity. There can be no very serious doubt that the extent of the risk of poisoning from lead which has actually been absorbed into the body, relates to the quantity absorbed and not to the nature of the compound to which the individual was exposed. (There may be an exception to this, but for the lower levels of prolonged absorption and accumulation in the body this statement can be accepted as valid with little or no doubt.) Thus, if we were to resort to such a standard, we could ignore all controversial and confusing matters related to the "toxicity", solubility, state of subdivision, of the several industrial lead compounds and devote our

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attention to the maintenance of safe conditions for the men.

Finally, I do not believe it is either practical or necessary to undertake at this time to set standards with respect to maximum allowable concentrations of various lead-containing industrial materials in the atmosphere of working establishments. Bloomfield's standard, a considerable improvement over that of Duckering (as promulgated by Legge), is largely a bench-mark - a reasonable approximation of the general situation. It relates to certain conditions and not to others. It is at once too high and too low. But it serves a useful purpose among the initiated, and hurts the cause very little among the dogmatic and ignorant. Until we are in better position to substantiate other figures, we had better leave it alone. We do not need a new standard of this kind nearly so much as we need performance in line with the implications and the purposes of the present standard. It is certain that our knowledge is not altogether adequate, but if American industry were to do half as well as present knowledge would justify, we would not be engaging in this kind of a discussion.

Sincerely yours,

Robert A. Kehoe, M. D.

FAK ef

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