July 10, 1969

Katharine R. Boucot, M.D.
Chief Editor
Archives of Environmental Health
349 Wister Road
Wynnewood, Pa. 19096

Dear Katharine:

I return to you herewith the paper of Dr. H. H. Sandstead and others with a recommendation for its outright rejection. You may wish to soften the flat refusal to accept it, by recommending certain alterations, the nature of which will be dictated by your own views and the necessary amenities, or perhaps, to the extent of your wishes, by my comments below. You (or other reviewers) may not agree with my verdict. In any case, it seems necessary to go beyond the recommendation for rejection by explaining my reasons.

First, there is a title which starts out with the single term, "Chronic Lead Poisoning." What comes after that is another matter to which I shall refer later. But, as a basis for relating the initial part of the title to the other part, there are said to be nine cases of chronic lead poisoning. I suspected that something was amiss when I saw this term, for there is no agreement at all among physicians as to what is implied by chronicity in lead poisoning. I dislike this indefinite and almost meaningless term intensely, as it is used. Does it mean that the disease is a long time in developing in dependence upon the building up of lead in the body to a toxic level, or is there a syndrome which can properly be identified as a chronic form of the disease? Unfortunately, in my opinion, the present authors take the latter view, without having any understanding of what is necessary to validate it, although the "long time" is a more or less silent but inferred part of it. Thus, the authors say in their introduction - "More frequently their illness (i.e., their chronic lead poisoning, not its sequelae or its ultimate effects due to organic impairment, but its manifestations), is occult (i.e., unlike lead poisoning). Its manifestations may include chronic anemia, hyperuricemia, arthritis, mild azotemia and neuropathy." Of these, anemia may be one of the manifestations of lead poisoning, and certain characteristic neurological lesions (but not a wide variety) may likely be attributed to the effects of lead. When I came to this statement, I went at once to Table 1 to see what were the findings of "chronic lead poisoning" in these nine patients.

One of the patients, the last patient listed in Table 1, contracted lead poisoning, according to the allegation in this manuscript, in industry (type, course and manifestations not given), but for a year and one-half prior to the time of the present findings, he has had no further exposure, and "mirabile dictu" he showed no obvious stigmata of lead poisoning other than (some degree of) "weakness of the right peroneus and the intrinsic muscles of the right hand and foot." The peroneus is not an extensor muscle, and there is nothing specifically indicative of plumbism in the weakness of "intrinsic muscles of the right hand and foot." Moreover, nothing is said to reveal the method of testing the strength

of these muscles, or to indicate the degree of weakness.

The other manifestations of chronic lead poisoning in the group of eight patients (omitting the case above) may be listed as follows: mild hypertension in 1 of 8 patients; hepatomegaly in 4 of 8, suspected of being cirrhosis of their livers (since when has the influence of alcohol upon the production of this lesion been ignored in favor of lead as the etiologic agent?); bromsulphalein retention in 3 of 8; azotemia in 5 of 8; arthritis in 1 of 8 "had had this manifestation" (of what?); anemia in 6 of 8 (what of the anemia of the alcoholic?).

Now I ask you, or any other clinician whom you may choose to consult, can any of these persons be said to be suffering from plumbism? Can this collection of chronic ailments be said, with any assurance, to be definitive effects of "occult" lead poisoning? Can a man who has suffered no exposure to lead for 1½ years be said to have lead poisoning unless he has some credible clinical evidence of illness? Can such a collection of chronic ailments as those listed above be said to be sequelae of lead poisoning in a group of persons who have long been drinking whiskey (how much and how long?) containing lead, in so far as this record is concerned? Even so, sequelae are not likely to be signs of active intoxication. These men answer this whole series of questions - or ignore them - by means of a stupid rule of thumb procedure which states that, when the urinary excretion of lead, in response to a specific regimen of chelation therapy, yields more than a certain quantity of lead in a 24-hour specimen, the patient has an increased body burden of lead and therefore has lead poisoning. What has clinical medicine come to in a university school of medicine when one makes the diagnosis of illness of any type on the basis of the presence of a potentially injurious agent?

When I come to realize what these men are categorizing as chronic lead poisoning, as a foundation for the investigation of the injurious effects induced by lead in the animal organism, I find myself profoundly shocked. And when, on such an utterly illogical basis, they can say that "the findings suggest that lead is the toxic agent responsible for the abnormalities observed," my shock becomes a depression. Something should be done, I think, to shake people of this type out of their intellectual slumber, and to bring them to realize that there are professional responsibilities associated with their expenditure of public funds (note the source of support of this work) wastefully.

The work done on the biochemical part of this paper, so far as I can judge, is interesting in itself, and may well have been performed excellently, but it turns out to have been entirely fruitless because of the falsity of the basic assumption as to the patients. So far as anyone can tell from this paper, the patients might have suited the purpose just about as well if they had been recruited at random from the streets.

I have no doubt that I have gone into unnecessary details in expressing my disapproval of this paper. I react badly to carelessly designed and sloppily conceived experimental work. I've had more than my share of this sort of thing lately, in the form of manuscripts sent to me for consideration and advice. Moreover, I just returned (not prior to the careful examination of this paper, but on this day just before writing this letter) from participation in a "pro" and "con" hearing on fluoridation at the State House in Columbus. I am not feeling particularly

charitable, therefore. I will say, however, that the authors, or the members of this group who are doing the biochemical work, should be encouraged to continue their investigations. They might even be justified in demonstrating, through publication, the potentialities of this type of investigation in a brief paper. It should be suggested to them, however, that further work should be done in relation to patients who have been investigated by appropriate clinical methods which will categorize them adequately in relation to types of organic injury, on the one hand, or in relation to any active toxic or other injurious agent which they undertake to convict.

With kindest regards,

Sincerely,

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

RAK:wp