

October 3, 1942.

Lieutenant Albert E. Abraham,
Medical Corps,
United States Army,
Dale Mabrey Field,
Tallahassee, Florida.

Dear Lt. Abraham:

I have just received a reprint of your article in the May issue of War Medicine, entitled "Clinical and Sub-clinical Lead Intoxication." I was greatly surprised when I read the clinical reports and the laboratory data and when I recognized that the conclusion arrived at had been voiced in large part on the presumptive evidence of lead exposure conveyed by the analytical data. My first impulse was to write the editor of War Medicine and to express some criticism of your article. On second thought, it seemed the better part of courtesy to direct my letter to you in the first instance, since in any event it will be necessary for me to call the attention of the Office of the Quartermaster General as well as some of my colleagues at the Army Medical School, to certain points in the discussion. You may be interested to know that we have studied the lead exposure associated with the operation of the gasoline stove which was used in this cooking. I am assuming that the regular field cooking equipment was available and that this was the cooking equipment referred to in your article. Our observations would indicate that significant lead exposure would not occur except under highly unusual circumstances. It is my opinion that the exposure described in your two cases are quite insignificant and, moreover, that the symptoms complained of were in no wise suggestive of lead intoxication. The laboratory data with the exception of the lead analyses, was not very suggestive, in my opinion, since basophilic stippling of the red cells can be found in moderate numbers in the blood of most persons.

Passing now to the urinary lead findings, the results are such on mere inspection as to indicate their complete lack of any significance. Inasmuch as both in the text and in the table the quantities are written out, I assume that no mistake has been made in recording the results obtained. Otherwise, it might be thought that reference was made here to micrograms of lead rather than milligrams. In any case, they are insignifi-

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cant. If they should be micrograms instead of milligrams, they are not high enough to be significant; if they are milligrams, they are completely beyond the range of possible lead excretion. No such values have ever been seen in human beings in the most acute cases of poisoning that are known. It is doubtful if the urinary lead excretion at its absolute maximum can ever exceed 2 1/2 to 3 1/2 milligrams per liter. Thus, excretion of 5 milligrams in 24 hours would be a phenomenon of very great rarity in the most acute, even fatal types of lead intoxication. These results, therefore, instead of proving lead exposure, have merely proved that the samples analyzed were grossly contaminated with lead from external sources. At least, this is the most logical explanation, since samples of urine will always be contaminated, unless they are collected with extreme precautions. The only other explanation is that the analyst contaminated the samples in his laboratory. Knowing nothing of the analytical facilities at the Fourth Corps Area Laboratory, I am not in position to judge that end of the matter, but in any case, such a result as 133 milligrams of lead per 24 hours, or even 9.1 milligrams per 24 hours is preposterous. I think it is most unfortunate that this article was written, and still more so that it was published, since it is likely to create certain difficulties among the armed forces without any corresponding gain in safety. I should feel that in some way the impression given by the article should be corrected, and I am writing you directly in the hope that you might be disposed to take such action as is necessary.

Cordially yours,

Robert A. Kehoe, M. D.

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