January 13, 1941

Mr. Philip Drinker
Department of Industrial Hygiene
Harvard University
55 Shattuck Street
Boston, Mass.

Dear Mr. Drinker:

I am availing myself of the suggestion in your letter of December 17th, that I write a critical note on Balknap's paper. I am sending you herewith a criticism which is a little longer than I could wish, but which I have not been able to reduce, without being critical and yet failing to give any sound basis for my criticism.

As you will note, I have not minced words in dealing with Belknap, and I have not felt it necessary to be cautious because I am quite sure of my ground. I do not accuse Belknap of intellectual dishonesty in presenting these data, but I do think he has been unnecessarily careless. Not only did I criticize these data at the meeting, but I subsequently talked with Belknap and pointed out to him with reasonable care, the basis of my criticism. Moreover, on several occasions, we have had the opportunity of checking certain analytical results of his on patients, and his results have been excessively high in practically every instance. He has adequate reason, therefore, to suspect these results, and to publish them in this form represents, in my opinion, something of the inaccuracy of approach that often characterizes good clinicians when they say that the results obtained are good enough for clinical and diagnostic purposes. Unfortunately in this instance, the results are not good enough and they are not presented for purely clinical consideration. Without doubt they will produce a good many medico-legal difficulties. This is particularly unfortunate since Belknap is a member of the Committee of the American Public Health Association, of which I am Chairman, and we are working out a

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report at this time in which we are attempting to clarify the situation which has been produced by just this kind of carelessness. Obviously it is not my function to discipline Belknap, but I do not think he should be permitted to weasel out of this situation, and for this reason I have been straight forward and unequivocal in what I have said.

I trust that this will not be either too long or too direct for use in your Journal, for I feel sure that to the extent that the article is reviewed by persons who are in the know, some criticisms will be made which are, perhaps, more pointed than mine.

Sincerely yours,

RAK:is

Robert A. Kehoe, M.D.

KE 0015268

February 6,1941

Mrs. Helen Lawson
The Journal of Industrial Hygiene and
Toxicology
55 Shattuck Street
Boston, Mass.

Dear Mrs. Lawson:

I have read Dr. Belknap's reply to my criticisms of his paper, and I see no advantage in my making further comments. It seems improbable that any useful purpose would be served by further detailed discussion of the relative merits of the various methods of analysis. The method as described by Dr. Belknap is open to serious criticism at a number of points. It is not the best version of the chromate method, and the latter, at its very best, has been shown to be inferior to various modifications of the dithizone method, to mention only one type of recently developed method of high precision. On this account, it is unfortunate that Dr. Belknap has made an irresponsible statement concerning methods, in the last paragraph of his reply. It is quite possible that he is unconvinced in this regard since he has not had the experience which would provide conviction. That, however, is irrelevant, since the published evidence on the subject is quite clear. The trouble with such a statement is that it confuses the issue with respect to the published data, by hiding behind the prestige of a method of analysis. Actually, the data consitute the issue,here. The analytical method is only one item in the methods of procedure, and is of definitely secondary importance. The data are erroneous and misleading, in that they are numerically inadequate to define the picture which is to be portrayed; they are excessively variable among themselves; and the conclusions reached from them are at variance with established physiological facts.

I think, considering the significance of these matters, and the need for clarity rather than confusion, that Mr. Drinker might wish to make some brief editorial comment pointing out the virtually universal acceptance of the superior sensitivity

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and accuracy of more recent methods for the determination of minute quantities of lead in biological material, as compared with any of the chemical separatory methods, including the chromate method. Such comment would remove that obsolete controversy from the field of action in this instance, and would tend to keep the uninformed from embarking upon the use of the chromate method in dealing with blood and urine. Such a suggestion may sound a bit naive to Mr. Drinker, but to me there is a bit of stigma to the Journal attached to its printing of such a statement as Belknap's with respect to methods. Experienced people will smile at the statement, but they will also wonder how it got by an editor as well informed as Mr. Drinker.

Incidentally, I find it impossible to believe that Mr. Fairhall agrees with Dr. Belknap's position, with respect to the data, or in the matter of analytical methods. If I am wrong in this I should greatly appreciate being so informed, for that would be a significant item of information.

Referring to Mr. Drinker's letter on my criticism, I certainly have no objection to the deletion of the word "gross" from the sentence indicated. It was not intended to be objectionable but rather to indicate extensive rather than minor inaccuracies in the measurement or interpretation of exposure. A very large error would be required to produce the results indicated by the analytical data on the urine. I explain this not as an objection to the deletion, but rather to indicate that I do not go out of my way to criticize or offend any of my colleagues.

Sincerely yours,

RAK:ls

Robert A. Kehoe, M.D.

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