November 15, 1941

Dr. Morris Fishbein
Journal of American Medical Association
535 North Dearborn Street
Chicago, Illinois

Dear Doctor Fishbein:

I am writing you about the paper "Tetraethyl Lead Poisoning, with Report of Cases" by Philip Work and Ragnar J. Ness, Denver, Colorado that you accepted for publication recently, and am communicating with you at the suggestion of Dr. J. F. Hammond, with whom I discussed this matter last week in your absence from Chicago.

My reasons for commenting upon this paper are three-fold:

(1) There is a certain amount of misinformation in the paper that should be corrected;

(2) Certain data of mine and Dr. Kehoe's were used without our consent or acknowledgment and more importantly, without our comment upon them and our interpretation of their meaning;

(3) The inferences that were drawn from our data are, I believe, erroneous and since our own data was misused in this fashion, I feel that the matter definitely calls for some comment and action.

For your information, I will outline the manner in which we became interested in these cases and the way in which the present situation arose.

In February 1941, we were called by Dr. R. J. Ness of Denver, for advice on the treatment of a case of tetraethyl lead intoxication which was presumed to have arisen during the course of operations involving the blending of tetraethyl lead with gasoline. We have had responsibility for the hygienic supervision of men engaged in blending operations and have previously reported (in the Journal of Industrial Hygiene) on the state of health of over 2,000 men so engaged. Since there has never been a case of lead poisoning or even

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a measurable increase in lead excretion in that group of men, we were very much interested in what seemed to be an unusual circumstance and went to Denver for the purpose of assisting with this case and of ascertaining the facts. While there, we learned that this case of poisoning had come about as the result of the operation of a small bootleg manufacturing plant and that a number of other individuals had been exposed during its operation. We arranged to examine the remainder of the employees, which examination I carried out in Dr. Ness' office. To my knowledge, Dr. Work did not see these cases, although he was the neurologic consultant on the original case and assisted Dr. Ness in that patient's management throughout the course of his illness.

Following my return to Cincinnati, Dr. Ness expressed the wish to present these cases to the students at the Medical School and to the County Medical Society. We were heartily in accord with this idea and sent Dr. Ness all of our clinical records and analytical data, together with certain interpretations. Since the case reports and analytical data were our own, we expressed our desire at that time to see and review any material that might be prepared for publication.

After the material had been used for presentation by Dr. Ness, it was some time later turned over to Dr. Work, who prepared the paper that you now have. We did not see this paper, nor to my knowledge did Dr. Ness see it in its finished form until a copy was sent him after it had been accepted for publication. I mention this latter fact to you merely to point out that though Dr. Ness' name is on the paper, what I have to say both above and subsequently ought not be taken as criticism or reflection in any way upon Dr. Ness' position in this matter. He has been and is most cooperative, and in fact placed in our hands his copy of the paper as soon as feasible after receiving it.

You can see that the manner in which this thing developed has created a very unfortunate situation. It is not the matter of credit that concerns us, but I have a very strong reaction against what is obvious misuse of data. There are minor points of error. The principal aberration which may be of significance, particularly from the medicolegal point of view, is the last conclusion in which the statement is made that organic lead intoxication may produce permanent clinical residua. Getting aside for

the moment the origin of data in the paper, I should like to point out that the patient was discharged from the hospital, improved, but not recovered, in April, and that Dr. Work's last examination was in July. Considering the seriousness of the man's illness and the nature of his exposure, it would not be expected that he would make a clinical recovery in that short period of time. By no stretch of reason would one be justified in considering any lesion or functional disturbance that persisted for only four months after a serious illness as being, of necessity, a permanent clinical residuum. Our experience has shown that clinical symptoms may persist until the lead content of blood and excreta have resumed substantially normal levels.

In point of fact, I examined this patient in company with Dr. Hess on Tuesday, November 4, and found no evidence of organic residua or of mental impairment. We are at the present time securing samples from this man and will have further information on the extent of his recovery chemically.

Without regard to any of the foregoing considerations, a further point that must be made, namely, that the exposure of this patient was not due to organic lead compounds alone, but there was in addition, serious and significant lead exposure to inorganic lead compounds in the form of dust and lead fumes. While one could not say that these alone would be adequate to cause an encephalopathy, yet the severe exposure to inorganic lead compounds made this case atypical in its behavior, as was noted on our examination records. It is well established that permanent residua may follow encephalopathy from inorganic lead compounds, but in none of our cases of organic lead intoxication have there been organic sequelae. Since this patient had both types of exposure of considerable magnitude, it is quite apparent that some type of logical tour de force is necessary to permit of the last conclusion.

I have labored these points and gone into the facts with considerable frankness since we feel it is proper to call upon you for advice as to procedure. Were this entire difficulty merely a matter of interpretation of previously published results, it would not be at all difficult to express our opinion in the form of a letter that could appear at the time of publication of the article.

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It is not so simple, however, and I believe you can well realize that the scope of any letter that included all of the facts would of necessity have to involve some statement that would tend to reflect rather seriously upon the authors. I would not like to do that if a better way can be found. It occurs to us that since the paper has not yet been printed, it may be possible to get the opinion of a reviewer personally acquainted with this type of intoxication who could reconsider this paper in light of the comments that we have made.

We would like very much to be apprized of your reaction. It is necessary for me to be in Chicago from time to time and if you or Dr. Hammond feel that anything can be gained by further discussion, I shall make it a point to be there some time when you are available.

Very truly yours,

WM:vr

Willard Machle, M. D.

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