



DEPARTMENT OF ENVIRONMENTAL HEALTH
KETTERING LABORATORY
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3223 Eden Avenue
Cincinnati, Ohio 45267

October 23, 1978

Lawrence E. Blanchard, Jr.
Executive Vice President
Ethyl Corporation
330 South Fourth Street
Richmond, Virginia 23217

Dear Larry:

Having received your letter of October 19th, I must write at once to let you know my situation in relation to the impending publication of Dr. Gross's report of the experimental results obtained by our investigation of the behavior of inorganic lead in the human body during my regime as Director of the Kettering Laboratory in the College of Medicine of the University of Cincinnati.

Incidentally, I am not including Sobel at Baton Rouge or Ter Haar, or Hesselberg in this letter for a number of good reasons (I think). I am not suggesting here that you may not wish to include them in any action which you may wish to take, but am merely following my own devices in that connection in an awkward (to say the least) situation. I shall describe my situation clearly and as honestly, as I know how.

Unfortunately, I allowed my retirement from this Department in the University of Cincinnati to slip up on me, since I was too busy to be concerned with that matter. Also, unfortunately, I was stricken by an utterly atypical cerebral hemorrhage before I could get around to a return to my desk in Kettering Laboratory and this extensive work program was not yet published. (I was going about for a time on some visits and consultations in order to maintain my voluntary professional relationships with a number of industrial organizations, within which I had studied inorganic lead problems. This took time, and in the course of that time, my illness occurred and very nearly did me in. I cannot yet account for this stroke, but I was impaired enough so that I required a little more than three years to show appreciable recovery). You may know that such recovery is unusual, and so I say, on another count, that my "stroke" was atypical and I am most fortunate to have recovered to the extent I have. About 2½ years ago, I recognized that I was coming out of the haze, and I have continued, slowly, to improve. I am now at work, having an office and a secretary, and am engaged in writing a book upon this experimental job, which I, with a number of my staff, carried out. My book, of course, will come out, officially, from this Department of the Medical Center of the University of Cincinnati. Something like another year will be required to do this, for I am slow, and am being very careful not to overdo.

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Now as to the work of Gross. When I returned to work, he, then a member of the Staff of the Department (not appointed by me) had begun an attempt to put together these data. I welcomed his effort, but soon recognized that he had no comprehension of what I had attempted to do. Furthermore, his statistical work, (as such work often does), obscured the facts rather than clarifying them. (Incidentally, the present Director of the Department did not reappoint him in 1977, after the first half of the year, and has told me since that he should have advised me of his intention not to continue him in the Department. An interim Director had been responsible for his appointment in which he had not accomplished anything of significance. After the first half of 1977, I became aware of Dr. Gross's incompetence, and after supporting his work to the tune of some thousands of dollars, I broke the association utterly. By this time Dr. Ter Haar and Bernard Sobel at Baton Rouge (a competent statistician, I believe) had been in contact with Gross and believed that his work was worth completing and publishing (just to get the data out where they could be seen by other investigators). When I broke with Gross, I told Sobel that I would not object to the publication of the data, without any discussion of their meaning and utterly without any practical applications to either general or industrial medicine. I have now the last version of his report (by himself) and I have informed Dr. Sobel that I do not object to its publication with such alterations as he (Sobel) considers appropriate. Incidentally, the use of the English language in the report by Dr. Gross is atrocious and I hope Dr. Sobel will improve it somewhat. However, I have simply washed my hands of any responsibility for his work (as I have told him), considering that it does not reflect on me or this Laboratory, and that I shall make my own report with complete discussions and applications. I agree, of course, that until I can supply a properly interpreted presentation of the data, it is of some use, to have the data presented in total, even in a somewhat unsatisfactory manner, in case I fail to do so myself.

This then is the story up to the present. I am sure that I do not shine out very well in the process, but I have done my best not to be a "dog in the manger", and in the long run, assuming that I shall do my job, nothing will have been lost in the shuffle. I would prefer, of course, that the job be done well at first, but I made a "gross" mistake (no clever indirection intended), and I shall have done my best to atone for the mistake.

I should have informed you of this situation long ago, and for that negligence I beg to apologize. I can only say in extenuation, that I have suffered the agonies of the damned in this situation, and have not known what to do until recently. I hope I am not being either grossly inconsiderate or unduly naive, when I take the attitude that when I have written my report (which is well underway) that of Dr. Gross will be only a sort of poor documentation of it.

I shall appreciate greatly any expression of your opinion on this matter and on my part in it, which you may care to give me.

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With kindest regards and also with my earnest desire to do my best to straighten out this entire problem. I have believed from the start that only by the application of our experimental methods could this be accomplished.

Cordially yours,

A handwritten signature in cursive script, appearing to read "Bob", written in dark ink.

Robert A. Kehoe, M.D.

RAK/di

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