



University of Cincinnati Medical Center

3223 Eden Avenue
Cincinnati, Ohio 45267

ENVIRONMENTAL HEALTH
LABORATORY

January 25, 1979

Arthur M. Diamond
University of Chicago
Department of Economics
1126 East 59th Street
Chicago, Illinois 60637

Dear Dr. Diamond:

I am not quite sure that I know just how to address you, but if I have done it improperly, I am sure you will forgive my mistake. In any case I am glad to reply to your questions as well as I can.

With reference to my investigations on lead, I shall tell you the whole story as well as I can.

In about 1930, (I am a little hazy on dates for a reason which will appear later in this letter) the director of research, Charles Kettering, and the man who extended the research on his ideas, for the General Motors Corporation, came a serious "cropper" in having undertaken to set up an establishment near Dayton, Ohio, for producing a composite mixture of chemicals for the purpose of eliminating the "knock" in gasoline-powered motors caused by the combustion of the motor fuel then available, when compressed. The eventual material which resulted from Mr. Kettering's brilliant experimentation, namely tetraethyl-lead could be removed from the engine (which it tended to clog) by being mixed with certain halogenated hydrocarbons to form what was known as Ethyl Fluid. This mixture introduced into suitable gasoline in small quantities (usually up to 3 c.c. per American gallon) appeared to eliminate almost dramatically the "knock" whereupon the motor operated smoothly and gradually eliminated the lead from the engine in the exhaust gas as lead fluoride or bromide, or chloride or oxide (some) and so permitted continuing smooth operations.

Unfortunately, the inventors of this antiknock mixture had no concepts of toxicity, and not too long after starting up the operating plant to produce these mixtures of tetraethyl-lead with the eliminating compound or compounds, a group of men so employed were poisoned and two of them died. At this point the U.S. Public Health Service intervened and closed the plant, until the matter could be investigated as it was to be, and was done, by the U.S. Public Health Service.

KE 0008753

N9808

Arthur M. Diamond
Page Two
January 25, 1979

At the time of the outbreak of lead poisoning in this plant, Mr. Kettering of G.M., who was a friend of my chief, Martin Fisher, Professor of Physiology in the College of Medicine in which I was Assistant Professor, (in charge of the Laboratory Course), came to him to ask simply and humbly, "what shall we do"? Martin Fisher was a busy man and it is not especially remarkable, that he turned Mr. Kettering over to me. In response to the same question I simply said "I haven't the vaguest idea", but I think I could find out if I had the right kind of Laboratory and the right kind of technical help. Thus came the issue out of which came the Laboratory, named subsequently "Kettering Laboratory" and, subsequently, the Department of the Medical College entitled The Department of Preventive Medicine and Occupational (Environmental) Health. Thus came my familiarity with American industry, which I proceeded at once learning about. This, from my point of view, is of more importance than the environmental problems of the country, especially the urban population of America, simply because it is much more serious. I recognize, of course, the problem of community air pollution, also that of the contamination of water, food, clothing etc., but I am deeply concerned about the thousands and thousands of people who work with dangerous or potentially dangerous materials about which they or we know little or nothing, in the serious process of making a living, and to this phase of American life I expect to devote whatever remains of my scientific life.

And now, after this lengthy introduction, to your problem of Dr. Gilfillan and the decline of the Roman empire. We have studied the problem of lead poisoning and the absorption, distribution and retention of lead in the human body or the opposite side of the latter problem ie. the elimination of lead from the human body. The most important feature of our studies is the outcome of some thirty-five years of the detailed study of the behavior of human subjects to whom lead was administered daily over the period of years; one group, to whom lead was administered by mouth for years, and a second, larger group, to whom lead was administered over considerable periods of time in the air they breathed in a respiratory chamber, on five days of the week. The lead was added to the air in various sizes of the particles and in various concentrations, all carefully controlled. The duration of such exposures per day were also varied, as were the lengths of over-all time for which they were exposed. Virtually every variable of this type was introduced for sufficient periods of time to reveal the response of the subjects to the specific variable. Thus, as I have indicated, some 35 years of time were required to reveal the behavior of lead in terms of intake, output, retention, urinary responses and lead in the blood, under various conditions, employing 4 subjects to whom oral intake was involved and 12 subjects whose exposure was essentially respiratory, as the variable.

This work ended in 1970, but shortly after my actual retirement I was stricken with a cerebral vascular accident, so that I didn't get back to work for some years. Only in the last four years have I returned to such condition that I can work intelligently on the preparation of this work for publication. Some time will be required yet to complete the job which will be published in book form. Perhaps a year hence will be required to achieve this. Through this and our entire work on the behavior of lead in the community, in industry, and in this Laboratory, I have learned to know that little fragments of work here and there

KE 0008754

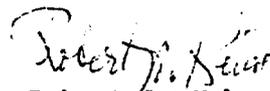
Arthur M. Diamond
Page Three
January 25, 1979

cannot properly be used to interpret the experiences of long ago. Thus, I can say, flatly that the theory of Gilfillan with respect to the Romans, based on fragments of information gained by chance here and there may be a fascinating experience, but is worth little more than the fairy tales we recite to our children or to the otherwise unwary listener. I say this in all kindness, generosity and respect for this nice man. The idea is of interest, without doubt, but the evidence as to its truth is too vague to be considered at all by a scientist. It may be, could be, true, but there is no means of separating fact from fancy. The lead content of an occasional body or fragment of a body long buried and subject to disintegration or contamination or elemental contact with earth, water, or whatever in an environment which has been subjected, in various places, to fire, flood, rainfall, earthquake, volcano, cultivation or manipulation, will, perhaps, have been subjected to one or many variables to such an extent as not even to be imaginable, much less to be categorized. The evidence is unassailable that lead is ubiquitous but unpredictable in quantity, excepting for certain simple situations. It exists in all parts of the earth, in all forms of living materials, in all mankind from the womb to the tomb in such variable quantities as often, if not usually, unpredictable. Considering that the life of man would seem to be more variable now than at any other age, it is nothing short of remarkable that people in the general population of America are only rarely affected (or so it seems) by the lead that is in their bodies, it seems doubtful that the case has ever been otherwise in the history of mankind. I therefore, am highly skeptical as to the alleged effect upon the Romans, but quite apart from my skepticism in this matter, the fact remains, I believe, that one can never know whether or not there is a shred of truth in this idea. I would not, in my present state of mind, waste a precious moment of the time remaining to me on this earth in pondering over this subject

Incidentally, with reference to any references in the literature to the investigation described above, I have made progress reports, fragmentary to be sure, when opportunities were afforded to me. I was not aware of this fact at all when I returned to Kettering Laboratory after my prolonged illness, and it was a pleasant surprise to me to encounter them. I give you herewith a few references to these. Unfortunately, they are not very enlightening. The best, perhaps, are given in the first, The Harben Lectures.

I apologize for the lengthy letter but I seem to find it necessary to explain myself to someone, in this case, yourself.

Cordially,



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

RAK/di

Enclosure

KE 0008755