

January 20, 1969

Professor Ronald E. Lane, C.B.E.
3 Daylesford Road
Cheadle, Cheshire
England

Dear Ron:

Your letter (and enclosures) of January 6 provides the proof that not all of the odd-balls among the professors in the physical sciences are confined to the United States of America. It was hardly to be supposed that they were, but the evidence is not always so clear as it is in the present instance. How it is that certain chemists and physicists have so suddenly acquired an authoritative knowledge of toxicology and clinical medicine is not apparent. Seemingly, although a comprehensive knowledge of chemistry involves a lifetime of study and first-hand experience, these lesser fields of biology and medicine can be picked up more or less incidentally as one follows his main line of endeavor. Even so, it is a bit discouraging to find that even the professors do not read intelligently so as to grasp the explicitly expressed limitations of a writing which they set about to criticize.

I shall indeed comment on the letter in some detail without limiting myself to the "Patterson Section and the Three Cities Survey." You are at complete liberty to make such use of what I say, or to modify it to suit your own purposes and style, as you may choose. I shall not write a separate letter to the editor, unless you specifically wish me to do so. I don't like to stir up a conflagration over our straightforward attempt to clarify certain specific issues.

Paragraph 1. This paragraph, coming from a chemist, is a strange example of willingness to patronize his betters, while taking them to task in an authoritarian manner.

Paragraph 2. Since children were excluded from consideration in the statement, this paragraph is irrelevant. Even so, it contains so many "statements of fact" that are either untrue or uncertain, as to require serious criticism. The "increased sensitivity" of children is an old shibboleth for which there is some historical and experimental evidence which, however, requires critical interpretation. It is known, says the oracle, that children handle inorganic lead differently from adults." The evidence grows that this is not true, since it has come to be feasible to deal quantitatively with such matters as dosage, absorption, distribution in the body, excretion and ultimate fate - and with the relations of these physiological factors to each other. Further, the situation in the infant is similar to that of the adult, in relation to the concentration of lead in the tissues and its effect upon the seriousness of the illness and the outcome (prognosis). Several of us in the U.S.A. have had a wider experience with the concentration of lead in the blood that is associated with the onset of toxic symptoms than have any of the Europeans. As you know, I do not mean the concentration that is found in relation to persistent symptoms, but rather to that present when the first symptoms

of illness appeared (exclusive of the level of ALA, so far as I am concerned, for I have not had long enough experience with the latter to know the full range of the possibilities).⁷ But, I repeat what I have said elsewhere, I've had the opportunity of following the course of childhood lead poisoning, by means of information transmitted by the clinicians in consulting with us as to our analytical findings and their meaning, in a city and group of hospitals that have had a lot of it, under good clinical study, for the past 40 years. Since the days of precise analytical data relating to the blood, as well as urine and tissues (beginning in 1937), I have had direct information about a large series of cases. Not once have we found less than 0.08 ± 0.01 micrograms of lead per 100 grams of whole blood of a child whose illness came to the attention of the clinician in the Children's Hospital and in the Pediatric Department of the Cincinnati General Hospital in prompt relation to its onset. There have been scattered reports of lower values, down to 0.04 ug/100 g. from other hospitals and clinics here and abroad, in which the necessities of analytical precision were not understood by the clinicians, and in which no mention was made of the duration of illness prior to analytical determinations. In fact, the authors of these articles have only rarely understood that they were stating a most astonishingly unusual fact by proclaiming that the level of 0.04, 0.05, or 0.06 ug/100 grams of whole blood had induced intoxication. The ignorant assumptions that have been involved in this medical area have been among the commonest of its phenomena, as expressed in the literature. (One despairs of developing in physicians, generally, an appropriate and necessary technical critique, on the one hand, or the recognition that there are matters beyond their comprehension, on the other. Chemists, too, should have had scientific training capable of arming them against such mistakes of judgment, but only occasionally has the discipline been effectual.)

I shall not comment on the third paragraph, since the folly of the author in challenging experts in their own field should, it seems, be apparent to anyone.

Paragraph 4. It is important to recognize that the "Three Cities Survey" provided a highly satisfactory confirmation of data collected and conclusions drawn by members of the Staff of the Kettering Laboratory (and its predecessor in the Department of Physiology of the College of Medicine of the University of Cincinnati), from 1925 on. These data have served to establish facts concerning lead in the environment of modern man, and about lead poisoning, as well as extensive information concerning the behavior of lead in the human organism. These facts have altered many of the concepts of a previous period (before methods of known accuracy and, later, of high sensitivity and reproducibility had become available). Many of the results obtained in these investigations, and in those of other contemporary investigators in many countries, as well as the conclusions drawn from them, have been accepted widely, but they had not been confirmed by comparable investigations in other hands. The confirmation provided by the joint technical efforts and the joint sponsorship of governmental, industrial, and academic experts, should have exerted a salutary effect in allaying public concern. It would have done so under ordinary circumstances, but their effect was offset, to some extent, by the publication (in the Archives of Environmental Health) of an article by C. C. Patterson, of the California Institute of Technology, proclaiming that the lead in the air (of American cities and towns) constituted a serious "lead insult" to the population subjected to exposure to the exhausts of automobiles using gasoline containing organo-lead compounds as fuel. Further articles, lay and scientific, and discussions and controversies arising out of Patterson's thesis, on the

background of a generally developing apprehension as to the effects on human health of the pollution of the atmosphere with a variety of contaminants (especially in congested urban centers), were instrumental in arousing considerable technical and public concern about the lead in the air.

For your private ear, there were certain serious defects in the report of the "Three Cities Survey," which I (a member of the working group concerned) could not eliminate without producing a real convulsion of feeling within the group. For example, a list of various classes and groups of subjects investigated during that period and previously, was set up opposite the average or mean concentration of lead in their blood, ranging increasingly downward from some rural groups to persons subjected to occupational exposure to lead, as evidence of the human response to increasing concentrations of lead in the air. This thesis was advanced as fact, despite the lack of any information concerning the alimentary intake and output of lead on the part of the individuals or the groups, or concerning other types of exposure beyond that in the ambient air. I simply could not convince the governmental people that such variables had to be taken into account before drawing any such conclusion. I might have been able, by sheer truculence, to have had this table removed from the report, but the effects within the group would have been most unfortunate, so that I simply stated the case and let the simple, erroneous presumption prevail.

There were some other faults in the report, chiefly due to soft-pedaling. It is a prime example of what a committee consensus can do to obscure the outcome of an investigation, certainly not by intention, but significantly. But as you will appreciate, I am duly grateful for confirmation of our data and methods, and I should not be surprised, after all of these years, by certain timid and oblique interpretations.

Paragraph 5. With reference to the organo-lead compounds in gasoline, there has been a serious misunderstanding since the early days of 1925, as to the effects of exposure to gasoline containing tetraethyllead (and, more recently in relation to tetramethyllead, as well). Almost no one who has not had direct experience with the problems seems to be able to understand why it is that gasoline containing tetraethyllead is a serious problem when the air comes to be laden with the vapor of leaded gasoline under conditions that result in complete vaporization of both the gasoline base and the tetraethyllead mixed therein. This occurs in connection with tank-cleaning, with the use of sprays of leaded gasoline in an enclosed space, with certain situations that may develop in using leaded gasoline as a degreasing or drycleaning agent. Moreover, laymen and inexperienced technical men assume that a considerable quantity of vaporized tetraethyllead or tetramethyllead (or both) occurs in the atmosphere in appreciable concentration, instead of being burned (and decomposed) to near completion in the engine. It is another example of people talking glibly about things they know nothing about, and quoting others who have made statements, some in writing, about matters which they do not understand.

I had not intended to be so lengthy in this matter, but I have been sorely irritated for a long time about letters and statements and publications of the instant type, and perhaps I may be forgiven for letting off a bit of steam in your understanding

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presence.

With best regards,

Sincerely,

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

RAK:wp

P.S. A bit of unavoidable delay in my reply to your letter has been occasioned by my being out of Cincinnati at the time it arrived here. Lucile and I have been in Florida during the intense cold weather of the past several weeks, so that my secretary had to forward your letter to me for a reply in longhand to be typed and forwarded to you.

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