

June 13, 1969

Mr. Edward Gross
Science Service, Inc.
1719 N Street, N.W.
Washington, D.C. 20036

Dear Mr. Gross:

I appreciate very much the manner in which you received the comments and implied advice in my recent letter.

Let me say in reply to your argument for the public discussion of controversial subjects that I do not disagree. I believe fully in the dissemination of information on any and all matters that are of concern to the public, and I consider the sound judgment of well informed laymen to have more weight than that of the involved scientist. What I must inveigh against is misinformation and misplaced emphasis on the several aspects of a controversy.

Let me illustrate the point. In preparation for the presentation of a paper on a "Toxicological Appraisal of Lead" (in the air) I, who have worked in this field for many years, and am fairly well informed as to the details, as well as the major issues, spent almost three months in reviewing the published literature on the subject during the past ten years (or thereabouts), making copious notes for my own purposes, before writing a line, I could not afford to omit anything of real significance that might well have escaped me while engaged in my own work. This is what is required to review a subject for the benefit of one's colleagues, many of whom are unfamiliar with the physiological facts and factors which enter into the consideration of such a matter.

The professional writer, in his relation to the public and its interest, is responsible for carrying out a sufficiently searching examination of the facts and of the personalities concerned in a significant public issue to (1) appreciate fully his own limitations in the appraisal of the information; (2) to be able to avoid misstatements of fact; and to clarify the issues whether conclusions concerning them can be arrived at, or must be postponed. This, I believe, is good reporting. I do not agree that anything less than this is worthy of the attention of the lay reader. I am also reasonably confident that the truth in these matters will come out, if we (our society, its legislators, etc.) do not rush in to do something, before we learn the truth.

I'll be interested in seeing your article, and I will comment on it, so far as I can, for your benefit.

Sincerely yours,

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

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SCIENCE SERVICE INC

THE INSTITUTION FOR THE POPULARIZATION OF SCIENCE (organized 1921 as a non-profit corporation with trustees nominated by the National Academy of Sciences, the National Research Council, and the American Association for the Advancement of Science) has as its purpose to educate and the Journalists Professor E. D. HARRISBURG, LEAD RECTOR

1719 N Street, N.W. WASHINGTON, D. C. 20036

Phone: (202) 331-1000 Area Code 202 Code Address 20036

June 9, 1969

Dr. Robert A. Kehoe
The Kettering Laboratory
College of Medicine
University of Cincinnati
Cincinnati, Ohio

Dear Dr. Kehoe:

First off let me thank you for your letter. I greatly appreciate the pains you took to assist me in the article and was surprised and and flattered that you would devote so much time and effort to helping me. I was even tempted to put your by-line on the letter and run it as the article. I still won't compare your word count and mine.

BUT (there is always a but in life), I do not agree with you that the best way to handle a controversial subject is to say nothing about it—even if that subject perforce must be treated superficially. I believe that the public and the scientific community have the right to be informed about the issue. Not to inform them about its existence would be unfair. You seem to disparage lay writers discussing scientific matters. All I can say to that is that if scientists would communicate to the laymen on their terms then lay writers wouldn't have to. If these arguments fail to convince you about the righteousness of reporting the lead controversy, then I must fall back on the words of Socrates, who said 2,400 years ago: "I gotta eat too, you know."

At any rate, I have studiously considered your comments and incorporated them into the article where I saw fit. I have also rechecked some of the things I have said with others in the field and acted accordingly. I have tried to be objective and fair as I could. I wish that I could comply with your desire not to be quoted, but to omit you would be like writing a book about the history of the American Revolution—the one in 1776—and not quote Patrick Henry, John Paul Jones & Company. I did drop one quote, however.

Again, I sincerely want to thank you for all your trouble, and if the office follows its usual procedure and you receive a copy of the article, I await your letter to the editor with bared chest.

Thank you.

Sincerely,

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May 28, 1969

Mr. Edward Gross
Science Service Inc.
1719 N Street, N.W.
Washington, D.C. 20036

Dear Mr. Gross:

Let me begin this letter by saying that in my opinion this article should not be published. It is, of necessity, and through no serious fault of yours, a deplorably superficial coverage of the subject, which cannot contribute usefully to the sound judgment of the public or to that of the scientists of the country, to whom, presumably, it is mainly directed. In fact, as I said to you on the telephone, the continued discussion of this subject in the press and other lay publications, can only add to the confusion which lately has surrounded this matter, and contributed to the apprehension of many persons.

I do not suppose for a moment, however, that you or other writers would be dissuaded from writing about this subject, and therefore, I will do what I promised to do. I do urge you, however, not to quote me or other persons in your article. Such statements as you have quoted from Dr. Patterson and myself, cannot possibly carry any weight with thoughtful people who do not know either of us, personally, or professionally, and therefore are irrelevant to the issues, while contributing some spurious dramatic character to the writing.

I started to write into the manuscript some modifications of your language. However, I gave this up after the first page, because mere editorial changes here and there could not alter the incorrect slant of the statements. Instead, I have commented, at some length, in a series of notations, separate from your manuscript. Your acceptance of my views will take the zip out of your article, to be sure, but then there is no zip to this subject in its present state. The issue here is not one of polemics between individuals of different experience and different views. It is rather the unexciting collection of more evidence over such a period of time as may be required. It is possible that the concentration of lead in the common environment of man under a variety of influences, may reach such a level as to be threatening. Methods are available for establishing the facts in this regard well in advance of any harmful effects, and there should be no reason for undue concern for the future. In saying this I do not minimize the importance of a careful professional surveillance of this and many another potential hazard to the public health as these are induced inevitably by the tremendous technological developments of our time. But I am sure that this cannot be done effectively if we are to be badgered continuously by every lay writer who has a yen to tell a story. What we need in this country in relation to our multitude of hygienic and social problems - not all of which are merely potential threats - is a great deal more of hard work and systematic observation, and more money (much of which at present is going down the military drain) for the support of such work, and a great deal less of talk and other forms of sound and fury.

Incidentally, your left handed comment on the presumably biased source of some of the funds employed by the staff of the Kettering Laboratory has in itself a wholly misleading significance. When in 1930, the Kettering Laboratory began its work of

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research, advisory service to industry, and, a little later, its graduate training of professional personnel in the field of industrial (occupational) medicine and hygiene, there was no source of funds other than those which were supplied by industrial organizations whose interest in having an unbiased source of information and advice to which to apply, at the actual cost to themselves of the services which could be provided for them in a public educational institution under the policies set by its Board of Directors. The lead problems of industry, in relation to their own employees and also to the public, were subsidized by a group of industrial organizations involving the lead trades. The problems associated with the production and use of fluorides, including the aluminum manufacturers, were supported by this group, and so on until there were scores of organizations who participated in the support of the Kettering Laboratory (and in its various educational activities, under agreements made between their officials and the Board of Directors of the University of Cincinnati). Under these policies, the work of the Laboratory (including the portion of it which I directed in person) became known in this country and abroad. Do you not agree that your fragmentary remark is more than a little misleading? I recognize that you cannot record the history of this Laboratory and its policies in your paper, but need you question its integrity, as indeed you have done, if only inadvertently. This would be appropriate to a scandal sheet, but hardly to Science Service.

I trust you will not misinterpret my comments as being hostile. They are not so intended. As an elder scientific investigator and a physician long concerned with the public welfare, I am merely suggesting that it is high time that we, as members of a community, should take a serious and responsible view of our primary functions of leadership and guidance.

Sincerely yours,

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

RAK:wp

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SCIENCE SERVICE INC.

THE INSTITUTION FOR THE POPULARIZATION OF SCIENCE organized in 1921 as a non-profit corporation with trustees nominated by the National Academy of Sciences, the National Research Council, the American Association for the Advancement of Science, the E. W. Ross Library and the Chemical Profession. E. G. DEMPSEY, JR., DIRECTOR

1719 N. Street, N.W. WASHINGTON, D. C. 20036

Telephone: 371-7551 (Area Code 202) Cable Address: Sciencinc

May 26, 1969

Dr. Robert A. Kehoe
Kettering Laboratory
Cincinnati, Ohio

Dear Dr. Kehoe:

As discussed in our phone conversation on Friday, here is a copy of my proposed article on lead pollution. Please feel free to correct any factual error, and I welcome any comments on any interpretations I have made.

Thank you for your assistance.

Yours truly,

Edward Gross

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Page 1:

The significance of the "threshold level" of lead in the blood is not as you and many others have thought, who have not examined the background of this standard in its relation to occupational lead poisoning. This means simply that if persons who are subjected to exposure to lead at their work can be protected to a degree which will not permit the concentration of lead in their blood to reach 0.08 mg. (80 micrograms) per 100 grams of whole blood, cases of lead poisoning will not occur. This does not mean that all persons who reach this level will become ill; on the contrary, very few of them may be expected to do so. Most cases of lead poisoning in industry at the time of the onset of illness have higher levels than this. It is the level at which the danger exists, so that if one wishes to prevent all cases, the individuals who approach this level should be removed from work involving exposure to lead. I am the person who first established this fact, from a large number of observations over a long period of years, and promulgated this standard. It is not to be supposed that this standard would suffice in relation to general environmental conditions, for several physiological and practical reasons. It is unusual to find an individual in the general population with half of that concentration, whereas only a small proportion yield results higher than 0.03 mg. (30 micrograms) per 100 grams. Here again, Dr. Patterson and many others, even some physicians who should know better, have failed to understand this criterion of preventive medicine in industry. Moreover, the diagnostic significance of the concentration of lead in the blood, which is that of interpreting the potential significance of the exposure to lead, rather than the presence or absence of illness, has not been understood by many of those who have presumed to talk about it.

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Bottom of Page 1 and going on to Page 2:

You make use of the term "subclinical" in relation to "levels," whereas it applies to effects, meaning that there are (or may be) effects which we cannot detect by clinical means. This is a vile word to use in this context, since, no matter what else is said it means "beyond detection." In short, it can be imagined, without a trace of evidence and despite our clinical familiarity with lead poisoning for centuries, that minute quantities of lead may cause effects of which we have no knowledge at all. This was Dr. Patterson's invention when he called this "lead insult." Not being a physician, and having no concept of toxicology, he made a naive suggestion which has been taken seriously. Remote effects such as premature ageing and shortening of life have been looked for in industry by a few physicians and have not been found.

You say there have been "indications of damage to red blood cells at subclinical levels" (using this term incorrectly again). I can't imagine what you mean, for there have been no suggestions of damage to the blood, even to the chemical features of the formation of blood, of persons whose exposure to lead is confined to that in the common environment. This should be deleted as wholly erroneous.

The second paragraph on this page should be deleted entirely. It is not appropriate to quote me, out of context, and this cannot fail to give the wrong impression of me, of the policies and scientific position of the Kettering Laboratory, and of the matter of which you are speaking.

The remainder of discussion on page two, including the further quotation (which is not incorrect), will contribute nothing to the understanding of the layman, be he scientist or intelligent citizen. for it has nothing to do with the problem of lead in the ambient atmosphere but relates only to the more severe forms of occupational exposure to lead.

Your reference to "opponents" and their views about man's "trouble with his present body burden," as well as the statement that "a severe injury or disease could release the lead in the body" serves, of course, to put the burden on other people, but the fact remains that this paragraph speaks of a possibility which was once believed to exist but is now discredited by all knowledgeable people. I would not advise you to make use of it, lest you be considered to be a deliberate alarmist. This would not contribute to your reliability as a writer.

In the second paragraph, you are, of course, at liberty to quote Dr. Patterson, but I think you should know that he is a chemist - a geochemist, not a biochemist - and that there is no foundation, beyond inference and speculation, for his belief in the scantiness of the body burden of lead of primitive man, or for the minuteness of the purely natural lead content of vegetation and animal life. On the contrary, there are data to indicate that his estimates are of a totally incorrect order of magnitude. But even if what he has said is true, it is not relevant to the present situation. Incontrovertable evidence exists to demonstrate that man has taken appreciable quantities of lead into his body from his general environment - food and beverages, and also some quantity, unknown prior to the middle nineteen thirties, in finely divided form in the air. The data have not been sufficient in number or accuracy, prior to 1936 or thereabouts, to reveal the quantities of lead in the general environment, but from indirect evidence, it cannot be doubted that they have been of the present, or near to the present, order of magnitude for a long time. They have not changed materially for thirty years, and yet cases of lead poisoning have been occurring in our population or that of other nations only when extra and highly significant sources of exposure to lead have cropped up in industry, or^{when} some highly irregular practice has been indulged in so as to contaminate food or beverages or otherwise satisfactory surroundings.

Page 4

I believe you should not attempt to discuss the shift in the way lead is used in relation to lead poisoning in childhood. Actually, the changed usage resulted from technological rather than hygienic reasons. Newer types of paint for internal decorative purposes came into use before the occurrence of lead poisoning in childhood became widely known, and because they were cheaper. The pattern of usage is much more complex than you have any idea of, so that your comments are irrelevant. The problem of lead poisoning in childhood is with us now in highly significant size and scope. The present limited use of lead containing paints in the interior of homes and other buildings does little to solve the problem of children in the poorer areas of our cities where the results of interior painting years ago persist and will continue to do so for a long time to come, unless we release large - very large - sums of money for the elimination of old housing and for the substitution of new. The uses of lead in modern times runs through a large series of industries (several hundred), and these are being changed almost daily. The data on quantities in relation to usage are available, from year to year, but evidently you are not familiar with them. In discussing the usage of lead, you use the terms "primary" and "secondary" which are very misleading, since "primary" usage refers to newly produced lead from ore, while "secondary" means reclaimed lead. It is true, however, that in the somewhat artificial list of the major classifications according to the usage of lead, that employed in the production of lead antiknock compounds ranks second in the U.S.A.

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Your comment on the Three-Cities Survey of 1962 (which also contained Kettering Laboratory analytical data for 1955-56) is not quite realistic. This investigation and publication was important in a variety of ways, but its principal value was the confirmation of data obtained in previous years by the Kettering Laboratory, mainly in Cincinnati and its environs, but also in Los Angeles and

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Pasadena. This survey, in which the Laboratory participated, extended the information more widely and involved larger numbers of people. Incidentally, although the difference was not very great or important, the average annual concentration of lead in the atmosphere of Cincinnati was lower than that in Philadelphia.

One other thing you should know in this connection is that the 11 relatively high values of lead in the blood of the subjects escaped my notice as a matter of any consequence until something was made of them in relation to the percentage of their occurrence in the entire group. The fact is that in every large series of analyses an occasional high result occurs out on the end of the statistical array, as a matter of the extreme of analytical deviation. It is necessary to check such results, and in this Laboratory this is done automatically, in recognition of this occasional problem. The methods of analysis here are highly sensitive, but the quantities of lead to be dealt with are minute. On checking these seeming aberrations, it turned out that these results had been in error. It was important that this be done for the reason that one of the men, from an aviation industry on the West Coast, had been reported as having 0.07 mg. of lead per 100 grams/ ^{of blood.} This, if it had been correct, would have indicated that this man had been subjected to exposure to lead very near to the dangerous level, so that the checking in his case was a stern necessity. However, the potential hazard vanished, with the finding of a normal level in his blood.

One other comment on the material on page 5 is justified. If you will examine the data in the report of the survey with respect to the possible effect of smoking on the absorption of lead, you will find some very thin evidence on this matter. The difference between smokers and non-smokers in these subjects is so slight as to be dubious, and moreover it does not always favor the non-smoker. There are other data and other reasons for the belief that the absorption of lead from tobacco smoke, even when the latter is inhaled regularly, is of

negligible proportions. If you are a smoker, this is one of the hazards of which you need have no fear.

The lower level of the concentration of lead in the blood of members of the rural population is a matter, at present, for conjecture. Perhaps the air is a factor, but it is of some importance, in this connection, to consider the certain other factors, as the diet and also the feminine representation in the population under investigation. Women, as a rule, have lower concentrations of lead in urine, blood and tissues. Why? We are not sure. It can hardly be the air. Thus you see, it is easy to be beguiled by incomplete data into drawing conclusions which are not wholly justified. I've been working on this matter for about forty years, and I am cautious, because I have been tempted to draw certain conclusions which, at this time, I can rejoice at not having put on the record.

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You are most incautious in stating that 30 to 50 percent "of the lead in body tissue(s) and fluids" derives from leaded gasoline. This indeed is a wild statement for which there is no valid evidence. Moreover, your later statement, that the lead in food and water comes by way of "such things as solder in cans, from pipes and paint in water systems, and from insecticides,"/ The data obtained by the analyses of food products obtained at their sources, vegetation from remote lands and primitive areas, and animal life direct from primitive and rural areas and from the seas, tell a totally different story. Our investigations in primitive areas in Mexico demonstrated that at least a third and almost certainly a half of the lead in food and American food products could be accounted for by the natural lead in the soil, water, vegetation and animal life. If this was the result of industrial contamination of the air which swept over this area, it happened before the use of leaded gasoline had come about. Thus, you should not give statement to

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bald facts which are either certainly untrue or are in grave doubt.

The same is true of the statement made about the fate of ingested and inhaled lead. The physiological facts are not in accord with your statement. I realize that your statement differs little from some of those in the literature which have been made by people, Patterson for one, who have no knowledge of physiology. This is one of the difficulties of keeping the facts straight. However, the extent of the absorption of lead from the alimentary tract varies with a number of factors, and it is misleading to make such flat statements.

In the case of the absorption of lead from the respiratory tract, your statement is physiologically absurd. The size of the particles inhaled determines where they go in this elaborate system of tubes and air sacs. Their composition (chemically) determines whether they are readily or difficultly absorbed, and above all, the part of the tract in which they are deposited is highly important. If their size is such that most of them are carried all the way into the depths (into the terminal air sacs), and if they are of a chemical composition as to be readily absorbed, then the proportional absorption of that inhaled would be considerable, but it would hardly ever reach the figure which you have given. Within 6 to 12 hours after its deposition, an appreciable proportion, under all but unusual conditions, would be moved out of the lungs and upward in the respiratory tubes toward the nasopharynx, from which much of it, probably most of it, would be swallowed. Some of this would be available for absorption from the alimentary tract. I think you will see how futile it is to speak of the absorption of a specific proportion of a somewhat variable concentration of lead from the ambient air. We do not know the facts here, and we shall not know them until the quantitative relationships, under a variety of conditions, have been determined through the use of radioactive compounds of lead in various degrees of subdivision. Only recently have certain radioactive isotopes of lead, which have a very short half-life (hours), come to be

available, and until this came about, no one would consider the use of radioactive compounds of lead for the investigation of the phenomena of pulmonary clearance by such means, because of the hazard of radiation. We shall soon have some of the facts in this regard which can be substituted for the guesses of those who know enough to make reasonable guesses, and for the flat statements of fact, by persons who know no better than to make them.

Page 8

You have missed the point, I believe, as have some others before you, as to the increase in the lead in the body from childhood to adult life. Under ordinary environmental conditions, as the body increases in size and weight, the quantity of lead within it appears to increase appreciably without noteworthy increase in the concentration in the individual tissues. That is to say that the equilibrium between intake and output (there is of course a gross increase in the intake of lead in food, beverages and air, as the size of the body increases) remains substantially unchanged and thus the concentration of lead within the body undergoes little or no change. This, as I have said, appears to be the case, the uncertain factor being that of the quantity of lead absorbed in the respiratory tract. The lead content of the bodies of deceased persons who appear, from their life histories, to have had no exposure to lead beyond that in the common environment, seems to bear out this concept of the situation, but one cannot be entirely certain of the facts until a large volume of representative data have been obtained by acceptable means. I have never been dogmatic about this, although I have been accused of this fault. Anyone who will take the trouble to read what I have said rather than what someone else has quoted me as having said, will learn that I am an investigator, not a propagandist.

Your last sentence, in interpreting the "anti-lead people" is pertinent. It is certainly not wise, nor would it be excusable, to wait until untoward effects show up. The fact is that there is no necessity for such a consideration. The actual danger from this hazard is not sudden in putting in its appearance, as long

industrial experience has shown. Rather, evidence of a shift in the magnitude of the overall environmental exposure of the population to lead is not difficult to establish. We have been doing this, at a higher level of exposure in industry, for many years. When evidence of danger appears in an industrial population, one can do either one or both of two preventive measures, depending upon the apparent severity of the findings. The extent of the exposure to lead can be reduced summarily by engineering means. If this involves any unduly prolonged period of delay, the operations can be interrupted or the workmen who are in potential danger can be moved to other work involving no, or considerably reduced, exposure to lead. In either of these cases, radical measures are not only unnecessary, but are actually unjustified and unwise. One does all that is necessary, without upsetting the industrial community, in the one instance, or without discommoding the entire population, in the case of the general environmental situation. This is not always possible in either industrial or general community life, because we do not know enough about some of the problems. However, the facts and procedures of preventive medicine are known, with respect to lead, and a large backlog of experience has demonstrated the efficacy of available methods.

Note:

You may wonder why I have gone to such trouble and spent so much of my time in attempting to inform you about these matters. I can only reply that I have been engaged for nearly fifty years in teaching medical students, and for more than twenty in graduate professional education. Here one is prepared to spend whatever time is necessary (or available) in dealing with a single student. The habits of thought and practice in this area of activity are acquired slowly, but they are tenacious. I still believe, naively perhaps, that it is worthwhile to aid a single individual in the consideration and appraisal of evidence. It happens that, in this period of the life of our people, fairly numerous hygienic problems are

putting in their appearance because of the speed and scope of the technologic developments of this modern period. These will increase in number and variety, as well as in severity, as further increases in our population, and varieties of technological activity, come about. We shall have to learn to deal with these rationally and effectively; and it will be extremely difficult to do this unless we are able to grow up into a sensible and responsible people aided by intelligent and responsible sources of information. We are far from either of these achievements, but one may hope and have faith that we can make important strides in both directions.