January 14, 1971

Honorable Allen J. Ellender
Chairman, Subcommittee on
Public Works
Committee on Appropriations
United States Senate

Dear Senator Ellender:

Dr. Seaborg's letter of December 16, 1970 is really full of inconsistencies. On the first page, he stresses the importance of the study of the long term effects of radium in humans and how the U.S.A.E.C. has supported it. We, the original Argonne group, would have to agree with the initial value of the study since we identified, traced, located, and enlisted the cooperation of the radium dial painters in the Illinois area without the support of the Argonne administration (in fact, against the wishes of the administration of the Radiological Physics Division) or of the U.S.A.E.C. The MIT project was quite dormant at this time. After we had located several hundred radium dial painters, we did receive support from the A.E.C.

A few years ago, a special meeting was held in Washington, D.C. to which the U.S.A.E.C. invited a number of outside consultants to review the radium project. It was the announced intention of the U.S.A.E.C. to either drop or at least drastically reduce the support of the radium project since most of the information had been obtained. As a result of this meeting, the New Jersey project was dropped and their patients turned over to Evans. Thus the U.S.A.E.C. does not have a record of really supporting the radium research project. The current great interest in the data on the long term effects of radium in humans arose as a result of the congressional hearings on "Radiation Exposure of Uranium Miners." However, instead of supporting a group that did have excellent rapport with a group of patients who contained only pure radium, they had to consolidate all studies of internal emitters in one "Center" under control of one man. Why? Could it be so that the data would be analysed to yield the desired results?

Let the record show that the support that I received at Loyola was only for half time. Interestingly, the data that the U.S.A.E.C. really needed was the data on infant mortality which MIT evidently had not bothered to collect. Let the

record also show that I sent the U.S.A.E.C. the data on infant mortality immediately after they called to ask if I had such data.

On the third page Dr. Seaborg states, "Only by independent and repetitive study may some differences or scientific opinoin emerge." How can Dr. Seaborg sign his name to such a statement? On one hand they took the New Jersey study away from the people who did that study and turned everything over to Evans, then they took our study away from us and combined everything in one new "Center" supposedly under Rowland but with Evans as the guiding light. Why does not Dr. Seaborg make any mention of the New Jersey Radium Project? Could it be that some one does not want anyone to investigate the conditions under which the State Police of New Jersey locked out of his office and took over all of his material? The facts really do not support the statement that the U.S.A.E.C. supports independent study when it comes to the evaluation of the long term effects of radioactive materials in humans.

Statements made in paragraph 3 of page 2 are both incorrect and perhaps, purposely, misleading. It is stated: "Doses were calculated routinely by the MIT group using a retention function for radium in humans developed by Dr. Norris at ANL in 1955. Dr. Miller's dose calculations for the ANL series were based on two different retention functions, neither of which were the Norris model."

THE TRUE FACTS: The parameters of the Norris power function, $R = 0.54\ t^{-0.52}$, were found by the analysis of the data obtained from the study of a group of mental patients located at the Elgin State Hospital in Illinois. The magnitude of the parameters, i.e., the 0.54 and the 0.52, are very dependent upon the elapsed time between the injection of the radium and the measurement. Norris used the elapsed times published in a paper by Schlundt:

> SCHLUNDT, H., Nerancy, J.T., and Morris, J.P. Detection and estimation of radium in living persons. IV. Retention of soluble radium salts administered intravenously. Am. J. Roentgenol. & Rad. Therapy, 1933, 30, 515-522.

I studied the patient's hospital charts and found that many of the elapsed times published by Schlundt were incorrect. Furthermore, Dr. Finkel and I traced, located, and visited Prof. Morris of the above cited paper and also called most of the M.D.s who gave the radium to the Elgin patients. Thus we now know why the published elapsed times were incorrect.

We reanalysed the data extensively and published the complete results in ANL 7217 and a condensed version in the open literature.

MILLER, C.E. and Finkel, A.J. Radium retention in man after multiple injections: the power function re-evaluated. Am. J. Roentgenol. & Rad. Therapy, 1968, 103, 871-880.

In these publications we demonstrated that the parameters of the Norris power function, $R = 0.54\ t^{-0.52}$, were incorrect, not because of any error on Norris's part but because he was given incorrect elapsed times. Now the real question is: "Why has Evans continued to use the old Norris equation when it has been shown that those parameters were based on incorrect elapsed times?"

During one talk that I gave, and at which Evans took copious notes, I demonstrated that the total accumulated dose calculated from a whole body measurement made, say, 40 years after the radium was ingested was not very dependent upon the parameters of the power function used. This results from the fact that the use of the incorrect power function yields an incorrect value for the amount of radium ingested 40 years before. Then the use of this incorrect amount to calculate the dose over the next 40 years results in about the same total dose regardless of the parameters employed. Consequently, all of their recalculations will never lead to any great changes in the total dose.

Do not be mislead. The one model used in (ANL-7680/ACRH-107) and COO-2088-1 was the Norris equation but with the more correct values of: $R = 0.3\ t^{-0.44}$.

Well, at least they now admit that the calculations only took one man-month and that most of that time was spent putting the information on tape. I assume that since MIT put all of the ANL data on tape, that these tapes -- or copies of them -- are now available to the "Center for Human Radiobiology" and thus they can quickly, say within one week, recalculate the doses with a number of different power functions.

It is utterly pointless to debate their need of a large staff for the "administrative, technical, and professional tasks and skills associated with maintaining productive contacts for examining such a population during life and after death are formidable." All I can say is that, with the help of one secretary, I traced, located, visited, measured, maintained records, analysed data, and kept in contact with all of the patients in the ANL series. See (ANL-7531/ACRH-106). I also carried on other research projects. So many patients have died during the course of the study that I could not now keep busy half time let alone full time. Thus I would like to know what all these people in the various field offices do to keep busy? According to my information, there are three people assigned to the office in St. Mary's Hospital in East Orange, N.J. I hope that information is incorrect. Let the records show that most of the 600 human subjects still alive do not have a measurable amount of radium in their bodies since they painted watches after the practice of tipping the

brush in their mouths was stopped. In addition, probably all of the supposedly 200 unlocated people worked during the second world war under rather good conditions. The data of table #8 of COO-2088-1 demonstrates that there are only 44 patients, many of whom are now deceased, out of the total ANL series who contained sufficient radium to produce even minimal detectable changes in the skeleton while only 27 of these 44 patients exhibited mild, moderate, or advanced skeletal changes. Thus I claim that very few people of any real value to the study really exist. If a large number does exist, I would like to know why MIT did not locate them before? Why is such great emphasis put on the human group now? Could it be that the two large dog projects can not be justified without a large human program?

In reply to the two criticisms on Page 3:

(1) The parameters used for the dose calculations were clearly referenced to Evans and it is true that Evans's values were given for a standard 70 kg man assumed to contain a 7 kg skeleton. The use of a 5 kg skeleton based on the standard 50 kg woman would have yielded values 7/5 times larger than those given. If we are going to quibble about this 7/5 factor, then let us face the fact that these skeletal weights for standard man or standard women are based on the assumption that the skeletal weight is about 10 per cent of the total body weight. Since the women studied at ANL ranged in weight from about 100 pounds (44.4 kg) to over 250 pounds (111 kg), the weights of their skeletons probably ranged from 4.5 kg (or less) to perhaps 10 kg (or more). I am aware of the argument that the weight of the skeleton does not increase as rapidly as the total body weight (fat). Consequently, the use of an average skeletal weight for these calculations simply introduces another uncertainty into the dose calculations.

(2) It is true that the dose from RaD---RaG was omitted. A sample dose calculation was purposely included in both (ANL-7680/ACRH-107) and COO-2088-1 so that any interested person could check the calculated doses. These dose calculations do not imply that this component is included. Since this point has been brought up, I will ask whoever answers for the U.S.A.E.C. just how much this RaD---RaG component will add to the total doses given in (ANL-7680/ACRH-107) or COO-2088-1. I know since I checked before I omitted it for rather valid reasons.

In reply to the personalized attack, (top of page 4) during the more than 18 years spent at Argonne, I often heard the following definition for research. "Research---Organized play for adults." In other words, research is not work but fun. Consequently, the particular phrase that someone "rose to" had been carefully choosen to say that they were having fun. I was careful to not question the biological justification for the use of such equations.

Returning to paragraph 2 of page 2 of Dr. Seaborg's letter, mention is made of (ANL 7280/ACHH 107) but no reference is made to COO-2088-1 also completed under this grant and submitted at the same time. Undoubtedly the discussion that begins on the bottom of page 33 and continues through page 36 is very distasteful since it details the reasons that a large uncertainty will always have to be assigned to any dose calculation. The additional uncertainty introduced by the use of an average skeletal weight was not discussed since this variability can be reduced by using 10% of the total body weight as the skeletal weight. This variability is insignificant compared to that introduced by the gross mal-distribution of radium throughout the skeleton. Incidently, I must be interested in obtaining constructive criticisms of COO-2088-1 since I have sent copies to recognized authorities in the field and asked them to please "tear it apart" and give me their true opinions.

The two criticisms contained in Dr. Seaborg's letter are really very interesting in that they both relate to adjustments that would increase the dose that each patient received by just a small amount. Evans has always had a closed mind with regard to the possibility that some patient who contained less than 1 micro-curie of radium might develop a tumor. This is demonstrated by the fact that he always questioned (in MIT progress reports) the Argonne diagnosis everytime we found a patient that contained less than one micro-curie who had a tumor.

Doctor Seaborg's letter does not really answer a thing. He does not justify the need of a new building, a large staff, or the consolidation of the projects into one group. He refers to a large number of patients who either do not exist or, if they do, include those who worked during world war II under such good conditions that they do not contain a detectable amount of radium. He states, "only by independent and repetitive study ....." and then consolidates to stifle.

Why is it so important that all data and research on the effects of radioactivity in humans be forced into one center under one man? Is the goal to control all data so that the analyses can be tailored to fit the desired conclusions? Doesn't anyone ever question why such a great number of reputable scientists, ignoring articles in popular magazines such as the December 15, 1970 LOOK, spend so much time and effort writing papers refuting Evans' theory, a theory that is so satisfying to the U.S.A.E.C. and the uranium mining industry. How do you explain the fact that it is not only American Scientist but well known scientists from outside the United States who will not accept Evans's analysis. For example, Hems, Goss, and Loutit of England have each written and published a paper to mention but three. How do you explain the fact that Evans has choosen to ignore the tumors of the central nervous system.

The only solution is to transfer the responsibility for all studies of the effects of radioactive materials in humans from the U.S.A.E.C. to some other agency that does not have a vested interest in the results.

I look forward to Dr. Seaborg answering any of the above questions.

Yours truly,

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