PRESS CONFERENCE

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November 17, 1977

Dr. Sidney Marks and Dr. Ethel Gilbert
Battelle-Northwest

Subject: MANCUSO/MILHAM STUDIES

(TOM BAUMAN) - We appreciate your coming out on such short notice. With all the questions being raised about the Mancuso and Milham studies, we thought it was important to get you all together at one time and answer the questions all at once. The people best qualified to do this locally are Dr. Sid Marks and Dr. Ethel Gilbert from Battelle-Northwest. They have been following this and have been working on the Milham/Mancuso studies for over a year now and are well qualified to answer questions and talk about it. Dr. Marks is Associate Program Manager of the Environmental and Safety Research Program Office at Battelle, and Dr. Gilbert is a Research Statistician. Sid will make an opening statement, and then Ethel will show you some viewgraphs.

(MEDIA REPRESENTATIVE)* - One clarification . . . I don't know . . . maybe he will address it in his opening comments . . . but you say they have been working on the study?

(TOM BAUMAN) - They have been analyzing the study. We will have copies of Sid's remarks as soon as they come out of the typewriter.

(SID MARKS) - The AEC/ERDA Health and Mortality Study was started in 1964 with Dr. Thomas F. Mancuso of the University of Pittsburgh as the principal investigator. The objective of the study was to investigate the health of workers in the nuclear industry, especially with respect to possible effects of radiation exposure on worker health. Mortality was selected as the most feasible measure of health experience, with ascertainment of life or death status to be carried out by the Social Security Administration. The initial plants included in the study were

*Since this material was transcribed from a tape of the press briefing, media representatives present were not always identified by name when asking questions. Wherever possible in this manuscript, their names are provided.

Hanford and Oak Ridge. Data were also collected from uranium feed plants early in the study and from Mound Laboratories more recently. Until the summer of 1976, Dr. Mancuso consistently reported to AEC and ERDA that his results were not suitable for publication.

Meanwhile, in March of 1974, the State of Washington contacted the AEC office in Richland and informed them of a study by Dr. Samuel Milham, Jr., of the Department of Social and Health Services. Milham's study had included analysis of the death certificates of 842 Hanford Project employees as a part of a statewide occupational mortality study. Dr. Milham believed that his analysis indicated a higher incidence of certain types of cancers among former Hanford employees than among state residents as a whole.

Subsequently, Dr. Milham met with Dr. Barkev Sanders, Dr. Mancuso's statistician, in Richland. To assure that Dr. Milham was not in any way discouraged from pursuing his work or publishing the results, AEC officials did not participate in his meeting with Dr. Sanders.

Dr. Milham earlier provided Dr. Mancuso with a copy of his draft paper.

In March of 1975, Dr. Mancuso was given advance notice of the termination of his ERDA contract, to be effective July 31, 1977, because of unfavorable peer reviews and a need to respond to the Congress and the public on the health of the workers. His reviewers, who were from universities and other agencies of the Government, urged that Mancuso publish any results of his, whether positive or negative.

In the spring of 1976, Dr. Mancuso replaced Dr. Sanders, who had been associated with the study from its inception, with Dr. Alice Stewart, a well-known British epidemiologist, and her statistician, Dr. George Kneale. Until Dr. Mancuso replaced Dr. Sanders, they had consistently found no evidence of radiation effect on the health of the workers, although they always stated that they would reserve judgment until additional time had elapsed and until the study was expanded to include all major plants in the industry. Within months after Dr. Stewart became associated with the study, she presented a paper at the October 1976 Health Physics

Society Symposium in Saratoga Springs, New York, in which she concluded that radiation exposure of the Hanford worker population caused radiation induced deaths for several cancer types. I also spoke at Saratoga Springs and described results of an independent Battelle, Pacific Northwest Laboratories, study of the Hanford mortality data, which in many respects disagreed with Dr. Mancuso and Stewart's findings.

In the studies conducted here, we have found that the overall mortality rate in the Hanford worker population is substantially lower than that of the U. S. population, which is not surprising in an industry with an excellent health care and safety program. Moreover, the overall cancer rate is lower than that of the general population.

Among the cancer types that have been considered on the basis of other evidence to be related to radiation, such as leukemia and cancer of the lung, the Hanford workers have had lower mortality rates than the U. S. population.

A couple of less common cancer types, cancer of the pancreas and a rare cancer of the bone marrow, multiple myeloma, have had somewhat increased mortality rates with a suggestion of a relationship to radiation exposure levels. That latter is based on very few cases, such as 1, 2, or 3 in certain exposure groups.

We consider that these are leads to follow in our further studies. The comparison of our information with results from other plants will enable us to reach more definite conclusions. In fact, these diseases have not been identified as typically associated with radiation exposure in larger studies, such as the Hiroshima and Nagasaki studies.

As. Dr. Gilbert will explain in her briefing, the possible casual relationship between low levels of radiation and cancer cited by Drs. Mancuso and Milham is based upon proportional analysis. This statistical method is a questionable approach inasmuch as any reduction in mortality from other causes, such as heart disease, stroke, or accidents, automatically increases the proportion attributed to cancer. Such reductions in noncancer rates have taken place among the Hanford workers.

We should also keep in mind that statistically significant relationships do not always imply cause and effect.

In closing, I want to emphasize that the studies by Dr. Mancuso, Battelle, and ORAU reflect the interest of the Department of Energy, its predecessor agencies, and their contractors in assuring that there are safe working conditions for all employees. We intend to continue these efforts.

Dr. Gilbert will now present the results of the study that has been going on at Battelle.

(TOM BAUMAN) - We will have copies of these viewgraphs for anyone who wants them.

(DR. GILBERT) - Since the results that we are going to present, or that Sid has already presented to some extent, are quite different from the conclusions that Mancuso and Stewart are coming to, I thought it might be well to talk about some of the differences in our analyses and their analyses, so, as you can see, I will be coming out with different conclusions. I am sure this must be somewhat confusing if you are not immediately in this field.

We have already referred to the fact that they have done proportional analysis, and we are doing a population-based analysis. In proportional analysis, one looks only at deaths and then looks at how many cancer deaths one has observed within these deaths; whereas, in a population-based analysis of deaths, one considers workers that are still alive and the risks of dying and relates the deaths to this population and risk.

I have a little example here that I think illustrates the difference in these methods and the problems that one can get into doing a proportional analysis.

Let's suppose first of all that we have two groups of people that we were interested in comparing with respect to their death rate. These might, for example, be unexposed workers, exposed workers, or they might be Hanford workers and some control population. We are interested in comparing these two groups of

people. We will assume that they are similar in respect to age and everything else of that sort, to keep the examples simple. Suppose we follow both of these groups of individuals for 20 or 30 years and observe how many died and what they died of. And let's suppose that in Group 1, ten individuals died, giving us a death rate of 10%, whereas in Group 2, twenty individuals died, giving us a group death rate also of 10%, when we started out with 200. This is from all causes. No, I'm sorry; this is just from cancer. The cancer rates are identical in these two populations. Now let's suppose that the death rate from noncancers, from noncancers we have 30 deaths from the first group, giving us a noncancer death rate of 30%, and, in the second group, we have 40 deaths, giving us a noncancer death rate of 20%. So we see that in these two groups, Group 1 had a higher death rate from noncancers than does group 2, whereas the cancer rates are identical. Now let's suppose in this population you do not have any information on the population and risk. That we look only at the people that have died in these two groups. We would then have something like what we see down here. In Group 1, we have 40 deaths, a total of 40 deaths. Ten of them died from cancer, and it looks like, therefore, that 25% of the deaths are due to cancer; and in Group 2, twenty of them had died of cancer out of a total of 60, which gives a proportion of 33%. If we looked only at this and didn't look at anything else, we would say that Group 2 has more cancer than Group 1. But, looking at the total picture, we see that what's happened is that this group had a higher death rate from noncancer, and really, the cancer rates are identical in the two groups. And this is really almost exactly what has happened in the Hanford study. The analysis that Mancuso has done and the analysis that Milham has done were proportional analyses. They looked only at deaths and, in Milham's case, he compared his deaths with Washington State and, because deaths from noncancer causes tend to be higher for Washington State in general than for Hanford employees, it looks like cancer has a higher proportion.

Similarly, in the Mancuso analysis, they compared exposure groups, and they get the same kind of thing going on. The proportional analysis can be deceptive, and that's what I'm trying to illustrate here.

Another problem with some of the other analyses that have been done is that they ought to take into account a number of variables that affect mortality and are also related to radiation exposure. And such variables can produce correlations if they are not properly controlled for. One such variable is calendar year. It takes awhile to accumulate a radiation exposure; it is built up over a number of years, so that most of the deaths that have had the higher exposures have occurred relatively recently in the late 60's and 70's, whereas the deaths with lower exposures could have occurred in the 40's, the 50's, the 60's. Now, this is especially important with causes of death such as lung cancer. Lung cancer is one of the death causes that the Mancuso study has implicated. Lung cancer rates for the U. S. population have about doubled between the 1940's and the 1970's. So if you compare high exposure groups who are almost all recent deaths with low exposure groups, which include the whole spectrum, you tend to get correlations just because you have not controlled for that. Another variable that is important is the length of employment. Obviously, the longer you have been employed at Hanford, the more opportunity you have had to accumulate recorded radiation exposure. In particular, the very short-term worker has practically no exposure. We have many such workers, many workers who worked here literally just a few weeks. These short-term workers have higher rates from noncancer causes for whatever reasons. I don't know just why this is so, but they do, in fact, have higher death rates from noncancer causes, and this tends to create some correlations when you don't consider the length of employment.

Still another variable that is important is occupation. Most of the radiation exposures are received in a few very specific occupations. These occupations are classified generally as craftsmen and operator types. I feel it is inappropriate to compare their death rates to, say, the death rates from managers and scientists on one hand and perhaps service workers on the other. They really should be compared with other craftsmen and operatives that are not receiving radiation exposure, which is what we have done in our analysis. Now, here at Hanford we have (at Battelle) analyzed this data in two different ways.

In the first type of analysis, we have related death rates of Hanford workers to those of the U. S. population in general. In the second analysis, we have tried to compare death rates for those with higher exposures to those with lower recorded exposures.

Now let's talk first about the first type of analysis. I might say first of all that there are limitations in interpreting a comparison of death rates in Hanford workers with those of the general population. I am sure you can all think of many ways in which the Hanford population is going to differ from that of the U. S. outside of radiation exposure, and all of this has to be taken into account in how one interprets these results. In particular, as Sid has already noted, this is an employed population. Employed workers typically have lower death rates than does the general population. It turns out that Hanford is no exception to this.

The method we used in carrying out our population analysis is to calculate a quantity called a standardized mortality ratio, or SMR for short. And what this involves is calculating the number of deaths we would expect in the Hanford population if the rates for the U. S. population prevailed. This takes into account the age and calendar year distribution of our population. So, we first have for each cause of death an observed number of deaths. That's what we actually see. And we have an expected number of deaths. That's the number of deaths from a particular cause that are expected based on age, calendar year, and specific rate for U. S. white males. We present the observed deaths as a percent of the expected deaths. That's what's known as the standardized mortality ratio. If the mortality experienced at the Hanford population is identical to that of the U. S. population, we get an SMR of 100 (the observed deaths would equal the expected deaths). On the other hand, if we had some adverse effect, then the observed deaths would exceed the expected, and we would get a ratio greater than 100 and vice versa.

Here are a few results relating to major causes of death. First, you will note that this deals with 20,000 white male workers at Hanford. You will note first of all that about 1/3 of them were there for less than two years. We separated

these and are now looking at the more interesting group, those that have been here for awhile. They are also the group that would have most of the higher radiation exposure. We see that the SMR death rates from all causes are about 75% of what one would expect based on U. S. statistics for the longer term (two or more years of employment) employee. As I say, that is fairly typical for the employed population. The cancer SMR is somewhat higher. That is also typical for employed populations. The bias that is involved in employed populations having lower death rates than the general population is not as great for cancer as it is for other causes. So this is something that is seen in almost any occupational population you would look at. Death rates from cancer tend to be higher than from other causes of death. Again, if we were to do a proportional analysis, because the cancer rate is higher than the noncancer rate, it looks like cancer is excessive in fact when they are both well below 100.

Looking at some specific cancers, these are SMR's just for the groups that were employed for two or more years. We see a lot of variation in the SMR's, due mostly to the small numbers of deaths involved when we get down to very specific cancers. None of these are significantly greater than 100. A few are slightly higher, but the fact is that they are within what one would consider random variation. I think it is particularly interesting that leukemia is so low. Leukemia, of course, is the cause of death that has been most strongly associated with radiation exposure in studies such as those of the atomic bomb survivors. We see that leukemia deaths are only about half of what one would normally expect. So, I think that fact is especially interesting because leukemia has a very short latent period, probably less than five years, as opposed to some other cancers. This means that, if there were something there, we probably would have seen it by now. As I mentioned, the other kind of analysis we did was to relate death rates to exposure data. We again calculate observed and expected deaths, except that this time, the expected deaths are the deaths that are expected if the radiation exposure had no effect. You look at death rates from all Hanford employees, apply the rates to various exposure groups, and calculate the number we would expect in a given exposure group if radiation had no effect. And we did this in a way that takes variables like age, calendar year, and occupation into account.

Now the exposure groups that we considered are less than 2 rems, 2-5 rems, 5-15 rems, and 15 or more rems. Here, we see the results of that analysis, and I will go over the interpretation of the results. I presented in this viewgraph the two higher exposure groups. Now, if radiation had an effect, one would expect to have more observed deaths than expected deaths in these two higher categories. Now, on the other hand, if radiation has no effect, we would expect the observed and expected numbers to be approximately equal. Looking first at all causes, we see that the number of deaths observed in the two higher exposure categories are less than one would expect, which is saying if anything, that the people who are getting higher exposures have lower death rates than those who have lower exposures.

(MIKE BERRIOCHOA) - Excuse me. The expected death rate, that's just the national norm?

(DR. GILBERT) - No, this particular analysis has nothing to do with national statistics. It is the number that one would expect in that exposure group if radiation had no effect.

(MIKE BERRIOCHOA) - Then how is that figure arrived at?

(DR. GILBERT) - Well, we look at essentially the death rate for all Hanford workers, apply that death rate to the number of people that have an exposure of 5-15 rems, and multiply to obtain an expected number of deaths. It's a little more complex than that, and we did it in a way that would allow for differences in age, calendar year, and occupation.

(MIKE BERRIOCHOA) - That number applies only to the Hanford population?

(DR. GILBERT) - Right.

OK. Looking at cancer, the observed and expected for the two higher categories are actually quite close. In the overall cancer, we see no evidence that those with higher exposures have more deaths from cancer than do those with lower exposures. Leukemia we might want to look at. As I mentioned, this is the cause

that has been associated with radiation in other studies. Here we see that, looking at the combined groups, we have a half of a death expected. We observed one death in these two categories. It is true one is greater than one-half. On the other hand, if only a half a death is expected, you can't get much closer than 0 or 1, so I can't get too excited over the fact that we have one death here. It is in the lower of the two groups. There are two causes of death which do show the observed to exceed the expected. One is cancer of the pancreas. Here we see, particularly in the higher group, we have three deaths observed and one expected. Also, myeloma, where we have a total of three deaths observed with a little over one expected. In the higher group, we have two observed with less than half a death expected. These do turn out to be what we call statistically significant. We feel, as Sid has already noted, that these two causes of death do warrant further attention. But you see they are based on a very small number of cases, and we cannot rule out the possibility, for example, that these results may be affected by things like pre-Hanford exposures or even misdiagnosis. Cancer of the pancreas is a difficult disease to diagnose. We are certainly going to continue to look at these. This is an ongoing study where we are continuing to get new data. These will be causes that we will want to look at particularly closely. But, looking at the overall picture, we do have those that are high, and we have some others, cancer of the pancreas, for example, and prostate, for example, in which we have no deaths where we expected about two. So, on balance, the cancer rate for these workers is no higher than one would expect.

I guess that concludes my presentation.

Following are questions/answers from press conference:

(Could not pick up question from tape; believe media representative is JINI DALEN)*-

(ANSWER) - Dr. Mancuso's contract was terminated July 31 of this year, and he has sought to focus his attention on his study and has criticized the agency for terminating his study.

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*See footnote on page 1.

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) - It is his contention that he was terminated because the
v a positive correlation between cancer and employment at Hanford
rnment knew that a full year ago and has chosen to keep the lid
. As he put it, because of the positive results, you were
any arrangements to announce the results. That's the way he

lly, his notice of termination was given 2-1/2 years before he
vell over a year before his positive results came out. At the
of termination was given to him, there was no reason to
ere any results different from any he had in the past.

)* - Dr. Milham said he decided to take up this issue with
1. He says he came away from the meeting with the impression
those statistics published because there was an anti-
he time.

Milham's impression, perhaps, but we have no reason to
ıl here or anybody here attempted to dissuade him from
He either asked or was invited to come over (I don't
e at the time) to discuss further information because
report at that time, said that his work should be the
It happens that at that time, there was a great deal more
ıvailable in the mortality study, and we got in touch
Dr. Sanders to make available to Dr. Milham the fact
more data than he had.

r. Marks, could you summarize then for us, perhaps put
have gone over here this morning. How accurate is
, it says the study shows a higher than normal rate
/ees at Hanford. How accurate is that? It includes

mention of a peer review. In layman's language, what was wrong? What did his peers find him lacking in?

(DR. MARKS) - If I may paraphrase his peer reviews, his peer reviewers found him lacking in his failure to publish results despite the nature of the results. It is customary in studies, conducted under any agency sponsorship, to provide to the scientific community and the public the results of the study or the plans or the methods by which you will proceed in edited publications as the work goes along, and, in the case of Dr. Mancuso's study, the peer reviewers, who were either from universities or from other agencies, felt that he was deficient in this respect. He was holding back too long. He was excessively focused on details in the data, and it was inappropriate to continue a study under an investigator when he was not coming forth with the results of his studies. And the peer reviewers really had no concern with what he was coming up with in his studies. In fact, if there were positive results, it was considered certainly appropriate to present those to the scientific community and the public. If there were negative results, this was also important.

(MEDIA REPRESENTATIVE)* - (Front portion garbled.) How accurate is this report?

(ANSWER) - We feel that on the basis of what we regard as a more reliable approach to it, that that report is not accurate, that the overall cancer rate in this worker population is not increased, and that the conclusions that have been drawn regarding specific cancer related deaths are not valid.

(MEDIA REPRESENTATIVE)* - What's wrong with the methodology?

(ANSWER) - The methodology is faulty because they have failed to take into consideration certain biases in their methodology, as Dr. Gilbert has pointed out to the group, and, also, they have failed to use the fact that they had a population base to work from, but used this proportional mortality procedure that Dr. Gilbert pointed out has deficiencies in it and can lead to biased results.

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*See footnote on page 1.

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