SESSION VI

EXTRAPOLATION TO LOW DOSES

CHAIRMEN:

ALVIN M. WEINBERG

AND

JOHN R. TOTTER

INTRODUCTION TO SESSION VI

Alvin M. Weinberg

Institute for Energy Analysis
Oak Ridge Associated Universities
Oak Ridge, TN 37830

This morning we are talking about Extrapolation to Low Doses. I am not a biologist nor an expert on statistics. I have written a paper in which I raised the following question: how do you formulate public policy with respect to standards of exposure in those instances where science is not proficient, either as a matter of principle or because science hasn't progressed sufficiently far to determine the dose response relation? There are a large class of questions that outwardly look like scientific questions and yet, in principle, can not be answered by science. I called such questions trans-scientific. The two examples I gave were rare events, and questions that had indeterminate answers because the underlying mathematical structure was indeterminate. In the latter category were questions like "What is the climate going to be five years from now?" or "What is the GNP going to be three years from now?", the point being that in those cases although the mathematical equations that describe the phenomena (at least in the case of climate) are well known, they are highly non-linear and the solutions to the equations may be unstable with respect to small changes in the initial conditions. Tiny differences in the initial conditions magnify into very large changes in the final outcome, the result being that the system lacks predictability, and this is intrinsic. A system becomes, as the mathematicians nowadays say, chaotic.

The other class of questions were those that I describe as rare events. For example, you can ask what is the effect of a million milliroentgens on the health of John Totter. You can get a pretty good answer to that. Then, you can ask another question which has exactly the same structure, what will one milliroentgen do to John Totter and the answer is not that easy to give. Of course, the answer you give is that nothing will happen to John Totter as a result of one milliroentgen applied to him. Yet that's not quite the answer, at least not if you assume the linear hypothesis, because then there is some probability that John Totter will contract cancer some time in the future as a result of that one milliroentgen.

My argument was that questions dealing with rare events, and cancer from low-level exposures has the character of being a rare event, were fundamentally beyond the proficiency of science to answer. Therefore, in order to deal with the matter from the point of view of public policy one probably ought to recast the issue.

We've heard about cost-benefit analysis in the discussion. Nuclear reactors are going to impose a certain cost in terms of health and a certain benefit which is only indirectly related to health. The advice that we get from the professional cost-benefit analyst is that you must measure the cost and measure the benefit. This was in the BEIR II report. Then you draw the line somewhere so that you can decide if you are going to do it or not do it as a result of this weighing of cost and benefit. There is a great deal of arbitrariness in this because the metric in which you measure the cost and the benefit are not the same.

In situations like this it is arguable whether conclusive scientific data can ever be obtained. Should one not be more explicit with respect to the arbitrariness and admit that the question of where the line should be drawn is to some degree a matter of taste, a matter of politics? Eventually, it is a matter of politics anyhow when you have to draw the line between what's an acceptable cost and what's an acceptable benefit.

Howard Adler and I have proposed that you go back to the original BEIR I report, which pointed out that there are two different ways of arriving at standards for low-level radiation. One was to start with the occupational doses and work downward, which is in fact the way it has been done. The BEIR committee said that that wasn't as logical as the other way, which was to relate the standard to the ambient background.

Howard Adler has proposed setting the standard for public exposure at a level "small" compared to the natural background. The argument hangs around the definition of "small". Adler did give an explicit definition for "small". "Small" should be taken as one standard deviation from the natural background. This turns out to be about twenty milliroentgens per year, where the mean natural background is about a hundred milliroentgens. It is interesting that this value is about what the EPA established for the allowable exposure to the public for the entire fuel cycle. I am told by people within EPA that the resemblance is not purely coincidental.

I understand that NCRP and other people are beginning to take seriously this idea of establishing the standards at a similar "de minimis" level in places where it is arguable whether you will ever have a definitive answer. In the Soviet Union the matter is apparently handled more simply because an acceptable level of exposure is established and I am told that they do not record exposures below the acceptable level. Therefore, it is impossible to multiply the number of individuals by the number of millirems per year and establish a dose to the exposed population.

Public policy has been largely concerned with exposure levels far lower than the levels that will be talked about today. Bill Russell said last night that he protracted the dose and he got it down to seven ten thousandths of a rem per minute which seemed like an awfully low number. That, however, amounts to roughly 365 roentgens per year. This turns out to be about 3,000 times the natural background. By contrast, the casualties estimated in the famous Rasmussen study come from the very large numbers of people who receive a single exposure of the order of, maybe, one year's background.