Safety Goal Project

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NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING

WORKSHOP ON FRAMEWORKS FOR

DEVELOPING A SAFETY GOAL

THIRD PLENARY SESSION

Rickey's Hyatt House

4219 El Camino Real

Palo Alto, California

Friday, 3 April 1981

The meeting was convened at 8:10 a.m., pursuant to notice, with George Sege, Program Chairman, Office of Policy Evaluation, presiding.

PRESENT:

Mssrs. Beyea, Bradburn, Bridenbaugh, Burstein, Charnoff, Cochran, Derby, Eisenbed, Ernst, Hutt, Joksimovic, Kouts, LaPorte, Lave, Levine, Lewis, Malsch, Lowrance, MacLean, Maxey, Mazur, O'Donnell, Okrent, Paige, Perrow, Salisbury, Sheldon, Slovic, Starr, Temme, Wald, Zebroski, et al.

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P R O C E E D I N G S

MR. SEGE: Good morning, ladies and gentlemen. We are about to begin the third plenary session of the NRC Safety Goal Workshop. The session will be devoted to the final reports of the panel chairmen, and subsequently to a discussion of those goals and to the discussion of the cross-cutting issues of the workshop.

Before we begin with the panel reports, I would like to turn the floor over to Walter Kato, who has some announcements.

(Discussion off the record)

MR. SEGE: Thank you, Walt. We are now ready to proceed to the report of panel A, quantitative safety goals, Dr. Herman Kouts.

DR. KOUTS: Well, George has asked that these reports be structured in a special way, and I will try to adhere to this as well as I can, although it is not going to be possible to do this altogether without being repetitious, so I will depart from this to some extent.

So I will first of all go through what I consider to be the highlights, or what we consider to be the highlights of the outcome of our discussions.

Let me tell you at the outset that yesterday afternoon, Hal Lewis, being bored with the rate and content of process, decided it would be worth summarizing the session,

and he did so, and most or a good part of what I am going to give is Hal Lewis's contribution, and you might recognize some of the fingerprints here.

Highlights. There should be thought of quantitative safety goals. This was important, because that was the purpose of our panel. There should be quantitative safety goals in order to enhance the protection of the public, not directly, but through making the regulatory process less capricious and more objective.

The standards must be clearly understood to be subject to political test. A standard can enforce quantitative analysis and rules and subsystem standards and these are objectives to be met.

It can provide a de minimus basis for deciding what measures are important enough for safety and what measures are not significant.

The goals should at their center consist of a qualitative state desired to be acheived, related to safety of the people. This should be supplemented by quantitative features which implied acheiving the qualitative goals. Somewhere, but not necessarily as part of the goals, there must be instructions on how to use these quantitative limits in a reasonably unambiguous way.

Several proposals of safety goals for nuclear power plants have been made. Included have been offerings by

individuals and groups with varied backgrounds and other objections.

These proposals address different statements of qualitative objectives, and they are accompanied by discussions of a variety of criteria that are said to have been used in arriving at the quantitative goals, yet the outcome, as the quantitative limits, is remarkably similar.

The limits are set on different quantities and in different cases; there is an overall consistency. This is at first surprising, because the guiding criteria has many arbitrary features, and it reflects a greater degree of concurrence than would be expected of work done in separate isolated monastic cells.

Regardless of this broad community, and of how this broad community of agreement may have been arrived at, it is heartening because most of the job of structuring rational safety goals with a broad consensus seems already to have been acheived.

However, there is less agreement on the way safety goals should be used. This may be as important a question or perhaps an even more important one than the goals themselves.

The goals should include requirements related to hazard status or substatus so that not only a final grade be acheived, and then Hal's comment here is that every professor knows the difficulty attached to giving only a single

grade.

The goals should also include quantitative components related to individuals highly at risk, and to the aggregate risk to society. They should include a component related to financial impact on society.

A limit on anticipated frequency of partial core damage -- this is very particular in conclusions -- a limit on anticipated frequency of partial core damage is not very useful at this time, because no one knows how to calculate that probability as reliably as would be needed.

The goals should respond to the question of how safe is safe enough, and should imply such a judgment. And this is not a unanimous view, but is almost unanimous, and ALARA type complements should not be included.

The licensing process should be deterministic, with the deterministic requirements justified through demonstration that they assure meeting the safety goals. Both the subsystem and the whole plant analysis can contribute to this, but it is recognized that in the present state of the art, a large element of judgment would still be involved, as well as recourse to operating experience.

The one exception to the deterministic rule should be that an applicant for a license should be free to propose a new system or a new subsystem and to prove by analysis that it better acheives the goal.

The numbers associated with the goals require political consensus, but the development of the techniques of calculation requires much more technical work. In particular, there may be some subgoals for which the calculation is now beyond the state of the art. This is a conflict. There is a conflict requiring resolution here, between desirability and complete feasibility. Hal says C'est la vie. That is the way it is in life anyway.

The goals should guarantee that as far as cost goes, the public benefit of nuclear power is greater than the risk, which is part of the overall cost, but this risk and cost should not be so unevenly distributed that any individual is unreasonably exposed to risk.

It is recognized that this tradeoff between public benefit and individual cost is inherent in any complex society, and the issue here is no different than elsewhere, and no simpler either.

The goals should be dynamic, to respond to progress in technology. The grandfathering of plants already approved should be normal policy in the absence of overriding safety considerations to the contrary.

This last point is especially true, because the more quantitative goals that we are discussing here will be addressed principally in plants that will not come into existence and operation for more than a decade, and they must

be applicable to conditions that we can foresee as possibly important then.

Political consensus and public acceptance are essential to the end product, but the responsibility of the Nuclear Regulatory Commission and not to satisfy them. These are not always compatible objectives, nor are they always conflicting. Hal's comment there is if doctors were licensed through a public hearing, we would have more charming quacks than we do now.

These are the points that we agreed on, or these are the highlights of the points that we agreed on.

One issue which is strongly at debate, and may require greater resolution or may not require greater resolution; this is the conceptual basis underlying quantitative limits.

Now, I pointed out earlier that there is a greater degree of coherence in the quantitative limits that people establish than there is in the basis that they use for getting there. Some favor one basis, some favor another.

Probably most in our panel agree that cost benefit or risk benefit analysis would be the preferable basis for setting quantitative limits included in safety goals, but we are not all very sure that analysis of this kind could be useful in the reports.

There is one suggestion as to means to settle

outstanding issues, and I will simply quote Hal a point here, which is that a finer group than this should meet at a finer place than this to flesh out the recommendations.

MR. SEGE: Thank you very much, we now turn to Panel B, Dr. Lester Lave.

DR. LAVE: You will be pleased to know that in the sweepstakes among the panels, that Panel B decided unanimously that it won the award among the three panels.

I would like to spend a couple of minutes updating the outline that I put on the board yesterday, going through the following -- or the comments that -- the areas that we hadn't gone through, and then try and follow the outline a bit.

We left off yesterday with some of the points about what it is that can be quantified, and the additional points that I would like to make from our discussion are some, I think, both philosophical and then then would entail both, and how it is that one can predict a macro event such as a major accident one expects to occur very rarely, and how well one can know that the probability of that is stated.

And basically we talked about the use of monitoring of micro-events such as component failures, plus some modeling, in order to better validate the models and to update the forecasts of macro events such as accidents, and there is an interactive process here which can enhance safety

to a large degree and enhance our knowledge about safety. We talked about the necessity for centralized data collection and analysis, that is, that if each individual nuclear facility were to try and go on the basis of its own experience, then there would be a vast waste of data.

And finally, although sabotage itself cannot be modeled with probability, one can try and get some upper bounds on the effect of sabotage and other kinds of events that really are unpredictable events.

The point is that after all this, there still will be some level of uncertainty which remains. There still will be some confidence intervals, if you want, and in particular, there will be some ignorance of unexperienced events, some of which can't even be clarified, particularly in the micro-data and of modeling so far.

But the key to the process is trying to collect data to learn and have a feedback process, sort of what one might think of as educated bootstrapping, and I think that in our look at what it is that is doable, it is certainly clear that some of the accident probabilities are inherently things that cannot be known with vast confidence, but there is a process by which one can know them better, perhaps acceptably well.

We did at some point yesterday, after being urged more than a dozen times, take a look at the name of the panel,

and try to do something about that.

Basically we decided with using qualitative safety goals or qualitative statements, is that they are nearly always vacuous. Peter Hutt reminded us of the Congressional language, that qualitative statements that have been part of the federal statutes for more than a hundred years, they are enduring goals, and yet if you were to ask or to be asked to translate them into some particular judgment at some time, you would be hard-pressed to come up with any kind of consistent judgment.

And so we thought about the complementary nature of qualitative and quantitative goals, and in particular, it seems clear that Congressional goals or Congressional statements in legislation almost by definition have got to be qualitative in nature.

That is, a statement like no undue risk, and so on. Unless the Congress is going to get into the business of equipping itself to deal as technical experts in narrow detail and confront issues on a continuing basis, which the Congress doesn't have the expertise and time to do, then Congress is going to be forced to stay with more or less bland generalizations.

However, once one gets to the agency level, we thought that there should be a combination of process goals, qualitative goals, and quantitative goals. The process goals

are ones for trying to get input from both the general public and from experts to involve people more generally, and to air value conflicts.

These are all, you will remember from your list of the attributes that regulators should consider, there are a vast number of attributes that regulators have to consider, and the question is how should these be raised well, and we had decided that these process goals were ways of raising them.

We thought that there was an advantage, in contradiction with Panel A, I guess, of various qualitative goals at the agency level, such as ALARA or good practice or licensing.

That is that, in the midst of having any quantitative goals, those quantitative goals only make sense if the system is operating the way that you desire it to operate, and we thought that there was a good, strong role for qualitative goals in the midst of all that, and finally down to quantitative goals, the problem here is that quantitative goals should change whenever there are changes in technology or changes in income or changes in the health status of the population, or changes in value.

That is, that the quantitative numerical goals are going to be goals which cannot last for decades or centuries. They will have to be reexamined all the time, and the agency cannot be in any way defensive about reexamining them. Okay.

In talking about the implementation of goals, we had a long session on what is a goal and what is a decision rule or a standard, and it turns out that that divided along disciplinary lines, with the engineers telling us what exactly a decision rule standard was, and all the rest of us being ignorant. I won't go into that now, but those of you who are engineers would know.

One of the things that the panel wanted to emphasize was the difference between design and performance standards. That is, that in general, standards should be emphasizing the bottom line of what you are trying to acheive, rather than trying to set out a specific process for getting there.

That is, that if your goal is to be stated, for example, in terms of the number of accidents of various sorts, then that is the way the quantitative goal ought to be stated, rather than trying to tell people in detail how to do things.

Now there were a series of major qualifications to that, about, for example, licensing procedures for operators, whether they should be licensed at all, but nonetheless, the discussion emphasized that performance standards were to be preferred wherever there was a choice.

We spent some time talking about consistency of goals at various levels. You think about the complex hierarchy I have, with Congressionally stated qualitative

goals, agency process qualitative and quantitative goals, and then various kinds of decision rules or standards and it is clear that there is no easy one-to-one relationship between goals at various levels, and one needs to have some fairly sophisticated model for making sure that these goals at various levels are consistent, and that they can be revised consistently when there are new data which come along. It is very important that the system be one which can change with new data.

We spent some time talking about the problem of how it is that you get the system to operate as desired. I think the characterization was that the life of a reactor operator is 99.9 percent boredom, and 0.1 percent terror, and there was some discussion about how it is that you get a system to perform that way, when you have somebody who is highly trained to -- sitting around on his rear end most of the time with absolutely nothing to do, but then having to jump forward in the right minute and not only do what is right, but to decide whether he or she should be using some standard mode or have to think for yourself.

If you put yourself in that situation and have some analyses that you might have, about, for example, what happens when you are driving your car and have a small accident or run into a snowstorm, you see how difficult it is. I think it is sort of good luck to -- what it is we are going to do

from reactor operators.

The point was made that there is a lot that can be learned from the training of pilots and radar operators at DEW line and so on, in trying to figure out how you do these things.

And we spent some time talking about this business of processes for identifying and involving stakeholders. One of the first points that is made is you don't have to worry about Congress. It will decide how it wants to be involved.

That is, you go along the way you want and Congress will call you up whenever it is that it wants to get into it. The point was that there are a series of questions, or pieces of information that have got to be gathered by the process of involving stakeholders.

For example, what attributes are important for the particular problem that you have got in mind, what are the distributional inequities that come from that, are the goals being met? We tend often to pooh-pooh what it is that the public can do in this, but one example from air pollution control is that a major amount of the enforcement is done by the public, looking for smoke plumes in the sky.

One needs to have a background of public input and discussion on the business of going from somewhat vacuous qualititative goals to very specific quantitative goals. That

is not a matter for a few experts to sit around in the room and decide. Rather, that is a series of complex judgments the public has to be involved in.

And finally, when the sum of public and other involvement about the adequacy of the process itself.

We have tried to talk about the nature of the process that would help and this wonderful word of felicitous came back again, and we wanted felicitous processes, that is, processes which give people information which is in a helpful format, and is of the right degree of explicitness. It is easy to overwhelm people, even trained scientists, with data in the wrong format being given in the wrong way, and have them simply find it too difficult to process that kind of information.

And finally, there was a lot of emphasis here from the experience of various other agencies that it is very important in the process however apprehensive the regulators might be not to patronize the public or other individuals who are involved, to sugar-coat information. The problem is, any time you try and sugar-coat it, there are at least a half-dozen people out there who are willing to jump up at a moment and say, don't you really mean to say, and then state it much worse than you would have ever done to begin with.

Well, at this point, I will go to the format that we were asked to conform with very briefly. I will try and

give you a summary of the summary. But first, let me remind you, I was told that the Roman emperor Justinian thought that earthquakes were caused by sodomy, a statement that most scientists are not very concerned about, but I understand there is lots of concern around San Francisco.

The point of the observation is that it is very hard to make good decisions unless you get your facts straight, and so a lot of our process is trying to get the facts straight.

So here we go with the summary of the summary, and I do not ascribe consensus on my panel to all of these notions. I have one tin ear, and the other one is turned off, and so what I heard in the panel discussions is the following:

First of all, we applaud the process of increasing specificity and of a more open systematic process. We think that that is salutary. We think that not only is it good for the public out there, but in particular it will be good for the NRC and the nuclear industry.

We agreed that there was an exemplary safety record to date for nuclear power. I think the agreement ended at that point as to what were the implications for the future, but at least on that, we seemed to have agreement. We agreed that the nuclear industry was not paranoid by thinking that it had lots of enemies out there. In fact, virtually every high technology industry and many industries that are not high

technology have lots of enemies out there. The nuclear industry is not unique and there is a great deal that can be learned from looking at the experience of other industries, many of whom have suffered more, or at least longer, than the nuclear industry.

We wanted to emphasize a multidimensional array of attributes and distributional effects that are involved, and try to emphasize that the key part of all this is that there must be tradeoffs that are made.

If somebody goes in in a sophistic way to think that all that must be done is to provide power or save lives, then they are having too simple a picture of all this. It is really in the nature of tradeoffs that have to be emphasized most.

We wanted to emphasize that there are disadvantages as well as advantages to being systematic, specific and open. As a practitioner in this arena, I believe that the disadvantages can be mitigated, and that that is something we ought to do, but one cannot rush in blandly here and believe that all is going to be right with the world if only we allow everybody access.

As I said a little earlier, much can be learned about the accident probabilities and consequences. It is important to do some systematic data collection and model in feedback, and so on, but there will be residual uncertainty.

That residual uncertainty is more or less major now, and should diminish over time as we get more operating experience.

The points, again made earlier, were the complementarity of qualitative and quantitative safety goals, consistency of goals at various levels, specifying how it is done versus performance standards, and a felicitous process for getting information to use. Thank you.

MR. SEGE: Thank you, Dr. Lave. We now turn to the last panel, Panel C, on economic, political, and social considerations, Dr. Paul Slovic.

DR. SLOVIC: Well, I am going to focus on the second day of deliberations primarily, except where we went back to some of the discussions of the first day and tried to clarify them.

This is a rough view of the major topic areas that we discussed over the two days. We talked about distributional questions, spatial and temporal equities, the treatment of kinetic risks, problems of scale, and that is the number of reactors and how that impacts on the goal process, the level of risk that should be targeted, the problem of risk aversion, the problems of the incentive systems and the question of process and verification.

I wouldn't at all ascribe equal weighting to these, and some of these are rather specific technical problems. Others, such as the process question, are very major

philosophical questions on which we recognize it may depend.

I will try to go through these one at a time, and summarize the views on each of them.

With regard to distributional questions, as I mentioned yesterday, we basically agreed that where there is an inequitable distribution of risks and benefits in the present, that some principle of compensation may be the way to address these inequities.

We had a greater problem with the intergenerational equities. We agreed that that was very difficult. We debated this kind of weight question -- as I mentioned yesterday. I won't go into that now.

One thing that I left off yesterday was the issue of whether or not by making these standards tough enough and reducing the risk level, in the goal process, this could kind of get around some of these questions. That is, if your target levels are low enough and that they are met, does that make the problem of equity well, sort of disappear, and some felt that this would, but basically there was some skepticism here and a feeling that really that you didn't really know how low is low enough to -- you know, so that you can not have to worry about these equity issues, and so it was left as an issue that these would be faced.

With regard to genetic risks, there was concern about the treatment in the ACRS proposal of using early and

delayed deaths as surrogates for genetic risks. There was mixed feeling as to just, you know, whether this was appropriate or not.

Some felt that this was adequate and that the report was complex -- that the system of quantitative goals as identified in the ACRS report was pretty complex and needed to be simplified and this would be one adequate way to do that. I think there was a little wider feeling, that the more explicit treatment of genetic effects should be attempted.

There was some disagreement as to the fine points of that treatment, whether or not it should be done in a strictly quantitative way or not.

We spent quite a while on problems of scale, some of which I touched upon yesterday. We talked about the problems of moving from a system of 70 or 150 reactors up to 500 or 1,000, and we asked, well, what are the problems that this might pose for the implementation of safety goals, and we sort of classified these into several different categories.

First, the institutional issues, dealing with whether or not one can maintain the desired standards of safety and sort of offset the slippage in the design, licensing, monitoring, and emergency response capabilities in the face of a large-scale system of reactors.

Also, questions of the training and availability of personnel, and the demands on the regulatory system of large

scale nuclear power.

There was a question of vulnerability due to an increasing dependence on nuclear power, and the relevance of the mix of energy technologies. There was a question of a contextual effects. There was a very subtle one, which we didn't go into in detail, except to point out that it could be very important, and an example of this would be as the scale increases, the frequency of accidents of various types will increase, and what are the implications of this for, you know, economically, psychologically, and so forth.

There was a feeling that this needed to be considered in the treatment of safety goals.

While there was agreement that this was important, there was lack of agreement as to what specifically the effects of scale would be, and I am saying we had quite sharp disagreements as to just what the effects of increased scale would be.

Finally, with regard to what might be the implications of scale effects, of course, this depends on what you think the implications are, but there was some feeling that this would have implications for how you design plants, for the regulatory load, that is, if you felt that -- if you anticipated a large-scale system, you would try to do things that would ease the load on the regulatory process, the sheer number of watchers and so forth, and also you would try

to set things up in a way that would minimize the demands on the emergency response capabilities, which might be overtaxed by large scale bases.

With regard to level of risks, we really didn't get involved in the question of what the actual level of numbers, or what the numbers should be, but rather the question of should the goals be stricter for nuclear power. As you might expect, there was a debate on this matter. Some people felt that there should be uniform standards. Others felt that because nuclear power was a newer technology and there were certain political problems involved, the standards should be stricter.

The question of uncertainty was brought up, and that was argued both ways, some saying that because of the uncertainties in risk estimates we should have stricter standards and others saying there are certain aspects of nuclear risks which are better known, than say, aspects of risks from for example fossil fuels, and this would argue in the other direction, so you can see that we reached no consensus here on this problem.

The question of risk aversions also received some discussion. We debated the assumption in the ACRS report of increasing aversiveness of a large number of deaths, this alpha greater than one. We discussed the philosophical basis for risk aversion, and the philosophical economic basis, some

of the reasons for it would be uncertainty and vulnerability and the possibility of higher order of effects. In fact, I think it leads to some desirable incentives for mitigation and prevention.

Others disagreed and argued that a linear function with alpha equal to one is really more appropriate because it minimizes the expected losses, so we -- I think while most people felt that increasing alpha greater than one was justifified, there was disagreement there.

There was concern, really, about the effects of second errors consequences here, that in fact that maybe this isn't a good mod for the impact, that you could have small accidents that lead to large-scale economic and social disturbance, and if this is the case, this would have important implications for the level of safety, and then this led in turn to a debate as to whether these secondary or higher order consequences, this ripple effect, really was within the responsibility of the NRC, that is, where do you draw the line on what constitutes health and safety risks, you know, do you go into these economic considerations? We did not resolve that.

sic

Finally, it was pointed out that there is an attempt in the present NRC ACRS proposal to treat some of these ripple or large scale secondary consequences via the ALARA provision, which could bring in prevention if it is justified on a cost

basis, but there was an uneasiness that this might be an awkward way to address this question.

There was a concern about the incentive systems. This was, I think, similar to what I saw on Channel 2. There is a feeling that we really need to look at systems for evaluating risk assessments that, you know, risk assessments should be judged just like other forecasters might be, and there was an allusion to weather forecasters, who have scoring systems and this sort of thing, and this leads to a concern that there really should be ways -- there should be an attempt to make risk assessment more readily evaluated, and there should be awards for good performance in this regard. This also relates to the question of feedback that Lester brought up.

And this also carries over to the development of incentive systems for evaluating and awarding operating performance in the plants themselves. It was felt that this was an important issue. It hasn't received much attention, but it needs consideration.

Last but not least, I think we had gotten into our most exciting discussion of the few days in the last two hours, where we treated the question of process, and I guess you could say that when it comes to setting safety goals, some people feel that the process would be the most important consideration, and this ties in with the question of

verification. How do you know that your goals are being met? Won't the question of trust and public acceptability be intimately linked with this? There was some concern that unless process is given adequate attention here and built into the goal fabric that nothing would change. You would have these goals, but it would really have no effect on anything. This was hotly debated, I should say, in the group.

There was -- it was pointed out that in the ACRS proposal, there is a call for a third party review, and this led to a lot of discussion about well, this is fine, but who does it? How do you choose these people, and with regard to -- just to contrast with Lester's summary, with regard to well, sure, you want public input here, but because it is a complex process, you can't have the public making this review directly. It has got to be experts.

And then the question is, as I am sure you have heard many times, whose experts, how do we select them? You know, do we have a Presidential Committee do this, what is the role of intervenor groups, should there be an industry funding for intervenor groups, you know, to participate in this review.

I think there was general agreement that this was an extremely important issue that was vital to the successful implementation of the safety goal process.

I think I will stop at this point.

MR. SEGE: Thank you, Paul. I want to thank all three of the panel chairmen for very informative reports. It can be a short line -- a life for these summaries.

We are now going to proceed with the discussion of the reports. The format that I am proposing is that we first cover comments that refer specifically to Panel A, to Panel B and to Panel C and then proceed into a wider discussion, involving comments not closely tied to a particular panel or applied across panels. First comments on Panel A and I see Dr. Okrent's hand up.

DR. OKRENT: Okay. I have, I think, four questions. I think I heard Dr. Kouts mention that the panel thought there should be some kind of a limit on financial impact. I wonder if he would be able to tell me whether that was thought to be a dollar value or some expected value or however they were going to frame it.

A second point was that the panel was against an ALARA. I wonder what the basis was for judging that. Did they not want groups to see whether there remained cost-effective improvements, even if one met the deterministic requirements that they said should be the basis for licensing.

The third is that they said that they thought licensing should be deterministic, that these deterministic requirements should be so set up that one would meet the

quantitative safety goals. I guess my question is, how would it be decided that these deterministic requirements did meet the goals, who would decide, and how would it be judged that these were adequate for all plants and so forth, and then the fourth one was, I think it was stated that it was proposed that there not be a limit on partial core damage, and I think the reason that was stated was that this was a very hard thing to calculate, and I would be curious to hear why they think that is a harder thing to calculate than other things like, for example, full core melt, or the likelihood of containment function in a certain way given core melt, or so forth.

MR. SEGE: Dr. Kouts.

DR. KOUTS: I will try my hand at these. Then I would like to ask other members of the panel to add to this as they see fit, because there is --

MR. SEGE: Excuse me, Dr. Kouts. I think that the recorder is straining to hear you. I would like to make a general announcement that the microphone set-up here is not the best possible, so would everyone please speak loudly and also would you please identify yourselves for the reporter as you start speaking, or if I identify you by name when I call on you, make sure the reporter knows who you are, especially if you are not sitting behind your own nametag that is visible to the reporter. Dr. Kouts, excuse me.

DR. KOUTS: Yes, I say, I will try my hand at them,

and then I will ask other members of the panel to chip in and give their expanded views.

First of all, on whether or not there should be a dollar value associated with property damage, well, that was the one suggestion that was made, and I expect that it is more reasonable to evaluate property damage in terms of dollars than it may be to estimate other kinds of damage, such as damage to human beings.

I think there would be not very much deviation of views on this. Dollars is a reasonable judgment for how you measure.

DR. OKRENT: Would this be like $5 billion is the largest accident possible, or --

DR. KOUTS: No, we didn't talk about limits or exactly how the dollar limit should be set. We only -- there was only a fairly general agreement that there ought to be some limit on the physical impact, the impact on say possessions or aspects of life associated with accidents of this kind. There should be a limit like this.

DR. OKRENT: All right, the reason I asked is we thought about this and found it hard to figure out how to put an upper limit, and if you didn't put an upper limit, then you get to an expected value, and we ended up trying to put it in the ALARA, so if you had a good formula, I would be interested in hearing it, but that is all right.

DR. KOUTS: No, we have no formula, nor did we enter this meeting expecting to find formulae for any of the numbers, any of the limits that might be chosen.

Now, on the question of why we are opposed to an ALARA concept. I think there is a dominant view that we are after a safety goal which is addressed to the question of how safe is safe enough, and when you have determined how safe safe enough is, then you should be content with that degree of safety.

If you go beyond this, you are introducing uncertainty into the regulatory system, and you are undermining the goal -- you are undermining one of the features that you demand of a safety goal, which is that it makes the regulatory process more rational. It introduces an irrational aspect into the system.

DR. COCHRAN: Did you apply that to the radiation protection standards workers, for example, also? I want to know what the difference is in the personnel --

DR. KOUTS: Whether there should be an ALARA concept applied to workers?

DR. COCHRAN: Should there, or if there should, advise me of the distinctions.

DR. KOUTS: Well, I don't know. We didn't even go into that, but maybe the same argument would apply there.

MR. O'DONNELL: A point of clarification on this.

The objection to the ALARA, was it based on ALARA as a qualitative concept, or if ALARA was expressed in terms of a quantitative cost-benefit approach?

DR. KOUTS: It is an overlay on a system which first of all judges whether or not you meet criteria which are based on how safe you should be.

Now, once you have decided how safe you should be, you then go back and say, can I get safer than that even for some kind of expenditure, and the view was that this is already at diminishing returns, because you are already safe enough, and that has been your judgment.

MR. O'DONNELL: Well, I was voicing an opinion on that. It seems to me if you say a number that is going to be an absolute value that -- beyond which you do nothing, your tendency would be to set it much lower than you would if you had something like the cost-benefit curve.

DR. KOUTS: That may well be. That may well be.

MR. O'DONNELL: And you would probably end up doing things to meet that absolute standard which are in fact not cost-effective, which would not be required, if you had an ALARA cost-benefit curve.

DR. KOUTS: That may well be, but it may be a more rational process.

Would any on the panel like to add to this?

MR. LEVINE: Well, I felt that if a safety goal were

to be proposed, one should do cost-benefit analyses about that goal, that is if that once the goal were expressed if the regulatory people wanted to change it, they would have to do cost-benefit to justify the change, or if a particular plant was felt not to meet the safety goal, the cost-benefit might provide a way of saying it didn't have to meet the safety goal.

DR. COCHRAN: Could you comment on my question?

MR. LEVINE: No.

MR. SEGE: Let me ask members of the workshop again if you will identify yourselves by name as you participate in the discussion, to help the reporter make an accurate technical record. That was Dr. Cochran asking the last question.

DR. KOUTS: Should I go on?

DR. OKRENT: Please.

DR. KOUTS: Okay. Now, on the question of how the safety goals can be applied within a deterministic framework, okay --

DR. OKRENT: Excuse me, the point was, how would you decide that the deterministic requirements met the safety goals and who would decide it?

DR. KOUTS: Yes, that was my interpretation of what you meant. How would it be decided. First of all, who would decide it? I think the regulatory -- the people who are

responsible for applying regulation do this. It is their responsibility to make whatever application is concerned in whatever decision process is to be used.

How do you determine whether a certain deterministic process meets a safety goal? Well, this sort of thing is already in practice. That is, there are analyses which are done on probabilistic bases, having as some objective, like don't increase whatever level of risk is implied by WASH-1400, how do you do this for a particular system or subsystem of the unit that you are considering, and one example has been for instance diesel power.

How many diesels and how reliable should diesels be in order to make sure that the supply of electric power, at least the supply of backup electric power will be adequate to keep risk down to a certain level.

This is a deterministic application of a kind of safety goal, and this is the kind of process which is had in mind. Now, would others like to add to that?

MR. SALISBURY: I might throw in that we -- I think a number of people said that to have a deterministic approach would be perhaps a transition period. A lot of us just didn't have enough competence in the pure probabilistic analysis to make an abrupt transition, just using that for determining whether a safety goal is met. We would like to have a period of verification in one sense where you are using

it as a backup to the normal licensing process, and to give it time to prove itself and to build up confidence that it might in fact be able to replace the deterministic licensing.

DR. OKRENT: Well, by the way, in fact that very position was the way we talked about it in Panel C, but the thrust of my question was the following, as Slovik indicated in Panel C, there was a lot of discussion about process. And there was at least -- well, there was a considerable element in the group that thought that there needed to be assurance in the public, a lot of assurance that in fact not only were there goals but that they were really being met, and so to some extent, my question came from that perspective. The other was, it seems to me if you have the deterministic requirements, unless you do full-fledged probabilistic analysis, you don't know if they have met the goals you have set, so from both of these points of view is why I raised the point.

DR. KOUTS: I think on that point, you may be correct, but you may be only partly correct. It may not be necessary to do a probabilistic analysis on every plant. You may be -- maybe you can get by with an analysis on a subset of plants.

DR. OKRENT: I agree with that.

DR. KOUTS: Okay. But if you applied the process, that is, doing a risk-benefit analysis or a risk analysis on

every plant, in order to be sure that it met the goal, this would be a completely different process than the one we have in mind.

After all, we are at present in a licensing system which relies on deterministic methods. Now, we are talking about possibly in the future using a risk analysis basis for determining what to do in regulatory matters.

Perhaps we are talking about a transition period, and perhaps there has to be a transition period. After all, we are here, and we want to get there, and somewhere in between we have to be in between. It seems to most of those on the panel that the best early application of quantitative safety goals is to establish that the deterministic requirements that have been placed in the regulatory structure so far are really rational and do correspond to rational safety goals. Hal?

DR. LEWIS: Well, I was just going to amplify essentially what you said, you know, and perhaps talk about how one might make a transition. Clearly, I don't think anyone is proposing that the way you license a plant tomorrow or in the near future be to simply impose on the applicant the requirement that he demonstrate that he meets, you know, if you like, the Okrent list of probabilities or certain hazard states.

You know, what I would like to see is first an

effort to force the NRC to justify the present deterministic standards through probabilistic risk assessment, and go through that and continue to license plants deterministically.

Then, permit licensees to begin to deviate by providing probabilistic justifications for deviations from the deterministic standards, so that for example if the deterministic standard requires that you have three framistands or call a diesel generator or whatever, to meet a quantitative safety standard, and it becomes possible for somebody to come in and demonstrate, if you like, by a probabilistic assessment, that he can get away with two because something else is stronger, and gets away with it, then that will become the practice rather than the exception, and I see the Panel walking into the tank nosefirst through that procedure.

DR. OKRENT: Well, in fact, I don't disagree, but I have just one related question. Panel 2 mentioned a preference for performance goals rather than design requirements, and Tom Hickford recently indicated in an article he thought that was the way to go, but in a sense, you just said put in three framistands, not provide a set of framistands that will do the following, so you are heading in fact toward design requirements in what I have heard, it seems.

DR. KOUTS: Well, what we have is requirements for

three framistands. Now, the first think we can do is find out whether that is a reasonable requirement.

DR. OKRENT: I was just making an observation.

DR. KOUTS: Okay. Okay, now let us turn to this question of the limit on partial core damage, and maybe one good way to answer that is to deal with the TMI situation, which after all, was partial core damage. It certainly was not a total core melt.

That certainly was a function of the specific details of the accident. Certainly a function, for instance, of how much the makeup system was throttled back, or the high-pressure injection system was throttled back in the course of the accident, over what period of time that took place.

So, you could make a curve of degree of core damage against that specific aspect of the accident, and you could in fact take into account other aspects of the accident, like how long was it before they realized that the block valve was closed, or at what time did they turn off the main coolant pumps, things of that sort. You have a great number of possible inputs to a calculation which determines what degree of core damage occur .

Now, on the other hand, there is a great continuum which would lead to complete core damage, and another great continuum that would lead to no core damage at all. In between,

there is a narrow, very difficult calculable range. If, after all, they had turned off the high pressure injection system entirely, and had walked away and nothing had ever been done after that, we know what the consequence would have been. That is calculable. We can't calculate the in-between states as well, and since we can't calculate them as well, it becomes not as useful to have criteria which involve such calculations.

DR. COCHRAN: Do I understand you to say that in talking about the frequency of these events, that it is much more difficult to calculate the frequency of person throttling back partially as opposed to throttling it back all the way, or not throttling it back at all, or something like that?

DR. KOUTS: No, I didn't really say that. If you are trying to distinguish, however, between a state which is ten percent damage to a core and 30 percent damage to a core, that is very hard to calculate.

DR. COCHRAN: I am trying to find out whether we know any more about the probability of a 30 percent or less than 30 percent core damage than we know about full core damage, and if you don't know how the operator is going to operate in terms of this 30 percent figure, why do you feel more comfortable, or do you feel more comfortable in knowing the frequency of events that will lead to a full core melt?

DR. KOUTS: Maybe the word comfortable is not the best word to use there. I certainly would feel that a calculation of total core damage, probability of total core damage is more reliable, is closer to a correct value, if there is a correct value, than one for partial core damage.

DR. COCHRAN: It is not obvious to me.

DR. KOUTS: Well, that is my conclusion. Would any on the panel like to add to this?

DR. BEYEA: One of the problems may be the definition of the intermediate hazard state might be able to -- it might be possible to change the definition to just say core uncovered, or some such statement which relatively well-defined. But I too find it difficult -- you know, as a nuclear critic, I too find it very difficult to deal with hazard state one. It seems very imprecise. I would have a very difficult time trying to estimate how you would do such a calculation.

DR. ZEBROSKI: If I may, what you can say about the difference between minor damage, which is loss of hermeticity, and major damage, which is -- or like TMI with great oxidation and melting, and melting of the vessel and melting of the containment, we can, if you look at the dynamics of these deterministically, you find anywhere from hours to tens to hundreds of hours difference between these different states, and I think it is totally fatuous to assume

that absent everybody dropping dead for long periods of time, that no recognition or action to recover during these long dynamic processes would occur.

Therefore, I think there is a very large difference in probability of gross core melting versus damage which has produced a lot of radiation signaling.

DR. KOUTS: That is very true, yeah.

DR. COCHRAN: But, well, you are talking about a difference in the probabilities as opposed to the uncertainties. I thought we were talking about how well we know the uncertainties of bars and not how well we know the probabilities. That is not true.

DR. KOUTS: Well, what Ed has done is pointed out another source of uncertainty in calculation of partial core damage, that I didn't bring up, and this is the effect of operator actions as they may determine the outcome in partial core damage, and Ed's point is that in fact, if an accident begins to develop, and time begins to pass, and you feel that you are getting near the edge, somebody always does something, and this leads to a very difficult to predict degree of core damage. That is my last point -- my point, it is not the last.

DR. ZEBROSKI: If the aim is to predict core damage on an absolute scale, you are right. If the aim is to say that there is a ratio between the likelihood of enormous

radiation signals coming out from loss of hermeticity, and a further process continued for tens of hundreds of hours without further action, I think that ratio is large and determinable and must be of the order of hundreds.

DR. KOUTS: Yeah, I would buy that.

MR. SEGE: Mr. O'Donnell?

MR. O'DONNELL: Well, it would seem to me that there are uncertainties in this intermediate range, but my understanding of the current methodology is that they are normally resolved in a -- so it is a conservative approach, that is, if you have an accident sequence in which you -- leads to inadequate high pressure injection into the core, you assume that that condition remains in effect, and therefore will lead to the ultimate consequences of full core damage, rather than trying to predict the intervention that would terminate or reverse that sequence prior to reaching that end, so I think the state of the art at present is such that many of these uncertainties are resolved in a conservative manner.

DR. KOUTS: Well, that is the situation now, in fact, and that is why in WASH-1400, one does deal only with one hazard state, which is complete core melt.

MR. O DONNELL: Another point, I think, on this, that the reason to have a hazard state in the first place, it seems to me, to be one of being sure that you are putting

some tension on accident prevention, and primarily to somehow divorce the mitigation and the prevention aspects of safety, and the concern with the large scale core melt events is that those are the ones that seriously challenge the mitigation effects, therefore you want to make sure that you have put a limit on those types of things that are going to challenge your containment feature.

The partial core melt events, such as TMI, really don't provide a serious challenge to the containment features anyway. Therefore, it doesn't seem necessary to have that additional intermediate state as a goal.

MR. SEGE: Vojin?

DR. JOKSIMOVIC: I would like to add on point that it was our consensus that we need not do PRA studies for all the plants. While that may or may not be true, and I say that because I have yet to see plants which are identical, we have to monitor performance of every plant in order to ensure the goals are met.

DR. KOUTS: Is that adequate?

Mr. SEGE: Do we want to consider the next questions?

DR. OKRENT: I think he has covered it.

MR. SEGE: Have you had a chance to complete your response?

DR. LEWIS: I wonder if I could just -- if you are

finished with Panel A's questions, add one extra comment on the ALARA question, on which Beyea and I have -- and the reason I am not an enthusiastic supporter of ALARA has two parts.

One is the one you mentioned, which is that if you have done the job well, you ought not then start niggling at its fringes. You should just keep asking yourself whether you have done the job well and keep improving the way you do it, but the other one is a somewhat different one, which never got mentioned, and that is that I have a problem with -- in the mechanism for setting quantitative safety goals, because I would like to set the overall goals purely in terms of a tradeoff between the risks and the benefits, of the electricity, but I find myself in a minority of not very far from one on our panel, and in particular, I know that there are many people who believe that one ought also to take into account comparison with alternate means of generating the same electricity, and I find something logically inconsistent if you go the route of setting the overall goals in comparison with alternate means of making the electricity and not do risk benefit, and then do your incremental goals through cost-benefit analysis, it just seems to me to be an incongruity, and I would be much more agreeable to an ALARA if one were to do the original job in a pure and solid way, but that is my personal view, which is

probably not shared by anybody else.

DR. KOUTS: Oh, I think, as I said, in the course of that summary I made, I think generally we would agree with your view that risk benefit analysis should be the basis for whatever is chosen, but we are not so sure you can do it.

DR. LEWIS: I was just reminding Dave that he often recommends doing comparative analyses.

DR. OKRENT: Well, if I can comment, I think society would prefer that an ALARA approach be used if it is practical, because I think they would think it is a mistake not to use something that is cost effective, in other words, if you can reduce the risks still further in a cost effective way, even if you have met some threshold level of acceptability, and you sometimes see that in the laws that Congress passes, so my hunch is that that is the way society feels, and I do agree with the statement made earlier. If you don't have it, I think the pressure will be to push your acceptance limits lower. I will leave it at that.

MR. SEGE: I think Dr. Cochran wants to add something on this subject.

DR. COCHRAN: Well, I just wanted to disagree with Lewis's approach because of the inequities in the distribution of the benefits and the costs, such as with the utilization of the electricity on the one hand and the people sitting

next to the plant on the other. But I think it is primarily for that reason that setting overall or some sort of basic limits would be a mistake to do benefit cost analysis --

MR. SEGE: Any further discussion on the subject of what is reasonably acheivable? Dr. Eisenbud?

DR. EISENBUD: I think that this is very much related to another item that Panel A had on their checklist, which incidentally I found rather complete, and that is the need to develop a definition of diminimus dose. I don't have any objection to the concept of ALARA, I think it makes sense. I do object to the extent to which it is carried out.

I think frequently it is carried to extremes far below -- probably quite a debate as to what we could agree is a diminimus level. In other words, we shouldn't be concerned quantitatively with whether an exposure is more or less.

And this brings me to another problem, a matter which wasn't on the list, which I perceive may be a great problem someday, and it has to do with the fact that if the TMI accident were slightly different than it was, there would have been contamination of land to a low degree, and to an extent which I would believe was diminimus, at least it would probably be far less than the contamination of land that we experienced during weapons tests, and about which we know a good deal about the doses which people have ultimately absorbed from this contamination.

Now, there have been experiences, and mostly in connection with the weapons programs, where land has been -- become contaminated, and in the absence of criteria, land has actually been scooped up and treated as a waste, to very very small -- involving very small amounts of radioactivity.

I suppose the most notable example being the incident in Palomares, Spain, where a bomb dropped out of an airplane and scattered plutonium on the landscape, and the question arose as to well, by what standards can they say that the land is not contaminated, because with the radiation techniques that are available, you can measure a very small amount of plutonium, and I don't recall the figures, but I do know that they actually bulldozed very very large areas of land and shipped the soil to this country and treated it as a low-level waste.

Now, there may be circumstances where this is justified, on a high enough level, but I would say that perhaps the most urgent quantitative criteria we need is a definition of what the diminimus dose is in relation to exposure to workers, exposure of the public, and in particular, the contamination of land.

MR. SEGE: Thank you. Any further comments on Panel A?

DR. OKRENT: Well, I just wanted to note that in Panel C, as Dr. Slovic noted, there was a discussion that

genetic effects were thought to be something that should be specifically called out in criteria, and one of the reasons that was given was that while there may be debate on whether or not there is some small amount of -- with regard to somatic effects that you could discount, that the genetic effects would be, the theory was, linear, and so forth.

And in that sense, somatic effects would not be a surrogate as we assume, and in fact if one followed the route just suggested, it would sort of reinforce that point, it seems to me, that you would have to spend -- to account down to very small effects for the genetic parts. I am not sure you get rid of the question.

MR. SEGE: Perhaps we can revisit this issue when we come to Panel C. Dr. MacLean had a comment.

DR. MacLEAN: Just a point of clarification, something that I didn't understand. If it was -- and ask Dr. Kouts to explain this to me. If the consensus on Panel A was that the responsibility of the NRC is to protect the public, not to satisfy it, then could you explain to me what -- or why you thought that the quantitative goals should be subject to political tests, and what you meant by this?

DR. KOUTS: Well --

MR. SEGE: If I may, can I try a response and see if Dr. Lewis and yourself agree with it? I assume that what Dr. Lewis and you had in mind was that when there is a

conflict between protecting the public and satisfying it that protection takes precedence, but ordinarily there should be no such conflict and that adequate protection also can provide adequate satisfaction to the public. Is that correct?

DR. LEWIS: Well, that is correct, but I also meant something different, and that is that in the end in democracy, which hopefully we will continue to have for a long time, the performance of any public agency is ultimately subject to political consensus and acquiescence, and to public scrutiny. That is the way we live.

But that in my personal view, that does not mean that a regulatory organization such as NRC, which has the responsibility to protect the public ought to cater to the public in carrying out its duty, and I have -- if you would like to be precise -- a specific example ,which is contentious and I hope it won't make -- which is the venting of the TMI 2 containment, in which it was clear to every expert early on that the protection of the public consisted of venting that containment, getting in there as quickly as possible and making sure that nothing worse happened.

But in catering to uninformed public views, the NRC, to its shame, in my view, dallied and dallied and dallied. It is in that context, I believe, that the overall performance of an agency, and therefore the ultimate safety goal, requires political consensus, but the performance of the

agency in the small should not be designed to please people.

In fact, I regard it as a dereliction of duty if it were to be designed to please people. It was that point that I meant, but then again, I may be alone on Panel A with that idea.

DR. BEYEA: Yes.

DR. JOKSIMOVIC: No, you are not alone.

DR. LEWIS: Yes --

MR. SEGE: Dr. Beyea, did you have something?

DR. BEYEA: Well, that is just a new example that was not discussed. That example was not discussed in our panel. Had it been, I would have taken violent exception to it. I think that the agency --

MR. SEGE: What would you have said if it had been discussed?

DR. BEYEA: Well, I think that the agency had to move very carefully. It was a question of imposing risks at some level on the neighboring population, and that population was very very much concerned about what would happen. I think the Governor of Pennsylvania took a good course of action, and asked nuclear critics to also evaluate the danger. I think NRC could have taken such a tack early on and sped the process up. It is now doing that at Three Mile Island, and is now involving nuclear critics also in the process.

I can't see that the NRC or any agency can be

confounded for being concerned about imposing risks on some subset of the population.

DR. LEWIS: It has been established that the Panel is not unanimously --

MR. SEGE: Yes, apparently so. I wonder if members of other panels would care to comment about this, on this issue? Dr. Eisenbud?

DR. EISENBUD: I have one more comment, if I may.

As I said earlier, I thought that the checklist which you read off was excellent. I think that the report would eventually be more valuable --

MR. SEGE: Excuse me, Dr. Eisenbud, but if you will leave the topic of the possible conflict in protecting and satisfying the public -- I thought that we should perhaps exhaust that topic and then let me recognize you again after that.

DR. EISENBUD: Sure. I am sorry.

MR. SEGE: Mr. Hutt?

MR. HUTT: I think it might be useful to look at what some other government agencies currently do in the health and safety field. Food and Drug Administration goes through a yearly process in setting its priorities and it takes three issues into account.

The first question it asks is where is the largest source of risk in the American food and drug supply, and it

sets out in a true risk assessment basis what the actual sources of risk are. The second question it asks is, where is the largest source of concern expressed by the American public. There is almost no congruity between number one and number two.

In fact, quite frequently, one finds literally things that the public is concerned about that FDA finds almost no source of risk and vice versa.

And the third question that it asks is where can the agency be most effective in terms of reducing risk, which isn't necessarily where the biggest source of risk is, because if one concludes, for example, that the largest source of risk is in the most popular food consumed by the public, you can't do very much about that.

So that the agency therefore puts all of these together, and there is no magic formula for doing it, in order to determine how to set its priorities, but I don't think anyone ought to confuse those three points, and the way that FDA does its business has, I think, some relevance for NRC.

MR. SEGE: Thank you. Mr. Maclean?

DR. MACLEAN: Okay, this did come up and was discussed a lot in Panel C, and off -- and sometimes the comparisons were made with other regulatory agencies, and I would point out that it seems to me that the political

climate is such that the NRC has special problems that perhaps some of the other regulatory agencies do not have, and it is important to take these seriously.

MR. HUTT: May I just ask what they are, what the differences are?

DR. MACLEAN: There is the difference in credibility, I think --

MR. SEGE: Let me ask for the purpose of convenience of discussion, we separate the issue of credibility from the issue of protection versus satisfaction.

DR. MACLEAN: Right. The only point I would make on this is if you separate -- if the claim about the necessity to satisfy the public is that if you interpret that as regulatory agencies ought to be acting so that the public in some sense is made happy, then clearly that is not their responsibility, but if you consider satisfaction of the public, if you interpret that to mean that the responsibility of the NRC is to satisfy the public that it is protecting the public, rather than to put protection -- if that is the interpretation of satisfaction, then I think there is a very serious philosophical difference about whether satisfying the public that it is being protected is more important than protecting the public, according to what a group of well-intentioned experts might believe, and it may be, I think, that you can defend the position that satisfying the public in that sense

might lead to policies that experts think do not protect it as well as other policies do, and then it is really debatable about whether you want to give the public what is good for it or you want to give the public what it deserves.

DR. KOUTS: I think it is clear that the intent of that particular sentence was more narrow than you feel it could range. It was certainly meant in the connotation of bread and circuses in Rome in the early part of the Christian era, and the impact of that on the Roman Empire.

MR. SEGE: Maybe we should take just one more comment and then take a break and come back. I believe Ms. Sheldon had a comment on the issue of protection versus satisfaction.

MS. SHELDON: I did. I have just changed it on the basis of Dr. Kouts' last remark. Having been involved in a number of the circuses in this field, I guess I would say that in spite of that, there are questions of due process involved. That is what I know about as a lawyer. And then you are going to visit risks upon members of the public -- there has to be, or at least the agency is moving in that direction, some concern for doing that in a fashion or through a process which does give due process, does provide for fundamental fairness and for meeting the requirements of law, so that -- and that in turn, to follow up what Dr. Beyea is saying, does involve critics as well as the well-intentioned

experts who think they are doing good for the public, so it is -- when I hear that kind of remark, it makes me unhappy, because regardless of the problems that we have in the process, the answer is not to throw it out, but to recognize our fundamental principles that we are talking about in a democracy and in the legal system. Decisions have to be made according to those, and we have to work on that rather than abandon it altogether, which is that, I believe that is the direction that we are moving with fast-tracking licensing, with developing safety goals that may be used in place of the licensing process, or to make it easier to get these things accomplished.

I am all for efficiency, and doing away with meaningless exercises, a lot of which I have been involved in, but I am at the same time not inclined to substitute public judgment in all areas for experts from within who talk only to each other.

MR. SEGE: Professor Perrow, can your comment wait until after the break?

DR. PERROW: It is germane to this.

MR. SEGE: Okay, well why don't we take your comment now.

DR. PERROW: I would just like to underline that point. I am profoundly depressed by what I have heard just in the last couple of minutes here, starting with Dr. Lewis's

remarks, because I thought the industry and its experts were learning something from TMI and it had had a salutory effect, and I am now very puzzled whether it did or not, because what we have heard now is a reassertion of the experts know best.

Now, the experts, all the experts agreed there couldn't be a TMI and then there was one. Then we have these people here who are subject to this trauma, which I thought we were now learning about also, that there are, in the risk-benefit calculations there are certain public traumas, a cost to the public even if they are not directly irradiated from this, that we should take into account.

So we have this unpredicted accident and the trauma, and then the response is, well, the experts know best, and so we can release the krypon, and why is the NRC in there involving the public in this decision? This disturbs me that we haven't learned that the public has to be involved, that the experts are not always right, that there is enormous trauma visited upon these people. They are bearing the costs that none of us bear, that don't live there, and they are even bearing the rate cost. They are paying for a small part of that one billion dollar cleanup. I had hoped we had learned something.

MR. SEG. . Dr. Kouts would like to respond to that.

DR. KOUTS: I would like to respond to Ms. Sheldon, and simply say that I did not detect that there were any

opponents of due process on our panel, and there was no proposal --

MR. SEGE: Would you speak up, please? I think we are losing some of your words.

DR. KOUTS: I did not detect that there were any opponents of due process on our panel, nor did I hear any remarks directed in a direction of -- placed in a direction averse to due process.

I think if you searched, you might find some view that -- a view of opposition to the way due process is sometimes used, that is all.

DR. COCHRAN: Herb, did your panel think that safety goals should encompass process goals?

DR. KOUTS: Process goals?

DR. COCHRAN: Goals to improve the process --

MR. SEGE: Dr. Cochran, let me ask if you will hold that until after the break.

MR. LEVINE : I would like to make a comment just before the break. It is incorrect to interpret the statement that Lewis made by saying it would deny due process. I would like to be more explicit than Herb Kouts was in answering the due process question.

I think what Lewis's point is that the experts should make their decision on the best expert basis they have and then subject it to public scrutiny. I think that is

exactly what Hal said. He did not imply that the experts should make the decision and then not let the public be involved. I think that is an incorrect reading.

MR. LEVINE: I think as Joe McCarthy would say, one point of order, what Saul says is right. Obviously, but I would go further. I believe the experts should have their deliberations in full view of the public. I have no problems with that, and can indeed take input from the public. The experts are now always right, but the public is often wrong, too, and I believe that in the end, the agency with rasponsibility for what is done ought to make its decisions on the basis of its responsibility, its responsibility is to protect the public and as Mr. Hutt has said, often what the public demands and what is in the best view of the agency best for the public are in public.

Clearly some accomodation has to be made. I would like to make the accomodation as close to doing the job to which people have sworn oaths, which is to protect the public, as can possibly be the case, and I could give examples.

I purposely picked the TMI event, because I knew that would raise hackles, and I was right. It did raise hackles, but to take examples out of other fields, take an example out of medicine, the public apparently would be happier if Laetrile were widely used in the treatment of

cancer, and I think most expert opinion is that that would be bad for the public. I know people who have died by going to chiropractors when they should have gone to doctors with their serious illnesses. There are plenty of places where we err in the direction of satisfying the public in areas in which it is really not expert, and I would like to see a regulatory agency take its responsibility seriously; not deny due process, obviously expose what it does as well as it can, but still carry out its job.

DR. LAVE: Don't forget that there are times when people died because they went to doctors instead of going to chiropractors.

MR. SEGE: There is a brief announcement that Walt Kato wants to make before the break and after the break we will turn to Dr. Cochran's comment.

(Brief recess)

MR. SEGE: The third plenary session of the NRC Safety Board workshop will resume. Dr. Cochran?

DR. COCHRAN: If the agenda before -- were to come up with a rational explanation of why Hal's tail is really a leg, I would think the group could sort of focus on whether the agenda was the right one.

And I have -- most of the discussion except for some that we perhaps had in yesterday's Panel C was sort of focussed on the implicit assumption that there were these sort

of safety goals on the one hand, and process on the other.

If one accepts the possibility that the Kemeny Commission admonishment was correct, that the reactors could not be operated safely unless there was a fundamental shift in attitude on the part of utilities, vendors and the regulators, or something to that effect, then I think one would have to agree that process is really important in terms of acheiving a safe operation of reactors, and the attitude of the regulators is important, and that the efforts of your organization, of your group should be expanded to look very carefully at what sort of process and attitudinal changes should be occurring to not only improve the safety of the operations but the acceptability of the -- as Dr. MacLean put it -- the acceptability of the way the regulatory agency performs, by the public, acceptability by the public.

I would like to see if we could get some sort of consensus amongst the three panels that the process is an integral part of the safety goals process, and is not a separate issue that is not germane to the program that you are undertaking.

MR. SEGE: Dr. Kouts?

DR. KOUTS: Yes, Tom, I think that is quite reasonable and in fact that was in one of the conclusions that I stated. Should I read that part again?

DR. COCHRAN: Sure.

MR. SEGE: I would like us to proceed to Panel B very soon, to make sure that we do reasonable justice to the topics that need to be discussed in Panel B and C while there is time still available.

Are there any comments that refer so specifically to Panel A that they need to be made at this time? Dr. Eisenbud?

DR. EISENBUD: Just a short comment, and I would hope that the final report in discussing the items on the list that Dr. Kouts gave us would address the question of the extent to which the present system of establishing quantitative safety goals was either deficient or in accord with the recommendations.

For example, the first one, which notes there should be quantitative safety goals, well, of course there are some now, and it would be more meaningful, I think, if the report would discuss what needs to be done to the system now in place, if there is a system in place.

This is also true, there is a statement that the goals should respond to new knowledge, and I think the present system is very deficient in that respect, and that recommendation should address what the situation is at the present time in the present system.

MR. SEGE: Dr. Kouts?

DR. KOUTS: Yes, you are right, Merril. It has got to be stated that way, and it was my plan. I had already seen

this defect in the way these words were put together, and that will be changed in the formal language.

MR. SEGE: Tom, Mr. Cochran?

DR. COCHRAN: George, you just acted as though my efforts were just a passing comment, and they may be out of order, in which case we can do this later, but I still want to see if I can get a consensus. I see that Herb seems to suggest that it is the consensus perhaps of Panel A that the process and now it is attitudinal issues should be part of the -- of your programs, which I don't really see much work done in that area, but maybe I am mistaken, but I would like to see if that is the consensus of the entire group, and not just of Panel C or Panel C and Panel A.

MR. SEGE: Dr. Kouts.

DR. KOUTS: I think you went the second time a little beyond where you went the first time, and I wouldn't say that Panel A arrived at something which included attitudinal process.

What we said was that somewhere, but not necessarily as part of the goals, that there must be instructions on how to use these quantitative limits in a reasonably unambiguous way, which means that the process of applying the goals has to be spelled out also, not necessarily as part of the structure of the goals themselves, though. Maybe in a different document.

MR. SEGE: Tom, I believe that your comment captures our issues, that cut across Panels B and C, as well as Panel A, so let me suggest that we proceed with the discussion on the reports from the other two panels, and then after that discussion if you feel that the issue of consensus on this subject should be taken up again, please bring it up at that point, and then we will revisit it, okay?

Mr. Ernst, I believe, has a comment that refers to both Panel A and Panel B, and that may be a good way to bridge the transition between the discussions of the two panel reports. Mr. Ernst.

MR. ERNST: Thank you, George. Also Panel C, I think, and the only reason I asked to make a statement now is I did run a verification process on the -- problem, and it shows what credibility of information does to you. You sort of decide you want to check and audit yourself, and I will have to be leaving at 11:05, I guess, to catch that 1:15 plan, for anybody else, because apparently that is the only limo, and I am not sure you would be through Panel A or B and C by then.

Maybe a need for me making this statement is less necessary now because of the last couple of minutes. I do want to agree with Tom that process, I think, is an important part of this goal setting.

My own personal view is that perhaps the setting of the goal has more problems associated with ethics and

sociopolitical aspects than technical, the technical aspects may be somewhat less demanding, and my viewpoint also , I think verification, some -- the technical aspects somehow in my view become more dominant as to how do you determine that the verification process is useful and credible.

I would -- again in my perception, I think, there has been a great deal of emphasis placed on the problems associated with goal-setting, and perhaps necessarily so less emphasis on the so-called verification process, which is a mixture of technical and process-oriented problems.

I would strongly urge that in the next session on this subject that we pay more explicit attention to the details of the process of verification and things that come to mind, which is far from a complete list, is uncertainty and incompleteness of any probabilistic analyses, verification of assumptions made by practitioners of the art, the question of resources required to perform analyses and to audit analyses, which I think is important and somewhat of a cost-benefit process, how do you implement in a cost-effective way.

I think public understanding and input to the process. While I agree that the technical aspects can best be addressed by the technical experts, I think the process has to be open and understandable to the public so that there can be some credibility to the process, and this takes a good deal of thought as to how to make this occur.

Questions regarding rigidity and flexibility of the use of any ensuing criteria I think are important in the regulatory or licensing process, depending on where you apply it, case by case, or just in the general regulatory process.

For example, would a ten to the minus four criterion have the same rigidity that a 300 rem to the thyroid have in Part 100? How would you really apply these kinds of criteria, should they evolve?

The question of who authenticates the verification process I think is extremely important. Is verification done plant by plant, plant type by plant type, or by more deterministic application in the various rules or criteria that might be set up to implement the goal?

And if -- just as a side question -- if verification does include differences in probability between core melt and core damage, I think this adds an increasing complexity to the problem.

I am sure there are other kinds of things that are important to the verification process, but I just want to for the record state that I think it is extremely important to address the verification process, otherwise the goal-setting will really largely be in vain.

MR. SEGE: Thank you, Mal. Other comments on Panel B? Dr. Perrow?

DR. PERROW: Yes, I was a little puzzled by the idea

of flexible goals, since I thought goals were something that would be rather stable, and Panel B emphasized yearly changes, they didn't emphasize yearly changes, but they allowed the possibility for even yearly changes in goals, and among the criteria, I think, were economic considerations, and I wondered if this meant that if a plant started losing money that you would then be flexible about your goals and relax the safety requirements?

Are you having flexibility on the downside as well as on the upside, or is just that you want to keep raising the goals?

DR. LAVE: The answer to the second question is no, we did not think that we should be taking account of economic criteria as to whether a plant starts losing money. We have not been beating our people of our sexual preference. We were simply remarking that in a system like the United States you have a hierarchy of laws which enjoy different degrees of flexibility.

You have a Constitution where that stays fixed over long periods of time, it is extremely difficult to amend it. You then have a set of statutory laws which are easier to do something about. You then have administrative regulations which are still easier to change. What we were trying to emphasize is that quantitative safety goals which involve specific numbers, for example, $1,000 a man-rem, is going to

change if for no other reason than because of inflation. Remember that under the last eight years, that the real value of a dollar has halved, and that means that $1,000 in man-rems means half as much now as it did before.

Well, if we are going to fix quantitative regulations in stone, then we are going to be hung up on our own petard, and so what we are trying to say here is that we want to ensure that if there are changes in economic conditions, there are changes in health, there are changes in values, that there has to be flexibility on these numbers.

If on the other hand you go to the almost vacuous qualitative goals, like no undue risk, then that statement can stay fixed for all time, but it will mean different things in different eras.

MR. SEGE: Thank you. Dr. Zebroski?

DR. ZEBROSKI: I would like to amplify one part of the report, if I have the chairman's agreement.

DR. LAVE: And what if you didn't?

DR. ZEBROSKI: I would go ahead.

I think there was a discussion that a quantitative safety goal is really meaningful only in the context of a considerable description of the support and implementation structure, and that includes codes and standards, operating procedings, regulatory procedures and structures, the process of learning from experience, and the use of due process in

validation of contentious items.

It is my personal opinion that a qualitative safety goal has two dimensions. One dimension is the addition of this support structure as part of the meaningfulness of a quantitative safety goal, and the other dimension is that the qualitative safety goal, stated in qualitative terms, is a felicitous summary, in our chairman's words, of the quantitative goal, of the effect of the quantitative goals.

I think we have a very eloquent dissertation from Mr. Hutt on the unreality of taking a vague qualitative goal and deriving from it meaningful quantitative goals. Certainly the support structure couldn't be derived from such a thing.

So I think that it is important, perhaps, to recognize that the quantitative goal is not derived by theorems or lemmas from a qualitative statement, but perhaps the reverse is more accurate.

Next point is that a safety goal which implies a waiting discipline to give consistency to the support and implementation structure, and give a significant gain in safety even if the nominal target values are identical to those in 10 CFR 20, 50 and 100, and that is because a low relevancy of some of the regulations and procedures clearly divert major resources from the important ones, and thus in some respects are directly counterproductive to better safety,

so having a goal which permits a rational process of weighting the important from the unimportant is itself a safety goal, even if there is no change in the target values.

The main difference which an explicit goal ideally can provide relative to 10 CFR 20, 50 and 100 is an explicit incorporation of the likelihoods of different events as a measure of the required specificity and intensity of legislation, regulation and enforcement.

And a further difference which I think mainly affects the support structure, but which is also very important in my view is that the concept of specifying the intensity and regulatory response action and the speed of response required as a function of the size of the perceived difference from the safe state is extremely -- would be extremely constructive in getting a more regular process.

In technical terms, this means that the size of the time integral of risk contribution determined the priority and speed of response required.

MR. SEGE: Thank you. Any other comments on Panel B? Mr. Derby?

MR. DERBY: I would like to address an issue that Panel B started with and set aside, because there was no real way to resolve it, and that is that some of the material that was given to us for our consideration we did not find suitable to guide or discussion over the next two and a half

days.

I assume that the Office of Policy Evaluation will prepare a draft policy statement as a result of our deliberations here and other meetings, and I expect to have more substance because we have had these meetings, and then copies are available, the expectation.

What I would like to do is summarize what I think that policy statement should contain, and I see three parts. The top part is some declaration of intent. Now, I don't see this as being particularly hard. Probably a compendium of statements like undue risk and the best we can do, and all that, but specifically there is some resolution anyway of the definition of what considerations are part of the process, what social attributes?

We came up with a list and didn't know what to do with it, things like land use, mortality, morbidity, genetic effects. There has been a lot of -- scale -- there have been a lot of parameters. I would like to see a policy statement say what is and what is not going to be part of the consideration.

There is a bottom part of this policy evaluation statement, policy statement. I think it has to identify the class of regulatory decisions that are going to be affected, or at least addressed by the policy statement.

The example that I have for myself is that most

routine regulatory decisions are based on conformance to professional or technical codes and standards, SME -- standards. We are not talking about that stuff. That is handled. We are talking about decisions that represent some kind of judgmental bridging of no undue risk and what do I do now?

Those decisions have to be clear to focus -- I think the middle part, and that is an explanation of what kind of management principles or operational rules, or whatever is going to be used, whatever process things are going to be used to develop these regulatory standards that follow from the top and go to the bottom.

Now, it doesn't have to solve the problem, but I think it has to lay out some kind of framework, that what are the questions that must be answered, specific questions? What are the tradeoffs that are going to be made? How are these questions going to be answered? I am sure there are scientific factual questions that, say, go find out who knows about these things and ask them what it is, and that solves our problems.

Then there is also some kind of consensus of opinion, whose opinion, and how does all this go together? I would find that a really nice document to read and respond to. I would find it had substance, and I would find that it would guide what I would see the next step is, is trying to realize

some of these advantages and avoid all the disadvantages that we all foresee.

MR. SEGE: Thank you, Steve. Any more comments on Panel B? Ms. Sheldon?

MS. SHELDON: I have a comment and a question. I guess I will start with the comment. It is more of a plea that any other documents that are prepared on this subject that end up in the Federal Register, and in which public comment is requested by written in English. I was really fairly disturbed by the difficulty of understanding the language used in the document.

In spite of the fact that I have had some experience in this field, it is heavily -- it is primarily bureaucratese, it is heavily weighted with jargon. There are words used that have meaning to me that apparently have different meaning to other people.

For example, decompose a problem.

DR. LEWIS: You were right.

MS. SHELDON: And we have thought, on my side of the street for some time, there was a lot of rot around. So I would just ask that you remember who it is that has to wade through this stuff if you are asking for public input and comment.

And I was made very aware during the course of the last two and a half days that the Alice in Wonderland

principle of words meaning only what you decide they are going to mean is very much in effect. That is a problem, lawyers have their own lingo and engineers have their own lingo, but if we are ever going to bridge the gap because us and you and the public and the various aspects of this whole problem, we have to have a common ground of language to do it, and that would be my comment and plea on that.

And my question is to Mr. Levine. During our plenary session yesterday, you indicated you found a list of disadvantages that we had come up with distasteful, which I thought was an interesting word, and I wondered what it was about them that you found distasteful.

MR. LEVINE: I said they applied -- you could make that list of comments about any model anyone makes in any field. There are inadequacies in all models, and I don't know why they were being applied particularly to this type of model.

MS. SHELDON: I don't think we intended to apply them with particularity, basically.

MR. LEVINE: That is not how I read it.

MR. HUTT: Nor was it intended to mean that the disadvantages outweighed the advantages. They simply were isolated as disadvantages.

MS. SHELDON: The only reason we listed them was that that was the problem we were looking at, as opposed to

any other given model or topic.

MR. SEGE: Thank you. These have been helpful additional remarks by members of Panel B largely, I wonder if the members of Panels A and C would like to offer some comments on the Panel B report?

MR. HUTT: Does the panel that gets the fewest comments get a prize?

MR. SEGE: Professor LaPorte?

DR. LAPORTE: My question is for hopefully expansion. There is a section that you dealt with, implementation of goals. In one of them, you made a distinction between goals for design as goals for performance, the distinction between those two aspects of goals, and I wonder if you could say a little bit more about that, particularly in the context of the problem of verification over time, is the thing I have been struggling, and I hope you can help us understand that a bit more.

DR. LAVE: Okay, the basic notion is that when you do a design standard, you tell people what you want them to do. When you have a performance standard, you tell them what it is you want to accomplish and let them do it any way they want, and one of the discussions that we had, where I was led kicking and screaming down the path had to do with whether you licensed reactor operators.

That is, should one not simply have a goal for the

utility that says that the following accident and microevent frequencies may not be exceeded, and let them hire whoever they want to run their reactor in the course of doing that.

And as I say, I was led kicking and screaming away from that, saying, no, no, there really are some reasons why it is that you would like skilled reactor operators around in order to know these things, but the emphasis, we did come to one point where we all agreed, and that was that requiring hours of classroom performance of reactor operators was a design standard that we could all do without, since those people who had either been in classes from the front end or the back end knew that a requirement of spending certain hours in class was not apropos of anything other than making sure people got enough rest during a period of time.

MR. BURSTEIN: It is known as a residency requirement.

DR. LAVE: Yes, a residency requirement.

But, there have been a number of examples in other -- other than in the nuclear area, where what were initially design standards have inhibited a vast amount of progress, for example, specifying cast iron pipe in houses rather than permitting plastic pipe, and it was that kind of an idea that we were looking at here, to try and not stifle innovation, to not try and get in the way of improving reactor safety.

DR. LAPORTE: The reason I raised the question, you

said in your comments that the preference was for performance criteria, and you have just helped me understand how that arose.

And I am interested in the problem of verification not only of essentially component parts, and your comments suggested that it was in the component parts section that you were concerned with performance criteria as distinguished from design criteria, and would you then -- to what degree would you push performance criteria as contrasted to design criteria, for the operation of power plants as a whole over the timelines likely to be experienced into the future of a plant, some 40 years?

Because the problem of verification, of whether you can in fact know what the performance is, to know whether you have met the criteria or not, as contrasted to a problem of essentially forecasting performance, seems to me to drive you toward the design criteria question, but I don't know what your panel did about this, but there seems to me to be considerable ambiguity in this and how you think about it.

DR. LAVE: Okay, well, I think that one of the things that we were clear about was that in those occasions when you didn't think you could write a performance standard, that all you could write was a design standard, that you clearly had at the bottom four other techniques which are deemed to be equally suitable, rather than trying to fix some

alternative and not let other people do it, but I guess as a general principle, although I will admit that there are times when you don't know enough to write a performance standard, that that is a reflection of your ignorance, and you know, it sometimes occurs, but that those are things you ought to get around.

There is a very strong preference for writing design standards, letting people on the spot find the best way of implementing those things, and for example it would be meaningless to write a design standard which said there shall be less than ten to the minus seven chance of killing 20,000 people per reactor year. That is not an event that one would monitor on.

But on the other hand, Ed Zebroski was convincing us that you could write some performance standards on more micro-events, such as the number of component failures, which would be equivalent to, at least in some model sense equivalent to these other ones, they would be things that could be verified.

DR. LAPORTE: I understand the principle, you know, the examples, pretty clearly, and I was hopeful that you had thought about an essentially conceptual sense, you knowing -- and you began to answer that -- knowing when you can move from one kind of criteria to another.

Obviously, vis-a-vis verification into the future,

you have to almost go by design. If you can't check it out and see whether you have met the performance criteria. At some point, you have to move from one type of criteria to the other.

DR. LAVE: Okay, let me answer that in a minute, but just let me make a point that when you are using design criteria, then you are inherently going to have a lot of skepticism about whether in fact that is so, and I would think that lots of the problems with nuclear reactor accidents is that about all you have are some design criteria. If you had performance criteria where you could trace micro-events to macro-considerations, macro accidents, then you could have much more agreement on meetings some of these criteria, but when you have to wave your hand a little bit and say, we believe that if people do things the way we say they are going to do, that everything will turn out all right, then you are going to have a lot of skepticism about it.

MR. SEGE: Dr. Zebroski?

DR. ZEBROSKI: Well, I think in many areas you use both, but I think one particularly happy relationship is where you have a performance criterion which then covers the issue of deterioration with time, because it has within it, say, a periodic testing requirement.

But the relationship to a design criterion, which is an almost ideal one, is to provide the design criterion as a

regulatory guide. It simply says of the many possible solutions, if you use this one, we will take it as a stipulation that you have met the performance, assuming that is really known by the regulator.

So, you can cover it both ways, and still leave the option for improvements in design or in operation which would still meet the performance criterion, so I think the performance criterion is both more powerful, more flexible, and more long-lasting than the design criterion.

MR. BURSTEIN: But I think you will all agree, as Burstein says, that one of these days you have got to get from what you want to do to how you are going to do it, and whether the design criteria becomes a regulatory or a licensing criteria, or whether it is the manufacturers' or utilities' method, ultimately it must devolve into a design, a mechanical type of specification, because someday, if all of this is to be worth anything, we are going to do something with it, and how to do it is the design specification or criterion.

DR. LAVE: Let me at least partially disagree with that. I can think of a really simple example, which is the drainpipe on a house. The building codes used to say that that had to be cast iron pipe and it had to be put in in the following way, and along came PVC pipe, which would do the job in everybody's estimation at least as well, at a small

fraction of the cost.

And again, I thought that what everybody agreed to, except the local plumbers, that what you wanted to have was a standard that said there shall be a pipe there which will contain water under the following conditions, shall last the following period of time, and so on, that is, that those are all performance criteria.

Now, in the end, when you were installing such a drainpipe, you really had two choices. The two choices were this cast iron pipe and the PVC pipe, and whenever somebody went out to do the job, then if they were using PVC pipe, there was a set of things they had to go through in order to install it well, but there is a difference between specifying in the code, the building code, that it has to be PVC pipe and it has to be done in this way, and simply saying it has got to perform in this way.

MR. BURSTEIN: My point, if I may, just in reply, and I agree with you completely, but I have carried it that one step farther, that in order to install it, you have got to buy it, you have got to hire the right kind of guy, you have got to give him or her the right kind of tools, and you have got to say put it from here to there, and you can't tell him use one or the other without some instruction as to which to do. You can't leave the choice infinitely hanging there, or else you never accomplish it.

MR. HUTT: I think the issue there is, is that a regulatory process?

MR. BURSTEIN: Not necessarily.

MR. HUTT: Okay.

MR. BURSTEIN: Not at all.

MR. SEGE: Any other comments on Panel B?

Mr. Bradburn?

MR. BRADBURN: Yes. I would like to talk about something that we confronted very briefly in Panel B and didn't resolve, and which a number of comments I have heard here this morning raise again as an issue that I am concerned about, and that is the issue of the possible -- or what is the scope of application of the safety goal after it is developed, in whatever process it may be developed?

I heard a number of people, I think, say this morning that we need to recognize a limitation that this goal will probably only apply to new plants, or to future plants, and I don't think that that necessarily is the case. I don't think it should be the case.

I think similarly we have been talking about pretty much the reactor plant in a vacuum, and certainly haven't addressed fuel supply or wasted disposal or the reliability of the resulting compliance system, or the possibility of applying it to a future breeder program, et cetera, et cetera, et cetera.

In my opinion, I think that needs to be addressed, in the statement of the goal. I personally feel there is a need for a broad application, to look at the whole issue rather than focus only on the reactor or the plant and the plant boundaries, and/or that at least the safety goal has to have stated within it the allowance for a decision process that will address that in a meaningful way.

That there is a need to develop a process for applying whatever is applicable to existing as well as to future plants in some way. I recognize that all of this will have some limitation, the limitations being that not everything that is stated in a goal will be applicable to existing plants, and recognizing that some plants may not make it, and that serious choices then have to be made.

MR. SEGE: This touches on issues that were discussed in Panel A, and perhaps Dr. Kouts would care to add to this.

DR. KOUTS: Yes, I think I detected the reference to Panel A, and I thought we had taken care of this with careful wording. Let me read again the wording, and the part referred to here says the goals should be dynamic to respond to progress in technology, but grandfathering plants already approved should be normal policy, in the absence of overriding safety considerations, and that qualifying phrase there has a lot of content in it, of course, because where you find that safety

goals are not met by existing plants, and it is meaningful that they don't satisfy safety goals, then you do something, and that was put in there specifically for that purpose.

Now, the reference to future plants, was it even meant to take into account the point that the breeders will probably come down the pike, and will have to be dealt with by any safety goals that you establish, and this point is addressed by these words.

This is especially true because the more quantitative goals will be addressed principally to plants, and the word "principally" is put there with the grandfathering and the reservation in grandfathering specifically in mind.

DR. BRADBURN: Principally to what? I am sorry, I didn't hear.

DR. LAVE: Principally to plants that will not come into existence and operation --

DR. BRADBURN: Okay.

DR. LAVE: -- for more than a decade, and the intent here is that safety goals be established to guide the future regulatory process, which probably will draw more heavily on things in that area.

MR. SEGE: Mr. Malsch?

MR. MALSCH: It just occurred to me, I have a question. Does that mean that we should not use safety goals

that are established to influence current rulemaking actions, as for example, on hydrogen control, degraded cores, anticipated transients without scram, and the like?

DR. KOUTS: Not at all.

MR. MALSCH: Which will make and conceivably apply to present as well as future plants?

DR. KOUTS: Not at all. Once you have a safety goal, I assume you will then use it in the way we talked about, that is, applying it to these deterministic requirements to find out what rationale lies behind them, and whether they fit the safety goals.

And these are things that are established in the course of rulemaking, as well as by other processes.

MR. SEGE: Mr. O' Donnell.

MR. O DONNELL: I think if you adopt the policy that they are going to be forward looking only, their value diminishes very rapidly. I think the ideal case, and I am not saying that you said that, but there seems to be an emphasis that --

DR. KOUTS: No, this is not an injunction on the way things are to happen. This is an expectation of the way things might happen, that they are -- after all, there will be more plants built and operating in the future, perhaps, than have been built up to now, and --

MR. O DONNELL: I understand, but it seems to me

that one of the most useful applications of this whole thing is to, as we discussed earlier, evaluate existing regulatory practice and find out where you are, what level of safety vis-a-vis these goals you provide, and I think automatically that says something about the existing plants.

Now, it may be that they all meet it, and it may be that some of them don't meet it, and then you have to apply -- and I think this is where if you have it in something such as an ALARA approach, you can make those hard decisions on some reasonable basis of balancing costs and benefits, but I think that it would be most useful if the structure of the goals and the application of them were broad enough to cover existing plants as well, and any rule changes that are being considered.

MR. MALSCH: My concern was that if we are to use safety goals for that purpose. It wasn't clear to me in what sense it was that existing plants were grandfathered.

MR. BURSTEIN: Excuse me, every plant that is now operating or will be allowed to operate has been judged to be safe. It is a requirement of the law. Marty, you know that perhaps better than anybody else. There is a safety evaluation report that has been issued, that has been litigated, that in most cases that I know of has going to have the validity of a court review.

DR. COCHRAN: That makes them safe, doesn't it?

MR. BURSTEIN: Whether -- I don't think that is the point. I think whether you use a safety goal or a safety evaluation, and you are not substituting one for the other, as we discussed yesterday, a determination has been made under due process that that design and the capacity for its operation will protect the public health and safety to some standard.

Now, if you want to change the standard, or if you want to codify some presently unquantifiable items, fine, but it is not to suggest that present plants have not met a safety goal. We started out with that on day one.

DR. KOUTS: Let me just take up the ATWS case as an example, and apply the kind of reasoning we have been talking about to this. The staff is on the path to arriving at some ATWS requirements. These ATWS requirements are going to be the subject of a regulatory process which will include a hearing, which will include public process.

The safety goals, the purpose of the safety goals in this connection is to act as a touchstone, to determine whether or not the staff endpoint meets the safety goals, that is, there is a process to determine that that is a rational conclusion.

If it is a rational conclusion, and if it is then used -- it is then the basis for requirements which are placed on all plants, future and present, presumably the

effect would be primarily on future plants, things have been done with respect to existing plants already, and you will have to go back and look to see if they meet the safety goals, the requirements with respect to all plants will have to be checked, and maybe even the staff position will be changed by this.

MR. SEGE: Dr. Lewis?

DR. LEWIS: I think it is worth emphasizing -- I agree with what Herb has said, worth emphasizing that ATWS and some of the others that were mentioned are subsystem exercises, and those incentives of overall safety goals, if one could accept them on the basis of benefit versus risk or cost in which risk is an element to the public, if they were to be set in a reasonable way, and it turned out that a number of plants currently in existence did not meet the standards, then serious consideration should be given to shutting them down. There is no question whatever about that, but as in the ongoing proceedings on subsystems, that is a good way to begin to exercise the use of this kind of -- I hate the word methodology, but methodology, on the subsystem, deal with this implication, but then in the end, when one sets -- and ATWS is a good example, and ATWS reliability or scram reliability criteria and its quantitative weight, based on a probability assessment, that probabilistic assessment has got to include its contribution to the overall

risk of the plant in the context of an overall limit on risk of the plant. But that is a fair ways down the pike.

MR. SEGE: Thank you. Are there any other comments on Panel B? If not, let us move on to Panel C, comments on the Panel C report, Dr. Lave?

DR. LAVE: We spent a bit of time talking about alpha, as to whether alpha was less than one or equal to one, or greater than one, and we thought of some situation where alpha is much smaller than one, for example, if you take all the Cambodians who have lost their lives in the last year or so, it is clear that that is not an alpha of two. It is probably an alpha of 0.01, or so.

That is, that there are situations where you seem to get an upper bound on the amount of concern that anybody can ever have, and that led to a sort of a general comment which -- I mean, being a modeler myself, I hesitate to say it, but I will say it, which is that what you call risk aversion, or what the ACRS has called risk aversion is much more complicated than a single parameter, alpha, being greater or less than one, and we didn't think it was terribly helpful to try and summarize it in that, and then to go on a bit, we thought, though, that the method we would recommend for other -- for the NRC to try and get at these was what we called a set of relevant comparisons, and we had huge debate on what was a relevant comparison, whether it was looking at

dams that was a relevant comparison, and comparing hard numbers, like the number of people who died from lightning strikes last year, to calculated numbers, like the expected number of deaths from a reactor, was a relevant comparison, so we didn't have a lot to offer as to what exactly was a relevant comparison, except a method for finding them, which was probably to try and ask people what were the relevant comparisons? Do you think of nuclear reactors and airplane rides in the same set of dimensions, or where it is , to try to find out what the public has in mind as to relevant comparisons as a way of doing things.

MR. SEGE: Thank you. Dr. Okrent?

DR. OKRENT: Well, I am by no means going to defend alpha equals 1.2. We did feel it was of use, in fact, to introduce the question of should there be risk aversion in some kind of quantitative safety goal formulation. There have been proposals in the past which included this and which did not.

The original Farmer curve, as many of you know, had risk aversion in it.

I will note one thing. It does in fact act as a surrogate for certain kinds of safety philosophy if you are so inclined. If you have an alpha larger than one, it tends to push you toward remote siting, other things being equal, because there are penalties in -- when you do the ALARA. When

you do the ALARA, it is worth more money, in other words.

Similarly, it tends to push you toward containment features which reduce the likelihood still further of the large events and so forth, so if you have in mind a qualitative safety philosophy that says, these are things you wish to encourage, you can do it by something of the sort.

Now, you know, there are other ways of achaiving the same purpose. This was a very simplistic way of putting something on the table.

MR. SEGE: Dr. Zebroski?

DR. ZEBROSKI: Dave, we discussed this in another context. That is, if you set your safety goal for this area as being substantially better than something else, either in a cost avoidance or risk avoidance or risks of alternate sources of supply, risk of deprivation, and so on, if you want to set it, say, two times or five times safer than alternate technologies, there is a possible implication that the misallocation of resources means that you directly kill more people in some other area, and so in that sense, at least, it raises a moral question of the advisability of setting an excessively tight safety goal for whatever noble reason might appear to be present.

The alpha seems to be one of the -- and I think therefore that whatever reasons or excuses for setting goals unreasonably low come up should be examined extremely closely,

and alpha is a very interesting one, but I think in several respects our immediate behavior in the regulatory process is to assume a rather low alpha, and two other examples that I think of in addition to Mr. Lave's is the occupational exposure, where clearly by reducing the individual allowable, you irradiate more people to greater total population man-rem, and the other one is the large increase in lung dose, probably a thousand times or a million times greater than the nuclear apparatus can do, from the energy conservation closure of buildings, and we are acting very irresponsibly in those areas, or at least not consequentially, and that effect amounts to a very small alpha.

MR. SEGE: Dr. Lewis.

DR. LEWIS: I just wanted to make a minor comment. First, in -- I agree with Ms. Sheldon about the call for clear language, and -- although I would be happy to see, in California it is okay to write things not in plain English, if you can at least write them in plain Spanish, in plain language, yes.

And in that context, I think we have determined risk aversion is a somewhat misleading term, and in the sense that I would like -- I don't want to defend alpha, or even speak in the context of alpha, but some kind of nonlinear penalty which one might call the penalty of scale, in the sense that one wants to penalize a large accident a bit more

than an accumulation of small accidents, makes sense not -- but not because the public is more averse to such accidents, but because there is more social damage done by a large accident than by an accumulation of small accidents.

There was one viewgraph, I have forgotten whose it was, that spoke of the ripple effect, which is the beginning of that phenomenon, so I think one should penalize large accidents a bit more. Just how, whether it is through an alpha mechanism or something, I don't know, but I would argue in favor of dropping the term "risk aversion," because that implies that one is doing it for the wrong reasons.

MR. SEGE: Dr. Kouts?

DR. KOUTS: I just wanted to respond to something Dave Okrent said about the value of alpha greater than one driving you to remote siting. I am not sure that is the case with respect to the way the criteria are established here, because they are established on latent effects, and not on the individual effects, and remote siting has very little effect on the accumulated societal risk in latent effects.

DR. OKRENT: Well, it has an effect. There really is a difference between Brown's Ferry and Indian Point for latent effects.

DR. KOUTS: That is not remoteness. That is part of the country.

DR. OKRENT: Well, I am just saying, the point that

I wanted to make is in fact, it has that trend. You can make it strong or weak as you wish, but let me just make a comment about whether or not society is risk averse or not.

There is a paper that recently came to my attention in Science and Public Policy, October, 1980, by a man named Sutcliffe, in which he mentions that the provincial government of Groningen in Holland adopted an interesting sliding scale in which accidents capable of causing ten deaths ought to have a probability of not exceeding one in 10,000, over 100 deaths not exceeding 1 in 100,000, that is linear, and of a thousand deaths, complete unacceptability.

So, instead of having an alpha of 1.2, they had one up to 1,000, and then it became infinite. The point I wish to make is in fact, there are such considerations which are active in society. This is, by the way, not with regard to nuclear. It happened to be hazardous chemicals and their explosion, and so forth, but it is an issue, and I am not saying that one should adopt any specific approach, but I don't think we can just dismiss it.

MR. SEGE: Mr. O'Donnell?

MR. O DONNELL: Before one adopts an alpha factor of greater than one, I think you should be aware that it does one thing that I think has an adverse effect, in that it reinforces an aspect of current regulatory policy that has been criticized by both the Kemeny Commission and Reboken

Commissions, and that is the regulatory preoccupation with large consequence low probability events, that is, the major double-ended pipe break, and the catastrophic accidents, and it tends to put resources in design and allocation of regulatory tension into those very events, and which when they are multiplied by an exponent of 1.2 become magnified in their importance, and it does not -- it takes attention away, I think, from the TMI type events that are in fact more probably and do -- may in fact confer a greater portion of the risk, so I think it does have that disincentive in terms of the end result.

DR. OKRENT: Can I make one comment there? I have heard this kind of statement now often enough from people in the industry that I am a little bit disturbed.

In the first place, I think the question of regulatory staff focus on the double-ended pipe break is not relevant to the question, because in fact the regulatory staff is working very hard to try to provide something to deal with this, and it is not, in their view, a serious accident, because they have provided something for it.

It seems to me that what the public is concerned about is not the interruptive event, which is what I will call TMI, but the event, in fact, where large amounts of radioactivity might get out, and for anybody to say that otherwise, I think it is just wrong -- I will put it that way.

Now, how one gets, let us say, more probably to an event involving large amounts of radioactivity may well indeed not be a double-ended pipe break. Maybe it in fact is a chain of errors, and if you want to say that, okay, but it is in fact the possibility of a large radioactive release that is the disturbing event, and I think you should, in my opinion, word it that way.

MR. O'DONNELL: If I can just respond very briefly, I think what I was trying to get at was that in fact a double-ended pipe break is one which consumes an enormous amount of time in terms of analysis and yeah, we do in fact have a way of dealing with it.

But as everyone was looking at the large pipe break, the stuck-open relief valve had essentially zero attention. The risk aversion factor I can see very conceivably leading to decisions that would favor introduction of core catchers and things of that nature which then become, appear to be cost-effective when you apply that, and may put resources into those things rather than into things such as operational training or indicators on stuck-open relief valves.

If it is introduced for the purpose of addressing public conceptions concerns, that may be very valid. I just wish to point out that it does have in fact, I think, this very real disincentive.

MR. SEGE: Thank you. Mr. Hutt?

MR. HUTT: I have been trying to draw analogies on this risk aversion issue to other government regulatory agencies, and it is interesting, the only place where I am aware that Congress has in a sense explicitly dealt with the issue, it has gone exactly the opposite from what you are suggesting. There is an alpha, as I think I said yesterday, of infinity under the Delaney clause, and that deals not with a catastrophic event of a large number of people being killed by one event. It deals, rather, with isolated occurrences over a long period of time that one can never quantify in terms of effects.

In contrast with some events under Toxic Substances Act, Food and Drug Act, Pesticides Law, where there could be catastrophic events such as botulism poisoning, thalidomide, things of that kind, where there is no special rule built in the way there is for carcinogenic contamination of foods and drugs as there is under the Delaney clause. I am not saying there is any necessarily global message to that, but I am not aware of a regulatory mechanism that is built in specially by Congress to show an especial risk aversion for the single catastrophic event.

MR. COCHRAN: Your mathematical statement is incorrect.

MR. HUTT: Well, I am not a mathematician, so I apologize for that.

DR. COCHRAN: Next to the alpha, Delaney is zero, not alpha of infinity.

MR. HUTT: Okay, well whether it is zero -- but it is an all or nothing -- it is a total risk aversion for -- I didn't know the mathematics of it.

MR. LEVINE: You would find infinity is just as good as seven for the coefficient.

MR. HUTT: I will let somebody else debate that.

MR. SEGE. Dr. Okrent.

DR. OKRENT: Again, without arguing pro or con on risk aversion, because in fact I have an ambivalent position, I do think that the Congress in fact over the years reacts strongly when the large event occurs, and we try to look at a correlation to coal mining safety legislation and accidents, you find a very interesting correlation with large accidents, even though there may have been many more people being killed with small accidents.

And if you look at the discussion on the Auburn Dam recently, a big issue was the fact that if it failed in fact it might kill on the order of three-quarters of a million people, and that this was a special consideration, and so forth.

And in fact, I would even argue that with regard to the point you just made, the question I think in Congress's mind is not truly that there is a risk to an

individual, but in fact that there are 200 million people involved, and so if the probability turned out to be ten to the minus four per year of some carcinogen introducing cancer, when you multiply it by two times ten to the eight people, in fact it is a big event which doesn't appear all on one day, but it is still a big event, and in fact most of the reactor effects, when you -- they are totalled up on one page, but the bulk of them are calculated to be statistical things that will occur spread over the years.

MR. HUTT: Okay, let me respond and agree in part and disagree in part. I would distinguish sharply between what it is that causes Congress to legislate, in which I agree totally with you, one can go back from 1900 to the present and trace every -- well, not every, but most regulatory statutes to catastrophes, no question about that, well-documented, but when they legislate, they show no particular risk aversion for a single catastrophic event as contrasted with a series of undocumentable, isolated occurrences, and indeed one can show they seem to be more concerned about the latter than the former. That was my point, David.

DR. OKRENT: In fact, I agree, and I said as much in a recent paper.

MR. HUTT: Okay. Then we have got to be right.

MR. LEVINE: Let me make just one minor comment,

and that is that Congress is of course inscrutable and I see nobody is taking my advice to avoid the term risk aversion, so for once, I will say the penalty of scale is practiced, and Bill Lawrence has just pointed this out to me, in the aviation industry, because if you look at the regulations the FAA within its Congressional mandate has imposed on airplanes, you find that they do increase in stringency with the size of the airplane, and therefore with the number of people at stake in a single accident.

An airplane above a certain size has to have slightly better equipment, slightly better qualified pilots. It works its way up according precisely in some cases to the weight of the airplane, others the number of passengers carried, so that there is an element of the penalty of scale built into the decisions of the FAA.

VOICE: But is it more than linear?

MR. HUTT: Again, I don't disagree that some agencies have done that. I was attempting to figure out, in a sense talking, thinking out loud as to whether Congress itself which supposedly represents the embodiment in our country of public opinion -- I say supposedly -- had ever sort of dealt with this issue of risk aversion explicitly. That is all I was trying to deal with.

MR. SEGE: Dr. Beyea?

DR. BEYEA: Well, Congress may not be the best

example. Another example of the public's aversion to events in which large numbers of people are killed is reflected in newspaper coverage, I think.

If you look, single deaths are very rarely reported unless it is a famous person such as Hal Lewis, one would not expect to see much attention given to it, but on the other hand, if you have five or six people, or a school bus, people are killed, there is a tremendous amount of attention.

And Three Mile Island is an example of the public's interest and concern for risk aversion itself.

MR. LEVINE: Apart from leaving subjective assassination attempts after this meeting -- you perhaps don't watch the same television news programs I do, because they seem to give more coverage to murders of single people than they do to a large catastrophe, and my complaint is that I have to wade through all this enormous coverage of single, rather minor events, before I find out that, oh, by the way, a revolution broke out in Poland today --

MR. SALISBURY: That is in Southern California.

MR. SEGE: Okay, Mr. Salisbury, would you care to add anything to this?

MR. SALISBURY: Not particularly.

MR. SEGE: Okay, Dr. Kouts?

DR. KOUTS: I would like to move on to the suggestion that genetic effects be included in the criteria,

or in the safety goals, and just ask what it means for -- when it is said that the individual risks and the societal risks are surrogate for genetic effects. I don't know what that means. That was said.

DR. OKRENT: Let me make a comment, but let somebody else address the question of why genetic effects should be there.

DR. KOUTS: I didn't ask that, but that is a good question. I just wondered what it meant for one to be surrogate for the other.

DR. OKRENT: The thought was that if you have acheived a low level of somatic effects, and of course, depending on how you have done your calculations, if you haven't done it in a way so that it is not a good accounting of genetic effects, you would have at the same time kept genetic effects to a rather low level, in that sense, and so there is a word other effects somewhere in this paper.

Now, I am not sure if I have answered your question.

DR. KOUTS: Yeah, you answered it.

MR. SEGE: Thank you. Any other comments on Panel C? Dr. Eisenbud?

DR. EISENBUD: I think there are two matters that don't seem to have been mentioned, and one of them I think does bear on the question of genetic effects. I am thinking of how one deals with an exceedingly small boast or

exceedingly small risk, let us say, of the order of ten to the minus six per person, to let us say a population of four billion people. You can look -- I think I could tell them individually he or she ought not to be concerned about a risk of ten to the minus six, but if I were let us say the United Nations or the World Health Organization looking at world health generally, and I saw that there was something that could be done to prevent ten to the minus six times four times ten to the ninth cancer cases, which would be, what 4,000 cancer cases, if it could be easily done, I think I would want to take an action.

Now, the EPA has recently begun to take some of the effluents from the fuel cycle that are long-lived, particularly carbon-14, for example, and they calculate the dose not only to people in this generation, but people over the lifetime of the C-14, which you might say is 25,000 years, you take 50,000 years, and take ten half-lives, and you end up with a staggering number of people to whom you apply an exceedingly small dose, in this case, maybe your individual risk is 10 to the minus tenth, and it does involve the genetic system as well as the possibility of producing cancer, because in a case of C-14, it can have an effect on the genetic material.

And this in turn leads to the question, which has not been discussed either, of what is our obligation to future

generations, what kind of a criteria should we develop which will give us the guidance we need in planning a waste repository if we have to compute doses many thousands of years into the future, and I don't think that we have the time today to discuss these issues, but I would mention them so that they can be in the record and perhaps be discussed at some future time.

MR. SEGE: It is my understanding that Panel C did take up that issue, and I wonder if Dr. Slovic or Dr. MacLean or someone in Panel C might make a comment on it.

DR. SLOVIC: Well, I will start. We discussed that extensively. We felt it was very important, but we could not get a good handle on just what the proper ethical guidelines might be. A number of them were suggested, like you know, providing the future with a menu of opportunities that was at least as good as that of the present.

We talked about the issue of trading off future well-being versus the cost that we impose upon them in order to acheive that level, and we decided we didn't know quite how to make that tradeoff, and we discussed the -- you know, whether or not by attempting to make the risks very low, the expected risks very low, this would somehow, you know, settle the issue so we really wouldn't have to get into this, and there was uneasiness with that, so perhaps Dr. MacLean would care to elaborate. We did wrestle with it.

MR. SEGE: Dr. Cochran?

DR. COCHRAN: Well, I would like to respond to another point that Merril made, if this is an appropriate time, not the one that Paul made.

MR. SEGE: Okay. Dr., do you have anything to add for second page, intergenerational effects?

DR. COCHRAN: Well, I think Merril has raised an issue that I think represents one of the pitfalls of the diminimus approach. There are some historical examples we can look at, turn to atmospheric tests, in terms of the atmospheric test ban. The fallout in the northern hemisphere let us take a number, four millirems, from all the tests, and when you multiply that times some risk number, like 200 cancers per 10 to the 6 man-rem exposure, you get a very small individual risk, that somebody might say is diminimus compared to the other risks every day, and yet when you run that over the three billion people or so in the northern hemisphere, you come out to some 3,000 or so cancers per year from atmospheric testing, which is a fairly decent argument for going underground, and also you can add a few deaths per test, which would be sort of random violence in the northern hemisphere, but yet it is random enough that the individual risks are fairly small, so I think there are many times when it is very proper, in fact I think most times when it is very proper to do -- take into consideration these very, very small

individual risks over a very large population.

MR. SEGE: Mr. Malsch?

MR. MALSCH: I just wanted to comment briefly on that. In my experience in discussing with people, let us say, who are writing environmental impact statements, talking about the releases of long-lived radionuclides, and you encounter sharp disagreement to the proposition that you ought to do calculations of that sort, and come up with numbers of, let us say, 3,000 cancers.

They disagree that that is meaningful, and what it really comes down to is they just don't believe the linear --

DR. COCHRAN: They do if they are genetic effects where there is no controversy over the linear model --

MR. MALSCH: No, all I am saying is that I think when you go do those calculations, it forces you to do some very hard thinking about the relationship between dose and effect that you have been using.

MR. SEGE: Mr. Hutt?

MR. HUTT: Well, our panel discussed the kind of question that Dr. Cochran has raised, at some length, and I think it is fair to say that there was virtually unanimity with his approach, recognizing that you must at the same time be honest with the public about what you are talking about, and Marty, I think that gets to your point. You can't say there will be 3,000 cancers per year. You can say that is

the upper bound risk with all these assumptions. The one think that our panel felt strongly about, as Lester pointed out, was not sugar-coating news, that you shouldn't talk about risk in terms that the public can't understand, ten to the minus five, ten to the minus six. You ought to put it right out there for everybody to understand. We are talking about so many cancer cases, so many deaths per year, upper bound risk, and here is what that all means. There is no sense trying to kid anybody about it.

MR. SEGE: Dr. Wald?

DR. WALD: Don't you feel that also in order not to fool anybody, that that figure of 3,000 cases should also have the multiplication of the existing burden of cancer multiplied by that same population size, 3,000 more out of what is really a fantastic number?

MR. HUTT: Absolutely.

DR. WALD: But if you hold up just that numerator, I don't think that is being honest.

MR. HUTT: There is no question about that. A good -- and the best example we came up within our panel was what happened when Congress decided to overrule the Food and Drug Administration and permit saccharine. The Food and Drug Administration went to Congress and said there will be somewhere between zero, and if I remember the figure right, 2,300 extra bladder cancer cases per year. We don't know

whether it will be zero or 2,300 and that is in relation to the universe, and Congress, acting on that information said, that is an acceptable risk for the population. Now, I don't want to argue whether they were right or wrong, but at least no one kidded Congress as to what the issue was. That is all that our panel felt was important.

MR. SEGE: Dr. Cochran?

DR. COCHRAN: I just want to bear out another pitfall in doing what you suggested. I think that it is very appropriate to make those observations so that people can comprehend the level of risk one is referring to. The pitfall, however, is to take the next big step and infer that therefore this exposure of whatever it is is diminimus, or this level of killing is diminimus.

The problem one runs into is in that type of formulation one is weighting cost versus cost instead of cost versus benefits when one is trying to determine whether the decision makes sense, and it is much better to add up the benefits and then see if they outweigh the costs rather than weigh the costs and then compare it to some extraneous cost that may be totally irrelevant to the decision.

MR. SEGE: All good things come to a close. We are approaching 11:30, and I would like to get a show of hands from the participants on the following question; after -- before we adjourn shortly in just a few minutes, are there

participants who would wish to have the record kept open for some additional time after the formal close of the meeting for the purpose of making statements for the record that time this morning has not permitted to be made in the course of the discussion. Is there any wish to make such additional statements that participants -- if the record is open, how many people want to make a statement?

MR. ZERBOSKI: Can I ask a counter question? Do we have an opportunity to correct the record, an opportunity to edit, let us say, our own remarks?

MR. SEGE: No, ordinarily the transcripts of meetings of this sort are not circulated for correction, and it is understood that they will be an uncorrected record of a competent reporter with ordinarily not more than the form of -- Dr. MacLean?

DR. MACLEAN: Well, then I think -- well, my own view on this is that because some people have to leave right at 11:30 that it might be a distortion of the record were a smaller forum to continue to make comments on the record.

MR. SEGE: Yes, I understand the point.

MR. LEVINE: I would like to support that, and suggest that we close the record when we are finished, and that you accept from the participants any supplementary material for your own benefit that they may provide, but as all lawyers know, you ought to allow things that go into the

record to be submitted to the group for counter-argument. Otherwise, we will have a distorted record.

MR. SEGE: I appreciate these comments, so suppose that in a couple of minutes we close both the workshop and the record for now. I would then reopen it then for insertion of any written statements that people may wish to give us the benefit of having, and then these written statements, I would ask our contact at Brookhaven to circulate to the other participants, in case that should stimulate a response.

I want to thank the panel chairmen and the participants for giving us the benefit of their knowledge and wisdom and energy and vigor with which this large and complex issue of a safety goal for nuclear regulation has been approached in the past two and a half days.

The discussion has been extremely helpful to me, and I am quite sure that it will prove extremely helpful to the Commission. Mr. O'Donnell?

MR. O'DONNELL: Could I just ask one question? What is the next step in the process?

MR. SEGE: The next step in the process, is the evaluation of the report by -- Brookhaven's preparation of the report will include the participation of the panel chairmen, the full summaries of the panel reports, as well as additional summaries by the Brookhaven rapporteurs, a draft

of the report will be circulated to the panel participants. The participants will have an opportunity to make comments which then we take into account.

If any participant should feel that he cannot respond in the relatively short time available for comments, or feels that having responded, his comments are not adequately effective, we would very much welcome any additional views or comments that the participants would like to submit directly to us. I would hope that any such additional views would be in the form of a -- in the length of a letter rather than a book, so that they could receive the sort of consideration by the Commission, and, you know, some points, evaluations -- steering group -- and his comments, all you want.

If there are no additional questions, thank you very much, and again, our session is adjourned.

(Whereupon, at 11:32 a.m., the Plenary Session in the above-entitled matter was adjourned.)

This is to certify that the attached proceedings before the

Nuclear Regulatory Commission

in the matter of:

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Docket Number: Safety Goal Workshop

Place of Proceeding: Palo Alto, California

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Michael Connolly

Official Reporter (Typed)

Michael Connolly

Official Reporter (Signature)