Safety Goal Project
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NUCLEAR REGULATORY COMMISSION

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In the Matter of:

WORKSHOP ON QUANTITATIVE SAFETY GOAL

PANEL A

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING

WORKSHOP ON QUANTITATIVE SAFETY GOAL

PANEL A

Palo Alto Room
Rickey's Hyatt House
4219 El Camino Real
Palo Alto, California
Thursday, 2 April 1981

The meeting was reconvened at 9:30 a.m., pursuant to adjournment, with Dr. Herbert J. C. Kouts, Panel Chairman.

PRESENT:

Messrs. Bernero, Beyea, Burstein, Joksimovic, Levine, Kato, Lewis, Lowrance, Mazur, Salisbury, Wald.

PROCEEDINGS

CHAIRMAN KOUTS: Yesterday when we broke off, I had a view graph up here and we had discussed it a little bit. To be a little more correct, I've added a few things.

Let me just start by summarizing what I think has been some agreed upon points of views connected with the ACRS concept so far among us.

There are three hazard states proposed in the ACRS document. The first having to do with significant core damage, the second having to do with core melt and the third having to do with release of fission products following core melting. And each of these has a limit set upon it and I think we've generally agreed that Hazard State Number 1, having to do with partial core damage is simply not an implementable goal, simply not an implementable safety goal, inasmuch as no one really knows how to calculate partial core damage. Is that true, we have agreed on this?

DR. BEYEA: At this time, but the situation could change.

CHAIRMAN KOUTS: The situation could change. It will be difficult to do this but at this point it's difficult to see this as a --

MR. BERNERO: I would just add to clarify we have some work going in that regard, trying to make that distinction. I don't know if it will be successful or not.

DR. LEWIS: As a general comment heard, though, just a general one, not specific to this, I don't think it's a good idea to ever say one cannot calculate something because it's never true in the absolute sense and people on the outside aren't as learned as the people who make that comment and think that it really is true that you can't calculate it, so you know, some qualifying word such as you can't calculate it as well as you'd like or you can calculate other things, is probably more honest and useful.

CHAIRMAN KOUTS: Allright. The uncertainty with which you can judge that you have met this criteria is very high.

DR. LEWIS: Yes. That's right.

DR. MAZUR: That's a better way to say it.

CHAIRMAN KOUTS: And as Carl, having recognized this and perhaps even dominating it not corollary to it is an acceptance of a view among this group that you can't calculate it reliably as well. You should not use it as far as safety goes. Is that an acceptable view also?

DR. MAZUR: It is if you can't calculate it.

CHAIRMAN KOUTS: Then it should not be part of the safety bill.

DR. BEYEA: You should try and avoid it. In some cases it may be unavoidable. Some calculations are.

CHAIRMAN KOUTS: It's a question of how useful is it

if you can't depend on it.

DR. MAZUR: If you can define its limits of uncertainty it can be useful.

CHAIRMAN KOUTS: I suppose if you can show that something is microscopic even with all error bars included to be microscopic then you can say its microscopic and use it that way.

DR. MAZUR: I would certainly say in a pragmatic sense I would definitely have no trouble going on but it just seems to me that there's some broader ways of interpreting that I would like to remain agnostic on without much time to think on it.

MR. LEVINE: Could I say something?

I think if you look at all the other elements of safety laws proposed by ACRS, there are models that are in existence that will allow you to calculate them. There is none for severely damaged cores at this time. And, I think it's a research -- I'm not saying that it can't be -- to calculate that. I think it's a research program to look into that.

DR. MAZUR: But what that's saying is you can't calculate the likelihood of having a severely damaged core, or let's just say that if you couldn't calculate the likelihood of having a severe release --

MR. LEVINE: A what?

DR. MAZUR: A severe radiation release then you shouldn't have a safety goal --

MR. LEVINE: Oh, yeah, I think if we had not done Wash-1400 this workshop would not be being held.

If those models did not exist we wouldn't have these conversations, at all.

CHAIRMAN KOUTS: No follow-up on that. Bob?

MR. BERNERO: I want to offer what I think might be a clarification by putting the proposed Hazard State 1 and Hazard State 2 as they exist in the ACRS report, into a somewhat different set of words.

What those two hazard states together are really saying if you look at the numbers, are that if you get the core into trouble, you should be able to stop short at least 4 out of 5 or 2 out of 3 times at core damage without large scale core melt.

That that would describe the capability of plant design and operator errors or operator intervention, that would be a level of protection.

DR. BEYEA: Can you define what core -- coin what core trouble is more precisely?

MR. BERNERO: Yes. They said that they described it in terms of cladding perforation and radioactive gas release. The theoretical distinction and the existence of a goal for it might have validity even though you can't

at this time calculate it and I think one should be careful to say if nobody knows how to calculate it with confidence today, does that necessarily or obviously foreclose the ability being developed tomorrow and a worthwhile additional goal being adopted?

MR. LEVINE: There's a second question about that. I agree with your question as another question, what does it add to the whole framework of things? If your primary motive in setting goals is to protect the safety of the public. Clearly the primary goals are those which affect the health of the public and these engineering goals must be secondary to that.

DR. MAZUR: I think though that goals are something we strive for, right? Wasn't that part of the discussion even though we might not have one at the moment?

MR. LEVINE: Goals are things to strive for, but you know, it's, I can't be postulating goals when no one has the foggiest notion of how to go about determining whether he meets it or not.

DR. MAZUR: Well, I'd rather sort of postulate a goal and then find out we can't handle it and then say okay, pragmatically we can't operationalize that today.

MR. LEVINE: Well, we know that today.

DR. BEYEA: Do we have to decide this now? Can we just say that Hazard 1 and Hazard 2 should be combined or

is this debateable? You know, are we going to -- should we get into debate at this point?

CHAIRMAN KOUTS: Any conclusions we can draw on this particular thing would be valuable because we're dealing with the question of how should safety goals be structured? What should be included in safety goals? And, I think this really is a fundamental question as to whether or not you should include in safety goals things that you know or don't know at present how to calculate with the reliability that you would like to demand.

MR. SALISBURY: May I ask for a little clarification here. I'm not sure, Saul, if what you're objecting to is the concept of hazard states per se, or the specific hazard states that are mentioned in the ACRS document, because it seems to me that if you can't -- if there isn't some kind of a sort of hazard state that you can define and then you can have some kind of an estimate for to me as a lay observer, that seems to imply which some other people have said in various ways, that you can't really determine these individuals risks and societal risks as well as you can if there are points within that reactor sequence, accident sequences and so forth, that you can't define well enough to make these kinds of estimates about.

MR. LEVINE: Let me try to respond to that.

I'm not objecting to hazard states in principle at

all. If you have that impression I would like to ---- you.

MR. SALISBURY: I wasn't sure.

MR. LEVINE: There are 3 hazard states specified. I'm objecting to 2 of them, not one of them. I also suggest that the hazard states are secondary considerations compared to the primary considerations, though it's really protecting the health and safety of the public and are handled by the individual - societal risk statements.

MR. SALISBURY: I think there is, too, I would agree that they are secondary but I don't think they're necessarily negligible.

MR. LEVINE: ---- I'm saying secondary, not negligible.

DR. BEYEA: At some point I would like to take a few minutes to come back to the hazard state question. Is this a good time to do that or should I wait until you've decided whether Hazard State 1 or 2 --

CHAIRMAN KOUTS: Well, we're dealing with a more general question about calculable and non-calculable aspects of criteria.

Do you have a comment on that?

DR. MAZUR: Yes, I do. I can visualize this. I can visualize our stating as a safety goal a low probability for some hazard states and we can't A-priority calculate the likelihood of that happening. However, it may be that

in ten years we'd have enough of those occurring that we are able to on an experiential basis say hey, that's over what our limit is so we better go back and figure something else out. It seems to me that's a very meaningful kind of a safety goal even though we can't A-priority calculate the probability of reaching it. But we might after the fact as the results of some unfortunate experiences decide we exceeded --

DR. JOKSIMOVIC: But we cannot afford that experience.

DR. MAZUR: Well, maybe we can't afford it but we might end up with it anyway.

MR. SALISBURY: Well, it seems to me to be more valuable that if there are hazard states that you can define you know, or hazard states --

DR. BEYEA: Well, you can define this state.

MR. SALISBURY: You can define and make a kind of an estimate of the probability of, that's more valuable then --

CHAIRMAN KOUTS: Let me clarify the situation if I can with respect to Hazard State Number 1.

We can certainly calculate probability of occurence of Hazard State Number 1 in any given application.

In order to do this calculation, we'd have to make a number of assumptions, and a lot of these assumptions

will be relatively adhoc. If we want to be sure that we're underestimating the probability that Hazard State 1 occurs, we will be a little conservative with respect to a number of these assumptions.

If we introduced all the conservatisms of this kind, we will find that we're calculating the probability of total melted core. That's the whole situation, because, probably, there may not be very much difference in probability between melting 30% of the core and melting 100% of the core and it's this range between 30% and 100% that's being discussed here. This is the difference that's being talked about.

MR. SALISBURY: It seems to me that as far as the safety goal would go, that's more of a technical difference that to me, anyway, doesn't have that much implication.

CHAIRMAN KOUTS: There's even an arbitrary character to the selection of 30% of the core.

MR. SALISBURY: Right, right. It would seem to me that just having a single hazard state which would be major core damage or something like that, assigning the probability of that would be equally as functional as far as a safety goal as dividing it up in a way --

DR. BEYEA: When you say major core damage it is not quantitative anymore.

MR. LEVINE: But there's another way to think about that. Maybe this will help. If you have a core that doesn't melt but is severely damaged, that core does not in and of itself threaten and taint the integrity of the containing building. It's only when you melt the core that the containment building integrity is threatened in a dependent way to the core building.

DR. BEYEA: Would you repeat that statement?

MR. LEVINE: It's only when the core melts that the integrity of the containment building is threatened in a dependant way, that is, depending on the core melting.

DR. BEYEA: Would there be enough steam pressure to burst the --

MR. LEVINE: When the core is not molten, the containment integrity is not threatened.

DR. BEYEA: Well, if it's -- I won't accept that right away. I'll wait on that.

MR. LEVINE: Well, I guess there are a few exceptions. There are a few exceptions where steam pressure can break the containment, but that always happenes in such a way that the core then melts, so --

MR. BERNERO: Excuse me, Saul. The little containments and hydrogen is the other.

CHAIRMAN KOUTS: But what Saul is saying is that when you break the containment you lose the cooling

quality --

MR. LEVINE: You melt the core anyhow.

So, if you're talking about a severely damaged core, you're talking about very little threat to the public which is our primary concern because if the containment holds together, very little is going to get out. Now, I'm leaving out the psychological problems such as those that happened at TMI.

So I see there's a great significant difference between Hazard State 1 and Hazard State 2 in terms of the real effects on people.

DR. MAZUR: I'm a little confused on the levels we're going, so I might well accept what you're saying about that particular thing, but in jumping from that particular case to a broader policy issue and that is that one shouldn't have to include in safety goals things that one can't calculate.

MR. LEVINE: Let me just explore that a little bit.

Reg Farmer proposed in 1967 a -- it wasn't a safety limit but people call it a safety limit and it's called the limit line, and it wasn't used very much by anybody because one didn't know how to make the calculations very well. They set up some models in the U.K. for using it for siting practices, but no one knew how to calculate accidents that

went beyond the -- design basis accident that is the heart of the regulatory process of the U.S.

This was not known until Wash-1400 was done and we would not -- what I said before, we would not be sitting here today talking about how to calculate public exposures realistically if Wash-1400 had not been done.

DR. MAZUR: Well, one way of knowing whether you've met those criteria without Wash-1400 is after a certain amount of experience, you can count the number of reactors that may have had those experiences and then you might be in a position to say that's more likely than --

MR. BURSTEIN: Then the problem is there is no such data base.

DR. MAZUR: No, but there may be 15 - 20 years down the line.

MR. LEVINE: But if our probabilities are right there might not be.

DR. MAZUR: Well, I'm frankly one of those people who isn't that trustful.

MR. BURSTEIN: Do you suggest we abandon the session and wait for 20 years?

DR. MAZUR: No, but I am suggesting that it might well be an idea not to foreclose the notion of defining a goal which we may be in a position in a few years to say--

CHAIRMAN KOUTS: In 15 years, that data base will

only be about twice as large as it is now.

DR. MAZUR: But the accident base might be substantially higher.

CHAIRMAN KOUTS: Maybe not, but that's ---

MR. BURSTEIN: But I heard you say, excuse me, Mr. Chairman, but I heard you say in answer to my question which your answer was, let us wait and see.

Now, that means we shouldn't be doing anything now because we haven't enough data on which to do it.

CHAIRMAN KOUTS: For Hazard State 1.

MR. BURSTEIN: I understand. But it gets back to something we discussed today perhaps in this respect, whether in order to achieve the goals we are really concerned with which happen to be those related to individual and societal risk, whether we should be fooling around with the intermediate hazards, the close-calls, the challenges, the frequency and perhaps the extent of those challenges to the systems that might cause the individual and societal risks to become a statistic and I guess perhaps part of the question we should address again is whether we need to have arguements about the number of challenges.

Really, what we're basically interested in is the actual probability and degree of individual and societal risks.

Do we need to define the mere misses?

MR. LEVINE: Do you have to define the failure probability of valves to operate those?

CHAIRMAN KOUTS: I'll take Hal Lewis.

MR. LEWIS: I just want to comment I think that's an excellent point and we're getting around it because it seems to me -- well, first let me just disagree with something that Saul said earlier which is, I think you can compromise the containment without melting the core and we can talk later about specific scenarios. I think one shouldn't be too flat about statements which may not be entirely true.

MR. LEVINE: I take it all back.

MR. LEWIS: This point is well taken. Before we discuss in any great detail whether we can calculate and we can calculate well or not the graded core states, the question that was just raised of whether we should go along on hazard states, I don't think we have a consensus on it and I'd like to make a point on it.

To disagree with another thing Saul said earlier and everyone has been saying, which is that our primary intention is the next level which is the protection of the health and safety of the public, I think we all agree that if not a secondary at least a parallel objective is to make the regulatory process more rational. Now, I don't believe that you can make the regulatory process more rational without inevitably improving the health and protection

of the public, and in fact, I would regard it as essential to do so and the idea of setting goals on hazard states I think goes to that point, to making regulatory processes more rational, not to determine a better approximation of the end state.

Just to use an analogy, I think we all get students who come in and say, they want to study physics. Why? Well, they want to understand general relativity but they don't have time to study the elementary mechanics and electricity, they just want to go right ahead and study general relativity. By the same token, I don't think you can do a good job on that next level which is the protection of the health and safety without making the regulatory process more rational. Now, whether setting goals at intermediate levels instead of a final result is a good way to do that I'm not commenting on, but certainly that's the context in which we ought to be talking about this. Period.

DR. BEYEA: I would like to speak in favor of the hazard state concept and I'd also like to go a little further and the fact that having thought about it last night and this morning, I think now that there should in fact be intermediate levels on hazard states. I'd like to also indicate a formulation of the problem which Tom Cochran gave me which I think is very useful. I'd just like to put

this on the board as a way of perhaps handling the discussion about hazards.

If you look at the individual risks and we call that something "Z" and if you have it on a large scale and make it a "Z" it's combined by two effects. And we have some uncertainty - "X" and some uncertainty "Y" and of course there's some uncertainty in "Z".

CHAIRMAN KOUTS: Are these probabilities which you're writing?

DR. BEYEA: No, it's on a log scale.

CHAIRMAN KOUTS: Yeah, this would be on a log scale. These are sequential probabilities.

DR. BEYEA: That's right. We might have in here -- core -- some joint probabilities. And then of course there's some uncertainty in "Z" and one way of setting the limits of how well you want to deal with certain issues, this must be -- mutual to some number - "W" and if you make this your criteria then it would show up uncertainties in Sigma X and Y as you want, as you please.

The alternative is to expect this each time to less than the sum WX and less than or equal to sum of WY. All right, now my own feeling now is that I'm nervous about this concept because that which I'm most happy about is experience. And the question is how well do I know Sigma X and how well do I know Sigma Y? It seems to me that

based -- where I have a lot of experience, I'm going to have more confidence.

For instance, the containment system. There is no way that anyone is going to convince me that we don't need containment systems because we've had some experience with them and some alternative containment systems, I would be worried about, I would not have much confidence in the analysis because there wasn't enough experience with the analysis. At least to the extent of the containment system, maybe with ECCS, I'm not sure, but I think now I withdraw my yesterday's agreement with the idea that when it goes to a WZ formulation, I would now like to see restriction on at least certain ways of breaking up the problem.

CHAIRMAN KOUTS: What you're saying, you'd be uneasy about retiring to Hazard State Number 3 completely. That you feel there ought to be something beyond doing just that?

DR. BEYEA: I think that the hazard state concept is useful and I think more than that, I think we should in fact put specific goals on each, certain states.

CHAIRMAN KOUTS: Well, that's one way of not retiring to Hazard State 3, but --

DR. JOKSIMOVIC: I think, what if I may interpret what he's saying is that if we end up with individual risk societal risk and not have hazard states, you'll be uncomfortable?

CHAIRMAN KOUTS: No, no, he will even accept hazard states -- what he's talking about is whether or not we should have simply a hazard state on melting a core plus bio- you know, the 10^{-6} probability that Bob put up there yesterday.

He says he's uneasy about that. He'd rather see the restriction on the core melt and another restriction on the probability of containment failure because he feels he's got successive protection this way and he feels he can depend more on the individual calculations, but what I'm saying is that maybe you don't have to go that far if you feel that unsure about retiring just to Hazard State 3. Maybe you could put limits on how far you could push the individual components.

MR. BURSTEIN: Isn't that what we do when we talk about components that go beyond that. My question simply put is why not get more comfortable by going farther down.

If a degree of security or comfort is what you're after, why not look at the ECCS system and put a number on it and why not get to the high pressure safety injection pump or the valves that you mentioned and put a number on that and, you know, the degree of comfort now gets to be perhaps the implementation of what the overall goal is and, the question of whether you're setting up engineering

criteria or whether you're setting overall goals and perhaps that might help draw the line as to where you're going to start setting the limits.

I can go down the list all the way to individual pieces of hardware, individual operating and maintenance procedure and write a very detailed specification rather than a goal or an objective.

DR. JOKSIMOVIC: Those would be reliability goals rather than safety goals.

DR. BEYEA: I'm not asking for going down to every last valve.

I think that's a mistake.

MR. LEVINE: I understand. I think there's another thing to consider.

Suppose -- and I want to talk about Hazard States 2 and 3.

CHAIRMAN KOUTS: That's really what we're talking about.

MR. LEVINE: Suppose it's decided for economic reasons by a reactor manufacturer or a utility manufacturer or whomever, that they are going to design a reactor that can be analyzed to have the core melt probably not to 10^{-4} but to 10^{-5}. Would you still want the Hazard State 3 to be two orders of magnitude different from that or only one order of magnitude would be enough?

DR. BEYEA: I want two orders of magnitude.

MR. LEVINE: Independently?

DR. BEYEA: Independently because I have the -- because of my lack of confidence, my lack of confidence in the calculation 10^{-5}.

CHAIRMAN KOUTS: That's the position that the ACRS document takes. They say they are unwilling to take 10^{-4} for core melt because there are questions connected with seismic problems, fires, etc.

DR. BEYEA: But that is to cover uncertainty, isn't it?

MR. LEVINE: That's to cover inadequacies in certain models.

CHAIRMAN KOUTS: Vojin?

DR. JOKSIMOVIC: I said something yesterday but I'd like to say it again today, which is basically my position on hazard states in general. And I'll read from my notes to be precise.

These are reliability or -- goals and as such it could be effectively argued that they should be outside the NRC's --. Mainly, they should be established but left to the industry and the insurance industry for implementation. NUREG-0739 recognizes this point but proposes them on the basis that A) they represent forerunners of most serious accidents, B) public and utility losses, and

C) potential traumatic effect on the public, and I would like to add Lewis's point D) which is they would contribute to more rationale in regulatory process.

Based on that, I would be willing to abide them, however, they in my opinion, are unnecessarily detailed, and a simple goal on the severe core degradation of something like 10^{-4} and perhaps another one to the -- mitigation would be perfectly evident.

CHAIRMAN KOUTS: In your last sentence you took back all you'd said before because you were opposed to having core degradation criteria and now you're willing to accept it. Isn't that true?

DR. JOKSIMOVIC: My last sentence is given that there is a need for -- these goals in essence are not public safety goals and ----- that they could be left to the utility industry or insurance industry for implementation.

However, given the points that have been made in NUREG-0739, the point that Harold made a few minutes ago, I'm willing to abide that, but I'm saying that as they're stated in the ACRS document in unncessary detail, and I would be happy with a simple goal on a core degradation and perhaps another one to reflect mitigation like condition of probability for release as opposed to necessarily relating that to the containment we discussed last night.

CHAIRMAN KOUTS: Just taking your view that severe core degradation should be left up to the industry on a logical basis as a goal, I think you'd find great difficulty in convincing public or NRC or Congress or anyone that the probability of things like TMI should not enter into safety goals.

DR. JOKSIMOVIC: Well, I think that the --industry and the insurance industry has learned a great deal from TMI and they don't --- any longer.

CHAIRMAN KOUTS: ---- would not satisfy the Congress, I'm sure.

DR. JOKSIMOVIC: But also, you know, one point I failed to mention is I believe that NRC should have jurisdiction of making spot checks but not necessarily doing it on application by application.

DR. MAZUR: I wonder why one makes the assumption that there is no harm to the public unless there is radiation leaks because it seems to me we have clear evidence to the contrary.

DR. JOKSIMOVIC: What happens when a dam breaks?

DR. MAZUR: There can be substantial harm to the public if there is a public -- dam. I'm not sure what the relevance of that is. If you have a serious malfunction that is reported, even though there is not a radiation leak, and you know quite well that that causes public harm

DR. JOKSIMOVIC: You mean stems from emotional stresses?

DR. MAZUR: Emotional stresses and others.

DR. JOKSIMOVIC: I wouldn't argue that, but --

DR. MAZUR: It's more than emotional, by the way, because the whole public response itself can bring about actual physical stresses like attempts to flee, to panic states, physical accidents, things of that sort. So right off the bat, by anything other than the most narrow definition of public harm, there surely is public harm with an accident like that.

DR. JOKSIMOVIC: Are you saying that we can't have any accidents?

DR. MAZUR: No, what I'm saying is an accident that is at a sufficient level to cause public concern and psychic and related physical effects even though there's no released radiation, surely that gets into the issue of whether or not we're protecting public health, that's point number 1. Point number 2 which is Harold's point is quite a separate one and that is given that we've had one of our criteria for the purposes of our safety goals is just simply rationalizing the regulatory procedure when one needs a demonstration of public harm, anyway, but it seems to me that the levels of accidents we're talking about satisfy both of those. There is public harm associated

with many of them even though there is a release of radiation. There seems to me by either way they fit within the curve of what we call the purposes of our safety goals.

DR. JOKSIMOVIC: So you would see no reason why they should be excluded?

DR. MAZUR: No, well, I could see some reasons why one might not want to choose the particular hazard states they have. I'm surely not denying them but, no, I see no reason to exclude some kind of accident below the level of radiation.

DR. JOKSIMOVIC: But you would agree they are of secondary importance as far as the public health and safety.

DR. MAZUR: I would agree that any lesser accident is of secondary importance to a more serious accident. Of course, that goes without saying. But I wouldn't agree that they are secondary in the sense of not to be considered.

CHAIRMAN KOUTS: Bob?

MR. BERNERO: I wonder if I could somewhere in here give this panel a clarification I promised to obtain yesterday.

If you recall, when I wrote on the board, I stated my continuing interpretation of the ACRS Strawman Safety Goal, that there was clear logical linkeage between Hazard State 2, Hazard State 3, and the probability of early

totality. 10^{-4} times 10^{-2} equals 10^{-6}. I asked Dave Okrent this morning and he said no.

DR. JOKSIMOVIC: Sounds right.

MR. BERNERO: He embellished on it by saying that if we felt that if you met Hazard State 2 goals, and Hazard State 3 goals, and had evacuation you would easily have met the early totality goal.

DR. JOKSIMOVIC: I agree with that.

DR. WALD: That makes a lot of assumptions about evacuations.

DR. BEYEA: Well, it wasn't perfect evacuation, it must have been some evacuation model.

MR. BERNERO: I don't know. He did not embellish on it. But to obtain the clarification I said I would, he and his colleagues did not integrally link the goals for Hazard State 2, Hazard State 3 and early totality as I had thought they did.

CHAIRMAN KOUTS: Then there was less rationale behind the choice of these values than I thought was there.

MR. LEVINE: That's what we've been trying to tell you.

DR. MAZUR: I want to define the level of debate here if I can. I hope we're not debating on whether those particular hazard states are ones that should be in there,

so much as we're stating in general --

CHAIRMAN KOUTS: I'm not going to fall back on asking a different question. I think I have the correct answer, too, and I'd like to check it out.

Do we now agree that there should be, that it is reasonable, then let's put it that way, it is reasonable and perhaps desirable to have safety goals linked to hazard states?

MR. LEVINE: Yes, independently of what they are. Without specifying.

CHAIRMAN KOUTS: That's fine. That's a good conclusion.

I think that we could probably agree that among these hazard states, there should be one connected to core melt probability.

Beyond this it requires some discussion.

DR. MAZUR: And expertise.

CHAIRMAN KOUTS: And expertise. And that's as perhaps far as we can go.

DR. BEYEA: Can we go further and talk about whether there should be a hazard state release to release after melt?

CHAIRMAN KOUTS: Shall we ask if there's agreement on that?

MR. LEVINE: I would not agree with that.

CHAIRMAN KOUTS: Should there be a hazard state connected with substantial release of radiation from the facility? That's really the hazard state --

MR. LEVINE: That's what they define.

DR. LEWIS: So that isolates the point between substantial release and damage to the public which means potassium iodide, and it could be others, and evacuation.

MR. LEVINE: Not necessarily. They define the large release as 10% of the core -- inventory and 90% of the --. That's a large enough release to cause significant consequence. Not the largest consequence. But significant consequences.

DR. LEWIS: No, I understand, but the purpose -- I'm just really now confused. The purpose of isolating specific points is to provide, if I understand what they wrote in the report, is to provide an incentive for intervention somewhere along the line short of ultimate catastrophy.

CHAIRMAN KOUTS: Absolutely. They call this a criteria mitigation. But between a substantial release and damage to the public, you're talking about things that include not just potassium iodide obviously, but sheltering, evacuation and things like that. So it's specifically to provide incentive to do things at that level that would include a significant release as a hazard state. Am I wrong? I'm really asking for information.

DR. BEYEA: I don't see that at all. It would seem to me that by putting another hazard state on, you'd make another restriction, like perhaps you had to have a container.

DR. LEWIS: Yes, that's right.

DR. BEYEA: There would be some restrictions --

MR. LEVINE: There already is a requirement for containment which the safety goal is not going to take a way.

DR. BEYEA: I'm sorry?

MR. LEVINE: There already exists a requirement for reactors to have containment buildings that this safety goal is not going to take away.

DR. BEYEA: It could if you -- it could if you just had a formulation where you had the hazard state which included core melt and a hazard state which looked at the hazard to the public. You could argue that you don't need a containment to meet those criteria--that you could have some other -- you could have sprays, you could have potassium iodide and evacuation. You would not need containment and still meet the overall risk goals.

MR. LEVINE: Why don't we put down that instead of the probability of a large release, write down that you have to have containment if that's what you want. I'd be satisfied with that.

DR. BEYEA: That would be another kind of hazard state then.

CHAIRMAN KOUTS: Now you're in the design criteria. Yes, Bob.

MR. LEVINE: I'm not trying to get rid of containment.

MR. BERNERO: I wonder if I could ask Hal -- what you just said a few minutes ago seems to raise the possibility of hazard state 4 which would be another conditional probability that perhaps would say something like, given a large scale release defined as what we defined it, the probability of an early totality anywhere on site shall be less than 0.1 or .01 or something.

DR. LEWIS: I think that's assumed in group 2 which is the public health consequences.

MR. BURSTEIN: Isn't that indeed what is said here?

MR. BERNERO: No, group 2 or rather group 3, Hazard State 3 is the conditional probability of release, large scale release given large scale fuel melt but that is not by Dave Okrent's clarification synonymous with early fatality.

DR. LEWIS: I understand, Bob. What I'm saying is after we get through the first table which is the hazard states, the next table includes limits on risks to the public,

MR. LEVINE: And they include considerations of sheltering.

CHAIRMAN KOUTS: I think now there's a duplication beginning to appear. Clearly. The question is, do we need this duplication.

MR. BERNERO: But in effect, are not these criteria, do not the criteria in their structure leave out altogether emergency public protection measures -- that one doesn't even need to consider that. The hazards states and the public protective measures might consider them, but there is no incentive built into the safety goal either to consider them or to have a performance standard for them.

DR. LEWIS: If there were no safety goals other than the hazard state safety goals, that would be true but when there are standards in addition to the hazard state goals, that is not so.

DR. LOWRANCE: Mr. Chairman, I suggest that we go onto the next category.

CHAIRMAN KOUTS: I'm prepared to do that. I think I've gotten about as much agreement --

MR. LEVINE: I think you've gotten about all you can get.

CHAIRMAN KOUTS: From now on its down hill.

MR. LEVINE: I wonder if I could ask Allan one

question that's a little bit off the track but I'm interested because of things he's been saying.

It's my opinion and it's strictly a lay opinion, that the psychological damage at TMI which I agree there was, was caused by the mishandling of the situation by the NRC and the public statements that were issued. And they were honest statements, but uninformed. Suppose they had not been made and the public had not been told that the reactor was going to blow up at any moment. Would the psychological damage have been much less?

DR. MAZUR: Sure, of course.

MR. SALISBURY: But it wouldn't be zero.

MR. LEVINE: But very much the order of magnitude--

DR. MAZUR: Oh, I fully agree with your analysis of the cause and sequence.

MR. LEVINE: Okay.

MR. SALISBURY: The question, would there have been more radiation released --

MR. LEVINE: That would be different.

DR. JOKSIMOVIC: For instance, in the Soviet Union, the emotional stress is zero when they have accidents.

Nobody knows about it.

MR. LEVINE: We ought to not publish reports of these things when they occur.

DR. MAZUR: That is not a corollary.

DR. BEYEA: So as I understand it it is the NRC's responsibility for causing the stress at Three Mile Island? Is that what you're saying?

MR. LEVINE: Not all of it but I think a large part of the stress was caused by the fact that someone in the NRC told an Associated Press or somebody, reporter, that the reactor was going to blow up at any moment.

DR. MAZUR: Well, I think there's more to it than that. We don't want to make it that narrow.

MR. BURSTEIN: But surely the fact that information, alarming information which in retrospect shouldn't have been so alarming, but maybe some of it should have been, got out and got reported and was clearly the thing that led to the psychic stress.

DR. BEYEA: I think you can argue in the causal chain that it might be the operators and management of GPU who were responsible. You put the causal chain back on and it seems to me the NRC operated in a prudent and responsible manner by assuming the worst.

MR. BURSTEIN: I don't want the record to hear that that's the final word. Certainly they did not help and we ought to go onto the next item.

MR. LEVINE: Why don't we go on? I'm sorry I asked the question.

CHAIRMAN KOUTS: Now in the ACRS proposal there are

two aspects to individual risks. First of all let me say that the document itself is not at all consistent to individual risks. The criteria are stated in terms of fence post individuals and the calculation which is given as an example is not that of a fence post individual, but is essentially the average person in the low population zone, or something like that. I think that's a detail we can recognize but it probably doesn't influence what we want to talk about here because that really is related to other numbers that were calculated.

There are two aspects of the risks. The first of these is the probability of an early death due to a reactor accident over the lifetime of an individual at the fencepost. This is the way the criteria is stated.

This is essentially the same criteria as is used in part 100.

The second is the probability of a delayed death by cancer due to all reactors at a site over the lifetime.

MR. LEVINE: I'm not sure I know what you meant by a comparison on part 100. The fencepost element?

CHAIRMAN KOUTS: Yes, the fencepost element. Part 100 is related, well, I maybe went a little too far there. There is a vague relationship of part 100 to the early aspects.

MR. LEVINE: The latents are calculated as fenceposts?

I'm not sure what point you're trying to make.

CHAIRMAN KOUTS: No point.

MR. LEVINE: Okay. Forget it.

CHAIRMAN KOUTS: It was unimportant.

MR. LEVINE: Okay.

CHAIRMAN KOUTS: So there are really two criteria connected to individual risk and this is the ACRS proposal.

Now I guess the first thing we ought to take up is whether individual risk, the most exposed individual, is suitable for criteria.

MR. BURSTEIN: I would say yes. In fact, I didn't hear any disagreement on this in any of our discussions.

MR. LEVINE: Others have proposed different criteria. Chauncey Starr, for instance, talks about how to think about risks to people in a geographical sense, that is the people nearby the reactor, 10,000 people nearby the reactor and then people in the general region of the reactor perhaps 100 to 200 miles.

I find that more satisfactory than most exposed. I'm not sure I can prove why, but I just find that concept is attractive to me.

MR. BURSTEIN: Isn't most exposed then, really the worst case for the average within the low population zone.

CHAIRMAN KOUTS: No, most exposed is fencepost person.

MR. LEVINE: Except they don't calculate it that way.

MR. BURSTEIN: It's a surrogate for that.

CHAIRMAN KOUTS: There's certainly a link. It wouldn't be varied place to place.

MR. LEVINE: I think we have to straighten out in the ACRS proposal if they really mean most exposed or if they really mean the average person within a short distance of the reactor.

CHAIRMAN KOUTS: Before we do that we have to determine this is an important distinction to make at this point or do we believe that a criteria having this general character is appropriate. I think we can agree on the general character aspect.

DR. JOKSIMOVIC: Do we have the benefit of Mr. Griesmeyer in the audience who is one of the authors in reports?

MR. GRIESMEYER: There's a footnote on page 62 in the document which describes in a little bit more detail of what we meant by most exposed individual. It's not exactly the fencepost individual. I don't know if that clarifies anything.

CHAIRMAN KOUTS: You mean there are other people who might be exposed more than the fence post individual?

MR. GRIESMEYER: We don't intend for it to be quite

that restrictive. The footnote explains it.

MR. LEVINE: It says averaged over a representative distribution of the population.

MR. BURSTEIN: Yes, that's what my understanding was.

MR. LEVINE: Then I think that the term most exposed is unfortunate.

CHAIRMAN KOUTS: I think so too, especially since that particular footnote only appears in one of the appendixes to this particular report, whereas the thing that has really been regarded as the ACRS proposal is this thing up in front which is the letter which was transmitted to the commission and in the letter which was transmitted to the commission, the only person discussed was the most exposed individual.

DR. WALD: Isn't the issue really, if we accept that it is important to have the criterion damage to the individual in the early death as the representative case, isn't the issue that you're really addressing about conservatism in the limits?

And when we're discussing about whether we're talking about the fencepost individual or an average individual at some --

MR. LEVINE: Five miles.

DR. WALD: At some distance, we're talking about

whether the limits should be conservative or not it appears to me.

DR. JOKSIMOVIC : But I think most of us would like to have a clear definition.

MR. BURSTEIN: I think we're recognizing the fact that there is a difference with distance.

DR. WALD: But in defining our quantitative safety goal, are you opting for designing the protection to meet the needs of the fencepost individual or are we settling for an average -- at some distance. This is the issue of do we put the conservatism in this and if we do, identify it, it seems to me.

CHAIRMAN KOUTS: Well, this is a departure from one part-100 type calculation. Because in part-100 you do calculate the fence post individual.

MR. LEVINE: I'd like to ask another question of Griesmeyer because inspite of what you said, I understand what you said about the footnote, but there's another place in here and I can't think of where it is but it said that it points out that the most exposed individual concept refers to a very few people and I think those words were used -- just a very few people. So it's apparently stated two ways in here or --?

MR. GRIESMEYER: Unfortunately, this was written by a committee.

MR. LEVINE: Are you saying the intent was not fencepost?

MR. GRIESMEYER: The fencepost individual wasn't there 24 hours a day.

DR. JOKSIMOVIC: What was the concept of the most exposed individual?

MR. LEVINE: How as the calculation --

MR. GRIESMEYER: Well, the calculation would be people who spend their time around this area. The most exposed individual might be somebody who works in the factory next door and he's only there 8 hours a day. We're not saying that you sit on the fencepost 24 hours a day.

DR. JOKSIMOVIC: For instance, at Three Mile Island, my recollection is that the most exposed individual was a fisherman on the island who received something like 80 milligrams. Is that the concept you had in mind?

MR. GRIESMEYER: That would be accurate.

CHAIRMAN KOUTS: Was he the most exposed individual?

DR. WALD: He was. That was a real most exposed but that was also a calculation for the fencepost individual who in effect was at the North Gate for 24 hours without moving and that was the 80 MR but the actual fisherman on the next island was actually less.

The point I wanted to make is in appendix A where this acceptance criteria used the application on page

105, the calculations presented here are not strictly for the maximum exposed individual, but in most cases they are, however, appropriate for most individuals near the plant. I'm not exactly sure what that means.

In the actual run through of the application of the criterion, there are two cases. In one, the individual is located initially within half a mile to one mile of the reactor and in case two an individual is located one to one and a half miles from the reactor.

DR. JOKSIMOVIC: That was done by another member of the committee.

MR. LEVINE: Another thing to point out -- in actual accident calculations there are some cases where you have a hot gas release where the cloud goes up over the people nearby and comes down later and so the most exposed individual in that sequence is further away.

CHAIRMAN KOUTS: You have an elevanted release.

DR. BEYEA: But when you average over all weather conditions then you tend to at least be flat and --.

MR. LEVINE: Right.

CHAIRMAN KOUTS: Bill Lowrance has a comment.

DR. LOWRANCE: I'd like to think a little bit about the tie between these hazard state goals and individual risk goals and how those relate back to plant design. In some ways there's no real difference if you -- because there's

a lot of overlap in the implications between the hazard state statement and the individual risk limit statements. Would somebody draw out for me why we need a second set of limits -- that is, individual limits. Individual risk limits. Why don't we just stop at the hazard state limits, that is, the release limits and things of that kind?

In what ways are these not redundant?

What does the designer or regulator do differently because he now has a second batter of considerations?

I'm assuming that we could eventually develop a really sophisticated set of hazard statements and goals.

CHAIRMAN KOUTS: I'll accept that. That's a good question.

MR. BURSTEIN: Among other things, it goes to how we deal with siting criteria.

DR. LOWRANCE: That's what I see.

DR. JOKSIMOVIC: You're also telling people what their risks are.

DR. LOWRANCE: I can derive that from the hazard states and information about the site.

DR. JOKSIMOVIC: Well, you can but not that many people can.

DR. LOWRANCE: Okay. But it's conceptually derivative.

DR. BEYEA: Isn't it the other way around?

DR. JOKSIMOVIC: It's a matter of -- you have to set a goal which is going to be acceptable and you have to come up with some kind of number which is going to be an acceptable number. We have to argue hard for rationale behind that number.

DR. LOWRANCE: We could say then let's adopt a set of individual hazard limits and the overall societal risks and then we'll derive, we'll go back to hazard states from that. I'm not saying that there aren't implications from going on --

MR. LEVINE: I think there is in fact in the goals as stated here the hazard states versus the public protection goals, not a one to one correspondence, but you cannot, if you take these hazard states derive these numbers or vice versa. They certainly aren't in most of the goals.

I think you need two sets, the primary one to protect the public and to say that in terms of protecting the public and the other one to help more in the other objective of safety goals to make their licensing process more rational.

CHAIRMAN KOUTS: Neil?

DR. WALD: Isn't another consideration that given only hazard states as the criteria, that there would not be any incentive to have mitigation outside of the plant, given the risks of the individual or society this puts more

emphasis on developing mitigating procedures which -- from which you can take credit in meeting that goal of the health goal.

MR. BURSTEIN: You're referring to things such as evacuation?

DR. LOWRANCE: Unless you take those things into account when you set your hazard state limits.

You could though, take those into account when you set your hazard state limits.

DR. WALD: It seems a little less logical to take credit for procedures out there in order to justify or compensate for deficiencies in the plant. I would not like to see guidance that pushes in that direction.

DR. BEYEA: It seems to me that if the risk is calculated, assuming certain evacuation models, then the NRC has some requirement, not some obligation, to ensure that those evacuation models will in fact be met and therefore, I think there is a reason to go to the final state and I think it makes some sense.

CHAIRMAN KOUTS: If Bill will let me rephrase his question, it is, why do we need the apparent duplication which is offered by criteria connected with hazard states and criteria connected with effects on the public, --

MR. BURSTEIN: Isn't that what we discussed yesterday and all morning up until we got into individual

risks?

CHAIRMAN KOUTS: It's all right if it is opened up again in a new guise. Then, we haven't really settled the question.

DR. LOWRANCE: No, I think there are some answers and I knew some, I just wanted to see what we could come up with and we have two or three.

CHAIRMAN KOUTS: One good answer is that they're not completely redundant.

They do have non-redundant aspects.

All right, let's get back to the individual goal aspects. Is there any substance to worrying about fencepost individuals versus some class of individuals nearby?

DR. JOKSIMOVIC: I think it's important whether we can define -- I idealogically like the concept of the most exposed individual. The question is whether we can define who that individual is. I think with calculation we can do it. I don't know why members of Professor Upton's staff didn't do it, and maybe Professor Wald can tell us if there is any problem that I'm missing, if I have a blind spot. I think in every calculation that members of my staff can do, we can tell exactly the place which has the highest dose.

DR. BEYEA: It's different in different accidents, here. The same individuals --

DR. JOKSIMOVIC: For every single accident category group it the way you like it -- we can always say what is the peak dose.

DR. BEYEA: But how would you calculate if in actual sequences to average them. Do you take the average dose -- do you sum over the highest exposed individual for each accident?

DR. JOKSIMOVIC: What we normally do is what Saul did in Wash-1400, we grouped into release categories and whatever release categories we talked about exact nuclear composition, was released into the environment, and then we take atmospheric dispersion codes and take things like -- into account, we can have -- curves throughout the environment.

DR. BEYEA: But they did not apply to the most exposed individual because the most exposed individual is not a well-defined concept -- it's just not a well-defined concept to begin with. It's only well-defined in individual accidents.

CHAIRMAN KOUTS: In principal, that calculation could have been made but it wasn't.

DR. BEYEA: How? How? What did you do?

CHAIRMAN KOUTS: Sum overall accidents.

DR. BEYEA: Then what are you actually measuring? Are you looking at the highest degree?

CHAIRMAN KOUTS: You're measuring integrated probability that person at the fencepost will have been exposed over all times, the amount of radiation on the average been exposed to.

DR. WALD: Are you equating the fence post person with the most exposed person?

CHAIRMAN KOUTS: No, no, you'll find some scenarios where the fencepost individual is not the most exposed in --

MR. LEVINE: If you do the -- calculation, the most exposed guy is at the fencepost.

DR. BEYEA: So the language would not be the most exposed individual but the individual at the greatest risk.

CHAIRMAN KOUTS: They are synonymous.

DR. BEYEA: No, because in some accident sequences, the person who is most at risk on the average is not the highest exposed individual.

MR. BURSTEIN: I'm confused by individual accidents and lifetime probabilities or averages and I think what I understand --

MR. LEVINE: It's ensembles we're talking about.

If you take the ensembles of all accidents, you do get a curve that says the most exposed individual is in-site boundaries.

DR. BEYEA: The most exposed individual on the

average? The average exposure?

CHAIRMAN KOUTS: It's not the average its the sum.

MR. LEVINE: The sum. The sum of all accidents at the plant. Even though in specific cases he could be further away from the site boundary, the ensemble puts him at the site boundary.

DR. BEYEA: Like I said before. I think that's true.

There are different ways you can define. That's all I'm -- depending on how you decide it to be.

MR. LEVINE: It could be a lot of ways but again, the ensemble of all the accident sequences puts the most exposed individual at the site.

The closest person.

DR. BEYEA: The ensemble average?

DR. JOKSIMOVIC: An integral. An integral.

DR. BEYEA: You're saying the sum of what?

MR. LEVINE: Of all the accident sequences calculated --

DR. BEYEA: Sum of what? Something over an accident sequence?

CHAIRMAN KOUTS: Sum of dose times probability?

DR. BEYEA: Sum of dose times probability is greatest at the site. That is a correct statement.

The most exposed individual -- now, that's a different statement. How do you define most exposed?

CHAIRMAN KOUTS: You put the individual in --

DR. BEYEA: If you want to define it that way, that's fine.

CHAIRMAN KOUTS: That's exactly what people have in mind. That concept.

MR. LEVINE: And the question is should you do that or should you do that instead of putting him at the site boundary, put him anywhere in a distance of five miles or two miles or something and say that's an average of the most exposed population as opposed to the most exposed individual.

CHAIRMAN KOUTS: Now, of course, in actual practice, the difference between these two concepts will appear as a result of the population distribution around the site they're considering and it may be that nobody really lives at the fencepost.

MR. LEVINE: That's right.

CHAIRMAN KOUTS: It may be that no one lives in this area or it may be that everybody lives in this area of the fencepost and very few people farther out in the low population zone. There's certainly an arbitrary character to selection of this --

MR. BURSTEIN: Excuse me. Is there anything to

prevent population changes that might characterize the site in the year 1980 but would be completely different in the year 2000?

CHAIRMAN KOUTS: We certainly have no restrictions on that.

MR. BURSTEIN: On that basis I suggest a more conservative dealing with that average individual or that most exposed individual.

CHAIRMAN KOUTS: What does that suggest?

MR. BURSTEIN: The fencepost.

CHAIRMAN KOUTS: The fencepost.

I think, I would accept, personally, any, either of these two interpretations but I prefer fencepost person because I think there's more logic to that selection as you say. That's how I come out and I don't find any strong basis for choice there. Is that an acceptable --

MR. LEVINE: I sort of -- where you are, except I'm slightly on the other side and with no strong way of saying why. I like Chauncey's concept and Chauncey's concept is the idea of looking at some distance from the reactor and averaging over that distance and saying those people should be reimbursed because they are at higher risks than others. Which is an attractice concept as opposed to practically.

MR. BURSTEIN: I don't know why we talk about

reimbursement at this stage of the game.

MR. LEVINE: I just mentioned it because it's part of his concept, that's all. I'm not opposing reimbursement.

DR. WALD: There seems to be sort of a toss up in the guidance from our second objective of increased rationality? Which is more useful from the standpoint of regulatory?

DR. JOKSIMOVIC: Site boundary has been traditionally used for many years.

CHAIRMAN KOUTS: Then you can tie that to -- evacuation, which is also arbitrary.

MR. LEVINE: I don't see any real difference. You have to -- the calculation of the model and bring one into the other.

CHAIRMAN KOUTS: And there's less -- well, I've said my view. I have a preference, but it's not absolute.

The next question which really comes up is whether there should be two criteria connected with the most exposed individual, one connectived with early death and one connected with late death.

DR. JOKSIMOVIC: I have a very strong view on that. The first point is that some magic factor of 5 over there which I found zero rationale for and then secondly I would like to question the concepts of the individual risk

of latent cancer and whether that is meaningful at all. Because latent cancer the way I understand it in all of the tests that statistically, identifying the effect on the population so not to any particular individual.

DR. MAZUR: You have a probability there. It's the same thing.

CHAIRMAN KOUTS: I don't understand your point there.

DR. JOKSIMOVIC: When we talk about particular individual, if he is exposed to 510 grams there's a 50% chance he's going to die.

CHAIRMAN KOUTS: An average person?

DR. JOKSIMOVIC: An average person. A biologically average person. So we know what we're talking about. When we talk about latent cancer, there's absolutely no way how we can assign 5 miligrams or 10 miligrams to a particular person. We integrate this over a large population to get an effect.

CHAIRMAN KOUTS: Are you argueing that there should not be a probability of latent cancer?

DR. JOKSIMOVIC: I am. I am.

CHAIRMAN KOUTS: To the most exposed individual.

DR. JOKSIMOVIC: I am.

MR. LEVINE: I think the basic concept of -- and Niel is the one who should say this but the basic concept of

of latent cancer as a population exposure is a statistical process.

If you add up the total exposure to people and say on the average this is what you're going to get -- but I don't think you can discriminate very much on the basis of the size of the dose to an individual, for latent cancer.

DR. MAZUR: How does that differ from talking about the probability of a given individual getting cancer? It's the same thing.

If you're going to get 10 cancers in a million population then the individual exposed to that thing then there's 10 of them with a chance they'd get cancer.

DR. JOKSIMOVIC: If an individual is exposed to a high level of radiation, we know there is going to be some effect.

DR. MAZUR: Some probability and effect.

DR. JOKSIMOVIC: Some probability of radiation injury, some probability of fatality, whatever, and we can talk in medical terms.

DR. MAZUR: And there is also some probability of getting cancer.

DR. JOKSIMOVIC: There would be no individual around -- plant who is going to be seriously affected by the -- of the plant unless there's a major accident. If

there is a major accident, then there is going to result some kind of high dosage and you can identify the individuals who have received those high dosages and you can evaluate the effect. If for instance you have --

DR. WALD: Even at that point, the end point you're using here of early death is still based on probability and -- based on the probability that that particular individual will die or live.

It is the probability just as much if that particular individual will be the one out of --

DR. JOKSIMOVIC: But you know - that in the range of, I don't know above 100 grams that there will be some effect and you can examine the individual and say that he's been exposed to radiation, right?

If a million people had received 5 miligrams each, you haven't the faintest idea if the radiation came from the sun or I don't know, some other source or nuclear power plant. Right.

So how can you assess the risks from a particular source?

DR. WALD: On a statistical basis.

DR. JOKSIMOVIC: But you can't attribute that to nuclear.

DR. WALD: That's another question.

DR. JOKSIMOVIC: -- attribute this to nuclear power

plants then.

DR. LEWIS: -- Something might die for other reasons. Suppose somebody got 400 grams and one week after the accident dies. Possibly he could have died anyway. He could have died from pneumonia that he had beforehand.

DR. JOKSIMOVIC: That's right. But I think there is certain probability and significant that he died from radiation injury, but if some individual dies from cancer and is exposed to 5 miligrams of radiation then the probability that he's dying because of that is infinitisimal.

MR. BERNERO: Can I interject? I'd like to ask Niel Wald to correct me. I'm going to venture a clarification and it may not be a correct one.

Both the calculation of early fatality and the calculation of latent fatality are statistical processes and a very crude example - if a 1000 -- are suffered by a population of two people, one dies and you can't tell which one because statistically, that dose, that population dose will kill one person, not two.

DR. LEWIS: The one Dr. Wald attends to will survive.

MR. BERNERO: Yes, in a similar fashion, with a lot tougher odds or a lot tougher situations for selecting who is likely to get it, as you go into a larger population base for a given -- exposure, you're still selecting or identifying that somebody is going to get a latent cancer.

You can't hand a label on him and give him continuing medical care but the process of identifying and calculating the statistical threat of latent cancer is really no different than calculating the probability of early death and so I don't see how the one is legitimate and the other is not.

DR. JOKSIMOVIC: The reason why I'm arguing this way, I'm saying it is not legitimate on the individual risks but it is legitimate in the societal risk.

CHAIRMAN KOUTS: Let me ask a question here.

One of the objectives of the quantitative safety goals is to protect the public health and safety. We agreed on that.

Are you saying it is not protecting the public health and safety if we pay attention to an increase -- a probability of incidents of cancer?

DR. JOKSIMOVIC: No. Any set of safety goals in my opinion is going to have individual risks and societal risks. And the early effects could be directed to individual risks and the latent effects could be attributed to societal risks so we're accounting for that by doing this together -- we're trying to double account.

DR. WALD: So your real question is whether or not the individual risks should be included?

DR. JOKSIMOVIC: No, no.

MR. LEVINE: May I try this question?

Two people get 510 grams and one dies of an early fatality and the other one does not, the one who does not die, is his probability of dying of a latent cancer fatality 5%? That is, 500 divided by 10,000?

I think that's the question you're asking, isn't it?

DR. JOKSIMOVIC: Thank you, Saul.

MR. LEVINE: Or do you have to caveat that by saying this man represents an average dose sensitive person and what is the variation around that average dose sensitive person?

Or is latent cancer fatality concept a massive population dose which you then statistically can't identify individual probability.

DR. WALD: The answer is you can identify individual probability if you know the dose of the individual.

MR. LEVINE: So you can divide 500 by 10,000, say that's the probability -- 10,000 -- latent roughly, 10,000 --- per latent cancer fatality.

DR. JOKSIMOVIC: You can, in fact -- we don't have a lawyer on this panel but this is what they do. Well, let's say TMI resulted in one latent cancer.

That's with no way of knowing who that individual is.

DR. WALD: Right.

DR. BEYEA: In a large accident you could imagine --

DR. WALD: On the other hand, an accident where the dose to an individual is high and that individual subsequently develops cancer, he certainly had a much greater probability of that that is the latent.

MR. LEVINE: Let's ask the question a different way.

DR. BEYEA: How many -- can a person get and not be killed in an early fatality? It might be 100 - 200 grams.

DR. WALD: It would be 50. That is the dosage would kill 50% of an average population with treatment, and you'd still have 50% of them die and somewhere around over 500 grams.

DR. BEYEA: But suppose you didn't get that, you got 200 and your chances of cancer are not very high.

MR. LEVINE: Dosage. Dosage.

DR. BEYEA: Doses of 200 -- which is below the threshold.

DR. WALD: For fatality, all right?

DR. BEYEA: So that the maximum individual risk is necessarily from cancer, is necessarily small.

It can't be greater than 1 in a hundred. Or 1 in 50 in any reactor accident.

DR. WALD: For the survivors?

DR. BEYEA: For the survivors. Even though there' is a limit because you're either killed from the early dose or so that there is a difference here, the fact that the risk of cancer can never be greater than some number.

DR. MAZUR: The incremental risk.

DR. BEYEA: The incremental risk. But you still can have a situation where the local population has so many excess cancers that you could say that those cancers were due to the accident. That situation could happen.

CHAIRMAN KOUTS: But I'll ask the question somewhat differently which makes it closer to what Vojin was saying and see if this takes care of your implicit concern here.

If we have, jumping ahead, a criterion on societal effects of an accident, related to the probability of induction of cancer, okay? Suppose we have a criterion like that and we agreed one should be acceptable and we also have criterion on individual risks associated with early death. Do you think a separate criterion on probability of cancer to the most exposed individual adds anything to the set of criteria?

I think that's the real question.

DR. BEYEA: I think it definitely does.

DR. JOKSIMOVIC: It doesn't add a thing.

DR. BEYEA: I think it does.

MR. LEVINE: Well, let me ask you another question. I want to give 10,000 people one -- of each. Can you then predict the probability of each of those individuals dying of cancer?

DR. WALD: Yes.

MR. LEVINE: The same ratio?

DR. WALD: Yeah.

MR. LEVINE: Okay.

DR. WALD: Based on the dose --

CHAIRMAN KOUTS: I'd like to discuss this with you at some point.

DR. WALD: This is getting into the -- of arguements about extrapolation and --

MR. LEVINE: Well, I'm staying out of the range of, you know --

CHAIRMAN KOUTS: If you want to take this down to the point of -- considering those who are most likely to develop leukemia, generally, I think the radiation on that subsets --

DR. MAZUR: Subsets.

DR. WALD: I was waiting to to see --.

Very definitely there are substantive risks but I'm not sure how you deal with that, that is, this equity issue.

CHAIRMAN KOUTS: Well, we have a disagreement. Why do you think this adds something?

DR. BEYEA: I think it adds something because people are concerned about cancer and when you talk about accidents, it's something people don't want to get and if you just have a concern about individual early death, you're not reaching the concerns of the people in the population. I think that if it's covered in the - I guess I don't worry too much whether it's covered in the societal risk or the individual risk as long as it is somewhere. But it seems to me at least to the public's point of view, we do have some responsibility to talk about the fear of cancer which is a very important fear in the population.

CHAIRMAN KOUTS: Allan, that's the point where there's a question of equity concerns because the most exposed individual is the one most likely to get cancer.

DR. MAZUR: No. Some subsets are more susceptible to the risks than others.

CHAIRMAN KOUTS: There is that also. That's not really relevant to our discussion.

DR. MAZUR: No, that was a -- remark. But I feel strongly that cancer should be one of the --

CHAIRMAN KOUTS: On an individual basis?

DR. MAZUR: On an individual basis.

MR. SALISBURY: I don't understand why or I don't

persuasive rationale for making a distinction between delayed death and immediate death.

It seems to me that for the purposes of a goal like this you should just combine the two and make it one standard.

MR. LEVINE: I like that. I would combine the two.

DR. BEYEA: What do you mean? Not treat them -- just the risk of death?

MR. LEVINE: Yes, which includes both.

DR. BEYEA: The sum of the two? The problem --

MR. LEVINE: That depends on how you want to add them. I wouldn't add them one to one.

MR. SALISBURY: I'm talking about adding them one to one.

MR. LEVINE: I would not.

DR. BEYEA: The reason you might not want to add them one to one is that there's a difference in terms of when the death occurs. So the life shortening effect is different. In otherwords, if you look instead of death at the reduction in life expectancy --

MR. SALISBURY: I understand that, but I don't find it persuasive.

CHAIRMAN KOUTS: Let me ask it this way.

Suppose you are offered two doors, right? And

if you enter that door there's a ten percent chance you will die.

MR. SALISBURY: What kind of death?

CHAIRMAN KOUTS: Just a 10% chance you're going to die.

MR. SALISBURY: Of the tiger?

CHAIRMAN KOUTS: There's another door. If you enter that door, there's a 10% chance you're going to die in 30 years. Which door are you going to go into?

MR. SALISBURY: Well, the answer to that is obvious.

DR. WALD: There's another point, distinction between these two and I have trouble seeing how to counter-add them because in the one, the early death, the relationship to the event is fairly clear and I think that's the point you were bringing out.

In the other, it may never come about. You have a statistical chance if you get hit by a car or smoke a lot of cigarettes or many other things the prediction will never be satisfied in an individual case.

From a public understanding point of view, to lump these two it would be very hard to explain.

MR. LEVINE: I think it's even more than that if you think of -- I agree with what you're saying. I'd like to give another example. If you have 50,000 auto fatalities

in the country a year, we sort of accept that and we spend some money on trying to reduce the number but it sort of goes by the board.

On the other hand, if you had 50,000 people being killed in one day in one city it would a calamity. And so the distribution and time and space of fatalities is very important to the public perception and latent cancer fatalities would be widely distributed in time and space. So they don't count the same in terms of public perception as early. I would not add them one to one.

DR. LOWRANCE: How would reactor designers or regulators use an individual immediate death limit as compared to the delayed death limits in doing their design work?

That is, how do the two different limits ultimately affect the design of and licensing of reactors.

MR. LEVINE: That's never been considered.

MR. BURSTEIN: Unless it gets translated back to an effluent release, from then on they're considered in the same mechanistic way but you start out with a different number.

CHAIRMAN KOUTS: That's true and it certainly effects the mitigating features you put into a plant, because the releases related to the delayed effect are not necessarily identical to the ones that lead to immediate effects.

MR. BERNERO: I'd just like to offer a comment. I tried to allude to that yesterday when I spoke of plant specific features and goals that would appear to a designer as a useful thing.

Hazard State 2 is a very useful example of that.

In the general usage of developing siting criteria for reactors or verifying existing siting criteria for reactors, both the latent fatality and vulnerability and the early fatality vulnerability would appear directly with certain assumptions about design and certain assumptions about emergency planning.

In the specific case in my mind would only appear as a redundant check on the hazard states goals and section of emergency measure which fall through the cracks on the hazard states. There is no hazard state which speaks to emergency measures and one -- the possibility that if the designer doesn't satisfy the early fatality goal with core melt prevention and containment integrity, one is forced to come up with another factor of ten or something with the fatality goal calculation.

DR. MAZUR: If I were a designer and there was a safety goal in early death but not in long term death, then I would try and design in whatever way I can so that if there were release it was rather low, but spread out all over the place.

CHAIRMAN KOUTS: No, no, no. That was the assumption -- there is no individual limit on late death, but there is a societal limit. That was the question.

DR. MAZUR: So you do have limits connected with latent cancers but you determine your calculation over everybody -- everybody.

CHAIRMAN KOUTS: If you use that case then, what I would do as a designer is keep the absolute amount low and disperse it over the lower population areas.

MR. BERNERO: Allan, that strategy is called remote siting. That's exactly what you do. You have a certain level of prevention that one presumes, and you look for a low population zone in which spread the unfortunate release that you can't prevent.

DR. MAZUR: So if you had individual cancer, then you would be somewhat prevented from doing that, too, right?

You have to keep the individual risk of the cancer down, then you couldn't drop the stuff in a low population area because you'd still be taking that one guy there and elevating the individual risk. Whereas, if you didn't have the individual risk, you could take that strategy.

That's an interesting possibility actually in the sense that it might be counterproductive than to have the individual cancer strategy because I'm just thinking as I'm talking and maybe this isn't sensible, but it seems to me it

might foreclose your strategy of dumping the stuff in a low population zone.

MR. LEVINE: I think you're giving more credit to the control than there really is.

DR. WALD: I think so too.

I think the case where the individual has the risk is really limited -- the matter of societal cancer is. I'm not really sure -- mean much.

CHAIRMAN KOUTS: If you found that the societal cancer risk is too high, you would use certain mitigating features such as thyroid removal if you were concerned about thyroid and whatever else. And, whatever you use to reduce the individual risk itself -- in fact, there's no way you could apply mitigating responses that would deal with the individual risk without at the same risk really dealing with the societal.

DR. MAZUR: That's exactly the point I'm making. Why does one need an individual risk of early death if you've got your thyroid taken out? Bob, I guess you've an answer?

MR. BERNERO: I think you have to be careful to distinguish -- when one looks at reactor sequences and consequences of those sequences, the population that is threatened with early death and immediate fatality is that population which is close to the reactor for the

first couple of miles. The population that is threatened with latent cancer is a much much larger population pool. It's a population over 200 miles.

MR. LEVINE: You get 90% of the cancer at 200 miles.

DR. MAZUR: If you're defining the population for early deaths that's going to take care of the people who are nearby, too.

MR. BERNERO: Yes, but if you go to the Zion reactor in Northern Illinois, the early threat is to the school house down the road which can't be evacuated.

The measures one takes in emergency planning to deal with that, to deal with the early death threat, are quite different from the ones that one would take to deal with the latent cancer.

DR. MAZUR: But that would be covered if I had a low population risk from early death, if I'm not going to allow more than 4 or 5 early deaths, I've got to get those kids out of there anyway, so I don't see any basic disagreement whether we're talking about early or late. To some extent, the societal number -- on the individual risks but in both cases then the fact that it's early or late doesn't change the logic of that. By the way, we could also add should there be some kind of genetic risk in that? We don't have to limit it.

CHAIRMAN KOUTS: We're going to get to that.

DR. MAZUR: But it seems to me again, that if we're talking about the structure of this thing, we might just as well go on with it at this point because we're dealing more with the structure of the --

MR. BURSTEIN: Excuse me, but are we getting to the point where early deaths are to be characterized as risks to the individual and cancer, latent cancer effects are a figment of societal effects?

CHAIRMAN KOUTS: No, not at all.

MR. BURSTEIN: That's what I keep hearing here.

CHAIRMAN KOUTS: I think that you'll find -- I think that if you were to analyze this problem very carefully you would find that of these four numbers proposed here of individual and societal risks, any three are independent and one is redundant.

MR. BURSTEIN: And that's the individual cancer death?

DR. BEYEA: Or, it could be the other one.

CHAIRMAN KOUTS: I think that's probably right but I think the farthest we can get here is to recognize this as a possibility and say that we can't settle it right here and it may be that you're just throwing in a redundant requirement but we'll find out about it.

MR. BURSTEIN: I think I understand that and I think that's what Niel pretty much said.

DR. MAZUR: I suspect that we could come to some consensus that if in fact there is a redundancy, that nobody in fact is going to impose a redundancy?

DR. BEYEA: No, that's not true. I would think that --

CHAIRMAN KOUTS: You may want the redundancy.

DR. BEYEA: In explaining things to the public. You may not want to leave out -- you might want to leave goals but people might object to them.

DR. WALD: Remember that.

We've already decided and I think rightly that the primary objective is public health and safety --

DR. BEYEA: It can't be designed primarily to explain to the public -- and a redundancy, it really doesn't hurt to add a redundancy for better public understanding.

DR. WALD: Unless it interferes with the rationalization and --

DR. BEYEA: Then it's not a redundancy.

DR. MAZUR: If indeed it is a redundancy it won't, though.

CHAIRMAN KOUTS: It may take 25% longer to calculate.

DR. WALD: Can I ask just one question before we leave and that's to say that the number they chose in the ACRS report of 200 rads as a threshhold above which you get I think 100% fatality, just ain't so.

DR. JOKSIMOVIC: No, they didn't mean that. Not the way it's stated. It's in Appendix A.

DR. WALD: I've lost it now, but maybe I didn't read it carefully enough but the calculations reflected in tables a.1 and a.2 are from 100% fatality for individuals who received a dose greater than the assumed threshhold value for non- -- (which is spelled wrong) effect. And they used two hundred rads and if I understand what I'm reading there, I think that's a rather wild and extremely arbitrary and not supportable --

MR. LEVINE: What page is that on?

DR. WALD: 109. The footnote.

They say this is a conservative approach, but if you use 700 it wouldn't matter. But nevertheless, I think it's unwise, but 200 -- is up in the paragraph, the first paragraph on 109.

DR. BEYEA: What number would you suggest they use instead?

DR. WALD: Well, the number that WASH-1400 used was 510 rads as an LV-50, an LV-100 --

DR. BEYEA: That is for extreme medical --

DR. WALD: No, not extreme. Just supportive.

DR. BEYEA: No, I disagree. Supportive is -- what is supportive?

DR. WALD: Here's the man who wrote it.

CHAIRMAN KOUTS: --- then of course there have been those criticisms of that.

DR. WALD: Yes, but there have been criticisms anyway. The 200 number as I remember was taken from --

DR. JOKSIMOVIC: By the way, while we're on these numbers, just for my education, is there some international agreement on these? On these types of numbers we got supportive treatment and lack of it and --?

DR. WALD: The German, I think, I forgot the name of the organization, --

MR. LEVINE: The German Risk Study.

DR. WALD: Yes, has come out with a similar --

DR. JOKSIMOVIC: I'm aware of that but --

DR. BEYEA: Sweden uses three hundred. Different countries use different things for different reasons.

MR. LEVINE: The German number was slightly lower, I think, because they didn't feel that they had as many hospitals per individual as we do but it's close.

CHAIRMAN KOUTS: Can we go on?

MR. LEVINE: Yes.

CHAIRMAN KOUTS: Let's go on to the societal risks now.

DR. MAZUR: Are we going to take up the [illegible]ic risks now?

CHAIRMAN KOUTS: Yes, we are, after we go through

what's in the ACRS, we'll go through the things that are not in the ACRS.

Societal risk I think is pretty much -- again, there are two limits attached to societal risk, one is cancer deaths per kilwatt hour of plant operation, and the other, expected early deaths per kilowatt hour of plant operation.

MR. LEVINE: I'm not sure the per kilowatt hour is a meaningful measure.

CHAIRMAN KOUTS: You're not?

MR. LEVINE: No. I think whenever a plant is hot and at pressure, --

CHAIRMAN KOUTS: Do you think that makes any difference?

MR. LEVINE: It's not operating but it's --

DR. JOKSIMOVIC: Related to the benefits?

MR. LEVINE: Relating to benefits?

CHAIRMAN KOUTS: Is that a factor of 5?

MR. LEVINE: All right.

DR. MAZUR: That's a real problem, that whole issue of societal deaths per -- then what's the unit in the denominator? ----- what he calls the scaling effect, I think indeed, you get a very large number of reactors going what looks like not so much on a small basis but it's a very different picture on a big basis. And if we're to

think of it per accident or per amount of electricity generated or per operating plant or just in absolute numbers. Maybe there is a notion that whatever the amount of electricity generated from nuclear power, there is some level for the whole society beyond which we don't want people dying, so that I think one must hold open the units of how one specifies the societal level.

CHAIRMAN KOUTS: I think a little farther on we take up the question of risk aversion.

DR. MAZUR: But this is different than risk aversion, really.

CHAIRMAN KOUTS: I thought you were just talking about that?

DR. MAZUR: No, no, I'm not talking about it so much whether it happens in a catastrophy or spread out -- they could be all spread out but it could be we're operating on a very large scale so that within limits we could speak of in terms of -- electrically generated. If there are a lot of nuclear power plants even though their deaths are occurring at spread out intervals a very large number of them and that may be just quite a different acceptance issue than if you were to operate fewer nuclear power plants at spread out intervals, a very large number of them and that may be just quite a different acceptance issue than it would operate if there were fewer nuclear power plants and those

spread out deaths are at a smaller number. It's not obvious.

MR. BERNERO: I wonder if I could just ask in this context if all of you reflected on the previous table of individual risks explicitly says, due to all reactors at a site which is an attempt to deal with that issue, not normalizing to a per reactor --

DR. MAZUR: That's when it aggregates all the reactors at one location -

MR. LEVINE: That sort of handles the earlies but it doesn't handle the latents.

DR. MAZUR: I don't know if that's the latent issue, but I think it's between the individual and the societal.

CHAIRMAN KOUTS: That incidentally is the criterion which would discourage nuclear parks.

MR. BERNERO: That's right.

MR. BURSTEIN: Unless the site boundary is 40 miles away.

MR. BERNERO: It raises a further question if one had the rigger and was drawing the line so fine, that if you built a two reactor site, and certified that you met that criteria, and then 10 years later concluded that it made a whole lot of sense to put two more reactors at that site, you are confronted with the -- choice of getting two extra-ordinary safer reactors or going in and beating

on the two existing reactors.

CHAIRMAN KOUTS: That's what I said. It discourages nuclear parks.

MR. BERNERO: Yes, but --

DR. MAZUR: Unless one pre-plans the nuclear park of course, to assume expansion capabilities by site boundaries which is one of the rationales for nuclear parks in the first place.

So I have a real problem in terms of the units that societal levels are stated in and it's not an issue of whether they are immediate deaths or cancer deaths or genetic problems but the denominator that one picks -- in otherwords, how many deaths in the society per whatever that is. It strikes me that -- it changes the picture a great deal.

CHAIRMAN KOUTS: As I interpret it, this is the goal which was selected for use in analyzing a specific reactor case and for any given reactor case we have essentially 1000 megawatt reactor and 1 million killowatt reactor operating at say 65% of the -- cycle over the course of the year and whatever number of hours that is, and it comes out to a certain number of hours.

DR. MAZUR: I interpret it somewhat differently. I interpret it as implicitly putting in a risk consideration and I think we have not yet settled the issue of if

that's the basis upon which you're going to select such numbers.

MR. SALISBURY: It would seem to me that this formulation for societal benefit does leave a big question which is the overall risk to society as a whole from nuclear endeavor which you may not want to incorporate into the specific safety goals but if you don't incorporate it here then it's going to have to be a matter of -- it's going to be factored in some other way.

DR. JOKSIMOVIC: --the national risk budget for nuclear power?

MR. SALISBURY: That would be one possible approach. Another approach would be to just change this to per person year for the total population rather than for reactor year, whatever.

DR. JOKSIMOVIC: But I thought that the arguement was about the -- the same kilowatt hours which --

MR. SALISBURY: That would be after a year, right?

MR. BURSTEIN: I've looked at the fuel cycle and it's not particulary in this document --

CHAIRMAN KOUTS: We're not looking at the fuel cycle.

MR. BUSTEIN: No, we haven't, but I'm speaking about our analysis of the environmental impact of nuclear power in general and that we have reserved for the nuclear

power a portion of that total budget.

CHAIRMAN KOUTS: Nobody's done that that I know of. I mean, nobody has reserved for the nuclear power plants any fraction of the budget for nuclear power in general.

MR. BURSTEIN: The budget hasn't been set, has it?

CHAIRMAN KOUTS:I don't think a budget has been set.

MR. BURSTEIN: I guess maybe that's correct. The budget has not been determined to be so much but we have analyzed the effects of the total fuel cycle on the population.

CHAIRMAN KOUTS: Yes, that's been done.

MR. BURSTEIN: And I guess the reasoning in the ACRS report is to try to relate societal risk to societal benefit as a philosophy and that's what determines this denominator. I guess we are trying to argue that particular philosophy.

MR. LEVINE: If that's its objective it fails largely in my opinion. I don't think it has any measurable societal effect.

CHAIRMAN KOUTS: I think even though it was stated in the supporting document that is essential to consider benefits as well as risks. That was never done.

MR. LEVINE: It was never done.

I think Allan's point is he would have the same problem whether per 10^{12} kilowatt hours or 10^{10} or whether per reactor year.

DR. MAZUR: Right.

MR. BURSTEIN: Are you going to the arbitrary list of the number or the philosophy?

DR. MAZUR: No, no. The philosophy question is dead, the way one sets the method --

DR. BEYEA: It seems to me that this is a natural indication -- let's start again.

A natural point, a natural target --- bring up the number, it seems to me it is really to determine with a comparison to the alternative, like the coal alternative.

DR. JOKSIMOVIC: It had.

DR. BEYEA: That's right. So this is a question of whether you believe in the alternative philosophy.

DR. MAZUR: Well, we got on that discussion yesterday about how-what philosophy needs to get at these things.

DR. BEYEA: Let me finish the comment on the rationale of choosing the coal level. First of all, taking into account the uncertainty overlap, and there's uncertainty in the cancer coefficient and there's uncertainty in determining the coal deaths, and I believe the ACRS number is

as a rather high and low limit. They pick a number 10 -- I think the number was 10 to 200 deaths per 10^{10} kilowatt hours.

DR. JOKSIMOVIC: That was the number they took for estimating for coal. And then they took the lower value of that to be the upper value.

DR. BEYEA: That number 10 can actually be lower if you look at the data. I would use a larger spread, more like 2 to 200 is the range of coal deaths depending on how you interpret the data. The same is true for nuclear cases. The cancer coefficient can be off. It may be too high, it may be too low. There's a fairly large uncertainty. So you have the problem -- if you look at the range and take the low number for coal and the high number for nuclear, you get a story where the risks for nuclear cancer risks would be actually higher for nuclear. That's one point.

The second point is that you also -- the question is whether if you're looking at new plants, shouldn't you be comparing this risk to the risk of new coal plants with scrubbers. And roughly you estimate that a new coal plant with scrubbers -- operating scrubbers, you look at a factor -- in the coal risk. Which makes me want to ask, if this number is related to coal, then shouldn't it explicitly say that it is tied to current coal technology?

DR. JOKSIMOVIC: It does. It's in effect.

DR. BEYEA: That it would be changed as coal technology gets better?

CHAIRMAN KOUTS: Well, we don't want to debate the numbers.

DR. JOKSIMOVIC: No.

We ought to debate a concept then.

DR. BEYEA: The concept is if you're going to base this on coal which is what you said has been done, I'm just trying to point out the coal numbers can be lower. That's all I'm trying to make a comment.

DR. JOKSIMOVIC: You're not objecting to a comparison with coal, you're objecting to the number associated with coal.

DR. BEYEA: I also think that when you compare it one time, you have to do it continuously then. You have to update it.

DR. JOKSIMOVIC: That's for developing checking the numbers whereas for my liking the ACRS number is a liberal for a number of reasons.

MR. SALISBURY: What Jan wants to do is set up a moving target where that goal would change as there are changes in coal or other technologies.

DR. JOKSIMOVIC: I think we all have moving targets. Congress changes the laws and we age and we learn

and we update.

CHAIRMAN KOUTS: Let me try to break the Gordian Knot here since it's almost time to do something else. Namely, go to lunch.

There may be some unease about the question of whether there's a natural limit to the electrical industry, let's put it that way, because really, it's not nuclear alone that you're talking about it's electrical--it's the effects of electricity and so on and in order to settle this you might have to do a cost benefit analysis, the kind that Hal Lewis has been pressing for.

We could make this point as an aside if we wanted to but it's over and above the thing which we're facing right here which is whether or not criteria attached to judging the adequacy of safety of a single plant should have attached to it criteria of this general kind but the societal effect per kilowatt hour of electricity produced such an effect.

DR. MAZUR: I accept what you're saying but I think you're missing my point. I'm saying that there lies an assumption that is not obviously a good one to make. I am not quite sure that because it is a bigger plant I will necessarily allow more side effects. That is a cost benefit assumption which you may or may not want to make. As we decided yesterday there are other philosophies.

CHAIRMAN KOUTS: That's the risk benefit analysis you must make on electricity generation in general because the only way you can do this with a smaller plant is make less electricity and in that case you might make two plants and then you end up with the same total societal effect.

DR. MAZUR: I spoke of a sociologically sensible way of determining acceptable risk and it may well be that in that sense many many people in this society figure no matter how much electricity you're going to give me, I will not accept more than a certain number of deaths per year.

CHAIRMAN KOUTS: From the electricity industry?

DR. MAZUR: From the electricity industry --- from the nuclear part of it from the energy in it -- I don't know. I'm not taking a position. I'm simply saying that there is a substantial value judgement made in there and I think you're passing over it and you don't seem to recognize --

DR. BEYEA: Can we note it?

DR. MAZUR: We can note it but if we go ahead with an acceptance I will object.

MR. LEVINE: If we accept what, Allan?

DR. MAZUR: If we accept the assumption that we will necessarily accept more societal deaths for more electricity

generated. I'm not talking about numbers and I'm not eve preferring strategies, I'm simply saying there is a value judgement in there and I don't feel -- to make that value judgement. I mean, if you ask me personally I'll give you an answer and we'll do it outside --. You seem to be passing over without recognizing that there's a value judgement.

MR. LEVINE: I think there's no question that there's a question of scale involved if you're looking at per unit or per ten units or whatever, and you're talking of hundreds of thousands of units there's a question of scale involved which affects the overall risks. I have no question about it.

DR. MAZUR: Correct.

CHAIRMAN KOUTS: I was not -- I haven't been directing any of this toward evaluation of total risk of an industry which is --

DR. MAZUR: And we need not and my point doesn't depend on that. The evaluation here implies that with larger plant, the more societal deaths we will accept. And its a value judgement that is not obvious.

CHAIRMAN KOUTS: I think there's a break in that logic.

DR. MAZUR: Deaths per amount of electricity generated.

CHAIRMAN KOUTS: We have some 500 gigawats of

electricity being generated in the country more or less now, and we're talking about one gigawat of that. The contribution of that to the effect of the 500 is just below the size of the pencil I'm going to use.

DR. MAZUR: But the standard says that you can have more societal deaths, the more electricity you generate from a plant.

DR. LEWIS: Allan has a good point, the example in air transportation is that the death per passenger mile went down when the number of passenger miles went up. So it can certainly go the other way. Nobody, I hope nobody is denying that, but it's still true that you can divide the number of deaths by the number of kilowatt hours generated. It doesn't suggest that is the difference between secant and the slope.

DR. JOKSIMOVIC: -- the assumption of --

DR. LEWIS: He's saying the fact that he --

DR. JOKSIMOVIC: He's not attacking that---.

DR. LEWIS: No, no, no.

DR. BEYEA: Why couldn't this number then drop each year? There's another way to meet your objective. But the point is that this number decreases each year so the total risk was -- page 9.

DR. MAZUR: Now this number says, let's see, for example the goal level. The average number of delayed

cancer deaths per 10^{10} kilowatt hours of electricity generated -- that means if I'm setting that as a safety limit, that means the more energy I generate the more people I can kill. Well, I'm saying that's a value judgement.

DR. LEWIS: No, I don't believe you're right. It's clearly an error. What I think is meant there is if you build a new plant and add it on to the existing data base and that produces so much electricity and if you don't change anything else, will have a potential -- it won't save lives.

DR. MAZUR: It will allow more lives to be killed. That's within your safety goals. You are willing to permit it. That's a value judgement.

DR. LEWIS: Oh, I see -- I misunderstood your --

CHAIRMAN KOUTS: I'm trying to address what his real problem is.

DR. LEWIS: Forgive me, I misundersttod.

DR. MAZUR: I don't have a problem, you guys have a problem.

CHAIRMAN KOUTS: There are limits to bacterial content of drinking water which is set on the basis of how many bacteria you find per drop of drinking water. The very terms that these are expressed in terms of bacteria per drop does not mean, considering that there is no limit on the number of drops you don't care how many bacteria exist

in the world. There's no relationship whatsoever.

DR. MAZUR: No, now if you allow me to drink 10 bacterium per glass of water and then the more glasses of water I drink the more bacteria I am allowed to digest.

CHAIRMAN KOUTS: You have a natural limit.

DR. BEYEA: What does that stand for, best available control --

CHAIRMAN KOUTS: And this is taken into account. You have a natural limit to the amount you can ingest and this is taken into account in setting the initial limits.

DR. MAZUR: Does it not follow that if I have a capacity of 10^{10} and that allows me a certain number of societal deaths per year, I now move my electric generation up to two times 10^{10} and I now allow twice that number of deaths?

MR. BERNERO: Yes.

DR. MAZUR: That is a value judgement. I might just as well decide --

CHAIRMAN KOUTS: This would not effect the total number of people killed by nuclear power --

MR. BERNERO: Yes, it would.

CHAIRMAN KOUTS: Within a 10th of a percent or something like that.

MR. BERNERO: No, that number represents the best estimate you can make of the real deaths associated with

nuclear power with a gigawat of nuclear power or 100 gigawats.

MR. LEVINE: Well, I think you have to -- another way to look at it is if you decide as a society that you need that much more electricity -- to nuclear power, and you did not make it nuclear power but you made it something else, the question is, which would be the -- how would you want to think about that problem?

DR. MAZUR: That is one philosophy of deciding. Another philosophy is are you getting the benefit to justify -- another philosophy is we won't tolerate more deaths, I just don't want the electricity. There are numbers of philosophies. What I'm saying is that's a value judgement.

MR. LEVINE: It is a value judgement.

DR. MAZUR: And we should recognize it as such.

MR. LEVINE: And I would like to recognize it as such.

DR. MAZUR: What?

MR. LEVINE: I would recognize it.

DR. JOKSIMOVIC: I don't think there is any disagreement.

DR. MAZUR: I appreciate it. I will work on equity now.

CHAIRMAN KOUTS: What is the outcome of this?

DR. MAZUR: My outcome is that I think this is a non-obvious point and to automatically accept the fact that

a population's societal goals will be stated in terms of permissible deaths per amount of electricity generated is a value judgement that is not trivial and ought to be considered against optional ways of doing it and that we shouldn't pass over it and we are, or most of us are.

DR. JOKSIMOVIC: You recognize not all of us.

MR. LEVINE: I would say that some of us have not passed over it but regard it as a value judgement that is not very significant.

DR. MAZUR: I regard it as very significant. I regard it as precisely what we were discussing yesterday in terms of the various philosophies one has whether it is a -- philosophy or a benefit philosophy or a sociological sensible philosophy. It's the same issue.

DR. BEYEA: Another way of putting the criteria, the safety goal criteria would be to say that the number of deaths or expected risks in the nuclear industry would be less than 100 deaths per year.

DR. MAZUR: Period. That would be another way. I'm not defending that, but that's another way of putting it.

DR. BEYEA: When you put it in those terms, you actually understand.

DR. MAZUR: Okay, I accept that alternate statement.

DR. JOKSIMOVIC: That would be in the national risk budget.

DR. BEYEA: That's right. That would be the criteria. That would be a rational criteria which could be dealt with within the regulatory framework.

DR. JOKSIMOVIC: But in order for that to be meaningful for the regulators it has to be divided somehow.

DR. BEYEA: Then divide it by the population after each year so each year that number is going to decrease. -- time to be a coefficient for the --. That's all it would mean.

DR. WALD: A certain number of deaths would be permissible.

DR. BEYEA: Yes.

DR. JOKSIMOVIC: But things are being done that way in the polution area.

DR. BEYEA: Basically, we don't want to get any worse. -- put a lid on it. Usually the lid is whatever the current level is and keep it where that is. Or bring it down.

CHAIRMAN KOUTS: Shall we recess?

(Thereupon the hearing was recessed for lunch to reconvene at 1:50 p.m.)

AFTERNOON PROCEEDINGS

CHAIRMAN KOUTS: We have still a great deal of ground to cover in fact, most of the grounds are yet uncovered and I would like to confine -- well, first of all we have a responsibility to finish the ACRS.

I'm going to confine this discussion to 2 or 3 aspects of it if that's all right with you gentleman, and then I would like to go into the question of other proposals which have been made for goals on risks and do some comparative discussions on the comparative aspects of the various things which have been proposed.

On the ACRS, we talked about the ALARA concept because this is particicularly one which Dave Okrent has asked that we pay some attention to. Do you feel that a safety goal should include an ALARA concept?

DR. JOKSIMOVIC: In my opinion for all present plants, yes, for new plants no.

CHAIRMAN KOUTS: Only on a basis for determining whether backfits should be made?

DR. JOKSIMOVIC: No.

MR. LEVINE: I think if you word it in terms of a cost benefit analysis to determine, I don't think ALARA is needed. I think what you need is a cost benefit requirement about a safety goal. That is if someone wants a safety goal lowered you have to do a cost benefit and if it shows

it's cost beneficial, then if someone wants to not meet a safety goal he has to do a cost benefit to show that that's justified also. I think that's implicit in ALARA, and I take ALARA to mean that.

CHAIRMAN KOUTS: ALARA is really a sort of cost benefit analysis, already.

MR. LEVINE: Although very muddled up.

DR. LOWRANCE: Again, back to what I've said twice before, the third round is so are those earlier state limits in a sense, really in effect cost benefit analysis both in the macro and micro and I wonder whether those again the hazard states and the individual risk limits and societal risk limits don't really in a sense take in anything else you might do under ALARA.

DR. LEWIS: I thought that there was a fight yesterday about whether the numbers associated with the safety goals could be determined by cost benefit or risk benefit analysis or by comparison with other technologies and both of you seem to be assuming that at the tail end, that is in terms of the ALARA concept you should work through cost benefit analyses and I think it would be a little bit funny to determine the original number through comparison with other technologies and determine the tail end through cost benefit analyses. I'm delighted to hear you both talking in terms of risk benefit but I thought

we had finessed that issue yesterday, because of disagreement.

DR. LOWRANCE: That wasn't my understanding from yesterday.

DR. LEWIS: Oh good, I'm delighted.

DR. LOWRANCE: I think it's interesting to compare technologies with each other but I don't think you can decide very much on that basis.

DR. LEWIS: But the numbers in the ACRS report as I understand it come in large parts by comparison with technologies. Now, we're not saying anything about these numbers but it's implicit in the risk for nuclear set 5 times below the risk for coal and things like that.

DR. LOWRANCE: Which in itself is based on cost benefit analysis and --.

DR. LEWIS: Well, to public acceptance is not really a cost benefit analysis because the costs of coal in terms of risks are only beginning to be understood.

In this conversation I think we're assuming that the whole thing is going to be done a rational way and I applaud it. I don't want to stop it.

MR. LEVINE: I don't see anything wrong with setting the basic numbers on a comparative basis but then doing cost benefit analysis without those basis?

I don't see anything illogical about that.

MR. BURSTEIN: I guess we're straying a little bit from the question but it seems to me that ALARA by definition is as we have said involves this cost benefit trade off and as such to me really has no place in a safety goal. If you were going to talk about setting a maximum level of exposure for occupants in that industry, without reference to the benefit aspect, it seems to me that that's more consistent with the safety goal.

Philosophically I'm troubled with the idea of making ALARA a safety goal.

MR. LEVINE: I would rather word it as a cost benefit requirement.

CHAIRMAN KOUTS: The logic of it is essentially this. You have a set of safety goals which are structured according to numbers which have to be met. It comes a proposal to build a plant with all the information necessary to do the judgement. You determine that the licensees, the proposed licensees application fits all the criteria. This is part of the process. You do the review and sure, it meets the safety goals. Then you do another analysis which says inspite of his meeting the safety goals, he can still do a cost effective improvement beyond this in plant safety applying ALARA criteria. He would then have to do that. This is what the inclusion of an ALARA requirement would be.

MR. BURSTEIN: Again we're talking between the lower limit and the --. And you're talking again about a cost benefit increment and I personally think that gets you back into the arguement. Right back into the arguement again and defeats the purpose of setting a specified limit.

CHAIRMAN KOUTS: That certainly goes beyond the specified limit.

MR. BURSTEIN: I think that that part of a table in my judgement doesn't belong there. As a safety goal.

CHAIRMAN KOUTS: How many people think there should be an ALARA?

MR. BURSTEIN: There shouldn't be?

CHAIRMAN KOUTS: Should.

DR. BEYEA: I do. I think there should be.

CHAIRMAN KOUTS: And opposed?

There seems to be a general view that there should not be an ALARA Limit.

I guess the point of view there is once you determine how safe enough safe really is, and you build to do it, then you're safe enough. This is the conclusion with one hold out.

MR. LEWIS: I do think that in the course of evolution if there are real changes in our understanding of the risks then adjustments may need to be made in the goals

and the issue of how you grandfather older goals has to be made on a judgemental basis in terms of the integrated societal risks, but certainly things will change. It is a dynamic environment.

DR. JOKSIMOVIC: And one of the things people are saying is that if we do institute the safety goals, they should be in a foundation of a period of time. -- the law, -- three years is what I read somewhere.

CHAIRMAN KOUTS: The next point I'd like to take up is how risk aversion is built into this model.

Risk Aversion is built into this model as far as I can determine in only one place. And this is in societal risks where you do an integral over all accidents, probability times consequences to an alpha power. And the alpha means risk.

DR. JOKSIMOVIC: Only for the -- effects.

CHAIRMAN KOUTS: Only for the early effects.

MR. LEVINE: But there's a risk aversion built into the relationship between earlies and latents also. A factor of 5.

CHAIRMAN KOUTS: In the early versus late but no risk aversion on the late in the sense of what -- was just saying.

MR. LEVINE: They're viewed differently, that's all. Not one for one.

CHAIRMAN KOUTS: There certainly is a risk aversion built in connected with the size of the accident, and the early societal effects. And there really are a number of options for the ways to build in that kind of risk aversion or an early versus late aversion to risk; perhaps other ways by which risk aversion could be built in.

MR. LEVINE: Or risk aversion to nuclear power.

CHAIRMAN KOUTS: Or risk aversion to nuclear power through some sort of criterion that nuclear power has to be safer than competitors, or something like that.

DR. LOWRANCE: For the record, Mr. Chairman I wonder if you would define risk aversion. That's a jargon term that probably is not widely understood.

CHAIRMAN KOUTS: I don't know if I can make it general, but would you like to give is one?

MR. BERNERO: Risk aversion is the mechanism by which the tolerable risk is decreased as the level of consequences increases in the usual practice.

CHAIRMAN KOUTS: That's only one application. There are other definitions.

DR. LEWIS: Can I make a try at one?

Risk aversion is referred to take into account the presumption which may even be true, that society does not like a large number of people to be killed at once and in taking that into account, the aversion is never meant that

it is an aversion. And in taking that into account one decides to somehow measure large accidents in clumps as costing more than integrated small accidents.

MR. LEVINE: Another definition of risk aversion is that any new technology will be somewhat more suspect with people than existing technology and therefore should be treated differently.

DR. LEWIS: No, I don't think so. That's not the sense in which it is used here.

DR. LOWRANCE: As I suspected, there are even here around this table different view of what risk aversion means.

MR. LEVINE: I'm trying to talk about the ways very different people use it.

MR. BERNERO: I would just like to make the point that the ACRS report includes risk aversion in the sense that I tried to define it which shows basically the question should there be another rheostat on the panel that depresses risk in total and the level of consequences go higher rather than just say 50,000 people a year from automobiles and 50,000 accidents is the same as 50,000 people a year from something else and one accident.

MR. LEVINE: I think the only reason the ACRS has to put a risk aversion factor in their goals is because they in all other aspects use at least on health effects use average

values. And if you in fact set a risk in safety goals that included a comparison with accidents that covered both large and small consequences as a function of probability, you would get that automatically and if you then derived average values from that formulation, they would include the risk aversion factor. And you wouldn't need to spell it out separately.

CHAIRMAN KOUTS: Well, I'd like to propose one conclusion that we might draw and that is including risk aversions attached to large accidents as a reasonable aspect would be.

DR. JOKSIMOVIC: I think we may wish to debate also whether how ---- is appropriate for latent effects.

CHAIRMAN KOUTS: Yea, but let's take the first one.

DR. LEWIS: I would like to support the use of an enhancement term for large single accidents and decry the use of the term risk aversion for it, because the damage done to society by a single large accident is greater than the sum of many small accidents for lots of reasons of which we've seen some. And therefore you ought to charge the system more if it has a potential for a large accident but not because society is more averse to large accidents so I'd like to support the factor and decry the term.

Do you like that?

DR. LOWRANCE: We agree.

DR. LEWIS: Done, done.

CHAIRMAN KOUTS: What are you going to call it? What are you going to substitute?

DR. LEWIS: People always say if you don't like this, do you have a better offer and I say that's never a fair question.

CHAIRMAN KOUTS: Any -- how do other people feel about this?

DR. LOWRANCE: I agree.

MR. LEVINE: Sounds reasonable.

CHAIRMAN KOUTS: Now, about the next question.

MR. LEVINE: We were endorsing the use of an alpha factor in this statement.

CHAIRMAN KOUTS: Not necessarily an alpha factor. It can take another mathetmatical form.

DR. LOWRANCE: There are other non-linear forms than the alpha factor.

It doesn't help the regulator very much though, until you've talked about how disproportionate you want that factor to be.

CHAIRMAN KOUTS: Then we get into numbers.

DR. LOWRANCE: I would just point out that we didn't solve the conceptual problems very much.

DR. LEWIS: The continuum ranges up to unity, doesn't it? Our camel got his nose in the tent. But I think

that determining the alpha factor is very important if it's to be an alpha factor or a logrithmic scale or something like that, is very important for the regulator, but so is the choice of every other number passed by in this effort and I think that in the end somebody, probably not this group, is going to have to work very very hard to find reasonable numbers.

CHAIRMAN KOUTS: Yes.

DR. BEYEA: I want to make a comment on what kind of effect this may have on the regulatory process. If you did make your alpha very very large, or some other number in your formula very large, then it seems to me you would ultimately at some point move toward smaller reactors. Individual reactors being much smaller with a much smaller amount of release of radioactivity which could be released so that there is a possible impact from a non-linear formulation on the design of the plant.

MR. LEVINE: I think if you made alpha very large you would not build any nuclear power plants.

DR. LEWIS: Or anything else for that matter.

DR. BEYEA: What if you dropped the inventory by 10?

MR. LEVINE: I don't think people would build those kinds of reactors.

DR. BEYEA: Well, suppose you did. Maybe they

wouldn't.

CHAIRMAN KOUTS: This I assume is what we just talked about refers to use of -- I don't know what to call it, I'll still call it risk aversion.

MR. SALISBURY: Risk scale factor.

MR. BERNERO: Severity scale factor.

DR. LEWIS: Scale penalty or assigned penalty.

CHAIRMAN KOUTS: Or immediate. Now how about this question which Vojin raises concerning latent effects?

MR. SALISBURY: I would think you would want a scale factor there, too. Greater than 1.

CHAIRMAN KOUTS: You think that an accident which would lead to 10,000 latent cancers would be worse than say 10 accidents each associated with 1,000 latent cancers?

MR. SALISBURY: Yes.

DR. LEWIS: I think so, too.

Because, inevitably it effects a particular region and as such it has concentrated interlocking social effects.

CHAIRMAN KOUTS: It is spread out over time.

DR. LEWIS: It is spread out over time, that's right, so you don't have as much but it's a little bit different.

CHAIRMAN KOUTS: Yes.

MR. LEVINE: It's spread over 30 years and it's

spread over a large number of miles, a very large number of miles.

DR. WALD: And it's spread over a large base of non-radiation cancer. I don't see the additional societal impact.

DR. LEWIS: I agree it's not large, but I can see a little bit of an extra things affecting more families and more neighbors and more neighborhoods. It's intangible and it's not very large. That's for somebody else to work out but I can see a possible affect there.

MR. BERNERO: May I suggest that there's two parameters together because the calculation tracks both of them more or less together, property damage and latentness.

DR. LEWIS: Yes, property damage, too.

DR. JOKSIMOVIC: Property damage is not proposed in the ACRS report.

MR. BERNERO: And it should not be.

CHAIRMAN KOUTS: Well, we haven't taken up the things that are not in it yet. What's the conclusion there? That you do want a non-linearity also here, but it need not be as strong.

DR. LEWIS: I seem to be the only one who would like one. I agree that it might be a weak one.

DR. JOKSIMOVIC: I'm with you. I'm the one that

offered the -- should be greater than unity.

CHAIRMAN KOUTS: Should be greater than unity, whatever that means.

DR. LEWIS: That's in the alpha concept.

CHAIRMAN KOUTS: Within the alpha concept. Any other views?

DR. BEYEA: I don't think it's very important. It's a minor question. First of all, the number of latent cancer deaths are not -- they're almost insensitive to a lot of plant parameters that depend on the released quantities and ---.

MR. LEVINE: But the probability depends on the design of the reactor.

DR. BEYEA: Of the release, but the number of cancer fatalities are mostly correllated to the release magnitude and are not correllated to whether they mention another variable. You don't have this rapid change of 1 or 2 deaths to 3000. In a reactor accident you're either going to get thousands of deaths or 10,000 of death or so, it doesn't seem to me that the formulation makes much sense.

DR. JOKSIMOVIC: If alpha is high, you would do nothing.

DR. BEYEA: You would do nothing?

DR. JOKSIMOVIC: Yeah.

DR. BEYEA: That depends on what your scale factor is. You scale it. You have an alpha and a scaling factor. What are you going to pick as your scale factor? One death?

DR. JOKSIMOVIC: The slope of the curve.

DR. BEYEA: It's a non-linear curve and you have to normalize it at some point.

DR. JOKSIMOVIC: --- that's a line.

CHAIRMAN KOUTS: I think we probably have a -- here. I'll just move on.

The third thing I want to take up is really tied to the way which ACRS proposes to use the criteria and this is it's inclusion of this list certification panel down at the bottom. Now, what they propose is as we talked about it yesterday this whole risk analysis done for each plant. Then you apply these criteria and then you establish you meet these goals.

There is a risk certification panel which is supposed to review the entire process and determine that sure enough, this is a reasonable evaluation of risk and a reasonable test of whether or not the plant meets the objectives and I suppose a concept like that may make sence supposing you use the safety goals in precisely this way. If you don't use the risk goals in these ways, the safety goals in this way, then presumably you don't need a risk certification panel and that's the sort of conclusion

I would draw.

So if we've come to a conclusion for some time that we shouldn't operate in that manner, then I guess we come to the conclusion that we don't need a risk certification panel.

MR. LEVINE: That's right. Fine.

DR. BEYEA: Except they will be used for some impact on the regulatory process and it might be useful to have some peer view, some outside review of this process and -- the ACRS would probably be a useful procedure.

CHAIRMAN KOUTS: Do you think the ACRS could be a risk certification panel?

DR. BEYEA: I think that they have indicated an ability to do such things, yes.

MR. BURSTEIN: I guess if you want to go back to the beginning of AEC/ACRS time that was indeed one of the purposes for which ACRS was created. It issued some very initial and early documented reports that really did do a peer review if you will of an application or of a particular problem they were faced with including an on-time review of naval reactor opportunities and designs.

CHAIRMAN KOUTS: I did those.

MR. BURSTEIN: On that basis, while the mechanism has changed, certainly that kind of a position was there. But getting back to this very important issue that if you

formalize this procedure as part of the licensing program, you're dead in the water. And I think in all due deference to repeating what we said in the panel session this morning, it seems to me that we ought to re-enunciate the need for a or rather the implication of a risk certification panel and the formalization of that process automatically puts you into the legal jurisprudence of licensing. And I guess I have a very sensitized aversion to that sort of thing.

MR. LEVINE: I thought we agreed yesterday we were not recommending that as an --.

MR. BURSTEIN: Indeed, and I'm merely re-emphasizing that.

CHAIRMAN KOUTS: Well, Jan is saying this, and it deserves some kicking around.

Suppose you use this risk assessment process and your safety goals you're talking about to review the adequacy of these deterministic requirements. Should there be a risk assessment panel to review that kind of thing?

MR. BURSTEIN: There's got to be some mechanism somewhere, someplace, there has to be some opportunity for perhaps assuring that the mechanistic criteria do indeed fulfill the requirements of the safety goals. Now whether that's done by one vehicle or another I'm not sure is important but clearly there must be some opportunity for that exercise to occur.

DR. LEWIS: Call it the risk assessment review group.

DR. BEYEA: That's what I was going to say, it is a continuous use committee.

CHAIRMAN KOUTS: Okay, that's pretty clear. Whatever you call it there has to be some mechanism but it does not have to be a quasi-judicial mechanism like the kind being proposed for the risk certification panel.

MR. BURSTEIN: That's my various serious concern.

CHAIRMAN KOUTS: I think those are the essential features of what's in the ACRS proposal.

Let's talk about what's not in it. Somewhere I've got some sheets of paper.

I've got a partial list of things which are not included in these criteria which might have been thought about for inclusion. I might read out what I've got. And you might have some others.

The number of people at risk above some amount. So this is a different way of measuring impact.

How about the number of people that might have to be evacuated included in some probablistic expression.

The ACRS criteria contains nothing on operating crews. Exposures to operating crews.

They don't refer to the societal effects of loss of vital industry should that occur. I'm thinking of TMI-1

and TMI-2 shut down over long periods of time as typical.

They include impact of multiple plants only in one aspect involving individual risk. They did not include genetic effects.

They do not include normal operation.

These are a number of things that are not included in the -- but if I've listed anything that anyone thinks should be in the criteria but aren't.

MR. BERNERO: Property damage.

DR. BEYEA: You think property damage should be included in the safety criteria?

MR. BERNERO: Yes, I think it should.

DR. JOKSIMOVIC: External.

MR. BERNERO: Yes. Off-site property damage. Third party.

And also the one I raised before about an explicit hazard state for off-site protective measures. There are hazard states for the plant but there was no corresponding one for the one I call hazard state 4. Given the large scale release, the probability of death shall be a figure of merit to describe how effective emergency response needs to be. There's no direct index of that in the ACRS goal structure.

MR. BURSTEIN: I guess I'm a little confused about what you think the lack -- what are these limits on

risks to expose individuals?

MR. BERNERO: They're a product. The risk to the exposed individual is a product of the risk of core melt and the risk of containment failure and the risk that the protective measures won't be successful in preventing the individual's exposure.

CHAIRMAN KOUTS: This is not a plant design aspect.

MR. BERNERO: Oh, yes it is. It's part of the siting and --

CHAIRMAN KOUTS: The evacuation plan?

MR. BERNERO: Yes, the evacuation plan structure. I think it's a legitimate --

MR. BURSTEIN: Maybe if I use the right source term in these calculations I won't need to worry about this.

MR. BERNERO: Well, I'm just saying with the goal structure that exists, it is just not confronted.

MR. BURSTEIN: It's confronted in the consequence models that have been written.

MR. BERNERO: Yes, but they assume certain evacuation.

DR. BEYEA: Is there anything in this goal structure --

MR. LEVINE: There's a PRA procedure being written and it's going to tell people how to do these things, and

the models.

MR. BERNERO: The point I was trying to make is previously we have said that it is, it would not be acceptable to meet the overall goal merely by prevention and throwing away the containment or merely by containment and throwing away the prevention of core value and I raise the question is it acceptable to meet the public health threat goal by the combination of preventing core melt in nice containments and you don't even need an emergency plan.

MR. BURSTEIN: Exactly. Absolutely, because otherwise you're back to this idea of ALARA again. You're saying well, if it doesn't cost much to evacuate the people let's move them anyway even if we don't need to. And then you engender some of the kinds of social reactions and impacts that you're trying to avoid.

MR. BERNERO: May I then suggest that you would be defining the goal structure in which no credit would be taken for?

MR. BURSTEIN: Not necessarily. And I agree again as I said before, you allow me to do the let's say the prevention system the way I think I can engineer it or the mitigation system or their combination in a more effective way and you allow me to use the realistic source term --

DR. BEYEA: Which is the realistic source term?

MR. BURSTEIN: -- maybe -- whatever we get of -- as opposed to --

CHAIRMAN KOUTS: It's not allowed yet.

MR. BERNERO: I want to know what he meant.

MR. BURSTEIN: As opposed to the worst case situation.

CHAIRMAN KOUTS: He means what's allowed.

MR. BURSTEIN: Then I think maybe in some cases I won't need evacuation. In other cases I may, but if you're going to not credit me with the difference between a Palo Verde and a Zion location-wise, then I think we have a great deal of difficulty with coming up with any limits.

DR. BEYEA: First of all, you are credited in this calculation with Palo Verde versus Zion --

MR. BURSTEIN: Well, I thought that's what we were trying to exclude.

DR. BEYEA: Your societal risk, that's included.

MR. BERNERO: No, no. If you had a safety goal that's met, given a large scale release the probability of early fatality is less than or equal to one tenth. Sun Desert meets it without evacuation because nobody lives there.

DR. LEWIS: And there's no nuclear plant there.

MR. BERNERO: I just raised the point of whether it is appropriate for a safety goal structure

since it has an index of performance for core melt prevention and an index for performance of containment success or integrity, should it not also at least consider having an index for performance for off-site protective measures?

DR. BEYEA: I would like to say it has to have one. If it does have one at the present time the index of performance is one. In this calculation you assume that the evacuation model works completely. Perfectly. There's no way you can calculate a dose without specifying post-accident behavior, isn't that correct? You have to specify what the people do. And in this approach they assume certain evacuation procedures and they may assume that is what's going to happen.

MR. LEVINE: Where do they say that?

DR. BEYEA: Because they do a calculation, they have a model.

MR. LEVINE: The models used in real risk assessments have a probability distribution on the evacuation model.

DR. BEYEA: That's right, so you're assuming some performance index for the evacuation model.

CHAIRMAN KOUTS: What they do is work perfectly and imperfectly.

MR. LEVINE: Yeah, they don't work very well, in fact.

DR. BEYEA: Whatever it is you have a model and you're assuming that --

MR. LEVINE: I also have a model for how to design a reactor vessel which is not in this safety goal.

DR. BEYEA: It is implicitly because you have a performance index and you have a performance index and you should implicitly state it.

MR. LEVINE: What performance index do I have that covers a reactor vessel failure?

DR. BEYEA: Because you have a performance index with a probability of less than a certain value.

MR. LEVINE: My point is, where do you stop in this manage'?

DR. BEYEA: You stop and make explicit, you have to at some point make explicit your assumptions. If you want to put a performance index of one on emergency planning, then okay, say it, that's what you're doing.

MR. LEVINE: I don't understand that at all.

CHAIRMAN KOUTS: I would take the point of view that there have to be, along with the safety goal which depends on risk assessment, there has to be some sort of prescription on how you do the risk assessment and included in this will be a number of aspects and a number of features, one of which may well be how you treat evacuation personnel. I would expect it to be there. I would not consider that to

be part of the safety goal, I would consider it to be part of a calculation you do in order to determine whether you need a safety goal, but it is prescribed. And that's how I would see it.

MR. BERNERO: It may be prescribed, but it may be unreal. If the political structure has it --

CHAIRMAN KOUTS: It will have to be matched if necessary with other requirements established by NRC.

MR. BERNERO: So you would merely address it in another form.

CHAIRMAN KOUTS: I don't really think it's a safety goal attached to the design of the reactor or to its initial license, except as another licensing requirement as it is now.

MR. BERNERO: I think it has as much stature as the siting requirements of the reactor. It's a part fo the siting.

MR. BURSTEIN: Oh, I don't know. I've got the regulation that says unless we have an emergency state plan approved by a certain date you shut off all nuclear plants in that state down.

CHAIRMAN KOUTS: But it's not in Part 50.

MR. BERNERO: It is. It's derived from Part 50 Appendix E, but that merely says that it is a vital part of the defense in depth requirement for the reactor.

MR. BURSTEIN: Right, the same as these others.

MR. BERNERO: The same as siting or core melt prevention or containment --

MR. LEVINE: Or SME Section 3.

CHAIRMAN KOUTS: Okay, we have to make a decision about what is included in this and what is put someplace else and what is part of the goal itself and what is part of that which you do to determine whether you meet the goal. In my view, I'd rather put anything associated with evacuation, in the prescription, the calculation requirements and in the additional requirements.

DR. BEYEA: You assume then that the NRC is making sure that evacuation will work and you're assuming they will make quality assurance work.

MR. LEVINE: They have a regulation now to make evacuation work.

MR. SALISBURY: The evacuation shall work. Period.

MR. LEVINE: Isn't it in Part 50 --?

DR. BEYEA: What if the success of evacuation is a crucial element in achieving the risk factor? I guess that's what worries me. It's not too important, if it's easy, if the methodology you assume in calculating these things is sort of, requires very loose evacuation models then I don't worry. If you keep people for 24 hours and get a calculation that way, then I'm not going to worry about it

because I think that can always be met. But if you're going to assume that people start getting a warning 15 minutes after the accident and make certain assumptions, then I may be worried that that is not a reasonable assumption to make in the calculation.

CHAIRMAN KOUTS: I would expect you might in a case like that make some representation over which would be aimed at lowering some of the numbers, put in some more --- or something like that.

DR. BEYEA: As long as we note that as a concern I'm not going to --

DR. JOKSIMOVIC: Are you proposing a condition of probabilities?

MR. BERNERO: Me, yes. As a candidate Hazard State 4.

DR. JOKSIMOVIC: How would you verify that?

MR. BERNERO: Analytically, just like you verify Hazard State 3. In fact, you might be better off, you might more easily do a drill over a little sector for Hazard State 4 than you could for Hazard State 3.

CHAIRMAN KOUTS: Could we have some indication of opinion on this matter?

DR. WALD: There probably is a real problem here if credit is taken in establishing the probability on the most exposed individual in the population because in the real

world it's very clear that the mitigation plans, not just evacuation but distribution of medications if necessary and some of the other plans are variable in the likelihood of accomplishment and it varies from bad to worse and if there's a real credit taken for these mitigation procedures in arriving at the other limits, I think this is an important point and as a safety goal, it needs to be stressed from the standpoint of what it may accomplishment for public health and safety.

MR. LEVINE: The models term the use of the effectiveness of the evacuation which are based on studies of real world evacuations that have occurred, plus a factor of 2 change in early fatalities.

CHAIRMAN KOUTS: A factor of 2?

MR. LEVINE: Two which is well within the error of calculation so it's not really important.

DR. WALD: How about the latents?

DR. BEYEA: I would like to comment on that.

MR. LEVINE: It doesn't effect the latents.

DR. BEYEA: That's when you assume --

DR. WALD: I'm talking about mitigation when you include potassium iodide for example.

MR. LEVINE: I'm just talking about evacuation purposes. There is also a cost benefit analysis done by Bob's people on potassium iodide which shows not to be very

cost effective, and most of the latents come from Cesium anyhow.

DR. BEYEA: I would like to make a number of comments about the comments that were made.

First of all, the evacuation model which I believe Saul was talking about is the Wash-1400 model, is that what you're referring to?

MR. LEVINE: Yes.

DR. BEYEA: Ineffective evacuation means no one spends more than 4 hours on contaminated grounds. That is not my definition of ineffective evacuation and so I think that should be specified.

Secondly, the cost effectiveness of potassium iodide was done assuming a melt down probability and there are differences of opinion on the meltdown probabilities. And I think that Cesium does account for the 3/4's or so latent cancers but that the other isotopes do account for some. Comment ended.

MR. LEVINE: Let's talk about core melt probabilities. There are now 3 risk assessments I know have been done since Wash-1400. They're all getting within a factor of two of the Wash-1400 core melt probability.

DR. BEYEA: And that means that the melt down probabilities of the two reactors is the same?

Or does that tell you the people are doing the

calculations the same?

MR. LEVINE: They're done by independent groups. They're all different, 3 different groups.

DR. BEYEA: Using the same event sequences?

MR. LEVINE: No, they're doing the whole darn study from start to finish.

DR. BEYEA: We're just going to have to disagree on that. As I look at the footnote and he Lewis Report and there are other people besides myself who just do not have the confidence that these calculations will ever tell you melt down probability. We believe that they're very useful calculations to do and they tell you a great deal about how to make a reactor safer but that does not mean they are actually useful in terms of meltdown probability. Which is why I want a safety factor.

DR. LEWIS: Let's keep the record straight about what our report says.

DR. BEYEA: The footnote in the report.

DR. LEWIS: The footnote was a minority of one, so don't quote it --.

CHAIRMAN KOUTS: Now, maybe we can come back to this question of whether there should be a 4th hazard state.

DR. MAZUR: It seems to me that there is -- it has the nature of details somewhat like our numbers and that is

we have decided I think in principle there should be Hazard States but the specific number of them and which are the hazard states seem to me to be rather specific details almost on the order of numbers should be assigned to the risks. So I would be inclined to put that in the same category.

DR. LEWIS: That's in the draft report I wrote.

CHAIRMAN KOUTS: I think you've solved our problem.

How about genetic effects?

DR. MAZUR: Could we have some comments from the medical expert of that adds meaningfully to the assessment of the whole thing? Once you've started counting the immediate deaths and the cancers?

Is it useful or not to start adding, you know, one could add morbidities as well as mortalities and obviously it comes to the point of diminishing returns. So is it useful or not useful do you think?

DR. WALD: I think it probably verges too much into the theoretical because considering our real world population in Hiroshima and Nagasaki 100,000 survivors with no demonstrative genetic effect in their offspring. It probably would not add.

MR. LEVINE: Do you recall the numbers we got in Wash-1400?

DR. WALD: I counted on you for that.

MR. LEVINE: 170 for the worst case compared to 8000

in the same population, but 170 was only for the first generation so you'd have to add that to the succeeding generation.

DR. WALD: I don't see that's really helpful.

CHAIRMAN KOUTS: To calculate what happens after the first generation --

DR. MAZUR: Well, it certainly establishes new territory for safety considerations.

DR. JOKSIMOVIC: What is the definition of genetic defects, by the way?

DR. WALD: It's very complicated. That's one of the problems. Depending on where you cut off on severity, the numbers vary, but I mean it could be anything, any detectable difference in the shape of an earlobe to major birth defects.

DR. JOKSIMOVIC: How do you attribute that to radiation rather than something else?

DR. WALD: A problemistic approach.

MR. LEVINE: It's a man-rem approach.

DR. JOKSIMOVIC: When you talked about the victims of Hiroshima and Nagasaki, you said there was no demonstrable genetic effect, then what is it they were looking for?

MR. LEVINE: That means statistically they were not different from the rest of the population.

DR. JOKSIMOVIC: Then I would say they're not

observable.

MR. LEVINE: That's what he said.

DR. WALD: Demonstrable is the word I used.

DR. JOKSIMOVIC: Then obviously we can't have something like that in the safety goal.

CHAIRMAN KOUTS: We have latent cancers and they're not --

MR. LEVINE: Demonstrable either.

DR. WALD: They're calculable.

MR. LEVINE: You can't identify --

DR. WALD: There's no increase --

MR. LEVINE: You would hardly notice their occurrence.

DR. WALD: There's no increase in genetic abnormalities in that population as compared to control and the trends, the chances which might have been expected on the theoretical basis -- sex ratios change in the offspring, none of those were significantly altered.

DR. JOKSIMOVIC: Is there an example of increased cancer rates which can be attributed to radiation?

DR. WALD: In Japan? Oh yes, definitely.

DR. JOKSIMOVIC: That's observable then?

DR. WALD: Definitely. You can't tell in the individual case that that one was due to radiation and the next one wasn't but definitely --

DR. JOKSIMOVIC: Japan versus Yugoslavia for instance -- you can go to a population and you can show the effect of Hiroshima and Nagasaki and you can say that the cancer incidence is higher because of the radiation attack.

DR. WALD: There's a dose response relationship then for the short latent period cancers like leukemia, there's a temporal relationship which allows you to say that these are attributed to the radiation exposure.

DR. JOKSIMOVIC: and that's the only example?

DR. WALD: No, there are others, but that's the largest that involves a population that was "normal" at the time of exposure.

DR. JOKSIMOVIC: What reports cover that?

DR. WALD: There's a whole series of reports. I guess the National Academy of Sciences biological effects of ionizing radiation report number 3--

DR. JOKSIMOVIC: 303?

DR. WALD: 303 has a good summary.

CHAIRMAN KOUTS: There was a series put out by the Atomic Bomb --

DR. WALD: Yes, it was a continuing series of the National Academy Atomic Bomb Casualty Commission but the summary, 303 is the most recent summary of them.

DR. BEYEA: I'm also not overly concerned about

including genetic effects or not including them. Again it's very difficult to know what the dose response function is for genetic damage.

Also, it's my understanding that the effects scale together so that the cancer, latent cancer goal is a surrogate for genetic goal -- except to the extent that the populations are different, the genetic effects may last more than one generation.

CHAIRMAN KOUTS: I don't think that's true. I don't think these scale. The genetic effects are quadratic effects.

DR. BEYEA: Is that true?

DR. WALD: The most genetic effects are latent and you only get the genetic effect appearing in the offspring and you have a combination of two of the genetic effects, in both parents. Recessive.

DR. LOWRANCE: That's not strictly true.

DR. BEYEA: There are some for which that's true and some for which that's not.

CHAIRMAN KOUTS: This is a common way of putting it.

DR. BEYEA: Doesn't WASH-1400 use a linear response for genetics?

MR. LEVINE: Yes.

DR. BEYEA: I've never known anyone to use a non-

linear effect for that.

DR. WALD: Mutation changes in the germ cells in material is one thing. The expression in terms of genetic visibility in the individual --

MR. LEVINE: Is another.

DR. WALD: Most of the defects are recessive which means that they won't show up unless both parents are contributing which then changes the --

DR. BEYEA: It still is linear.

DR. WALD: I don't think it will contribute.

CHAIRMAN KOUTS: Since nobody is pressing --

DR. BEYEA: Let me just finish the comment.

Also, if you do the calculations the total number of genetic effects averaged out 5 generations or so, would comparable to the cancer deaths so you're talking about as I understand it by knowing genetic effects, would be comparable to a cancer death, so you're talking about as I understand it by knowing genetic effects, assuming the effect is not important, -- that factor 2 in the total number of health effects if you do include genetic effects. It is not major. From a public relations point of view, however, it might be useful to discuss it.

DR. MAZUR: We included that as a --

DR. BEYEA: Well, I don't think public relations is necessarily a bad word, but from the public information

point of view it might be useful to discuss genetic effects and why they are not especially needed to be discussed specifically in a safety goal.

DR. JOKSIMOVIC: They haven't been observed.

DR. BEYEA: The mutations by radiation have certainly been observed. The question is how important the effect is and as I understand it the data was considered much more important years ago than it is considered now.

DR. WALD: Theoretically, they're there if the population is large enough and if the right matings take place and if the people that have damage to their germ cells reproduced at all -- there are a lot of ifs.

DR. JOKSIMOVIC: Allowing for the condition of probability.

DR. LEWIS: Isn't there a general consensus that the social impact of genetic changes is lower than the social impact of the carcinogenic changes? Among the people I've talked to they seem to recognize the existence of a genetic change and then for many reasons the number of them that are recessive and all those other things tend to say that if you can limit the cancer producing impact you can limit the genetic ones as well. Is that fair?

DR. WALD: That's reasonable.

CHAIRMAN KOUTS: Can we leave it?

Does anyone have anything more on this subject?

Are there any other --? I've been going down this category of things which are not included in the ACRS proposal for a safety goal and I guess we have identified one which might be added. Are there any others you'd like to discuss?

MR. BERNERO: Property damage wasn't discussed, was it?

CHAIRMAN KOUTS: Yes, we put it in.

MR. LEVINE: We put property damage in? I don't recall the discussion at all.

CHAIRMAN KOUTS: I'm sorry, I must apologize.

DR. MAZUR: Did we discuss equity?

CHAIRMAN KOUTS: No, we did not.

Allan would like to inject equity as a possible feature --

DR. MAZUR: Risk criterion. It at least should be given some consideration as a safety goal.

DR. LOWRANCE: What form did that take?

DR. MAZUR: If one thinks of the basic safety goal as saying there should be a minimum amount of risks say, to a population, to a person or a population. An equity consideration would modify that to say that within that differential of risk between two people in two classes of people should be either minimized or compensated.

DR. LEWIS: That would drive you, wouldn't it, I

just want to understand what you're saying, I understand what you mean: if you were serving a large area with electricity therefore you would prefer to provide with many small plants so that the risk to all the people receiving the electricity is equalized as well as possible? Is that a consequence of what you're suggesting?

DR. MAZUR: That's one way one could address it and in effect, it surely would be the only or most meaningful way of addressing it. Another way of addressing it would be that one makes sure that the level of risk is sufficiently small so the impact between the highest and lowest is just negligible or not terribly interesting.

Another way is Chancey Starr's way and that is effectively the issue of what we call side payments and another way that Dave has mentioned is that it may in fact be that there isn't that equity problem with nuclear power plants. There's something of an -- issue there if one starts off with the recognition that I think is non-debateable that risks in this society are inquitably distributed.

For example, the poor get more of them than the richer and it may be that the way nuclear power plants are sited, in fact that rectifies some of the existing inequities and work your way around it, you know, maybe you-- the fact you leave them out of the urban ghettos helps things,

so I really don't have an answer for that. I just feel that it is an issue of substantial concern and I would footnote it that remembering the strangeness of our discussion I got little weird looks when I raised it yesterday, apparently it came up quite independently in the other discussion groups as well as an issue.

DR. JOKSIMOVIC: What are the precedents for this type of approach? I know Chauncey Starr told us last time that he -- give some reduction the rates to people living nearby the nuclear power plant. But I haven't hear one -- is there somebody near the airport who will get --

DR. MAZUR: There's a de facto situation and that is people who live in a community near a plant get a rather large tax base from the plant but it's not planned that way. It's a de facto and turns out to be a kind of an equity. Actually, it's probably an over-equity -- it probably shifts the equity the other way, so I really don't have an answer to your question.

I don't know of a particular instance where this was planned. I can simply report that it is an issue of substantial concern. In fact, my lateness to arrive after lunch was due to the fact that I was stranded at Stanford without a taxi cab in discussing some of these issues with a philosopher there and he independently raised

the equity issue.

DR. JOKSIMOVIC: It is a popular subject.

DR. MAZUR: In a group of people who worry about this it's a popular subject.

CHAIRMAN KOUTS: Bill?

DR. LOWRANCE: Such things as disaster insurance are in the same effect, -- make compensation of this sort isn't that true?

Had TMI really been a much worse accident, had there been substantial releases and -- all damage to persons, I have no question that the Federal Disaster Relief Program would have compensated them, the survivors.

If this were a routine thing in some very nuclear future, I can't believe we wouldn't have the equivalent of black lung disease, I mean black lung insurance which we currently pay to 115,000 Americans.

DR. MAZUR: The Price-Anderson arguement of course is that counters what might be an equity producing effect.

DR. LOWRANCE: The Price-Anderson is not the only insurance for the industry, and it's not the only form that can be brought into play after an accident.

MR. LEVINE: I wonder are there examples in the real world where equity payments or compensations have been made for very low risks?

DR. MAZUR: Yes, my Aunt happened to argue a case

before the Illinois Supreme Court, if you want some family, where something like that happened. I'm sure there were many legal cases where there were payments actu_ y, without certainty, even that a given instrument or company causes but just showing the reasonable likelihood that it may have happened.

DR. LOWRANCE: Several of the recently settled DES exposure cases are of that kind in which women cannot reconstruct for the record even what brand of DES they took 20 or 30 years ago.

DR. MAZUR: Love Canal seems to have some aspect of latent --

MR. BURSTEIN: Is that not compensation for actual damages?

DR. MAZUR: That's right.

MR. BURSTEIN: As opposed to compensation for different exposure to risk?

DR. MAZUR: One is an after the fact -- taking the harm as opposed to taking the risk without getting the harm.

DR. BEYEA: The only example I know of is the new Massachusetts law for citing of hazardous facilities, particularly toxic waste disposal facilities and the new law is the developer must go into the community and negotiate with the community as to what he will do to get them to go along with that. The problem is the law hasn't been

operative yet so we don't know how it works but there is a law now on the books in one state.

MR. LEVINE: What's the level of risk there? I really want to talk about low probability.

DR. BEYEA: Isn't that a low probability of release from a toxic facility to dispose of waste?

MR. LEVINE: Radioactive waste?

DR. BEYEA: No, it's chemical waste.

MR. LEVINE: Well, I don't know what the risk is.

MR. BURSTEIN: About the only thing I know of that may have some approach to this is what happens when you make payments for the taking of a right of way for a transmission line in which you are really paying for the inconvenience of having a tower perhaps, but a line that goes over your property under which you cannot build.

Now that really is some advance compensation for some kind of inconvenience.

DR. MAZUR: That's the iminent domain.

MR. BURSTEIN: Yes, and in a title of iminent domain there have been some actual of lands for highways for public purposes, but here where the land is still used and useful to the farmer where he can plant his crops or something but he has some inconvenience and perhaps some restriction for which he is compensated and there is an equity element involved in that kind of a thing but it's not

specified as such. It's specified really as a contribution for damages and not for exposure to potential risk.

DR. WALD: Question. If there were an equity payment, say Starr's model would that have any impact on subsequent claims in the event something did happen?

MR. BURSTEIN: Speaking as a non-lawyer, yes, because you've already accepted the risk. And you'd get paid for it.

DR. MAZUR: It depends a little bit on the words of the language. I'm just raising a consideration and there are a number of ways of addressing it and if -- the distinction Saul made is a very good one about the issue are you being paid to take the risk or are you only being paid afterward once it turns out you suffered from it? And I could see that if one took a safety goal -- the equity considerations, how one responded to it might be of different forms. It may just be if there were an accident and everybody who got cancers even though we don't know whose cancers were associated with the particular accident might get some kind of payment. On the other hand it might be that you were only paid for taking the risk even if there never was an incident and that is people who live near a plant might have I guess the Starr proposal of lower electric rates.

On the other hand, there might be totally different

kinds of solutions altogether. I think from the sociological sensible viewpoint we are really dealing with risk levels that are sufficiently small that the difference between the ones who take the high risk and the ones who take the low risk is small enough that that might in itself handle the equity issue, but I wouldn't foreclose it. It seems to me that that's a determination to be made.

DR. BEYEA: I have a point clarification here. If the equity issue is taken care of in the safety goal, then you don't need the payments. It's only when the equity issue is not taken into account in the safety goal that you're going to have to make compensations, isn't that correct?

DR. MAZUR: No, I think it depends on how the goal is stated. If I just state the equity goal in a very elemental primitive way, and it says that no person or class of persons should have to accept risks that are acceptably higher than another person or class of persons takes that that's the goal.

Now how one acts out that goal, how one meets that goal --

CHAIRMAN KOUTS: Compensation doesn't change, it doesn't meet that goal.

DR. MAZUR: You're right. It might not. I'm sorry. I didn't state it properly. If a person or class of persons

accepts a successively higher risk than another person or class of persons, without compensation then the goal is said to be not met. That is the goal. How one implements that, you see, is quite open.

DR. BEYEA: I think that's a mixture of goals but I guess you could define it that way.

DR. MAZUR: I don't think it's a mixture of goals. It does take into the issue what the risk is, but the goal here is how risk is distributed. That's the emphasis.

CHAIRMAN KOUTS: Hal Lewis?

DR. LEWIS: I understand what Allan is saying and I'd like to speak against it if I may.

DR. MAZUR: But at least you know what I said.

DR. LEWIS: That's progress, but whether it's progress will be judged by future generations.

I'd like to speak against it because where something of value is taken from somebody by the government or utility or a large company, you know, without their consent or without their ability to prevent it, clearly they should be compensated.

Where a power line is put over my property and it reduces the value of the property to me, I should be compensated, no question.

The idea that if a major accident producing 1000 cancers according to the best estimates from the experts

at the time happened, but you don't know which of the 100,000 people it was, 100,000 cancers it was, heck yes, compensate them all because you're never going to be able to tell. But, clearly damage has been done if that's the judgements of the estimates at the time.

But to go further where damage has not been done in the conjecture of damage, I'm afraid, although I understand the arguement for it, afraid it's a never-ending g-- because of the complexity of the society.

If I lived near Ila Vista in Santa Barbara which I do not, that's pretty risky. There are just so many things around my house that expose me to many risks due to a multitude of causes than somebody who lives down the street who in turn has his multitude of risks, that if we were to endorse this I think it would just be another step toward making this an extraordinarily self-centered and litigious society which I'd just as soon avoid.

What I don't think we should do around this table is settling the issue by arguing with case law is what we've been doing.

DR. MAZUR: Let me just respond to you Hal and that is the arguement is a good one and I could make the same arguement about making special safety goals for nuclear power. I am surrounded by risks that we're not making special safety goals for. So why are we doing it in

this case? Well, by the same token we're surrounded by risks that we don't make special equity issues for so why do it in this case? The answer is the same.

DR. LEWIS: I would like to argue the case on the merits of doing it and I'm expressing myself against it. I'm saying that I'm against it. I would like to us not make the decision according to whether there are precedents or case law, but in terms of whether it makes sense as a societal objective to start down this road everytime we do something of providing something special for people who may be threatened by it.

There's a way in other areas that we know, and now I'm arguing case law in which it usually happens when the value of the property goes down and if you can demonstrate that the value of the property goes down you have a case against the people who did that to you and that's a genuine damage although it's based on conjectured risk but still it's a genuine and immediate damage. I understand that, but if the value of the property in the vicinity of the nuclear plant doesn't go down but in fact goes up because the tax rate goes down, then I find it very hard to throw in more on the basis of conjectured injury, bearing in mind that if there's a real injury, we have a social responsibility to get in there.

DR. MAZUR: I would agree with you. If indeed

there isn't an equity problem, I certainly wouldn't try and make it and create an equity. However, I don't know that that's the case and in an A-priority there seems to me quite likely that there is an equity issue.

DR. LEWIS: No, no, I'm not denying the people near a plant are more at risk than the people far from the plant. That is true.

DR. MAZUR: Well then?

DR. LEWIS: I think that's common to so many other things that to start doing them one at a time instead of just recognizing that we have a complex society which on one issue I'm more at risk than you and on another you're more at risk than I am and it's only when it gets well above the noise level that we start negotiating because at that point I'm beginning to lose property or things of value, and if I can demonstrate the loss of a thing of value which in this case should be a reduction in the value of my house, or real estate values in the vicinity of the plant, then absolutely go in for compensation. If that doesn't happen then society is saying it isn't that big of a deal.

DR. MAZUR: I understand what you're saying. You're saying that nuclear power shouldn't be so generous that we make a special deal out of it but if that's the case, why are we sitting here making a special deal out of it.

DR. LEWIS: That question has occurred to me in the last two days.

DR. MAZUR: For consistency purposes, for rational purposes, if we're making a special deal out of it on the one hand, we should be making a special deal out of it on the other hand.

DR. LEWIS: No, I think there is a special reason which is that the government in it's infinite wisdom has made nuclear power a special deal. It is treated in a special way-the safety issues are regarded by the government which has created the NRC which has created us as a special deal and my personal view what we're here for is to help the NRC do it's job more rationally or less capriciously depending on your own terminology. I think that's a useful objective.

DR. MAZUR: I accept that arguement completely and would apply it to the equity issue. And you have pointed out why it has assumed a generous ratio and why it should be treated as such.

DR. WALD: Not all of the issues related to nuclear energy are special cases and it would seem to me that our society has enough mechanisms, for example the 25 million dollar class action settlement for the population around TMI. We have enough mechanisms in place now to take care of inequities after the fact that we don't

need a special -- even if nuclear energy may be a special case, but this aspect of it, safety considerations, we may not have adequate controls and we have to impose special ones.

MR. LEVINE: There is a special provision for treatment of equity afterwards and that's Price-Anderson. There is a special provision for nuclear power for treatment of equity after an event.

DR. MAZUR: I would say Price-Anderson is a bad example because that really limits the equity payments, but the ethical issue is the before the fact issue.

MR. BURSTEIN: I don't want to debate Price-Anderson. There are things to be said in different ways.

CHAIRMAN KOUTS: Yes, I don't want to debate this.

DR. MAZUR: I would like to suggest that we probably will cover this to death but I would like to suggest that it's handled already well enough.

CHAIRMAN KOUTS: I would like to drop it on the basis that it seems to me only remotely related to safety goals and I don't see how you would use this on a basis for determining whether a nuclear plant met a certain standard of safety, so.

DR. MAZUR: Well, I could see how you might do it but I don't want to pursue it obviously. If I'm the only

person here that wants to.

MR. BURSTEIN: Economic compensation?

CHAIRMAN KOUTS: Well, the fact that you pay the man down the street doesn't make the plant any safer. Or less safer.

DR. MAZUR: Well, that's true. The fact that you do a lot of things we're talking about doesn't make it any more or less safer.

If it is indeed the case that I'm only the one concerned about it --

DR. BEYEA: I think it, it should be taken up somewhere else. I think it's a siting issue and most discussion nowadays about compensation equity comes up when you want to site an unpopular facility and so I think it is a valid issue and an important issue that belongs somewhere else.

MR. SALISBURY: I guess I'm tentatively on Allan's side. I can see a rationale for stating that there will be some -- that the difference in risk between the fencepost person and somebody else in society should be below a certain level of difference but, again, I guess I don't feel I know enough to really assess whether that is in fact the case because the risk is so negligible to begin with.

CHAIRMAN KOUTS: Okay. I think we've exhausted the

ACRS report ourselves. I would like to take up competing safety concepts and I'm at something of a loss.

We have about two and a half hours of actual work time left during which time we have maybe half a dozen concepts to pay some attention to. I've done some structuring of what I take to be people's concepts and I would probably be wrong enough so that I would lose time today, presenting them as interpretations of what people have in mind.

MR. BURSTEIN: What do you intend to do with these competing concepts?

CHAIRMAN KOUTS: I'd like to flash them up and let -- these are different structures of the way safety goals --

MR. BURSTEIN: You're not intending to have this endorsed by this committee?

CHAIRMAN KOUTS: Oh, no, we're just commenting on it.

MR. BURSTEIN: Or any of these others?

CHAIRMAN KOUTS: No.

MR. BURSTEIN: So is this merely an invitation to show there are different methods or different approaches?

CHAIRMAN KOUTS: The whole idea is that we would like to be able to make some recommendations on how safety goals should be structured. What should their content be? What should they deal with? And, one way of doing this is

to take up as many as possible of things which have been proposed by different people and see if these have aspects we think should be concluded in safety goals as the commission laid it out.

Not in numerical aspects, but what is addressed, how it is addressed, how is it applied. Now I think if we go to something like Vojin's concept, it's a very different concept than the ACRS concept. Would you like to say a little bit about it?

DR. BEYEA: Before we start, could we say that we'll take a half an hour for each of those so that we don't run out of time.

CHAIRMAN KOUTS: Yeah, we might do that. Half an hour might take care of four and that would be about right. Some of them are quite simple. It would not take anything like that. I hope you're not going to go through all those view graphs?

DR. JOKSIMOVIC: Just a few.

CHAIRMAN KOUTS: In fact if you had something that corresponded with that, that would be excellent.

DR. JOKSIMOVIC: Let me go through what I have.

I would assume that not very many of you have read my papers on this subject. I know that some have but not very many. So I'll try to, in a similar fashion that we have discussed -- proposal, I have a view graph

which says which is the key ingredients of the approach that we favor.

DR. MAZUR: What is GA?

DR. JOKSIMOVIC: That happens to be the company I work for. General Atomic.

It's my perspective of the perspective of the people working for me and then subsequently endorsed by the company.

We did it basically because we felt that there was a need for this. Of removing --- that's a problem I address --. And, we felt at the outset of the exercise that there are two ways of doing it. One is via the risk budget approach comparing to other risks that professor Lewis doesn't like and I listen to him occasionally and I also consulted a number of other individuals-they told me don't do it that way so I dropped this approach and I adopted an approach which at the time I called professional judgement and after reading Paul Slovic type of literature of the professional judgement, I don't want to call it that any longer. I think the approach is the hybrid approach and it takes into account professional judgement. It takes into account knowledge of PRA and it takes into account experience. So in Paul Slovic type of language it has the kind of attributes that he assigns to the number of approaches as opposed to the attributes he assigned to professional

judgement, so I had used that term before I learned about his definition for that term.

And as I said, it places full emphasis on knowledge of probability risk assessment. However, I have recognized for many years that we have to provide a bridge between PRA and the existing deterministic approach which has been employed in licensing for many years and hence, I felt that we don't do that, that if we don't provide that linkage, that we don't do a complete job. So one of the objectives that I had was to provide the linkage with this existing deterministic approach and hence, in order to do that, I have retained the concept of defense in depth that we mentioned and I fully endorse it, but I'm saying that that the way it was implemented had weaknesses.

I want to add extra strenth to that by focussing with PRA technique. Then I have defined the regions which I will describe in a minute.

In addition to designing the -- region, I have added two more regions and I have introduced the concept of no identifiable public injury which I will explain when I come to the diagram. Also, I prefered to use limit lines rather than integrals and the reason why I mentioned the issue yesterday when we discussed the ACRS approach, I had an illustration to show you why.

I've also recognized that in doing so, this is not

only a technical approach, it's a -- approach, and there has to be some kind of a way to show that we can have ranges depending on how one technology is viewed versus the others.

And also I used the same type of a range to say that you can have a new plant and an old plant -- and here's how you can do that.

And the bottom line is that we came up with an acceptable individual, societal and public property risks.

Now here's the diagram which makes an attempt to illustrate the concept, in the usual F/C diagram which is so common in problemistic risk assessment.

I have said that I have defined three regions in the F/C diagrams, the design basis region, a safety margin of design capability region and safety research region.

And my consequence is no identifiable public injury and I -- used the limit line which is this.

And in coming up with the probability scale or what the frequency scale should be, I took into account what was the current base of experience in the United States.

CHAIRMAN KOUTS: Can you tell me what the X-X's are there?

DR. JOKSIMOVIC: X? Consequence.

CHAIRMAN KOUTS: And what does no identifiable public injury -- what is it there?

DR. JOKSIMOVIC: It's associated with 10^{-4} so you

imagine a line over here.

DR. MAZUR: I still don't get that you mean that you get 10^{-4} as the --

CHAIRMAN KOUTS: He's going to tell how he applies it.

DR. JOKSIMOVIC: I have a diagram which is a precursor of the -- so it explains the concept.

Why 10^{-4}?

I have associated the 10^{-4} --

CHAIRMAN KOUTS: We don't want to talk about numbers. You have 3 numbers.

DR. JOKSIMOVIC: Right, but there's a rationale behind the numbers.

CHAIRMAN KOUTS: I think it's enough for our purposes to say there is a rationale.

DR. JOKSIMOVIC: I'd like to say what it is.

CHAIRMAN KOUTS: Except that we don't have much time. I'm just trying to cut this problem down in discussion of why we choose this number and not this number.

DR. JOKSIMOVIC: So you will take my word that there is a rationale for that?

CHAIRMAN KOUTS: Yes. I know there's a rationale.

MR. LEVINE: Some kind of rationale.

DR. JOKSIMOVIC: There is some kind of rationale for all these numbers.

DR. MAZUR: A compelling rationale?

DR. JOKSIMOVIC: More than that. Overwhelming.

I promised to illustrate why limit lines rather than integrals. We struggled with that for a while and when we used integrals -- come up with this kind of situation where all three integrals can be the same.

That is, if you should give me this way, this goes that way and there was a question in my mind whether this type of a distribution would be acceptable and I concluded for all practical purposes it wouldn't be. And hence would propose to have a limit line which basically doesn't move as opposed to an integral which can move anywhere, depending on if you can suppress one portion and we can have a bulge in the other portion. And you can put that bulge in the high frequency region which we wouldn't want to do. We want to limit the high consequence low frequency type of region by having a line which also has a risk aversion of whatever the terms are we define. So we had a steady line to shoot at as opposed to a shifting line.

While this applies to the individual risk concept, we basically drew the line at several points and the key point was the point of no identifiable injury to the public. So that's my point which I call VJ. I link this point to Appendix i and hence I've included the whole operation into

into account. 68-4

MR. LEVINE: What is Appendix i?

DR. JOKSIMOVIC: It is the NRC guideline with regards to normal operation.

I re-emphasized, I wanted to provide the linkage with the -- process. So there are protective action guides and there's one that says evacuation requirements. So that one coincided with my VJ point. There's also -- 100 which points out my point 10^{-5}. At 10^{-6} I wanted to make sure that the probability of accute fatality was small and I took the threshhold of 1% of that. Then I drew the line which became my limit line for the individual risk.

Now for societal risks, I took the approach, that they can vary depending on the society and the political climate we live in and hence, I wanted to retain my concept of no identifiable public injury but I also wanted to show the range depending on what the -- climate might be.

DR. MAZUR: What does it mean to say no identifiable public injury? Does that mean you can't point to that person or statistically expect any based on the calculation models?

DR. JOKSIMOVIC: In the previous one, it means Professory Wald wouldn't be able to identify whether a subject has been exposed to radiation or not.

DR. MAZUR: Then there may well be an expected increase in cancer from the calculation model?

DR. JOKSIMOVIC: On the societal type of basis, with regard to a balanced risk line, it means a certain number of latent cancers which are a very small fraction of a spontaneous cancer rate. The cancer would be within the annual variations of a spontaneous cancer rates.

It is my understanding that by talking to a number of individuals that in a large population of 2 million people or so, you will see any -- in the spontaneous cancer rates. And my number over here would not be any higher than the delta in any of htese radiations and hence would not be identifiable.

You would not be able to attribute that to radiation, exclusively.

DR. MAZUR: Would you explain the rationale for why you think that's interesting? That's not to say they weren't there. It doesn't say that it's of the magnitude the noise is. What is the rationale that gives that some okayness that we've got a number of cancers reproduced since -- your statistics, we can't be terribly confident that they're there.

DR. JOKSIMOVIC: The number of 10^{-4} which I didn't get to explain is basically the lifetime of the nuclear power program in the United States.

And if within that lifetime it does not cause any significant impact on the public I believe that nuclear power

should be -- .

CHAIRMAN KOUTS: 166 reactors per year.

DR. MAZUR: I feel you're really producing a fair number of cancers but since there's so much noise in the cancer statistics you can't really see that.

MR. LEVINE: I think it's a semantic problem with you do you have predictable public injuries but not identifiable. I think Allan is it would be more honest to call them predictable, but not identifiable as opposed to ---.

DR. JOKSIMOVIC: I'm very glad you mentioned that Saul, because subsequent to this type of -- I've established a point of no predictable injury either which is as strict as one can get, which is this point here.

DR. MAZUR: So it would follow if we had a given cancer incidence rate from nuclear power it wasn't identifiable but then we get thorough revamping of our statistics to remove the noise of reporting.

DR. BEYEA: The statistical noise, you cannot remove it.

DR. MAZUR: Well, it may be because the reporting system is awful.

DR. BEYEA: No, the statistical effect --

CHAIRMAN KOUTS: That's on distribution.

This is just random numbers.

DR. MAZUR: Surely in terms of year to year cancer statistics there's lots of noise from bad measurements.

CHAIRMAN KOUTS: Which would make it even worse than that.

This is the minimum you can get to with this rate of cancers.

DR. BEYEA: Then I have lost -- I'm still not clear on identifiability issue. I thought identifiability meant if it wasn't identified, it was in the magnitude of noise that you see from year to year; well, surely part of that noise is due to bad reporting categories.

CHAIRMAN KOUTS: He's only taken that part of the noise which is still casting.

DR. MAZUR: We don't quite know what part it is.

CHAIRMAN KOUTS: That part is the square root of the number of cancers.

DR. BEYEA: You have no way of partitioning out what is measurement error from what is true substance. It has to be greater. The measurement error has to be greater than minimum.

DR. LEWIS: As a point of fluctuation, is this the square root of N or is it larger? You said there was a fluctation going from year to year -- is it larger than the square root of N?

DR. JOKSIMOVIC: I have to admit I don't remember

that but I do remember the numbers we looked at were within that.

DR. LEWIS: But the question of whether it's larger than the square root of N is relevant to what people are now arguing about.

DR. JOKSIMOVIC: I don't remember that.

DR. BEYEA: I think you have an issue in point. I'm not quite sure it's a rationale. But a lot of people have used the same rationale. The FDA has used such a rationale setting proposed standards on action on contaminated crops after an accident, etc., etc., so it has been used before. That doesn't mean it should be used.

DR. MAZUR: It's identifiable.

DR. BEYEA: Yes, it's a small fraction of the yearly fluctuations.

DR. MAZUR: Then it follows to me that what is acceptable becomes unacceptable if you can prove --

DR. BEYEA: You either accept it or you don't. You can't justify it.

DR. JOKSIMOVIC: In the interest of time, I'll move to the next one which is my last proposal which is a roughly simplistic one and by the way, my alpha is 1.5.

My last proposal deals with the subject Bob and I have been raising and that's the subject of public property damage and this is a very tough one to set -- even tougher

than the other two. So I've done a limited amount of thinking but I visualize the world without a Price-Anderson act. I believe that in 1987 there comes up maybe a Price-Anderson Act would expire and it may not be removed and so I think we have to look at that kind of world. And in the absence of a Price-Anderson Act, I believe the insurance industry has to cover this and I've acted as a consultant to insurance companies so I have learned something from them and one thing is that they're perfectly rational.

So, to, -- I'm not suggesting the NRC is not. All I'm suggesting is that in their case, you know, I mean, you know, they absolutely have no problem with alpha equals per unity and I tried it a number of times so I put alpha equals unity on here. Now, I also detected that under no circumstances would they spend more than so many millions of dollars and right now that stands about 300.

And then when I asked them the question what the hell does that mean to you, 300 million? Then during numerous discussions I had we agreed that that's a number like 10^{-5}. And I asked them if that number -- I think if you understand the structure of American Nuclear Insurance, it's a multi-national organization where they have Lloyds and European companies and apparently these individuals are insuring all sorts of things in the world and the number 10^{-5} was the lowest number they would go to. So that means

at 10^{-5} they're willing to put up 300 million dollars risk. So that's how I arrived at this point over here.

DR. LEWIS: Is that per year or per day?

DR. JOKSIMOVIC: This is the frequency per year.

CHAIRMAN KOUTS: Can you plot TMI up there?

DR. JOKSIMOVIC: Can I plot TMI? I'm talking about external property damage here.

MR. BERNERO: TMI was 300.

CHAIRMAN KOUTS: That was not public damage.

MR. BERNERO: Broken turbans and mucked up containment buildings.

CHAIRMAN KOUTS: Why would they be more reluctant to put up money for 3rd party insurance than for direct property damage?

DR. JOKSIMOVIC: No, they wouldn't, but they would still put the same amount of money. They wouldn't put more than that.

DR. BEYEA: TMI was a low budget limit line. If you put TMI up there you're way above.

DR. JOKSIMOVIC: I'm talking about external property damage.

DR. BEYEA: It doesn't matter.

MR. BERNERO: There are two factors here that seem to be confused. An insurer has an upper limit on what he can insure because he doesn't have that much money. And an

insurer has a rate he charges for what he will insure and 10^{-5} per year probabilities will set his rate, not his upper limit.

At 10^{-1} per year, he's going to want 10% plus gravy as a premium. He'll insure your Harley-Davidson motor cycle for 1/3rd of it's value per year against theft because about 2 out of 3 of them are stolen every year.

DR. JOKSIMOVIC: But they would put no more than a certain amount of money irrespective of what the premium is.

MR. BERNERO: True. Yeah. They literally cannot guarantee anything. They don't have the resources to guarantee a billion dollars.

DR. JOKSIMOVIC: Well even if they did, they wouldn't consider that as proven.

DR. MAZUR: Sure they would. If they have plenty of resources and it was a 1 in 3 thing and they charge you more than 33% of the premium, why wouldn't they?

MR. BURSTEIN: As we know that target has moved over a period of time, in response to the number of participants -- the premium level, the rate of inflation, the cost of replacement and all the other typical ingredients of insurance policies. I don't think that whether that 300 or 200 or 400 is significant --

DR. JOKSIMOVIC: No, it isn't. I'm just saying that

this is a concept.

MR. SALISBURY: What your dollars --

MR. BURSTEIN: That's what I meant. It wasn't significant.

DR. JOKSIMOVIC: 80. 80. Would be the round number.

CHAIRMAN KOUTS: This is the application of Vojin's concept. First of all, we talk about individual risks. The application of this is through what is defined as an accident in --, I guess accident means core melt in this case. There are things which we call accidents. And you have three limits which are Limit A, Limit B, and Limit C with 10^{-4}, 10^{-5} and 10^{-6}.

The probability of what is called -- what you do is analyze in determining what accidents can occur. You focus on an accident. You do a probablistic calculation to determine what probability this accident is and if this probability exceeds this limit A which is 10^{-4} in your assumptions, you have to do something about this. You have to redesign the plans. You have to introduce mitigating features, whatever is necessary to bring this probability to less than 10^{-4}.

DR. JOKSIMOVIC: To bring the point in the F/C diagram within acceptable regions.

CHAIRMAN KOUTS: Bring it down, yes.

DR. LEWIS: But if I had that diagram, you have to

introduce features to bring it down below that.

If the probability is between 10^{-4} and 10^{-5}, then you have to analyze--

DR. JOKSIMOVIC: Here's a point, let's say, hypothetically let's say we're at this point. It's outside the limit line and we have to do something. If we shift it from this point to this point we're in the right direction and so we're reducing the consequences in this event. The other thing we can do is try to educe the probability of this event and we'll go this way and then we'll manage to get next to this point But in doing so, you want to watch, because as you're reducing the probability and may increase the consequences and you don't want to do that.

CHAIRMAN KOUTS: If the probability is clean -- 10^{-5} and 10^{-6} this is what's called a research region, and you don't have to do anything, probably but you have to do research.

DR. LEWIS: That means you only do research on unimportant things?

CHAIRMAN KOUTS: This is one of the questions --

MR. BERNERO: It's a tradition.

MR. LEVINE: That's the idea. We're going to cut off this gravy train somehow.

DR. LEWIS: That's illogical, thank you.

DR. BEYEA: This is the definition.

CHAIRMAN KOUTS: Finally it's a probability low enough --

I have one question concerning this process because if you do this item by item -- if you look at one event tree, another event tree and so on, you may end up with individual points which fall within your limit line and you say well, okay, in each case I'm all right but when you add them up you go outside your limit line and you may end up with a probability of damage to the reactor from all events which have been analyzed this way like 10^{-3} which is outside the limit line. What do you do about that?

DR. JOKSIMOVIC: I'd correct it.

CHAIRMAN KOUTS: Well, that's not part of your process, though.

Because, your process is determining what to do in order to make the proper defense against individual accident sequences if you might analyze.

DR. JOKSIMOVIC: I don't to -- in designing a plant this is basically a major design tool and in going through this process, I've done all that. Then I move into the individual risk and societal risk to make sure I met those lines.

CHAIRMAN KOUTS: I think we're at coffee time.

(Thereupon, a 30 minute break was taken at 3:35 p.m.)

MR. LEVINE: I'd like to spend a minute first explaining my safety goals which I never felt we needed safety goals until the last year and in self defense I think I explained yesterday why I think we need safety goals. I think you all have a copy of this and the only reason I want to mention this at all is not to press it on you but to illustrate that there's another format one can use that avoids some problems.

I propose the safety goal that's about a tenth of a percent of the total of man caused accidents. ------ higher easily. The other thing to note about it is that it includes early fatalities and latent cancer fatalities with the idea that you add the latent 1/30th of the latent cancer fatalities to the early fatalities.

And, this kind of goal appeals to me because it's easily explainable. The rationale is visible and explainable to people. I think they can understand it.

DR. LEWIS: Why 1/30th?

MR. LEVINE: There's a thesis for MIT ---- which suggests there's a factor of 30. Kinchin uses a factor of 30 and that's why I chose a factor of 30. I think it certainly should not be added to one.

DR. LEWIS: I agree with not one to one. They do for negotiation between one and one --.

MR. LEVINE: But that's why I used 1/30th. Enough

of that. I'll put up a couple view graphs which I will apologize for that would be illegible. If you have copies of it then I will just point the things.

What this does is it shows the various people who have proposed safety goals and this is a statement of the goals. Notice only a few people have suggested cost benefit goals, but they're the same essentially and if anything they're a little conservative as opposed to realistic. This is in the ACRS goals and you find various kinds of statements by different people and different organizations and you can read your own copies and see what they mean.

What's more important I think is the next view graph again which you have copies of which tries to compare these. I did this work because I felt it was very difficult to comprehend what everybody was talking about unless you try to put the goals in commensurate terms.

And you can for instance take my curve or Vojin's curve and derive from it many of the values that the ACRS uses. As I try to derive the values, where I could based on the information given by the authors, of equivalent numbers. I have also put down the Wash-1400 numbers on the bottom line so one gets some idea of what's going on. I recognize the Wash-1400 numbers applied to a hundred reactor industry or an average of one BWR or one PWR at a

site that typifies 68 sites so certainly not a number to compare a risk assessment of a specific reactor to a specific site to these numbers.

And notice from my curve I can derive all kinds of numbers that Okrent has in his, except for the core melt probability, and I've just used a value similar to the Wash-1400 value for that. I also put in an average of an exposed individual as opposed to a most exposed individual and the way that was done was to compute for early fatalities the average probability to people within 10 miles of a reactor so you divide the societal risk by the area people in a ten mile area of a reactor and I use the median population density of 68 sites in the U.S.

You can do the same for delayed by using a 200 mile area. Notice Chauncey Starr doesn't quite fit the pattern. He has not separated early and latent which I think is wise and he's using a distance criteria which is an average of people in the vicinity of the reactor and an average of people further away from the reactor so his numbers don't fit the pattern. And you can see certainly that if you look at this column, the various goals proposed by the various people do not vary very much. Kinchin's very low, but of course he has a problem with very high population density sites. Otherwise it's only about a factor of 10 variations.

And if I raise my curve by a factor of 10, this would come out 1 or 1.5. It would be very similar to AIF in Oakland. And you can go through and compare these numbers. So this is just a way of thinking about them.

I can say that Vojin, myself and Kinchen have all proposed a curve. The curve format is just as popular as the other formats. And again, I'm not pushing it. I think it has the obvious advantage of communicability to the public.

DR. LEWIS: And for each curve there exists a graph paper from which it's a straight line?

MR. LEVINE: Yes.

DR. JOKSIMOVIC: The location is acceptable --.

MR. LEVINE: And I also think it handles the risk aversion for large consequences but the curve does compare the small and large consequences. The small and large consequences from other technologies. So that factor can be looked up.

I think that's about all I can say about it without getting involved in a lot of detail.

DR. WALD: For clarification -- what is --

MR. LEVINE: That should be one.

DR. WALD: That's the AIF?

MR. LEVINE: The AIF was point 1 and they raised it to the last point to 1.0, on the previous slot. I think

it's correct on the 2nd slot.

By the way the black boxes are the actual numbers as opposed to goals and all other numbers are derived numbers.

MR. BERNERO: If I understand correctly, along with that change they're adding a core melt probability.

MR. LEVINE: I think I have it here.

MR. BERNERO: The AIF?

MR. LEVINE: I'll show it.

MR. BERNERO: Oh you have it, I didn't see it. I was looking at the wrong curve, the wrong graph. Yes, you've got it, that's it.

CHAIRMAN KOUTS: This bears a lot of looking and thinking but the thing that stands out as far as I'm concerned is that there really is not much variation in proposals that people have made.

You can renormalize them pretty broadly and be quite comfortable.

MR. LEVINE: If you look down any column it's hard to find more than factors of 10, mostly, sometimes you find larger factors. They're not very large. I guess in this column you see large differences from the average values.

CHAIRMAN KOUTS: Different people have used different kinds of logic in deriving where they finally ended up and in spite of that the numbers come out roughly

comparable. The structure comes out roughly comparable. And as far as I'm concerned this means that the underlying logic is not as important anymore because regardless of which pathway you take you end up in the same place.

DR. LOWRANCE: Is there a reason for that? Isn't it just that everybody is taking the same basic approach in saying here's what we are doing, here's what we can meet and therefore that's a reasonable goal?

CHAIRMAN KOUTS: There may be a substrate here which is not really revealed in all this process, but in principle some people have tried to construct risks which have some factor of being lower than risks of competing sources of energy. This is one source. Other people have tried to get their risks derived from other kinds of logic and they all end up at about the same place.

MR. LEVINE: I think the other thing you ought to note is the ACRS specifies more factors than anyone else. They have 7 and I left two out, 9, I left out 1 and 3 on here. That 9 factors -- Starr has 2, AIF has 4. Joksimovic has one factor and a curve, I have one factor and a curve, Kinchin has two factors and a curve. Corkerton has -- John, shouldn't some of these be in boxes in this -- is 10^{-6} in a box?

MR. WREATHALL: -- most exposed individuals 10^{-5}, normal operation and accidents, faulty reactor site, so that's

an interpretive number.

MR. LEVINE: So most people have suggested far fewer goals than the ACRS has. I think that's important. So they're generally in the said structure.

DR. LOWRANCE: You began -- I think it's important to look into the logic of something like this statement that you propose a certain curve. It looks like a reasonable sort of curve to me, but where does it come from and surely it's going to cost us a lot more -- start somewhere else.

MR. LEVINE: What I said yesterday, that the nuclear risk would be a small fraction of the sum of all other technological risks.

CHAIRMAN KOUTS: That's a different logic.

MR. LEVINE: That's the logic I followed.

DR. LOWRANCE: And you said that's one/tenth?

MR. LEVINE: But you can argue whether I picked the right number but that's the way I did it.

DR. LOWRANCE: Can you give us any guidance on what kind of number you would -- how you choose that number. That's what is so --

MR. LEVINE: --- choose one percent of the sum as being a negligible fraction of the sum, it wouldn't change the sum significantly. I would say that we're not going to have 100 new technologies in my lifetime, or maybe in the lifetime of the reactor industry.

DR. BEYEA: You said here in how you define social risk in your Table A, a curve be drawn to 1/10th to the lowest non-nuclear risk.

MR. LEVINE: That happens to be about 1/10th of 1% of the sum.

DR. BEYEA: What about the risk of being killed by radios? I mean, the risk of being killed by radios is pretty low.

MR. LEVINE: Again that's another -- I should have said this. I believe that the most rational comparison is on the basis of accidents that have both small and large consequences with large probability differences.

I think that's the most rational comparison to be made.

DR. BEYEA: Your Table A needs a better definition for what you're doing because you're taking the lowest non-nuclear risk of a subset that you think is comparable, mainly, 1/10th of air crash persons on --.

MR. LEVINE: Yes, but I just as easily could have said 1/10th of 1% of the total.

DR. BEYEA: I think that would have made more sense.

MR. LEVINE: And that's what I'm saying today.

DR. MAZUR: It's sort of arbitrary. How do you categorize to compare with?

MR. LEVINE: The comparisons I've made are on the basis of what people have proposed. I've tried to compare all the parameters that have been proposed by everybody.

DR. MAZUR: I'm sorry. I meant in terms of life. You take the criterion of 1/10th of the lowest non-nuclear risk. What is a non-nuclear risk? How do you categorize it?

MR. LEVINE: You look at the curve. The curve identifies all the risks.

DR. MAZUR: These. These are arbitrarily exclusive or even singularly --

MR. LEVINE: There may be others in there.

DR. MAZUR: Well, not only are there others in there but one could conceptualize -- differently.

MR. LEVINE: They're based on data and analytical projections.

DR. MAZUR: I know they are but I'm saying that first of all, categorization is somewhat arbitrary and second of all the selection of which ones to include -- which categories to include and which not to include are --

MR. LEVINE: The ones we chose to include were the ones we thought would be the largest. We also thought that chemical plant risks ought to be in there and we simply couldn't --

DR. MAZUR: What about risks from nuclear bomb accidents or --

MR. LEVINE: Oh, I don't believe we should put war in.

DR. MAZUR: That's not war. I'm not saying war, I'm saying nuclear bomb accident errors in setting off a nuclear bomb, or what about the recommitant DNA accidents or botulism accidents or that flouridation may inadvertently kill our population and we could go on and on.

DR. LEWIS: I agree with Allan on that. Would one argue that if there were to be --

MR. LEVINE: They would all react to the total. They would make this choice more conservative.

DR. LEWIS: But suppose the world were to go to pot in some obscure way that does produce extra risks to us. I don't know whether it's botulism or whatever, so that the total man-caused moved up by a factor of 10, would you then advocate relaxing the controls on reactors?

MR. LEVINE: I haven't said I would relax the controls on the reactors.

DR. LEWIS: But well, why not if that's the rationale?

MR. LEVINE: I might. I might.

DR. LEWIS: I see. I wouldn't.

MR. LEVINE: I have to think about that.

DR. LEWIS: I don't want to reopen the question about whether the comparison is a reasonable way to begi- but --

MR. LEVINE: I said yes, I favor your view in -- I don't know how to --.

The first speech I wrote on how one should set safety goals said we should do it just the way you want, by risk benefit analysis. I said that's a 10 year project let's get it right to e pose to public view.

DR. LEWIS: That's because you have --

MR. LEVINE: Now wait a minute, let me finish. And I have only gone to this comparison route to defend ourselves against what the NRC is doing. We just have to have the goal faster than that. And I think the comparison route is probably pretty good.

DR. LEWIS: A reporter recently said to .e are you saying extremism in the defense of liberty, and so forth?

CHAIRMAN KOUTS: Here is the Atomic Industrial Forum reasoning. It says for the individual health effects, should not result in a significant increase of annual mortality risks for its' significant shortening or its expected s. .istical life span. They come up with a 1/10th of a percent of total mortality risks and one percent of accident mortality risks for the fencepost person. Now for their societal risks, they say should be no more than a small

fraction of the average background incidents of health effects. And his comes out to 10^{-5} of total mortality and 5^{-5} of total cancer and you come up with about the same number.

There's certainly an amount of arbitrariness in all of these factors that are chosen here but varying them by a factor of five or so, that doesn't make that much difference. You're still in the same boat.

MR. LEVINE: That's one way in fact of getting a cost benefit risk judgement. To ask people what they think it ought to be and get a number.

CHAIRMAN KOUTS: Well, this is Allan's major suggestion which I thought was --.

DR. MAZUR: I would argue that the reason there is so much convergence on these things --

MR. LEVINE: There's some history we don't know about.

DR. MAZUR: Right. As I said before, if Wash-1400 weren't done, nobody would be here. Because nobody would have thought about it or have any idea what the numbers were.

DR. LOWRANCE: And that stems from Chauncey Starr's earlier --

MR. LEVINE: What?

DR. LOWRANCE: Some of that stems from Chauncey Starr's earlier works. This curve is about 10 years old.

And, first printed by Starr.

MR. LEVINE: No it isn't. That curve was first printed in Wash-1400.

DR. LOWRANCE: Well, you put the nuclear plant on it.

MR. LEVINE: What?

DR. LOWRANCE: You added the nuclear plant.

MR. LEVINE: No, Chauncey never did that curve. He did a different kind of curve. Sorry about that.

DR. LEWIS: But just for fun, I think it would be fun to construct a curve which visually would be a totally different expression. For example, constructed curve for -- is over here and constructed curves that have a lot of things below the --

MR. LEVINE: That's very interesting. I wanted to talk about that.

DR. MAZUR: I wouldn't even worry about it.

MR. LEVINE: The shape of the tab is incorrect. It changes the shape significantly because Okrent's study had been failures that caused consequences out here at probabilities up around here.

DR. MAZUR: You mean you had a man loss?

MR. LEVINE: It's predicted.

And, it's predicted at high probabilities down to the -2 or 3. And we just didn't want to scare people about

Dan's and my report. His report is published separately. Let them look at it. We financed his report and it's been published. Our only point is we wanted to show that nuclear risk did not present extraordinarily large consequences compared to other risks. That there are other -- earthquakes go out to here and with some estimates of a million fatalities.

DR. MAZUR: So the shapes of the curves can be somewhat different.

MR. LEVINE: If you want to do this right you really ought to do that right. Do that better than we've done.

DR. BEYEA: But you're not proposing to put earthquakes on this?

MR. LEVINE: No. Just technological.

CHAIRMAN KOUTS: Well, as far as I'm concerned we've accomplished our objectives.

DR. BEYEA: There's one more thing I would like to look at and that is in the, I believe in Chauncey Starr's proposal this idea that one can rely on the utility to protect plants themselves, that's what it says.

I would offer that that is not a valid assumption and we should not make such an assumption particularly because the experience we had suggested the utilities don't know how to protect investments in plants, particularly the experience we had at Three Mile Island.

So the record doesn't look good and now we're being asked to say that the utilities are going to do better. And that as a result of Three Mile Island we can now expect the utility people to pay attention.

It's not clear to me what the utilities can be expected to do. Utilities executives are not experienced with reactors. Although all they can do is to rely on other people and tell them to do better. Except for certain utilities such as TVA which have a large staff of their own, a very large staff of their own nuclear capability, but in general, I don't see what special expertise the utilities have to protect their own investment.

I would prefer to see the NRC still have its hand in this to a certain extent until the utilities demonstrate an ability to protecht their investment, I don't want them to do it.

DR. JOKSIMOVIC: How would that happen?

DR. BEYEA: How would they mess up their own investment?

DR. JOKSIMOVIC: No.

MR. LEVINE: What would they have to do to satisfy you?

DR. BEYEA: Well, they'd have to go for a long period of time -- I'll probably never be satisfied without the NRC in there except after hundreds and hundreds of years

experience with reactors.

DR. JOKSIMOVIC: Some of us have worked in Great Britain and NAI which is the equivalent of NRC, they don't get into the details of the plant design and the records are satisfactory.

DR. BEYEA: I'm not advocating for NRC to get into every little design. I'm criticizing the assumption that the regulatory system need not worry about the protection of -- need not worry about certain kinds of accidents because the utility would take care of it automatically to protect its own investment. I'm challenging that assumption.

MR. BERNERO: Jan, would you relate your comment as to the distance of hazard state 1 and 2?

Are you thereby justifying --

DR. BEYEA: Yes.

MR. BERNERO: Hazard state 1 and 2?

DR. BEYEA: Yes.

MR. BERNERO: And are you thereby justifying hazard state 1 and 2?

DR. BEYEA: Yes, I think it would make some sense to have a hazard state 1.

DR. JOKSIMOVIC: You're saying you wouldn't buy safety goals if they were just based on individual risk and societal risk and the property damage? In addition

to that, you would like to see the hazard states and that gives you the confidence?

DR. BEYEA: Well, I might want to throw out the hazard state for another reason, like Saul Levine mentioned because it is not defineable in any simple way so I would not accept the arguement that we don't need a distinction of hazard state 2 because the utility is going to protect its own investment. That's the arguement that I --

MR. LEVINE: I think you have no faith to --

DR. BEYEA: I think INPO may do a good job. But I don't think the utilities have demonstrated an ability to protect their own investments.

You have to convince me that they've now changed.

MR. BURSTEIN: What makes you think -- excuse me. As the only utility guy here I resent this whole subject. I don't think it has anything to do with what we're talking about but let me say very candidly if the NRC was so good in the first place, Three Mile Island wouldn't have happened, if they'd done their job and the whole concept of short term lessons learned and backfitting and other things is the same admission that you're now accusing the utilities of being guilty of incompetence and incapacity and not having adequate staffing. You know, it seems to me that such comparisons don't lead to constructive appreciа-

tion of what we're trying to do. TVA may be the largest single staff but they've also had Sequoia's, and they've also had Brown's Ferry fires and a utility up in Wisconsin who has only 65 people operating the plant didn't. I don't know what that tells you but it suggests to me that maybe when you get too far removed from where the action is in a large organization you lose something. I think there's enough concern and as I said before at this meeting for the utilities to survive for them to implement as a restrictive safety application as may be imposed by regulation if not more so. Now we all learn by experience and sometimes the bad experiences are the best teachers and I would suggest not for one moment to have the utilities not learn from Three Mile Island. The same as everybody else has.

But I think we can't ignore the lessons of history and the lessons of experience and sure, we don't have and perhaps in our lifetimes we will never see an adequate data base to ensure against the kind of things we're concerned with here. But that doesn't mean we shouldn't attempt as I think Saul Levine and all the others have suggested here to do what I think what Harold would say, the best we can. And, I'm convinced personally that the best we can involves perhaps some selfish greedy types of criteria like self-preservation and financial

integrity.

DR. BEYEA: Could you explain to me what a utility can do, the executive owner of a utility can do to ensure the safety of his plant? Not to lose his investment?

MR. BURSTEIN: Among other things, he can do all the things that NRC has suggested needs to be backfitted and many of which some plant had prior to that notice -- he can do a number of things in the way of assuring the kinds of things of items that were mentioned in the respect to a steam driven feed pump. Not every plant, I think Saul's purge, showed 6 plants needed revision to make the electric features of a steam driven feed pump operative in the loss of an AC and 6 didn't need any and there are groups inbetween. There are variations in what utilities do.

Selection and qualification of personnel. We've heard a lot about the operator error contribution to the Three Mile Island accident. We know that even by NRC inspection that different utilities get different report cards on how they operate and how they maintain and how frequently they do preventive maintenance so there are different philosophies among utilities and I think by attention to good design -- let me also say that I would not want to remove the role of the consultant to utility by saying that each utility must have all of its expertise on its own staff. That would not only throw a lot of good

people out of work but it also would give us inbreeding and while that's good for british flowers and complexions it can sometimes give us very narrow view of the world.

I think that good design certainly, the quality of construction and a high degree of intelligent operation and although Harold and I have had discussions about reactor operators versus judgement in operation and analysis in operation, I think we're really closer together than we are farther apart and I think that's the essence of recognition of what constitutes basically the successful nuclear experience. But we can debate this and go on from that for a long period of time. As a practical matter it doesn't make any difference. While I would wish it so, the NRC isn't going to go away.

DR. LEWIS: It could merge with DOE and call it the Atomic Energy Commission.

MR. BURSTEIN: And I think we're going to live with the need, not only to demonstrate to our lending institutions or what you call the insurance industry, but to the public, that we have indeed the ability to fulfill the promise that we made when we started out. Let me again make one point that so far, the beneficiaries of nuclear power are the customers and not the utility officials and not the utility stockholders. The rate of return financially is identical to the utility and its

IMAGE EVALUATION TEST TARGET (MT-3)

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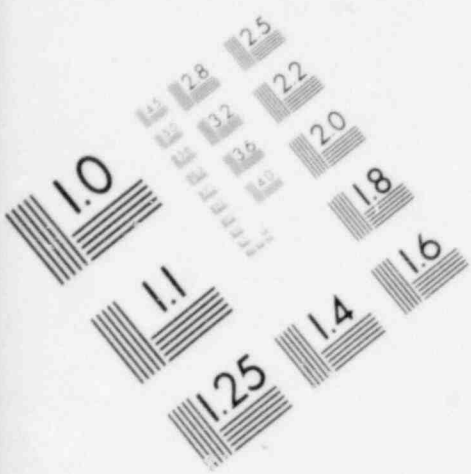

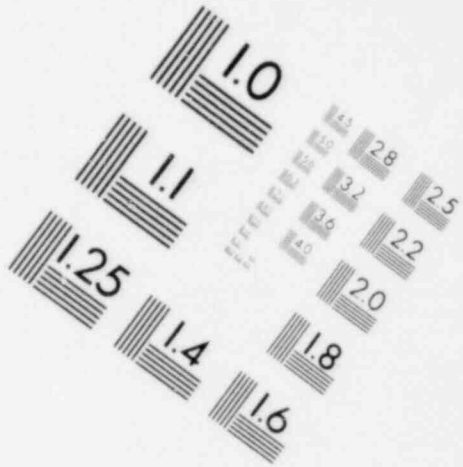

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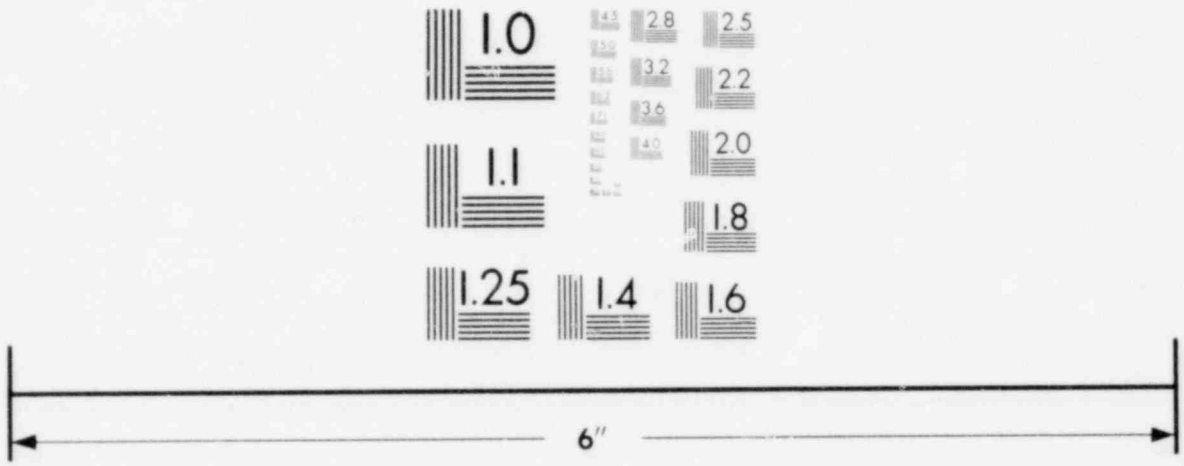

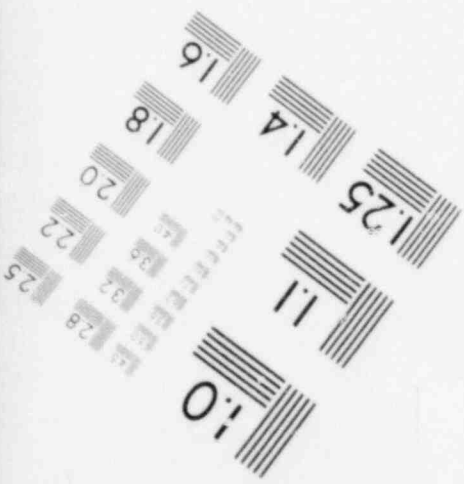

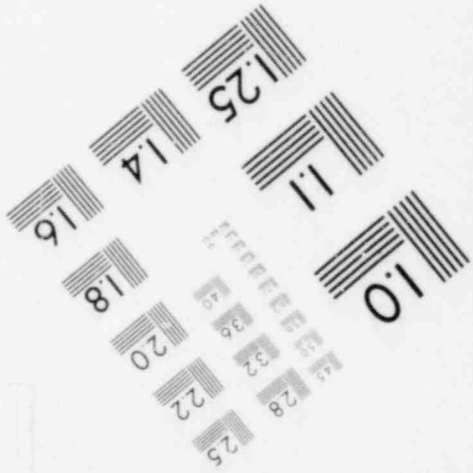

stockholders, whether it fills a coal plant or a nuclear plant.

DR. BEYEA: It depends on the rate.

MR. BURSTEIN: No, I said the rate of return.

DR. MAZUR: But you're making it on the more rate base, right?

MR. BURSTEIN: What does that mean?

DR. MAZUR: It means you've got more money coming in.

MR. BURSTEIN: Total dollars is not the criteria.

DR. BEYEA: No, that's not true. The rate of return -- depends on the success which you can raise the money to get the capital to build the plants you need to build.

MR. BURSTEIN: The point I wanted to make is that on the basis of risk, there is also financial risk in those areas as well as technical risks to the public and while we can go round these matters, as I said before, we're not going to resolve them in this fashion, however, I do think it is unfair and irresponsible not to consider that the utilities have just as much say if not more so in public health and safety as anybody else. End of speech.

CHAIRMAN KOUTS: Hal?

DR. LEWIS: I think this end of the table is beginning to wonder if we're wandering from our objectives.

CHAIRMAN KOUTS: We've accomplished it.

DR. LEWIS: In that case let me speak only for myself. I wondered whether we did.

CHAIRMAN KOUTS: I define the objectives.

DR. LEWIS: It's a subjective judgement.

I wait with baited breath but not held breath to hear what you say tomorrow we've accomplished.

MR. LEVINE: I have a request. Are you going to write a little thing that we can take home with us? Whatever you're going to say tomorrow?

CHAIRMAN KOUTS: Yes. It will be typed out tomorrow. But that will not be the end product of this. It will be the basis for the end product which I'll try to put together on the plane as I go back.

DR. LEWIS: I wonder if I could be even more intrusive and I think it's been very interesting to hear some of the extremely detailed discussions we've had around the table but I've tried to put myself in the position of our sponsor and mentor and asked myself what he's getting out of this and it seems to me earlier, that although he's derived a great deal of wisdom from what has passed around the table, wisdome that he surely had before, that it might also be helpful if we had some understanding of some general points that we really could agree on that he could take home as kind of -- if not directives, at least

a consensus of the group and it is in that context that I wrote those two pages for you and I wonder whether it would be appropriate to give me a couple of minutes to read them to people and see whether they represent any kind of view to which some people could subscribe.

I know it's late to start that kind of thing and I hate to delay the cocktail hour, is that a fair thing to do?

MR. LEVINE: Yes.

MR. BERNERO: If at the end of that if I could have five minutes to bounce one more thing off the group I would appreciate it.

DR. LEWIS: I don't want to take long, but a few hours ago I began to wonder about how this is all going to come together and I have a compulsion to try to write things down. What I wrote was I know in some ways objectionable to some individuals and it was my honest effort to think through and there are no numbers in it because it seems to me that numbers are things we, you know, require detailed study by groups both different and differently constituted than this and with more time, but what I wrote down was the following and let me read it through instead of screening each sentence because it's a page and a half. It goes like this:

One. There should be quantitative safety goals

in order to enhance the protection of the public, not directly but by making the regulatory process less capricious and more objective, and then A) an understood standard to meet to subject in the end to political test. It forces quantitative analysis of rules and sub-system standards and it provides a diminimous basis, a basis for diminimous standards.

Two. The goals should include a small number of hazard state or sub-state requirements or sub-goals so that not only a final grade be achieved. Every professor knows the difficulty of giving only a final grade is my comment to that. Every professor understands that.

Three. The licensing process should be deterministic with deterministic basis justified through demonstration that are assuredly meeting the safety goals. Both sub-system and whole plant analyses can contribute to this and it is recognized that in the present state of the art, a large element of judgement will 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 sub-system and to attempt to prove by analysis whether it better achieves the goal. That's different perhaps.

Four. The numbers associated with the goal require

political consensus with the development of techniques for calculation requires much more technical work. In particular, there may be some sub-goals for which the calculation is now beyond the state of the art. There is an interaction here between desirability and feasibility and then I put in c'est la vie. That's certainly true in my life.

Five. The goals should guarantee so far as possible that the public benefit of nuclear power is substantially greater than the risk which is part of the overall cost which should not be so unevenly distributed that any individual is unreasonably exposed to risk. This is an effort to meet the equity issue. It is recognized that this trade-off between public benefit and individual cost is inherent in any complex society and the issues are no different and no simpler here.

Six. The goals should be dynamic as technology progesses but grandfathering plants already approved should be normal policy in the absence of overriding the safety considerations.

Seven. Political consensus and public acceptance are essential for the end product but the responsibility of the NRC is to protect the public not to satisfy it. These are not always compatible and (if doctors were licensed through a public hearing we would have even more charming

quacks than we now do.) Then the ultimate recommendation.

Eight. A finer group than this should meet in a finer place than this to flush out these recommendations.

That is a serious effort to try to summarize some general conclusions which might conceivably be of value.

MR. BURSTEIN: I would take 7½ but that finer group business --.

DR. MAZUR: I think that's very good. Can I make one bit of an editorial?

To demand political consensus, not even to request it is an unusual thing because we don't operate on political consensus. Our decision making is based on decisions without consensus.

DR. LEWIS: By consensus I don't mean unanimity. I mean political acceptance. What we normally do, you know, we can't do things --

DR. MAZUR: But that's definitely not consensus.

DR. LEWIS: I see, I may have misused the word. Forgive me. I certainly don't mean unanimity. I mean, you know, that you can do it. In a democratic society.

DR.MAZUR: I'm grateful for your inclusion of the equity item. On the basis of that, I withdraw any other objection.

DR. LEWIS: I was not trying to make you withdraw.

I was trying to pick out the finer point of your position.

DR. BEYEA: I thought that the list by and large is excellent but I still have a problem though with the statement that the benefits should exceed the risk. I don't know how you make such a calculation except on an individual basis so I'd like some explanation. You said that the benefits of nuclear power should exceed the risk.

DR. LEWIS: Yes.

DR. BEYEA: Are you talking about dollars? Are you converting risk to dollars?

DR. LEWIS: No, I'm not, I said a finer group should meet in a finer place to flush out these principles. I'm not prevented -- we're now argueing -- I'm not prevented from establishing principles which I would find difficulty implementing. I usually try to start by establishing the principles and then try to implement them instead of setting the principles to match what I can implement.

And I think the ultimate societal objective is to make the benefits exceed the risks. I grant as well as anyone else. I've tried. It's very difficult. One way of doing it and there are many, is to translate everything into dollars and another is to translate everything into lives and another is to translate everything into loss of life expectancy. There are many ways -- that's the level of detail that we can talk about at great

length and I'm not particularly personally an advocate of any of these but I wouldn't like the general point. The objective is that the benefit exceed the risk and be obfiscated by forcing me to set a dollar value on lives which I'm happy to do but you know, my judgement about it would be different from yours depending on whose life we're talking about. It becomes very individual, sure.

MR. LEVINE: I just wondered, does that statement about that benefit being greater than the risk mean that you are likely to require that the numbers be set by means of a benefit risk assessment?

DR. LEWIS: No. Because the term -- I carefully avoided the term cost benefit analysis or risk benefit analysis or risk benefit technology or methodology.

Because, you know what I mean. There is so much masquerade under that that I don't subscribe -- that I don't endorse that but the general principle in some rational way one should make the benefits exceed the risks, I agree to. They just showed me Comar's thing which says the benefit should greatly exceed the risk and I don't agree with that, or the costs, I mean, more than the risks, because when we go into the store and buy things, we don't ask that their values greatly exceed what we spend for them. We ask only that it exceed what we spend for them. That's the way we run a society and I don't see why we should run this

particular aspect of society in an irrational way.

MR. LEVINE: I also didn't follow what you said about grandfathering. I'm not sure of the meaning.

DR. LEWIS: Grandfathering. Well, the concern there is -- maybe you don't want to have this debate on this document.

CHAIRMAN KOUTS: No, no, that's fine.

DR. LEWIS: What I mean is that you run the risk in a new thing like this which is -- forgive me, it is hard to do, that when you set the numbers for the first time, a finer group and ultimately the regulatory agency sets numbers, it may set them badly. In either direction. And, therefore, there will be a period as one gets experience with quantitative goals and with quantitative analysis in which these numbers will change. I don't want that to introduce an excessive unpredictability into the licensing process. So, I would say if a plant is approved at the construction level as having met a set of regulatory deterministic regulatory requirements which meet a safety goal and we later learn that that particular set of regulatory requirements doesn't meet the safety goal, I would normally grandfather the plant unless there really is an overriding safety issue. If there is an overriding one there is no question that that takes precedence over anybody's investment. That's what I mean by grandfathering.

DR. JOKSIMOVIC: A clarifying question.

You said licensing should be deterministic.

DR. LEWIS: Yes, that's my view.

DR. JOKSIMOVIC: As opposed to probablistic.

DR. LEWIS: That's my view.

DR. JOKSIMOVIC: I know what people mean by that but I'm not sure I know what you mean by that.

DR. LEWIS: What I mean by that is the general principle of law that a person should know what requirements he has to meet to get a license. And I don't know a way to do that without having deterministic requirements. Examples maybe clarify it better than anything.

The requirement, forgive me, on an airplane wing is declared in terms of a particular stress level it has to meet. That's deterministic. That stress level is determined through very complex analyses of the probability of hitting gusts greater than a certain speed of gusts with the fact that the wing loading is proportionate to the square of the sum of the airplane speed and the gust speed and it comes from the fact that with a given stress level, the probability that a wing gets blown off is at an acceptable point. Still, at the licensing level, what the person has to show is that the airplane meets the stress level. And that's my view of the proper role of the probablistic assessment at this level. Maybe when we're all

gone and other more learned, finer committees are sitting at this very table it will be possible to do better but I see it happening that way.

DR. JOKSIMOVIC: I recall that Reg Farmer and John Wreathall verified it, told me that when the British airport authorities applied for license for a blind landing for the planes at the London/Heathrow airport, they had to meet a quantitative goal. Are you opposing that sort of criteria?

DR. LEWIS: No, in a case in which the whole act can be subjected to a quantitative probablistic risk assessment, I have no problem. I don't think we're there yet and I have no problem --

MR. LEVINE: So you would say at this time?

DR. LEWIS: At this time, sure. No, if in the end and let me just say what trap I'm trying to avoid, the trap I'm trying to avoid is if when we're to go to that procedure for plant analyses, you know, in which you simply say you have to prove to my satisfaction that the probability of killing X people is less than PØ then I can only see that happening if one is -- I've said this before -- if one is prescriptive about the techniques -- if one takes the guide book for how to do probablistic risk assessment that is being developed at NRC and makes everybody use it and I think that at least at this stage it's not going to be that

fine a guidebook. Forgive me. If there were such a fine guidebook then I would have no problem with the final grade.

DR. JOKSIMOVIC: I haven't seen the guidebook for the London/Heathrow airport but I don't think it's that [illegible] either.

I don't see that there's much of a difference between that or let's say 10^{-4} for probability of core melt.

DR. LEWIS: No, no. First of all I think it was 10^{-6} per landing. I've even forgotten what the system was, it's not the same as R CAT 3. It's a different system and I'm really not that informed on it.

We have zero landing too. We have so-called CAT 3 landings, category 3 landings and I just don't know in any detail what -- of that analysis, but I assume that lots of pilots -- it's an automatic system. There were failure rates for the essential components probably measured by long run-ins on the components and these things were factored probably into credible estimates.

DR. JOKSIMOVIC: -- Reg Farmer. Are you familiar with that, John?

MR. WREATHALL: On which, this landing technique?

DR. JOKSIMOVIC: Yes.

MR. WREATHALL: The way in which it was done in

practice was a combination of analytical techniques and also experience. There had to be x number of landings carried out in perfect flying weather -- landings fully hands off. -- a demonstration of the analytical process. It was valid. But the question would be --- whether the analysis was valid. And if it was clear -- decide whether that analysis was valid or not. There wasn't a textbook given -- technique. That could have been queried, it wasn't. If it would have been it would have validated the cost analysis.

DR. JOKSIMOVIC: In my mind there is no difference between that and having a probability for ten miles for core melt.

DR. LEWIS: I guess in my view it is because that would be a much easier job.

But now we're haggling. As the joke goes we're haggling over price. Because I would certainly agree that the credibility of a full probablistic analysis that would product a probability consequence curve on which some reasonably random sample of experts agree, if that could be done in a credible way I would have no problem in gradually shifting the licensing over to that. I just don't think we're anywhere near that now.

MR. LEVINE: My understanding is that the analysis that was done in effect did not predict 10^{-6} but it was 10^{-5} and decided to go ahead anyhow. Is that correct?

That's what Farmer -- Farmer's analysis said 10^{-5}.

DR. LEWIS: I'm simply not sufficiently familiar with it. I do know that for the analogous system that the U.S. Cat 3 landing, it was not done in such a systematic way by our FAA but it's still acceptable for properly equipped aircraft and properly trained pilots.

DR. JOKSIMOVIC: Reg Farmer is of the opinion that we do the best analysis in the nuclear field.

DR. LEWIS: Well, this is an issue we could perhaps resolve then later because as a general principal, I do not believe that at this time but I don't know what this particular analysis was. I do know what we do in the aviation field and in this country, and we're simply dealing with simpler systems at that level, also remembering that the fraction of time which an airport is below a hundred feet which is category 2 which is a triviality, is infinitisimal. The fraction of a time that an airport is under instrument conditions which is below a thousand feet, the last time I remember looking at the numbers, the worst airport in the country is Los Angeles and that was only 10%. So the vast majority of the time a landing, well, you know that from experience is a triviality -- every now and then it comes down to a few hundred feet. For my airplane my limit is 200 feet -- I can only go below 200 feet and I've only once failed to make it in on a landing.

but I didn't get killed.

DR. JOKSIMOVIC: What is the probability?

DR. LEWIS: I'm only saying that such a system gets tested very rarely.

DR. JOKSIMOVIC: Oh really? At London, too?

DR. LEWIS: Yes, at London, too.

MR. SALISBURY: They've cleaned up all the fog there and stopped burning coal.

DR. JOKSIMOVIC: I would appreciate it if Hal would put out a statement over there to this effect, that these -- temporal.

DR. LEWIS: I would have no compunction about putting in a statement which would go roughly like this. This -- I hate the word methodology but this is a dynamic field and ultimately it may be that one can sufficiently credibly predict the probability consequences curve for reactor accidents with sufficient skill that one could use them directly in the regulatory process, but I personally do not think that time is now.

DR. JOKSIMOVIC: Yes, but what do we have now?

DR. LEWIS: We have a deterministic system which is based on --.

DR. JOKSIMOVIC: What was that to bear in mind what we're comparing against?

DR. LEWIS: That's right.

DR. JOKSIMOVIC: And I've been --- PRA as a tool for making decisions than we've been doing in the last x years.

DR. LEWIS: Of course you know I agree with that but it's the level of decision that we're talking about and I believe that is a far better tool for making the decisions about the deterministic requirements that go into the licensing process.

DR. JOKSIMOVIC: But do you believe that as a result of what we do somebody has to say at the NRC that in the auxiliary feed water system we need three pumps? Or would you let designers make the decisions and its much better to tell the designers they must meet the goal of 10^{-3} but demand or something as opposed to how many plants and how many valves and that kind of stuff.

MR. SALISBURY: Didn't you agree earlier that there should be a transition period?

DR. JOKSIMOVIC: Yes.

MR. SALISBURY: Why do we keep going over the same thing?

DR. MAZUR: I think Hal is saying the same thing with slightly different words.

DR. LEWIS: There was astonishingly little dissent. I'm shocked.

MR. LEVINE: I think Herb has some additional

points.

CHAIRMAN KOUTS: I have quite a bit to add.

DR. LEWIS: That's right. This was a core, I hoped of things.

MR. BERNERO: For some time I've been looking forward to a finer group than this --in another place. --- safety goal, and I recognize that many probablistic analysis are done on plants and a concern that I've had for some time now is that in doing an analysis of a plant, one is frequently analyzing a part of the plant -- a safety concern arose. Is the auxiliary feedwater system seismically qualified or did you discover that there were defective motors in some system and one is frequently confronted with a situation wherein all you have is a single accident sequence calculation and probably a rough one at that.

You don't have good confidence bounds on it. It's a best estimate, -- and all. And in attempting to deal with that, a number of us have discussed it at length inside the staff and we came up with the following logic, at least this is the logic I hold.

Some years ago, Wash-1400 calculated the probability of core melt and the probability of containment failure and the probability of early fatality and the probability of latent cancers and so on, and there was great controversy

about it. The controversy was centered on the credibility of the analysis and certainly of the analysis and not whether that level of risk if credible was acceptable or not. It was almost a tacit consent if you could really show that the risk is like that it's acceptable, so I start from the point and say that Wash-1400 and later analyses that may be incestuous -- there may merely be children of Wash-1400 they suggest that the probability of core melt is in the range of 10^{-5} to 10^{-4} per year and in that context we said before and I think most reasonable people would agree the dominant risk sequences in that range may be worth fixing so that if one finds an accident sequence in a random analysis for a plant for which there exists no complete spectrum of analysis, all you know is that one thing, that if you calculate a core melt accident sequence it's probability is in the range of less than 10^{-5}, leave it alone. If it's in the range of 10^{-5} per year to 10^{-4} per year it may be worth fixing. And if it is higher than that, to a reasonable degree, you can trade exposure time for level of risk with the following: The plant is nominally rated to operate for about 30 years. So if the probability of core melt is 10 times higher than that Wash-1400 rate, than the exposure time ought to be at least 10 times less than that, so that if the probability of core melt is in the range of 10^{-4} to 10^{-3} per year then you ought to fix it but

it is reasonable to take up to a few years to fix it and if you go up another decade then it's only a few months to fix it. And if you go up another decade in a few days you shut it down.

That's what was -- by the way it's mis-typed in that draft, NUREG0764 or whatever the number is that was handed out. I wonder what your reaction is to that primitive logic or how to deal, just as an interim measure, how to deal with the occasional accident sequence calculation that cannot be dealt with with any consequences -- you can't assign solid confidence bounds to it. You can't say how it compars to all the other risks in that plant because you haven't calculated them.

MR. LEVINE: The problem is that in all your discussion here you've ignored what size release there might be in that sequence. If the size release is 1/10th of the largest release in Wash-1400 has essentially no calculable consequences, so I think you have to be able to pin some consequences on it to be able to think about it.

MR. BERNERO: Admittedly. One has to look at it and say, is the -- there are a number of factors that would bias that. Is it a very serious release, or a mild release? Is it a popular site? Is it Indian Point or is it Palo Verde or Crystal River or some place that's surrounded by alligators. And also who did the analysis?

How thoroughly did he do it. Many of those analyses are primitive in the extreme.

MR. LEVINE: I would be very hesitant on making any judgements on primitive analyses, as a matter of fact.

MR. BERNERO: There would be highly objective correction factors, too. But the core of the logic -- it's happening every day that individual do things, a variety of methods of calculating core melt probability. And immediately there is a great bewilderment -- how urgent is that? There's one that the ACRS has bounced around with us for some time. Auxiliary feedwater system reliability for older PWR's where the seismic design criteria were not applied to that system. There's a very primitive analysis of the probability of core melt caused by a loss of auxiliary feedwater --

MR. LEVINE: Due to an earthquake.

MR. BERNERO: Due to an earthquake. And that would be a fairly serious release.

MR. LEVINE: You don't melt the core just due to loss of auxiliary feedwater; you have to have some other things happen, too.

MR. BERNERO: The analysis tried to trace core melt due to --

DR. LEWIS: You obviously need some other failure.

MR. LEVINE: You have to lose electric power.

And that's a very tough analysis to do realistically -- the seismic part of it.

MR. BERNERO: I agree it's very tough, in fact the technology --

MR. LEVINE: --- asked me how much credence I would give to a primitive analysis of that.

MR. BERNERO: I - more asking the group for their reaction as the logic of using Wash-1400 as an acceptable level of risk until some better basis is available. Just asserting that if the level of risk calculated by Wash-1400 were real, at least for the time being, it could be considered an acceptable number.

MR. LEVINE: I have no quarrel with the numbers.

MR. BERNERO: And secondly saying that one can trade exposure time for level of probability.

MR. LEVINE: I have no problem with that concept. I have no problem with the numbers you spoke about but I am very leary about the competence of the analysis that would be made and you're just going to have to look at those one by one.

MR. BERNERO: And make a subjective correction.

MR. LEVINE: I don't know whether it's subjective or what but I think you have to look at the analysis very carefully. I don't know who is doing those analyses, I don't know if they're people who thought they were ---

MR. BERNERO: You can only make the judgements when you look at the specific analysis, but the core thing, if it's a virtually perfect analysis of a single sequence is what I'm asking you, you know, if it's the best available analysis --

MR. LEVINE: It's a very good analysis. I have no problem with your numbers or your concept.

DR. BEYEA: It sounds logical in many ways but I would also want to know how long -- it might only take a month of two to fix -- why would you give them, why would you say three years unless the analysis might be wrong. It seems the only time you come up against a constraint is if it will take a long time to fix.

MR. BERNERO: I put it in a way which a regulator is forced to put it. You have until to fix. It's a limit more than an objective. It wouldn't be an objective unless it took about 3 years to fix it. It would be designed and analyzed and fixed and bound the time.

DR. BEYEA: I would think you'd want it fixed as reasonably as -- or AFARA Principle.

MR. BEYEA: As fast as reasonably achievable.

MR. BURSTEIN: I think I have to agree with Jan that sometimes it's a matter of the practical, perhaps, procurement manufactured delivery of accessories or devices to repair and one might have a real serious potential problem.

The option is either to shut the plant down for two years while you wait for that piece to come in or to perhaps assume a different risk level because perhaps the costs, the social costs of being without that energy for that period of time might be significant in comparison to the potential risk and we do that and have done that all the time. So I have some trouble with the methodology again because I'm not sure what the nature of the fix could be. Obviously if you come under the curve, if your several sequences show that breaker DB47 comes up 16 times as one of the causative things, you look at that instantly because you could replace or fix a breaker in a matter of a few days but if you have to install a third pump as an example, that might take 3 years. And I don't know what you do in the meantime.

MR. BERNERO: That's the very thing I'm talking to. It is a rationale for saying yes indeed you can wait until the next review until shut down to replace that device, or yes indeed you can wait until the cable manufacturer provides that specialty cable which is three years hence, or no I'm sorry, you cannot, the level of risk is too high. You either fix it in two days or the plant is shut down.

DR. JOKSIMOVIC: The probabilities aren't going to tell you what the level of risk is.

MR. BERNERO: One has to correct the analysis -- it's a core melt.

MR. LEVINE: If it's a small release you have plenty of time. If it's a very large release --

DR. MAZUR: I'm not sure. You seem to be addressing it as if there was an answer you want us to give. And obviously it's a judgemental thing. There isn't an appropriate clear response.

MR. BERNERO: Principally, the thing I'm interested in is your reaction to using the calculated level of risk in Wash-1400 as an assertable standard until the doctor comes with another one. A better one, a more finely tuned level of this.

DR. MAZUR: Wash-1400 is given that whether manifestly or latently the way it functions as an assurance about nuclear power plants it surely seems to be a very low statement of risk. As these things go, if you want just gut responses then I would think if one could be assured that that level of risk would be able to be adhered to, given a -- thing anyway, I'd certainly have no problem with it.

MR. BERNERO: Then the other is the trade-off, the exposure time. Now, there are going to be many subjective corrections -- does that probability of core melt really represent risk -- was that analysis a best estimate or a very conservative bound or a highly -- you know, there are many many biases that cannot be stated

in formulas --

DR. BEYEA: Why not take 1/10th of it? Why not take ten times the Wash-1400 probability to be sure -- to be conservative.

DR. MAZUR: But it's so super-optimistic already.

DR. JOKSIMOVIC: Are you saying that instead of using ACRS or Levine or Joksimovic or Kinchen, you want to use Wash-1400 numbers in this type of analysis?

MR. BERNERO: I mean temporary core melt probability. Goal or limit.

DR. JOKSIMOVIC: That's not telling you about the risk.

MR. BERNERO: That's a luxury I don't have.

DR. MAZUR: You're saying you want to know if you've got a quick thing to worry about.

MR. BERNERO: Yes, an index of --

DR. MAZUR: I'd like to go to what Jan said. In some of our discussions, if you recall the UCS added a fa-tor of 100 to Wash-1400.

DR. BEYEA: On probability?

MR. BERNERO: On risk. And if one looked at this curve which Saul handed out which is really -- Wash-1400 as a similar one. If you took the reactor risk with the present scale of reactor's order of magnitude, -- and even multiply it by a 100, one might still argue that at least

temporarily acceptable.

We just used Wash-1400 --. What you're really dealing with is a 1 in 12,000 probability because that is in a year.

MR. BERNERO: Well, I just made it linear. A decade in probability is a decade in time.

DR. LEWIS: What troubles me a little bit is in terms of trading time for risk, that's obviously done everywhere all the time, but Wash-1400 as it came up yesterday is a particular reactor. There are other reactors right now out there if you were to do the same calculation would come out substantially higher.

MR. LEVINE: That's just what he's talking about.

MR. BERNERO: I believe that.

I find there are toe nails, I find there are thumbs and I find there are left arms.

DR. LEWIS: I thought you were talking about newly invented sequences. The same old sequence done for a different reactor.

MR. BERNERO: No, I mean any sequence done for any reactor where that's all I have before me, is a one accident sequence.

DR. LEWIS: But the same sequences that were in Wash-1400 or maybe new ones depending on the design.

MR. BERNERO: S2HF in the ice condenser was a new

accident sequence. And S2HF was a given proba--TPI for Gran Gulf sits on the table before me now. 8×10^{-4} per year. What should I do with it?

DR. BEYEA: Plus or minus how many factors of 10?

MR. LEVINE: The first thing you have to do is make sure it's a competent analysis.

DR. LEWIS: That's right.

MR. BERNERO: And what biases are in it and what conservativisms, how thorough.

MR. LEVINE: And then find out how large the release is. If it's less than a category 3 forget about it.

DR. LEWIS: I'm going to have to call Dan right this moment. That's a joke.

MR. BURSTEIN: Herb, did you have some things you wanted to add?

CHAIRMAN KOUTS: Not a thing.

MR. BURSTEIN: I thought Saul said you were going to embellish on Harold's --

CHAIRMAN KOUTS: I'm going to embellish it, but tomorrow.

DR. MAZUR: May I just ask a question? Unfortunately I have to leave early in the morning and will miss a lot of the summation, but could you just sort of give us your gut reaction in terms of what kinds of expectations you had yesterday morning. Is it disappointing, is it --

MR. BERNERO: A finer group in a finer setting might --.

I'll tell you what my reaction is. I felt that we came into this workshop with a lot less homework done in advance than we should have had.

MR. LEVINE: By the NRC.

MR. BERNERO: By the NRC. I think we could have been much better structured and much clearer in what was asked of you and much clearer in how we structured the activity to gain the best fruits of your labor. However, I have found a higher yield than I expected considering that lack of structure. I think, although I can't speak for the other two groups except for what I heard this morning, I think that we are getting more than we deserve out of it.

DR. LEWIS: One constructive suggestion is that a larger ratio of plenary to panel session would have been fruitful because the way it is now, the introduction which I unfortunately missed yesterday, there was a little bit of round tabling this morning and that's it. We have a final report tomorrow.

MR. LEVINE: I would speculate that if this group had been expanded slightly with some other disciplines that it has the other two panels will determine to be unnecessary. I'm speculating based on what I heard this morning not what

we're going to hear from them finally. I may be wrong.

DR. LEWIS: I rarely agree with Saul, but --.

MR. BERNERO: Let me put out a tentative proposal and see if you have a comment to make on it. I would think that the next round would consist of a much more structured proposal of a goal with advocates of that proposal not only ready to explain why the goal is constructed that way but to begin by presenting the goal and why it was structured and why the numbers and why the logic and all that stuff.

DR. LEWIS: It is a peer review of a proposal.

MR. BERNERO: And then the group would be redundant-- there must be separate groups but their charter would be the same. They would each go off in their corner and independently critique that and then come back in the grand ballroom.

DR. MAZUR: That's very interesting by the way, if you maintain it and raise the issue of how reliable are the critiques. Does one get a different critique depending on what group you gave it to or do the groups come to some agreement on these proposals which is an interesting issue and the critique of the proposal depends on the composition of the group and you can't put too much credence in the critique but if you get a similar critique from several groups operating independently you've got a lot more faith.

MR. BERNERO: I've done it that way before on other

issues, other workshop structures, and it's been quite satisfactory.

CHAIRMAN KOUTS: Let's be clear on what was expected out of this--what was expected out of this operation was not to derive a safety goal, not choose among safety goals, not to establish anything particular about safety goals except to discuss, one safety goal in particular and then other safety goals as well as they had been proposed. One safety goal in particular because it had been proposed to the commission in a normal document and other safety goals because they had been proposed by various individuals with a lot of thought and a lot of work over a period of time and out of this, to try to establish whether the approach which is in here or this approach is suitably modified or some other approach suitably modified or some mixture of all of these things -- the structure at any rate of all of these approaches could be used as a basis of the next step in the process. And the next step in this process is one which is to be taken in house by the NRC.

Now, before, when is it George, July is the time being thought of for the next step in this process?

MR. SEGE: The next paper due to the commission is scheduled for August of this year. ---

DR. JOKSIMOVIC: When is the next workshop then?

MR. SEGE: The next workshop based on that schedule

would have to be held probably the second part of July sometime.

CHAIRMAN KOUTS: So there has to be a proposal formulated for that time which is based in part on documents which are gathered into the NRC and based in part on comments on these documents which have been generated in the course of these meetings and that will be the stalking horse for the next group that will be meeting to consider this matter.

It was not meant to arrive at this next step at this meeting by any means. It was not meant that we go out of this meeting with a proposed safety goal by any means.

And all of the round discussions which have taken place here I think really do contribute to input to the next step even though they may not have generated one thing in particular they've generated the aspects of that thing in particular that you've fabricated.

Well, it is 24 minutes after 5. Our schedule says 5:30 we break and I think Parkinsons has almost won again. I think returns can become negative if they diminish far enough.

(Thereupon, at 5:30 p.m., the meeting 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

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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)