Safety Goal Project

NUCLEAR REGULATORY COMMISSION (45 FR 71023)

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In the Matter of:

WORKSHOP ON QUALITATIVE SAFETY GOAL

PANEL B

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NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING

WORKSHOP ON QUALITATIVE SAFETY GOAL

PANEL B

Edwards 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. Lester Lave, Panel Chairman.

PRESENT:

Messrs. Bradburn, Bridenbaugh, Derby, Eisenbud, Hutt, Cerbone, Libarkin, Maxey, Sheldon, Temme, Zebroski, Whipple.

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P R O C E E D I N G S

CHAIRMAN LAVE: Let's get started.

I'm going to try and only flip on the overhead for a short period of time today, because it interferes with the recording. It occurred to me when Norman and I were talking on the way over here that one could think of qualitative goals, process goals and quantitative goals. In the course of doing this, I think that McClain's remark was right to the point. That you have to decide what measure of consistency you want.

If you use a process measure of consistency, then it's clear that quantitative safety goals are going to lead to inconsistencies. And so, you have to decide what is the proper measure of consistency that you want.

Second the matter that one has to look at among these three types of goals is to what extent will these goals obtain none safety goals. Let me just present you an argument about that. If one has in mind is trying to quantify a vast number of attributes consistently and quantitatively and there's no alternative -- there's no real alternative to preventative cost analysis, at least that generic framework does all of these things consistently and there just is not an alternative to doing that and so, if for example none -- such none safety goals as economics or aesthetics and so on are terribly important and it is necessary that be treated in

some consistent fashion, then I don't believe that either qualitative or process goals are going to wind up doing what we want. We have to spend --

That's a set of introductory comments.

MR. DERBY: I have a question.

CHAIRMAN LAVE: Go ahead.

MR. DERBY: After we have this discussion, what should we -- In having this discussion, what should we aim towards? Some set of things that we feel or statements that we feel we can support or not support?

CHAIRMAN LAVE: Our alleged mission which we have to spend at least five or ten minutes on is dealing with qualitative safety goals and it occurred to me that it doesn't make any sense to talk about qualitative safety goals without at the same time talking about process goals.

Now, we have process set out as our item number six for much more explicit consideration and we don't need to go into that in detail now, but I think that we do have to say a word about qualitative goals. I have a sense that that's sort of a strawman that was set up in the this whole process. That is that they want to have a set of arguments that say qualatative safety goals such as as low a risk as possible don't make any sense and would like us to set out a couple arguments as to why this is so. But perhaps I'm inferring other people's intentions too much.

MR. DERBY: That helps me a little bit. Reading all of that stuff, I was a little bit -- It was a little hard for me to understand what would be -- What I would write down as a list of possible qualitative safety goals, just, you know, whether they were good or bad if someone looked at them and said, yes, that's a qualitative safety goals. They may not like it or they may think it's inadequate. Certainly, I would assign a label to this statement as a qualitative safety goal. Is there a large list? Is there a small list?

MR. TEMME: If you look at that list of examples that we were provided with, there are a couple of things in there that you could call qualitative safety goals. There are some other things in there that I think you could call qualitative safety standards.

MR. HUTT: What is a qualitative safety goal? Just as an example.

MR. TEMME: No individual shall receive his own undue burdent of risk.

DR. EISENBUD: No.

MR. TEEME: As low as reasonably achievable.

DR. EISENBUD: No. I'll give you two. One is that the industry should operate at least as safe as other generating --

MR. TEMME: That's kind of straddling qualitative and quantitative.

MR. HUTT: I'm sorry. We went through this before. I regard that as quantitative.

MR. DERBY: So, do I.

MR. HUTT: Because you can't tell whether it is as safe as anything until you quantify both and compare them.

DR. EISENBUD: That's a philosophical conclusion. An intuitive conclusion --

MR. DERBY: Maybe there's a set of words that imply quantitative measurement and a set of words that do not.

MR. BRIDENBAUGH: We're probably talking about the difference between policy and procedure.

DR. BRADBURN: I think there are two possible distinctions. One has to do with whether the distinction is between more precisely and more vaguely quantifiable. That is sometimes you have things that I think of as basic quantifiable. That is something that is low. Low being a sort of -- you know, as opposed to high, so you have some order kind of thing. Or as compared with X. You have something -- Or equal to. That implies a -- In order to make sense out of it, implies there is some sort of underlying quantification that is possible. But it may not be possible to precisely quantify it and you may -- the kinds of terms you use maybe this and this called vague quantity, infrequently. That's a vague quantifier.

Now, the other one -- The other approach as to what

one might call qualifying and in this sense is as low as is what is reasonable and proper, I think of as a kind of -- I think that to my mind, the criteria for those sort of things, that you want, in fact, terms which are in their nature changable. That is, what I think of when we term quality - is setting a standard which is essentially dynamic and you don't want --

MS. SHELDON: The prudent man.

DR. BRADBURN: And precisely you want terms like that because you are assuming that over time, they will change and that you cannot precisely and do not want to precisely -- because you want it to be about change.

And when you set a quantitative goal like so many deaths per thousand or whatever,that gives you a fixed quantity and once you set it, it would be very hard to change it. If you have a qualitative standard in that sense. That is deliberately appeals to some concensus of standard concensus, which may or may not be there, but and you have your process set up to figure out or interpret what the prudent man would do or what is reasonable and proper and what's as low as possible or whatever, then you are saying that we don't want a set of standards that are not only immutable, but even terribly rigid and we want to develop something as we go along.

MR. HUTT: Let me suggest that Congress, itself, has never said in any statute and I think I said yesterday,

anything but a qualitative goal. Because the word, safe, itself, is qualitative not quantitative.

MR. EISENBUD: The Clean Air Act --

MR. HUTT: Is qualitative, not quantitative.

DR. EISENBUD: The Clean Air Act has quantitative goals. It actually specifies what the reductions should be and the emmissions of carbon monoxide.

CHAIRMAN LAVE: For automobiles.

DR. EISENBUD: For automobiles.

MR. HUTT: Okay, I stand corrected. That makes the point, though, that virtually all sort of generalized safety statutes rely upon vague language that it's of almost no utility whatever to a regulatory agency in trying to regulate on specific applications of that general language.

MS. SHELDON: It's sort of an old generation of environmental statutes.

MR. HUTT: Yes.

MS. SHELDON: The news, Clean Air Act and TOSCA and RIFRA and whatever are --

MR. HUTT: Sorry, TOSCA and RIFRA use exactly the same incomprehensible language.

MS. SHELDON: There's a trend towards more specificity in those two than NIPO or --

MR. HUTT: What TOSCA says is that there shall be no unreasonable risk. That's all the relevant statuatory

language. So, it is the rare exception of which you may have given me only one and there may have been others in history. Even the Delaney Clause is not really quantitative, because as Lester pointed out, it all depends upon how good your detection methodology is as to what the current standard is. Zero doesn't mean zero as everyone knows.

Therefore, I have to conclude that anything short of real numbers,and a comparison involves real numbers, is qualitative.

DR. EISENBUD: That's why I think that if we describe as our goal, this new industry should be at least as safe as the industries with which it is competing, namely coal and oil and gas and so on, that's a qualitative goal.

I'll give you another qualitative goal --

MR. HUTT: I think we'd better come to closure on that, because there's a vast difference in this group as to what is quantitative or qualitative.

DR. EISENBUD: Nobody has come up with another example of a qualitative that we will accept.

MR. HUTT: As low as reasonably --

DR. EISENBUD: That doesn't mean anything.

MR. DERBY: I don't think yours means anything either, because --

MR. HUTT: Not unless you can quantify it.

MR. DERBY : Hasn't the effort that has been put

in the nuclear business from coal and what's the competing industry, solar?

DR. EISENBUD: We could declare a national policy that we don't want to have any malnurished children in this country.

MR. HUTT: We've already done that, I might say.

DR. EISENBUD: That's a qualitative statement, but to implement that policy, you've got to get into some very highly quantitative matters.

CHAIRMAN LAVE: Let me just follow up this thought for a second and get back to what Norman said a minute ago. I think that the point is that on this specific occasion in the 1970 amendments of the Clean Air Act, where Congress set down emmission levels for automobiles, they learned to rue that. That is that when Congress comes in and writes specific things in the law, that's terrible, just terrible, because it is so cumbersome to amend the statute and Congress knew at the time when they put that in, that those numbers were not really fixed numbers that were easily defended.

This discussion has convinced me that Congress should not, in fact, try and write quantitative safety goals into anything. That, in fact, if you want to give these the status of law, then you want to do it in a process where it is much more easy to amend them than a Congressional statute. As for example, agency rule making, where I take it, that is much

easier to amend than getting Congress to change the Clean Air Act. And so one might thing about a sort of tiered system where Congress puts in a qualitative safety goals that every body can salute. The agency then, tries to translate that into some numbers for each generation or for each decade or whatever it is, you know, as of current state of income and society and the current state of knowledge which we believe that as low as reasonably achievable means the following quantitative safety goals. Which of course then get periodically -- You can even have the agency have it's own rule making pass out of existence every decade unless affirmed or something of that sort.

DR. ZEBROSKI: I think I agree with the concept, but I wonder if the process wouldn't be better done the other way around. It seems to me that you have -- You can get fairly readily estimates of risk levels, of probabilities or consequences which would represent ideal practice. The best that the technology could provide, given perfect execution, or near perfect execution. Those generally would be very very pleasing kinds of situations if you believe them. Then, you could say in reality, you never get perfect execution and there are acts of God and acts of human carelessness or sabotage which degrade the system somewhat and so you try and make the system resilient to that.

So, the idea of setting a goal which is near the

ideal, of course, leaves you open to infinite litigation. It's equivalent, if you set the goal near the ideal, then it's equivalent to legislating the business out of existence, because the regularity predictability disappears.

If you set the goal or a process -- it really can't be a goal but a process, on what you perceive to be as far into the concern of the public as you can go with the intention that the typical value be somewnat better, then you have at least eliminated some of the litigious potential or reduced it to those things which are real.

Let's assume that the real goal comes out somewhere in between the tension between these two extremes and then the qualitative, but even so, it won't be a goal that can be expressed as a number, even though there's a ten to the minus fifth number that is floating around in the U.S. and is essentially the number used in Britain. It's meaningful only if you have a large manual of practice that goes with it. You can't really describe the goal as a single number. That manual of practice which makes the goal meaningful is basically incomprehensible to the public. So, it seems to me that one role of the qualitative goal is to represent as fairly as possible in everyday rhetoric what the effect of the quantitative goal and its associated practice is likely to be.

Not pretend, however, that the qualitative goal really is the guide for the regulator, because it's unworkable.

It leads you to an endless mish-mash.

MR. DERBY: There's something here that's emerging that's the difference between a goal and a standard, I think. If we can somehow explain that difference and what one is used for, I think that what we're saying is that there is a statement of purpose and perhaps the nuclear counterpart to the Delaney amendment would be something like no one will die as a result of a nuclear accident. Something like. It would be the goal.

That doesn't help anybody try to regulate nuclear power, but that certainly is a qualitative goal. I certainly support it and applaud it, which is one aspect of a qualitative goal, but I don't know how to take that to a standard.

MR. HUTT: You would support?

MR. DERBY: Sure. I mean I don't particularly think any --

MS. SHELDON: It's a great objective.

MR. HUTT: No, no, but I mean, you really would be willing to devote the country's resources to achieving that goal?

MR. DERBY: No, no. Somebody says, is that a great goal as compared to if someone should die as a result of nuclear accidents, I would say, well, gee, given the choice between those qualitative goals, how much we should devote I don't know. How hard --

MR. HUTT: Wait, wait. If you're going to say it's a goal, that means that you're willing to devote time, effort to achieve the goal.

MR. DERBY: That's what I'm saying. It's not a great goal, but it's certainly --

MR. HUTT: I don't think it's really a useful goal.

MR. DERBY: I haven't heard that's been useful. I just wanted to throw it out there. It's something that I haven't heard so that you can talk about what things like -- that sound like that. I mean, that's as good as, it should be less risky than competing industries.

MR. TEMME: No, it's better than that one.

CHAIRMAN LAVE: In much of the recent legislation, there's a preamble that sets about what Steve just said. If you take the Clean Water Act, the Clean Water Act says no discharge -- zero discharge in the waterways by 1985. The Clean Air Act, visability, not other -- And I am offended by reading the preambles to the Acts. Nonetheless, if there is any point in which one is going to put this pablum, then, that's the place, but that does not state safety goals. It is just there. By asking Congress --

MS. SHELDON: No legal effect, either.

MR. HUTT: I'm sorry, a lot of those preambles are in fact reinforced by court decisions. I can cite a good many instances where they have absolutely been critical along

with legislative history that reflects the same kind of diffuse goal that is totally unrealistic and unachievable and it confuses the process.

MS. SHELDON: I think it's very hard if you're talking about Congressional intent unless it's accompanied by something substantive in the statute.

MR. DERBY: That's my point. We have a point here to make a statement about qualitative goals that say what's good and bad about them. We've got a whole host of words like this that we dislike. YOu don't like them because you don't know what to do with them.

MS. SHELDON: I can't win with them.

MR. DERBY: You don't like them because you can't win with them. The regulator doesn't like them because they --

MR. HUTT: Set unrealistic goals, unachievable goals.

MR. DERBY: -- unachievable goals.

MS. SHELDON: That doesn't do anybody any good.

MR. DERBY: It doesn't do anybody any good. Maybe we could describe as part of this qualitative goal, part of the things that we could say is that there are goals and we may be able to list them and give our opinion on them and say, good, these are silly. What is it about a qualitative goal that is useful and is there any such goal that has been enunciated yet? And if not, can you lay one out?

DR. BRADBURN: Is there anything in legislation, regulations and so forth, which says something like life is risky and you have some sort of general standard of what ordinary course of event, whether it is prudent or not prudent, demands risky -- a normal operating risk level? And then making some sort of comparison?

MR. DERBY: It implies some sort of quantitative or procedural --

MS. SHELDON: Very often, activities are measured against that kind of vague of standard. You're judging whether some thing is ultra hazardous and in cases languages is usually, is this the normal level of risk that would be expected in the community from some activity.

MR. HUTT: The specific statuatory language of unreasonable risk, which you find in the Consumer Product Safety Act and TOSCA, in particular, will put in there to encompass two thoughts. One is yours and the other is then weighing the benefits into the process, too. Both of those concepts are built into that statuatory language.

DR. EISENBUD: Lester?

CHAIRMAN LAVE: Yes.

DR. EISENBUD: I hope all of you have read either Dave Okrent's article. I'm sure there are others. I can't think of others at the moment. In which he listed the number of dollars spent in various industries to prevent a death.

MR. HUTT: Which article is that?

DR. EISENBUD: It's in Science, about two years ago. It was, you know, he calculated how much money goes into highway construction for the purposes of eliminating accidents. It was quite impressive, because in round numbers, we generally spend somewhere between maybe $20,000 a year -- $20,000 or a few hundred thousand dollars to prevent deaths. In the nuclear industry, it remains higher than that. What is known as a magnitude.

I think that some of you may have detected that my thinking is a little bit different and it would probably be the concensus of whatever comes out of this panel. I suppose it's because I have to -- having been in the public health field for a long time, I have to think of this field in relation to other fields in which -- the kind of attention that we're giving this question, we could do some good.

There are about two million deaths a year in this country. Of those two million, there are eighty thousand very premature deaths because of smoking. Associated with smoking are about a hundred thousand heart disease -- the cardio-vascular system -- heart disease or stroke. There are twenty thousand suicides, many of which have an environmental component. Suicides are much higher to the contrary of what most people think. Suicides are much higher. The suicides among the underpriviledged than the part of the community

that is more priviledged. Many of the suicides live in what we call the ghetto.

There are twenty thousand homicides and that has environmental component although, not always. There are fifty thousand automobile accidents. Certainly a big environmental component in that. There are about two hundred thousand deaths that could be premature deaths that could be avoided if we equalize the socio-economic conditions of the country. I don't know how we could do that. I don't know that it would be desirable or even possible, but the point is that among the poor people, the death rates are so much higher. We're not talking about enfant mortality, which is very much higher. I'm talking about the people in the middle years of life. The death rates. You could easily eliminate 200 deaths in that way that are entirely environmental.

Among the more fortunate, there are about one hundred thousand deaths that is estimated and this is hard to really get at. I don't know what you would think. Not because of malnutrition in the under nutrition sense, but because people eat the wrong foods. And there are about twenty thousand deaths of cirrhosis of the liver, most of which are due to alcohol.

MR. HUTT: Half of those traffic accidents are due to alcohol.

DR. EISENBUD: Due to alcohol, too. But the point that I want to make is that 25 percent if you add it up and I just jotted it down and added it up. About 25 percent of all death have an environmental component, which if we could put the kind of energy into the solution of those components, that Karin, your firm and the firms that -- the organizations you deal with, we could have a very very substantial effect on the tables of mortality and morbidity in this country.

If we eliminated all energy and somehow or other could get our energy so that we don't have the bad effects of not having energy, I doubt whether even after a decade, you would be able to detect with the best of statistical techniques that Lester could gather, any changes in the tables of morbidity and mortality, unless you get -- until you get into the occupational groups and see what happens to miners and so on.

You know you see these big figures on the number of deaths due to air pollution and fossil fuel combustion which I don't happen to subscribe to, but assuming that they're correct, you still couldn't measure that difference. You could not measure 50,000 respiratory disease deaths. I don't know what the figure is Lester. Around 50,000 out of the total number of respiratorial disease deaths that occur among the --

So, what I'm saying is is that I think we should not

become so zealous in our approach to what needs to be done in this industry that we lose sight of the fact that we are living in a very complex society in which we normally have accidents and deaths and other forms of power generations and occupations that people have throughout the social system that we've evolved. I'm not sure I know how the risk would relate to what we're doing here.

I don't expect that the Rapporteur will put any of it into the report of the meeting, but I think that it is something that we should bear in mind.

CHAIRMAN LAVE: Can I give a kind of an answer to that? Then Karin can correct me. I would guess that first of all that -- I was co-chairman of task force for the American College of Preventive Medicine trying to look at what preventive medicine could do and many of the deaths that you are talking about Merril, can't really do anything about. Unless we're prepared to snatch cigarettes away from people or get rid of alcohol and inforce it, we just can't do those things.

DR. EISENBUD: We're doing it. Do you realize that it has become unpopular, socially unacceptable to smoke. Is there a smoker around this table? You go into restaurants and people don't want to admit. The cigarette consumption is going down --

CHAIRMAN LAVE: Thirty year olds.

DR. EISENBUD: All right. And we've got to do the same thing. The environmental organizations ought to be getting at the young kids in the schools instead of teaching them to go out and pick up aluminum cans and worry about PCBs. They ought to get that kind of zeal into getting them to quit smoking. Help explain to them what happens to the babies of mothers of smoking three packs a day.

They're not doing that.

CHAIRMAN LAVE: I agree with that, but the other -- the problem with nuclear. There are a set of people who are concerned about routine admissions from nuclear plants. I regard those people as simply ignorant. They just don't know what the facts are. But when you get away from that, I think the nature of the concern is about accidents and about waste disposal and if people believed the Rasmussen numbers on accidents, then I don't think you would have a lot of quarrel. They don't believe those numbers and they don't think that those numbers are wrong by a factor of two or a factor of three. They think those numbers are wrong by a factor of a thousand or a million.

That really gets back to the epistomology stuff that we were doing. But, I think that's the nature of the concern. And one can't pat them on the head and say,there there, look at these numbers. The numbers do, indeed, speak for themselves. The numbers are real. That is, if you believe

those numbers on nuclear, then it is hard to say, okay, I am anti nuclear. I don't think we ought to do that. The numbers are -- and that really is a matter of getting people to appreciate that they live in a risky world. But at least the objections that I hear are not that no those numbers are too high, we have to make them lower. They don't believe the numbers at all.

MS. SHELDON: I think that's true.

CHAIRMAN LAVE: Would you, without putting your back up against the wall, would you subscribe to the notion that at least it is the not the belief in the numbers that is the principle problem with the anti nuclear. That is what they pick out --

MR. BRIDENBAUGH: What was your question, Lester?

CHAIRMAN LAVE: I'm going to withdraw it. I was digging a hole and seeing the dirt --

DR. ZEBROSKI: I think the risk risk thing needs to be part of the qualitative goal statement and it may be one of the -- it may be the real role of the qualitative thing is to help test the validity of the target values for the quantitative goal.

Let me take my own digression into Merrill's field. I was struck by a trend analysis of homicides in New York City. On the present trend, the life expectation of adult males living in New York City that he has one chance in

sixty of being a victim of a homocide.

That's only the beginning.

DR. EISENBUD: That's not uniformly distributed. It's socio-economic.

DR. ZEBROSKI: Let me reinforce that. If he's a black male, the odds are one in twenty and you -- people react and say, that's impossible. And then you multiply out 2,000 a year in the population and you get these spectacular numbers.

Now, that's the good news. The bad news is that New York City is seventh in ranking on this scale of issues. So when you -- And you say further that we have done many things in legislation or in court decisions which effect those numbers by enormously greater amounts than the total energy or industrial side of society. Then you get to the question of are you going to be a consequential person or legislator or politician if you go after something that you perceive to be the public fade and neglect the ones that are much more important.

Statistics in the California cities are interesting also. San Francisco has gone down in murder in the last decade, but it leads in burglary and rape and again you can relate some of these things to some of the actions taken by town councils and by polic departments and judicial decisions. So, I think the consequential person would say we are doing

things in society where some seemingly simple decision make a far greater effect upon human misery and death risks than the things which we are looking at which are in ten to the minus fifth range. So, it suggests to me as another test for the quantitative values that one might have for safety goals is that they bear a reasonable relationship not just to other energy technologies, but to the total sources of death and injury in the environment.

This is basically what the British -- I have a British document which in effect is their equivalent of the safety goal statement and what it is is a great many criteria and procedure statements as well as numerical statements. But, they say that it is only the general envelope of getting a risk level from their nuclear enterprise which is of the order of tenth to the minus fifth effect on potential death per year per person which they say is then smaller than the local variations, almost block by block or city by city in that statistic from other causes.

So, if you're down on the noise level, it's pointless to do more on that, because if you over allocate resources to reducing that, you're increasing the deaths from other causes.

MR. HUTT: True. But you have to be careful not to carry that down to the noise level to an extreme, i.e., you don't look at the worst area. This is what we were talking about yesterday during the break. You don't compare risks

to what I regard as two things in this country you can't compare -- or three things we can't compare -- we came up with. Automobile fatalities, cigarette smoking and liquor. Those have special places in our society due to social and other emotional factors that you can't rationalize and I don't think we ought to say that as long as we're safe in cigarette smoking, we're okay. I mean, that to me is a useless exerise.

MR. LIBARKIN: There's an argument that that noise level proposal may ignore.

You can do that all right for the things you know about, but there's a large component of possible effect that you don't know about. I've known people and still know people who hold to that view with an almost religious fervor. That you just don't know enough to be able to make that statement. They can't be specific. I don't know what it is they mean, but that's a view that is fairly wide spread and it's not going to be dealt with by --

DR. ZEBROSKI: I've heard that argument and Dave Okrent is discussing that in the other panel this morning on the question of how big a risk aversion coefficient you should put in to allow for unknowns, but I'm pointing out that that is exactly a two-edged sword. We make a -- We can pass a law or a judicial opinion on the extent to which we restrain pschopathic murders. We have made a social decision

that they're eligible for parole on fairly short times and they go out and kill again. There is one spectacular case of a guy who killed 120 women that they know of and maybe a great many more that they don't know of. In last months Readers Digest if you're interested in a really horror story.

But, that came in part because we made a decision whose consequences were not perceived. So, it cuts both ways. The unperceived consequences of the noise level are just as important as the unperceived consequences of particular energy technology and here again, I think we're getting into the area where because you hae studied something so intensively, everybody makes this remark. We know more about toxicity effects of nuclear materials and radiation than we know about toxicity of iron and nickle and lead which have been in society for thirty centuries.

So, the limits to knowledge is the academics copout. It seems to me that is just as bad as making legislation that make jobs for lawyers. It's saying that since we don't know something, we should do more research before we decide. That's true across the whole board. It's not unique to this area.

CHAIRMAN LAVE: But there's a problem with your argument, Ed. In a sense it comes down to saying that if the Commissioner of FDA can't do something about cigarettes, then he shouldn't do anything and unless we can get at the biggest

problems, then there's no point in doing anything around.

DR. ZEBROSKI: I'm trying precisely to offset that one, if you make it comparing with coal or other energy sources, because the obvious point there is that you can do something about coal emmissions and people are doing things about it. The Clean Air Act is having a lot of effect and perhaps on SO_2, we've already killed the issue that the data on SO_2 now is beginning to look like the requirements are far tighter than the probable effects as they were perceived some years ago.

I don't think we're going to relax those requirements even if the biological data becomes very convincing that the SO_2 thing was over reacted to, but -- So the comparison with something which itself is amenable to easy change, I think, is a trap. But if you compare it to the whole noise level of local variations in morbidity or death, from all costs --

MR. HUTT: The English approach?

DR. ZEBROSKI: The English approach.

MR. HUTT: Which is laid out in this yellow book.

MR. BRIDENBAUGH: Isn't there though implicit in that view that -- You said this yesterday, I think. There's a finite amount of resource that we have that we can devote to regulation or research or making things better. Implicit in that view is that if you don't spend it here, you will spend it there. If you weren't working on making nuclear

plants safer, you might be out stopping people from smoking, let's say. Or convincing them that they shouldn't.

The two don't necessarily -- Aren't necessarily related.

DR. EISENBUD: They are in away.

One of the things that I deplore and we have discussed this among ourselves in the department where I work is that so many of us at the level of professor and we have 12 professors. Five of us are associate directors. That's a fairly large department. And a very large percentage of our time is spent in what I would call defensive science.

We go to meetings and I would almost have to include this one in that category as one that is probably not deserving of three or four days of my time. When there are so many other things that I could be doing. On the other hand, the subject has such visibility that I think that we want to participate so that at least hopefully we can have something constructive come out of it. I'd much rather be trying to find some way to keep those CETA kids employed this summer and keep the playgrounds open. They're going to be closed in New York City, because they don't have -- It's an environmental problem. Kids have to play in the street instead of in the playgrounds.

These are the things that we need a mobilized community to deal with and we haven't got them, because that

part of the community is worried about the wrong t ing.

MR. MALSCH: Don't you think it's difficult, though as a practical matter, for say, the chairman of the NRC with responsibility over only nuclear power to decide, say, for example, not to spend ten million dollars on improving emergency core cooling systems because ten million dollars would be better spent by some one else to decrease cigarette smoking when he has no control over what is spent on cigarette smoking and no insurance that the money will be spent for that purpose.

MR. HUTT: Let me deal with that. I don't think that obviously he could say that in those terms. But what I found lacking from this yellow book. I can't remember the numbers and names. Is it called NUREG? NUREG 0739, was any context except in almost in footnote form. There was no context of comparison with the safety of other sources of energy and absolutely not a shred of information about how these various target numbers of safety were reached.

When I talked to the author earlier at the break, he said that a lot of work had gone into and he has done a lot of work. It was mentioned a Science article that he produced a couple of years ago. Into precisely that kind of issue of what a societal concept of acceptable risks may be. A very very difficult and slippery concept.

YOu don't have to have those two pieces of informa-

tion and make them controlling factors in every decision, but they certainly are relevant information against which you can judge your own conclusions.

MR. MALSCH: Well, they're good to judge and sort of sense a proportion on which you're doing. I don't know how good they are in reaching precise conclusions.

MR. HUTT: I completely agree with you. You can't base everything on it, but not to have them here at all. I found a real lack in this document, frankly.

DR. ZEBROSKI: There could be tests of the guideline targets, but not administratively --

MR. HUTT: To make sure that you're in the ball-park among other things and I think that I mentioned yesterday that FDA did that when it picked a level of acceptable risks for carcinogenic animal drugs. What it did was went back and it didn't use it as the controlling factor, but it looked at what other acceptable risks were and said, we seem to be in sort of the same general area, so, we can't be too far wrong. That's the way Martin, that I think it can be very usefully used.

MR. LIBARKIN: It sounds like some of you are proposing to rewrite the executive summary to WASH 1400, which was a disaster. I hope that's not the kind of thing that is being suggested again.

MR. HUTT: I don't understand what you mean.

MR. LIBARKIN: I'm sorry. That document was a very -- I'll give you my view -- a very expensive and vigorous and good exercise for its time and still today and it was very carefully done. Although a lot of the care that went into it wasn't make explicit.

MR. BRIDENBAUGH: Are you speaking of the executive summary or WASH 1400?

MR. LIBARKIN: I'm talking about WASH 1400. Then somebody and I'm not clear who took some of the results and put them into what they thought was a relevant context which compared them with all kinds of other funny things like snake bite and lightening strikes and drew some conclusions about how we all ought to think from now on about white water reactors because of that. Which doesn't didn't stand up to the test of public acceptability or close review by other people.

It was embarrassing for everybody involved with it, I think, after the fact. And it called down some -- on the Agency and I don't really think that personally that it would be fruitful to do that all over again.

MR. MALSCH: It did get the Price-Anderson Act renewed.

MR. LIBARKIN: That is what it was intended to do and it did and thereafter if we could have burned it, it would have been fine.

DR. BRADBURN: Let's expand on exactly what was the nature of what was wrong with it. Was it because that we were using the wrong comparisons or that we were using --

MR. LIBARKIN: They were using comparisons that might be relevant or might not be relevant. They were relevant in the sense of being sort of statistical conclusions about how many people a year die from various things and it was obviously argumentative and it didn't allow for any suggestion that perhaps while that may be true that the nuclear power plants that as far as we know them can be said to perhaps pose less risk than these other things, there was no hint that there was any question at all that the numbers that were being compared were not of the same quality.

MR. HUTT: Anyone who deals with these numbers has to first of all distinquish between the hard and the soft numbers. There are some extremely hard numbers, mainly we can count how many people died in automobile accidents. It's not very difficult. They're dead and there's a pretty good record of them and those -- in fact, those records are exceedingly good.

MR. BRIDENBAUGH: That's his point. They compared the hard numbers, too.

MR. HUTT: You can. You have to be very very careful to make clear what you're doing. I don't think anyone around here wants to deceive anybody, but to say that there

should be no comparison, I find just unacceptable to me. To say that if you're going to set a safety level in a vaccuum, is impossible. You might as well go right back to as safe as reasonably achievable or just an 1800 concept.

CHAIRMAN LAVE: You can only make relevant comparisons. Relevant. That's the key factor. You're taking a look at snake bites and something else, they may not deem to be relevant comparisons.

MR.HUTT: I concur on that. Among other things you've got to choose voluntary risks and compare that with voluntary risks and choose involuntary risks and compare that with involuntary risks.

CHAIRMAN LAVE: There are a whole set of dimensions that are relevant here that are just a wider rate and I think that one of the problems may be that you may not find a set of comparisons with nuclear that the majority of the public would deem to be a relevant comparison so that you can look at these.

MR. LIBARKIN: There is also a confusion that I was just guilty of.

You're talking, I think, about comparing a goal that one sets or a standard with what is already going on. In fact, what I was talking about and what happened was that you took two specific power plant designs and reached some conclusions about the risk of those plants and compared them

with those other things. It's not quite the same thing.

The comparison with a goal in the abstract maybe a wonderful thing to do, but as it was done, it wasn't done and I guess I'm concerned about what will happen in the future. It was not left at that. It was then turned into a conclusion about all of the existing, at that time, light water reactors. It was not exactly the same thing.

MR. HUTT: But what concerns me is that we've got a lot of different almost conflicting views here. One view is that the driving force that in part comes from the courts and in part comes from the frustration of regulatory agencies themselves to quantify so that they can do a better job.

You have two recent court decisions. In fact, more than just two recent. There's a court decision that came out three years ago under the Consumer Product Safety Act where CPSC had required a warning on all swimming pool slides. Warning that a paraplegic. And it turned out that the risk of paraplegia from going down a swimming pool slide in the wrong way was one in ten million. That was a document risk and the courts overturned that and said that's too small a risk to warn against. That is not a sufficient risk that people should be warned about.

So, agencies are being forced and then there was the Supreme Court decision on the Benzine case saying that OSHA had to deal with significant risk. There was the FDA decision

in the acrylonitirle bottle case saying that FDA had to decide what level of risk was dimenomous before it could -- It just couldn't say we're going to ban acyrlonitirle across the board.

So, you've got the courts and plus, as I said, the regulatory agencies and on the one hand trying to figure out how to codify and how to set a level of dimenomous or acceptable risk, which ever way is significant or insignificant risks. It's the opposite side of the same coin.

On the other hand, I hear people around here saying you can't compare anything to anything and the public won't accept comparisons and if you do so, it's politically infeasible and it will just get you into a lot of problems.

Now, you can't do both. YOu can't have it your way and the other way. You've got to go one way or the other. Either you're going to quantify and compare or you're going to give it all up and go back to my version of the 1800 theory of regulating and just say that nothing can be injurious to health. Just have ad hoc decisions that have no rationale whatever.

CHAIRMAN LAVE: Peter, while agreeing with a large part of that speech, let me try again. I guess that some of the comparisons that I have seen produced for nuclear power seem to me to be extraordinarily insensitive and the question at least from a social science viewpoint is, what are the

dimensions that make a comparison relevant.

MR. HUTT: I heard Karin say that almost no comparison was relevant.

MS. SHELDON: No, I didn't say that.

MR. HUTT: Maybe you're the best person to ask what comparisons would be relevant?

MR. SHELDON: Well, I'll tell you one that isn't relevant. The risk of living around a nuclear power plant. A risk of being injured in an accident from a nuclear power plant which is next door to you as the same as flying in an airplane. That kind of thing.

MR. HUTT: Can you explain why that is not relevant?

MS. SHELDON: Well, it's a voluntary involuntary situation to begin with.

MR. HUTT: On the contrary. I can live wherever I want and I can choose to get into an airplane or not. I have equal choice on those.

CHAIRMAN LAVE: Excuse me. I'm going to let Chris Whipple come in here.

MR. WHIPPLE: I'm listening to all of this. I was asked to give a paper at a meeting two months ago on the uses and abuses of risk comparisons and as a result I read through them and tried to find out what those people liked and didn't like about them. I think I got some explanations that I'll suggest.

One is that often risk comparisons are given simply to provide people with calibration. That is what does ten to the minus five mean? In which case it has some relevance maybe a nuclear plant and an airplane provided that you're simply trying to provide a sense of the scale of the numbers.

But just as often the comparisons have implicit decision logic in them. Which is, if one tenth to the -- if one ten to the minus fifth risk is acceptable,so should be all ten to the minus five risks. And that's a faulty decision logic if you don't also go into cost effectiveness, control opportunities; if you don't go into benefits and a lot of other things.

I think a lot of people are upset because you see comparisons of skiing with nuclear power with airplanes with birth control pills with no sense of what the decision logic is. No sense of what the alternatives to any of those things are and yet the feeling that you're being shoved to some conclusion that something is acceptable or not.

So, I think if you get back to what Peter was talking about earlier on cost effectiveness comparisons, that's a lot different than gross risk benefit comparisons. That has some relevance, because that points for opportunities for resource reallocations that can save lives. Looking at the gross numbers, you'd better be careful what you're asking those comparisons to do for you.

MR. HUTT: Let me ask Chris, is it in your review of this. Let me tell you why I responded to Karin the way I did. Certainly it is valid to compare voluntary risk against voluntary risk. I view where I live as entirely voluntary within reasonable limits.

MS. SHELDON: You're lucky.

MR. HUTT: I say within reasonable limits within the sense that one could move ten miles in any given direction without a hell of a lot of difficulty in an area where there is public transportation, let's say. If I have to live in Washington, D.C. If it were a tiny town, I agree with you that it would be a harder thing to do, but --

MR. DERBY: Where the reactors are.

MR. HUTT: But, if in addition you factor in that there you could get the same job in a lot of instances in a town a hundred miles away, it is not all -- And we have an extraordinarily mobile society and probably ninety percent of the people in that tiny town came from somewhere else and therefore could move somewhere else. I don't see why that isn't at least pretty much voluntary. Certainly it is as voluntary as getting into an airplane which in many instances is less voluntary. I had no choice how to come out here. No choice whatever. Less choice than where I live.

DR. BRADBURN: But you're weighing the decision to come in the context of the fact that --

MR. WHIPPLE: I'm not sure for your case there might be other distinctions that are more important than voluntary. There is always the degree that you are compensated for your risks and as part of your job you have to make business trips in an airplane. One is the degree in which risks are compensated. I think that's probably as important as the voluntariness. The other is the degree to which individuals have a feeling of personal control over their risk taking which comes out of some of the psychological studies --

MS. SHELDON: That's part of voluntariness, too.

MR. WHIPPLE: Yes, but -- I guess I've run into the distinctions of -- is driving to work voluntary or involuntary and being unable to answer that, trying to look for other determinants.

But, you're right. There is quality factors and dimensions on risk like the degree in which it is catastrophic. The degree in which it is compensated or in public or private. I think that Lester has used those terms before that are very relevant. So, comparing recreational activities with nuclear power are clearly inappropriate. Those are opposite ends of some spectrum of voluntariness. Transportation systems seem to be in a grey middle area. Drug risks seems to be certainly more voluntary than nuclear power risks, but not exactly in the same category with skiing and motorcycle riding.

MR. HUTT: Have you ranged these on a scale? Have you done any kind of research trying to range them on a scale? I haven't, but Paul Slovic and Brook Fishoff have. They've asked people to evaluate risks and rank them on voluntariness, degree to which there is scientific knowledge, degree to which there is public knowledge. There are seven or eight --

MR. DERBY: Stress,which is one. Anxiety.

MR. WHIPPLE: People seem to be able to consistently rank these dimensions and then when you try to look at a determinant of attitude toward perceived risks based on these dimensions, unfortunately, it gets pretty weak. The catastrophic potential seems to be the strongest factor and that cross correlates tightly with being involuntary.

In order to write a function that says here is how people perceive and respond and accept risk based on its dimensional characteristics, nobody is there. I don't think you can get that.

MR. HUTT: Let me ask it in a different way. In the bases of what you know about it, do you think that trying to use risk comparison is just a no win situation and I don't want to mischaracterize what Morton said, but what he seemed to imply just shouldn't get into risk comparisons or do you think it can be done to some extent?

MR. WHIPPLE: I think that it is very helpful provided that you make your decision logic explicit. That is

we want to choose an energy system nationally and we want to look a coal verses nuclear.

MS. SHELDON: That, I think, is the most relevant comparison there is. Because we have to have energy. The public knows that and what they don't know is on a sort of quantitative bases how nuclear stacks up against coal.

MR. WHIPPLE: There's a second level which is a cost effectiveness level. We want to have, you know, two day hold up of BWR radiation releases verses four day hold up kind of thing. How many dollars per REM are we talking here? What other risk reduction opportunities do we have in the nuclear field? How do those compare with other risk reduction opportunities throughout society and these are the kind of comparisons that --

MR. HUTT: You say that is valid? Because I thought we had generally concluded that it wasn't. Unless it was in a very closely related area, i.e., someone said -- I think it was Martin -- how do you justify we won't spend a million dollars more on reducing the level of risk of nuclear energy. Instead, it should be put into reducing cigarette smoking.

MR. WHIPPLE: Again, you have to put those qualifiers in that the NRC isn't going to give their left over budget to FDA. And as you get further and further away --

MR. HUTT: That's my secret hidden agenda.

MR. WHIPPLE: -- the cost effectiveness criteria themselves become less useful. But I think your concept as a mental reference point and going back to the idea of being calibrated. I mean the fact that people now talk about two hundred, three hundred and a million dollars per life as a cost effectiveness figure comes out of the fact that that there has been cost comparisons between different decisions by government agencies. You can get a rule of thumb.

CHAIRMAN LAVE: Let me qualify and perhaps contradict a little bit of what was just said. There's a paper that was just done by Graham and Votell trying to look at implicit values of life and various decisions by government agencies. You'll find a vast range in --

MR. HUTT: Where was that paper published?

CHAIRMAN LAVE: It has not been published.

DR. EISENBUD: Can you give us some idea of what the range is?

CHAIRMAN LAVE: About ten thousand dollars to three or four hundred million dollars.

DR. EISENBUD: Which is the highest industry?

CHAIRMAN LAVE: Well, no. They don't even use what I think is the largest one. If you looked at the value that you put on astronauts lives in one of the moon missions. By setting up that fire escape system. If you calculated out what the probability was of being able to save somebody's life

and what the thing cost, you are certainly talking in the trillions of dollars to the implicit value of life that you put there.

So, the range is vast and I guess I would not expect there to be very much if any comparability between agencies in these decisions. Most agencies are really surprised when you tease out the implicit value of life and say what you guys are deciding. They sit back and say, is that right? They don't have any idea how to react to it, even.

MR. HUTT: There's not consistency within any agency. There is no attempt to be consistent within an agency.

MR. WHIPPLE: Except DOT, there is a --

MR. HUTT: Okay, but within EPA or FDA on daily decisions on drugs and pesticides. It is a subject that is not even considered.

CHAIRMAN LAVE: I think that one has to be careful in trying to redefine numbers and say that because this agency did that or because the rip in a dam is so much that it must be that people have thought about that and it's the right number. I just reject that. It is not true. People have not thought about it and some of Dave Okrent's comparisons with hydroelectric projects or dams in general just seem to me to be -- or Canvey Island is the one. You point to Canvey Island. Canvey Island seems to me to be a scandal.

I've not talked with a lot of English residents, but I would have guessed that they regard Canvey Island as being a scandal. And it's hard to say, well, we're going to be a hundred times better than Canvey Island. They would start throwing eggs at you. That's not acceptable.

That's the wrong kind of thing. It's like saying that our drugs are going to be ten times better than Thalidomide, wonderful, great. That's why we'll strive for in the future.

MR. WHIPPLE: By the way, I think that Okrent has done so much of the -- he's done some of the risk comparisons, but the cost effectiveness comparisons, I think Dick Shwing has done most of those.

CHAIRMAN LAVE: That's right. Can I just differentiate to make sure that everybody understands? One of the things that I really object to in some of these statements about trying to get risk -- making risk comparisons commensurate is because they're talking about one of the two blades of a scizzors. That is you're talking about what the risk level is, not what it would cost society to do something about that.

Let me just assert that a risk level of one in a million, it could cost you nothing to get rid of that, is something you ought to do. That's precisely the usual Delaney Clause case, where you have one, a multiplier identi-

cal with 24 others in terms of cost and property, but this one is suspected carcinogen and the question is, should you take it off the market? I think the obvious answer is yes, of course, you should take it off the market. I don't care if the risk level is one in a million or one in ten million. Those are easy kinds of cases.

MR. HUTT: They're so easy, of course they never arise.

CHAIRMAN LAVE: Certainly never arise as important issues.

It seems to me that without trying to make this into something bigger than it need be that it is having more calculations done that need to be made. That it is really cost effectiveness that you want to look at in each case. What will it cost you to reduce the risk further. That that the kind of comparison that we always ought to be talking about.

Not to say that any risk of ten to the minus sixth is acceptable or ten to the minus seventh is acceptable. It is just not true. It depends what it would cost you to do away with it. You just can't arbitrarily take a number and say any risk that is more than that is okay.

MR. WHIPPLE: NUREG 07300 does both or tend to do both.

CHAIRMAN LAVE: I no.

MR. WHIPPLE: It sets both risks and cost effectiveness criteria.

MS. SHELDON: It also depends on the risk. Is it risk of sudden death? Is it risk of your kids having leukemia? Is it risk of getting cancer ten, fifteen, thirty years down the line?

I don't know. I'm very bothered by this. It gets so etherial. I mean this is where people say, you guys are all nuts.

MR. HUTT: Well, Karin, I have great sympathy for that, but the problem is that the people who are driving towards some rationality are generally the people who are in there in the agencies making the decisions. I sat there for four years and had to decide ultimately -- because I had to sign every regulation that went out -- which drugs would get approved. Which ones wouldn't. Which animal drugs would get approved and leave residues potentially in human food and which ones wouldn't. Which food additives and color additives would stay on the market and which ones aren't.

What people on the outside frequently don't understand is that there is no such thing as a regulatory vaccuum. You either make a decision -- If you decide, I'll put that off until next week, you are saying that the American public will be exposed to that chemical for the week that I'm making my mind.

MS. SHELDON: Inaction equals action.

MR. HUTT: And therefore, what you're saying is -- and this is where I've always had my difficulty with my scientific brethren in the academic universities who always want to do more tests before decisions are made -- that you've got to make a decision on the spot and you hope that it's as good as you can make. You never have enough information to make that decision. You never know whether it's the right decision or the wrong decision, ultimately, because there is no way of verifying it in the real world. In the food and drug world where you've got twenty thousand different chemical components of the food supply and if you either leave the new food additive on the market or take it off, you'll never know whether you did good,bad or indifferent.

Whether, for example, it might be replaced by another one as I think happened when cyclomate came off the market and saccharin took its place that was more dangerous. I don't thinkthere is very many scientists that disagree with that proposition that cyclomate is clearly under any standard what ever safer than saccharin. But these are the kinds of things where you're got two choices. Either you're going to let people like me exercise pure judgment and say in the pit of my stomach, I think this stuff is okay or it isn't on the bases of all of this generalize scientific information laid out before you where you have ten minutes to make a decision

on a typical day at FDA or you're going to say, we're going to try and quantify it to the extent that we can recognizing all the worts and blemishes on the quantification process to get a little more rationality. More, not total rationality into the process.

It isn't a question of -- I would say, of quantification being the magic bullet. Here's where I've argued with Arthur Upton when he was a head of NCI. He was opposed to risk assessment, because he said those aren't real numbers. To which, I said, I don't give a goddamn whether they're real numbers or not, I think it will help me make a better decision than I would make without those numbers.

That's where the heart of the issue is. Because I am convinced thirty years from now that someone will come along and look at what I did in an embryonic way back in 1972 with risk assessment and animal drugs and say, Peter you were dead wrong. Now, we have a much better answer. Just as in 1972 I was looking at the two part per billion answer saying that that is dead wrong. I've got a better answer in risk assessment. This is a process of improvement the entire time and that is where I get terribly frustrated as the former regulator that the public doesn't understand that it isn't that you're looking for the answer, the solution to how to how to regulate. You're just looking for something better than what you had yesterday.

Sorry for that little speech.

MS.SHELDON: That's fine. I think we understand that. I guess, if I was being naive and simplistic, I would say, let's not have that attitude or that process or whatever until we know.

MR. HUTT: Then what you have is no food. You have no food.

MS. SHELDON: Well, we've had food throughout the history of human existence, before we had butilated hydroxy toluleen and all the rest of the stuff that takes up the entire side of a cardboard box.

MR. HUTT: Okay, but the problem is that what we're discovering now is that the natural foods that we've had all of these years are more dangerous than the synthetic ingredients that we're now putting in foods.

MS. SHELDON: I think that's debatible.

MR. HUTT: Okay, but when you come up and when you show by the best scientific evidence available today that the biggest sources of cancer in the diet are pepper, tea and peanuts, then you've got to think a little bit. And probably, the one of the hypothesis --

MS. SHELDON: -- grandma's homemade whole wheat bread is going to be worse for me than fresh Horizons or whatever the hell it is that has wood pulp.

DR. ZEBROSKI: Would your grandma make bread for me.

I don't have a grandma to make by bread.

MS. SHELDON: You should learn how to do your own.

MR. HUTT: But, Karin, the point is is that there is this holistic philosophy that if it's natural, it's got to be good or whatever and that has caused just enormous difficulties in a regulatory agency where you can prove that that is just flatly false in all kinds of food products and all kinds of natural sources of danger in the environment.

MS. SHELDON: Sure. Any of these things, if you jump on whole scale, there's no absolutely truth in any one of them. It's a reaction. It's a trend. What have you.

I think we've gotten side tracked.

MR. HUTT: Let's get back to the point and that is is the public prepared, because if it isn't prepared, we might as well give up. Is the public prepared to accept the concept that quantification is an attempt to step forward. Not to solve, not as I said to have a magic bullet that is going to solve the issue of what is safe or how much safe is acceptable. How much safety or risk is acceptable, but just one step forward in a regulatory process trying to harmonize a lot of very difficult issues and make things a little more rational and a little more consistent, hopping that we'll, in the future, find an even better step forward.

Because if your answer is that the public just doesn't accept all of these goddamn risk comparisons. It is

all a bunch of malarky, we might as well go back to sheer judgment. Maybe a regulator is better off setting himself up as god and saying, I will hereby pronounce that this is safe and this is unsafe rather than trying to quantify. Even though it is infinitely more irrational.

MR. DERBY: I would like to talk about --

CHAIRMAN LAVE: Wait a minute. Wait a minute. I would like to give Karin a chance to say something, if she chooses.

MS. SHELDON: I think that quantification is important. I also think that having a qualitative safety goal is important as well. If you have both of these things functioning in a given system, that the numbers ought to reflect the best available information that you have. They ought to be, this is what we can do or as close as what we can do and it reflects conclusions about safety. And then it becomes almost a minimum standard. You at least have to get here, if the numbers are at all realistic and then the dynamic and flexible part of the goal on top of it and I see goal as an objective rather than a rule and I think we are confusing the two -- is something that says, every day and every way we try and make things better and better.

That you overlay those on to p of the numbers so that for each particular facility, each site, each daily operation, there is an objective -- a principle in operation that you

take the numbers.

That isn't all that you have to do. But, you at least come to that level and then you continue in a precise specific way to try to improve what you're doing.

MR. HUTT: Where it is cost effective.

MS. SHELDON: Where it is cost effective to do that.

MR. HUTT: That's in a statement in a sense that this NUREG document, the two approaches, the quantitative and the qualitative.

MS. SHELDON: I think you need both. I think you have them to some extent in the present regulations with the ALARA. Now, I have some problems with that ALARA over the numbers, but I think it's a good way of going about things, because although the designers and the engineers want numbers and I think that's necessary, you need to provide and we haven't talked about this -- You need to provide the managers and the people who operate these things on a daily bases with some guide posts.

Once we have the numbers in place, is that enough? The answer is no. In operating the machine and in carrying out the daily functions, there has to be an objective for operation that you are after and that is where the qualitative safety goal comes in in terms of the agency.

It doesn't simply make the public feel better. I think it's got to be a management principle. You build the

machine as best as you can. You build it to meet certain quantifiable goals, but then you operate it to make the qualitative safety goal.

I saw this well expressed in the District Courts opinion in the Karen Silkwood case. Where, unfortunately, the company took the position that all it had to do was to meet the numbers and that made its operations safe. That that represented limits for the leases and what not that the NRC had implicitly or otherwise indicated that were safe for the public and because what was in Karen Silkwood's body did not exceed the limits prescribed by the NRC; therefore, there was no legally cognizable injury. The court did not agree, of course, but wended its way through the standards and these low as reasonably achievable principles and said what is required of this industry on an individual facility bases is to meet those numerical standards, but to every day and for the particular characteristics of that facility and that site go further and try to make that operation as low as reasonably achievable.

And, of course, he found that what had happened there did not meet that.

DR. EISENBUD: What was the size of the award?

MS. SHELDON: Ten million dollars in punitive damages for negligent contamination.

DR. EISENBUD: Do you think that's morally, poli-

tically, economically --

MS. SHELDON: It was Gerry Spence who was the trial lawyer. I'm not sure that that will hold up on appeal, but I was referring only to the District Court opinion which was scholarly and I think discussed how I would see that system as working and I think that the public will feel much less apprehensive and much more trusting of nuclear power if both of these things go along.

And the advantage, I think, was discussed earlier by Mr. Bradburn in terms of the ability to be dynamic. The flexibility that is inherent in the qualitative goal on top of some quantitative -- ramble, ramble. I'm sorry.

MR. HUTT: No. I find that very helpful.

DR. EISENBUD: I was just going to rise to a point of information, which I suppose I can talk to Karin about afterwards, but I think that it does have some relevance. I'll bring it up now.

You mentioned that you don't see why, Karin, you can't go back to the way grandma made bread. You're so young. It probably wasn't your grandma, except if it was, it must be faily exceptional.

MS. SHELDON: It was my mother, actually. You can come to my house and I'll give you my bread.

DR. EISENBUD: My wife bakes bread, too, but it's not a general practice and it hasn't been since about the

turn of the century when the population was around eighty million and farming practices were far more efficient than they were today, because they operated on solar power and the only fertilizers were manures which were basically recycled solar energy and muscle power that was used on the farm.

If we have to go back to that system today, it would not only be people on the horn of Africa that is starving, we would be starving in this country. Because in order to feed 225 million people, we have to pump energy into the soil and it becomes inefficient. We get less calories out per unit of energy put in by far. By a factor of twenty or thirty. These are the subtilties of the society in which we live.

I think that these realities may be in some cases not easily understood. If I can just close with some simple example. Because I live some distance from New York City, my neighbors can't understand why we should have power plants next to us since the electricity, most of it, goes down to New York City, particularly since they are nuclear power plants. And although these are educated people, they don't understand that they depend on New York City for the tools that they buy, for the television broadcasts and the newspapers they read. In other words, the electricity goes down there, but the products come back to suburban New York and benefit the people.

People just aren't that philosophical or don't appreciate these inter relationships. That's the complexities of these questions that we are discussing.

MS. SHELDON: I think they do. I also think as a general proposition one should question realities, because they're not always just because they are the best thing. I don't think we have to accept everything that is around you. I would be a lot better off if I didn't have to battle my daughter everytime we went to the grocery store over Captain Crunch and all the rest of the garbage that is in there.

And I also think that there are a lot of motivations going on. That we have chemicals in food and additives and whatever for a whole variety of reasons, not the least of which is the profit motive. General Mills, et. al. That's a whole debate and we don't need to get into it.

CHAIRMAN LAVE: Right. Ed?

DR. ZEBROSKI: I guess I can't resist -- it's not what I intended to say. It's a comment on Merrill's thing. I think that what troubles me about some of the back to nature movement is that they are elitist in the sense that they imply retroactive birth control which can come about --

MS. SHELDON: What does that mean?

DR. ZEBROSKI: -- through war or social chaos in the transition from the caring capacity of the country doesn't happen to be enough when somebody starves then the

rise in human misery will be pretty dramatic. That's just a kind of a throw away.

I was trying to say more to the other point of Karin's which I can agree with. Which it seems to me that any goal statement of itself is meaningless unless you say a great deal more about the application environment of which point that I think Chris Whipple has also made a very good look at. It's the question of legitimization of not only the goal but the process of execution. I think legitimization is very difficult issue. At one time, if you had something that was highly contentious in the technical field and you had some prestigious body, a university or a national academy, review it, that tended to get, at least a reasonable sector of public acceptance that that was an honest view of the subject.

I think one of the problems now is that virtually nothing is considered legitimate. That is is not subject to challenge as being biased or shall we say bought against the public interest. I believe given all of that environment, nevertheless legitimization by review, by knowledgable bodies of people who are not beholden to the issue in any obvious way or unobvious way and basically say that the process is a reasonable one, is a necessary part of the legitimization process to make it work. Otherwise, you're wasting your time in getting a technique which is basically incomprehensible to

the public.

So, you need several stages of levels. I think the qualitative statement is tricky in that respect, because legitimization of a qualitative statement is very difficult. All you can do is to say that that qualitative statement bears some reasonable relationship to the detailed discipline which is being applied on the safety goal. But, even then you get into debates like the executive summary of WASH 1400 which was critized because it attempted to do this and was then emplied that that was part of a decision process which probably, it really wasn't. It was an attempt to legitimize past decisions as being within generally an envelope.

So, I think -- I'm troubled in a way that this conference didn't start out with a separate panel on this question of how you would use a safety goal. How you would apply it in regulation. How you relate it to the qualitative goals and perticularly what the legitimization process would be.

I think that -- Let's see, George isn't here. I think the intention is to hold a series of panels of this kind over the year, both this year and the future years and I'm sure ACRS is going to do similar things. That seems to me is a little bit a part of the legitimization thing, but I'm not sure that that works anymore.

Why isn't the fact that ACRS review all NRC decisions

regarded as a significant improvement in legitimacy of the NRC actions? It really isn't.

MR. HUTT: Ed, I found Martin's explanation of that perfectly rational. But you can't decide what you're going to do with something until you've got something there to look at and see how good it is. Your suggesting that you put the cart before the horse. That you decide how this is going to be used before you even know whether it's a valid concept, valid enough to use it.

From a regulatory standpoint and I tend to look at this is a regulator, though I no longer am, I thought his explanation of that was perfectly reasonable. Don't ask us at NRC to decide how we're going to use something, when we don't even know what it is yet?

How does he know whether it is going to be a goal or a regulation, whether it is going to be used in one way or another until we know what it is? How can we even recommend which way it's going to be used until we know how much quantification, how much reliance we around this table would be willing to put in it?

DR. ZEBROSKI: I guess what I was -- Maybe it's a utopian dream. I was hopping that people like the legal counsel counsel and Bob Baernarro who thought a great deal about these things, could say -- here's a potential structure for transition from the present unstructured safety goal.

We have safety goals. There's a massive criteria and regulations which embody -- you can't equate to a set of safety goals -- And here's a potential future, somewhat more precise and hopefully in some respects more workable technique which can evolve out of this process over a period of several years.

I think that transition process, if it is described, would help relieve the tremendous anxiety that any new goal that you get falls on the sword that it immediately provokes an endless legal debate on -- Now, that you have a criteria, all past decisions must be suspect and you must redo everything which would shut everything down again.

So, if I were to say, what is the objective of a safety goal from a standpoint of an ideal log intervenor, it would be to make more opportunities for denial by delay. That is just such an obvious pitfall. Absent some safeguard against that, I think you'll get the reaction that Sol Burstein expressed this morning that there would be intense resentment to that process.

I think that needs to be somewhat neutralized.

CHAIRMAN LAVE: Can I say that on our agenda implementation is one of the next things to get to. Let me summarize in my poor way what I have heard in the last hour and a half and see whether everybody salutes the flag.

That is -- I think that what I heard is that in the

Congressional statues, we think they're can't be other than a qualitative goal. For example, no undue risk. That at the Agency level, there are probably three kinds of goals or procedures that one wants to talk about. That it is necessary -- desirable for a number of reasons to have quantitative safety goals that talk about what the current belief is of how you translate no undue risk.

Secondly, there ought to be some process goals. That's another item on the agenda that have to do with how you establish standards, get public comment and so on.

And that finally, there ought to be some qualitative goals that are stated in here such as that each company or vendor or whatever it is, should be always looking at cost effectiveness ALARA as a further criteria in addition to the quantitative safety goals.

That's what I heard.

MR. DERBY: Can I speak to that. I've been writing down a summary myself and it's almost like that. Let me give you -- I heard three things. One of them is that a satisfactory qualitative goal has to say something about what is a relevant comparison to other risks.

MR. TEMME: The risk risk --

MR. [illegible]: The risk risk thing. It's qualitative and how does it -- I heard a lot about the fact that -- the preamble, at least, has to say something about where it fits

in and what is a relevant comparison.

I didn't hear anything about a quantitative goal. I think we ought to be --

DR. BRADBURN: You mean a particular --

MR. DERBY: I didn't hear anything about a quantitative goal. I just heard about -- I heard people talk about quantitative standards. I did not really hear anybody go after and say anything about quantitative goals.

MR. HUTT: I don't understand the difference between them.

MR. DERBY: The difference is that a quantitative goal is something that you do not have to meet as a standard.

MR. HUTT: I think that's highly unrealistic. I don't even know what that means.

MR. TEMME: That's totally realistic.

MR. DERBY: That's totally realistic.

MR. HUTT: You mean if you never have to meet anything --

MR. TEMME: You've been saying the same thing to us yourself for a day and a half.

MR. DERBY: Exactly.

MR. HUTT: Goals that are irrelevant to whether you meet them or not?

MR. TEMME: Oh, no.

MR. DERBY: Standards is what you mean.

MR. TEMME: Irrelevant is not the word.

MR. DERBY: Excuse me. Standards is what you mean.

There are two other things that weren't on the list. Let me finish those and we'll talk about it.

MR. HUTT: I don't understand what a goal is still.

MR. DERBY: Well, maybe we ought to have that discussion.

DR. ZEBROSKI: It's something you never achieve.

MR. DERBY: That's probably true.

There seems to be in this satisfactory qualitative goal, discussion about the management principle for learning about unknown factors and some management principle that gives directions for setting quantitative rules that implement the goal. Here's the analysis, this is the limit. You're over, you're okay. If you're under, however it looks. The accept reject standard that implements this qualitative goal.

That's in addition to what you said and I'd like to draw a strong strong criticism of these goal and standards. It's all put together with objectives and procedural rules which are accept reject limitations. I think a lot of the discussion looses that distinction and gets confused.

CHAIRMAN LAVE: I guess that I don't understand what subtlety we're trying to put out by the difference a standard and a goal.

MR. HUTT: I don't either.

MR. DERBY: I see it as a strong difference, because I as an engineer, met standards. And there was no question in anybody's mind that if I did an analysis that whatever I was suppose to calculate, I calculated to meet a particular standard and if I didn't meet it, I went back and I did what ever was necessary to meet.

The purpose of having that standard there was the goal of not having the pressure vessel rupture or something like that. If there was a particular event that was going to be avoided. Now to avoid that event, you would give me directions as an engineer. The thing had to be translated in to a specific limits that I had to meet in my calculations, so that by meeting that, there was a general agreement within the technical community that I had met the goal of having a newly conditioned -- that we were operating in. I had met the goal.

Now, that is the difference that I'm talking about.

MR. HUTT: That sounds wrong. You met the standard, not the goal. You never meet the goal.

DR. EISENBUD: There is a difference. The goal is the end that you want to achieve. The standard is the means to that end.

MS. SHELDON: Right.

MR. DERBY: I think that that was what I described.

MR. HUTT: Okay, but then you never meet the goal.

At least, you never know whether you have met the goal.

MR. TEMME: That's the point. There are situations in which you can know that you didn't meet the goal.

MR. HUTT: Precisely, but you can never prove that you have met the goal.

MR. TEMME: In this situation, it is not a very good goal.

MR. HUTT: In that event, why do you have a goal?

MR. DERBY: At least to give focus to standards.

MR. HUTT: But a goal is absolutely useless.

DR. BRADBURN: What I heard him saying is that esssentially the goals are qualitative statements and the standards are the quantitative. No undue risk is your goal.

MS. SHELDON: As low as reasonably achievable.

MR. HUTT: That isn't a goal. That is in your language -- that's a standard.

MR. DERBY: Absolutely not. I have no idea what that means.

MR. CERBONE: Standards have specific numbers assigned to it.

MS. SHELDON: Right.

DR. EISENBUD: The goal in Palo Alto is to keep buildings like this one from collapsing. To achieve that goal they have standards. Presumably the standards have been met in construction of this building.

That raises an interesting point.

MR. HUTT: We'll never know whether that goal has been achieved.

DR. EISENBUD: Is this building safe? I would say it's safe and I think that if the public asked whoever is in charge whether this building was safe, that person would say, yes, if he has records to show it was built.

The trouble is in the nuclear industry, we never say that it is safe. We say well, it has a finite probability of falling down. The probability maybe, I don't know, ten to the minus three per year modified by the age of the building. Of course, if you have a wind at a certain velocity, that probability might go down to even maybe ten to the minus two.

And we confuse the public. The public wants to know if the reactor is safe if the authorities think that it is safe.

MR. LIBARKIN: You keep saying that you never know whether you meet the goals. That's only true if you've stated them negatively. If you state them postively after a time, you know that you've met them.

The goals for the operation of a power plant is a capacity factor of seventy percent and you establish standards for equipment that you meet in order to meet that goal. At the end of the year, you can look back and see whether you've met the goal.

MR. HUTT: You can say that you've met the standards. You can't say that you've made the goal, because the goal is not to have that operate for one year and not explode.

MR. LIBARKIN: I wasn't talking about something so drastic.

MR. HUTT: Your goal is to have it operate into infinity and not have it explode or a lifetime.

MR. DERBY: You've met the goal after they've commissioned the plant and the thing hadn't exploded. I guess we've met the goal.

CHAIRMAN LAVE: Okay, can I go back? It's clear that they are both quantitative safety standards and quantitative safety goals.

MR. DERBY: Okay.

CHAIRMAN LAVE: That is a quantitative safety goal might be, for example, less than ten deaths in a thousand megawatt reactor per year. Calculated someway. That's a quantitative safety standard.

I think the goal we're talking about -- For goals you're talking about something that is not an operational statement. The standard is an operational statement. The goal is someplace that you're trying to get to. You need to set up operational standards to get to -- operational statements to get to a goal. Part of this is what I had in the outline, if I can go back to that for a second.

It talks about making -- getting some consistency between different levels. Between the on high statement and some stuff about what a reactor operator does before he comes on to the job.

MR. DERBY: This is a management principle of what direction the goal gives to quantitative statements.

CHAIRMAN LAVE: That is, we're trying to get a series of things a quite different levels operating statements about whether you let a reactor operator drink a fifth of bourbon before he comes on the job. You have some things there and then you've got a set of procedures about how we calculate pressures in vessels and so on. These are very different and very hard to say that they're commensurate somehow. Yet, until they are made commensurate, you don't have any kind of consistency that you can draw out of it.

That's a difficult set of principles. I guess I see it edging over into that right now and I don't want to right at the moment.

MR. DERBY: I don't want to go in right now, but what I'm saying is that one attribute of a qualitative goal would be some explanation of that to give some direction to that. It could be adjectives or the nouns or the verbs have operational meaning to people that they can take those on reading and are not mystified as to what they mean and can begin drawing together this hierarchy that you've describe --

This operational system.

It seems to me to be a nice thing to have.

MR. HUTT: Can you give me an example?

MR. DERBY: My problem is that I can only give you negative examples. As low as reasonably achievable. Who the heil knows what is reasonable and what's achievable.

MR. HUTT: That's exactly it. That's why as a regulator, I look at something like that and I rip up the paper and toss that out and say that's useless. It doesn't tell me what to do on this problem today.

MR. DERBY: That's right and you should applaud my annunciation of such a characteristic and qualitative goal, because I think that the qualitative goals should have the ability that you as a regulator should know what to do when you see it.

MR. BRIDENBAUGH: It seems to me that we're -- you have to recognize that -- which was stated in the beginning of the meeting - - there are purposes of the goal for a regulator and for the designer and for the public. The regulator would like to have a nice clean goal no goal thing to tell him it's okay or not okay.

The designer can use a goal of ALARA to design his equipment to make it better than it has been in the past.

MR. HUTT: It doesn't tell the designer, Dale, because the designer among other things when he designs it isn't

going to know whether the regulator is going to agree with him on whether it meets ALARA. I don't think it's very helpful. I'm not a designer, but I would find that very troubling if I was designer.

Putting it over into my area, if I go tell the drug industry to design their drugs to kill as few people as they can, that's not going to help them very much.

MR. BRIDENBAUGH: You do have to have a cost effectiveness thing built into it certainly.

DR. BRADBURN: Let me ask somebody. If a general goal were phrased -- You say essentially as low as reasonably achievable, doesn't tell you very much. Supposing somebody said, well, the goal should be no lower than is reasonably necessary. Would that have any flavor or connotation or anything to it?

MR. DERBY: No.

MR. HUTT: It would just require me to sit there and look at that sentence about ten more times and conclude that it wasn't helpful.

CHAIRMAN LAVE: In the interest of keeping us on task, I now have taken my slide and have added two things to the bottom. And would propose that with all of these easy things out of the way, we get back to epistomology.

I keep on insisting that we've got to look at for awhile and I don't think we've done very well. If we can

keep the task for fifteen minutes, I think we can satisfy that and then this afternoon we get to go on to the interesting subjects, namely, the implementation and the process stuff.

DR. ZEBROSKI: Let me try once more. This is epistomology. It still seems to me that the qualitative statement is the necessary bridge to comprehension of people who will not read the tables of numbers.

You can either say that the quantitative goals, standards, procedures and that whole discipline derive from the qualitative goal, but equally you can say that given that I have a set of established procedures and a mechanism for improving them with time, I believe that they will result in given level of safety.

I can say that that body of detailed procedure then is likely to result in meeting this generally stated goal in qualitative terms. I think that pragmatically, that's the only way that it can work. You can't really go the other way. You have to work at the body of process and say what you think it will produce. Because you're not talking about events for which you have statistical experience. Largely, we're talking about controlling events which are hypothetical or for which the experience is one in 500 or a thousand years -- operating years.

So, you can't go on experience on this, you can only say as you do in building bridges, if I can make it better

than some previous bridge and previous bridges haven't fallen down for 500 years I can reasonably expect that as a responsible regulator, I'm going to achieve that goal.

MR. HUTT: I have no quarrel with what you said, but the standard as low as reasonably achievable is uniquely unhelpful in reaching that decision.

DR. ZEBROSKI: I agree with you.

MR. HUTT: It does not -- I guess in my language, it does not produce reproducable results. If you gave the same set of facts and this is the criterian that I always used as a regulator -- you've got to have decision rules that can be taken by different people and applied independently to the same facts and reach the same results.

MR. BRIDENBAUGH: That in itself is a goal.

DR. ZEBROSKI: ALARA has really been the -- I think has just this relationship that I'm talking about. The procedure or the standard has been to make sure that you've expended at least a thousand dollars per man REM avoided. If you do that, you've got ALARA. I think that's been the operational definition of --

MR. HUTT: I have no problem with that kind of an operational definition. That's superb. That gives you reproducable results.

CHAIRMAN LAVE: Can we --

DR. BRADBURN: Can I just say the difference between

that specific dollars per something or other and the general one, I think gets back to this question that I mentioned earlier about whether it is something that is fixed or dynamic. Because, presumably, particularly if you're taking a dollar and given inflation and so forth, you're not going to want that dollar figure to stay fixed.

What the operational definition is, you're going to want to change.

MR. HUTT: That's a moving target over time.

DR. BRADBURN: If you right something in a way that is something in a fixed quantitative standard in a way. To change it is going to have to be -- You can change it in light of some either over arching concept or something of this sort.

It seems to me ultimately in a peculiar way does get back to some kind of general qualitative standard. When we use standards of reasonableness. All of these sort of things that the law and the hose is filled with.

At one level, it doesn't help you in the day to day decisions, except that as you are somehow or other reiterating those and feeding back and developing new notions of what's possible or reasonable or whatever. It seems to me that some kind of over arching standard like that is helpful.

It does seem to me that things which are phrased as low as, are different from something that is phrased as no

higher than or only as low as kinds of things, because it does put a different sort of thrust on what kind of thing you're really looking at. Are you trying to minimize something or is it really just get down to some -- as long as it is under some general thing. Concretely it may not help you, but it seems to me the thrust of the writing things in that way would be very different.

If you wrote the general sort of thing and said as long as they didn't need to kill any more of the people than necessary, in some sense. I mean it's like being around doctors. Do they kill more people than they cure.

What's the ratio? Can you reduce the ratio. It seems to me that kind of -- standard just as a concept does have some meaning, but what exactly it is and certainly how it is helpful as an individual standard.

MR. HUTT: Well, all I can say is that any regulator will pass by the qualitative standard on the way to the operational definition in a split second and will focus all the time on the operational definition. Because even if there is no explicit operational definition of the kind that Ed ennunciated, a monetary or any other one,the review work -- the individual regulator down in the bowels of the beaurocracy has got to make the decision, will make up his own. You can't make decisions on generalized qualitative goals. It doesn't permit you to make a decision.

So, even in the absence of an agency-wide or even division-wide operational definition, the individuals will make up their own and quite frequently I discovered in my four years, they were all different within the same division. But, everyone had to have one or they couldn't make any decision at all.

CHAIRMAN LAVE: I guess I think we're agreed on this stuff, that you need to have some qualitative goals. That it is very helpful in achieving some consistency to translate those quantitative goals or standards or whatever at any point in time and that you're going to change that translation from time to time. As scientific knowledge changes, as inflation goes on, as the level of income changes, as -- If you find out whether you did or you did not have a world war or other -- Just a number of circumstances that are going to change.

Your qualitative goal -- A well stated qualitative goal could stay fixed throughout all that, even though your quantitative goals varied a great deal.

MR. HUTT: The FDA qualitative goal for food safety hasn't changed since 1875.

MR. DERBY: Is it well stated?

MR. HUTT: Very well stated. You don't injure human health. I couldn't state it any better if I had to.

DR. EISENBUD: We're taking an all ready existing

industry --

MR. TEMPLE: Has it been met?

MR. HUTT: Of course.

DR. EISENBUD: We're dealing with an already existing industry. It's about 37 years old, something like that. Title XX has existed now for about 20 years or so. We are attempting to develop a system for regulating or sharing the safety of an industry who has existed for a long time. What if, by evolution, we have evolved a system that de facto has produced a level of operation which is safe guarding the public. Wouldn't that make it easy to be able to say that the system, however it grew up like Topsy -- it's a little better here, it's a little better here -- is working.

MR. HUTT: Merrill, let's assume that it is true. I'm willing to assume since I come with no knowledge whatever in this field that you are absolutely 100 percent accurate in that. Then my question and I thought I heard Lester ask the same question yesterday, is why don't you write down in understandable fourth grade English form exactly what the operational principles have been that have evolved that have led to that situation?

They are only partially, as I understand it, written down in the Parts 20, 50 and 100. I think I have the parts down right of the Code of Federal Regulations. Why don't you write down all the ones that are implicit that have led to this

situation and then we'll just agree that we'll adopt them and we'll put them down.

DR. EISENBUD: No. About all you can do -- we can't possible certify that the system is working. It may be over safe. I think it is in many areas. I know that Ed would agree that they are.

MR. HUTT: Why can't we codify what these implicit things were?

DR. EISENBUD: It probably ought to be the subject of the summer conference that I see. The next workshop. I wish it was the subject of this workshop.

MR. HUTT: Your hypothesis is correct. We shouldn't change things for the sake of changing things.

DR. EISENBUD: I didn't raise a hypothesis. I said let's look at the present system. And then I said, what if it turns out that we think that it is safe enough?

MR. DERBY: How are we going to evaluate the present system?

MR. HUTT: How are we going to know?

CHAIRMAN LAVE: Excuse me. I have this friend who is a researcher in learning development and she says that the whole problem that you have whenever you teach is the length of time that you can keep people on task. That is, a natural thing that happens with a two year old -- his span of attention is limited and they go wandering off.

DR. EISENBUD: Are you comparing me to a two year old?

CHAIRMAN LAVE: No. I'm saying that committees are like two year olds. YOu start down a path and there's this beautiful red herring that appears and everybody goes --

Ladies and gentlemen, back to task. Honest to god, let's do epistomology and let's get rid of it forever.

Question? What kinds of things are knowable and what do you do about that? What are the falicitous approaches for dealing with the terribly difficult subjects of reactor accidents? How can you deal with that?

One method of dealing with that is precisely the reactor safety study where you do some complex analysis. You make assumptions as are needed and you wind up with an effort that many people inside the industry and outside the industry find not credible. Is there a way that you can do it better?

If the answer is, there isn't, then we're in real trouble.

MR. HUTT: Lester, I heard Merrill say that there was a better way to do it and maybe that's -- I didn't think that it was a red herring, but let me explain why.

If you take the hypothesis that the plants that are built today, the decisions have been made over the years by whatever the agencies that have been involved were correct and produced safe plants, then what you do, is that you take

an emperical approach. You go look at the decisions that were made. You write down the decision rules that you extract from those decisions and you therefore define safety in terms of those rules.

Am I mischaracterizing, Merrill, what --

CHAIRMAN LAVE: I understand that. The question is, is there --

MR. HUTT: That's a different way of doing it then sort of starting from scratch and doing it by pure analysis.

CHAIRMAN LAVE: Is there general agreement? Do we think that there is general agreement that people inside the industry or outside the industry feel confident that the current set of plants are adequately safe?

MR. TEMME: No, there's not such agreement and that isn't the only question to ask, because the safety of the plants is one part of it. Does the process work, is another part of it. Does the process --

DR. EISENBUD: You mean the regulatory process?

MR. TEMME: The 10CFR20, 10CFR50 and 10CFR100 and the red guides and the branch technical positions and the rest of it, work? The answer is, no. A lot of people are dissatisfied with the process. People inside the industry and outside of it and in the regulatory agencies are dissatisfied with it.

CHAIRMAN LAVE: So, that writing down what has

occurred in the past isn't going to help?

DR. EISENBUD: Yes, it would help.

MR. TEMME: It would help, but the hypothesis that it is working well is in my view wrong.

MR. HUTT: I wasn't dealing, Mark, with the question of process, just with the end result. Are the plants safe?

MR. BRIDENBAUGH: I think that you could probably get agreement from most people that the number of people that have been killed by commercial nuclear power plants to date is not excessive. That's a very bland way of stating it, but I don't know of anybody who would dispute that.

MR. HUTT: That's been Merrill's argument for the last day and a half.

MR. BRIDENBAUGH: That's the bottom line, end result.

MR. TEMME: If that's your measure of safety.

DR. EISENBUD: I think the process can be improved. I think the only way that you can improve the process, clarify the goals, is to analysis it. Is to look at what you've done. Look at the experience we've had over these twenty years regulating the construction of these power plants.

MR. HUTT: Is it at least feasible to extract from the emperical evidence those decision rules and say they -- so far that maybe on a cost effective bases we can improve them here and there. At least we have already established

a level of safety that has not proved either to be unworkable in the sense of being feasible from a technical standpoint or a monetary standpoint or that it has been proved to be unsatisfactory, generally to the public, because it has not produced an excessive amount of injury.

MR. CERBONE: What we heard from panel A this morning was that there was no intent to have these goals replaced under present regulatory rules. Isn't that right? I heard Kouts said that there should be quantitative safety goals. They may be difficult to implement and they may take forever to implement. There's no sense that -- That's saying that they're not going to stop the licensing process until they get these goals.

MR. MALSCH: That's just the view of Panel A. It remains to be seen what will become of the whole exercise. Panel A's view may or may not prevail over the long run.

MR. CERBONE: So you see supplementing the present regulation?

MR. HUTT: I think what Martin said earlier, if I can paraphrase and what I said in his absence, was how can a regulator know what they're going to do with safety goals until the safety goals are there so that you can determine how reliable and useful they are? The first thing to do is to see what can be quantified and what can't be quantified. But certainly if we and the other panels came up with the conclu-

sion that nothing could be quantified, of course, you wouldn't have goals. I really think that is putting the cart before the horse.

MR. MALSCH: I also want to make a comment. I don't think that if you looked at agency practice in the accident area and tried to see whether there were -- I think if you looked at agency practice in the accident area and tried to see whether there is any largly consistent set of operational principles, you could find any.

MR. HUTT: Merrill, do you disagree?

MR. LIBARKIN: There is a set of principles written down.

DR. EISENBUD: I really don't know what you mean.

MR. MALSCH: Well, the question would be, what you have in the regulations, principally in Part 30,is a set of broadly stated engineering design principles which as I understand it as a practical matter of are of no value whatsoever in actually reviewing individual license applications.

In reviewing individual license applications, instead what is used is what is called the Standard Review Plan. This is essentially an elaborate cookbook which leads reviewers step by step through the process of comparing information on applications against certain rather specific principles.

I don't think that there is any general philosophy that you could glean from that detailed set of principles from

the Standard Review Plan. I think it was developed over a series of years through a process of negotiation and application -- best judgment and I think the process defies rationale in the sense of any generally stated underlying principles.

I think part of the purpose of this exercise is to maybe impose some rationality into the process.

MR. HUTT: Merrill, do you want to disagree with that?

DR. EISENBUD: I think for a long time the industry flew by the seat of its pants. After all the first reactors -- they were big reactors that they built at Hanford in 1943 or '42. It went critical at 275 megawatts and they were built in 18 months and there were no rules. They operated safely for many years. From that point on, there were I guess between what we've done and what the Europeans have done and the Russians as I said the other day, there are surely a thousand reactors whose experience is relevant to the question we discuss. They may not all be commercial power reactors, but the experience you get is relevant.

Here we are today. It's 1981 and we've got a code of federal regulations which provides the basic ground rules on which the various procedures are based and I think that by and large it worked. I think that the record has been a good one and it can be improved and I think it should be improved.

What I would disagree with you is, if you take, what did you call it? The standard plant or reference plant and I don't know. There might be a better way of doing it, but let's look at the procedure. Let's recommend that somebody look at the procedure to see if it can't be streamlined.

I think a problem is that the regulatory apparatus now for some years has been trying to satisfy all people and the easiest way to satisfy intervenors or industry perhaps in some cases is to go along with it,particularly if the action makes the plant safer.

There's a considerable amount of drapery on these plants which is costly and which may indirectly someday lead to accidents as a matter of fact. The plants are probably more complex than they need to be and the procedures are probably more complex then they need to be. But, I'm not prejudging that. I'm saying probably, probably that I believe that to be the case. I think that it ought to be looked at. But whether they should be looked at and looked at because it would make sense to simplify them simply for the sake of doing things by the most expeditious manner or you should make changes because it would assure greater safety, more safety than the system is now providing is the basic question. I don't think we address that at all.

MR. DERBY: What is epistomology?

MR. HUTT: And why do we have to look at it?

Since we have refused to do so, even under your whip.

CHAIRMAN LAVE: Let me try and pose the question.

MS. SHELDON: Who do you think you are anyway?

CHAIRMAN LAVE: Let me try and pose the question. Is there some way that one can inquire about safety levels. Sorry. About accident levels in a fashion would be deemed first of all by the experts -- by the insiders -- as being consistent reproducable and exceptable and then secondly, can be made to translate in some fairly consistent way with a set of safety goals -- quantitative safety goals. That is, I'm getting back to, again, what attempt to do this with WASH 1400? Are there improvements that can be made in that procedure or are there alternative procedures and how well can one do in answering these questions?

What is the level of certainity? For example, the level of uncertainty is going to be several times -- is the standard deviation going to be several times or ten times or a hundred times the vast numerical answer that you arrive at?

MR. DERBY: Let me share my experience and answer at least part of that question. The more narrowly you define your model. Meaning, you generally have a set of assumptions that you work from. You assign probabilities based on those assumptions which include a set of information and you calculate something.

The more assumptions that you have, then this un-

certainty that you address in the final answer, or whether or not that answer means anything, generally moves to an examination of those assumptions.

The fewer assumptions that you have -- Excuse me, the final point is that the more assumptions that you make, then there is generally a lot of statistical evidence, data, scientific kinds of stuff that can go into the model that makes the model agreeable to a lot of people. It's just that those people say, well, that's your assumption, then, yes, your calculation and everything follows from your assumption and we have no problem with. Boy, I don't like your assumptions and if I change them, I change the answer a lot.

If you want to get those kind of considerations into your model and to quantify those considerations and assumptions that you've made, then you get into areas that people cannot resolve disagreements in any scientific fashion.

So, to go towards an answer of your question, what is quantifiable, if you want to say quantifiable as a -- As a decision analyst, I can quantify anything. I can put a number on anything. The trouble is, that number will change for everybody in the room. If I want concensus on a particular number, then I have to drive very quickly towards established scientific principles of which there is concensus on those principles and then any number I calculate or quantify would be satisfactory to all the --

CHAIRMAN LAVE: Let me take a specific example. WASH 1400 assumes independence of failure throughout. That's an assumption and you can agree with it or disagree with whatever it is. But it seems to me that's an assumption which is amenable to investigation. That is, we don't have to have 50 million Frenchmen voting yea or ney about it. You can look at it.

MR. HUTT: How do you look at that, Lester?

CHAIRMAN LAVE: I'm sorry?

MR. HUTT: How do you look at that?

CHAIRMAN LAVE: You look at the kinds of events which would cause failure of one component and ask whether those kinds of events are or the various events which take a look at the set of events that would cause failure of one component and ask whether those events would cause -- would be likely to cause failure of more than a single component.

MR. DERBY: A scientific solution to that problem would be the gathering data of equivalent systems over a long period of time so that your sample statistics gave you some degree of confidence in your results.

My experience has been that number one, there is no set of equivalent systems that one can draw data from and that number two, even if there are, you don't have enough time and effort to get the data because the numbers that are generally being bandied about are small and take an extremely

long time to get sample statistics on.

To do the calculations --

MR. ZEBROSKI: I really have to descent with that. There's a very major effort to do just that and what you can do at any instant in time is to put a bounding value on the numbers and you reduce the uncertainty as further experience accumulates. We now have 70 reactors that are tied together by a telegraphic network which exchanges data every day on every failure that occurs in every plant. We've had 56 plants overseas that have joined that network.

So,you're unaware of what is going on when you make that statement.

MR. DERBY: Ed, listen, instead of saying that I'm unaware of that particular issue, let's just go back to the modeling part. I'm not talking about things that are amenable to that kind of data collection. I'm talking about things that are not amenable and there are major portions of this network that you have that are not amenable to data collection and I draw that experience from the British --

DR. ZEBROSKI: Again, that's just not true. When you talk about fixed forma data collection, you're dead right, but if you're saying that any observation or occurence that is troublesome whether it is safety grade or not is now being looked at.

MR. DERBY: Everything that contributes to a

sequence or dominant sequence in that reactor can be examined from a common cause, common mode, independence data collection thing in its pure form. Anything that I want in exhaustive study of all parameters in the plant is now having a data collection -- That's my point. I find it incredibly hard to believe that that kind of data collection would consume more people, energy and effort than I would imagine would be available and I don't know. I'm uninformed -- You're absolutely right. I am uninformed about that particular data collection, but I'm not uninformed about the effort that has to go through to get completeness on all issues on sequences of --

DR. ZEBROSKI: That's academic nonesense because all you have to do again is say compared with what? You say compared, say, with the learning process in building bridges were infinitely better. If you say compared with the learning process of space shuttles, we're probably not as good. If you say compared with the learning process on aircraft, we're getting there very closely. So, I think that perfection is -- you know, if you're talking about perfection, I'll agree with you that it is not perfect, but if you say what it is relative, say, to even when it was two years ago, or what it is relative to many other industries which lead to acceptable risk, it s a tremendous change in the situation.

I think you have to be aware of it before you say

it can't be done.

MR. DERBY: I'm not saying that it can't be done. I'm saying there is a difference in what one will agree to. The point that I am making is what can be agreed to is what is quantifiable is got to generally be tempered by a very good exercise of the scientific method and peer review. That is all that I am saying.

If it turns out as what you say that all -- and I mean all impacts, all assumptions that one has to make in a probablistic risk assessment can be evaluated by the scientific process, that the cause and effect relationships are well known, that the data that explains those cause and effect relationships are well-known, then fine. That's wonderful.

DR. ZEBROSKI: As soon as you say all, then everybody falls off the wagon. That's a little bit too comprehensive.

MR. DERBY: All important, all dominant sequences.

DR. ZEBROSKI: I'll only observe that nobody has discovered a new dominant sequence since 1975.

MR. DERBY: Take those dominant sequences and we'll talk about those. Okay?

DR. ZEBROSKI: Nobody has discovered a new one.

MR. DERBY: Instead of talking about discovering a new one, let's talk about the ingredients --

DR. ZEBROSKI: And we have some pretty good theory

on what the probability of postulated hidden sequences is and what their probability of existence is with time, given that you have this learning and observation process.

MR. DERBY: This is a theory and this is a theory reaching to the point of -- That's what I'm talking about. Theories are assumptions and if this theory has emerged in the last 18 months, I'd have to say that it probably has not gone through the scientific process and the review that, say, the thermodynamics have gone through or some other equivalent engineering notion.

That's all I'm saying that in order to get agreement, you don't have to have a theory.

DR. ZEBROSKI: It has gone through that process in the sense that this is precisely the process that NASA and DOT used in getting systems with extremely high reliability. At one time we used to discount the NASA process in saying, well, they only had to make things work for an hour or two and therefore you could test for a thousand hours and be pretty satisfied that it would work for an hour or two. But, in fact, that process intelligently applied has made space probes that last ten times the test period reliably in the deep space probes which have been going on for many years.

So, there is a possibility that when you use this process intelligently that you get an extrapolation of expected performance far beyond your immediate experience. That's the

discipline that I think is now being applied.

MR. DERBY: Okay, I will agree that the scientific review of the application of that discipline, the nuclear reactor business, will releave a lot of the subjectivity in many parts of the probalistic risk assessments. I do not dispute that.

What I am saying is that line between what assumptions that have to be made that are not sustained by the scientific process are generally felt to be subjective. Now, one can put numbers on that, but you do not get agreement. You're saying that the collection of data resolves those diagreements, fine. Until those disagreements are resolved --

DR. ZEBROSKI: No, it doesnt' resolve them, but it puts bounding limits on how big the disagreement can be.

MR. DERBY: I would call that towards resolving disagreements, but I think my point is still valid. It is that dividing line that makes things quantifiable for regulatory use and quantifiable in the sense that one can produce a number.

MR. LIBARKIN: When you say that there has been no new dominant sequence identified in some given period of time, you're talking, aren't you about that subset of all the bad actors that for example that does not include things like sabatoge initiated events? It's a particular set up of --

MR. ZEBROSKI: It's true in the U.S. It is not

true overseas. They are very much analyzed sabatoge initiated events and surprisingly enough, they don't invent very many new dominant sequences. The extreme one is the Israeli study of events where you have simultaneous penetration of containment and reactor vessel by high-velocity missles. For obvious reasons, they look at that.

Even that one, surprisingly enough starts to look like a big break look very quickly.

MR. LIBARKIN: You are including those sorts of things in that --

DR. ZEBROSKI: No, I'm just saying that they're not being ignored. I haven't personally been involved in that.

MR. LIBARKIN: I wasn't aware that anything like that was going on.

DR. ZEBROSKI: Bob Bernarro is chairman of the committee. You ought to know about it.

MR. HUTT: Lester, can we also reach an agreement on what time we're going to have lunch?

CHAIRMAN LAVE: Yes, I would guess in one minute and twelve seconds.

Let me sharpen my questions, which is that clearly one can not, if we had an event, a reactor accident, which somebody believed had probability say, ten to the minus, then the number of reactor years of experience that you would have to get if all that you were doing was looking for that

in this crude way in order to say, yes we believe it is no more than a factor of ten different from that, would be enormously large. The epistomological question here is what are the ways by which you can take other observations so that you can narrow down what the possible range is.

Then instead of talking about a billion reactor years of experience or a trillion reactor years of experience, you can talk about, say, a thousand reactor years of experience or a different way of putting it is what are the clever ways in which one can make use of a very finite number of years of experience in order to tighten the bounds on these almost meaninglessly small probabilities like ten to the minus seventh so that they become meaningful --

DR. BRADBURN: Times up.

CHAIRMAN LAVE: I'm sorry?

DR. BRADBURN: Times up.

CHAIRMAN LAVE: Let's go to lunch.

(Whereupon, at 12:20 p.m., the morning session was adjourned to reconvene at 1:30 p.m. this same day.)

CHAIRMAN LAVE: I was trying to answer the question I had posed generally, but really to add, I had thought about what -- how much could one narrow, what techniques could one use to narrow or how much could you narrow the amount of uncertainty about low probability accidents.

MR. TEMME: It's a good question, but I'm compelled to ask, why do we care? What is your reason for asking?

CHAIRMAN LAVE: I think that one of the major problems that people have with nuclear power is the doubt that in fact those reactors are anything like as safe as they are. If the probability of loosing lots of people is much much greater than WASH 1400 states, then you have something quite different.

For example, one of the calculations that I carried out comparing coal with nuclear -- simply compares routine operations. It is easy to then add on to that by looking at expected deaths using WASH 1400 kinds of figures and of course it contributes a neglible amount. But if you multiply those WASH 1400 figures by a factor of 100, then the ball game is a different game.

DR. ZEBROSKI: Let me address that last point, because that's the -- maybe one of the easiest ones. If the WASH 1400 figures are off by a factor of 100, then you would be at about a 95 or 98 percent confidence level of having

seen a much bigger accident than TMI already.

CHAIRMAN LAVE: Okay, that's helpful.

MR. HUTT: Is that indisputable?

DR. ZEBROSKI: That's straight out of the statistics.

MR. HUTT: That's statistical.

MR. DERBY: It's a statistical thing and I dispute the -- I would dispute as a statistical person, that statistical model. Let me explain why. It is not a personal thing. Statistical models assume equivalents of situations. The one reactor year in one place and one kind of reactor is much like another. It seems to me that one can reformulate the problem that most of these reactors are very early in their lives. We've got 30 year lifetimes and most of the reactor experience is in the first ten or five years of operation.

If one draws from the experience of how reliablity works, there is a break in period. There's the running period where things are fairly reasonable and there's the wearing out period. ONe can legitimately ask the question, are the designs for 30 years avoiding the wearout period. There's in the backend of these reactors that one has to look at.

I think that it is a little bit premature to rely on statistical models that assume these kind of constancies.

DR. ZEBROSKI: Let me one up you on that one, then. Statistical model assumes a homogenious population or a

limited population. If you assume an inhomogenious distribution, then you have to assume, is the population worsening or improving. So, implicit in giving any credibility to this confidence level as a measure of adequacy of WASH 1400, is -- you have to assume something about the whole environment and the culture that it is in.

If you had a situation in which degradation of operators, training, maintenance practices, replacement practices and so on was routine, then your comment, I think would have great validity. I think the reality is, if anything, those issues are all tightening up with time. We're going for much greater training and education of operators and more simulator training than they did before. They're being taught to recognize very rare events which they were never trained for in the past. That was a hangup at Three Mile Island. And the actual experience on the reliability is that it increases monetonically with Plant H for the plants that have operated.

The operating factor increases monetonically with Plant H for most plants, not all.

MR. DERBY: That's saying that the plant produces power more than it doesn't. That doesn't say much to me about safety parameters.

DR. ZEBROSKI: You're talking about reliability at the component level.

MR. DERBY: Yes, I am.

DR. ZEBROSKI: If the component is unreliable, the plant doesn't run. So, the reliability is not just a question of the mean time between failures. It's mean time between overhaul and repairs and degree of redundancy.

I'm saying on that kind of a scale, which is exactly in the middle of your discipline, the trend is in a favorable direction.

MR. DERBY: We're at the front end of that and from the events and things that we've done in the nuclear business to identify events and as you're saying activities of spotting good training programs and I would suspect statistics and things to change. I don't know. I'm saying that if you want to rely on a statistical model, what you're saying is that we should feel good that we're going in the right direction.

But to rely without that information --

DR. ZEBROSKI: No, I didn't say that. I basically simply said that -- I'm making the same point that Peter made that if you take this as a proof of something, you can argue endless uncertainties. If you take it as a reasonable measure of the direction of the trend, it's certainly not unfavorable.

MR. DERBY: It is not unfavorable. I agree. I totally agree, but to dismiss things like modeling assump-

tions. To say that it is over 95 percent confidence that we should see -- I'm paraphrasing you -- we should see an accident much worse than TMI, at this time. There's an awful lot of modeling that that --

DR. ZEBROSKI: You're just repeating the cliche that any given model is never complete.

MR. DERBY: It is not a cliche.

DR. ZEBROSKI: It is a cliche, because that criticism has been made endlessly on WASH 1400 and it is just not valid.

MR. DERBY: It is not a cliche, Ed. What I'm saying is that there are specific assumptions that one makes to reach that conclusion in which if you look at them closely may or may not affect the answer. And I am not saying that it's a proof of much.

MR. TEMME: Ithink the question was, is that a debatable assertion or is it irrefutable or something of that sort.

DR. ZEBROSKI: If it's a proof, I agree that it is not a proof. If you agree that it's a reasonable guide for reasonable minds, it surely is.

MR. HUTT: What you're saying is that it is debatable, but it is the best one can do at this time. Over time, we may improve upon it.

DR. ZEBROSKI: It certainly would be imprudent to

ignore an opposite signal. If there was a gradual degradation, you sure as heck would do something about it.

MR. DERBY: That would be far more informative than the fact that nothing is happening when one looks at the model, because I can think of a number of reasons why --

DR. ZEBROSKI: I take some heart out of the fact that a similar discipline is given as ten year or more lifetime of deep space probes, which had the face the period that the test period was much shorter than the operating period and you do your homework of getting enough redundancy and reliability combination to get long lifetime, even with observation and maintenance. The advantage of the nuclear plant relative to the satellite is that you can do observation and maintenance and the observation has grown enormously more sensitive and beyond line in many cases.

MR.DERBY: That's the two-edged sword. If one does this observation and maintenance well, then in fact, you improve things. It's a decision making system. It is not something that you just set off and --

DR. ZEBROSKI: Let me give you my caveat which goes in your direction. The caveat really is that the whole population doesn't have outliers of people who don't practice what is known to be good practice and good engineering and good science. Supposidly INPO is trying to ferret that out and hopefully more constructively than the NRC has been able

to do.

MR. DERBY: That is the point of these standards. Is to not sit there and evaluate whether or not good practice is satisfactory or not. I think it's counting the angels on the head of a pin. What it's for is to identify what performance measures and how we're going to implement those performance measures for people who don't act to the level of competence that one would expect is there. The outliers, if you care to use the statistical term. I judge it in terms of confidence and responsibility.

That's exactly what you want to do and that's exactly what you have to learn. What is competence and what is.

DR. ZEBROSKI: One other just sort of technical answer to your question. The confidence at a given mapping of malfunction is reasonably complete and therefore the remedies and criteria and codes and standards cover the necessary things. Is a function of cumulative operating experience, given that you are recording and intelligently reacting to it.

Mean time between failure of jet engines has gone from a couple of hundred hours to five thousand hours by the operation of that process. Something called a Duane Reliability Curve. You plot the log of time against the log of mean time to failure or major overhaul and you get a generally improving slope with time if you have that process really

working.

Perception that arose out of the TMI accident is that that process sure as heck wasn't working after the early '70s. In other words, there were a number of plants that went from about a dozen to four dozen in a short period of time. The informal communication and operating experience and the consequential analysis just wasn't getting around the loop. The classic is that there are two or three very good precursors to TMI. One of them four years earlier and one of them 18 months earlier and the guys in the plant never got the word on it.

That defi ncy, I think, is being cured to a very large extent now. The word gets around very quickly and the consequential analysis is being done much more intensely than it was even 18 months ago.

MR. DERBY: Which brings --

DR. ZEBROSKI: Let me finish, though. So, the confidence on the completeness of your model of potential failures if you postulate that you're taking the small the signal seriously, you can make some good theory, but that gives you an increasingly high confidence level that long change that lead to serious consequences will be reduced in frequency or probability by -- You can argue whether it is squared power or fourth power, but there is a strong dependence on this process.

MR. DERBY: I agree. The focus that I have in use of those models is exactly the point of whether or not there is that information exchange. Whether or not there is the kind of reaction that goes on and I think it would be the focus of regulatory qualitative goals and things to set something out as a management principle for learning and for finding out whether or not the plants meet this.

One of the things that I truly believe about nuclear power that is -- you do not have a statistical process here. People learn and react and if information is given to people and it's in a form that they can react to, then things will be made better as exactly as you've said, but it's not been my experience that the process is set up to do that. The processes sometimes prevent that.

I don't know why it prevents it and I have no reason -- I can't offer a solution. But it seems to me that the direction that people -- the qualitative goals ought to have in it is exactly one of those attributes. How do you set up a regulatory -- to learn and to react appropriately, not to have people go out there and --

DR. ZEBROSKI: I would be very uncomfortable with a goal that is otherwise reasonable. Which did not have as part of it a statement that this cumulative learning process was an essential part on a highly disciplined bases -- is an essential part of the system. Otherwise the goal alone

would apply only to the design in it's pristine state operated by ideal people and that's not enough.

MR. DERBY: I agree. We are together on that.

CHAIRMAN LAVE: Don't you have the following kind of difficulty, that is that I do believe that the feedback process is going to help a lot, but if you start having rare events. I mean we start looking at rare events. Some of those you have no experience on so far. Therefore, any convergent that is going on without taking account of these rare of these rare events. You don't know anything about it. You haven't gotten the experience with reacting to them. You don't really know what might go on. So, some of these remote events could still have hugh consequences and you just don't have any experience on them.

DR. ZEBROSKI: You're describing accurately why TMI happened. At the utility level there is over dependence on experience and if you're talking about the kind of an event that happens every year or five years or even every ten years, then the analyst operating at a distance has nothing to add to the situation. The learning by experience works and can work very well.

If you're talking about an event that happens once in 50 or 500 years, relying on experience is the most dangerous thing you can do. I'm just reinforcing your statement. That's when you need the good hypothetical analysis of what

kind of bad events can occur. What observables there would be if they occurred and what remedies that the operator has available to terminate them. Or to recover from them.

Now, the hangup in that field is that historically, it's been captured by the what-ifers who answer the question, if a terrible thing happens to the country side, what are the steps that had to have been there? You get a catalogue of the things that were necessary to get a terrible disaster.

There has been relatively little analysis and that leads to a statistical emplausability that you end up having to take very unusual conditions of a whole series of things in order to get that resolved. The discipline of taking a -- And then the operators complain when they're given training on that kind of event. Well the plant never works that way. I never see even the beginning of things that happen this way, therefore -- And the procedure is written on the bases of such analysis is useless.

So, the discipline of doing physically realistic analysis. If the core is melting here's what you'd see. If the vessel is being damaged, here is what you'd see. If the core is on the floor and doing unpleasant things,here is what you'd see and here's what you can do about it. That kind of analysis at a practical level for industry has just really started since TMI. It has never been done by the NRC. It has never been done by DOE and industry felt that they were

precluded from doing it since it was not a designed bases accident.

So, I think that UCS has been helpful in that respect. They really raised these issues as contentions and properly so. The NRC, is now, I guess, putting 42 million a year in the testing of this kind. Industry has set up the degraded core effort to study these questions and get the best science that you can get out of them.

There's quite a bit of experimentation that is going on on realistic -- One of the problems with the LOFT program for example, it is very easy to -- If you're trying to make a worse case statement, it's very easy to postulate a series of test parameters for a LOFT test, some of which are mutually inconsistent. So, you're doing a test that has no correspondence to a physical reality.

Their getting out of that kick. They're trying to do more realistic tests and sometimes succeeding. So, I think there is a change in the system which is constructive and the question is really it's a race between how fast you can implement it and the public perception -- I guess one other thing we should add to the safety goals which I think is much tougher.

The plant can be totally safe to the public, but extremely dangerous to the operator, financially and that really says that the probability of prolonged outage or major

equipment damage as well as major core damage must be reduced very substantially over past history. Because there are now 37 years of outageous in excess of six months in U.S. nuclear plants. Most of these have not been publicly dramatic, but in terms of replacement power costs and financial impacts, most of those 37 years occurred before 1973, so the oil impact was much smaller. So the impact of a future pattern of that kind, economically and socially,would be much grater.

Worse yet, the public perception since we don't have a good safety goal mechanism -- public perception that an equipment damage is a near miss to a major catastrophe is almost universal. At least some part of the media will treat any of these events as if we almost lost Sacramento or Detroit or whatever. I think that unless the industry can reduce the flow of that kind of event, which is much tougher than public safety in my opinion, that alone would prevent reordering the new plants even if the financial and regulatory climate would improve.

So, that's a point that we're suggesting. Let me say how that comes back to the safety goal, because there is a very nasty connection to the safety goal. The safety goal if it's expressed as some of the trial runs have been expressed, usually involves a product of several probabilities. The probability of hurting the core. The probability of spreading radioactivity. The probability of environmental consequences.

As the regulation has been practiced, there's a tendency to regulate each piece of the chain independently. In other words, if you're perceived to be -- higher than some criterion on one probability, the fact that you're better on several other factors tends to be given as a credit that you don't have to fix it right away, but still go fix it.

A consequential safety goal, it would seem to me, would at least have some insulation on that because it's extremely counter productive if the situation is such that if I make a change which at low cost gives me an improvement in safety on a particular segment of the risk, but as a consequence of that I take two risks. I take, first of all, the risk that I get no credit for and secondly, that the change itself might provoke a hearing process with a delay.

And so you get a tremendous inertia in the industry to make changes even when they're sensible. In fact, that's the most counterproductive element that I see to this learning process. We can learn. We can document. We can communicate, but the implementation has this very real inhibition to it, unless it's something very simple and very uninvolved.

I think that's part of the thing that we're -- Vic Stello is right in the middle of that one, because as he makes his actions more and more punitive against the operators, the communication gets choked off and the tendency to white-wash, I think, is inevitably going to -- is a dramatic drop

in operator error reporting in two years. I think '76 and '77. There was about a 30 percent decrease. This is not on safety issues. This is just on plant reliability, the EEI data base, had a sudden discontinuity infrequency of operator error caused incidents. Well, clearly the more punitive environment to the operator, made them want to go that way.

So, I think that one of the criteria, one of the attributes of a good safety goals is that it be consequential. That you look at the overall public risk and credit an improvement in any segment of the thing.

Right now, at least one piece of the NRC committee chaired by Guy Erlotto is busily going the other way on the degraded core issue. The only credit mitigation will not credit prevention. It is tremendously counter productive. Now we can analyze and find the thing that will reduce some probability of occurence and the utility says, I get no credit for it. Why bother?

There are some ills in that kind which I think that's why people say that we need structural changes in the NRC. That environment has to be --

Sorry, I got so wound up on this, but it seems to me that that is one of the key environmental questions of the effectiveness of the safety goal.

CHAIRMAN LAVE: Let me just go back and just try and make sure that I am now enlightened about relatively

common accidents and accident sequences.

I'm still feeling at sea about uncommon accident sequences such as those that might start from sabatoge or terror or something of that sort and wondering how we can go back and build those.

MR. HUTT: Lester, isn't that the same question as what is the shape of the dose response curve at a low dose on a brand new chemical that you don't know anything about? That you have to deal -- you know no more about that then you do about the likelyhood of sabatoge and you have to use some modeling which has all the problems that you've already heard. Modeling has all the room for debate and yet you've got to do the best that you can and put right out for everyone to understand what the limitations are, which anybody who does -- modeling does. If you don't know, but here's our assumptions. Here's the best we can do and here's why it's better than doing nothing and proceed from there.

Rcognize that you can't quantify it to the -- in the true sense, but you can give an upper bound to it, based upon certain modeling and if over a period of time you're proved to be wrong in one direction or another, which will inevitably be true. Either it will be better or it will be worse, then you correct your model and proceed on from there. Then years from now.

MR. LIBARKIN: It may be the same kind of a question

There's at least some theoretical possibility that in connection with the low dose response of a new chemical and mechanical equipment theoretically, you can do things that will let you learn something and reduce the uncertainities --

MR. HUTT: I'm sorry -- there isn't. Well theoretically other than the so-called famous Mega Mouse Study, which no one has ever proposed. There is no way that you will ever find it.

MR. LIBARKIN: Okay, then it may be the same one. What I wanted to throw out as a question was, is there a fundamental difference between what we can learn about the probabilities of equipment response and the probabilities associated with human actions and inactions and conclusions.

MR. HUTT: In the biological field you have an even broader perameter, because the slope of that curve is somewhere between one and zero. And you don't know. There maybe a threshold and there maybe an absolute linear relationship. You just -- And there is no way that you'll ever find out that anyone knows that.

DR. ZEBROSKI: But the exception, I guess the way the rules run now, sabatoge is defined at some modest level, like 16 men with a bazooka and a bag of explosives or something like that. At least up to that level, there is some look at sabatoge questions. However, there is very intense look at seizmic questions and there's routinely you look at

things which are not looked at in most other public risk. For instance, routinely you look at the risk of an aircraft crash into a building containment. You don't look at the risk of an aircraft crash into Candlestick Park in mid-game, but the probabilities are not that different.

One, in fact, off the end of one of the major runways of the San Francisco Airport and landing approach could well end up in Candlestick Park in mid-game. So, I think you get caught on a real dilemma to the extent to which you look at implausible sabatoge scenarios and publish them, people worry that you increase their probability.

MR. HUTT: We went through this a little bit earlier this week of running through a war games type of issue on recombitant DNA used in sabatoge. The same exact questions. How it could be done, who would do it, under what circumstances, what the probability is.

CHAIRMAN LAVE: I think that precisely what you can answer is what the probability of the human action of that sort. That conceptionally is an unknown. We don't have any random process that's generating that human action that you can look at.

MR. LIBARKIN: And there is no theoretical approach that anybody knows that is analogous to collecting data and doing other -- things.

MR. HUTT: I said it was very simple. All you have

to do is count the number of mad scientists.

MR. DERBY: There are qualitative approaches in -- I did some work along those lines of sabatoging. You're absolutely right. There is no observation data gathering kinds of things,but it's handled qualitatively along the lines that you're doing. You say, how many people in the world would really want to do this?

DR. ZEBROSKI: It is being treated though, very much like a dominant sequence. People are saying that if you are going to attack a plant, you have to have certain resources and certain man power and certain skills and you can define those attributes and you can say what can I do to detect and defend it as early as possible.

MR. DERBY: Probabilities is what I'm talking about.

DR. ZEBROSKI: No, forgetting probability. This is very deterministic. Sandia is actually running mine attacks on barbed wire and electronically protected plants and figuring out how to penetrate them and succeeding a good deal of the time.

MR. DERBY: What turns out, is that it probably easier to do all that stuff if you work in the plant.

MR. BRIDENBAUGH: That's the other thing, because at least certain times, you've got at least hundreds and sometimes thousands of people in the plant. So, you've got a big population there.

MR. ZEBROSKI: There's a great resistance at one time to doing either -- a union resistance to psychological testing or any security clearance. They're now being required by the NRC and I think properly so.

MR. HUTT: If you can get close enough to the president to shoot him, you can certainly get to--

MR. LIBARKIN: Maybe the a conclusion should be that any safety goals you have as a large qualitative component sort of thing.

MR. HUTT: I think that we can spend all day debating the risk of sabatoge. I think it's sufficient if we realize that we can't quantify it in the same way you can quantify other things. You, therefore, do the best you can and lay out all of the uncertainties.

CHAIRMAN LAVE: I think that it is relevant to know what are the consequences. In some sense, what do our upper bounds look like for something occurring. If you had some piece of technology so vulnerable in society that you could essentially wipe out society if somebody chose to sabatoge that, then that would be very close to an unacceptable risk and so, now how far -- I'm clear that we're not talking about anything remotely comparable to that, although for recombitant DNA, we might be.

MR. HUTT: In incombitant DNA was that we clearly were not. Today, you wouldn't have anything that you could

use to do that and not in the foreseeable future.

CHAIRMAN LAVE: But, I would guess that the approach would be one of trying to in fact see what those upper bounds look like and see whether you could get agrement on it.

DR. ZEBROSKI: Upper bounds of what? Sabatoges?

CHAIRMAN LAVE: Sabatoges, yes.

DR. ZEBROSKI: We were discussing this before the meeting got started and the pretty extreme scenario that was being looked at by the Israelis and was published about January this year, is a sequence in which you blow up the containment and penetrate the reactor vessel with a missle. The interesting thing is that that very quickly looks like Big Break Loca and you get into simply the probabilities that you can supply water to the system.

They didn't have the advantage in doing that study of our present perceptions now of the advantages of wet verses dry so that they assumed that the system would go dry and stay dry indefinitely, which then gives you transport of large amounts of long -- activity. And so the paper then describes the relative amounts for different scenarios of such attacks.

Our perception, if we were to redo this paper, today, we would add the further observation that somewhere in the first ten to 100 hours -- First of all you would have a great deal of water around from the penetration of the system. The

water that's in the system ends up in the basement and sits there. The core melts and drops into that water and for quite awhile nothing happens other than the release of some rare gasses, which is what essentially happened to TMI.

After some additional hundreds of hours, that water would be gone and then you would start to get into the scenario the Israelis looked at. The question, then, is did somebody bring up a garden hose or a fire engine within that few hundred hour period and if so, then the thing does not turn into an ecological disaster.

You certainly have local contamination that would be very nasty. You wouldn't be able to use the plant, but public health effects would probably be near zero.

MR. BRIDENBAUGH: Are you convinced, Ed, that a steam explosion is a non event?

DR. ZEBROSKI: Pretty much. We're trying to make them. We have a slid tap furnace in the Commonwealth system where we're dumping tens and hundreds of kilograms of molten oxide into the water at various rates and trying to see if we can make a steam explosion.

MR. BRIDENBAUGH: When is that going to be finished? Do you know?

DR. ZEBROSKI: It's an opportunistic. Well the theory is pretty good. This is really trying to demonstrate the bounds of the theory. This is Bob Henry's study and the

professor -- I'm forgetting. There's a Swedish guy who has also done the theory. There's a great deal of experimentation with many fluids so tat the bounds of where you can get an explosion you can set very specifically. You have to get certain particle size. Certain date of heat transfer and certain pressure situation. At least in laboratory experiments up to fairly sizeable scale, those laws hold very closely. So, there is even a scale of testing which is what we're --

MR. BRIDENBAUGH: When you say large scale, what are you talking -- kilogram quantities, would you say?

DR. ZEBROSKI: Ten to 100 kilograms. And Karlsrue I think on some years time will go up to 500 kilograms.

The real issue there is very simple. Nobody has figures out a way to get a coherent drop of the core. If you get something that is more like the pouring of a molten ladle than you never get enough energy per unit time to get anything like an explosion. You get popcorn.

MR. DERBY: All that relates to the quality of standard. There are parts of a standard that don't lend themselves to the quantity of analysis, to date.

DR. ZEBROSKI: You have quantitative analysis today and we publish quite a bit of it, for which you would like to validate as much as possible, both the laws and the scale modeling with experiments and that is what is going on.

The NRC has a very big program of that kind. They will probably melt down LOFT one of these days to confirm it. They can schedule it for '83 as a matter of fact.

Isn't that right?

MR. LIBARKIN: '83 is good.

CHAIRMAN LAVE: Where is that going to be?

MS. SHELDON: Let's make our reservations now for the other direction.

DR. ZEBROSKI: Maybe this is desensitization therapy, because I think the melt down will turn out to be a very tame event.

MR. LIBARKIN: I don't know if there really isn't a proposal to do that.

DR. ZEBROSKI: I think the hang up will be the cost of clean up and decommission, but I think the likelihood of that does anything -- that your predictions will -- surely over predict the actual systems response, because predictions tend to give favor to coherence which is very hard to achieve if you wanted to.

Let me say what I mean by coherence. The reactor has a power distribution so that the center of it is hotter than the next ring, which is hotter than the next ring and so on. And to get big accidents of the steam explosion type, you have to assume that the whole core melts instantly and uniformly and then dispurses itself in the particles of size

smaller than grain sugar. And if it doesn't do both of these things, you get no explosion. So, that both the theory and the experiments say that you can't really get those things in any reality. That's the catalogue that you have to get to if you are to postulate this happen. But whether you could make it happen, if you wanted to, would be very very difficult.

CHAIRMAN LAVE: I feel enlightened now on epistomology. So, let's go on.

We're up to five and six. I don't feel terribly strong about five or six. Does anybody have any strong feelings?

MR. TEMME: The only strong feeling that I would attempt to reiterate is that I think it's important to distinguish between goals and decision rules or standards.

CHAIRMAN LAVE: That's what we're going to try and talk about under five.

MR. TEMME: Some people this morning said that they didn't see the difference. If we got over that or not.

MR. DERBY: We didn't.

MR. TEMME: I think that you explained the difference. You have a goal which is that there -- it goes something like there should be less than one person in a million who dies as a result of a particular carcinogenic material in his environment. I'm paraphrasing, but it goes something like that. That's a goal.

You have clear rules which you apply to each suspect

material, based on the data that you have on how many rats died and so forth and you use 99 percent confidence limits and very conservative rules, etcetera, but they are rules.

You have a convincing logic that if you follow the rules there's at least a chance that you'll meet the goals.

MR. HUTT: The rules, I assume, are dictated by the goals.

MR. TEMME: They are certainly very well connected. The point is, you really don't without measuring your performance against the goal. You aren't going around to the morgue and finding out who died of which carcinogen.

The goal is what you're aiming at. But the standard or the rule is what you measure yourself with.

MR. HUTT: Okay, I understand, but what was said earlier was I guess what troubled me. The implication was that the goals either would be so vague as to be unhelpful, such as no unreasonable risk, which is --

MR. TEMME: I certainly don't mean to imply that when I say --

MR. HUTT: I think your example,just now, is the best one. Because if you have a quantitative goal that can not be measured because you can't go and measure, as you say, the number of people who will die or the number of catastrophes or whatever. You can't at any point in time be certain that you've met your goal, but it is still a quantitative goal.

It is the sum of the parts of the various standards which necessarily -- not just out of vaguly related, but which necessarily add up to that goal.

MR. TEMME: It's the relationship that becomes a difficult issue. In your situation, you are able to define and live with certain bounding kinds of rules that Steve pointed out and I agree with him. When we begin to deal with accident sequences and their probabilities, that becomes more difficult to do to select the --

MR. HUTT: But I think that we've agreed -- Stephen had agreed now, that yes you've got to do the best you can and you've also got to realize that it is not perfection. So, I think that we've resolved that part of the issue.

MR. TEMME: And in fact we know for sure that we will have to make compromises.

MR. HUTT: And as Norman said, it's got to change over time. It's got to be a series of operational rules that you will revise as experience dictates.

Okay, I understand better the use of this concept goal which I had been very confused. People kept saying that you never met the goal. That is not my view. If you meet all of the standards, you've met the goal. Unless you've found out that your standards were wrong and weren't adequate to meet the goal, in which case, you change the standards, but you do meet the goal. It's just that you can't -- There's

no objective way of measuring or proving that you've met the goal.

MR. TEMME: That's an important point.

MR. HUTT: Then, I'm satisfied.

MR. BRIDENBAUGH: You can have a two step goal, too. A minimum standard and then an objective which may be somewhat higher than that or lower depending on how you frame it.

MR. HUTT: It might be useful. Maybe we need a little bit of terminology here. Because the first thing you have is a generalized qualitative Congressional mandate. And one can argue, that's the goal and there's no undue risk. I don't know what the AEC statute says.

MS. SHELDON: Undue risk to health and safety.

MR. HUTT: I didn't even know that. All right. No undue risk. It doesn't make any difference. You could phrase it ten different ways and it wouldn't be any better or worse. That obviously one has to do something with that, because that is meaningless. So, you reduce that down to a different type of goal. A quantitative goal and -- of the kind that you were talking about Mark and then you further reduce that down to operational rules that if followed to the best of everyones present belief based on modeling and uncertainty and all those qualifications built in. If you follow those decisional rules, you meet your goal, which is a surrogate for the Congressional standard. I don't know we're using,

but we've got to get some terminology in here that will explain that.

CHAIRMAN LAVE: That's right and if your modeling is incorrect and your standards or rules don't meet your goal.

MR. DERBY: You can advance the methodolgy somehow. You can gather data. You can have modeling insights. Those change the standards, they don't change the goal.

MR. HUTT: No, they help you reach the goal in a better way to be more sure that you reach your goal.

MR. DERBY: So your procedures change with information if you -- a well stated goal doesn't change very often.

MR. HUTT: But, I think we ought to make it quite clear that the goal and the statuatory standard are different. The goal is a surrogate for the statuatory standard since the statuatory standard is unhelpful or essentially meaningless.

MR. TEMME: What I hear you saying is that the quantitative goal is a surrogate for the qualitative goal.

MR. HUTT: Okay.

MR. TEMME: And neither of them is necessarily the decision rule.

MR. HUTT: Correct. Fine, that's a fine way to put it.

MS. SHELDON: It occurs to me. This is just some-

something that occurred to me. One function of the qualitative goal or the Congressional statement is to provide for a check through the courts by society on what the agency is doing with the quantitative goals. I don't find undue risk unmeaningful in the context of what I do. It allows for an examination of particular activities and a particular situation and checking them. Putting them up against the standard and then some judgment about whether what was done constitutes or does not constitute undue risk or inadequate protection.

There is a function for that language and I don't think that we ought to toss it out.

MR.HUTT: I'm not arguing. First of all, we can't toss it out, because Congress wrote it.

MS. SHELDON: For the future.

MR. HUTT: But, beyond that, my argument, Karin, simply is that one could take any of fifteen different phrases --

MS. SHELDON: Sure. There are fifteen different phrases.

MR. HUTT: -- and different statutes over the last 200 years and plug anyone of them in and it wouldn't make a bit of difference and therefore it is such a level of abstraction as to not lead to consistent reproducible decisions. That's my concern.

MS. SHELDON: I think that whatever the language is

though, it expresses an attempt -- a general intent about what these activities are or are not going to do. Impair the public health, create an undue risk or what have you and that needs to be translated into things that the people are actually working with the nuts and bolts can operate with on a day to day bases.

That's what I was trying to get at this morning and I think what you were trying to get at.

MR. HUTT: I have no quarrel. I was, in a sense, overstating my lack of enthusiasm for Congressional language.

MR. LIBARKIN: What is that make the court conclusions in this checking process against whether or not the risk is undue? What is it that makes those conclusions other than arbitrary or random from court to court?

MR. HUTT: Not much.

MR. LIBARKIN: What do the courts do, heaven help them, to decide?

MS. SHELDON: That's hard. They try to look -- Well, first they duck behind the arbitrary and capricious standard and they say wait a minute, I can't substitute my judgment for that of the agency in this respect, because the agency is charged with knowing about these things and has the expertise, so I'm just going to judge by the whole accumulated bunch of cases or rules about what is or is not arbitrary or capricious. Basically was there a rational bases for the

conclusion that the agency came to? Did it come out of consideration of evidence on both sides? Things of that sort. They look to see if the agency decision seems to reflect rationality and evidence.

So, if they don't duck that and in some instances, they won't, then they try to wend their way through the evidence presented by both sides and thrash it out and come to some conclusion about whether in fact what was done did or did not violate the statute.

DR. ZEBROSKI: Karin, is there any legal -- It seems to me that lawyers have their own goods and society has rather different goods. I know of no -- I have many lawyer friends, but I know of no lawyer who would --

MS. SHELDON: Some of your best friends are?

DR. ZEBROSKI: -- argue that the -- in one situation giving a family three thousand dollars as compensation for the death of a child and in another case giving three million dollars compensation for mental anguish that there was anything wrong with that. In fact, they just applaud the lawyer who was clever enough to get thirty percent of three million dollars.

Now, I don't think that that is necessarily a social virtue. I think your bases for judgment of what's good in the courts is very flawed and I think there is at least in this state a tremendous resentment of the court system right

now.

MR. HUTT: Let me defend Karin for one moment. Karin was not defending the fact that different courts come to different conclusions. As I understood her, she was stating a fact.

DR. ZEBROSKI: I understand, but there is not even a shall we say Hypocratic Oath which says maybe that's not good for society.

MR. HUTT: I think that courts and lawyers firmly believe that is not good for society, but no one yet has figured out away to put courts and lawyers into strait jackets. Anymore than one can put scientists into straitjackets or regulatory agencies into straight jackets. The problem is very simple and any good lawyer knows this.

Karin certainly knows it and Martin knows it and that is --

MS. SHELDON: Wait a minute. When you say any good lawyer and then I know it.

MR. HUTT: No. Any lawyer that practices before courts knows that any of three things can happen when you get before a court. You can win. You can loose. The court can screw it up beyond redemption. Two out of the three are not very good. In all seriousness, you know that if you're going before the D.C. Circuit,then you get three judges, you can pretty much tell what the result is going to be before you

get there. If you go down to the Fifth Circuit, you know your chances are infinitely better. Talk about risk assessment. We ought to have risk assessments on courts.

MS. SHELDON: And lawyers do it all the time. Where should we go?

MR. HUTT: You're darn right. That's the first question, not the second one. The first question is which court will we go to and then you worry about what your legal arguments are.

MR. LIBARKIN: Isn't the law what the last judge said it was.

MR. HUTT: Absolutely. Never believe that the law is what the statute says. The law is what the judges say.

MS. SHELDON: The judges of the circuit you are in.

MR. HUTT: Yes.

So, that is a problem that is inherent in our judicial system and one that frankly we can't do very much about today. I don't think we ought to spend a lot of time on it.

No, it is not a virtue of the system and no one would ever pretend it is.

MS. SHELDON: I don't understand quite and maybe this is something I shouldn't admit. I think you all vastly overrate the problems of lawyers in litigation. You all seem to be terrified of it and I'd like to know what is it that we

have won or what is it that we have done over the past ten years that has caused this?

MR. DERBY: Delay.

CHAIRMAN LAVE: It's not on the subject of the panel today.

MS. SHELDON: Well, okay. Delay is not lawyers.

DR. EISENBUD: I wrote the original draft of part 20 at a time when it wasn't appreciated by the people who asked me to do it what the role of the lawyers would be. So a couple of my buddies and I, -- we were at the AEC -- put together what we thought would be a subsequent -- and it got no where and believe me what came out bore no relationship to what was originally proposed by the people who understood what the needs were for regulating the affluence and providing radiation limits.

I think that was the turning point in the history of atomic energy regulation. Of course, there wasn't any regulation up to that point.

CHAIRMAN LAVE: I have this question, if I could sort of bring us back to task, after these interesting interludes -- enlightening interludes -- the reason why we're really here. That is the question that I had is the one of how is it that you translate goals from one level to another? That is from Congressional statutes to quantitative safety goals of the agency to rules. How do you go down in the

process and then how do you go back up again once you've learned something?

Is there a system that one can talk about that will try to insure that these levels are consistent with one another? That what you learn at one level gets reflected up and down.

MR. LIBARKIN: Do you mean is there one existing now?

CHAIRMAN LAVE: No. Could there be one? I think the answer surely is no.

MR. LIBARKIN: I was going to tell you, it's an easy question.

MR. HUTT: Are you talking conceptually? Are you looking for a two minute discussion of the fundamentals of administrative law or what? I'm not quite sure what the question is, Lester.

CHAIRMAN LAVE: I guess that what I have in mind when we're thinking about feasibility of a quantitative safety goal is that -- and it talks about implementation, now, that a large part of the implementation is going to be taking something which sounds like a valid meaningful statement on one level and making sure that it is translated into valid meaningful statements at other levels and that experience gets in there and is reflected back and forth.

I guess that I certainly have a feeling that you

can do this. Conceptually, there's no reason why you can't do it.

MR. BRIDENBAUGH: I have a question and I guess it's a legal question. There is a process for doing it, isn't there? There is a process for rule making. And that is in place and really the only different question here is how do you perform the rule making in a meaningful way and in away that the public will accept it? Isn't that the issue?

CHAIRMAN LAVE: I guess I see two issues. Rule making, I think -- the largest component of rule making is sort of getting at some value issues and trying to settle them. The other component is one of getting the technical stuff straight and I think that rule making is a terrible process for getting the technical stuff straight. It's organized all wrong and it's just terrible for that.

You certainly don't want to try and get the technical experts together and agreeing in a rule making procedure.

MR. HUTT: Well, Lester, let's just take a moment. There are all kinds of different mechanisms that one can use in administrative law to take a concept of this nature and employ it in regulations. I think that's what you're asking. How can you do this?

There are however, two fundamental ways of doing it. Two, I would say, fundamentally different ways and they've

been variously characterized as goals and regulations or goals and rules. I would like to rephrase that, because the better from the legal standpoint, the differences between a guideline and a regulation.

Increasingly agencies are differentiating between these. A regulation is something that is legally binding. Now within a regulation, you can have all degrees of flexibility. You could adopt a safety goal and safety standards both, using our new found terminology, here. You could adopt those by regulation, but still have them entirely flexible and allow people to either apply them or not apply them depending upon all kinds of different factors.

The fact that it's embodied and here a lot of scientists I think get confused. The fact that you put something in a regulation doesn't make it inflexible. Even without changing it, because you can allow, just as an example -- In the area of the Radiation for Safety and Health Act which I helped implement for four years at FDA. We build into all the regulations there all the requirements for x-ray machines variations and all you had to do was, if you were a maker of a machine and you thought you had a better way of doing it, you applied for a variance. Nobody had to change the standard, i.e., the regulation. FDA unilaterially granted the variance and published the notice in the Federal Register saying we just granted a variance and that was the

end of it.

So, you can build in and the idea that a regulation is inflexible,carved in stone until you go through a whole other rule making procedure is simply, flatly wrong.

Now, second,you don't have to go to a formal regulation which does involve notice and comment rule making. You can use the other way of doing it, informally, which is a guideline. A guideline doesn't even have to appear in the Federal Register. Guidelines are increasingly used by FDA in this area of risk assessment and related areas.

Basically, what it means and FDA is the only agency that has a regulation defining the difference between a regulation and a guideline, because it is of critical importance. The guideline binds the government, but not the industry and that's the fascinating part of it. A guideline says, if you meet these criteria or standards or goals or whatever it is, then we the government will accept them. But, if you think you can do it in a better way, then you can do it any goddamn way you want as long as you can show to us that it's as good as these goals.

The best example of that is in the area of toxicological protocols. Merrill, you will recognize this. FDA has a whole series of guideline protocols. Anybody who wants to run a three generation rat reproduction study has two choices. They can do it according to the protocol. In which

case, it will be accepted by the government without any question or they can do it their own way and justify why they did it different and if there is good scientific reason for it, it will be accepted by the government and if it isn't, there won't be.

It is not, in short,a binding requirement. It is instead, as the word implies, a guideline. Now, one could debate what is the difference between a guideline as I just discussed it and a flexible regulation and there you're getting into a more Jesuiticle how many fairies dance on the head of a pin, because when you get right down to it, there is probably -- Are there some Jesuits here?

I'm sorry. Withdraw that statement. Erase the tape. I just got a no on that.

In any event -- there you get into the most subtle distinctions that I can easily, with no difficulty at all devise a guideline that is identical to a flexible regulation and vise-versa. So, that there is a range, when you get to the details of administrative law -- there is a range -- a continuum, if you will, from the most binding to the most flexible and I don't think we ought to waste time debating on all of the subtle differences.

The real question is, what effect do you want these to have? If we can decide that, then let NRC decide how to promulgate the damn thing. That's trivial. The question is

what impact do you want whatever these goals and standards are to have on the process?

MR. MALSCH: There's an example of that in the current NRC regulations. Let's take for example what is perhaps the most detailed regulations. Regulations on emergency core cooling systems. You have the statuatory standard of no undue risk. There's an implicit behind the ECCS rule -- goal, if you will -- that says that means that the chances of the emergency core cooling system not functioning because of an engineering design error in causing a melt down of the core should be very small.

That led to binding criteria, one of which is, the calculating -- temperature following the postulated loss of cooling accident should be no greater than 2200 degrees. But, then below that there were promulgated very detailed provisions regarding the evaluation models which are computer codes to use to demonstrate whether or not in this example the peak clouding temperature standard had been met. And in specifying those codes, the Agency was very careful to distinguish between required features of evaluation models, which every evaluation model had to contain and acceptable features of evaluation models. Which, if the model contained them, it was acceptable, but they needed to contain them if they wanted to develop some other model, that was fine.

MR. HUTT: Let me ask, since obviously we have

someone -- a consumer advocate and a representative of government, both lawyers if anyone wants to add to or differ from anything I said there?

I just want to make sure that we have the legal parameters.

MS. SHELDON: Right. I think your comment about you have to decide what impact you want it to have, is appropriate. Of course, on our side, we are happier when more things are more fixed in stone. I would prefer regulations to policy to guidelines in terms of trying to get people to implement things or to follow things that are written down. But in terms of the agency perspective, it may be that the other gives more flexibility.

MR. MARSCH: I will add though, that there has developed a bias against exceptions or exemptions from regulations. At least in NRC practice, because there's a prevailing attitude that somehow, it's bad to have to ask for one.

MR. HUTT: That's a -- You don't disagree, either of you with the description of what I set out. I agree that everyone has a different view as to what is good, bad or indifferent. Okay, sorry Merrill.

DR. EISENBUD: I just want to comment, because I think it was you, Karin, who asked --

MS. SHELDON: Why do you always pick on me?

DR. EISENBUD: First let me give you an example of the kind of thing that happens. It turned out in the early 1940s that -- I hate to keep going back to the ancient time, but that was the time before lawyers, you see, in this business. The metal berillium turned out to be extraordinarily toxic. The AEC found that it was the only customer for the product which was produced by two producing plants. Although, it wasn't required to under the Act, since it's the only customer, would write into the contract that standards had to be met, because there weren't any standards.

So, we set up some studies to establish standards. That was -- Those studies took a little less than a year. You know, in 1947 dollars, cost $50,000. The standards exist to this day. They have eliminated Berillium disease from the United States for all practical purposes from the world, because they've been adopted everywhere.

In 1975, OSHA decided that there was evidence that berillium was a human carcinogen. They decided to adjust the standard by a factor of two downward. A hearing was held. The issue hasn't been resolved. I guess you could say that the hearing is still open or the record is still open or whatever it is. The cost of the company in legal fees for the last five years has been over a million dollars.

What we can't understand --

MR. HUTT: It's like a minor proceeding.

MS. SHELDON: Speak for yourself Covington and Burling.

MR. HUTT: Let the record show that there was laughter following that remark.

DR. EISENBUD: We don't understand the extraordinary degree of choreographic orchestration that goes into these ballets that we witness. This story of Part 20 is very interesting. The year that they set up the regulatory apparatus within the AEC, they turned to our group and said, would you write a protection. They knew that it was going to end up in the Federal Register. They knew that it was going to Part 20.

We turned out a draft without the aid of lawyers. It was a good document. You'd never recognize what finally went into the Federal Register and so -- It's hard for us to understand these things.

MR. HUTT: Let me now get to my own personal view of what is better to do in terms of regulations and guidelines etcetera. When I first went to the government -- in fact, in my four years in the government, I spent writing Federal Register documents basically. When I first started out every single one was a regulation. My own view was that greater specificity was a better way of regulating. More effective and efficient. Towards the end of my time there, I became more convinced that while in some instances, it was indeed a

more effective way of regulating. In many other instances, it was less effective because where it engendered enormous amounts of controversy, you spent more -- it was less efficient. Let me put it that way.

You spent more time and effort debating the regulation and spent more time litigating it in the same way you're talking about, Merrill, then you would by using a guideline where you don't need to have one of these incredible proceedings. Moreover, it didn't really end the controversy, because under any system you're going to have to look at and no matter how much Martin you described NRC may not like to do it, under any rational system you've got to provide for variances, because --

MR. MALSCH: Not all of our regulations do.

MR. HUTT: You just simply -- No one writing a regulation, scientist or a lawyer or both, is going to be able to figure out all of the problems ahead of time so that you won't need exceptions and variances and increasingly --

There was a third thing that began to worry me more and more. Again, it's a bigger problem when you get into the more complex technological areas and I assume this is certainly one of the most complex. You can't forsee all of the improvements in technology and if you're in a rule making, you've got to then change the rule each time. You just can't get there from here. You can't do it fast enough to keep up

with changing technology; where you can change a guideline literally overnight. And therefore, it is a more efficient way of proceeding.

I was fascinated, Lester, that our mutual friend Dick Merrill who succeeded me as General Counsel at FDA and who is now Dean at the Law School at the University of Virginia -- Dick and I were at a conference recently. We were dealing with the regulation of new drugs, obviously, not with nuclear energy. But Dick startled me by making a quite independent -- He and I never discussed this. He made that one of the major points that if he were in government today, he would increasingly rely upon informal regulatory mechanisms like guidelines rather than formal mechanisms like rule making which is becoming less and less efficient every day and I couldn't agree more with him on that.

DR. ZEBROSKI: Are you suggesting the option of pilot run, so to speak, on a safety goal and the things that go with it has been discussed by the people working in NRC, that you -- Or they call it interum rule -- Where you, in effect,try out and see how many variances you get and if you get too many, perhaps you should modify the rule.

Maybe what you're suggesting is a better way to go. Either go, primary, the guideline route. Perhaps for quite a number of years and then codify good practice in a rule or a standard after you have experience at the guideline level.

MR. HUTT: This is what FDA has done, interestingly, in the area of setting tolerance for unavoidable contaminants in food. It uses guidelines, not formal regulations. For example, the amount of aflatoxin allowed in peanuts, to this day, twenty years after the first guideline was established, has not been set into regulation, because FDA is not yet convinced that it has reached the optimum level -- low enough level that the industry is capable of going to. There's, you know -- There's some very good sound regulatory policy, efficiency and other reasons for doing that.

I will have to confess, Ed, I am out of my field when I say -- I can offer no opinion in these specific circumstances whether that approach would be best here or not. I don't know enough this in a day and a half, obviously, to say that. But, I would simply discuss the principle that the degree of flexibility in that kind of approach may be better from everyones standpont. Because it does provide the one thing -- You must remember, it does provide some certainty and that is if people do meet the guidelines. They have assurance that the government will accept that as a reasonable way to go. It's not the only way, but it is a permissible way.

DR. EISENBUD: Are there guidelines in x-ray and microwave for you?

MR. HUTT: In the x-ray field, no. They went the standard approach in the x-ray. One good reason. The statute

required it.

DR. EISENBUD: Let me say that among radiation protection, it is the FDA rules and regulations I greatly admire as compared to what the NRC has.

MR. HUTT: But there are two things and you may be referring to it in a separate question. There is a performance standard for x-ray machines. There are also guidelines for human exposure to radiation and those guidelines can be changed by a stroke of a pen overnight. So, yes. The former are regulations, the latter are guidelines.

And they have been changed, as you know.

DR. ZEBROSKI: Would a guideline, however, fail on the test of some assurances to bowie for a period of time. That is flexibility is one objective, but another objective is predictability.

MR. HUTT: I took care of that when I defined guideline for FDA. What I said was in defining it was that if you meet the guideline at the time that it is established. If, for example, you run your rat reproduction study at the time the guideline exists and the government subsequently changes the guideline, that you will not have to go back and rerun your rat reproduction study unless the government explicitly finds that there is a substantial danger to the public health. In other words, they can't change the rules of the game unless there is awfully good reason to do so.

That makes good sense on both sides, because otherwise, you'd have to rerun it every couple of years when the guideline changed. The answer is no, in short. So, you can build stability into the system without loosing flexibility in a sense. It's a grandfathering type of process.

MR. BRIDENBAUGH: In the nuclear business, of course, we have regulatory guides, which I assume are the same sort of bases. They're changed often, but the applicability of the reg guides date back to the time of issuance of license, generally. So, that's how that predictability is covered there supposedly.

MR. HUTT: They can't force you to tear down your plant and rebuild it in accordance with the latest guidelines?

MR. BRIDENBAUGH. The only way that it could be done -- into regulation that says that if it is determined that a significant improvement in safety could be made by --

MR. HUTT: Exactly the same kind of concept that we built into FDA's regs. Again, I'm not in a position of arguing whether that would or would not be a good idea in this industry, because I don't know enough about it.

CHAIRMAN LAVE: Can I change the subject a bit. One of the things that I thought we were chaffing at the bit to do was to take a look at when you've got some standards that everybody is proud of, how you make sure that the industry actually lives up to that standard. That is that performance

corresponds with your initial design.

MR. DERBY: I'd like to take a crack at what led you to that statement, which was you asked what the connection between the goal and the decision rules happen to be.

The administrative law gives the format, I believe, of something that is a regulation and a guideline. In my world, how one fills out a blank sheet of paper that perhaps has the title, decision rule, one has to do something that then is either labeled a guideline or a regulation.

My comments are directed at how one fills out that sheet of paper and what is going on in your mind and how other people can have dispute with it. You essentially build a model. You say and I think one example that might be fairly clear is quality of life. There are a bunch of things that one should not reduce the quality of life. What is the quality of life? Now you've got to make a list of measurable parameters which singly or taking together somehow means quality of life. So, that if they go up, the quality of life goes up and if they go down, the quality of life goes down and there's this intuitive relationship between your notion of quality of life and these parameters.

In the nuclear industry, there is undue risk. So, someone has to sit down with a blank sheet of paper and write down what risk is. So, that if that goes up, risk goes up and if it goes down, risk goes down. That's a very subjective

part of the process. It is the key and weakest deterministic link in going from an overall goal to the variety of standards that are developed underneath it. It is that separation into the various aspects that you want to address in this particular standards and what measures that you want to use.

Other than to acknowledge that it is in the eyes of the beholder whether that is good or bad, I know of no objective way or scientific way of making that list. One cannot collect data. One cannot do statistics. One defines the problem in terms of more detailed description of what risk happens to be.

That's one aspect of what you're talking about. What is performance? What is the rule for performance? What is the measure for performance and how does this come together to mean undue risk? I don't know how to make that procedural. I don't know how to make that a regulation.

MR. HUTT: What do you mean? What's the problem with compliance, with determining compliance?

MR. DERBY: Once you have such things.

MR. HUTT: But if we agree that what you would have would be a series of decision standards, putting aside the question of whether they're guidelines or regulations -- we can call them standards -- That if followed, would lead you to the presumption that you have met your goal. Then what you do is check and see whether people have complied with the

standards. What's so difficult about that?

MR. TEMME: I think Lester is introducing a sort of different factor, here. In my mind, if you have a quantitatively stated standard that has to do with risk, your compliance with it is a calculation that you do, which uses accepted data, accepted models and so forth. Accepted commentorial rules and out comes the numbers and you look at them and you either comply or you don't comply. That's compliance to a quantitative standard.

Now, the question, I think that is being raised here is what is it that should be done to see to it that the utility operator of the nuclear plant is doing things right day today.

MR. HUTT: You send inspectors in.

MR. TEMME: Perhaps. There is a quality assurance activity that is needed. I'm not quite sure how you fit that into the quantitative role. I think it is needed and maybe it's the one good example of a qualitative goal or set of qualitative rules that we ought to bring forward in this workshop.

As a matter of fact, in this stuff that George gave us. In the list of examples of non-quantitative goals, he's got this thing called organizational excellence and spirit which if you read all of his material, means look at the way Admiral Rickover has been doing it. There's a couple of

other related things: qualifications of people. I don't see those things fitting directly under the qualitative risk standard.

CHAIRMAN LAVE: Let me try on this. That is, you have two very different approaches. One approach is to simply sit back, wait for something untoward to happen and say, you've violated the goal.

MR. TEMME: You can't do that.

CHAIRMAN LAVE: Well, you can do anything you want. I mean, you --

MR. TEMME: You can't rationally do that.

CHAIRMAN LAVE: The usual distinction is between design standards and performance standards. And here by a performance standard, I would mean that you could either set your performance standard at the level of 20,000 people being killed and say, tsk, tsk, you've violated our rule or you could set it on the level of how many components fail in your reactor during a year time or something related to safety.

You could have micro events that don't occur once every ten thousand years and you can say to the utility, other than monitoring component failure, we don't care who you hire as reactor operators. We don't care what kind of maintenance you do. That's all up to you. All we care about are the number of itsy-bitsy little failures that we see and if you have more than this, you're dead.

So, that's sort of the extreme of the performance standard. The design standard, the espirit de corp is to go in and say, no, in order to be a reactor operator, you've got to have the following qualifications: You can't be a homosexual and you have to have this license. Or in order to be a reactor operator, you've got to be a member of this stuff. You've got to march to the reactor operator's song --

MS. SHELDON: You sound like -- when you put that standard in. I do sex discrimination cases, too. Preference cases, I should say.

CHAIRMAN LAVE: That is, you can talk about what the training level ought to be. I'll give you a very strong feeling that the NRC ought to keep its hands out of all of training, licensure and all that sort of stuff. Because they're going to muck it up. I think that if Zebroski, acting for the industry, wants to try and define optimal standards and so on, which the government has nothing to do with and the industry likes those and wants to participate, that's just fine with me. But that is really quite different.

I feel very strongly and can give you a number of examples of areas where design standards just get you into trouble time after time after time and if you can possibly measure performance, that your standards ought to be performance standards.

I was really quite convinced by what you and Ed

told me earlier about being able to measure micro events in there. And if you can measure these micro events and you can find the relationship between those micro events and larger safety events, then for god's sake, let's stay with performance.

MR. TEMME: I don't think that has much to do with it -- performance standard. That's my opinion. You go back to the same thing. When you're working to a risk rule, your compliance is just a result of a calculation. It's not necessarily limited to just the design of a plant. If your model recognizes that the way in which the plant is used has an effect on the risk envelope, then you attempt to put that information into your model and it gets a little difficult to do, where at least conceptually you can put it in.

MR. HUTT: Can you monitor that aspect of it?

MR. TEMME: I think that's the question.

MR. HUTT: Because I was going to throw out an analogy --

MR. TEMME: In certain respects, I think you can.

MR. HUTT: What FDA does, for example, in food manufacturing and in monitoring the conduct of toxicology tests, they have good manufacturing and good laboratory practice regulations that are billed on what they call a hazard analysis concept. Inspectors go or company inspectors -- either government or company inspectors go in and work at what they refer to as the critical control points. Both in --

if it's a toxicology test, they make sure that everything is recorded properly, etcetera. They don't care what the results are. Just that it be recorded properly.

In a food manufacturing plant, if it's low acid food, canned food, they go and make sure that the temperatures for the kill were recorded correctly and in the recording, the process has integrity. They are looking for, in short, the integrity of the process in both instances, not for what you might call the details of any kind.

Is that what you say can be done here?

MR. TEMME: Yes, in fact, there already exists -- When you're licensed there is a whole set of observable things that have to happen on a continuous bases for you to keep operating and if you can show somehow a relationship between the occurance and non-occurance of a certain set of these observable things, the relationship between that and the calculated risk number, then you can back out of that a set of these requirements, that must continue to be met.

So, what you do -- part of your compliance is that you meet the calculated risk number, but conditional on certain technical requirements that are on going. In that sense, you can address this.

MR. HUTT: Is there any reason why that isn't adequate?

MR. TEMME: None that I can see. There's a modeling

difficulty.

CHAIRMAN LAVE: Let me ask a question in my least low profile way. As intended to stop beating whoever your sexual preference is going to be.

Does that mean that we're all agreed that we shouldn't be licensing reactor operators.

MS. SHELDON: No.

MR. TEMME: I don't think we agreed to that.

MR. MALSCH: Conceptually, suppose you did a risk or an analysis of a likelihood of a certain kind of accident in order to meet your safety goal or standards. Conceptually, this is what you should do then -- is to go back and see which parts of the design or operation were important to you in reaching those conclusions.

In certain parts of the -- analysis depended upon the operator doing certain things correctly or not correctly. They're exercising a sound judgment. They're not doing something stupid, then necessarily you're relying upon qualifications and the good sense of operators and that in turn would lead you to licensing operators.

MR. HUTT: It could lead you, but not necessarily lead you.

MR. MALSCH: It could.

CHAIRMAN LAVE: But another thing that goes on here generally is that you have in class requirements as distinct

from test requirements. That is that you probably -- I don't know. You probably require the reactor operator have gone through the following course sequence. And I would have thought that all you want to require was this guy be able to perform on your simulator in the way that you deem to be a standard way. That you don't care how he got that skill level.

MR. TEMME: I guess I'm missing the point, maybe. If that's the case, the way you certify that he does perform on the simulator the way he should is to license him.

CHAIRMAN LAVE: That's fine. I'm backing off a little bit, but I'm asking what are the things that at least economists get uptight about is a set of licensing or other requirements that generally are close to unenforceable and B) distract from what the real criteria are. That is, if you want people to be competent then requiring that they've spent a number of hours in class speaking as a professor of long standing, it's the stupidest thing you can do.

People sleep best in class as I can testify. So, what you want is someone to be able to perform in some way after the fact. And my humble sexuality was the other example of this. People start building in requirements that have nothing to do with what it is you want to get to know, you want to get out of people. If you want to license them, okay, you're bringing me around to saying that we want to license them. But, I guess I want a performance standard on licensing.

MR. LIBARKIN: The NRCs licensing procedure requires a number of elements, one of which is some course work and a test and another of which is actual on-hands work in the control room. I don't want to say what my opinion is, but I can offer a rationale for the former and that is that we don't only want the operator to be able to operate well when the plant is doing what you've designed and built it to do. You want to be able to function well in emergencies. By their nature, emergencies are not always forseeable in great detail.

So, you would like him to have some rather fundamental understandings about what can happen and what it would look like to him if it did happen in these machines. And I think everybody's opinion who thinks about it now a days is the fact that it wasn't done very well before. At least, not well enough and we're going to do more of it.

MS. SHELDON: One of the questions is, how much independent judgment is exercised just on an ordinary day to day bases. If all you're asking the individual to do is to follow a very explicit set of instructions and do the same thing every day and you're not leaving anything up to his or her judgment or intelligence or anything else, then it's fine.

But, I think -- We don't have the right person on the panel in this issue on this thing. Tod McCort among other people has written a good bit about this. The need for quality managers and operators in the system. Having this

espirit, this commitment to excellence -- a vigilance over the operation day to day with an eye toward doing everything that is possible to make sure that it functions properly and that accidents are avoided.

I simply don't know. It was my impression that there was still in the day to day operation of a plant a fair amount of opportunity for judgment. In that case, I think that you ought to make sure that you have the very best people running these facilities that you can.

That's the problem. There are a limited number of very best and as you increase the system, you increase the probability that you won't have the very best.

DR. BRADBURN: I just want to make a general comment about -- There is a generic problem of organizational management. In systems where the critical things you're really worried about don't happen very often and don't want to happen very often. It is very hard to keep morale up and keep people functioning at the level that you want them to, because most of the time, things aren't happening and everything has been designed so that it is not going to happen very often and so forth.

That's a species of problem which usually is not very well understood in all things about management and organizations. This happens to be one that is particularly difficult. What you really are sort of asking people is to

quote, give their best and all this sort of thing for something that may never happen.

MR. TEMME: And be bored the rest of the time.

DR. BRADBURN: That's right. You need a certain level of getting people aware of the danger in a way. You're getting people who are extremely overqualified at one level, because they're all qualified to do something that they may never have to do and indeed in the ideal case, they won't ever have to do.

MR. TEMME: Maybe we should consult the Strategic Air Command. They sort of have that problem.

DR. BRADBURN: The place where this first came up was really where people monitor Dew Line type things and so forth, because they're watching for something that hardly ever happens or may never happen.

When it does happen, then a whole set of complicated things have to -- ninety-nine point nine percent of the time when it does, they know it's wrong. The radar starts doing what ever it is doing and you know that it is phony.

But, I guess, the analogy would be with pilots where you put them on simulators and all hell breaks loose from the simulator. A wing falls off and all this sort of thing. You train these guys and if you thought of the right things on the simulator, then that person is ready to go.

That's the problem of how you train them and how

you certify them that they're the right kind of people for when the moment occurs. That's a different problem from the day to day handling of kind of boredom, essentially of it. How do you keep them from going to sleep.

DR. ZEBROSKI: One of the most creative suggestions that is kicking around about it now and which is being played with a little bit is to -- particularly now that they require a degreed person, the shift technical advisor. You can guarantee that he is going to be bored most of time. So, one of the creative suggestions is to give him what amounts to a TV game in which you play over a variety of these scenarios periodically and you can make a wide -- One of the real hangups of what Karin was saying is that the definition of --

MR. HUTT: In the remainder of his time, he could write papers on risk assessment.

DR. ZEBROSKI: In the remainder of his time, he should be taking account of this operating experience stuff that is falling in from every where, which is a big volume of data. The hangup on the judgment mode is that the operators are very heavily trained to do everything in the skill mode rather than the cognitive mode and in fact, there are great penalties for improvising when you shouldn't. So, one of the things that is very weak in the system now is sort of what triggers a transition from a skill mode where you say, I

recognize -- procedure 28 verses saying that this is not one of the situations in the rule book and I start using judgment. That is not in the system now. There's an attempt to remedy it with something called ATOGS Abnormal Transcient Operating Guidelines, but even there, the -- what is the trigger for transition from one mode of operation to the cognitive mode is not very well defined.

The big simulators are a weak tool in this respect, because they don't handle the severe accidents very well. They don't carry them far enough. They always assume, just as they did before TMI and still today, assume that you successfully recover before it takes it out to the horrible end. So, the TV game doesn't have that limitation. You can play the parameters out to extreme conditions and test a persons responses or comprehension of the situation far beyond what a simulator does.

I think this will come to be. It's an idea that sprung up a number of places. We're in the middle of it right here in San Jose at SNUFF simulator and DOE has a project to do it next year. I think NRC is talking about it for the '83 budget.

DR. EISENBUD: You can score the operators and their tests and rate them on a national bases.

DR. ZEBROSKI: I think it's more analogous to requalification on instruments that the pilots should have.

The simulator trains them in the first place, but most pilots will tell you that you should avoid getting into instrument flight conditions, certainly in a private aircraft, unless you've requalified within the last month or two on instruments. There's a roughly ten hour commitment of time every couple of months to do that. I think that this is the analogous thing to that situation, where again you're coping with relatively inprobable events.

The pilots say this all the time. The pilot thing is ninety-nine point nine percent sheer boredom and one tenth percent sheer terror.

DR. EISENBUD: One of the main developments of the TMI accident in relation to the question of what the relevant roles of industry and government should be. I think that as long as -- says that the accident demonstrated the enormous economic consequences of an accident that may not have severe health and safety effects. I can see where utility executives might not really believe that you can have a full melt down and that you can put out large quantitites of radioactive materials. They don't really believe that the reactor could be a hazard to the public. So, they'll be willing to sit back and let the industry regulate them and just do what industry says and live with it.

But if they know that even before you create a hazard with the public, you create disasters -- economic

costs of disasterous consequences, then there's an internal motivation to do something about it. I think that's a big difference. There are many things now that industry will self regulate where previously, I don't think they would. I guess that's why some of the things that Ed is interested in has developed. would you agree with that, Ed?

DR. ZEBROSKI: Yes, there's still a fundamental perception conflict though which puts us much closer to NRC in view point than most utilities. That is when we see a troublesome situation, we intuitively multiply it by a hundred. There are a hundred reactors and the fact that it may be a very low probability on a per reactor bases still makes it something you worry about on a national bases.

The local guy in his hierarchy of priorities, that might seem so low in likelihood, he may say, I've got other fish to fry. Don't bother me with your worries. So, we have an educational process in many cases. It does very much depend on the perception of the chief executives and the executive vice presidents. If they're not sensitive to this multiplier effect then the people down the line always say we don't need help, we can do it ourselves and the chief executive will go that way. If he's strong, he will say, I can't afford that risk. He's aware of the risk and the incentive to take this more pessimistic view and you make remedies which on a local bases, you wouldn't have made.

History or recommendations made by NRC and vendors which don't have the force of law is really very scattered. The implementation, I think Dale Bridenbaugh was involved in one campaign, I remember, which after two years of strenuous effort had something like ten percent implementation, even though it was strongly recommended by General Electric.

That's perhaps an extreme case, but the implementation short of regulation is slow partly because people say their so bogged down with a large number of regulations which have relatively small consequence and a large amount of paper work. So, the other perception, a very real cultural problem, is that the manpower requirements to meet the paper mill have increased so dramatically on the nuclear side that there's a real jealousy a.d a cultural conflict between the fossil part of the utilities and the nuclear part.

How come those nuclear boys are getting all of these staff positions and all of these secretaries and all of these filing clerks and so on,which we don't have in our coal plant and why should they get all of these luxuries.

MR. TEMME: Just be patient. Their time is coming.

CHAIRMAN LAVE: Why is it that one needs to have these skilled operators all on site. That is, isn't there some way of centralizing, so that instead of having ninety-nine percent boredom, you might have only eighty-five percent boredom. That is somebody could be responsible for simultan-

eously monitoring five reactors?

DR. ZEBROSKI: That question has been raised when you have four units on the site, for example. Do you need four shift technical advisors and so far, I think the answer is that you do.

CHAIRMAN LAVE: Is there a good reason?

DR. ZEBROSKI: I don't think so,because there are very few events you can postulate for which times of the order of fifteen or thirty minutes are very critical, so the French take the position that if the man is generally -- They have had until this year -- The French have always had several degree level engineers within what they say within ten or fifteen areas of the plant. Which is to say that he lives in the immediate vacinity. He's on-call at all hours. They felt that that was sufficient coverage. I think that probably it is. I don't really see too much of a deficiency on that.

Another thing that is coming along is something that is called a technical support center where you basically get the key information by remote display without having to go to the control room. That gives another element of possibility and most plants will have this in about a year. The buildings are built and the wires are being pulled and so. So, that's one.

CHAIRMAN LAVE: Aside from the manpower problem,

that is, how many people can you have that are technically qualified? It seems to me that this is the way of getting at Norman's problem. Where if you're in charge of ten plants, then you can expect that one of these events will occur.

Not the total disaster, but things that will require your skill level fairly often.

DR. ZEBROSKI: I think there's another incentive for this and we preach about this all the time. That you can have a response to a troublesome event which is totally adequate from a safety standpoint, but which is far from optimum with respect to protecting the equipment or getting the amount of outage. So, there's a financial incentive of doing a lot better. I think in the long haul, public safety will turn out to be one of the weaker criterion for good plant operation.

CHAIRMAN LAVE: I thought that we had lots of burning discussions on this implementation issue. We don't seem to. Is it the hour?

MR. BRIDENBAUGH: I have one question that is maybe implementation, but I really think it relates back to item number one on your list. Let me ask it.

Have we really talked about what the scope of application of this safety goal is in terms of how much of the nuclear plant is involved? The reason I ask this is that most of our conversation today has been talking about or

looking at nuclear plant and the fence around it and nothing in the way of waste disposal, fuel supply, etcetera. I think we've brushed over that pretty much.

MR. LIBARKIN: The ground rules that were set up for the exercise -- while being cognizant of the fact that that had to be done at some point to try to do this safety goal thing, whatever that is, with respect to the reactor, itself, the power plant.

MR. BRIDENBAUGH: We threw those out the first five minutes. I recognize that, but it seems to me that we should make some decision or decide not to decide or decide to disagree on that, before we all finish up here.

CHAIRMAN LAVE: I guess, in my usual compulsive mode, I wonder whether we might finish this stuff that we absolutely need to finish, which I don't think will take a lot longer and then there are a number of issues, which you have time, then, to get at.

Can I propose that as a mode?

MR. BRIDENBAUGH: Sure.

DR. ZEBROSKI: Point of order, if I might.

While your summary this morning was elegant, it left out many things which I thought the panel had said, which were also significant. At least for a couple of the items, I've jotted down a few sentences of things which I think were also important, which were ommitted. I wonder if we shouldn't

consider that process for the whole -- I was planning to just give you a couple of these things in lieu of appointing a subcommittee to work with you on it.

MR. HUTT: We were going to take a vote as to whether to let you do it tomorrow morning or not.

CHAIRMAN LAVE: The process is the following. First of all, I had a set of notes. I did not want to make those slides be a thousand words. And so, there are things in my notes that I simply didn't do. I already spoke twenty minutes this morning instead of ten and was happy that nobody was throwing bricks at me. But the process is the following:

First of all, I would be delighted to get comments from each of you about those things in those set of slides that should be there and aren't. Secondly, after this whole thing is over, since you won't have a chance to do that tomorrow -- Well, maybe you will have a chance. That is, you can jot down some notes during the report and discussion and give some further things to me. But the process is going to be that the transcript from this stuff will provide the bases of the kind of a first draft and then Ralph and I are going to start massaging that and getting a more comprehensive report out of it.

When we have something that the two of us are satisfied with, then you all see it and have a chance to try it again and tell us what ought to be there. Needless to say,

it is more efficent and so on if we have your input before the first draft. I mean I hate to start at that point incorporating new things and all that kind of thing. But, you'll still have a chance along the way. Then, finally, let me say in my own defence that this is not a report that any of you will quote, sign, all right?

That is, that it is my report. So, we don't have any minority reports. We don't have anybody who refuses to sign.

MR. HUTT: We won't have any fun at all. You've just destroyed half of our enjoyment.

CHAIRMAN LAVE: Having said that, let me say that I am anxious to get everybody's input as completely as possible to reflect disagreements and all that kind of stuff, but I had an unfortunate previous exercise where a prominent person said, I will not sign this. I said, nobody asked you to.

MR. BRIDENBAUGH: Well, I wouldn't sign it, if I could.

DR. EISENBUD: Let's just massage that one a little bit, because if there is going to be a report to the NRC, which will have in the report an appendix concerning who it was that attended the conference -- the composition that the panels. I think that by implication, there may be a suggestion that we do as individuals endorse what is going

into the report and maybe there ought to be some discussion tomorrow as to just what the mechanism is going to be to transmit the apparent concensus to where ever it has to go.

MR. HUTT: Let me call for a suggestion. The Office of Technology Assessment deals with this exactly the same way that you undoubtedly know, Lester. I've served on many of their committees and every report up front which lists the members of the panel who not only do not sign, but sometimes, they're not consulted on what the final report says, says exactly that. That these people were brought in. Their view were obtained and they had no further responsibility whatever for the report and I think this will undoubtedly say the same thing.

Besides, if anybody really wants, they can read this transcript and find out what we said. I didn't see any people rushing over to buy copies of the transcript.

THE REPORTER: That's not my fault.

CHAIRMAN LAVE: That sounds like a good sign to announce that we ought to take a ten minute break here.

(A brief recess)

CHAIRMAN LAVE: We are on the last item of the agenda that we drew up the first day. Mainly, the process for involving Congress, public, industry and universities, etcetera. When we complete that item, which I don't think will take a terribly long period of time, then I think we ought

to sort of throw open the floor to trying to sort of sketch out a new agenda of items that should have been covered and weren't or items that somebody feels even though we're sure that they'll take place in a different workshop at a different time ought to be mentioned and structured as best one can.

At this point, assuming that we'll go to five-thirty again, we have three hours at our disposal of discussion time.

MS. SHELDON: It's four o'clock.

CHAIRMAN LAVE: I understand. An hour and a half tonight and then an hour and a half tomorrow morning.

MR. HUTT: How do we start tomorrow morning?

CHAIRMAN LAVE: Tomorrow morning we come here at eight o'clock, go on to nine-thirty, take our break, go into the plenary session and then wind up.

George?

MR. SEGE: Tomorrow is devoted entirely to the plenary session.

CHAIRMAN LAVE: Okay, I'm sorry.

Let me ask the question. Item six that we had on the original agenda was the process for involving Congress, public, industry and universities, etcetera. I think that is an important topic, but I'm happy to be advised if people don't think so.

DR. ZEBROSKI: I can offer an opinion on it, because it has been discussed --

CHAIRMAN LAVE: Without talking to the substance. Just agenda stuff, only, right now.

DR. ZEBROSKI: It goes to the question of what the role of the qualitative goals is.

MR. HUTT: I think that we've got to keep it on the agenda.

DR. EISENBUD: Some of you may have the sense, because I'm not a particularly subtle person. I've been a little disturbed about the lack of orientation of these several hours of discussions we've had these two days and the point that I made over and over again is that I thought that we have to have a better understanding of the defacto system of regulation and goal definition that has evolved over these several decades and George Sege who I just told that to, told me -- George, if I misquote you, please say so.

That the NRC has not had goals up to now and this is greatly at variance with my understanding.

MR. TEMME: I think George wants to make a comment.

MR. HUTT: Come forward and be recognized.

MR. SEGE: The Reporter is signaling that I should come closer to the microphone.

I want to add something to was characterized by your remark at the intermission. The NRC has in fact had safety goals which are embodied in the steel and concrete operational practices of plants. What existed up to this point and what

the effort is now is systematic and explicit articulation of the goal.

CHAIRMAN LAVE: I'm sorry. Let me also not be subtle. I think that we have spent a lot of time talking about this so far. That is certainly both the point that we should do it and have talked about the history of it and I think that that doesn't preclude spending more time, but I guess that I think that Item six on the agenda has had no time so far and that it would be quite remiss to go and not do that.

I'm perfectly prepared to come back to this and I'm prepared to go on beyond five-thirty to come back to it as need be, but I guess right now, I think that if we seem to have some general agreement that we ought to do item six on the agenda and then we can come back.

DR. ZEBROSKI: I suggest out of kindness to physiology that we limit six to about a half an hour.

CHAIRMAN LAVE: Okay, delighted.

MR. LIBARKIN: It may be possible to eliminate one of the aspects of that. I think that with respect to the involvement of the Congress, if anyone contemplates -- again, I'm not an expert in Congressional relations, but I have observed the Congress in its relationship to first the AEC and now the NRC for some years.

If anyone expects that the Commission will formulate some proposed set of goals of whatever nature and then go to

the Congress and say,here they are Congress, now you please tell us whether they're okay or not, they're going to be disappointed.

I don't expect nor should anyone expect Congress to function that way. What they do is, they sort of let you go along and give you enough rope and then at the next budget process either the authorization or the appropriation, they drag you in and want to know why you've been so stupid to have done what you did.

I think that's the way the Congress has historically interacted with the Commission and I presume the same with other Federal agencies, but maybe not.

It would be nice in an ideal world to have a process whereby they wrote these statutes with all of these fairly vague but good sounding exhortations in them and then the agencies went away and came up with some proposed set of implementing rules and then could go back to the Congress and say, here Congress, tell us if these are what you had in mind and if not, where we should change them. At least with my observation, it doesn't work that way.

The Congress will involve itself to the extent that it wants to. That's about as much as one can expect, I think.

MR. BRIDENBAUGH: Isn't there, in this particular case, though, didn't the Congress request specifically that the NRC --

MR. LIBARKIN: The Senate did. The House didn't. I don't know where it's going to come out. I guess I could go further and say that even if that particular authorization bill did come out with a requirement that the Agency establish a safety goal, I still don't believe that having prompted a proposal, the Agency could reasonably expect the Congress to tell us what it thought of --

MR. BRIDENBAUGH: I agree. There is absolutely no way of predicting what they might do with it, if anything.

MR. LIBARKIN: I'm predicing that they will not say whether they like it or not until they find -- they feel the need to. Maybe I'm being unfair to them. I've never observed them to do that.

MR. TEMME: I don't think it's unfair. That's our process. The question here is is it a felicitous one, I guess.

MR. HUTT: Maybe it's a lot better. After all it allows the Agency to proceed in what it perceives to be the best way.

MR. LIBARKIN: I'm suggesting that we don't have to concern ourselves with worrying to much about how we should structure this thing so as to involve the Congress. They will or they won't as they choose.

CHAIRMAN LAVE: The point is well taken. Can we get to the other part of the list, I guess that I had written up there: public, industry, university experts and so on.

MR. DERBY: Let me explain how I look at the problem and what role these people have. I think it was confusing when I was talking before about the subjective parts of detailing the decision roles. But, my question is how one comes up with a list of relevant social attributes that would be used in the decision rules and in the calcutions? And we keep up with a list and in distributional effects.

We said, if we had to put all of these things in the decision rule, the whole process would grind to a halt. Well, some of these are picked off and aggregations are made that draw distinctions in disputes. The one you and I were talking about with deaths a life expectancy. I think we both agree that we don't want to kill anybody, but aside from how one starts from here -- how one wants to measure that in terms of concrete thickness is a serious issue, if you miss something in your calculational scheme.

I see these groups of people somehow creating a list and selecting exactly what will be used in the decision rules as the calculational -- Is it important a decision to calculate life expectancy or a number of deaths? Is that a big deal to people? Yes or No.

MR. TEMME: Which groups of people do you see doing this?

MR. DERBY: Who ever feels that they are a stakeholder. If there's a process,a felicitous process, that is

their role, is what I'm saying. It's not their role to be a technical person, because a technical person -- that's their role. It's their role to say something and --

MS. SHELDON: The question is, what is a felicitous process? How and where do they do that? What kind of a forum? Adversarial, non-adversarial? Rule making?

MR. DERBY: My point is, this is what they're trying to do to process to meet this -- are you going to have an adversarial process that challenges science and the natural laws that seem to come out of the engineering and scientific fields. Is that an adversarial process? Does that make sense? I don't know. I don't think it does.

I don't see that being the role of involving the public and the universities and things like that in the formulation of the qualitative goals and the quantiative decision rules. I'm just saying that coming up with relevant social attributes, that process has this goal. I don't see the goal as being explained. I'm sorry.

CHAIRMAN LAVE: Let me take a page out of Merrill's book. We might start this off by saying, how about a quick characterization of the current process by which these actors are involved. Not Congress, but public, industry, universities, scientists and so on and what's wrong with that? How about that as way of getting --

DR. ZEBROSKI: I think the process goes backwards

and forwards. I think we need to talk about the process a little bit before we say who the actors are. It seems to me that the defacto safety goal of the NRC was to establish a large number of procedural rules, guidelines, regulations and then with the WASH 1400 kind of process, saying how good has the result been and in principle had you done the process in reverse, you could have stated a target risk envelope and find all the rules and regulation that got you there.

I happen to believe that the forward way of stating a general safety goal and then deriving from that a vast apparatus of implementation is basically impossible. I think the safety goal, basically, can only be a codification of your best judgment of what a given procedural and regulatory appartus is achieving. So, in that sense, it's not a statement to Congress to vote on whether pi is 3.0 or 3.1,but it's a statement to Congress that certain kinds of judgment says that the way the process is operating is equivalent to striving about this quality.

I think the real issue for the public involvement is the quality and logic of the processes relating this body of practice to the estimated consequences of the body of practice and that has on the one hand some statistical evidence. On the other hand some analytical evidence and then overlaying it some judgments of the levels of uncertainty, which are there.

But, I'm really very doubtful that saying I'm shoo-

ting for ten to the minus fifth deaths per person, yer per plant year in the vacinity of the power plant can of itself lead to the enormous body of practice that's required to make that plausible. You really have to do it the other way, which is the codification of a great many engineering judgments. A great deal of testing and the accumulation of the operating experience that leads to the plausibility of that conclusion.

I think there's an essential unreality in the assumption that the process works in reverse. That if you set an ideal number and then all the things make it true, fall into place.

CHAIRMAN LAVE: Can I enumerate three reasons for having -- three stakes the public has -- things the public ought to be testifying about in a hearing or raising in a hearing. One is what attributes ought to be considered among this laundry list we have and how they ought to be considered.

The second is, are the quantitative goals commensurate with qualitative goals. That is, do your quantitative goals, ten to the minus sixth or whatever it is, correspond to undue risk. That is, let's hear from people about that. Those, after all, are matters of opinion.

And I guess, the third one would be some notion of whether the goals are, in fact, being met somehow. Let's hear about that again. They're legitimate public concerns in that. And again, I'm not talking about terribly technical

levels on any of these things. I'm talking about general issues being raised where it seems that the public has a concern in each one of these.

MR. DERBY: In the third area, you mean as somehow a vote of confidence that the NRC is doing it's job? Is that what you're --

CHAIRMAN LAVE: Let me put it the following way. Suppose that you get a group someplace observes that their reactor -- something is going wrong with their reactor, an awful lot of the time in some laymen's perception. It seems to me that they have a legitimate role in inquiring as to whether the NRC knows about this. If it does, then what's going on in kind of laymen's terms.

That is, it's a public audit function. Is this agency doing its job?

And again, I don't mean by that something terribly complicated and technical, but if you took a look at air pollution control enforcement, you would see that an awful lot of that gets done by people looking out the window and saying,there's smoke coming out of that stake and there shouldn't be and calling up and doing something about it.

Are those three reasonable? Do we have other functions to add?

MR. TEMME: I think there is also sort of, entirely aside of the measurement of actual events. There are some

things that can be done judgmentally on whether the process is adequate. For example, if a large number of the regulations and a large part of the follow up is being protested by some segment of both industry and the intervenors, as being relatively trivial items and which are gumming up the system so it performs less well on the more important items, then I think there's a question of adequacy of process. Again, the Kemeny and Rigoven reports came down with chapter and verse of a great deal on that.

So, it seems to me that there is a role for the public and ideally for -- maybe not ideally, but at least a legitimate role in terms of other agency of an oversight committee of Congress or something like ENSOC to perform this function.

Is your process adequate? Are you managing it? And that comes to my suggestion of an integration statement being part of the qualitative safety goal that unless you have the vigilance to keep the process healthy at all times, then goal, itself, is meaningless.

I think that is a role of the public and the legislature and oversight bodies of various kinds.

MR. BRIDENBAUH: When you say that's the role of the public, how would that be accomplished?

MR. TEMME: If we had faith in our institutions, we would say, I think classically in other cases, this has been

done by an oversight committee of Congress looking into a particular agency and questioning it and sharpening its activity. I think the President's commissions, the two of them, the Kemeny and now Babbitts have some of that function.

In other words, it seems to me that you need people of high repute that are not beholden to either beaurocracy or to industry looking at the system and saying does it appear adequate given that you take a little more time to be informed than the average person of the public will be able to do.

I think that it's a mistake that Congress hasn't done this much more. They divided up the responsibility in '74 to such a fragmented way that I think that process from Congress essentially has failed to operate for a number of years. I think that the description here is that they call you up and swear at you for awhile, but that's really an oversight function.

MR. LIBARKIN: I think that I would have another suggestion for things that the public might be interested in in the process of formulating the set of goals. It seems to me that it's inherent in everything that I've heard about, the notion that having decided and achieved concensus, we possibly could do, that there is an appropriate goal or set of goals toward which we all should strive that having established that, there's kind of an equity that's been achieved

among the whole population. That is that it's vital effect that everybody benefits to the extent that everybody benefits. There are a set of people who are uniquely burdened by the residual risk and that the assumption that is built in to all the discussion is that that residual risk is small enough that nevertheless those people are in some state of equity with everyone else.

I would think that might warrant some discussion from the public. Is that, in fact, their perception? Is their perception important? Is there some mechanism by which their perception, if it's different from that, could be moved more toward a more comfortable feeling? All of those notions I think would be useful to consider.

CHAIRMAN LAVE: I really consider that as being part of the first one. Which attributes ought to be considered and whether they're being met.

MR. DERBY: Yes, the distributional effects is I think what you're talking about.

MR. LIBARKIN: No, I don't think so. I think that it is different in kind. You can consider distributional effects and decide that if we get the residual risk down below some level, they're not important. You can do that as the NRC and you can do that as the Congress and you can do that as the body of experts and you can even do that as the spokesmen for the active public or something like that?

DR. BRADBURN: The mobilized public.

MR. LIBARKIN: In fact, is that the perception of the three people who are within one mile of that plant? Is that perception important? Does anybody care? I don't know what the answer are, but somehow I don't consider it part of the whole other set of considerations first.

CHAIRMAN LAVE: I think that in the process, we want to make sure that they have a quorum for being heard and in our political system, that's about all we can do. If they're heard and we think their case is meritorious, then we do something about it. If we don't think their case is meritorious, then we don't do something about it, but I guess that the process at a minimum ought to make sure that they're heard.

I think that a process that required unanimity would be one that guaranteed that nothing would ever be done.

MR. DERBY: I have a question about felicitous process. I see a different process where there's a great public gather felicitous to all concerned for a formulation of what the goal and it's attendant decision rules will be and whether or not those decision rules involve the public on the scale that setting up the regulation and the decision rules involved is another question. It's another issue.

We certainly do not want to eliminate or cannot eliminate intervenor action on the licensing of a plant, but

are we talking about here felicitous process of somehow adjusting what we have today to something different after we formulate this qualitative goal and decision rules? That's the question. I don't have any answers.

Is that an issue that we ought to discuss?

CHAIRMAN LAVE: Yes, I think that's an issue. I presume that at the back of the NRC's minds, we're talking about establishing quantitative safety goals or doing something to enhance the current process. It seems to me that if you establish quantitative safety goals and didn't provide some mechanism for these other groups to be involved in them you would not have in anyway enhanced the process.

DR. EISENBUD: Lester, I don't see anything in the guidelines that we got that suggest that we discuss this subject. George was asking me whether we were sticking to the guidelines and how can we justify when I look at the workshop objectives -- there are three of them. I don't think that our interaction with Congress or the institutions and the public is involved in any of these three questions that we are suppose to address and the relative short period of time of two and a half days.

CHAIRMAN LAVE: I guess I'm reflecting the discussions that I heard before the NRC when the Commissioners were considering this whole program. Now, I'll certainly admit that not everything was done at every single session. But an

issue that loomed large in all that was the process by which other groups get involved in commenting upon these new goals that you're going to put in and how they all get implemented.

DR. EISENBUD: We have a very serious semantic problem, which I have detected in this discussion and even more so in the discussion just outside. To me at any rate, the goal is the stratetic objective.

And then there's an enormous amount of tactical work of planning that has to be done. George, said that his goals now and he says this in a disparging way, are incorporated in the steel and concrete of these reactors. I say, nonesence. That is a tactical means by which the goals that they have are being achieved.

If we leave here tomorrow without an understanding of what the goals are at the present time and what needs to be done to strengthen these goals, then this conference will fail. I think that is the most important thing that we have to discuss.

If we go around the table and I find that -- I will even step out of the room while you vote, then I'm wrong. You can do it the way you want to do it. I don't see how we can leave this conference without a clear understanding of what the -- what needs to be done about the goals of the NRC and that can't be decided without reference to their present goals, if there are any.

MR. HUTT: Well, let me ask the question that I asked before, Merrill, of you. We're talking now about hopefully establishing some quantified goals. In the past, as I understand it, there have been some degree of quantification that has not been as quantified as suggested in this yellow covered book, the name of which constantly escapes me. I can't remember that number. Does one have to go through all the building and concrete examples that -- to elucidate from that sort of on an empirical bases, what in fact the quantified goal has been on ad hoc bases in every decision on 100 existing plants or how many there may be. Is that what you're suggesting?

DR. EISENBUD: No. Because you asked the question like that, it seems all the more reason that we should discuss it.

MR.HUTT: We've discussed it for a day and a half.

DR. EISENBUD: No we haven't.

MR. HUTT: I think we have. I asked the question several times.

DR. EISENBUD: But everytime I get started, I'm told that we've spent enough time on that.

Now, the basic goal is established by the Congress.

MR. HUTT: We've all agreed that it's useless.

DR. EISENBUD: That's the policy. Now to implement that goal, the series of government agencies starting with

the AEC and running up through the NRC, did a number of things. The routine operations, which were the easiest, they adopted basically, the guidelines written by the ICRP, the International Commission on Radiation Protection and our national commission with ALARA cranked in or the concept of ALARA comes from the ICRP.

MR. HUTT: And ALARA is equally -- it doesn't make any difference whether you say ALARA or no undue risk. There is no difference. It's hopefully unhelpful in making decisions.

DR. EISENBUD: But the problem was that as the reactors came to be built than it was recognized that the criteria about which the Congressional goal could be achieved did not apply to accidents. So there was a debate that went on for a considerable period of time among the advisory committees and I guess there must have been hearings, though I don't remember them, in which Parts 50 and 100 were adopted to deal with the accident condition and what may have seemed like arbitrary assumptions of what were written into them. Mainly that the reactor should be designed so that certain doses to the public would not be exceeded.

I don't remember any longer what those numbers are, because I had very little to do with this. I think it was 50 RADS, is it?

Those doses can be expressed through the risk coefficents in cancers.

This is what the AEC and the NRC said that they were willing to accept. Then the question came up -- two questions came up. One question was -- Oh, I'm sorry. Then having done that, then the bricks and mortar and steel were put in place with instrumentations required to make sure that everything was working right in order to achieve the dose levels.

Then, two questions came up.

MR. HUTT: They did not deal with accidents, you said?

DR. EISENBUD: Yes, they did deal with accidents.

MR. TEMME: With a certain class of accidents and that is where the problem arises. They dealt with design based accidents.

DR. EISENBUD: That's right. Then the next question comes up. Two questions. One is, can an accident happen that is worse than the so-called design bases accident and if so, what is the probability that it will happen. I guess it was this WASH 1400 was written to quantitate that. Despite the fact that executive summary was thrown out, it probably should never have been written in that -- time.

It is a document that lends itself to analysis. It even, if I'm not right, Ed, didn't TMI type of accident was in the expected frequency?

DR. ZEBROSKI: About 20 to 30 percent confidence

level.

DR. EISENBUD: It even said that this would happen I think, in 400 reactor years or something like that.

I think this is the structure that has evolved over the years and it does constitute a method of achieving certain goals that were enunicated. Maybe WASH 1400 ought to be done. Maybe somebody has some ideas --

MR. HUTT: WASH 1400 is not set out in guidelines or regulations at the moment, correct?

DR. EISENBUD: No, it just gives probabilities.

Now, the guidelines and regulations and this is another thing that should be understood. I haven't seen them and I suppose if we stack them up on the table, how many feet of bookshelves do we have in NUREGS?

MR. HUTT: I don't understand what your objection is, if what this is an attempt to do is to take the information in that study, WASH 1400, and that feet of books and to draw out of it generalized principles in quantitative form and state those as quantified goals. I'm not sure that I understand to this moment, Merrill, where you object to that.

I would think that your line of reasoning would lead you to conclude that that's a wise thing to do.

DR. EISENBUD: No, I think that what has to be decided is whether the accidents of certain severity at a projected frequency are acceptable or not acceptable. Does

this mean that you're willing to accept greater risk or less risk? And that's what is covered in this document that wasn't even mentioned.

MR. HUTT: That's what we're talking about, isn't it?

DR. EISENBUD: The letter that went with this report -- I think would make a good interesting paper. I don't think we discussed this.

MR. HUTT: I have a different perception, because since I've been working on this, this is the only document that I've read, so I certainly have been discussing it.

MR. LIBARKIN: Maybe I can help some. One of the perceived problems at least within some segments of NRC, is that, in fact, what you say is correct. That if one goes through the existing regulations they lead eventually to some releases and potential exposures which are in some sense considered to be acceptable. But what they don't touch on is there some frequency of challenge which is acceptable?

You build all kinds of things to make sure that having assumed certain things happen and certain releases occur, that they are not exceeded and then you build other things to make sure that you don't do that too often, but what is the acceptable frequency of challenge? That's not touched on anywhere. That's not illuminated any place and it certainly intuitively, at least, is important.

It leads to a situation in which when something occurs or is learned that suggests that the frequency of challenge is for some plant or class of plants somewhat larger than you thought it might be, what do you do with that fact? You don't know how to perceive. You have an ad hoc decision to make which is terribly disruptive and unsettling to everyone, because there is no guidance that the regulator has. It's true that having made an assumption about certain things that one will allow to occur, you can reach a conclusion based on that assumption and the exposures that result about certain probabilities of health effects, but it's the other end of it that is not really illuminated. It's all deterministic. It's all based on -- we assume that this happens and after that happens, certain other things happen. But it's that first step about which there is little guidance.

DR. EISENBUD: Is the problem that you just raised related to the question of establishing safety goals?

MR. LIBARKIN: I think so.

MR. HUTT: I thought that what and let's go to the document that you referred to -- I thought that that cover letter was talking explicitly about attempting to set quantified limits on risk.

MR. TEMME: It does.

MR. HUTT: Am I incorrect?

DR. EISENBUD: And then they go on to do it.

MR. HUTT: I'm sorry, then. I thought you were objecting to that.

DR. EISENBUD: Objecting to this? No.

MR. BRIDENBAUGH: The other document that we received was the final draft of NUREG 0764, which of course summarizes that one and brings in a little more information, but it's basically the same information.

DR. EISENBUD: Yes, accept that I would like to hear some discussion as to why it was that I read that and didn't get anything out of it and why should anybody be sending a document up to the commission unless -- that I read on the airplane and couldn't even write down what I learned out of it.

CHAIRMAN LAVE: Merrill, in some sense, that's your position as a citizen whose tax money is at stake.

MR. HUTT: That's endemic to government anyway. I don't think we can resolve that.

DR. EISENBUD: We were asked specifically to discuss these two documents. That document is going up to the Commission as I understand it or maybe it's already gone up.

MR. LIBARKIN: It's gone.

MS. MAXEY: Let me ask? Is there any other regulatory agency, for example, the FDA that is going through this same exercise in parallel with NRC?

MR. HUTT: Margaret, I don't think it would be fair

to say that it is quite in parallel. What FDA has been doing as I have described is similar, but not exactly in parallel, for example.

DR. MAXEY: It is seeking safety goals on any bases of experience --

MR. HUTT: It is definitely seeking safety goals and it is quantifying them and it is doing exactly what the NRC is doing here. Looking back at past ad hoc decisions, trying to extract from them generalizable principles, quantifying them and then adopting them as guidelines, regulations, goals, it doesn't make any difference.

DR. MAXEY: What function has the setting of qualitative safety goals in that exercise?

MR. HUTT: Lester, I would have to say that they're moving away from qualitative to quantitative, because qualitative, again, just lead to ad hoc inconsistent decisions.

DR. MAXEY: Then why has this panel convened?

MR. HUTT: That's a good question and I don't know enough to provide any help on the answer as to whether all agencies are sufficiently similar that they can do the same kind of thing. The question is, for example, here, can accidents be sufficiently quantified in probabilistic terms and I think we've reached sort of a concensus that we hope so, but there is going to be a process of working our way through it. It's not going to be as easy. And one can argue,

maybe, it's a little bit easier for FDA. One can argue that it's a little less easy. I don't know. Let's not try to get into that.

It's not particularly easy for, I would say, for any government agency to do. The FDA sits there to this day with no idea of what the, as I said, lower end of the dose response curve is for any chemical that it regulates. It knows that it is somewhere between, quite literally, zero and one and that's quite a big difference.

DR. ZEBROSKI: I think that we're back into the semantics of qualitative safety goals. If you define two possible -- First of all, let's assume that the question really is what is the role of qualitative safety goals as a complement to quantitative, which is -- what the conference is about -- an attempt to quantify. And the question really is, what is the role of the qualitative safety goal related to the quantitative one.

I see two different definistions of qualitative and two different roles. One role is that given that I have a quantitative safety goal, I need to qualitatively state a great many things about the code, standards and environment in which the implementation of that goal would have a chance of being meaningful. And then having described that qualitatively, then I think the other role of the qualitative statement is to say, given that I have this quantitative goal and

this qualitative apparatus to implement, I can state a qualitative summary what I think I will achieve which is understandable to the legislator or layman, which is a measure of what the agency is trying to achieve with it's quantitative goal.

I personally to believe that it is factualist to assume that you can derive it the other way around. That you state a vague qualitative goal and derive a quantitative one and all the apparatus to support it from it. Except in the -- You can do it, but it is done intuitively by a large number of ad hoc decisions. And then having that body of law, if you will, you work backwards and say qualitatively, here's what I think that I have achieved.

MR. HUTT: And then see whether it makes any sense. Some times it does and sometimes it doesn't and you also find that you have a lot of those famous outliers that made no sense whatever and that you're ashamed of. Sassafras tea.

MR. DERBY: It's essentially -- you've got to start from the top down and go as far as your experience can take you and you've got to start from the bottom up and go as far as that goes and try to cover this gap. That's all.

MR. HUTT: I'd like to go back to Merrill's question, because I still don't understand, Merrill, what it is that is troubling you. If in fact we are taking those current standards in parts, 10, 50 and 100 or was it 20, 50 and 100.

Then we are taking the vast body of information based upon licensing and citing decisions over the last x number of years and then we are adding in the WASH 1400 report and projections and trying to draw out of that quantified standards, putting aside whether they're to be in what legal form.

You seem to be concerned about that process and I have not yet and maybe it's a failing on my part -- I'm not sure why it is that you're concerned about it. Because no one is saying that what is there today is wrong. We're simply saying that we're trying to move on to understand it a little bit better.

DR. EISENBUD: The implication of this exercise is that there are no safety criteria.

MR. HUTT: No. I don't accept that.

DR. EISENBUD: George Sege told me that.

MR. TEMME: That's funny. He didn't tell me that.

MR. HUTT: What he said, as I heard him, was the same thing that I used to say at FDA. That yes, FDA since 1906 has had safety goals. They've gotten sharper and more consistent and hopefully better science oriented and more articulated and more rational over the years. That doesn't mean that they never existed before.

As I heard George, he said exactly the same thing. That they were embodied in the bricks and mortar or whatever one calls it in this industry, of the decisions that were made

and now there's an attempt to articulate them a little more concisely. That's all. Not that they weren't there.

DR. EISENBUD: I would disagree that the goals is embodied in the bricks and mortar. I would say that the tactics by which the goal is to be achieved are embodied in the bricks and mortar.

You're being very tolerant, Lester. I think you can shut me up at any moment now, but why not read the three workshop objectives to see if we're any closer to those objectives then when we started. Then I won't comment anymore.

CHAIRMAN LAVE: Why don't you read them?

DR. EISENBUD: The first is, what are the principle criteria and considerations for selecting a safety goal? What are desirable and undesirable features?

MR. HUTT: What page are we on?

DR. EISENBUD: We're on page two.

MS. SHELDON: General guidelines, Workshop objectives. Item three.

CHAIRMAN LAVE: I guess I think we've covered that rather precisely.

MR. HUTT: I think we have too, number one.

DR. EISENBUD: What constraints limit efficacy of safety goal approaches? For example, what limitations are there from data base methodological institution and socio-economic standpoints.

MR. HUTT: I think we've dealt with that extensively.

DR. EISENBUD: What are the issues of social impact and value judgment? We've dealt with that. The thing that is missing is what is wrong with the present goals?

CHAIRMAN LAVE: I thought we dealt with that. That was number one. That is, why are we doing this exercise? At least, that's what I thought number one meant.

DR. ZEBROSKI: Let me offer an observation. I think there is a pragmatic defacto safety goal that the Commission is working to right now, but which is not codified. The instructions that they have given to the ASLB for TMI I and for Zion Indian Point has been very simple.

Tell us how these plants stack up relative to the average plant as typified by WASH 1400? And if it is better in some areas, worse in other areas, how does that effect the overall risk envelope.

That's been a qualitative statement with a quantitative implication that you've got to compare with this guideline. It also implies the who technique of doing the risk assessment by a probabilistic kind of process and getting good data into it. However, it has no status of law. It doesn't even have che status of prior practice. I think that it is new, essentially, this year. And I think what we're talking about is whether a codification of such a process which is what I think is implied by most of the things in the

yellow book is a good way to go.

DR. EISENBUD: And that's really a qualitative question and one this panel can answer.

CHAIRMAN LAVE: I think that is precisely a qualitative question and I think that as with all qualitative questions, you could either answer it by telling me what you feel about it or else you could analyze it. I guess, I would have thought the six things that we were talking about were really attempts to analyze in some detail whether that qualitative question was a sort of yes or no.

MR. HUTT: May I state my surprise and admiration that we have covered the three subjects that we were suppose to cover. It was not clear to me until a couple of minutes ago that that would be case.

CHAIRMAN LAVE: I think that with the sensible people that we had here, that it was clear that we would get at what the primary questions were no matter what the agenda was. So, I don't express surprise at all.

Let me go back and make sure that we do cover this last item; namely, can we say in five seconds -- I have somebody taking me literally. I should say, tersely, what is good or bad about the ACRS proposal.

Let me try, given the discussion that we've got, I would have said that it employs some comparison which have got to be looked at in detail to see whether those comparisons

grab people and my personal reaction is that, I don't think that they do particularly.

A second part of that has to do with risk aversion and Dave Okrent asked us whether we would talk about that again. I'm not sure -- My own feeling would have been that that is not a very interesting subject to talk at. That the best way of seeing whether there is risk aversion or not or whether it makes a lot of difference whether kill a hundred people one at a time or a hundred at a time, that the answer to that is best looked at by analogy, by comparisons. Not by asking somebody, in your gut, do you feel this one way or another. I don't know how to react to that one way or another.

And I certainly don't think that the mathematics of saying, do you realize if you have an alpha of two and you kill a thousand people that it's equivalent to killing a million people one at a time. That doesn't help me at all. And so, it's really getting back to the comparison and I don't think we're going to do that around this table.

MR. HUTT: Incidentally, that's one area where the comparison to food and drugs is fascinating, because when you make a mistake with a thalidomyde or a botulism, as I pointed out yesterday. Not only do you have dead bodies, but you've got a good number of them. You can count them, without any great difficulty.

CHAIRMAN LAVE: It's in a sense a thousand at a time.

MR. HUTT: That's right.

DR. ZEBROSKI: You don't line them up in one place.

MR. HUTT: Oh, yes. You can go back for example to the 1902, eleven people being killed in St. Louis. As a result, the 1902 Biologics Act. 1937, in November of 1937, the Elxir of Sulfanilamide disaster where 110 or 120 people killed by that drug and resulted in enactment of the Food, Drug and Cosmetic Act of 1938. The thalidomyde disaster of 1962, resulting in the 1962 drug amendments. You can count them. They are all laid out in Congressional history.

DR. ZEBROSKI: The other impact issue that's been argued by some social scientists and by others, too, is that the impact of having all of those events in a single community or a single region should be given a power function or a weighting function greater than the linear.

MR. HUTT: I'm not sure in our day of modern media that that would make any difference whether it was one community or one in a hundred communities.

MR. DERBY: I think the fact that it's all in one place is a proxy for whatever occurs after that news is declared.

MR. HUTT: Norman might be able to shed better insight on that.

DR. BRADBURN: The general belief of which there is some evidence is that people do weight a collective -- a

geographically collective disaster in different ways.

Actually, there is some social structural reasons why you might -- there is a sense in which the damage to the community is worse when it happens collectively. Two hundred people in one town wiped out at once. I think, has a greater collective damage on that community then two hundred people, even if they all die simultaneously distributed around the country.

MS. SHELDON: There's a fair amount of data on that.

MR.HUTT: That explains the Atlanta phenomena.

DR. BRADBURN: Right and in particularly, if it happens to be the elite. That's why, in some sense, a charter plane accident like the Atlanta one which happened to get a lot of people all of them who had -- or a company plane. You could wipe out a company if a single.

MR. HUTT: -- and olympic skating teams.

DR. BRADBURN: So there is a real sense in which many collective disasters do have damage above and beyond -- once you're thinking about damage more than the deaths of the individuals.

MR. HUTT: But, Norman, let me ask this. If you had a situation where a drug -- and we haven't had this in some time -- but a drug got out that killed a hundred people, but no more than one -- well, let's make it 50 -- no more than one in each state. With modern media, wouldn't that be

tallied up within about two hours and it would be viewed as a 50 person catastrophe.

DR. EISENBUD: That's what happened with the tampons.

DR. BRADBURN: They're different, but how much it effects the coefficient -- I expect at some level, if you do a lot of research, you could figure out the difference between two or three hundred all in one company or one town -- particularly if it's a small town as compared to 200 all at the same time, but -- due to the same cause,which is spread around by the media as compared with 200 -- Like in an occupational disease which is distributed over some period of time.

Those are all differences and they probably -- to something in terms of the risk aversive. There are many w f you spend a lot time, you could probably figure out that they would effect those coefficients in some way.

I'm not sure that that really speaks to the issue specifically as how you go to that type of analysis -- or how that type of analysis mathematically helps you exactly in your regulations.

CHAIRMAN LAVE: I think that again, the point that we were trying to make before is that these are areas where we don't have well quantified, well established beliefs and you have to sort of tease them out of people by asking not real hypotheticals, but hypotheticals which are kind of

plausible in particular comparisons of one sort or another. But one has to be very sensitive to those kind of comparisons so that you get ones that are relevant and I would have guessed that -- by the way, one of the arguments that Slovic and Fishoff make for an alpha greater than one is the media. They say that if you kill off people one at a time, they don't make the headlines of the paper, you don't remember them. The vast majority of coal miners are killed in accidents one or two at a time and you never hear about them. When you have a lot of coal miners being killed like in Farmington, then you remember that.

I would have thought that with respect to angio sarcoma from vynal choloride or toxic shock syndrome from tampons or thalidomyde, that the media has a way of bringing these all together so it wouldn't -- in that explanation, it wouldn't make any difference whether it was geographically centralized or not.

And I must say that my own reaction is not particularly one of saying that -- I mean that the statistic we have is of 2,000 people a year being murdered in New York City. I'm not clear that I feel that I would react terribly differently if it turned out that a town of 2,000 was wiped out by the Hell's Angels and that we randomly selected one town a year of 2,000 and wiped them out that way, as distinct from killing 2,000 people in New York. It's not clear to me that

I would really feel terribly differently about all of that. I don't know. It's not something that comes out and hits me in the eye that I know that I feel differently one way or another or whether I would feel a million times worse if it were a town of 2,000 than I would if it's 2,000 people in New York.

I'm sure that I wouldn't feel a million times worse. I don't know quite what to do with those things, but I think that if we did these comparisons of actual events and tried to tease out our emotional reactions to them, we might come to something.

MR. LIBARKIN: It may be --

DR. ZEBROSKI: I think that this is right to the point -- if I may interrupt. I think to the point that Karin made earlier, that regardless of probability, if you have at a hypothetical level an extremely severe event, you don't want it, no matter what the benefit. So, I think that's a codification of a large alpha, if you will, which is certainly characteristic of a sizable piece of the population. So, I think it's a nontrivial -- Maybe you or I don't feel that, but I think there are a great many people who do.

MR. HUTT: Wouldn't you say the same thing about one person -- It's like toxic shock syndrome. I feel that same way if it's one person -- It's one out of one, after all. Particularly, if it's someone that you know as compared to

a hundred. In other words, I don't think if it's just a large number of people. It's a catastrophic event. Death is a catastrophy.

MR. LIBARKIN: It may be who ever coined risk aversion did anyone a disservice, because it focuses everybody under emotional reactions and if you consider events of the magnitude of a major earthquake and to be grizzly about it, it costs more to bury all of those bodies than it does to bury bodies one at a time in the same numbers.

There are economic effects which are associated with very very large catastrophies that we don't see. That may be one reason for answering the question positively, should there be such a multiplying effect.

MR. DERBY: I want to make a point that I think is important here. These -- If you take this NUREG 0739 as a set of decision rules, then I would argue very strongly as an engineer that notions of risk aversion and calculations and coefficients have no place in decision rules. Absolutely none at all for exactly the reasons that you're talking about. That is not to say that in the formulation of a qualitative goals and in that jump from the goals to establishing the decision rules, it plays a very important and is a very difficult issue because of all the things that we're talking about here.

I think that distinction is an important one. That

you don't want to bring this kind of discussion, which, even though we're out of our element, I don't think there's a lot of people who are in the element.

MR. LIBARKIN: But, if having had the discussion you decide that there is such a place for that kind of thing in the decision rules, it gives you guidance as to how much money you ought to spend to prevent those accidents and what that says is that the amount of money you spend is not only linear with the number of bodies that you're going to count after the accident. I think that it has a perfect place in the decision rules. There may be disagreement about whether it is linear or not. This thing suggests that it is not linear and you can argue about that --

MR. DERBY: Obviously the thing that is cooked up in the yellow book is a proxy for all the things that we're hearing. It's a proxy for feelings. It's a proxy for economic dislocations. It's a proxy for genetic damage and all that other good stuff. If it's a proxy, then is it a good proxy? I don't know. It just seems that -- in the way I've responded to it, it's a very serious issue and it is very hard, I think to try to formalize this thing into a decision ruling. That's my opinion on the subject and I think like this -- not liking large catastrophic loses of people in single locations is really the place for qualitative goals to be formulated, addressing that specific issue.

And the decision rules follow from it.

CHAIRMAN LAVE: But surely, without trying to get at the merit of it, which I don't think we're going to do.

MR. TEMME: The merit of which?

CHAIRMAN LAVE: Of whether alpha is greater or less than one, for example.

MR. HUTT: You mean we don't even get a chance to vote on it.

CHAIRMAN LAVE: You don't even get a chance to vote on it and neither fifty million Frenchmen or --

I think that surely one can point out that that is precisely a social science behavioral question. That is, how do people feel about it. Not just how do we feel about it. How does that great two hundred and twenty million people out there feel about this? We're a democracy after all. If those people feel that alpha is really greater than one, then we're going to see --

MR. DERBY: I guess that that is my point. The mathematical formulation is only appealing to a very small group of people who think that way. Perhaps the issue could be reformulated in a way that you can -- the people who like the mathematical formulation after the qualitative goal meets this referendum, then they say, well in our little minds the mathematical model of alpha is effective.

MR. BRIDENBAUGH: I think it is easy to come to the

general conclusion that alpha is greater than one, but when you start to try to find out how much greater, that's when you really have trouble.

CHAIRMAN LAVE: Let me make an argument, dammit. If we're going to speak to this -- I believe alpha is far smaller than one. Far smaller than one. Take a look at the damn Cambodians. Take a look at almost all of these events where you wipe out huge numbers of people. Look at the number of Russians killed during World War II and so on. Just take a look at all of that and compare it with sort of individual events which occur. It seems to me that civilization or human kind looks at these things and say, oh, gee did we loose fifty million Russians? Too bad.

MS. SHELDON: I don't think so. I think you get to a point where you can't absorb the numbers. How many is six million Jews, for heaven sake. It is mindboggling.

It's not that you dismiss it, it's just that you are so overwhelmed with the number that you can't --

DR. ZEBROSKI: But nobody is in charge of that question. There is no agency. As a matter of fact, I think this is the point we were making earlier in national policy. They disconnect between domestic and foreign policy. People have observed that no government has a very good job of bridging it. You often do things in national policy and sometimes in foreign policy which greatly increase internation-

al tension and the risk to war. Or even that you get into a situation that you're going to loose badly. When you look at those historically, nobody is in charge of those questions. So, on a national level, we act as though alpha is very small, but I think that it is more correctly described. It goes through a maxim that if it's ten million people, we don't worry about it. That's beyond our control. That's god's will or whatever.

If it's one person, the child next door, then we give that a very large or if it's, say, two twins next door alpha is clearly considerably greater than one if they're to be killed simultaneously.

MR. HUTT: Lester, let me extend your and argue not only are you correct, but the isolated death, if it's cancer and that's what we're talking about potentially with radiation, has an alpha greater than one. Because it is such an emotional and this is probably more easily documented than anything. If you look at statuatory law where Congress singles out time and again cancer as being worse than any other form of death or illness and therefore you can argue that a natural catastrophe of the kind of blowing up a plant that would blow up people would be -- for less than one, but just a single death of cancer from a leak of radiation at a high level would have an alpha of greater than one.

I suggest we leave the whole damn subject alone.

I don't think that makes any sense either.

CHAIRMAN LAVE: Let me come back to the point again. We're not here around the table trying to decide all the subtleties of this. I think that it is a complex set of questions and somebody has got to pay some attention to it and I think that I would strongly recommend to the NRC that this -- some attention be paid. And in particular, I think the mechanism for paying attention is this business of comparison that we're worrying about earlier.

One of the things that economists are fond of pointing out is that people say one thing and do a different thing. You don't want to believe hypothetical that are posed to people if they're really hypothetical. And the closer you can come to the real situation, the more belief you might have in them.

MR. HUTT: I haven't looked at the Delaney Clause in that way, but I guess that shows an alpha of infinite.

DR. ZEBROSKI: But I think the question that Dave Okrent put which is how risk aversive should rules or guidelines be. I think if you go a strictly moral route, which is to say, I don't want to take a protective action as a bureaucrat, which has as a possible consequence that protect my territory at the expense of killing more people in another territory. In other words, I regulate nuclear energy out of existence and I may kill twice as many people with coal, but

that's not my department, so I'm morally clean. I think that's immoral.

I think Dave was asking the question that we have a great deal of what appears to be emotional pressure to set the goal values on a risk aversive bases. In other words, much lower than you would say from a comparative risk assessment or lower, even, than that natural variation argument that the British use. If you set it much lower and you recognize that it may have this immoral consequence of killing more people somewhere else, you have to find some justification for it.

In an alpha of two is a pretty good justification for setting a highly risk aversive value. The question is whether that's a valid reason for setting --

MR. HUTT: The fact that Lester's --

DR. ZEBROSKI: Even more deadly -- let me make one more point on risk aversion -- If you have a high risk aversion coefficient, you have a high rate of panic from minor events. You will evacuate people and kill them on the highway when there is in fact no danger to them.

MR. HUTT: Okay, but let's combine your quite valid point and the one made by Margaret repeatedly yesterday of comparing various industries but keeping them within the energy industry and then look at alpha. The question that ought to be put to people is, is it worse to die of cancer

over a long period of time or to be buried in a coal mine. Which way would you rather die?

DR. ZEBROSKI: That would be hypothetical because most people never go down in coal mines. They wipe that alternative out.

MR. HUTT: I'll tell you. I'd rather get cancer.

CHAIRMAN LAVE: It isn't the way posing the question in your society -- your ideal society right here. The one that we're going to have. Do you want to have 200 coal miners a year being killed or do you want to have some number of people dying of cancer because of --

That's the way of posing the question. Let's not ask about you. Would you rather loose your right ear or your left big toe? The answer I'm sure -- we can tease those kind of answers out of you and we'll find out exactly what Peter Hutt thinks about the world and we'll all have quietly gone to sleep in the meantime.

Ed, I think that there are people who run around trying to think of what conceivable reason could be used to show that having beaten on nuclear power was really the right thing to have done and I think that if we get enough clever people, we can get enough reasons: alpha greater than one or something else stuck in there. That doesn't seem to me to be a very helpful route to go down. What you want to do is to pose a question in the most relevant way you can and the way

people find easiest to ask. What would you like in your ideal society given that we can't have everybody living to be a hundred and have everybody be rich and so on. That is if we talk about a society that we're likely to observe in the future. What kind of trade-offs would you like to see there.

DR. ZEBROSKI: I think that the thing that is missing is that the trade-offs are not made explicit in those cases. I think this is where both the NRC and the industry are doing a miserable job of communication. Even the economic trade-off is now so befuddled.

I was amazed to find Harry Rowan who I never found to be a friend of anything nuclear bemoaning the fact that every man, woman and child in California is paying about $400 a year for the priviledge of having Diablo Canyon sit idle, because they're buying that much more oil from Indonesia and elsewhere. And he thought this was appalling. I suspect that there isn't a thousand of a percent of the electorate that is aware of that trade off.

And there are many others of that kind. There are trade-offs from deaths of other sources. The trade-off on world tension and the liklihood of war. None of these are made explicit. I think this is one of the disasters of this situation.

CHAIRMAN LAVE: That's why in our infinite wisdom and at Margaret's suggestion we have item three -- sorry, two,

namely, what is safety, what are the goals? We didn't let anybody get away simply suggesting that changing the number of premature deaths or life-expectancy was all need one consider. I think that that point has to be reinforced and I'm certainly aware of it and I won't let anybody get away with it.

I think that there are all sorts of problems here. To get back to Norman's comment that not only is information scarce and expensive among the public out there, but forcing the public to think about things they don't want to think about, you do at your own peril.

Most people go through their daily lives without confronting it every minute the nature of the safety trade-offs and so on and so on that they're making. If you force them to consider those things, just watch out. If you get your head back, you're lucky. So, one has to find some mechanism for doing these.

DR. BRADBURN: Just to follow that for one second. I'm thinking of your statement about saccharin relative to cyclomate. There is a sense that things get overloaded. If you've done it in the other order. If people had gone after saccharin first. In fact, saccharin is practically dying out in use.

MR. HUTT: Actually, what happened is that they were used in combination. Succaryl was a ten to one cyclomate saccharin combination. Cyclomate did not have as much

sweetening power.

DR. BRADBURN: There is some point in which people just -- you know --

DR. ZEBROSKI: Carcinogen of the week.

DR. BRADBURN: Right. I'm going to shut it off. I no longer care. You can have one or you can have two. Then they begin to start telling me that everything is deadly. I'm just going to stop paying attention.

MR. HUTT: A good example of that is what I think I mentioned to some of you. Two years ago, in the Federal Register, FDA flatly said that if there had to be a warning statement on every food that contained some carcinogenic constituent, you'd have to put a warning on, and this is a quote, many if not most food products in the market place today. That didn't get picked up by anybody.

DR. EISENBUD: Because of the natural carcinogens or because of the --

MR. HUTT: The contaminents. The various contaminents. It included all sources of contaminents. All trace contaminents of one kind or another.

DR. EISENBUD: Including the aflatoxins.

MR. HUTT: Sure. That didn't include things that you purposely added.

CHAIRMAN LAVE: But all of these constituents, safforal -- there are some spices that contain.

MR. HUTT: That's it. FDA is terribly concerned. They have turned off the public. NObody has paid any attention.

MS. SHELDON: I think that's true, but I still don't think that that means that you don't try. I think what you're saying reflects also and unfortunately a somewhat of a patronizing attitude toward the public. People need to have this information. It's critical to have this information and this is supposedly a democracy and people are suppose to make choices or be involved in making choices.

The equipment to make the choices isn't out there.

DR. BRADBUN: Let me just say one thing. You say the equipment for them isn't out there, but if you're going to take that view -- I don't see any sense to disagree with that -- but you want also to have to have a much better sense about people's capacity to process information.

What always gets me at meetings like this, particularly with highly technical people is that they have an assumption that everybody is as interested in the topic and has the background and so forth that they are. As if most of the world out there wasn't interested in doing other things and that particular topic -- They're competing in some sense for interest -- where most people, it's a very small portion of what they can attend to.

The ability of people -- anybody, even me and this

is not being patronizing is the statement about people's ability to process information. There are limits to how much information that you can process at different levels.

You organize it in different ways to get -- and so forth and where you get it and it takes time. We have so much time. Time is a limited resource. You're standing all sorts of -- I don't think it's a question of being patronizing. I, as a student, am amazed at how well people -- efficiently rational people operate. You've got to understand the context in which they're operating.

CHAIRMAN LAVE: A felicitous framework for presenting information -- I guess what I would have said about that -- I would have thought that qualitative information about carcinogenicity is not a felicitous framework. It's felicitous if there are three carcinogens in the world. If you've got lots of carcinogens in the world and all sorts of carcinogens that you can't do anything about, then that's not felicitous.

For my own purposes, what would be felicitous would be quantiative information where somebody said, pepper has this chance of causing cancer, but I also don't think that that information would be felicitous to most people. That's not patronizing. That's got to do with the way people think about the world.

I think that one of the things that the National

Academy of Sciences panel on saccharin tried to think about was, is there a felicitous way that you can label -- that you can present some of this information so that you're not being -- the ultimate of patronizing is making the regulatory decision that says that you can't have it. Like sassafras tea. You can't have it. That's much more patronizing than putting that information or labeling or whatever it is.

They did not think that labeling was a terribly useful way of talking about toxics in food.

MR. HUTT: They came up with a sillier idea. They were going to have a special section in the food store for risky foods. Absolutely true.

DR. ZEBROSKI: They would have it next to the risque magazines.

CHAIRMAN LAVE: There's a presumption that you wouldn't get into that unless you were eighteen years old.

MR. LIBARKIN: Is there a conclusion that you could state about the concensus or whatever concerning risk aversion and its place in this whole exercise.

CHAIRMAN LAVE: I guess that I would have thought that risk aversion like the other attributes of safety is terribly important and any quantitative safety goals have to reflect them. I thought in the discussion that we had, we had decided that A) a good way of getting at what peoples feelings were about these was by using comparisons, by trying

to take a look at current behavior. Showing it to people. Asking them whether they really believe that. Whether those comparisons reflected trying to get comparison of like figures in all of this. I think that that's the kind of recommendation that I would make to the NRC.

MR. TEMME: Let me, if I may add something to that, say a few words about Alpha. I think I agree, too. Risk aversion is important. I think there are two sides to this to be considered. The first is, how do people feel about risk aversion, the big uncertainty about that. The second thing is, what's to be done about how people feel about it. There is some difficulty with that especially in the face of the uncertainty about the first part.

I am personally am very dissatisfied with the particular treatment of it in this book. I see no more than a very superficial connection between even the concept of the coefficient alpha and the real issues and even greater degree of superficiality about the assignment of the value of 1.2 to alpha. If it is to be treated in a quantiative manner in decision rules, I think it deserves a great deal of better treatment than what I see here.

DR. EISENBUD: Is it being proposed? I guess it would have to be incorporated in the decision rules. That hadn't occurred to me. Up to now, it's been a theoretical exercise.

MR. TEMME: There are other ways of incorporating it into decision rules, I would hope. Maybe even in qualitative ways, would be better than what's being done here.

DR. ZEBROSKI: It's rationalizing what everybody now agrees was bad behavior on the part of the regulators over the years, which was over emphasis on intra spectrum accidents to the neglect of more probable --

MR. TEMME: Yes, I agree.

DR. EISENBUD: Also the fact that it gives recognition to mispreceptions on the part of the public that causes them to have these aversions. You've got the same problem in Food and Drug, for example. I think that probably the average person, the educated person, is of the opinion that the presence of carcinogens -- man-made carcinogens in foods consumed by humans are responsible for a detectable increase in cancer. Yet, that's not so.

MR. HUTT: Probably the opposite is true. All the preservatives have undoubtedly reduced -- been the cause of massive reduction of cancer of the stomach, which has occurred. It has more than been cut in half.

DR. EISENBUD: This aversion to carcinogens of food, also, does that require that FDA be stricter about carcinogens in food than they would be let's say about carcinogens in over the counter drugs.

DR. ZEBROSKI: I think that Karin's point, though, in

informing the public, even if you say people are not interested or they're bored or they don't have the processing capacity. I think that the informing the public in an authoritative way nevertheless in a public health sense is very effective.

I can think of two examples. The tremendous decline in smoking for people who are literate and over thirty and this unknown reason for the decline in certain kinds of heart disease that probably reflect that people job and play tennis more and watch cholesteral. None of these are deterministic. I don't think that anybody has really done it as epidimiology, but they're real facts.

MR. HUTT: Lester, I'd like to move on to something that is related and that was touched on once or twice in the course of our discussions and see whether there is agreement or disagreement on this.

That is and it comes up in the question of communicating with the public and others. There has long been concern on the part of regulators with a single excpetion that you've pointed out this morning of talking about risk in terms of numbers of dead. How many lives saved or lost, etcetera. I stated at the outset yesterday that I thought it was important to do that, because you can't communicate with the public unless you're up front and credible and honest and straightforward and tell them what you're talking about.

Talking about risk levels of the difference between ten to the minus five and ten to the minus six and I don't understand and I've been working in it a long time and I'm sure that nobody else out there would really understand except in very vague general terms.

DR. EISENBUD: You understand it, but you don't have a subjective --

MR. HUTT: You don't know what it means in real world terms. My experience, which I think I related, was initially I was terribly reluctant to ever talk in those terms, because it was a new concept and I didn't want to be the one on behalf of my agency to get shot at on that issue, so I carefully avoided it. But, I learned a lesson at the same time -- and I related this to a couple of you at the break -- I released back in the early seventies, the filth guidelines that FDA had -- the filth in food. Guidelines that had existed secret since 1911.

They weren't changed, they weren't increased or decreased. They were simply released to the public. They caused an absolute furor for about six months. That was the only thing that everybody talked about. They didn't want to know how many rat hairs there were in their chocolate bar.

But then it all disappeared after six months and people no longer got worried about that. They learned that yes, indeed, if you have any food at all, you're going to have

rat hairs in it or you're not going to have any food. You have those two choices, basically. People therefore are able to come to grips with real numbers and real examples when they're forced to. I think that it was to me a valuable lesson and I increasingly as I was in government was very concerned about failing to come out with real world terms and meeting issues head on and saying, we're talking about five people dying a year.

I used to use this all the time in the vaccine example of saying that what you do when you put out a new vaccine, is you purposely kill two or three people in the country in order to avoid a different four or five hundred dying. But it is just as though you line up those two or three and shoot them. You've got to tell the public that's what you're doing when you vaccinate people and people can understand it in those terms, but when you use all of these probability terms, nobody understands what you're talking about.

Now, I'd like to throw that out and see if there are people here who disagree and who are fearful of what the public reaction would be.

DR. EISENBUD: There is ignorance. Maybe this is a patronizing statement, but -- I remember when the EPA had asked, what I think was silly calculations, Lester. The thirteen people who died in Pittsburgh as a result of an

inversion that they had. The mayor of Pittsburgh issued an immediate order to his staff to find out who the thirteen people were who died.

MR. HUTT: The problem is, unless you're willing to meet that head on -- You see, that doesn't trouble me. I'd go to the mayor of Pittsburgh and say, you don't understand and here's the way it is and here's why we use that calculation. You'll never know. You'll never find out and it's no different than all of these other examples and give him fifty other examples and educate him.

The only other alternative is to presume that they are not only stupid, but they're going to remain stupid for all time and that they'll never understand anything and I'm unwilling to make that assumptic .

CHAIRMAN LAVE: Not only must they remain stupid, they won't interfere and that's -- I'm prepared to believe that some people who are going to remain stupid, but they're going to interfere and I'm going -- you'd better educate them.

MR. HUTT: I'm a great believer in doing everything the government can to open up its processes. To make them clearer instead of more obscure and make them more quantitative in terms of exactly what you're talking about. Tell them what the trade-offs are. Bring them into the process and let the public find out how difficult these decisions are. They aren't

always easy on the outside looking in.

DR. MAXEY: Do you see any institutional deficiency though in the way in which the regulatory system is -- operating.

MR. HUTT: Margaret, I'm not being critical here of NRC. What I was saying is, I would prefer to see numbers like ten to the minus five brought into real world terms. That's all and talk about it in the maximum number of deaths that could be anticipated. FDA did this with saccharin. FDA said translated all the risk levels on saccharin meant that there would be between zero and I think 2,300 new liver cancer or bladder -- I keep saying liver -- bladder cancer cases each year. Somewhere between zero and 2,300. That's the best we can do. You, the American Congress, can decide whether you want that or not. They decided.

DR. EISENBUD: How do you explain the reaction to the -- Upton's calculations at the time of TMI on the one day he said that there might be half a cancer or maybe one cancer, and on the next day there might be two cancers produced. Califano was attacked for giving misinformation.

MR. HUTT: What you're saying is there are problems in communication. Yes, of course, there are. But it would be no different if you changed it from ten to the minus five to ten to the minus six. They would attack you for changing.

I'm willing to accept the problems that go along

with it. Here is where I say that the benefits of better education outweigh the risks. There are risks.

MR. MALSCH: There is an example of that in AEC and NRC practice, where for years, the AEC refused to calculate series accident consequences and refused to have so-called -- accident consequences discussed in any public forum including these proceedings. With the result, I'm sure, that people became convinced the results of so-called -- actions were absolutely catastrophic.

It was like going to a doctor and saying, what would happen if I took this pill and the answer is, I can't tell you. I won't tell you what they are. -- you will die on the spot and that was a practice that AEC and NRC followed for years with probably disasterous consequences.

MR. TEMME: With a lot of encouragement from the industry.

MR. HUTT: Mark, you were apprehensive when I first brought this up.

MR. TEMME: That wasn't really what I was apprehensive about. I'm in full agreement with what you say. I don't think we should run around in fear of quoting what we mean when we talk about risk. That wasn't the issue at all.

DR. EISENBUD: I think that I might adjust it in one way that perhaps what should have been done at the time of Three Mile Island,for example, was to have said that while

we anticipate, based on what we know about cancer incidence, that among the half million people, there will be 125,000 cancers in the next -- in the lifetime of the population and that, as a result of this accident, there could be an additional -- either no additional cancers or possibly one.

MR. BRIDENBAUGH: Isn't that what was said? That's what I heard said. That's not what runs on the headlines.

CHAIRMAN LAVE: I think that a lot of the task of problem of nuclear power is a legacy of not being open about all of this stuff. That leads to a profound distrust and that takes a long time to dissipate that mistrust.

MS.SHELDON: I think that's the fundamental problem. The Agency is perceived as dishonest and not worthy of trust.

DR. EISENBUD: Published WASH 740 is an unclassified document in 1957?

MR. MALSCH: Yes, and it refused to have the document considered in any licensing proceeding.

DR. EISENBUD: Well, I don't have the history of that. The point is that it was in the public domain.

MR. MALSCH: The problem is that it refused to consider it in ways in which people considered it to be --

MR. HUTT: I don't want to debate necessarily all the past -- but I would think that something could be said about that in -- under this rubic of number six of implementation. How you go about communicating this. It might be

helpful.

MS. SHELDON: One comment on your comment that the public has trouble processing information or that they have other agendas in their lives. There are certain things that are more important than others, obviously, and if anything has grabbed public attention over the last three or four years, it's the increase in their fuel bill, probably, among other things. This kind of issue is one of the kind that you could expect people to be interested in and to worry about.

Everyone recognizes that we need energy. Most everyone is cognizant of the difficulties with oil supply and the potential conflicts in the Middle East so this is not the level of issue that you're going to have to strain to put before the public. I think there is already an interest in and a concern about having the right information.

If we can't make fundamental decisions about energy, I can't see another area where we could be expected to.

MR.HUTT: I have to tell you just as a foot note and some of you may be amused about. Some people, when economists, I will confess, Lester, in Chicago, in particular when FDA released its filth guidelines for food, were very concerned that FDA had been using these to ban food and said that the right way to do it is to have every food labeled with the amount of rat hairs and then let the market place take care of it.

Talk about a limitation on ability to process knowledge. I've never heard anybody who wanted to have that information on their food. There are certain things people don't want to know about.

CHAIRMAN LAVE: You could have some right wing consumerist who would say, not only do you have to label it, but you can't charge people for the rat hairs.

I suggest at this point that we disband until 8:00 tomorrow morning when we go in Plenary session. There will be lots of time, as I understand it, for people to make their points and tomorrow you're not precluded from commenting on the Panel B report.

Thank you.

(Whereupon at 5:30 p.m., the discussion of Panel B was concluded.)

This is to certify that the attached proceedings before the

Nuclear Regulatory Commission

in the matter of:

Date of Proceeding: 2 April 1981

Docket Number: Safety Goal Workshop

Place of Proceeding: Palo Alto, California

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

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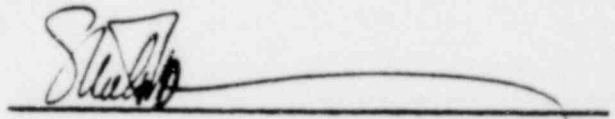

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