NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE MEETING

on

RELIABILITY AND PROBABILISTIC ASSESSMENT

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Wednesday, 12 September 1979

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

RELIABILITY AND PROBABILISTIC ASSESSMENT

Century IV Room
Airport Quality Inn
Los Angeles, California

Wednesday, 12 September 1979

The ACRS Subcommittee on Reliability and Probabilistic Assessment met, pursuant to adjournment, at 8:30 a.m., Dr. David Okrent, chairman of the subcommittee, presiding.

PRESENT:

DR. DAVID OKRENT, Chairman of the Subcommittee

PROF. WILLIAM KERR, Member

DR. HAROLD LEWIS, Member

DR. J. CARSON MARK, Member

P R O C E E D I N G S

DR. OKRENT: This is a continuation of the subcommittee meeting on reliability and probablistic assessment. This morning, I suppose we start out continuing the discussion of how do you try to develop criterion with regard to acceptable risks or nonacceptable risks, as the case may be. And also, at some point during the morning come back to the topic of the priorities and the probablistic analysis staff program. At least talk a little bit and see what we want to do and when we can talk about it again, and so forth. I guess our status is with regard to the first topic is yesterday we heard what the staff contemplates doing during the next 12 months. And I suppose one thing we should consider doing this morning is seeing whether we have any comments we want to offer on what they are planning to do.

I think we should also think about how the ACRS meeting, this subcommittee in particular, should proceed both independently and cooperatively, let's say, with the staff and perhaps out of such thinking we may arrive at some areas in which we would like to see the staff develop some information since they have large financial resources.

(Laughter.)

And we are such a small office -- we are just a small office.

I wonder first if the subcommittee members here want

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to provide any general or specific thoughts.

DR. MARK: I am sorry Hal isn't here, because I wish to disagree with him.

DR. OKRENT: He will be here. I assume the smog or fog or haze, as the case may be, at the airport is once again delaying his arrival.

DR. MARK: This still might be the place to express an opinion, and that is that the work that was referred to not described on obtaining of what one might hope as near as possible a similar basis, the risk features for coal generation of energy. It seems to me of great importance in connection with the general objective here. And perhaps coal is enough. It is not necessary to bring in the rights, nor to bring in tidal tower or something which doesn't exist. Coal does exist and is being used.

The only other thing would perhaps be oil, if one wanted to wonder about it. But coal is a must, and that is absolutely necessary from the point of view of attempting to discuss and proceed onto what might be acceptable for nuclear energy, because if coal is zero in all respects and nuclear energy is something, then there is no acceptable level for nuclear energy if that were true. But, it isn't true. If they were equal one might say coal has to be given priority spot, number one, because it is understandable if they are exactly equal. I don't believe they are equal, and I don't

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suppose coal is as well known. But it seems to me there is -- there isn't a trap in this instance Hal referred to it. Just because you write the numbers down and one is bigger than the other, that settles everything, and that certainly is not the case. But the number must really be in hand and be developed on a basis that can be defended as being comparable. That is certainly how I feel about that item. It is a necessary part of the general plan here.

DR. OKRENT: Well, I guess I would support that point of view and, in fact, urge that the NRC staff and whatever studies they are having done, look at some aspects of coal which when you think about them resembles kinds of things that have been or are being looked at for nuclear. But at least in some of these analyses I have seen have been left out.

For example, it is not clear to me whether for coal if you have clean-up processes taking the SO_2 out and so forth, whether people have looked very hard about the long-term storage of the wastes and what their effects might be over the same time periods that you are looking at for nuclear.

There are certainly lots of waste. They are not necessarily harmless. The EPA hasn't developed equivalent standards for their disposal as it is trying to develop for high-level waste and so forth. Similarly, there certainly are other things emitted into the atmosphere from coal besides sulfate, radioactivity and estimates should be made albeit and

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certainly for their potential effects.

And, of course, we talked about the CO_2 problem. Coal is not the only contributor, but it will be an important contributor. So, you need to find a way of factoring this in. And, things of this sort.

The other thing that wasn't clear to me, I guess from what Dr. Vesely was saying yesterday was whether SAI is putting kind of a risk aversion into its analysis of effects. I think, myself, if you are going to include risk aversion, by that I mean you pay more if you have accidents killing many people at one time than you would for the body count, per se, that you do it only after you have computed an expected value of whatever it is and then you say if there is the following risk aversion I would get an additional result.

I guess, as I have indicated elsewhere in writing I myself don't think society really practices strong risk aversions for large accidents in many activities. And if you are going to do this for nuclear, you had better go back and look at all kinds of activities in the United States that I think would be ruled out out of hand if you applied risk aversion.

A simple example is if you have a dam that can kill 100,000 people, the probability that you would need if you use a square or cube volume, the probability of nonfailure is achievable, I would say.

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PROF. KERR: I hate for this to develop into a commentary, by an ACRS subcommittee on what other ACRS subcommittee members have been saying, but I do want to comment on the coal and the risk aversion.

It seems to me that there isn't any question that some additional work on coal cycle risk is needed because the results of the Inhaber study are being used by the people in licensing, and they feel that they are likely to be under attack and that they need better data. It seems to me that it's the reason this study ought to be done. I don't think personally that it ought to be done in connection with an effort to determine what is an acceptable risk for nuclear because I think it has some relevance, if only peripheral relevance, and it seems to me that it has direct application, however, in a licensing process. And one can justify work on it on that basis. And that is the way it ought to be justified. That is the emphasis that I'd like to be given to it.

As far as the risk aversion is concerned, Dave, it seems to me that what you said about the dam decision is quite logical. But it isn't the way the public makes a decision. And it seems to me at some point in this study one needs also to try to determine the way in which people view risk aversion. And I think -- I don't understand why they do, but I think there are certain situations in which there is an aversion to large accidents in practice, and in other cases,

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the other one is an example in which there is not. So, it is not going to be a matter of just calculating by mathematics. I think probably we have to try to understand why people make decisions consciously or unconsciously the way they do.

I have some additional comments on what we heard yesterday, if I may. It seems to me that the start toward what I would define as an effort to define in quantitative risk criterion or set of criteria is starting at a new reasonable way. And one seems to be making use on big system information and previous work, and certainly this ought to be done.

It also seems to me from what I saw that the effort and exploring some sort of definition of acceptable risk as contrasted with efforts to tidy up the method of calculating insistent risk, I think that it's reasonable and probably the writing of what I perceive to be some sort of handbook is a reasonable wrap-up of the first phase. It isn't clear to me, though, what the audience for the handbook is expected to be.

I think if I were PAS, unless you have already given this careful thought, I would want to give it some thought. If it were being written, for example, for NRR or for RES or PAS or Congress or the Commissions.

I think it is important because it seems to me, as I read the handbook, it does not bear on the ultimate question which I interpret to be acceptable risk for a nuclear fuel cycle. It rather is an effort to collect and perhaps explain

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the conventional wisdom that already exists in the field. And if, for example, when deciding that the ultimate user of the handbook is only RES, I am not sure how useful it is to RES in the outlying --

And maybe -- I am a little reluctant to use the word -- overkill, because ultimately you have to decide what to do next. And the way this outline looks, it strikes me that the product may be of more use to the other citizens that they can, once it is published, can use it to impress future clients. Look, this is what we have turned out, and we are experts in this field, and here is proof that it is to RES in deciding what to do next.

Now, admittedly I have only looked at an outline, so this is clearly a superficial or an observation based on a superficial examination. I also would think that what I perceive to be a juxtaposition or a lumping of a method for using quantitative risk criteria which are already with some uncertainties calculable with existing techniques. In the licensing process, ACRS and other groups have urged that efforts be made to do this. But the lumping of that task with the task of trying to determine what an acceptable risk is, perhaps that lumping is desirable and inevitable. But, I think if one is going to do it that way, one has to be very careful that the acceptability part of the task doesn't get lost, and there are lots of reasons that it could get lost.

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In the first place, it is very difficult. And in the second place, nobody probably knows how to do it. Or as the other parts of the test, the people do sort of know how to do it and people do have backgrc.nd, and so there is going to be a tendency to put emphasis on that. And that emphasis is deserved. But I think the acceptability part deserves emphasis, too.

I would urge -- as you go along you continually emphasize that both of these are important. I think there is a considerab'e linkage that ha_ to be there, but one also has to be very careful that one doesn't completely submerge the other. And now, I am going to engage in politics or semantics or whatever, but I think we are forced to here.

I w uld urge, plead, exort the staff not to continue talking about what I heard them talking about yesterday, which was using WASH-1400 as a criteria for several reasons:

In the first place, I think what we are really talking about is developing quantitative risk criteria. That is what we ought to say we are trying to do or what you are trying to do. I don't really think we are talking about using WASH-1400. WASH-1400 is a historical document. It is extremely important. It is a pioneering effort, and it is great, but it is already obsolete in terms of results. We talked about some of the reasons that it is obsolete. That doesn't mean it is bad, it just means we now know more than we did then.

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And I am sure the developers, which there are some here, hoped that it wouldn't stop with that document. That is going forward the way which one hoped it would.

So, I think we mislead ourselves. But I think we particularly mislead a considerable segment of the public if we keep talking about using WASH-1400 as a criteria. And since it is discredited in the minds of some people, albeit unjustly, it nevertheless is. I think if you don't talk about it, you don't really need, you make it easier to communicate with people what you are trying to do.

On another, but related topic, I am puzzled when one looks for a particular risk number as being an appropriate one to use for licensing, and indeed, perhaps I misunderstood that the only justification of it is that it was calculated for the Surry plant, which is a pretty good plant. I would think one would certainly look at that and it would be an important contributor to the final decision. But it seems to me one needs for justification than that. And indeed, I would guess that one might find it desirable to look at something like a sliding scale of risk.

It might well be for example that for a given class of plants, based on historical considerations or others, that one risk number is appropriate, where for plants being -- coming on line today, a different number is appropriate. And if one looks to the future, perhaps even a different number is

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appropriate. It isn't that all clear to me that one number, and particularly a number based on one plant is a good way even to start. But that is pretty subjective.

Now, in the acceptability work, I was struck by the absence of any mention with the possible exception of sort of an offhand reference to geographics of any specific investigation of acceptability of nuclear power to the female fraction of the population. I mention this because many of the polls I have seen indicate that the perception of the hazard of nuclear power is more serious to the female fraction, and significantly more serious to the female fraction of the population than it is to the male. I don't know that this is so, but I have seen enough evidence that I think there is certainly considerable evidence. And I have also had personal contact with people that would convince me that this well could be the case.

Now, if one looks at why it might be the case, I could think of at least a couple of reasons. In the first place, there is a perception of a possible -- of genetic damage. And, it is the nature of the species, I guess, that women are maybe more concerned about this than men. I think it is interesting that this perception exists, because as far as I know, there isn't any evidence of any genetic damage to humans. We certainly would have to be aware of the possibility, and I guess it is likely that you see all sorts of statements about this horrible genetic damage, but yet none is observed.

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That is a parenthetical. There also, I think, is another reason for this concern, and that is because of the again perceived linkage of nuclear power with weapons. I think there is also a considerable concern about -- on the part of women about their sons and their daughters and what nuclear weapons may do. There may be other reasons. Even I think these are real reasons or perceived reasons.

But the point I am going to make is that if one is looking at acceptability of nuclear power risks, one can't ignore this, because I personally think it is significant and it is different from the coal cycle. If there is any bias in the coal cycle, it probably ought to be on the part of males. because most miners nowadays are males. There are a few females. And that is probably where the damage lies. But it seems to me that there ought to be somebody in the acceptability business looking at this. Those are the comments I have at this point.

Now, one other vague point, Dave, when we use the phrase, "very easily acceptable risk," we think in terms of a risk which if sufficient fraction of the population will accept it, otherwise the project isn't acceptable.

Or, do we think of something that they ought to accept in the view of a smaller group of the population, like the NAS, EPA, HEW, plus the agencies directly involved, plus the peer groups of those. That is a particularly conceivable

definition of acceptable. It would, of course, aim for another step not mentioned, and that is bringing in such a way the facts that bear that the population would look at these and come to accept them.

If you just take a poll and heaven knows what sort of answer you will get, and you will get the answer first off that nuclear power isn't accept le. So, there is another step involved here besides collecting data. And that is how it is to be made use of.

I think this is already vague, and I understand that, but I believe it is there. Congress might be the proper target. If Congress accepts it, then by definition, it is acceptable. They declare war, and everybody agrees that war is properly declared, for instance.

DR. OKRENT: Well, in fact, I am going to agree that Congress is the proper target mark. That, I think, is another thing that needs to be somewhere in mind. I don't know what you do about it with this program at this point.

PROF. KERR: Congress, it seems, is inevitably the target. From a number of quarters they are a target in this instance. Congressmen themselves make decisions. But they also are a pressure point for what the public -- however it communicates with Congress perceives to be acceptable or desirable.

DR. OKRENT: Well, I guess I am certainly conscious

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of the fact that the reaction of large sectors of the public to what seems to be the same risks from an engineering point of view is different. I think there are probably quite a few reasons why these reactions are different. And among these is an abstinence of better or more information on how these risks are estimated or what they are estimated to be. I don't think the public necessarily has the same concept of the magnitude and so forth, as you would get from let's say independent objective estimates of these. Now, I think that is a real situation. In other words, I think there is a considerable difference on what the public reception is and what these things are estimated to be or where you have statistics of what they are. And, of course, there are surveys where they have looked at how the public views things for which it has statistics, and they come out wrong frequently.

Like there should be a bigger death rate for botulism than there is because it receives more play in the media.

The second thing is if you don't pose a question in terms of alternatives and the alternatives are real alternatives, I think you can get a rather different response to a poll. And I would urge that if there is any sampling of opinion via the NRC programs, that this is kept in mind. I think it is a very important aspect of decision making. I frequently go to the polls in November with the feeling I don't like either

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candidate, so if somebody asked me, do you like Candidate A, I would say no. If he asked me only, do you like Candidate B, I would say no. But when I get there I have to vote, unless I decide to have a no vote for some reason, so you end up faced with a choice of alternatives in real life. And I think that should be the case in assessing preferences.

DR. SAUNDERS: It is demonstrated again and again that science has taken simple surveys that are so precise that if you don't have the results you want, you can formulate the question in such a manner as to receive approval for your program. That has been demonstrated time and time again. I do not really favor letting people poll their ignorance to decide the course of this country, or taking a consensus of ignorance. I think we ought to talk to people in Congress, as you suggest, and present the alternatives.

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PROF. KERR: I agree with you but I think in order to remove some of the ignorance, one needs to know where it is, what are the perceptions that exist, and insofar as one can, why they exist.

DR. SAUNDERS: All right. I think that's right.

DR. OKRENT: I certainly would like to support Dr. Kerr's suggestions that you not let the question of what constitutes acceptable risk be submerged in your efforts during the next year as well as for the long term. I understand your interest in having something that, to the licensing staff, looks workable.

But looking workable is certainly not sufficient and at this stage it may or may not be necessary in the sense that what one might try to do as part of one's effort is to try to look at what constitutes a definition of acceptable risk that one thinks society might provide general agreement to, and one could then go back and see, is it workable, can you meet it, in fact, and you might decide that it doesn't match up on either of those two counts or you may find that with modifications or whatever, it can somehow be compatible with workability and so forth.

Again, you are not saying you divorce the consideration of workability, but again, we come back to the single failure criterion. It is workable, although even there the staff has had to make special definitions for

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special cases, as you well know. But it was not necessarily sufficient. Maybe I should turn the discussion for a little bit to what the subcommittee members think the ACRS should try to do of its own initiative, aside from what the staff and other groups are doing.

DR. VESELY: Could I interject at this point? I certainly agree with Dr. Kerr's comment that -- I don't see the final criteria or even the unacceptability criteria as being a WASH-1400 criteria. We were certainly going to use it as a bases and modify and extend but I don't see us proposing WASH-1400 as a criteria. If that came through, that is my mistake. That is my fault. I want to use that as a source of information along with a lot of other information.

I see the criteria as being different. I would see it as being quite different from WASH-1400.

PROF. KERR: I agree with you, Bill.

DR. VESELY: It is a good point.

DR. OKRENT: Let's see. If we try to think as to how the ACRS itself might try to develop approaches to acceptable risk there are different possibilities that come to mind. One is of course that we would have subcommittee meetings at appropriate times. The second is that we might try to have what you would call a symposia, where we try to invite people from outside our immediate community to offer

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thought on this.

We can try to get a few or many consultants in the area working more closely with us, in addition to Dr. Saunders, and I think Dr. Wilson will probably serve with us. He is out of the country now. We have two ACRS fellows who are, I think, going to be working in the area, I believe. We can try to approach in some way other agencies, if we wanted to. For example, the National Science Foundation or the National Academy are doing certain things now in the area.

So there are various kinds of steps that we can try to do. We don't need to have symposia or so forth. I think it would be useful to try to get participation from a range of bodies if we could do it in meaningful ways, if they are interested. I made a list of possible groups to whom one might look for either comments or contributions or whatever, and without trying to make it a complete list and thinking only in the U.S. for the moment, let me just read. For example --

DR. MARK: Could I ask, it is not really off the track, I hope -- are people on the staff or are any of the rest of us aware of scheduled meetings which under some auspices or other are going to have discussions that bear on this field? Or maybe AIF would have a symposium on the field. That would be worth knowing if it were the case.

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DR. OKRENT: I know of one which is by invitation. It is called Societal Risk Assessment, How Safe is Safe Enough?, being held at the General Motors Technical Center, October 8-9, 1979.

DR. MARK: Under the auspices of —

DR. OKRENT: General Motors. They have four sessions, morning and afternoon of each of the two days. They don't have on their agenda the specific topic, what are quantitative risk acceptance criteria and how should we approach them.

DR. MARK: Would there be a point if someone involved in the general effort in the agency should, in fact, attend?

DR. OKRENT: I plan to attend this one.

DR. MARK: Okay. That covers my point.

DR. VESELY: The staff has been working with the National Science Foundation and National Academy of Engineering to hold a workshop on the use of probabilistic techniques in decision-making, of which one of the topics will be the acceptability criteria. The specific date for that has not been established, but it is scheduled for near the end of the year, December or January, and we have contributed money to help with the administration of that workship. This is a part of this project, our acceptability risk project which is due for completion, as I said, in

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January.

We will keep the ACRS informed of the date when we get that, as soon as tnat is established. That is supposed to be pretty big in the Washington area. We are looking at several days to a week symposia with various speakers and workshops on the use of these techniques in attempting to make decisions.

DR. SAUNDERS: When will this be?

DR. VESELY: It has not been established, the specific dates, but tentatively January or February.

DR. SAUNDERS: Of this next year?

DR. VESELY: Yes, I think that is appropriate.

DR. OKRENT: Is that the workshop alluded to in the announcement of what was for 1979 by division of policy research and analysis of the National Science Foundation?

DR. VESELY: Yes.

DR. OKRENT: That would be the first in the series?

DR. VESELY: Yes.

DR. OKRENT: They talk about the National Academy of Sciences contracting to conduct one or more workshops.

DR. VESELY: Yes. Right now there is one. There may be a possibility of two.

DR. OKRENT: I think we would be interested in knowing not only when but what the detailed structure of the

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workshop is, as soon as it is convenient, because we certainly wouldn't want them to sponsor another one like it covering the same areas.

The only other question I have at the moment is you indicated the emphasis would be on the tools that one uses in making these decisions, rather than how you develop risk acceptance criteria and what should be the risk acceptance criteria, if I understood you correctly.

DR. VESELY: Yes.

DR. OKRENT: And that is, in fact, the same flavor as this one in Detroit and it is the same flavor as the last one I went to, the Mitre Corporation held it about February or something like that of last year. So maybe if we were going to hold one it would try to address the specific question of what our acceptance criteria and why as distinct from talking about the tools again, unless, in fact, that becomes an important part of the workshop the NAS is planning.

DR. VESELY: That was a part. I believe one day was to be spent on that, but I will get you more information on that. I think we still have some input on attempting to expand that area, if you would want it in this workshop.

DR. MARK: I thought it was also mentioned that this might be the first of more than one.

DR. VESELY: Yes.

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DR. MARK: Clearly, the thing you mentioned would follow a discussion of the tools. The discussion we heard yesterday, the staff, this particular project isn't awfully close to the point of being able to go to that second step.

DR. VESELY: I think that is a good point, because having this symposium or the second workshop on acceptability, acceptable risk criteria, having that in six months or seven or eight months would allow us and our project to propose criteria along with the others to be reviewed and criticized and critiqued in this symposia. I think that is a good suggestion, if you do have one, to hold it six months to eight months --

DR. MARK: It could be held under broader auspices than just the ACRS or NRC. There is a value to that, to have it fit in a general context, than to have it seem to be fomenting on only one point.

DR. OKRENT: Let's see. When do you think the IEEE would have their sets of criteria? They are beginning October 1? Six months from that would be --

DR. VESELY: That is our scheduled date.

DR. OKRENT: That would be April 1.

DR. VESELY: Yes. In the spring.

DR. OKRENT: So possibly a meeting in April would be something we should plan for. You do have to plan somewhat ahead and that would be reasonable timing,

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following on something in January or February. I like the idea you have of something broader. I think it would be good if the ACRS were somehow involved in arranging, helping to arrange — not the full arrangement, but helping to arrange the program for a — or a section of — for one in April.

If the NSF-National Academy group would like to think about possibly scheduling a second one in that area, that topic, in April — not the week of the ACRS meeting. I think it would be worth knowing soon. You have a way of following up on that?

DR. VESELY: Yes. I can let you know next week on that matter, and contact NSF.

DR. OKRENT: Fine. A reason for suggesting April is May may be a bad month for the ACRS members. There may be a visit to talk with other regulatory groups and other things. In any event, April would fit in with the general timing that we are talking about, it seems.

DR. MARK: Something not more than six months after the first would fit in and you mentioned May as being not a first choice, by far, but April is perhaps unduly specific. June — if this thing doesn't happen until February, then April is very close.

DR. OKRENT: Yes.

DR. VESELY: I will get back to you on that.

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PROF. KERR: I personally like June.

DR. OKRENT: You like June better than April?

PROF. KERR: Yes.

DR. MARK: Within six months of the other is a way of describing it.

DR. OKRENT: All right. June is, then, another time to look at and I guess in June you have two weeks that are probably not good weeks because the ACRS meeting, and there is an American Nuclear Society meeting that some people would be going to. I don't know what other meetings there are.

DR. VESELY: We can get those dates from Gary.

DR. OKRENT: Yes. May may turn out to be available for other reasons but right now I think if May is preferably left out from the timing, I think that would be useful to try to develop some preliminary ideas on. In other words, is there interest in some kind of a meeting and could it be done in some way under the NSF, NAS, NRC arrangement and if not we might try to go ahead and do it in another way. I think we certainly would want rather as broad input — but focused, if we could, towards the questions of risk acceptance criteria and I guess not only for the nuclear fuel cycle, in my opinion, but certainly including the nuclear fuel cycle in reactors.

PROF. KERR: Dave, it seems to me that it would

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be worthwhile for ACRS or some segment thereof to try to assemble information that would be useful. A symposium may be the best way to collect information but it also would be helpful if we knew what information we think might be available out there and what it is that would be useful to us.

I have jotted down a few things and this may all exist in pockets, but for example, how do physicians decide on treatments. That certainly has to be a risk benefit evaluation there. Has anyone looked at this? Probably somebody has. I don't mean individual physicians do it, necessarily, but collectively there must be some empirical basis for decisions that are made.

DR. OKRENT: There are some papers in the literature, the medical profession, but what I sometimes call medical technology or wnatever, where they have looked at the risks and benefits of specific procedures and --

PROF. KERR: It seems to me in spite of minor disagreements, in general there is public acceptance of the way in which physicians make decisions, generally. I don't know why; maybe I am even wrong, but I think there is. It would be interesting to know if this is so and if so, can one see why it is and on what basis are these decisions made.

In a related but different area, how does the Food

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and Drug Administration make its decisions? There is a stylized basis, and indeed, maybe all of this information is readily available. Is there a formal risk-benefit balancing methodology now used or is one under development?

Again in a somewhat related area, there are standards for microwave radiation. How were they arrived at? I know work has been done on this, I am just not familiar with the details of it.

And there are, of course, standards for nuclear radiation and one does now see in some discussions the way in which these are arrived at and effort to equalize or make similar the risks from nuclear radiation with other comparable risks. We may have as much information on that as we need, but it is an important part of the picture in reactor risks. Not necessarily the whole picture. I don't know.

But if there is enough history there that it would be useful, it would be well, perhaps, to collect it.

DR. OKRENT: We have other examples. Are there other examples that come to your mind?

PROF. KERR: Those are the things that I have, right now.

DR. OKRENT: I agree that we should have, in some relatively readable form, but in some detail, information of this sort. I don't know to what extent the staff expects to

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have this from the report that is to be written by January or the extent to which they think this will be available, on what time scale from the work Brookhaven is going to do.

DR. VESELY: I believe the handbook will contain special chapters on case studies, collecting as much information as the author has found available. I know they will specifically be looking at the Food and Drug Administration and the kinds of techniques and decisions made there, if only attempting to categorize it into these various -- for decision types of categories.

We expect the handbooks to contain many case histories and documentations of past decisions. And we will send that to the ACRS, again, when that comes out. Whether that is directly pertinent to quantitative and risk criteria for the nuclear industry is something else, because they are doing a broader search of qualitative -- many kinds of decision-making, attempting to categorize and examine these criteria. I don't think the emphasis is on quantitative balancing of risk versus benefits per se.

I should say, though --

PROF. KERR: Physicians have to do this all the time, consciously or unconsciously. And indeed, the medical profession is perhaps less exempt -- or more exempt from scrut_y. And they insist that they have a good bit of flexibility and freedom to do things that if the benefit is

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deemed appropriate, that one would avoid in another profession. Whether it is systmatic or logical, the risk benefit balancing is done daily by physicians.

DR. EDISON: There could be a mitigating factor here in the case of physicians, in that we rely on them, we depend on them almost like a father image to heal us. I have a dog who will do nearly anything I say because I feed him and make him happy and he knows where that food comes from and we also know who makes us well. So when it comes time for them to make a decision, we look to the physicians, I think, and trust them. We have to. We have no choice with something very personally important to us, our own personal health. I do know of instances, though, where this kind of statistical decision-making is made.

PROF. KERR: I would say that many people do, but it is not universal that physicians are trusted. There are people who won't have anything to do with physicians, for religious or other convictions. So it is not universal.

DR. EDISON: I recall an instance where I heard a physician quote a statistic as a basis for treatment. Something like when you have strep throat, you take penicillin for nine days, or in 80 percent or some of the cases you get rheumatic fever, so they do have in some instances those kinds of criteria.

DR. VESELY: I would like to make the suggestion

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that this may be one area where the ACRS fellows might perhaps work, because — work with us or cooperate. Those case documents that are not covered by the handbook, we are planning to have Brookhaven help collate and collect this information, but because of manpower limitations and other tasks, I think that would be helpful.

PROF. KERR: My comments were meant to contribute to what I mentioned earlier, which was sort of an assembly of information that we thought we might find useful before we embark on the ways of collecting the information. I wasn't suggesting that necessarily you —

DR. VESELY: Yes, but I think it is important, too, to collect information on these kinds of activities and just how to do this in the time frame we are talking about —

DR. OKRENT: Presumably you can make a fairly good guess on which agencies or which case studies will be in the handbook; is that right?

DR. VESELY: Right.

DR. OKRENT: So we can ascertain that at some time in the near future and think about how to proceed.

DR. VESELY: We can give that to you within several weeks, as a matter of fact, not until January -- we don't have to wait until January. I think several weeks, we could have that information.

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DR. OKRENT: All right, fine. I think that would be helpful.

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DR. VESELY: I have a question with regard to the new U.S. involvement, particularly the Europeans. There have been groups set up such as -- which we have been interacting with, and particularly, the CSNI group, which is the task force on world events in nuclear power plants, and they have several working groups. Several inputs address the acceptability criteria question in a very general manner. This may be a means of getting their input, since these groups, the European Common Market inputs. The Japanese are also involved in this, in these workshops, in this task force.

I think there is a structure already set up examing risk and nuclear risk in particularly the quantitative aspects of it that we might call upon and ask them to help us in this area.

DR. OKRENT: Does the CSNI group have a working group that is trying to develop quantitative risk acceptance criteria now?

DR. VESELY: No, not per se. They have working groups on decision theoretic approaches, models to use. But a group could be -- I believe a group could be assembled from the working groups already in existence. That would not take that much time.

DR. OKRENT: You are a member of the CSNI?

DR. VESELY: Yes. We could request them to

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address this question and see how they could help us.

DR. OKRENT: It seems worthwhile having you advise them of the effort that we are trying to undertake and see whether they wish to participate on the planning scale that we are thinking about.

DR. VESELY: We will probably try to meet with representatives of some of the regulatory groups in Europe and see whether they have specific thoughts in this area. I think that we don't take the place of the CSNI working group. That would be something different.

DR. OKRENT: To advise you of what is transpiring at the moment, we are looking at steps the ACRS should take with regard to the development of acceptance criteria, risk acceptance criteria.

One of the developments that has evolved this morning is the following: The NSF, NRC, and National Academy of Sciences are in the process of developing a workshop or symposium for January or February, which I think will relate to decision analysis and its relation to the risk acceptance criteria, or something like this. It is not now focused on quantitative risk acceptance criteria, but they might include that as part of the meeting. And we talked about the possibility of trying to arrange a later meeting, and April and June were mentioned, which would focus specifically on potential criteria for quantitative

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risk acceptance.

Vesely, who is working with the NSF NAS group, is going to look into whether they would be favorable toward a second symposium, and, if so, we might try to work jointly in trying to arrange a second one. If they decide that is not in their planning scale or whatever, we may try to arrange one in some other way. But it looks April through June would be a time when people would have developed some specific possible criteria.

DR. LEWIS: What is the objective of the NAS-NSF-NRC thing? Maybe I am asking something that has already been answered, in which case I shouldn't.

DR. VESELY: The objective of the present workshop is kind of general. It is to just present methods of using probabilistic techniques in decisionmaking kinds of things that you can do with various approaches, a workshop surveying state-of-the-art.

DR. LEWIS: But outside of the energy area, just in general?

DR. VESELY: In general. Now, the specific implications will tend to be focused on the energy nuclear problems. It is not going to address specifically acceptable risk criteria, numerical criteria. That was talked about as coming up, being discussed for a day. We are still in formulations.

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NSF has sent out brochures asking for inputs.

DR. LEWIS: I see.

DR. VESELY: We may still have time to formulate even the first workshop, and to focus it on specific issues that may be of interest, maybe of even more interest in this risk criteria area.

DR. MARK: In this connection, there has been a suggestion of this being the first of several workshops.

DR. VESELY: That's right. The first workshop covering the tools and models, and being the approaches, kinds of approaches you do use in attempting to use these tools in decisionmaking. And perhaps the second workshop would now focus on the acceptable risk criteria, per se.

DR. LEWIS: Which group at the Academy is involved?

DR. VESELY: I don't know.

DR. LEWIS: That is the National Academy of Sciences or Engineering?

DR. VESELY: Engineering.

DR. OKRENT: The announcement that came out of the NSF, dated August 1, 1979, says that the NSF has contracted with the National Academy of Sciences to conduct one or more workshops on risks in decisionmaking.

DR. VESELY: We are going to have -- if we are going to have a second workshop, we will have to go back and

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identify funding for that. It is about $50,000, on that order per workshop. The administrative costs. I don't see us having a problem with that if they would do that.

DR. OKRENT: I feel I have been through too many of these on the methods. I wish people would start trying to talk about possible answers. That's my own reaction.

DR. VESELY: The workshop is going to take some specific problems, questions of — some very practical questions, test intervals, how do you incorporate uncertainties, and what do you do with uncertainties actually calculated. It certainly isn't focusing on the risk criteria, per se.

DR. LEWIS: I guess I sort of share the uneasiness that Dave expressed. We are not, I hope, thinking of just waiting for the clear answers to our problems to come out of this series of symposia, I hope.

DR. VESELY: I hope not, either.

DR. OKRENT: Again, the application you mentioned is a nice tidy one. I don't think you need a big workshop for it. That's my own reaction.

If you still have a chance to modify the first one, I would suggest you think about the possibility of having more time on exploratory trial balloon, or whatever you want to say, approaches to the hard problem. We were trying to discuss things that the ACRS should be doing, and

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one of the things we had been thinking about is should we hold a symposium or make sure that there was one which focused on the heart of the matter.

I guess Vesely has said he would let us know within a relatively short time whether either this one — and I think that is unlikely — or a next one in April through June under the NSF-NAS auspices could do that. Because, if they were set up to do that, we could try to cooperate; if not, we might want to proceed in some other way.

DR. VESELY: We will contact NSF next week and let you know next week.

DR. LEWIS: It is just if the symposia are essentially friendly conventions on that decision theoretic times, that will not help us do our job. It may be fun, but it won't help us do our job.

DR. VESELY: In this specific area.

DR. LEWIS: That's right. Which is our job.

PROF. KERR: You missed Dr. Saunder's comment earlier, and I would urge that he give it to you in private, about decision theory. It was quite relevent to what you said.

(Laughter.)

DR. OKRENT: It seems to me, in fact, if we had a symposium and people were presenting trial-balloon criteria,

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we ought to have some governors there and some congressmen and so forth, who might not be prepared to advance their own proposals but they might be willing to react to other proposals.

To me, that would be a way of getting meaningful public input, if you could so arrange. And it seems that there are quite a few congressmen and senators interested in this area, and, I suspect, more and more governors.

DR. MARK: As long as it is held in their state.

DR. LEWIS: We might even have a few people who have made decisions.

(Laughter.)

DR. OKRENT: One of the things the subcommittee should think on is what other groups or individuals would be likely to gain access to or get input from or however you want to state it, and in what context.

Let me give an example. There may be questions like the following: Is there some relationship, as some people have said, between economic factors and what risk acceptance criteria are reasonable from a national point of view? In other words, is there some optimum amount of money that the nation should spend to adduce risk directly when -- and when you proceed you may be increasing risk by an unstable economy or whatever it is?

If we think that is a potentially relevant piece

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of information, is there some way we want to get such input and how? Another kind of question is: Are there legal constraints on how one could develop quantitative risk acceptance criteria or try to apply them, recognizing how the courts work and so forth? And that gets back to the workability, but one could try to think about that in a somewhat general way.

These are just kinds of things that come to mind. I think they, at least in my mind, relate to the overall question. I am not quite sure how we would get meaningful input from these things unless we have a way of inspiring it.

DR. LEWIS: I agree with you that those are the hard questions. In the inverse order, the legality is not clear in my mind. If you were to set criterion which essentially -- think in terms of cars specified that there shall be no more than a certain number of head-on collisions killing no more than a certain number of people as a criterion for automotive safety. It is not at all clear how that would fare in the cars when the few people who do get killed -- forgive me -- their relatives come in and claim that an essentially administrative-legal decision has been made to deprive them of life.

I am speaking as an ignoramus on these things. I don't know how they stand. It is closely related to the

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first issue, the question of economics. And, again, the professional decisionmakers just differ entirely from the governors and the general public on the question of whether human life is invaluable and where we make societal decisions that do doom people, as we do all the time by setting the speed limit at 55 instead of 25, for example. We never do it overtly; we don't do it on the basis of rational decision theory; we do it by default, essentially sweeping the subject into the taboo arena in talking about it.

PROF. KERR: We do have at least a minor example of such a criterion already in Appendix I of 10 CFR 50, in which we relate the reduction of calculated dose to cost. And if one relates that dose to the potential for fatalities, then there is a direct linkage. And nobody has taken it to court yet. And you are quite right: It might be attacked.

DR. LEWIS: I am not so much concerned in that case about court. I am concerned that where we do thing like that, we tend to put extremely high value -- inconscionable high value -- on human life. The standard example I always use is the amount you are allowed to spend to reduce the exposure to the public per man-rem. I forget what the number is. But if one extrapolates it to the prevention of cancer, it means we should be spending -- and

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again I forget the exact numbers —

DR. OKRENT: Five to 10 million dollars.

DR. LEWIS: Per life. And since 400,000 people in the United States die of cancer every year, then we should be spending many times the gross national product on the prevention of cancer, if we were serious about it.

It is an example of something we do without actually discussing it, because the only way we can discuss the value of human life is either in court where the decisions set a value in the end, or at cocktail parties.

DR. MARK: Or in church.

DR. LEWIS: Or in church. Where the value is easy to set. These are terribly important issues that we are going to point toward a quantitative criterion, because a quantitative criterion will be tantamount to saying you are going to let a certain amount of people get damaged.

PROF. KERR: Indeed, though, it seems to me that one might consider something analogous to Appendix I in which one either achieves a risk as low as reasonably achievable or reliability as high as reasonably achievable. I don't know which is better to talk about, either ALARA or AHARA.

DR. OKRENT: My guess, in fact, is, assume if one developed quantitative risk acceptance criteria, there would still be an ALARA principle over and above the minimum

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acceptance standards.

DR. LEWIS: The key word in ALARA is the last "on."

DR. OKRENT: Yes. How would we go about getting input on the legal or economic aspects. It won't occur automatically on a time scale that we are interested in. I suspect —

PROF. KERR: Are you talking specifically about nuclear power? At least one of the people, the man from Clark University you had on your list, certainly has been doing this kind of work. Whether it will appear as part of the handbook, I don't know.

DR. OKRENT: There was an economist from Harvard on the group, and a geographer from Clark. I don't know what we will get in the economics area, and I don't think there are any lawyers in the handbook preparation work.

DR. LEWIS: I think it would be very interesting to — and I say that only because I don't know how it would turn out — to pose a question, heaven help us, to the NRC general counsel, with a straw man, perhaps of the type that Bill was talking about yesterday: If one were to set nondeterministic but probabilistic acceptance criteria for reactors of the form "thou shalt demonstrate within the current state-of-the-art, which will be defined in this area as a regulation as time goes by, that your reactor will not

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kill more than 75 people in the next century," how do they think that would stand up, legally?

I would be very interested in the answer to that.

PROF. KERR: That is a very good suggestion, because it might keep a significant number of lawyers occupied for some significant time.

DR. LEWIS: And keep them out of other mischief, you mean?

PROF. KERR: I would not go that far, but "gainfully or usefully occupied."

(Laughter.)

DR. VESELY: We are planning to ask our counsel those kinds of questions. So, we are in the process of doing that anyhow.

DR. LEWIS: I am not being whimsical. I would be interested in the answer.

DR. VESELY: Yes.

MR. ROWSOME: I am getting an increasingly clear perception that what we need to do here is to draft verbally a set of rather abstract criteria, perhaps almost an ideal code of law, a proposed bill that might go through Congress, or a policy statement that might be issued by the White House that sets available ground rules but not quantitative criteria, that address issues such as the ALARA issue, a hard-and-fast criterion that nuclear risks will not be among

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the principal contributors to human mortality, plus an ALARA term, plus a bound on the ALARA, and interpretation of the "reasonable," together with perhaps a statement that we ought not to be leaving a particularly hazardous legacy to future generations; perhaps even touching on the proliferation dimension and so forth.

Then, if we can circulate a draft of that and start getting feedback from congressmen, from governors, from lawyers, we can start working on appendices that turn these into quantitative criteria.

But I think we have to get some concurrence on the ground rules, the conceptual framework within which we are working on quantitative criteria, so that we might try to draft a few pages of words that are really rather abstract and rather general policy statements which will serve as guidelines for quantitative work and also as points for broad public-political-legal policy review.

DR. LEWIS: There are examples in other arenas other than safety which people do use quantitative failure rate criteria. Lots of electronics or computers are qualified in terms of the NPDF, the mean time between failures, which is specified as to having to be larger than a certain amount, because failures normally accepted in some military equipment don't threaten human life. We find that a completely acceptable way to make specifications on these

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things.

On the other hand, in the safety arena, for example — I always use airplane wings — you have a deterministic criterion on the design of the wing, and just as in nuclear power, you make the deterministic criterion sufficiently stiff so that deep in your heart you know wings won't fall off too often. But you never quantify this latter point; you just go to the place at which you can't stand the traffic, and then you go on. And that is what we do in the nuclear industry.

So, we are talking about a major deviation, and I don't know other counter examples, all safety-related things.

DR. SAUNDERS: Since the early '70s, the fatigue calculations for wing strength have exceeded the deterministic strength by a factor of — the 747 had a strength documentation that was seven feet high, and we had a fatigue-life demonstration that was a nine-foot-high document. So, what — that was all probabilistic — so I don't think your last statement was totally correct.

DR. LEWIS: The documentation on fatigue may have been thicker. It doesn't necessarily mean it was wiser. And, in particular, the fatigue resesarch was probabilistic, but in the end, one was defined in deterministic fatigue lives for members, and we are rescheming airplanes because

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they have exceeded their fatigue life. So, in the end, it ended with a deterministic criteria.

DR. SAUNDERS: I think that is not quite correct, either.

DR. OKRENT: I would suggest you figure it out in the next 10 minutes while we have a break.

(Brief recess.)

DR. OKRENT: If we could reconvene.

I think there is some interest in looking a little bit further at the planned symposium that the National Academy will run under the auspices of NSF and NRC.

DR. LEWIS: It looks as if, from reading this letter, which I have never read before, that the initiative came from the House Committee on Science and Technology, and is -- who is the Chairman of that?

DR. OKRENT: George Brown is the Chairman of a Subcommittee, and he may also be Chairman of the full Committee.

DR. LEWIS: In any case, the motivation came from Science & Technology, and there is a reference to a report which you must have of this, which presumably was based on some hearings, of which there is probably also a transcript. It would certainly be very interesting to me to get a better view of the legislative intent here from the House report and from their hearings, see who they heard from, and find out, perhaps hope to bend this a little bit to be more useful to us.

Certainly, from the NSF letter, one reads all of the right concerns. So I would be very interested to see the House report on whatever the hearings were on which it was based.

DR. OKRENT: I would, too.

One of the members of the Subcommittee on Science,

Technology and Research, or whatever is the order, Congressman Ritter, I believe was his name, introduced a bill which, if it were to pass, would authorize the Office of Science and Technology, I believe, to undertake a program of comparative risk studies. This, of course, is in that vein. I don't know where the bill stands. It was introduced, I believe, in late July, and probably not very far along the Congressional path.

But it was referred to a Committee of which Congressman Ritter is a member.

DR. LEWIS: In addition, yesterday the House Committee on Science and Technology was holding hearings on the NSF budget, and I know the subject of yesterday's hearings was whether the NSF was doing enough to promote innovation and clear original thinking in the United States. I don't know how the hearings went.

DR. OKRENT: I guess there is some interest in knowing to what extent the symposium that the Academy is planning to hold in either January or February can still be modified, or whether the structure and the people who are going to give the papers and so forth is pretty well established.

DR. VESELY: We are going to have to talk to them and get back to you. We will do that next week, as I said.

DR. LEWIS: In addition, I guess, is it May, there will be a symposium on Three Mile Island, which is being run

by the New York Academy of Sciences, and there is some linkage. I guess the guy who is running it is an aide to Brown, who is either Chairman or on the Committee on Science and Technology. There is a Congressional linkage to that symposium, which will be entirely devoted to the implications of Three Mile Island.

There are things happening. It would be nice to link them and make them as productive as possible.

DR. OKRENT: I don't know about the one in May. I think I heard that the AAAS is having a session --I don't know whether it is a panel or what -- on Three Mile Island in its meeting in San Francisco in early January. But that is probably something different.

DR. LEWIS: Yes.

DR. OKRENT: I guess the sense of the Subcommittee's thinking, Dr. Vesely, is that if it is practical to take this first symposium and not leave it all on methods or on what I will call easier applications, and make it a forerunner of maybe a second symposium aiming toward what are the problems and what are the possibilities and what are the suggestions for risk acceptance criteria, there is interest in that direction from the ACRS. And --

DR. VESELY: We will see if we can modify the first symposium to focus on those questions.

DR. LEWIS: As I read the list of things in the NSF letter, they are certainly the kinds of things that we are

interested in. So if the symposium is directed toward answering those, I would be happy.

DR. OKRENT: Except as stated in this letter dated August 1, 1979, it talks about setting up an agenda for future research. I guess we are interested in trying to see if we can set up an agenda to address the problem this year. This is the difference.

DR. LEWIS: Right.

DR. OKRENT: Maybe they need two days on the first and two days on the second, or something to meet their needs.

Are there other comments with regard to that meeting?

PROF. KERR: Let me see the letter.

DR. OKRENT: One thing I have been wondering about myself is, should we consciously try to bring in groups, for example, like the Council on Environmental Quality to see if they have proposals on risk acceptance criteria; and if so, how?

DR. LEWIS: Who is the Chairman now?

DR. OKRENT: I'm not sure who the Chairman is. The previous Chairman was Mr. Warren, I think. I was under the impression he had -- Speth, Gus Speth. They seem to have broad interests in various technological systems, and I think it would be perhaps interesting to see what they might propose, and not strictly within the nuclear reactor framework or even necessarily in energy systems alone, since they have rather

broad interests.

One could ask whether they have some kind of a broad approach here. I don't know whether that is practical or how they would respond. Again, another speculative question: Are there -- I will use the word "public interest groups" -- that either have developed proposals in this area or would be interested in developing proposals, from the Environmental Defense Fund, for example, or others. Again, if I were to approach them, I think I would ask, do they have approaches within some broad context which would include nuclear reactors. At least initially, I would put the question to somebody limited to nuclear reactors. At least that would be my own attitude.

I don't know whether you think that is a potentially useful area to explore, and if so, how.

DR. VESELY: I think it is worth contacting the Council of Environmental Quality. Whether we will get anything from public interest groups in the time frame we are talking about, I don't know. I would have to see.

Perhaps a symposium might be the best method for getting their views. I think we will pursue that idea and see how -- see where it leads us.

DR. OKRENT: I would think the Sierra Club ought to be asked, myself. They are active in various matters that relate to public health and safety. Again, I have myself a

two-track sort of thought. I think it is quite possible that they will have opinions in this area. I think if they haven't developed opinions in this area, it would be appropriate for them to try to develop opinions in this area on some kind of broad scale, and not be making sort of recommendations in isolation of this aspect.

DR. VESELY: We were planning on asking the Sierra Club or certainly contacting them in the review stage. In the formulation stage we weren't planning to, but we certainly will contact them and see if they have any ideas.

I am very concerned that when you start asking all of these groups in the formulation stage, you really don't come up with anything. You spend all of your time talking to people and not formulating. I would like to split the formulation stage, where we actually can get as much input as possible in that time frame, but come up with some strong criteria, and then have these various groups focus on specific criteria and critique and review and give their opinions.

PROF. KERR: But if there is some mechanism that you can let people know what you are doing, so they can begin to give some thought -- if a group is hit cold with a formulation, eventually they can give enough thought to it to make intelligent comments. If they are hit cold with a week to respond, it is almost impossible.

DR. VESELY: We weren't planning on that time frame.

But what you are talking about or suggesting is perhaps getting out letters or contacting them of our intent. I think that is a very good idea. We will do that quite soon.

We are preparing a list of institutions and individuals and groups, and we would like to have your input on individuals, groups, that you see would be interested or might contribute, in order to further complete our list.

DR. LEWIS: Did the House Science and Technology approach OTA on the subject?

DR. VESELY: I don't know.

DR. LEWIS: That would seem to be a natural route.

DR. OKRENT: Getting back to this question of whether, in the formulation, it is useful to at least invite participation on a fairly broad base, I must say my own inclination is very much favorable toward trying to at least invite participation on a formulation.

In the first place, we may get some rather interesting suggestions. Secondly, I think people who have tried to formulate criteria that they might have to defend before their peers find themselves, I think, in a different position critiquing other people's formulations than if they have never tried to do it themselves.

So I think I can see a double kind of merit.

DR. VESELY: We will certainly consider that.

The intent of the handbook that we have been

developing, the general considerations on acceptable risk criteria, were to get inputs from various groups. So that was done to some extent.

I think with the time frame we are talking about, we will certainly try to get as much input as we can. But we are going to have to work that question as time goes on.

I am more concerned on getting the groups and task forces established first, and then bring these people into -- at some scheduled manner, with the convenience of the task force, to hear their views while they are formulating or if they choose.

I personally don't think that we can do as much in the formulation stage as we could in the review stage. But I think we will hear from several groups, several individuals, as many as we can in this formulation stage. But if we are talking six to eight months, it is going to be tight, and I see the review stage after that taking one, one and a half years, where again it isn't -- we will get their views and comments in, and then modify or update even the unacceptability criteria, the very simple criteria that we would be investigating.

I don't see this formulation stage as being, by no means attempting to get the final criteria to be used. I do want straw man criteria out, with all due respect to the word "straw" and its importance, to allow the critiques -- to allow

these individual groups to focus on proposed criteria.

We really are pushing, for this first six months, to get something specific out. You can spend two years hearing all of these people and their comments, and they are very important. And we have to focus them. We have to formulate some actual criteria, and that is going to depend on some people getting together and knocking their heads and saying -- and iterating and getting as much input as they can and coming up with some criteria to be reviewed, to be put to the public, to be put to Congressmen, to be iterated several times. But you have got to start to see -- we have been spending the past two years on getting all of these comments, and -- in this general program we have or this acceptability criteria, we have attempted to describe in that work the various concerns and proposals and considerations in a very general manner.

What we are trying to do now is to focus the question on specific criteria.

DR. MARK: A question concerning the OTA: Does anyone know if they have already conducted a study, not of nuclear acceptable risks, but of a closely analogous kind of question? I don't think they are worth asking unless they have done this study, because they are not really a strong technical group at all.

But if they have had a study, they have drawn in some other people and put together a document, and it usually

is respectable. And one could check and see if they have done anything which would seem to help put some of them in a position to have useful comments.

The same thing applies to the research arm of the Library of Congress, which does very nice work when they do it. If they have done anything of this sort, I don't know. But they do do a surprisingly wide range of things.

DR. OKRENT: I think Dr. Whipple may have a comment on part of this.

DR. WHIPPLE: I know a little bit of the recent history of what has happened at OTA. Several months ago they had an RFP out for, I guess, a half a million dollar study and five or six tasks on this. And in about a week or within the week that they were to announce the contracts, the whole project was cancelled because of budget miscalculations, as they put it.

And the project manager, who was Paul Brown, and I guess the department manager are both no longer with OTA. So they are presumably available.

But whatever knowledge there is on risk in OTA, I think, has vanished.

DR. OKRENT: That reminds me, Bill, I think the NRC might try to see whether Coates is interested in going with the NRC, if you are interested in looking at risk matters, because he has been thinking in the area off and on for quite

a while.

DR. VESELY: We can get his telephone number and address from Chris.

DR. WHIPPLE: I have his number at OTA. I am not sure when he left. It was some time in September.

DR. OKRENT: He was a pretty sharp critic of studies, in my experience.

Let me ask the Subcommittee: Do you think we should consider putting a notice in "Science" of some kind that we are undertaking something?

DR. LEWIS: Sure. Let's put a notice or alert somebody to interview you and write a damaging report. That will attract attention.

DR OKRENT: That would be even better.

(Laughter.)

DR. LEWIS: That's not a bad idea.

DR. SAUNDERS: I think that is very good.

DR. OKRENT: I suppose if we said you were going to second-guess the National Academy, that would be more --

(Laughter.)

DR. LEWIS: Quite seriously, we could certainly call somebody there and ask if they have any interest in this trend toward thinking about quantitative risk assessment. It is an interesting question. I don't remember "Science" having had an article of any kind on the general question of

quantitative risk or risk, and it might not hurt to suggest that they do that and let them decide whether they want to talk to you about it or not.

DR. OKRENT: Another kind of group that I have been wondering about how we could bring them in, if we could or should, are the institutions, Resources for the Future or the Brookings Institution or others like this. I don't know whether they only do things when they have contracts or whether they have free funds within their institutes, so that they can take on studies.

Since this is becoming a matter of public policy interest, in view of the Committee on Science and Technology asking NSF to get into it, it is not just now in fact an NRC question, nor was it ever. Again, I am not quite sure how one would approach such groups. But it seems to me it is something that might be worth thinking about.

DR. LEWIS: The Resources for the Future, of course, just published this big thick thing about energy risk.

DR. OKRENT: In fact, I wanted to get a copy of that. If you could look into that, Gary. I haven't seen it. Have you?

DR. LEWIS: Yes. It is that thick (Indicating). I have skimmed through it. It is not an unreasonable thing. It is superficial on a lot of things. I have devoted approximately ten minutes to rippling through it, so I can't give

you a definitive review yet.

The general trend of things we are talking about here, which are essentially to broaden the interest in the relevant groups in the general point of quantitative risk assessment, I think makes a lot of sense. If you let a lot of people know that you are pushing in this direction and thinking in these terms, you will get inputs automatically from people who are concerned. And so, the more people in the media and in the Congress that one can involve, even in a peripheral way, I think the better off we will be.

I don't think anything can happen in the White House until after October 25th, if then.

DR. MARK: This year or next?

DR. LEWIS: This year.

(Laughter.)

DR. LEWIS: I said "if then".

(Laughter.)

DR. LEWIS: But OSTP would be a natural place to generate some interest in these matters, and it would be worth doing.

DR. OKRENT: I guess we will have to think about how one could make these groups aware of what we are trying to do and find out their potential interest. Maybe we will think on it and Vesely will think on it, and we will get together on this sort of thing.

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Now, I am assuming that within the ACRS Subcommittee, its fellows, consultants, will be trying to develop our own whatever you want to call it, frameworks for quantitative risk acceptance criteria or ideas, and so forth, and not depend only on the IEEE to come in with a set and so forth.

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(Pause.)

DR. LEWIS: Do you have any impression that when you speak to people outside our own narrow little world and use the term, "quantitative risk acceptance criteria," they have any idea what you are talking about?

DR. OKRENT: It probably depends on how narrow your world is.

DR. LEWIS: You're right. I used an ill-defined, non-quantitative term.

DR. OKRENT: There are people in the FDA using quantitative criteria, for example.

DR. LEWIS: Yes.

DR. OKRENT: And elsewhere in the government.

PROF. KERR: I think the terminology and interest is growing, so that the population of people is not as small as it was three or four years ago.

DR. LEWIS: Even within FDA, the criteria that go with carcinogenesis in food additives are not quantitative criteria. They have this, if I understand it correctly, criteria in which is the material has been shown to cause cancer in laboratory animals, it must be banned.

There is no acceptable level.

PROF. KERR: That is fairly quantitative, zero.

(Laughter.)

DR. OKRENT: But there are proposals and they may,

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in fact, be using these for interim guidance that if something is thought to produce not more than one in one million chance of producing serious adverse effects on the population, it is acceptable.

I have heard someone from FDA state this, in fact, at the MITRE workshop last winter. Now how they use this vis-a-vis the Delaney clause, I don't know. They may apply it to things where the Delaney clause is not applicable because it is a naturally occurring substance, or whatever.

I can't recall.

In other words, in the context of broad exposure to the population.

DR. LEWIS: I was speaking to the Delaney issue.

DR. OKRENT: In fact there is a series of articles that Chris Whipple called to my attention by Peter Hutt, who was chief or general counsel of the FDA, or something like this, and assistant commissioner, if I recall correctly, some years back, in which he has, in fact, if I understand it correctly, proposed that they start using risk analysis in trying to decide what to do in the FDA and not try to use an all or nothing or qualitative approach, or whatever.

DR. WHIPPLE: That is in the October '78 Journal of Food, Drug and Cosmetic Law.

DR. LEWIS: I don't take that.

DR. OKRENT: It is a thoughtful series of articles.

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PROF. KERR: Is it too thick to consider Xerox?

DR. OKRENT: No.

PROF. KERR: I would be interested in a copy.

DR. OKRENT: I will have a copy. I will get Gary to make it for the subcommittee. In fact, I would think that we ought to try to find out whether someone like Peter Hutt and others like him who have been thinking seriously on the matter, are interested enough that they would like to be active in what we are trying to do here.

Again, I don't know quite how to go at this. He is a member of a Washington law firm and I don't anticipate the ACRS can pay his normal hourly fee.

(Laughter.)

PROF. KERR: Maybe we could pay him for one hour sometime.

(Laughter.)

DR. LEWIS: It doesn't even pay our normal hourly fee.

(Laughter.)

DR. LEWIS: The thing that is emerging is that there are a number of other agencies that are grappling with the same problem. And it may be that after the TMI dust settles -- again, if it ever does -- some kind of coordinated activity, either through OTA or through OSTP -- might make a lot of sense. It would help to bring the issues to the

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front in a broader context, with perhaps a little less of the emotional involvement that nuclear energy now has.

It might help to make for a rational treatment of the problem.

DR. MARK: The AMA made — had a group of its own studying the hazards of various power techniques. I don't know that they really did any work or whether they just read the Inhaber report and transcribed it, or what.

But they are interested in acceptability and if the chairman of that group were still interested in the subject, there could be a possible question there. It is not the whole AMA that one would want to tangle with at all.

DR. OKRENT: I wonder if there is anyone in the government who thinks about the question that Kerr raised: What are the risks that are accepted and what are the trade-offs in the practice of medicine?

My experience five or six years ago was it was very hard to find anyone in the government or the AMA who was a source of information, let alone criteria in that area.

DR. LEWIS: Is Hardin Jones still alive? I feel funny asking. He spent a lot of his career dealing with these medical statistical questions and learned a lot, a very fine — he was at Berkeley.

DR. OKRENT: I don't know.

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DR. LEWIS: The medical profession has dealt with some of these. For example, they went through the agony of recommending and then retracting on routine mammography on statistical -- on a statistical basis.

So that kind of issue has arisen and there are, even in the medical profession, some fairly sophisticated people who have dealt with these problems.

It would be nice to get a hold of them.

PROF. KERR: I think it is a source of useful information if we know where to look.

DR. OKRENT: There is a group within the framework of the National Academy which is called Council on Medicine, or something of this sort, which includes a lot of rather knowledgeable people, and that might be a place, among others, to look into that area.

DR. WHIPPLE: A suggestion on that point. I think for the main part of medical practice, the profession has the luxury of seeking simply to choose the minimal risk avenue available to them rather than trying to worry about costs or the availability of services.

But there might be some application -- the name of the committee sticks in my mind: The Committee on the Ethics of Experimentation with Human Subjects.

I believe there is a small department at Harvard that deals with those issues. I have seen references to it,

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but not much more beyond that.

I would certainly think that they have dealt with these issues.

DR. OKRENT: That would be relevant, I think. Are there other comments or suggestions of the subcommittee members on any of these areas?

(No response.)

I guess what we will have to do is try to see what Dr. Vesely develops along these lines in the next week or two, and then see what additional steps we think we should take.

I will assume that the subcommittee is in favor of trying to assure a fairly broad input, if possible.

DR. LEWIS: Yes.

PROF. KERR: I think we are obligated to give good advice. Dave, since I missed the early part of yesterday's meeting, was there a discussion of what DOE may be doing in this area?

DR. OKRENT: There was no discussion of what DOE may be doing in this area yesterday.

PROF. KERR: Do we know? I assume at some point we should try to find out. And I would guess they may be doing something, but I personally do not know what they are doing.

DR. OKRENT: In fact, when they made a list of

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agencies that we ought to contact, it happens DOE was the first one I wrote down, although I didn't mention it in the ones we were discussing today.

I think it is a good point. I am not sure that there is a single contact point at DOE.

PROF. KERR: It seems to me that there ought to be at least an informal liaison between NRC is doing — maybe it exists.

DR. VESELY: It does exist. It is not doing anything on acceptable risk criteria, per se.

PROF. KERR: Are they doing anything on comparative risks, for example, that would be useful to you as a point of reference?

DR. VESELY: They have some projects, one through Sandia, for example, that are looking at the use of probabilstic techniques in licensing and making suggestions which are touching on the need for quantitative criteria and how you might express those.

But not really, to our knowledge, not really in any concentrated manner.

We are certainly going to involve DOE as well as EPA as much as we can.

PROF. KERR: I am surprised, I guess, that somebody is not at least thinking about comparative risks among various alternative energy sources. It seems to me that that is one

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of the things of which you want to at least be conscious. Cost is important, but risks, environmental consequences, all of these, it seems to me, have to enter into a decision.

I would expect that somebody in the department would be giving some thought to that. You think perhaps not?

DR. VESELY: I think that they are doing that. There have been studies already — for example, WASH-1400 was used as the reactor accident portion of the nuclear fuel cycle risk, as compared with the coal cycle.

We have, again — the reason that we had the coal versus nuclear fuel cycle comparative risk performed is that we could not find any studies that had been performed that were ongoing dealing directly with the issues that we were concerned with.

They are informed of what we are doing.

The DOE risk analysis effort is not a very large effort. It has been scaled down considerably from the previous years. I think the NRC efforts are almost a factor of two, or magnitude larger.

So the DOE efforts have been scaled down to looking at suggestions or methods, relatively small efforts, to our knowledge.

But we certainly will keep them — want them involved and I think they are important to be involved, as well as EPA.

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DR. OKRENT: We might consider whether ACRS should write to the Secretary of DOE telling them we are planning to look at this and welcoming their participation. And in fact, there are other agencies.

It is my impression, based on some conversation I had with somebody at one of the Washington consulting groups some time ago, that they have done a study for the Department of Agriculture on —

DR. SAUNDERS: Pesticides.

DR. OKRENT: Risks and so forth. So some departments that you wouldn't ordinarily think about in these connections not only have interests, but are developing, let's say, a background.

Actually, the Department of Agriculture has certain aspects of — that relate to the use of chemicals in society, for example, under its aegis jointly with others.

And the Corps of Engineers, for example, is a group that is currently looking at the safety of dams, formerly a law passed by Congress and signed by the President.

So in addition to the ones they build, I think they have somewhat broader responsibilities.

So there are many groups.

The Coast Guard has been thinking about risk acceptance criteria, although I don't know that they have developed quantitative ones. The Department of Transportation,

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of course, was the father organization to the Coast Guard.

Maybe we should move on to such other topics as we would like to cover during this subcommittee meeting.

I guess we should see if there are other avenues than the ones mentioned that the subcommittee members think we should explore. At least have in mind at this time.

Are there any comments any of the members of the public would like to make in connection with what they have heard or haven't heard?

(No response.)

I don't see any volunteers. If there are no further thoughts in this area, maybe we might go back to the questions that we were addressing yesterday concerning the probabilistic assessment staff, or whatever it is, PAS program and priorities to see whether we have further questions in that area, or ideas as to what information we would like to develop and so forth.

It is my impression from what I heard Mr. Rowsome say prior to the beginning of the meeting today that he thinks that in a couple of months or so they will have further thoughts on their priorities and where they think the effort will be developed for FY '80 and '81.

So that what we have heard yesterday is not necessarily the last word.

MR. ROWSOME: Indeed, it's not. We are looking now

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at the beginning of the process of overhauling the priorities. And while that will be an ongoing effort, we hope to have preliminary results, as you suggest, in the order of a month or two to guide Fiscal '80 and to guide us until we have a chance to digest the implications of the Keminy Commission report.

And we will be making further re-assessments, then, and perhaps further re-assessments again after the Rogovin committee's report comes out slated for the end of the year.

So this will be an interative process.

DR. OKRENT: One question that somebody will be addressing, presumably, within the NRC is the overall balance of effort in research, is it the proper one?

By that I don't mean within what is now considered the PAS program, what are the priorities. But if one looks at all of the areas in which the NRC is doing research, or might be doing research, and I will use the term "research" loosely, is that appropriate?

In other words, is the existing emphasis and level of expenditure appropriate?

Now does PAS do any thinking along those lines or does it think only within the framework of its area of responsibility and budget allocations?

MR. ROWSOME: No, we think on broader lines. So, the

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whole of research is rethinking research priorities. We are participating in that effort, not only just as an element of research in the organizational sense, but because the risk assessment work gives us tools to help prioritize the experimental program and whatnot.

So that we do have an advisory role. We have input on priorities to the other — to the divisions. That has manifested itself thus far in two or three forms.

For example, the plan to set up a three-way coordinating committee to coordinate code development, the experimental program and the PAS, to provide PAS input into that prioritization.

But I would have imagined that it will grow to include other things in time, perhaps not on a scale of one or two months, but certainly on the long-range scale, yes.

DR. OKRENT: There have been suggestions from time to time that more research should be done on transients and small LOCAs. That is one example.

There have been suggestions that more should be done on systems design questions and things related to your new program, whose initials I have forgotten.

MR. ROWSOME: IREP.

DR. OKRENT: There have been suggestions that more should be done to evaluate methods of possibly mitigating serious accidents. Now you can do more by putting in more

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money, or if you don't have more money, you have to cut something out.

Now has anybody looked at whether the existing effort on large LOCAs, which is a —

MR. ROWSOME: The whole planning effort since Three Mile Island since the very concept of a TMI supplement surfaced well before I came aboard, has been to move in the direction of small LOCAs transients, exploring a wider range of accident scenarios and systems effects.

And I believe we are moving on this as hard and as fast as the institutional inertia permits. We are not just sitting back and saying that we will go on the same path and we will do a little over here and a little over there.

It is really a profound reorganization, reorientation, the effort.

That's my impression.

DR. OKRENT: I am sure that there are changes in thinking going on. What I am not quite sure of is how the Office of Research will decide how to recommend an allocation of funds, or how it should be done. And also, whether the piece of change is an optimum.

MR. ROWSOME: Saul gave Bob Budnitz the responsibility of chairing the working group on research priorities with me and Tom Murley and Tom Arseno on the group, and we have met a couple of times. We haven't really been able to digest the

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problem yet. But we are working on rethinking the very way priorities are set, the way planning is done on research because we have perceived, as you perceive, that it would be seductively easy to slip into just the institution inertia and not rethink these things deeply enough.

So that the process is slow. But I think it is doing a reassessment from the ground up.

DR. OKRENT: This group you just mentioned, did they have some time scale in which they are supposed to arrive at something, or just an ongoing process?

MR. ROWSOME: I don't recall Saul's guidelines in particular. I think he wanted some feedback within a month. But I believe he expected, and we certainly interpreted that to be preliminary and not final.

DR. MARK: Would you be saying that these deliberations could affect the cuts in distribution of cuts in the FY '80 package?

MR. ROWSOME: Very plausibly, yes, I would expect so.

DR. LEWIS: Bob, himself, of course, was on the risk assessment review group which came out very strongly for revamping the research program. So we can't expect him to be unsympathetic to the idea.

BY BWH

DR. OKRENT: I guess it may be worthwhile suggesting that the office of research think about its coming in to meet with the Subcommittee on Reactor Safety Research prior to November ACRS meeting, in view of what you have just said, because the ACRS is supposed to be trying to address priorities in your program. There is a Subcommittee meeting scheduled — I think right now it is probably on election day, but the specific day may be adjusted depending on some other Subcommittee meetings. I can't recall, but it is one of the days before the November ACRS meeting, and it appears from what you have said that that would be a time when you would have developed some thoughts.

It would also be shortly after the Keminy Commission report is out, so we would have had a chance to see, at least in a brief way, what it has to say, assuming there are enough copies printed that copies reach the ACRS on the first printing.

PROF. KERR: You are not referring to the 4th of December, clearly?

DR. OKRENT: November. Did I say December?

DR. LEWIS: You did say November.

PROF. KERR: There is a December 4 meeting scheduled, I note.

DR. OKRENT: That's true. There is also one scheduled for November and one for December.

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PROF. KERR: It is probably Tuesday, the 6th, then.

DR. OKRENT: It is subject to change. It could end up being the 5th or the 7th if something else should up on the 6th. In other words — because we have the possibility of scheduling a meeting with foreign regulatory people in that period, and we would shift to accommodate them.

Anyway, why don't you, if you can, advise Saul and ask Gary to ask Tom McCreless to advise Saul Levine of our interest in being able to talk about priorities. Saul is going to be out of the country, I think, that week. That may present a problem to him. My impression from conversation I had with him is he thought he was going to be away during the water reactor safety resesarch program because there was some meeting in Europe that he needed to participate in. I can't recall what it is. But perhaps there would have been enough discussion that we can meet with his alter egos or whatever it they are called.

I think it would be helpful if we could have a first round of discussion of the broader priority question, not only within PAS.

MR. ROWSOME: That sounds quite appropriate. I don't know that we will have written material in the sense of a report by that time, but we can certainly present to

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you the state of our thinking and state of our progress, give you a progress report, at that point.

DR. OKRENT: Coming back to the specific program we heard yesterday within PAS, are there comments or questions of subcommmittee members?

PROF. KERR: We did ask for further information — what was to be done about floods — didn't we?

DR. OKRENT: There was discussion on the matter of floods. Were you here when we talked about it?

PROF. KERR: Given the numbers that were preliminary, we should keep in touch with that, it seems to me. I think the numbers — the emphasis was that these were preliminary numbers, so we simply need to make certain that we follow that particular investigation.

DR. OKRENT: I agree.

DR. LEWIS: I notice in the IREP statement, in phase 1, the survey would develop a data bank to cover the susceptibility of all operating reactors of the top five dominant sequences in WASH-1400. Is there something magic about five, or is that a budget allocation?

DR. EDISON: There is nothing magic about five. If you go back and look at WASH-1400 and look at the probabilities associated with the sequences, they are different for every sequence.

DR. LEWIS: Yes.

by BWH

DR. EDISON: And what we did was to look at where it looked like there was somewhat of a jump down to the next sequence in probability —

DR. LEWIS: You didn't let people grade exams —

DR. EDISON: I have never had to do that, but I have had suspicions that that is the way it is done.

DR. LEWIS: Yes, that is the way it is done. When you say dominant sequence, what does that mean, specifically?

DR. EDISON: We refer to dominant contributors to risk. And in these instances, in the case of the PWR, one of the dominant sequences was the loss of off-site power. In the case of the boiler, it was an ATWS type of sequence. Those are two of the five sequences.

DR. LEWIS: I was wondering, since every sequence has both a probability and a consequence associated with it, I was wondering how you were weighting the probability and consequence in deciding what was dominant.

DR. EDISON: The consequence was not weighted in.

DR. MARK: Was not the release category?

DR. EDISON: Yes.

DR. MARK: You are going to weight them by curies estimated to be released with probability. It will be curies per year per sequence?

DR. EDISON: That's right.

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DR. LEWIS: Is that the criterion?

DR. EDISON: Yes, it is really the release.

DR. LEWIS: You are multiplying — I am trying to find out quantitatively how you are doing it — you are taking from WASH-1400 for every sequence or for the top sequences the probability, multiplying by the number of curies release associated with that probability, and using that as a figure or merit or demerit?

DR. EDISON: We did not multiply. We simply went into the main report, chapter 5, and pulled out the summary table and took the top five probability events.

DR. MARK: You took the probability.

DR. EDISON: That's right.

DR. MARK: In the case of ATWS for PWRs —

DR. EDISON: But let me —

DR. MARK: This is a very misleading number.

DR. EDISON: We did not go down to category 7 releases, for example. We didn't do that.

DR. LEWIS: So, what you took was the highest probability sequences in the high-release categories, but not weighting for which release category?

DR. EDISON: That's right. And there seemed to be a rather natural break-off there. And furthermore, I would like to say that I don't think that our programs are wedded to five sequences.

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DR. LEWIS: I was sure of that. I was just wondering what made you stop at five.

But, in any case, what you are not doing, what I infer, is giving extra weighting in your considerations to lower probability, higher release sequences?

DR. EDISON: That's right.

DR. LEWIS: So, if you take the probabilities in the top high-release sequences, you will automatically gravitate away from the low-probability high-consequence events because the probabilities do tend to go up as the release categories — as the releases decrease?

DR. EDISON: That's right.

DR. OKRENT: Let me raise a couple of harder questions. The first is the PAS — is the PAS doing any thinking on what should be the design basis for hydrogen generation in light water reactors?

MR. ROWSOME: Only to the extent that it is in the list of things for which a priority will be assigned but we have not given it an assignment.

DR. OKRENT: As you know, this is a fairly short-term issue. It can be made a long-term issue by not doing anything.

(Laughter.)

MR. ROWSOME: I fully expect that it will come up in our discussions with the Lessons Learned Task Force, and,

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based on how much they intend to do, how far they intend to take charge of setting the policy in that regard and to what extent they ask our assistance, we will then plan what we want to do, whether it should be short term or long term, in response to their needs.

DR. OKRENT: Suppose somebody came up to you and said, "If you are able to develop recommended positions in three weeks, for consideration, not necessarily the position, but here is a possible position that is not completely implausible out of hand," would you be able to come up with such?

MR. ROWSOME: We have approached a number of problems in that kind of crash-program fashion, giving a handful of people who have worked extensively in risk assessment and reliability to simply sit down and hash out ideas in a collegial fashion, very much the way you all do when you are preparing your report, and prepare recommendations in the course of a day's work or two days' work or three days' work.

Such things have been done in the past. They could be done in the future. They do eat into our man-hours somewhat. But they do get results on a short time scale, results that usually need a lot of further follow-up work. It is possible to give that treatment to this issue. We may, in fact, do that. I don't know. I will wait until --

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my inclination would be to wait until I hear what Roger Mattson wants us to do in this context before I would go ahead.

DR. OKRENT: Let me pursue this a little bit. It seems to me, if you think about this question generically, you end up having to consider "should I think about a spectrum of accidents, and, if I don't, why don't I; and if I do, how do I?" And in that regard, you get into questions in addition to hydrogen generation. In fact, at least as of now, we expect that this topic will be a major part of the afternoon session of the Three Mile Island accident implication subcommittee meeting, the day before the October meeting of ACRS.

It would seem to me that the people in PAS have been thinking things like accidents beyond design basis; they have been thinking about other kinds of containment approaches, various things of this sort.

I guess it is not clear to me that this discussion, which I expect to occur for the first time and it may not be the last time, in the near term to occur then, it is not clear to me why we should have the benefit of the thinking of the group in PAS.

Now, is there any reason that you can't respond to a request from an ACRS subcommittee as to what your thoughts are in that area, whether or not the long-term Lessons

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Learned Task Force has asked you by then?

MR. ROWSOME: I think that is a reasonable request, yes.

DR. MARK: Assuming it is the first subcommittee to issue an invitation.

DR. OKRENT: Why don't you assume you have that request?

MR. ROWSOME: The October meeting, do you know the date offhand?

DR. LEWIS: 3rd.

DR. OKRENT: I would expect it to be the afternoon of the 3rd, unless there is a change in our current plans.

MR. ROWSOME: Do you want to leave it quite open-ended? Do you want to provide a narrower focus? Do you want us to simply talk about the topology of the hydrogen issue and our perspectives on that, or hypothetical regulatory positions? That is a little more difficult.

DR. OKRENT: It is the latter, I think, that would be the most helpful, or alternative hypothetical regulatory positions and their pros and cons. And you would inevitably, I guess, bring in those aspects of the topology of the accident as was relevant.

But I think a description only of the quality of the accidents is not what we need.

DR. MARK: Are you thinking, Dave, primarily of

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hydrogen that gets released to containment or hydrogen that gets stuffing up the pipes in the primary pressure system?

DR. OKRENT: I guess my own thinking is that the issue that is on the table is the containment response in these events. And we are thinking, by the way, of BWRs ice condensers. All right?

MR. ROWSOME: Yes.

DR. OKRENT: I think that could be interesting.

DR. SAUNDERS: Can I ask about the use of the word "topology." Is it as precise in its technical content as Mr. Vesely's use of the word "cybernetic development" yesterday?

(Laughter.)

MR. ROWSOME: I used it in the sense that Hal Lewis used it in the risk assessment review group report. I used it to mean a consideration of the natural break points, the natural ensembles, the natural partition of the space of these considerations in the packages that are more readily digestible.

DR. SAUNDERS: I think that is better than the use of the "cybernetic."

DR. LEWIS: It is a precise mathematical use of the term.

DR. SAUNDERS: That is much better. I am sorry about that, Bill.

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DR. VESELY: That's quite all right.

DR. SAUNDERS: Thank you.

DR. OKRENT: While I am exploring how the ACRS can benefit from possible input from the PAS staff, let me ask a couple of other questions. On that same day, in fact, in the morning, one of the things we expect to look at is are there implications for BWRs that arise out of the TMI-2 accident. Presumably, implications may arise in a more broad perspective than just that you would like to have heaters on the pressurizers, which, of course, doesn't exist in the BWR, that sort of thing. Is that an area in which the PAS has thought, or could develop ideas?

MR. ROWSOME: Your question is a little too general for me to know how to answer it. I did mention yesterday that the Bulletins and Orders Task Force has been thinking about BWRs and has come to us for help in that context. They had some concerns involving ECCS actuation and the adequacy of instrumentation, in part motivated by TMI and in part motivated by Oyster Creek. And they wanted our help in specifying studies to be done by the owners group, the licensee.

We are not working on something that matches in scope the generality of your question.

DR. OKRENT: In effect, you could say that my question could be rephrased in part. Have you thought about

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what the Bulletins and Orders group should be asking you to tell them? In other words, they pose a specific question to you.

MR. ROWSOME: Yes.

DR. OKRENT: And then —

MR. ROWSOME: We have no coherent effort to do that. I guess the answer to your question is "No." On the other hand, in a still more abstract sense, it goes well beyond BWRs, in particular, and we are looking at the lessons we think we had learned, things we should be more alert to in system reliability analysis, probabilistic safety analysis, risk assessment, the things in our scope of responsibility.

A lot of thought has been given to that, and it is reflected in things like the planning for the TMI supplement and the continuing work on priorities, so that we are doing work that is both more general than your question and much more specific than your question, but nothing quite incongruence with the scope of your question.

DR. OKRENT: Is there some reason why the question that I pose is something that PAS shouldn't contribute to?

MR. ROWSOME: This is a topological issue, if you will. We really have to limit what we try to do, and it may be that that is a topological package in this space of concerns, a subspace that would be a natural and would be

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appropriate. But I would have to give that more thought. We clearly can't do everything.

And one of the central problems in our priority effort is to think about how best to package what we do to obtain the maximum benefit with available resources, and the issue of whether that is an appropriate scope will certainly come up.

DR. OKRENT: I am raising this as a specific example of a more general question in my own mind. I think we earlier at this meeting and in previous meetings have been discussing the potential for increasing the capability of the people in NRR — the reactor licensing, whatever their designation is — to include probabilistic aspects in their decisionmaking and thinking and so forth.

At the moment, I have to assume this is going to take time, and it may be really a matter of years before we have a considerable body of people in the licensing division who have the benefit of the different things that go into a WASH-1400 study. And it is going to be even harder to have individuals in the licensing group who know all of the different things that go into WASH-1400 types of studies, let alone part.

It is not clear to me that the licensing process has to or should wait in getting the possible benefits from such thinking until they have a substantial body of people

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in licensing that could -- that are experienced in this area, have a background.

If my premise is correct, one is led to ask what are other avenues for the short term?

MR. ROWSOME: We have suggested some, and I would be delighted to get your suggestions for others. We have in mind the executive seminar, the course, the improved course, for line office personnel, and the process of getting their feet wet by getting their participation in the integrated reliability evaluation program. Those are our inspirations at the moment. They are already, of course, perceiving the need to move in this direction, as demonstrated by the Lessons Learned Task Force report and some of the Bulletins and Orders Task Force initiatives.

If you have further inspirations on how we can help this process along, we would welcome them.

DR. OKRENT: I am not sure you will welcome them.

MR. ROWSOME: We will welcome the ideas. We may not embrace them with enthusiasm, but we do welcome any inspirations you may come up with.

DR. OKRENT: I recognize that you have a limited number of personnel, and that is not an easy thing to get around. It is, I guess, not clear to me that with regard to the licensing process that the system should be one wherein PAS at this stage waits to be asked for contribution in this

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specific area. That certainly should be available to the licensing people.

But it seems to me that if the knowledge or insight or so forth that the PAS people have developed could contribute to improving the safety of the reactors currently being reviewed in other ways than they are currently being asked to help, we should not miss that opportunity, and we should ask ourselves is there some practical way in which to do this. And I suspect that there are practical ways. It would infringe on things that might lead to NRR getting some suggestions that they didn't like or they say we already have enough to do and you are giving us more things to think about and that would eat into what certain people were doing with their time.

But I am not so concerned about that, because if that were accepted as something PAS could do, I think it could be done in terms of an augmented PAS each time you take on something you get some people from NRR who are in the part of it as for the auxiliary feedwater study, which, again, was a restricted one.

I use this question of are there generic implications of BWRs as one example, and, of course, I gave an earlier example of the hydrogen question. I think the thinking in my mind need not be that restrictive, even.

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MR. ROWSOME: I share that perception. The thinking I have done about it thus far had extended principally to the items I touched on yesterday, to wit: I felt our role involved three principal functions: the direct service to the line offices, the applications of the state of the art to the licensing process, which is in fairly close congruence with what you are talking about and the improvements in the state of the art in reliability and probabilistic safety analysis. The developmental research focus of our efforts.

I had been thinking in terms of trying to make our research product more accessible and useful, more visible to the line offices and to the nuclear community, but I think you are pointing in a direction, perhaps, that you had suggested when you asked us yesterday to keep you informed of the problems we turn up, that we should institute a policy of sending warning flags over to NRR at the same time.

Something along that line makes great sense. It will have to be given a good deal of thought so that we don't find our time taken up by judgments of, is this appropriate to package and send? But at the same time, to get the benefits of what insights and perceptions we do come up with in the course of our work out to you and to NRR and I&E and whatnot by a more direct route than we have been

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doing it in the past. I think that is a good point, and something I ought to think about — we ought to think about.

DR. OKRENT: That is a related question, at least as I raised it yesterday. It was in the context of — from your research program things development, did you have criteria for such notifications and so forth, and what, I guess, I am exploring here a little bit is whether you should take a more overt role in trying to develop considerations for the licensing activity. Not on all activities, but there may be certain kinds of licensing questions where PAS may have a contribution to make that is unique.

MR. ROWSOME: I am inclined to agree with you. I don't believe it is within my responsibility to make that kind of a policy decision, to what extent we do this, but I am sure I can have some influence over it. My inclination would be to encourage that we develop these paths and utilize them. That is a good point.

DR. OKRENT: Let's see if I can ask one more question along these lines, going successively, I guess, to more general questions. There is a considerable amount of momentum in research programs just as there is a certain amount of momentum in licensing programs. And you have been talking about how people are thinking about how should we change the research program and -- I guess what isn't clear

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to me is how one asks the question on a broad enough scale. In other words, if we didn't have momentum already that we felt somehow obligated to, where do we think we should be doing research, since that is an obligation of the office of research? And would it look like what we now have? And if it would look radically different, on what basis do we defend the maintaining — most of the existing momentum? It is not clear to me —

MR. ROWSOME: Momentum doesn't have to be defended. It is just there. It can be fought, perhaps, but —

DR. OKRENT: I am familiar with the problems that arise from an existing momentum, but I am wondering whether anyone inside the NRC — and I will exclude committees that the NRC has set up like the Rogovin Committee — are trying to look at the research program starting, as I say, first from a position unbiased by what the existing program is.

Are you aware of whether there is that kind of an effort?

MR. ROWSOME: Saul, Bob, Tom Murley and Frank Arsenо and I are all thinking along those lines. It is only formally organized in the sense that Saul delegated to Bob to get us all together to work on it.

To address the other dimensions of your question, I am not aware of such efforts elsewhere in the Commission,

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but I could easily be ignorant of them if they exist.

DR. OKRENT: I'm out of questions. Other subcommittee members nave any questions for the NRC?

DR. MARK: I would like to go back to a point, and I am not sure that there is real content to it, but on those five favored sequences from WASH-1400, I really think you might look at the sequences you pick and compare them with the ones, the numbers, what would stand out if you multiplied release quantities, factors, times probabilities.

It is awfully hard for me to see why one would not think of including, if it should be a different one, the one that gave the highest number in that sense.

MR. ROWSOME: I think that is in there.

DR. MARK: It wasn't in from the way it was described.

MR. ROWSOME: It is not in the sense that anyone sat down and formally did a calculation and multiplied it out. I think these sequences stand out above background in the high consequence release categories by enough that it is unambigous. Without using the formal calculation.

DR. MARK: I wanted to be sure that you picked the ones that would have the largest estimated release, so that having considered the ones with high probability and big release with those probabilities so small --

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MR. ROWSOME: It is without doubt these are the dominant sequences for the plants but I don't believe there is any ambiguity that these are risk-dominant in WASH-1400.

DR. EDISON: These were all in the top three release categories, as I recall. We didn't make a big issue out of trying to pick the exact five or the exact six or whatever. We went through the table. If you go through release categories four and five it drops down considerably compared to the first three categories, and when you get out to categories six and seven --

DR. MARK: What drops down?

DR. EDISON: The probabilities.

DR. MARK: But the release goes up -- excuse me, the release goes down, the probability goes up.

DR. EDISON: I am saying in categories four and five that is not the case.

DR. MARK: My only point is it seems to me it would be worth checking this aspect, that the biggest net annual release is included in the set you are proposing to use for comparisons.

MR. ROWSOME: I think that is unambiguous.

DR. MARK: It is unambiguous as long as you look for it.

DR. EDISON: One of the preliminary results we have seen in the methods application program is that some of

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the other plants have different dominant sequences. For example, one we reported on in the past has been with the ice condenser containment, the possibility of floor drains not being removed during refueling is a dominant sequence. So we are not really locked into a magic number of five that are. We are going to be considering —

DR. MARK: Yes. I fully agree with that. I am just remembering an instance in which within the staff, not PAS, frequency being high, associated with release which was low and used therefore to guide a lot of statements on the fact that they must be sure not to have this event happen more often than once in a blue moon, whereas all of the release really came from a different one and the probability was low.

DR. EDISON: It was smoothed over?

DR. MARK: Didn't look at anything but the probability. That is the reason for the remark and it may just be as you say. We have checked it both ways and this is still the same set.

DR. EDISON: The thought is in our mind, in picking these sequences out, the thought was risk.

DR. OKRENT: I wonder if I could look at that program in a different perspective. Let me preface the question by a comment. I have the feeling that the program you are planning is the right program to have done in 1975

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and '76.

DR. EDISON: '73.

DR. OKRENT: Well, no. After you had WASH-1400. I am not so sure it is adequate for today. I think it is useful today, don't misunderstand me. So within some bigger picture it is not so clear to me that this is adequate for today in view of the time it will take, and so forth.

DR. SAUNDERS: I think it is adequate. It may not be as good as can be obtained.

DR. EDISON: Adequate for what?

DR. OKRENT: From the point of view of the public health and safety. I am measuring that way, not on any other criteria, for some of the reasons we talked about during the meeting. In the next two years, you will get quite a bit of information, but perhaps not as much as one would like to have. And maybe I am wrong. Maybe somebody has thought this through as to just what is the possible contribution to safety that can come from studies of this sort, or an expanded set of studies, or this supplement to the related types of studies and so forth.

This is, in fact, an adequate program, or more than adequate, or it may not even be needed from the public safety point of view. I am giving you my own personal opinion that, as I understand what you are going to do, it is neither optimum nor adequate. And when I use the term

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adequate I think by an expanded program beyond what you have now -- and I don't mean expanded by your office, necessarily -- I think one could probably find other matters of interest. And let's say the existing reactors that one would find in an earlier stage and therefore in a probabilistic way reduce the public risk.

I don't know that there is any highly probable accident lurking there, but I am just speculating in my own mind that one might be able to find contributors that were of importance, statistically, at an earlier stage.

DR. EDISON: This would not be our only program. There would be other efforts in parallel and other fronts.

DR. OKRENT: I can only form an opinion now in terms of the programs I have heard discussed as planned or likely to be undertaken. And -- at least to me, there is a broader question of whether over and above this study, assuming it is followed along the lines you have outlined, over and above it makes sense for other kinds of -- for additional studies to be done.

I think you could say that it not for PAS, your research office. In other words, so if it is really a bigger picture, a broader -- a wider body that would have to decide this -- but I think the subcommitee ought to think on this.

DR. LEWIS: Yes.

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DR. OKRENT: And see whether in fact we wish to in some way develop any suggestions in this area.

MR. ROWSOME: My own thoughts along those lines are these: I think we need as a tool for use in prioritizing research something that might be called value impact or -- if you will pardon the expression, decision theoretic tools, that allow us to tackle the question of uncertainties in a broader sense than simply finding distributions and estimates of confidence bounds.

I think we need to organize an effort to question our premises, to ask ourselves questions like, We believe issue thus-and-so is put to bed or is of this magnitude or is dominant. Let's entertain the hypothesis that that is wrong, that that is dead wrong and it is either much worse or it may be much better than we think.

If that is true, why might it be true? Where are there weak spots and loopholes in the robustness of our thinking? And what would be the consequences of our being severly wrong? If we have grossly over-estimated something, are we throwing resources away that could be better used somewhere we have grossly estimated some hazard phenomena, some generic category of the safety issue? Are we missing the boat by not probing for the tender spots in our understanding and to weight these factors together -- pardon the expression, in a decision theoretic kind of way,

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and use the perspectives we get from this in the sense of the topology of this question that emerges from this study as one of the tools in setting our priorities, in orienting our focus.

And since I think we need that tool now, at the outset, I think that is something we ought to give the crash program treatment to. We ought to sit down, think about it and get something to use today and identify where we need better tools and define that as a medium-scale research effort in itself, and where we need long range effort to scope that out, too.

DR. OKRENT: Are there other questions or comments?

(No response.)

DR. OKRENT: If not, I guess we will plan on having contacts in the near term to learn how things are progressing on the various topics we have discussed, to see when it is appropriate for another meeting of this subcommittee on any of the three major topics that we talked about. We won't try to anticipate right now when that will be.

Did I see a hand at the back?

MR. APOSTOLAKIS: I would like to make a comment on something that was said yesterday. Dr. Vesely was talking about uncertainties and he said that you can do it

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either using classical statistics or Bayesian methods. And you can do it in a Bayesian way, you can propagate and then Dr. Saunders said, you can do that but it is meaningless. I would like to say that there are a considerable number of people who think otherwise. That's all.

DR. SAUNDERS: I certainly think that's true.

(Laughter.)

MR. APOSTOLAKIS: Fine.

DR. OKRENT: If there are no other —

DR. MARK: Did you wish to have understood that all Bayesian renditions are meaningless?

DR. SAUNDERS: I think not.

DR. OKRENT: Any other comments?

(No response.)

DR. OKRENT: In that case, I will adjourn this meeting.

(Whereupon, at 12:00 noon, the meeting was adjourned.)