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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

CRBR SUBCOMMITTEE and

STRUCTURES AND MATERIALS WORKING GROUP

Room 1046
1717 H Street, N.W.
Washington, D.C.

Thursday, ~~May~~ March 17, 1983

The Subcommittee on CRBR and Structures and Materials Working Group met, pursuant to notice and recess, at 8:30 a.m., Dr. Max W. Carbon, Chairman of the Subcommittee on CRBR, presiding.

ACRS MEMBERS PRESENT:

M. Carbon, Chairman
J. Mark, Member

ACRS CONSULTANTS PRESENT:

Mr. Abdel-Khalik
W. Kastenberg
W. Lipinski
Z. Zudans

DESIGNATED FEDERAL EMPLOYEE:

P. Boehnert

NRC STAFF PRESENT:

R. Stark
W. Morris
C. Bell
C. Allen

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delete B. White

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PROCEEDINGS

(8:35 a.m.)

MR. CARBON: The meeting will now to order. This is a continuation of the Advisory Committee on Reactor Safeguards Subcommittee on CRBR and Structures and Materials Working Group. My name is Carbon, the subcommittee chairman. The purpose of this second-day meeting will be devoted to the discussion of the HCDA issue for CRBR. We will proceed with the meeting, and I'll call upon Mr. Curtis Allen of the NRC Staff.

MR. ALLEN: Good morning. As Dr. Carbon indicated, this subject today is the HCDA energetics. The staff's presentation will be given essentially by Dr. Theofanous and Dr. Bell. They'll discuss the results of their work on developming -- that they've done for the staff in developing an assessment of the energetics in CDAs and the CRBRP.

As you can see, the report is extensive, it's sitting on the table in front of you, and it will be a long presentation, and we urge patience on your part in hearing the presentation. It's a complex story and they have a lot of things to say.

I have a few introductory comments I'd like to make before I summarize the status of the review and introduce them. These are largely a few comments about the staff's approach to CDA evaluations in general, and the role of CDA energetics in evaluating CDAs in particular.

As we discussed yesterday, the staff is also of the opinion that CDAs should be classed as beyond the design basis events. However, we do feel, as was also mentioned yesterday, that because they are considered to be potentially significant risk contributors, and until we understand that potential better, we feel it's prudent to be able to accomodate such events. And that's why we evaluate CDAs. We evaluate them so we can determine reasonable accommodation requirements, and the capabilities of the system to accommodate those events. Another reason is to develop information for use in making judgments about the risk of CDAs, and that's essentially the staff's attitude in that regard.

Before I turn to the role of energetics in CDA evaluation in particular, we had a discussion yesterday about important differences between the TMBDB and SMBDB scenarios, and I thought I'd just try to illustrate that point a little bit this morning. To do that, I've taken a figure from the applicant's CRBRP-3 TMBDB scenario. It's a sketch that illustrates the reactor cavity domain and the reactor containment building environment domain. The reactor vessel and the core sits down inside the reactor cavity, and these are concrete walls, they're steel-lined, et cetera. This is the operating floor (indicating), this is the head axis area and here is the reactor vessel head.

The reactor containment building environment is isolated from the below operating floor structures; it is sealed off from the containment primary heat transport system.

Given a CDA, when a CDA starts developing in the core, as long as the head remains intact, the CDA is constrained within the cavity region, and if the CDA sequence is non-energetic or is of a small energetics that it doesn't challenge the integrity of the head, the progression of the CDA remains inside the cavity region. The core debris winds up penetrating the vessel, and it flushes the sodium and the debris down under the cavity, and then it progressing goes along the TMEDB long-term scenario that you heard about last week.

If the energetics are large enough to fail the head, then that opens the direct communication between the disrupted or disrupting core and the reactor containment building environment early in the transient. And you heard yesterday these things develop in the order of 15 to 20 seconds; the challenge is developed in that range, to the head. So that should that happen, if a sodium spray fire results, you might over-pressurize and challenge the integrity of the containment.

If the energetics were very large -- and this is very unlikely, -- missiles associated with a head failure like that could also present a challenge.

This is the -- the potential for head failure, therefore, is the reason for focusing on the capability -- the energy absorption capability of the head. That's the only point I wanted to make about that. And that's all this slide says. That this is -- really, the role of energetics in CDAs is to determine if CDAs can lead to an early containment challenge or early containment failure.

And the reactor head failure provides a threshold test to examine that question. And we heard yesterday and we believe that the head can accomodate the impact of a sodium slug having kinetic energy of 75 megajoules. I emphasize it can because that's contingent on the applicant resolution of the design -- the proposed design capability deficiency that we heard discussed yesterday.

Therefore, that provides a good way to test the energetic potential of CDAs; against the energy absorption capability of the reactor vessel head.

Finally, turning to a summary of where we are in this review, the applicant's analyses were given in CRBRP's 3 Volume 1 and GPR 523. We had a number of meetings with the applicant. They culminated in questions which were submitted to the applicant and they provided answers to us. At that time, we initiated a special task to develop an independent assessment of CDA energetics. This was directed by Dr. Theofanous and Dr. Bell. You're going to hear a lot more about that shortly.

They gave a preliminary progress report to the subcommittee back in November of 1982; the final report issued March 11, 1983, and it provides the technical basis of that report; it provides the technical basis for the staff's conclusions on the CDA energetics. One of the conclusions is that we recommend that the plenum fission gas compaction potential be eliminated by design, and the applicants agreed to address this concern. You'll hear a good deal about that in the course of the presentation. And assuming the elimination of that concern, we believe that the proposed structural design capability, the 661 megajoules and 75 megajoules slug kinetic impact energy is adequate, and given this capability we believe that the vessel head failure through the CDA energetics is physically unreasonable.

I know there was a lot of confusion about the 661 75 number, yesterday, a lot of discussion about that, and I think we have the right people here today to discuss that. Theo and Charlie have done an awful lot of work in this area, and I think at the end of the day everybody in the room should have a very clear picture of how these numbers are generated and how they're applied.

And I think with that, I would turn the meeting over to Theo.

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MR. THEOFANOUS: First of all, my apologies for not being able to give you this document 1 or 2 days in advance so you would have time to read it before this presentation. In view of this fact, this morning I would like to give you a kind of an overview, walk you through this document in what is an overview way and, hopefully, this will help you as you try to go through it. It will help you to find things where they are and so on, and especially try to identify for yourselves the ideas that you are more interested in to look at.

The document appears to be very lengthy one, and it is for this reason that we separated the figures from the text. And the text is only 250 pages, and the rest is figures. And by the time the final figures are drawn and incorporated in the text, the actual size of it will be quite a bit reduced, the visual size will be reduced.

As you realize, we are concerned about the initial impact of people being afraid even to look at it, by it being so big. But we feel that we did not go to any unnecessarily lengthy discussions there. In fact, in some areas maybe you might find it somewhat skimpy.

The point is that the whole area to be assessed correctly is rather broad, has a lot of facets to it. It is very complicated. And in order for one to be able to convey exactly what one has done, it cannot be done in just a few words. It is rather complicated.

The structure of the report, I would say a couple of words as far as how the report can be read or how we would recommend that you read it. By looking at the table of contents you will find out that we have broken it down, the whole thing, into just a few important steps. And the report is organized so that each one of the steps is more or less like a unit, so that you, by looking at the chapter section, you can go back just going like this through the figures and the text, side by side. You can identify the section that you are interested in. And then within that, the whole thing is a complete unit. Hopefully, that will help you to read it. And so even the page numbers were according to units.

So to start with then, this is our goal today is to give you a summary of this independent assessment which we have just completed and is put into this NUREG document of 5224.

And to start off, we want to take a look at the different kinds of core-disruptive accidents. As you realize, there are many, many different ways by which one can enter into a coremelt situation. And in an attempt to assess energetics, one needs to somehow abstract the complexity and be able to come up with some generic way of looking at things. And one major classification among the different core-disruptive accidents is between protected and unprotected ones. This is important because if the reactor is scrammed, the heating rates, of course, are very low. And one needs to sort of different kinds of sorts of assumptions for getting this core to melt. And normally, this involves waiting for a very, very long time without cooling.

The heatup rates are on the order of a degree per second. On the other hand, in the unprotected situations, the power continues to be normal and the heating rates are higher, and suddenly this implies a set of phenomena that is very different between those two cases.

Now, furthermore, the unprotected accidents can be further classified into two major categories, depending as to whether the sodium is in the core when the disruption takes place versus when the sodium is outside of the core.

And this is -- now, don't laugh at what I am telling you here -- but we have the genetic CDAs associated with lesser flow accident, while the other ones involve the sodium in, coming under the name of transient overpower.

Basically, here we are losing pumping capability while the core continues to produce power at normal levels. That leads to core disruption that is preceded by sodium voiding. Now, it so happens that in some of those reactors, the reactivity effects associated with the loss of sodium are such that the loss of flow eventually winds up as a transient overpower. But at least the beginning phenomenology is specific to sodium out of the core as the core begins to disrupt.

On the other extent here, the pumps continue to pump normal flow through the core while the power for some reason increases. And if that power increases rapidly enough, it leads to fuel melting while the sodium is still running by the pins, and therefore we begin to have a disruption with the sodium in.

In this case here, referred to as the loss of heat sink accident, the power is decay heat levels, and typically about 1 percent by the time that one is concerned about core disruption. And in fact, under these conditions, as you will see later, the sodium must have gone out of the core; otherwise, the core would not have disrupted. And what is more, because of the very low heating rates, even the steel has had time to melt and get out of the core. So that is still our situation. It still is out.

Yes?

MR. MARK: Theo, that 1 degree Kelvin per second of the LOHS is applicable approximately what time after --

MR. THEOFANOUS: This is several hours, like 10 after.

MR. MARK: Well, then for several hours it goes through a factor of 10 in the decay heat.

MR. THEOFANOUS: Yes.

MR. MARK: So if --

MR. THEOFANOUS: This number here is applicable to something like 10 hours, to answer your question.

MR. MARK: Okay. So it's much higher at time zero?

MR. THEOFANOUS: Well, yes, but it will drop so quickly that within just a few seconds, it is to a few degrees, maybe not out, but a few degrees.

MR. MARK: Yes. Okay.

MR. THEOFANOUS: This is just to show the order of magnitude, it is not really to look at the detail of the numbers.

Now, in addition, we have, of course, other possibilities. And one that is quite prominent is the very, very severe earthquakes, seismic, severe seismic we also set out. Here they have combinations of this as caused by the seismic event, and we might also have other situations such as, for example, fracture of the core support. This is an accident postulated and studied to quite a great extent by the British, in particular, for the last few years. We think that the failure of the core support is in the category of very, very low probability events, that does not deserve very detailed evaluation.

Fuel failure propagation also is a very -- has been a very favorite kind of scenario. From the very early days people have studied that for quite a long time. And the general conclusion there is that this is not really a problem from the point of view of achieving core disruption from fuel failure propagation.

As a possibility of transient overpower followed by a loss-of-flow accident, this would be coming about because when one attempts to scram the reactor, the scram signal is sent, the trip signal is sent both to the pumps and to the scram system. And one might conceive of a situation where the scram system fails to work but the trip to the pump works. We examined that,and we think that it is really in the noise level of probabilities. It is such a low probability that it does not need to be considered.

The point here is that if one is left completely free to think of eveything that comes to mind, one can always, I guess, construct situations that may be more severe than the ones that we are going to talk to you about today. We have looked at the whole spectrum of things, and we have tried to discriminate between things that we consider to be worthwhile from the point of view of maybe in the way, way out low probability range, contribute something to the risk, versus other events that maybe are in the hypothetical sense can cause higher consequences, but if one looks a the probabilities, one comes to the conclusion that this is so unlikely that really fall in the category of events of the earth opening up or things of this type.

MR. LIPINSKI: Will you consider, on the last item there, the DOS, what do you consider low probability? What is the number? What number do you have in mind?

MR. THEOFANOUS: This one was examined from the point of view of the instrumentation in the reactor, and the people that looked at that at SAI came to the conclusion that it is several orders of magnitude probability than TOP. The number was not determined absolutely for this event, but it was determined in conjunction with how much lower probability versus having a straight TOP. In other words, you try to find out how many additional failures have to take place in order to cause this event.

MR. LIPINSKI: Okay.

MR. THEOFANOUS: You find out that you need all kinds of common-mode failures between electrical and mechanical systems, and it is this nature of things that make that very low.

MR. LIPINSKI: That's right. And to answer that question again, what is the probability of the TOP?

MR. THEOFANOUS: Well, I think that for this question you might get different answers depending upon whom you ask, and it is not part of our charter to look at the probabilities of initiators.

MR. LIPINSKI: Then how can you answer my question, because the TOP event, the last one, is the probability of failure to scram.

MR. THEOFANOUS: Yes.

MR. LIPINSKI: And given the fact that we have a number and a probability of failure to scram, then that sets this extremely low probability numerically.

MR. THEOFANOUS: Yes.

MR. LIPINSKI: And now if you are saying that that is several orders of magnitude lower --

MR. THEOFANOUS: Yes.

MR. LIPINSKI: -- then I can spot your TOP event.

MR. THEOFANOUS: You can what?

MR. LIPINSKI: The transient overpower event probability itself without the failure to scram.

MR. THEOFANOUS: Well, typically, this unprotected events have probabilities of the order of 10-5. That is -- and somebody might say 10-6. There are a few studies that have been prepared by Sandia and the other by SAI, and they are in the category of 10-5, 10-4, 10-6. It is, maybe all told, in magnitudes around 10-5.

This level of probability coupled with the potential consequences, at least as these people see those possibilities of CDAs, to assess that, a judgment was made many years ago and people have followed that through, that they have to believe that.

Now, if you couple on top of that additional events that make these events even lower probability and bring them down to 10-6, 10-7, 10-8, that is where you begin to lose a lot of -- and that is how we discriminate between the things that we do want to consider and things that we don't want to consider.

Now, I don't want to get stuck in the details or of actually is it the TOP, is it 10-5 or 10-6, because even the people actually working on that have disagreements. What I want to say is that we consider the TOP is an event that has to be looked at. However, by considering this combination, it is still so much lower probability than this first kind of an event that we decided not to look at it.

MR. LIPINSKI: That depends on what kinds of numbers you have been given for that TOP LOF event and as to whether you believe them.

MR. THEOFANOUS: Well --

MR. LIPINSKI: Because that's based on the analysis of the hardware and the system as it stands.

MR. THEOFANOUS: Right. Well, I think that it is referenced in the report; in fact, the analysis that was done with respect to this and the considerations, all the detailed arguments with respect to the CRBR system in particular.

Now, based on those considerations, this combination is, in our opinion, much lower probability, several orders of magnitude lower than the straight TOP or the straight LOF. All right. So less than that. This is the basis for excluding that, independent of what the TOP LOF probabilities are. But nevertheless, we know that these straight events are already very low probability. So I think your argument would be very appropriate if we had -- if those were probable events or not very probable, maybe in the order of 10-2, 10-3, then maybe by losing another one or two orders of magnitude, we are not totally in the incredible domain.

But by those being already very extremely improbable, coupled on the top of that, another two or three orders of magnitude less in probability, that's what makes it kind of go over the hill. So that's the logic of it.

Another category that falls in the same situation is in the TOP itself. We are going to come to that later on in the discussion. You -- the question is what kind of TOPs do you want to consider. Obviously, you know that if you are going to pull out a bank of control rods, it's going to be much more probable than pulling out all the controls at the same time, all the rods.

So some discrimination has to be made because the difference of probability between all those different events are significant -- are not insignificant.

So we looked into that aspect of it, and in fact we determined almost like in a crossroad of a reasonable thing to look at it, to look at versus again events that they are of such a low probability that the ramp rates are really -- can be considered hypothetical.

MR. LIPINSKI: Let me make one comment to the chairman. Based on this TOP LOF event, the numbers are based on the ATWS probabilities, because this is a two-part system and basically, we have ATWS with this TOP event failure to respond, we also have the pump tripping for the LOF. And assuming that one part of the circ acts and the other does not, the probability of the failure to scram is now directly related to the ATWS issue. And early arguments, we had a set of data, and the industry was saying we have rectification. Now with the latest information based on parts that are being examined and that cannot scram, we have effectively negative rectification, saying we haven't learned a thing in terms of maintaining this hardware, and so far the discussion has not been held as to what the new number is for the data base.

And to assume that we have a nice number on this plant based on the analysis that has been done, everything is contingent upon proper maintenance. And the industry's record right now is poor in that respect.

So I would very much question what somebody tells you for the probability of failure to scram.

MR. THEOFANOUS: Well, would you encourage us then to take this kind of an event within the spectrum of events that it doesn't need to be considered as far as CDA in this?

MR. LIPINSKI: I don't think it's any jore or less probable than the TOP itself. In this particular event, it's mechanically possible based on the way the plant is built.

MR. THEOFANOUS: Obviously, I don't know why you say that, because you can identify additional systems it's safe to fail for this event for this combination to happen versus the initial event on the straight event. And I think it's intuitive on this event, although I am not asking you to say that, but if you required more systems to fail requiring extreme common-mode failures, there has to be a lower probability. Maybe not as much as you are saying, but it has to be lower probability. I can't see how it can be higher probability or even the same.

MR. LIPINSKI: Okay. But to say it's extremely low compared to the others you are throwing out?

MR. THEOFANOUS: I think that -- well, I was not planning to get into this discussion here today, so I really cannot give you much more. But I think what I can do is I can get you the document on which you are really basing this judgment.

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MR. LIPINSKI: All right.

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MR. GROSS: If I could add a word, I'm not sure that Mr. Lipinski was at the full committee meeting last week where we discussed the scram breakers. I think part of his concern comes from the Salem event.

MR. LIPINSKI: That's right.

MR. GROSS: The ATWS event.

MR. CARBON: Can you speak a little louder?

MR. GROSS: Last week at the full committee meeting we did present to the full committee the significant differences in our design versus the typical scram breakers such as those that are at the Salem plant. Our scram breakers are entirely different. They are much smaller. They are fully enclosed and sealed. They are not anywhere near as sensitive to maintenance in order to continue.

Only one of the two systems, and we have two, of course, independent systems, and only one of those systems is dependent upon scram breakers. So I recognize your concern, but I believe the differences in our design are significant and need to be taken into consideration.

MR. ZUDANS: But this meeting has nothing to do with Walt's correction. You are talking about TOP and he is talking about the combination of two.

MR. GROSS: I understand, but I believe part of his concern is that he doesn't believe that the TOP, that the ATWS is as low a probability as perhaps some people felt it was

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previously.

MR. ZUDANS: Not really. The way I heard the question, he wants to know simply what is the probability of a combination of these events. That is all.

MR. LIPINSKI: The point is you have got some theoretical numbers you are calculating based on the design you see on paper in front of you. But given the fact that that design is completely overriden by maintenance procedures and we end up with a new set of numbers, how can I believe the set of numbers you are presenting from a nice, clean analysis assuming perfect maintenance?

MR. MARK: There is a difference between the system here and the light-water reactors, which have only one set of rods. This has two redundant rods. So the ATWS number as it might be modified for light-water reactors has nothing to do with this.

MR. LIPINSKI: No, but their number as might be modified due to poor maintenance is the number of interest.

MR. MARK: One has to look at the system and ask if it is subject to the same diseases. If it's a sealed unit, then maintenance may in fact be irrelevant. Of the switch bars, anyway.

MR. LIPINSKI: Not looking at the details of the hardware, all I am doing is making a general statement -- maybe it was the specifics -- that the vulnerability is not

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there.

MR. MARK: Look, you are absolutely right, it has to be relooked at with the same question as Salem, the light-water things. If they apply, then they have to be accepted, and if they don't apply, then it needs to be pointed out.

MR. THEOFANOUS: To continue, then, in the following we are going to take a look at the loss of flow, TOP and loss of heat sink, in that order. Those of you who were here last time in November, you remember that we put most of our emphasis in the loss of flow accident.

We did that because, number one, we could identify areas that we were concerned with as far as energetics, and really this case not being so for the other two situations.

Having now completed the analysis, we think that this is a correct judgment, and the presentation and the report is really then with this emphasis on the loss of flow. This is not to say, however, that the unique aspects of the other two initiators were not considered, and in fact we think there are some interesting aspects associated with them and we are going to talk about them later today.

But the emphasis is on the loss of flow, and the overall approach here is to first consider the structural capability of the vessel. Obviously, you need to have a yardstick against which to measure energetic behavior, and that yardstick is provided by the structural capability of the primary system.

As it turns out in this case, in addition to the head just talked about, there is another like a mini-containment inside the containment, and that is this core cage, we call it, and this is made out of structures, the core support structure, the core barrel and the upper internal structure. It is a cage because it is not completely closed up. Things can leak out, but structurally, if you like to put a big force, a high-pressure source in there, that source does not manifest itself on the outside unless there is some failure on this cage, on this structure.

Now, here we establish this yardstick, then. We go through the disruption, through the core disruption phenomenology, and we attempt to establish a general framework. That is, in very, very rough lines, we like to know how the accident progresses, what are the different steps, the different configurations that the core might find itself as it goes from initiation to the termination, to the end of this accident. This is what we call framework.

Here the discussion can be viewed or the whole thing can be viewed in two major parts. One is the initiating phase. That is where the disruption begins. The geometry is only beginning to be lost and therefore is much better defined from that point of view. And then the other one we call the disruption phase, and that takes on from the point where the

j-1-3,5

fuel actually begins to move at some rather large distances until the action is terminated.

One word about termination. Our task is to look at energetics. Therefore, when we say termination, we refer to the termination of energetic potential. What it means is the achievement of permanently subcritical state and not termination of the accident. Even after the core has gone through this termination of the energetic concerns, as you already know from previous discussions, it has to go through in the containment eventually somehow by decay heat.

Now then, within this framework we look at this framework and try to identify if there is any potential for energetic behavior. So we search, then, for energetic events within this general phenomenology, and we can classify them in terms of two classes, again, of energetic events. One is pertinent to the initiating phase. Here you will recall from the last discussion, in order to produce energetic events, we need to have rather forced fuel motions. Plain gravity will not do it here because the fuel just begins to disrupt and this disruption is rather incoherent because of the power distribution in the core.

The only way to do it, then, is to have forced fuel motion. That means you require pressure somehow. On the other hand, in the disruption phase a lot of the fuel of the core moves, therefore coherence might be achieved, and

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therefore even gravity alone can do the job of producing energetics and that is the kind of situation that we call recriticality.

So we identify, then, initiating phase of energetics and recriticality. But then independently of whether we identify the possibility of such energetic events, we somehow have to see these come to a termination, come to an end, and obviously there are other non-energetic or mild ways by which the fuel can get out of the core, and we try to identify and see what is the potential for that.

So again, we search over this whole range of phenomenology, the whole accident progression and identify paths through which the fuel actually can get out of the core. When 40 percent of the fuel gets out, you can call it energetically terminated.

At the beginning, again, in the initiating phase, the only available parts for the fuel to get out is the coolant passages. This is up and down through the actual blankets and up and down. Those are very tiny, small paths and the fuel as it tries to get out might freeze there and might plug them. So one of the important considerations is are those parts available.

Furthermore, it is possible that those might plug up before any fuel attempts, and this is when this cladding melts before the fuel, at least in the initial stages of the

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loss of flow accident, and this cladding can be moved upwards and downwards and can plug and freeze and completely isolate from that point of view these actual paths.

Later on when the fuel actually continues to move in what we call extended motions and continues to produce heat, not only through decay heat but, we believe, also through neutronic activity, through recriticalities, because of the continuous presence of gravity, the sub-assembly walls begin to become attacked and melt, and therefore the gaps between the sub-assemblies now become available, and that is what is shown here.

So in the disruption phase, new paths become available for the fuel to get out. Again, those paths are about 1000 degrees lower temperature than the fuel melting temperature, a very large delta t. And again, will those paths allow for the fuel to get out before freezing occurs and therefore isolation.

Finally, the control assemblies, upon melting of the walls, provide additional paths for the fuel to escape, and again similar considerations apply here.

One important thing that I will come to again and again and I want to impress upon you is that as the disruption phase increases, as we go from the initial first pins to fail and first little fuel to begin to move, as we go down to whole subassembly molten, maybe more than one

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subassemblies melting together and molten, more and more of those paths become available and eventually you will see that the actual system becomes overwhelmed by these relief paths. It is almost like trying to keep water in a sieve, and you know that is very difficult.

Here I am showing the sections or units within this loss of flow, which is Chapter 2 in the report, that addresses some of those areas, and we are going to try to walk through this, then, in a relatively brief fashion, through each one of them in that order.

MR. MARK: Theo, a question of a general sort. You did say that if you disperse 40 percent of the fuel, then recriticality is excluded. If I take the fuel and melt it all and magically put it in the pool in the bottom of the vessel, is it or isn't it critical if there is no sodium? Sixty percent will be critical if there is a good sodium layer on top, is that correct?

MR. THEOFANOUS: In fact, I don't think the sodium layer makes a big difference. Maybe if there is some steel around it might make some difference, but 40 percent is -- 60 percent is critical if you have the normal mixture of fuel and steel and internal blankets all mixed up within the core configuration.

MR. MARK: At the bottom of the --

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MR. THEOFANOUS: Inside the core. I don't care where you have it, but inside the original core configuration. Now, if you bring in also the radial blankets, and if you are going to do that, if you are going to let everything melt and go down to the bottom, probably also by that time maybe you bring in the axial blankets, the lower blankets, maybe even some of theradial blankets,then you have enough dilution that in fact without any loss of fuel the thing will be subcritical.

MR. MARK: It is that kind of question I am wondering about.

MR. THEOFANOUS: It is this kind of thing. I think you will see that better when we discuss the loss of heat sink accident, because in this case, in fact, you don't lose any fuel. All the fuel stays inside,but what happens is more and more radial blankets come in. I think I will be able to answer your questions there.

Now, the details of this whole thing are sometimes so overwhelming that it can cause a lot of confusion. What I am saying is one can get hung up with details in looking at these core disruption accidents and really misunderstand certain simple features of them.

What we are trying to do here is trying to do exactly that, trying to look at them in a simple way, to really try to comprehend what is going on. I think this is

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done well with this kind of picture. We call this a generic core-disruptive accident progression, and it is generic to the loss of flow accident in particular.

Here it is shown that from initiation, what I said before, from the initiation of core disruption involving just a few pins failing, until the complete core disruption, which is the whole core all molten inside the original core confines, this we call homogeneous whole core pool. The core will go from this initial disruption to this final disruption through successive stages of melting materials and relegating materials.

This is like a continuum of states here, and we describe this continuum state in terms of two representative ones. One is when the subassembly walls are largely intact, so at most we have individual subassembly pools, and this is this stage over here. The next stage comes about because of the heterogeneous core design, and here I would like to put up a figure donated to me from the last meeting from the project, and that shows very well, I think, an intermediate stage between the individual subassembly pool scale and whole core pool stage because of the pressures of the internal blankets.

These are low-power regions and much colder, so when the drivers are quickly melting, within a fraction of a second, they will remain intact. Of course, they will be

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attacked by melt from inside and outside, but you can see that there is at least for a short period of time this structure over an annular pool, shown here by this red.

That is the reason, then, we have identified an intermediate stage, the annular pool stage.

Now, through this continuum of advanced disruption stage, there is a potential for the system to terminate the accident energetically, that is, to obtain energetic termination. This termination can happen either energetically, either in a forceful way, and by that we mean pressures developed of significant magnitude to forcefully eject the fuel materials into the sodium pool and to actually do that with enough force that some energy is delivered to the sodium pool, or it can terminate in a mild way again by escape through all those different relief paths. This process here we call hydrodynamic disassembly or disassembly, this process over here we call dispersal.

Now, depending on which of those exit paths the system can take for energetic termination, we have a different probability or a different chance of failing the system, and that is why, then, we have identified here four paths that go from the disassembly process into failing the reactor vessel, and that is what we mean by ex-vessel containment, and each one of those paths will have its own number, its own probability.

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Now, to have a quantitative view of which way things are going here, one could put numbers in each one of those paths, probability numbers. You see there they are split point decisions. At this point what is the chance of going into energetic disassembly, what is the chance of going into subassembly pool, and what is the chance of terminating?

Now, one could express those things by words, and I would say the most likely possibility here is to go into a pool, and then from here on the most likely probability is to disperse. On the other hand, as you see, there is more than one step here and very quickly one gets confused with words if one tries to do this way.

So we have bitten the bullet, I guess, and tried to put numbers here,and this has caused some controversy because people in general get nervous when one talks about numbers, especially about things that are somewhat maybe not amenable to exact quantification.

But we think that it is an important exercise to put this whole thing into perspective, not to put in perspective the situation to a whole PRA, because that involves another step, but to put in perspective the whole sequences within a core disruptive accident.

END
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(Slide.)

So I would like, then, to show you the approach that we took in doing that. The procedure is, first, to identify or define a set of probability numbers on an order of magnitude basis and give definitions to them. And we have chosen three levels; one is characterized by 10^{-1} as an event that really can be obtained by age of spectrum assumptions. This is an event, for example, that one normally would not expect on a best estimate, not even on a low probability, but one has to really go to the edge of the spectrum before one can claim that this event has occurred.

The next level, 10^{-2}, is when one would require to make out of spectrum assumptions in order for this event to be realizeable. And this implies that we understand the phenomena controlling, and therefore, we can come up to the edge of the spectrum. Here, the trend is different. Here we are trying to look at the phenomena from the other end of it and consider what is physically possible, and really, this kind of a circumstance is so far out from what we expect that this will be outside of the spectrum.

So when we are at this level, I guess I am saying we know better the best estimate behavior when you come to the edge of it. When we are here (indicating) we're exploring more to the outer spectrum and coming up to the edge on that side.

Now, if you have two events, one like this and one like that in series, one following the other, you would have a probability of 10^{-2}, 10^{-3}, and physically, I think it is reasonable to claim that such a sequence with two very unlikely situations would really be classified as physically unreasonable. So that's how our definition of what is physically unreasonable.

Now, having done that, we need to develop the basis for judging each one of the parts that were given before,that you saw in the generic accident progression, so we can identify put numbers to them. Now again, to do this precisely, one would have to have the complete probability distributions, and actually, quantify each one of the parts not with a number but with a probability distribution.

We claim that really, we don't have the information to do that. It's a step that cannot really be accomplished in a reliable and meaningful way at this time. One can always put probability distributions and carry through the exercise, but nobody has done it yet and I think it will take some time before anybody actually attempts to do that in a reasonably confident way.

On the other hand, if we ask a different question, say what if we can assign those numbers as high confidence numbers at the end of those probability distributions. We don't have to know the details in order to know what is out at the end, and if we try to do that in a conservative fashion, we can actually accomplish this step as we have done in the report.

Now after this, of course, things are simple, we can carry out the arithmetic, following each one of the composites we are interested in, and in particular, we cancome out with the probability for vessel failure by summing up the probabilities of those four parts that were shown on the previous slide.

Now we have a number at the end, and that is the probability for failing the vessel, and this number was obtained in terms of these definitions. It would be very wrong if one would take that number now and go and stick it into a probability risk assessment study that is based on a different set of numbers; a lot of them actually based on experience. Therefore, we emphasize that with the start, one has to take this number and go back and convert it into the physical meaning associated with this based on those definitions here. If the number is less than 10^{-3}, then we can say that the failure of the vessel is not considered physically reasonable; it is physically unreasonable. And that is really the bottom line of that.

And you will see, we will come to the bottom line. Now, if one wanted to see what is the impact of this kind of treatment to a total probabilistic approach, considering both the front end of the core disruption, the initiator side, as well as the back end of it, considering the radiological aspects, one has to go back and look at the context of the numbers assigned to both of the sides, what would be a correct number to assign to physically unreasonable states. And that's the number that has to go in there, then, in the analysis.

MR. KASTENBERG: Do you also come to the converse of that? In other words, you must have assigned number 1 to your best estimate; something that is. I mean, if you heat up the core you'll boil the sodium. That has to have a 1. So then if you propagate everything and you conserve probability, you have to have something that is physically reasonable at the end, I presume.

MR. THEOFANOUS: Right, sure.

MR. KASTENBERG: And you do have it.

MR. THEOFANOUS: Yes, and you will see that. At the end we're going to put the numbers, and you will see the many of the parts, in fact, have 1, and therefore,this way if one wanted to arrive at the best estimate scenario, or if you wanted, you could find out which way it goes.

So with these more or less introductory materials, I think we are going to go into some of the technical details, and to start out by considering the CRBR's actual capability.

This is a schematic of the system. And here are some of the acronyms we use; this is the UIS or operating internal structures, and that is a rather large structure that supports from the top with four strong columns. Here you see the core barrell, and this is the radial shield; there's a number of subassemblies there that contain the steel mass, there's no core. The core is in the middle, and this is, of course, the core structure.

Also,there is another structure up here, the upper core structure, and this is made up of blankets and the fission gas plenum, steel.

So to a large extent, this is just an empty hole kind of structure, like a honeycomb.

Now, the failure mode through the system, then, the US -- see, there's pressure, there's an energetic event suddenly releasing high pressure into the core and can, of course, block the UIS and the cones can buckle, and that would make this whole thing be translated upwards and the high pressures, then, would manifest themselves in the bottom of the pool, the pool will accelerate and heat the head. This is the vessel head structure, and this slug impact, then, can cause damage to the head, and the appropriate way to look at that is in terms of the impact kinetic energy.

For the UIS, the failure mode is buckling, as I said before, and for that, one needs to know the pressure history on the UIS.

Now, the structural evaluations we have done show that the core support structure is by far stronger than any of those 2 structures, so it doesn't even enter the picture. That is, the thing is going to fall apart in other places much before the core support structure gives way.

On the core bottom, however, that can fail also by radial strain under this body, and again, for one to evaluate that one needs to know what is the pressure time history of the bounding of this core bottom.

We have done the analysis, then, according to this kind of a logic, and we found out that the approximate approach to failing this cage, this stucture here, is at an event of about 100 dollars per second in a two-phase disassembly mode. And I emphasize that because there is some very great difference between the configurations to which these assemblies take place.

And this is one that basically turns around and terminates by variable pressures.

Now, after the structure fails, as I said before, the full accelerates, this work is done, and obviously, this requires a high level of energy, and for this energy, then, to be of significance as far as the vessel head structure is concerned one must approach the 200 dollars per second, two-phase disassembly rates.

MR. ZUDANS: Theo, what makes the core accelerate upward? Just the pump pressure?

MR. THEOFANOUS: No, no, this is an exiting event now, and how ones goes about getting an energetic event you will hear in the rest of it. But here we are hypothesizing that suddenly the core becomes molten and has developed high pressures.

MR. ZUDANS: And it can't go down.

MR. THEOFANOUS: These pressures, then, develop within this cage, so it's going to load all the sides of it, upward and otherwise. And if the thing fails, then, of course, we're going to load also this other side.

The reference yields, just for normalizing with what you know from previous discussions, the 100 dollars per second, two-phase disassembly has an 1150 megajoule ultimate work potential. This is kind of short terminology to use instead of this expansion to an atmosphere. Normally, if we want to characterize how much an energetic event is, one takes the initial state, following the neutronic termination; this has a temperature distribution, a pressure distribution, and then one lets that expand pocket by pocket adiabatically until it all comes out to one atmosphere pressure. And then one calculates the PV curve from that, the PV work, and this is what we are referring to as ultimate work potential; that is,the most possible work that anyone can ever do from this kind of an event.

MR. MARK: Isn't that similar to PVs throughout the vaporized region, initially?

MR. THEOFANOUS: Initially, yes. And here, for the 200 dollars per second is 25, 50 megajoules; very significant amount of energy.

MR. MARK: It would seem to me more graspable if instead of the ultimate work potential you just said the product of PV throughout the core initially is,like, 1150 megajoules.

MR. THEOFANOUS: Well, that would require many more words to say it --

(Laughter.)

Or, the usual expression is expansion to an atmosphere; it is the same thing.

MR. MARK: I'm familiar with that, but obviously meaningless.

MR. THEOFANOUS: This is really a controversial item because many people don't like to see these big numbers and they think really more meaningful things. Essentially, everything is going to be contained inside the vessel. A more meaningful thing is to do this PV expansion only to the cover gas volume, but then again, one loses a little bit from doing that because every system has its owncover gas, and maybe even the same system that might have a different cover gas. So this is really a thermodynamic definition, generic, that really conveys the magnitude of the energy possibly available, and as long as we remember that really, those levels of energy never manifest themselves -- one should never make the mistake that those numbers actually show up in terms of the work .

MR. MARK: Well, you mean they never get converted to kinetic energy.

MR. THEOFANOUS: Yes.

Now, the applicant has used a 661 megajoule source to record. They have neglected all those structures. Basically they let this equivalent PV carb that gives this 661 megajoules by again, adiabatic fuel, and let it expand upward unrestrained, accelerate the slug. At the time of impact, there was a kinetic energy in its volume and all the materials in the core, and they counted up all that kinetic energy and they found that to be around 75 megajoules. Some of the energy, of course, if you do the PV work for this source here, it is about 100 megajoules, but they did that taking into account the structural deformations and some of that went into strain, so the total kinetic energy was 75 megajoules, counting the core materials.

And then they made the step that said we will design the head for a 75 megajoules slug impact energy. This involves a situation now to try to account for a kinetic energy that's inside the bubble with liquid moving inside the vapor space and then going and heating the liquid.

So as you see, the approach that we took is somewhat idealized, in the sense that it neglects important, in our opinion, very important mitigator of energetics into the pool. And this is an important mitigator because as you will see, this 100 dollars per second energy level really we don't even expect to have it under the worst possible conditions in the core.

So from that point of view, it is really an ideal situation. If there was a range of expected ramp rates that go up to 150, let's say. So maybe in that case, by neglecting all this mitigator, may be not too bad because maybe a portion of events only are covered by that portion, and there's another large portion that is not covered.

But what youare saying here is the whole thing is covered by this, and if you neglect that, you basically neglect the necessity of completely mitigating the whole energy release.

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ms

Now, what I have to do is actually walk you through the different steps of showing how this characterization of $100 and $200 per second energy levels was obtained for the failure of those structures.

MR. ZUDANS: Theo, the 75 megajoule impact, kinetic energy impact, of course neglected all the internals, and that means they made a clear path for this. Now, if you do not neglect all the internals, you would get less kinetic energy at the impact but you would strain the vessel more.

Now, I know that the tests show that you can get twice the strain with the upper internals than you get without. Did you analyze this?

MR. THEOFANOUS: Yes. In fact, we are looking at that in a realistic way. We are looking with the internals and everything there. The reason I am bringing this up here is because if one were to look at these numbers here and these numbers, one might say, like somebody told me in a previous presentation I made, the Staff, who is more conservative here? You give higher numbers but you are coming at the bottom line and saying that this very, very high level of energy still, as you say, will give you the tendency the same that the applicant gets.

What I want to say is that the two approaches are so different, and the one is so idealized that I don't want to venture into saying who is more conservative, who is less conservative. We will just give you our story here, that we think is consistent, and then you have to make your judgment as far as how the two compare.

MR. ZUDANS: Well, you didn't get my point. Which failure modes?

MR. THEOFANOUS: We are going to consider all three failure modes.

MR. ZUDANS: But it doesn't show vessel failure mode. You only show column buckling --

MR. THEOFANOUS: We want to talk about the vessel failure mode also, but the point here is that we don't really, are not really overly concerned about the vessel failure, although we are looking at it, because the primary concern is the failure of the upper head. Like Carson said, the failure of the head has the potential of releasing materials into the upper containment, and one is concerned from the point of view of violating the containment. Now, if materials were released into the cavity, you are talking about a different set of events from the point of view of failing, basically releasing any radioactivity to the outside.

MR. ZUDANS: I just want to know whether your analysis considered vessel failure as a failure mode.

MR. THEOFANOUS: As a failure mode.

MR. ZUDANS: Correct. Because you list failure modes and it does not include vessel -- core barrel strength,

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column buckling --

MR. THEOFANOUS: You see, I am keying the whole discussion here, and as it should be, the whole energetic discussion, keyed to generating failure of the upper -- of the vessel head structure, potential generation of missiles, basically potential threat to the containment.

Now, there are other ways, there are other parts of the system, and there may be other parts that can fail. We are not concerned about all of those other parts from the point of view of energetics.

MR. DICKSON: Excuse me. I would like to add something here because I read yesterday from a figure that contained a typographical error. The numbers I read were that SM3 had a 2.8 percent strain, and SM2 had a 1.4.

MR. THEOFANOUS: That is correct.

MR. DICKSON: The truth was -- that's a typo -- that should have been 4.4. It turns out that when you include the upper internals in the test, the thing that is strained more is the core barrel and not the vessel up near the head.

MR. ZUDANS: What was the strain in the vessel near the head on that figure that was labeled 1.4, 2.8? I thought the labels were switched.

MR. DICKSON: It was 2.8 and 4.4. The graph at the bottom is correct.

MR. THEOFANOUS: The 1.4 and 2.8 still is essentially close to 80 percent higher.

MR. DICKSON: Yes, but it was the SM2 that had the greater strain. SM3 had less, the reason being that more of the energy went into straining the core barrel.

MR. THEOFANOUS: That doesn't make physical sense.

MR. DICKSON: Well, it does. If the UIS is sufficiently strong, the vessel isn't strained at all. All the strain will be taken up in the core barrel. With an infinitely strong upper internal structure, the only thing that can take up energy will be straining of the core barrel.

MR. ZUDANS: In other words, it prevents the pressure variable penetrating upward.

MR. DICKSON: That is correct, and that was experimentally determined.

MR. THEOFANOUS: What is happening here is this slug accelerates and hits the top, and because of the impact, the pressure is what strains the vessel. Now, obviously if you keep the slug from accelerating, and you can do that by keeping the UIS in place, obviously you are going to be having less strain up here and more strain down here.

MR. ZUDANS: It is not you that confused me. It was yesterday's incorrect number.

MR. THEOFANOUS: I heard about yesterday's discussion.

So now, then, looking at what we have done in terms of analysis to analyze these failure modes, we need the pressure history of the boundaries. After we get this pressure history, we can do a structural analysis to take the strain of the barrel, and the same thing for the upper internal structure.

For one to do this correctly, one would require to do a couple of fluid structural analyses. This capability really is within reach. Rexco can do that but it is not really all geared up to do this now. Therefore, we had to resort to an approximation technique, and the way we did it is to get the pressure histories on the boundaries, assuming these boundaries are rigid first. That maximizes the pressure history on the boundaries.

Now, having done that, go and do a structural analysis and find out about how much of a strain we have. Now, having gotten the strains, we can go back and do the initial expansion analysis again to find the new pressure histories on the boundaries but allowing for the displacement that we calculated before.

The new pressure histories now are applied to the structural, this two-step approach, applied to the structural analysis, and now we come up with new strains. We have done that, considered the two levels, the $100 and the $200 per second level, and we obtained in the first one convergence in both steps. The strain was about 2 percent, 2.5 percent,

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while in the other level, in the 200 level, the second time around the strain was somewhat lower than the first one, so it was not an exactly convergent situation but it was on the conservative side.

Now, remember that this is a case in which already we know we were going to fail that. We are primarily concerned now for failing the head, and therefore this lack of convergence is not really a big problem.

MR. ZUDANS: Theo, the first step would not be conservative pressure distribution, if I understoood you correctly. If you took rigid walls and derived the pressure history and turned around and applied that pressure history to the walls to get the deformation strength and repeated the pressure history computation but now with the displacements as a function of previously computed pressure, then that expansion would be larger than it really is because your actual pressure --

MR. THEOFANOUS: That's right.

MR. ZUDANS: And you repeated those steps?

MR. THEOFANOUS: But if you go back and you converge your --

MR. ZUDANS: That is what I am asking.

MR. THEOFANOUS: And you converge. That's what I'm saying. That's right.

MR. ZUDANS: Then it would be okay.

MR. THEOFANOUS: So now I want to give you the picture here again of the energetic event. There is no more energy released in the core, and typically there will be a pressure distribution in the core. There is a high flux region in the center, and that's where the peak pressures, peak temperatures are going to be.

Now therefore, one would expect to have a temperature, radial and axial temperature distribution and pressure distribution. Obviously, the pressure up here will not be the same as the pressure up there.

Now, if this whole thing was solid or liquid and had good transmission characteristics, that would be very close to being true. However, we have a two-phased mixture here that is very compliant. It has a lot of void inside it. As a result of that, the pressures that are manifested in the center, actually they never materialize on the walls. That is a very important thing because if this mitigating aspect, this compliance of the medium inside the core, was not there, there would be no mechanism to get the energy in the first place. One really needs to understand that.

If this whole thing was all liquid, let's say, one can postulate all liquid, and say I'm going to put into that all liquid \$500 per second or \$1000 per second, one could generate almost no energy at all because of the thermal expansion characteristics of the liquid. You know, very high pressures, very quick shutdown, and very low energy deposited in the fuel. That is well known for many, many years. So the main reason that one can ever expect -- I don't say will, but ever expect to get energy here is because of the compliance of the system. Therefore, it is a very gross conservatism in error if one does not take into account this compliance in calculating the loads on the system, and that is what we were trying to do here.

The core barrel will strain under loading, and that will provide additional expansion, additional energy absorption within that case than will ever appear up on the expansion of the pool. Therefore, that represents an important loss. The same thing with the upper core structure. The upper core structure, as I said, is very much a void structure and is going to be accelerated upwards and is going to hit the UIS. But again, it is a crushable thing and it is going to take energy to crush it, and also it will produce an expansion, which means a drop in pressure here before even any of this pressure can appear up in the pool. So this is some of the important phenomena, and you can see here the trends.

This is the center core pressure as a function of time from an expansion, and this is the $200 per second, two-phase disassembly.

You see, we are starting out at extremely high

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pressures, 1500 bar, but it very quickly drops down to just a few hundred, 200 to 300 bar, and then goes like this.

Now, this asymptotes out because remember that we did this part of the calculation for the cage for a completely closed system. We didn't allow for the fuel to get out. As a result of that, these are going down.

On the other hand, this expansion, that same expansion -- which, by the way, was done using the SIMMER code, and this is just a simple state adiabatic expansion, and the fact is you will see later we accounted for every bit of the energy in the thermodynamic sense at the end.

So in the core barrel, however, the pressure, at most it is about 700 bar for a short time and then quickly drops down again. Of course, eventually all the pressures have to come together.

As far as straining the core barrel, this part here is very important, this high part, and the short-livedness of it is important.

Now, I want to along the way contrast that with what the project has done. They have taken basically the peak pressure, not the 1500, because they didn't consider such high energy levels, but they took the peak pressure of the 661 case and applied it directly, I guess, on the pool and in accelerating from time zero, and in the study -- not the project, but in the study that was published in the

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Chicago conference of reactor safety, in which the core barrel was examined from this set of conditions to the 661, that peak core pressure was applied directly on the core barrel.

As a result of that, they obtained displacements on the order of 12.5 percent or strains of 12.5 percent. For our cases we need much higher energy to obtain the same displacements, and that isn't exactly because of this attentuation of the pressure, because of the compliance of the medium.

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This is the expansion, the P delta p curve, and this 200 dollars per second. If one was to do a pocket by pocket isentropic expansion to the total free volume that is inside that enclosure, that takes into account the strain of the core barrel, as I said before, all the internal voids, the displacement of the upper core structure, adds up to about]4 cubic meters. If one was to do this PV work, one would obtain 520 megajoules; that's maximum thermodynamic work potential.

By doing a similar calculation in the straining, as I described before, we find that about 180 megajoules have gone into the core barrell as strain energy. And 340 megajoules have gone into the upper core structure as kinetic energy. And the sum of the two is this number up here. So you see it's a numerical calculation, and we are coming out -- we haven't dissipated energy, we haven't lost any energy; it is only that some went to strain and some went to kinetic energy.

Now, one might say that this upper core structure is moving upwards with this very high kinetic energy; can that add anything to the damage in the system. Because if that cannot, what this really implies, because of all this internal expansion inside the cage, really somehow were dissipated 520 megajoules, which is a really significant number.

Well, you can visualize this upper core structure again is a thing that is accelerated and moves very rapidly, but goes and heats up the structure or mass that is much larger in size and is strongly supported from the top. And it goes and crushes and all the damage is absorbed into ashes and dissipates. And all of that energy then will show up as kinetic energy. Up to this point.

So now then, having done that, we found that the 100 dollars per second somehow dissipated all of it inside the bubble, gave very minimal, only 2 percent strain on the core barrel, or 2.5 percent in the core barrel and just one or two inches, very little, on the upper internal structure. So this 1150 megajoules we feel is safely and totally contained in that cage.

Now, we consider the second threshold which is the long-term expansion, and that only comes into the picture if this cage fails, and that requires a high level of energy than the 1150.

The next level we picked to analyze went all the way up to the energy level that would be required to produce enough kinetic energy in the pool to start compromising or approaching the design limit of the vessel head. And that is the 2550 megajoules at 200 dollars per second.

The situation here is as follows. Obviously, under this very high energy release the core barrel will strain, and we estimate, following the short-term approach analysis I described before, that the core barrel will strain maybe quite a bit, maybe up to 15, 20 percent. So that is really going up there to the limit and depending on whom you talk to somebody will say maybe it will fail, or somebody may say it's really up there but if you took into account that the vessel itself is going to strain and that it's going to come up against the guard vessel and there's going to be another stiffening effect from that, so maybe this might give a little bit maybe it will not become a total catastrophic failure. But it's right up there.

By the way, this analysis of the strain of the core barrel was done taking into account the stiffening effect of the vessel wall and the presence of sodium also taking into account the drop in pressure from the edge of the core to the core barrel interface because of additional convergence and additional divergence, I should say.

MR. ZUDANS: In this calculation, was it assumed that the upper internal structure effectively close to core barrel and that reactor vessel volume above it did not participate in expansion process?

MR. THEOFANOUS: The first part of the calculation was done by neglecting the relief of the pressure because of the displacement of the --

MR. ZUDANS: So it was like a closed --

MR. THEOFANOUS: It was very close, it was conservative. Now in reality, when that begins to move out, the pressure will be relieved and you will have less volume.

However, again, remember that mostly in this kind of forms -- and I must admit I don't have a very good feel for that, but just looking at the results, it's very, very clear that in a very, very short time with very, very high pressures, those are the ones that are producing most of the energy release into the structure; that has produced most of the strain. And then from then on, basically it just keeps on moving like that.

But I do want to make the point, however, that both this model here as well as this model, I mean the structural models, were checked very carefully because obviously,it's an important part of the story. We went into each sector, number one, benchmarking against the SRI tests, structural test, and more really because we were -- because we thought we found some interesting discrepancies between our results and this paper I mentioned to you from the Chicago conference that was giving about the same kinds of strains for what we thought were different pressure histories. We took their pressure history that they used in there to get the strains and we put it in our model and really got exactly the same deformation. So we have an additional confidence then that our structural results are consistent in terms of the methods applied and techniques to such a core disruption which has really received extensive study and verification over many, many years.

MR. ZUDANS: In all this process, the core support plate and the core barrel and connection to the conical skirt, were they assumed to be rigid? The bottom end was closed -- ?

MR. THEOFANOUS: The bottom end was closed and rigid, that's right.

Now, then, what happens here, if we have this 2550 megajoule kind of an event, the UIS would buckle, will move up by buckling the columns, and that will then generate paths for the high pressure zone to expand. And therefore, a bubble will form here, and on the other hand, there will be another drop in pressure between the core and the bubble, and this is caused because of the additional acceleration required as the fluid flushes to fill up the bubble volume.

And this amounts to about a half -- the pressure here is about one-half of the pressure up here, and that is shown schematically like this (indicating). This is how the core pressure decays with time in this long-term expansion, and this is how the bubble pressure decays with time, typically maintaining this well-known critical pressure vessel about .5.

This we refer to as the throttle effect, and you can see that that is very important because over this part of the transient the pressure really does not decay very rapidly; you are almost near the pipe, so if you're thinking in terms of the delivery to the slug in terms of P delta v, yousee here the delta v is the same in both cases, you see that in this case you're going to get a warp that's about half as much as you would have gotten if you neglected this.

MR. ZUDANS: I'm trying to find the slides in your handout --

MR. THEOFANOUS: You won't find them there because those were not made directly from the --

MR. ZUDANS: This is not very easy for me to follow because you have so many additional loads on the slides. It's just no way we can copy them and I think this is part of your argument.

MR. THEOFANOUS: Right. The idea here is that -- again, I think the problem is that you do not have the document involved ahead of time. But the idea is that I want to -- I didn't want to restrain myself only to the figures that are only from the pages because then I wouldn't be able to explain to you --

MR. ZUDANS: Can't we get a copy of the slides made?

MR. THEOFANOUS: What we can do, however, is we can make copies of those on the break and you can then take notes right on it.

MR. CARBON: Let's just take a break at this time and make copies of all these slides for everyone.

Zenons, would you like to go back and start back someplace?

MR. ZUDANS: No, that's fine, as long as we get the pictures.

MR. CARBON: Let's break and start a little earlier.

(A short recess was taken.)

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MR. CARBON: Let's resume the meeting.

Go ahead, Theo. Could you move your microphone up closer?

MR. THEOFANOUS: Okay. I was suggesting we start a couple of slides back, and now we can focus on the vuegraphs.

MR. CARBON: Just give the entire presentation if you will.

MR. THEOFANOUS: Give what?

MR. CARBON: Give the entire presentation.

MR. THEOFANOUS: Oh, the entire presentation? Yes.

MR. CARBON: Unless that takes you too far back.

MR. THEOFANOUS: No, I will do so.

The structure that first we see the energy in this, from the core is this enclosure, this cage made out of the UIS, the core barrel and the core support structure. The core support structure is very strong, much stronger than those two other ones. Therefore, this internal containment, or this cage, will generally fail first through here or through here. The failure modes would be radial strain of the core barrel and buckling of the coils of the UIS.

And what we need in order to be able to evaluate those is the pressure history of the boundaries. Assuming that -- and also we find that there is a release of $100 per second, corresponding to 1150 megajoules ultimate worth potential, is the kind of an easy way to approach this failure limit.

Now beyond that point, and after the UIS has failed, there is a path for this energy to show up in the sodium pool. It will accelerate the sodium pool then, and this pool is going to hit the head. That is the mode of transmission of energy from this source to the vessel head structure.

Here, therefore, one is interested on the slug import energy. This is quantified by the import kinetic energy, and we find about $200 per second energy release or energetic level in a two-phase mode. And a two-phase disassembly is what is required to approach the design limit of 75 megajoules import kinetic energy.

This corresponds to 2550 megajoules,and this is to be contrasted to the number used as a source by the applicant as 661 megajoules. And I doubt if there is any reason to go back and repeat all this discussion about what the applicant has done.

Now the analysis that we carry -- and we tried to be persistent here in terms of where the source is and how does it show, how does the energy show in different points in the expansion -- can be considered in terms of two parts. One we call the short-term expansion, and that is what it takes for this to expand against these boundaries, these inner boundaries, and kind of come up to some kind of a positive charge equilibration.

This sort of expansion gives for us the pressure histories of the boundaries, and as a result of that, we can determine the failure limits or the failure thresholds.

The longer-term expansion requires that this is moved, that the sodium slug accelerate, and this is taking place on the order of magnitude of about 50 milliseconds while this first shorter expansion takes place on the order of 20 milliseconds.

So that's what I will show next then is these two steps. First step: The part of the core that is undergoing this disassembly is going to end up at the end of the end of the neutronic excursion with a temperature history. That implies the pressure history, and that is going to be highly nonuniform because of the peaking of the flux in the center.

Because of the compliance of the medium -- and again, it has to be compliant because if it is not, we wouldn't have the energetics in the first place -- this very highly centered pressures are not going to show up in the boundaries. There will be a delay in the decay and an expansion associated with this decay.

And that will show up here. The core pressure in the center as a function of time as determined by an adiabatic calculation, you see that within 5 milliseconds goes from a very high volume, an initial volume of 1500 bars down to just a few hundred bars.

The -- by comparison, the core barrel pressure history, taking into account the transmission characteristics of this medium, rises to a few hundred bars and then kind of comes and merges with the center pressure as the whole thing comes together to equilibrium or requires equilibrium.

The upper core structure, seeing these high pressures, is going to translate upward, is going to crash. Now, that is a complicated problem that hasn't really been done before. And the way that we are doing it -- I didn't mention that before; maybe it's helpful here -- the way we are doing it is by taking the mass of this structure, just as an initial constraint; we modeled that as the mass being there, and now seeing these pressures and therefore being accelerated.

And we allow it, allow this mass numerically to come to a full dense condition. And until it comes to full dense condition, really the loading of the UIS is very minimal.

In reality, what happens between the beginning of this crossing forces and until the mass comes to full dense condition, what really happens is that the UIS is there with its strength, much, much stronger, of course, than this little structure here, so it provides the backup against which this structure can crash.

This takes place over a period of about 20 milliseconds. This process of translation and crashing is only 20 milliseconds then that the UIS actually begins to see the loads. At that time, therefore, the UIS is going to see the loads of this magnitude instead of loads of this magnitude.

So the effect of the translation and crashing and the straining of the core barrel is to basically dissipate energy through our expansion and dropping pressures before the pool sees any of the pressures present in the core. We have heard about these losses.

So those are really, I think, the important ingredients here, the important considerations, as to why one would be far off the target here if one was to take this traditional approach of pocket-by-pocket expansion of the core in applying these resulting pressures to all the boundaries of interest.

Yes?

MR. CARBON: I would like to have a little bit more discussion about the upper end boundary conditions. The upper core structure is supported on top against this closure that really goes and supports against the head with columns. And during this calculation you are assuming that that upper -- at the top of your blue arrow -- that line does not move?

MR. THEOFANOUS: Yes. The shorter calculation is then with this goes and this goes and this goes. All of the first step of this calculation with the boundaries being fixed rigid. We do the expansion. And you come up with the pressure histories like is shown here in these two boundaries.

Now, since we know that, we go in and do a structural calculation for this and for that. This will give us some strains. Depending on the pressure, the srains might be quite a bit.

Now, this strain then is taken back into the original expansion. We allow this boundary now to move and get us back in basically up to an equivalent amount of strain as we calculated before.

MR. ZUDANS: Including the top?

MR. THEOFANOUS: And including the top. Right. Well, in fact, the top, in the case of the -- in the top we didn't do that in fact, because, you see, the top before it even begins to work, the two come together. So the transmission characteristics and so on are not all that important.

So we never really -- for some of them, you never really took credit for any losses in venting or displacemnt of the UIS in that short of a period. However, this is in the immediate vicinity; it has to do a lot with the dissipating of this and the pressure history, and that's the reason we take into account this early straining of the core barrel.

So in the second time around, and allow this to expand to an equivalent amount of strain that we calculate in the previous step, now we calculate pressures in the core boundary as a result of this new expansion. Take that new pressure now, put it back into the structure, and calculate again. We find that for $100 per second, the second time around we come up with the same strain. Therefore, we think we have conversed enough, and although the calculation is done in another or a couple of fashions, I think we are in good shape as far as predicting the ultimate strain of that.

We find there is only 2 percent or 3 percent. It is of no consequence therefore to the failure of the core barrel.

Now, for the UIS, you see the UIS loading only becomes relevant at a much later time, about 20 milliseconds. And that's after all these very early dynamic effects have really dissipated. At that time, as you see, the pressure here and the pressure on the UIS itself is very similar.

MR. ABDEL-KHALIK: I find this somewhat counter-intuitive. 1150 megajoules is about half a ton of TNT.

MR. THEOFANOUS: Yes.

MR. ABDEL-KHALIK: And to say that this will result in only a 2 percent strain is quite counter-intuitive. I realize that you have to take into account the fact that you have a very compressible fluid or compressible medium in there to allow for the voiding. But my concern then, should one worry about less energetic events with considerably less than 1150 megajoules where you would not have such a compliant medium?

MR. THEOFANOUS: Well, first of all, let me make a remark about the intuition aspect of it. In this business, we cannot afford to go by intuition. If you like intuition, you can come up to some very strange results. All I can say is that we have done that systematically and correctly and benchmarked every step of the way.

Now, what I am saying is that if this was a medium with a void fraction of something less than -- less than 10 percent, maybe 5 percent -- well, in fact, even less than that -- it has to be essentially a pretty solid system in order to transmit and transmit loads directly. And by that time, you should have not enough energy.

You see, even at the $200 per second level, you build up pressures of a few hundred bars for a very, very short time, and that is 2500 megajoules of energy that you put in there. If you take events less than $100 per second, it will be of no consequence at all.

MR. ABDEL-KHALIK: If one were to do a thought experiment, and let's look at the different combinations that would result in 2 percent strain, and you are saying that one of these is the case that we have here, 1150 megajoules with highly voided system --

MR. THEOFANOUS: Well, it is not highly voided. Okay. It is not highly voided. We are talking about the void fraction of about only about 40 percent,40-50 percent.

MR. ABDEL-KHALIK: Completely solid system, how much energy would I have to have in order to produce 2 percent strain? Is it 100 megajoules, 50 megajoules, 10 megajoules?

MR. THEOFANOUS: In terms of the equivalent amount of energy that we are talking about here, I think that to produce 2 percent strain, you would have to have probably, I don't know, I am only guessing, 600.

MR. ABDEL-KHALIK: I was going to say I think the REXCO application gives you basically that kind of an answer, and it's about a factor of 2 lower.

MR. THEOFANOUS: Yes. 600 megajoules.

MR. BELL: So that if you have a completely incompressible fluid in there, you don't have -- you have liquid sodium in the channels, you would get 2 percent strain if your energy release is half of 1150 megajoules. Is that what you're saying?

MR. CARBON: Would you give us your name?

MR. BELL: Charles Bell, Los Alamos.

MR. THEOFANOUS: Really, that's what the applicant has done. They did not take into account the losses because of the compliant nature of the systems. So that whatever pressure was calculated to be right here, they're pulling it up to the boundaries. In fact, not only that, but they put it up into the pool also. I don't think it's really too much counting on intuition. I don't think it comes to mind.

MR. FAUSKE: Hans Fauske. I think it's also misleading to try to compare this kind of situation with TNT. It's very different. In the TNT equivalent, you're talking about very high shock waves and the damage potential as a result can be very different. Your intuition, if you go into the TNT, may indeed be correct, but that's not applicable in this case.

MR. ABDEL-KHALIK: I don't have a pressure history here to compare with, so all I had was total energy release. The time scales are different than I understood. The response would be different.

MR. FAUSKE: They would be very different. You are talking about very different pressure levels.

MR. THEOFANOUS: All right. So I think that I was here in the process of describing or discussing how this calculation was done did not take into account the venting that would occur as soon as the UIS begins to move.

Here we go to the energy partition. There is a couple of 14 cubic meters of expansion taking place in the short term. And this corresponds to -- under the pressure curve -- to 520 megajoules. And that is the worth potential according to thermal dynamics, pocket-by-pocket maximum worth potential.

And you look at the results of the calculation now, the numerical calculation, and you see that 180 megajoules has gone into a strain in the core barrel and 340 megajoules has done into this, not by a mass -- remember now how we model that -- not by a mass of the upper core structure. It's moving upward with some velocity as in a mass.

The -- not much difficulty, I guess, in understanding what this means -- the considerations concerning this number may be a little more entangled here. The way we approach it is that this kinetic energy is inside the bottle, so to speak. It cannot manifest itself, certainly not to the head, and even less so to the pool.

This kinetic energy will hit the UIS bottle. All right. Now, a lot of that energy is going to go into crossing the structure itself. Remember that this we did not take into account because we did basically remodel that as a fluid; the whole construction was a fluid and was allowed to basically squeeze out the void and come closer and closer together without any dissipating characteristics of that thing as it was doing that.

So some of that then is going to go into strain energy with that structure itself as it compresses and crosses into a solid mass. Some of it is going to be absorbed in the UIS cones. Some of it, in fact, might be even transmitted to the head as some load. But those loads are much lower mechanically than the capability of the head. They are coming in at a very different time frame. And again, I think, to visualize that, let me just show you the time frame here.

This is the time at which the UIS is impacted and begins to feel the force and therefore begins to move. Now, it takes time to accelerate, of course. All the mass takes time to overcome pressure and accelerate. And during this time, where the venting process and the formation of the bubble begins, as you will see in a minute. The bubble hits. At about this time, assuming as you will see in a minute that the UIS has failed instantaneously and was displaced instantaneously to its maximum position when the bubble hits. That is for the purpose of estimating conservatively this complicated process from the point of view of getting maximum slug impact energy at that time.

So, in reality, in fact, the bubble will hit somewhat later, maybe 50-60 milliseconds. So the time at which the head is going to see this impact kinetic energy is around that time. The head is going to be loaded by the slug at around that time. A significant time difference. So it's just like you take that energy and give a little kick to the head before, and you wait and give it a real big burst later from the slug.

MR. ZUDANS: Okay, Theo, but the real question in my mind is how much this little initial hit is, because this is where the problem lies. It's 350 megajoules.

MR. THEOFANOUS: Are you saying the head --

MR. ZUDANS: Hold on. Hold on. 340 megajoules. Some of those megajoules, although you call them all losses, but they are not, some of those megajoules would be absorbed in crashing the upper core structure, some of them. Some of it will be absorbed in crashing the support columns. The rest of it will be compacted mass in the form of kinetic energy flying upward. And there's nothing else to stop it but the head.

MR. THEOFANOUS: Well, no, really what happens then is an exchange of energy. And in fact, we did the calculation and you can see this by the problem, for example, we can completely neglect the strength of the columns, find out what would b the kinetic energy of the UIS if it was a free body hit by that 340 megajoules. They didn't talk about the very diffferent masses involved. Okay. And we did that, and we found out -- I forget the number, Charlie, do you remember the number?

MR. BELL: 80.

MR. THEOFANOUS: It was 80. The kinetic energy of the UIS.

MR. ZUDANS: What was the --

MR. THEOFANOUS: Because of that exchange of energy.

MR. ZUDANS: Well, you call this 340 kinetic energy of the UCS.

MR. THEOFANOUS: That is the upper core structure. What's that?

MR. ZUDANS: All right.

MR. THEOFANOUS: No, what was it hits this? That's a very heavy mass.

MR. ZUDANS: All right.

MR. THEOFANOUS: You see that in the limit. I think it's a very unrealistic way of seeing it, very conservative. You can see that it's a two-body problem. One must --

MR. ZUDANS: That's fine but it's still an energy conservation problem.

MR. THEOFANOUS: Right.

MR. ZUDANS: 340 unless you dissipated something in terms of inelastic deformations or something in the fluid; it's still there whether it has a low velocity or not, whether it's a large mass or a small mass.

MR. THEOFANOUS: Well, it really depends. And I think what I am saying here -- I don't know what you're driving at -- but what I am saying is that we have this 340 megajoules. That is calculated as the maximum kinetic energy in this structure, neglecting its own dissipating characteritics, its own own class of characteristics.

Now, a portion of that, as I said before, is going to go into strain energy, into just what it takes to make that into a bundle, a complete mass. We don't know how much that is because it is not easy to calculate.

Secondly, another portion of that is going to go to the cones, and some residual portion is going to go maybe, if that is to fail -- but if that fails, remember, that fails up here and it fails gradually because of the quasi-static levelling of the pressure. That, if you took only that mass hitting the head, that UIS was not going to fail. Okay.

So a portion of that then only is left to add to the top of this pressure level as it tries to displace the UIS and fail it by buckling the cones. All right.

What we are saying is really it is of no consequence at all because we are going to fail the UIS anyway. Now, I think it is also clear that you can't fail the head by pushing it from the bottom through those four cones. There's no way they can do that. So the most that you have is the cones to fail. Now, if that was to happen, another way of looking at the problem is see it as a two-body problem, forget about the crashing, which I think is extremely significant of this and the crashing of that, just take the 340 megajoules and do a two-body problem. Even if you did that you don't end up with much kinetic energy on the UIS. By the time the bubble forms, this will begin to see forces from above also that will tend to make it all disappear.

MR. ZUDANS: I guess I begin to see what your reasoning is. In other words, there will be a number of interactions with the UCS and UIS as the elastic plastic mass and then some of the energy through these interactions would be either transferred to through the sodium or lost otherwise, but it will never be available as a net kinetic energy to move it end to end.

MR. THEOFANOUS: That's right. The important point

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here is those interactions are happening in this time frame.

MR. ZUDANS: Okay.

MR. THEOFANOUS: And the head is going to be really loaded from the sustained pressures accelerating the sodium slug is going to be all the way up here at a much later time. So they are not additive.

MR. ZUDANS: All right.

MR. THEOFANOUS: All right. So I guess we covered that.

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Long-term expansion will take place because of the buckling of the UIS cones; a bubble will form. The pressures in the core are going to be much higher than ever possibly you could have in the bubble because it takes pressure to accelerate the material to produce the flashing, and that flashing is what is keeping the pressure in the bubble up.

As a result of that, we find the typical kind of thing that you see in experiments and is common knowledge; that the pressure in the bubble will be about one-half of the pressure in the core. Just doing a simple PV expansion work, you can see then that just due to this mechanism that we call throttle effect, we have a reduction by a factor of 2 of the potential kinetic energy of the slide at time of impact. That is an effect, also, that the applicant has not taken into account.

The calculation was done assuming an adiabatic bubble. That is, we did not take into account any augmentation of those pressures because of sodium entering the bubble, introducing additional pressures. We have looked into this problem by means of calculations, sensitivity calculations, in which we put sodium coming in from the pool in the bubble to build up pressure. But when that happens, this pressure builds up but then the expansion slows down somewhat, there's some kind of a cancelling effect and you don't really get much of an effect.

Furthermore, what we know from experiments -- and there are a couple more experiments done on this -- when you blow down a flashing fluid into a pool that is highly subcooled, as happens, for example. by flushing hot water into cold water, there's a lot of mixing going on, a lot of entrainment and a lot of heat losses, as you might expect, so that the actually water that you deliver in terms of slug energy is much, much less than even this factor of 2 that you see up here.

On the other hand, however, we also know from experiments that if the pool is volatile, this volatility of the pool can interfere with the mixing from the process, and the results look much more like adiabatic results. In fact, we have a couple of experiments that we have run in which we used freon here and hot water here. And we found out that the results are exactly adiabatic.

So it's on this basis, then, that we did a calculation on the best estimate basis, and we think also it's conservative in terms of the adiabatic bubble.

Now then, let's see what happened to the rest of the energy. We are talking about a delta v expansion after the 14 cubic meters, as happened here in the short term. Now we have another 15 cubic meter expansion that is the covered gas volume. This 15 cubic meters, if it was done isentropically according to thermodynamics, should give us 160 megajoules. This number is 160 megajoules. Well, we got in the calculation 80 megajoules.

Another point that has to be made, both during the short-term as well as the long-term expansions there is other heat transfer processes in the losses that we think are present, and those result because of the interaction with hot materials with cold steel that is present both here, as well as in the upper core structure. These losses are significant and if one actually had done some similar calculations they would show that one might be talking about another reduction maybe by a factor of 3 or 4 in energy releases. But those are a little more difficult to take into account, and people tend to ask a lot of questions about them because we don't like to be questioned and we're not going to talk about that. And we are not basing our results on these mechanisms.

Now then, to summarize all this situation, here we have the structural capability of the vessel head structure in particular, kinetic energy versus reactivity rate. Again, this is for two-phased disassemblies. What we are showing here are the two limits I just talked; one limit is the 175 to 200 megajoule impact energy in the head, and we think that is the -- well, that is the limit extended by the project to the design of the head. And this is the 100, 275 megajoule with some uncertainty for the threshold for finding the UIS.

Now, already we discussed that unless this fails, there is no way that we can release any energy in the sodium pool. And that is shown here by this green line, that up to this kind of level of energetics we have really almost no impact energy to speak of. However, as soon as the failure point is reached, then there is a jump because now suddenly, the minute the cones buckle, you're going to get the bubble, there'll be some release, and that release may be close to 50 megajoules. And from then on, gradually this increases with the ramp rate, and the other point we have made is this calculation of this point, which is a 200 dollars per second approaching the 75. In fact, for that one case that we did under adiabatic conditions, we got exactly 80 megajoules for this 200 dollars per second case.

Therefore, what this implies is two things. Number one, if one was to expect ramp rates releasing energies equivalent to less than this 100 dollars per second, there would be no structural damage to the head at all.

Furthermore, one concludes from that that inorder to cause structural damage you don't need 110 or 120, but there is still another very big significant margin on top of it that the vessel can take. We think that this is completely outside the realm of possibility, but it is good to know that it has also got that additional margin on top.

So now, then, once we go through the rest of it, we should be keying our thoughts to, number one, to this number, and number two, remembering that there is also this margin on top.

MR. KASTENBERG: Theo, do I interpret that graph correctly if I assume that the green bar means you're somewhat insensitive to the ramp rate between 100 and 200 dollars a second in respect to impact kinetic energy? Or is it just the way you've drawn it?

MR. THEOFANOUS: Well, this is a qualitative graph, but I think that this is true, there is some -- there is not a very great sensitivity, put it that way.

MR. KASTENBERG: Can you give us a physical feeling for why?

MR. THEOFANOUS: No, this has already been taken into account. This is not taken into account here. We take that in the case of when the UIS fails. After the UIS fails, we assume it to be completely displaced to its maximum position and really do the expansion process.

MR. ZUDANS: Theo, the transition is if the UIS fails.

MR. THEOFANOUS: That's why the transition. That's why you see that great sensitivity up here, so if you actually wanted to try it without the UIS, it would be more of a kind of smooth curve. Maybe that's why -- .

MR. KASTENBERG: I see.

MR. THEOFANOUS: Now we are going to go through the accident sequences. We are going to begin with the initiating phase and walk through all the terminology and the various stages until we come to the termination. Each step of the way, we are going to be concerned about one thing, and that is, what is the potential for generating ramp rates in two-phase systems on the order of 100 dollars per second. That's the question we want to answer in every step of the way.

MR. ZUDANS: I'd still like to return back to the previous -- I'd just like to tell you what bothers me, and maybe if you think about --

MR. THEOFANOUS: There's still something that bothers you?

(Laughter.)

MR. ZUDANS: Oh, yes. You see, your argument about the two-body interaction seems to hold water until the UIS phase.

MR. THEOFANOUS: You mean, there's no interaction after it fails?

MR. ZUDANS: I don't think so, because there's no reason for the motion to reverse. Once it began to go up -- for the both masses, UCS and URS, will follow up until they compact the columns completely.

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MR. THEOFANOUS: Right. We aren't saying that this occurs.

MR. THEOFANOUS: I know. Sure. We are saying interaction is not only this way. It can be like this. The question if there is a big body and there is another smaller body, and it comes up and hits it. The question is how much energy it can transmit to that big body.

MR. ZUDANS: Inelastic impact.

MR. THEOFANOUS: Not all of it because of the process of transmission.

MR. ZUDANS: That's what I said.

MR. THEOFANOUS: It goes and hits a car. The tree doesn't go down the road. The car crashes.

MR. ZUDANS: All of the energy will go into the combination of the bodies.

MR. THEOFANOUS: That's what I'm saying. One goes into dissipation in the first body. The other part goes into buckling the columns. The residual will go into the kinetic energy of the bodies.

MR. ZUDANS: And that is the residual that I am concerned about. How much is that?

MR. THEOFANOUS: The maximum of that can be 80 megajoules.

MR. ZUDANS: Eighty?

MR. THEOFANOUS: Eighty.

MR. ZUDANS: How did you arrive at that figure?

MR. THEOFANOUS: Just taking a big mass and a small mass and hit it and just the two move together.

MR. ZUDANS: Where the energy would dissipate in the buckling of the UIS, can that be computed? You could dissipate some in the sodium because there is a flow, but there would be significant piece of 340 that would be of kinetic energy of two bodies, and it will continue to crash the columns against the head, and that could be substantially more significant than that you get through the buckle.

MR. DICKSON: Conservation of momentum. If you have two bodies in collision, a small body colliding purely elasticity with no loss of energy, they reverse themselves to conserve the total outward momentum. The kinetic energy that is distributed between them is conserved, but it's not in the same direction.

MR. ZUDANS: That's correct, but there is no way for this other -- the smaller body to go back, because it has a pressure load that is accelerated in the first place.

MR. DICKSON: But if you do the calculations conserving momentum and then worrying about where the kinetic energy went, you find you have to worry about where a lot of kinetic energy went when the little body hits the big body. And that has to be conserved, independent of whether you are making it an idealized two-body problem or not.

MR. BELL: Well, perhaps. I do agree. Whether you do the inelastic two-body problem, the final kinetic energy of those two bodies moving together, it is also about 80 megajoules. In other words, the 340 is reduced to 80.

MR. ZUDANS: Okay.

MR. BELL: That 80 is continuing to move up.

MR. ZUDANS: Okay. And that would be helped by the head earlier than the pressure --

MR. BELL: No, no. It is removed several meters from the head.

MR. ZUDANS: How?

MR. BELL: Because of its initial location. It has to transfer other than what could be transferred directly through the columns which are buckling, and that is fairly small.

This kinetic energy is in the body, and it's located I think on the order of four or five meters down below the head, so by the time of a few meters -- I don't know, 20 meters per second -- by the time that moves through the distance to hit the head, the pool has already been accelerated to hit the head long, long before that, simply because the pool only has to move two-thirds of a meter, and this other object down deep in the pool has to move four or five meters.

MR. ZUDANS: That sounds reasonably all right.

MR. BELL: So you effectively have a staged impact, if you will. The pool hits, and if this body were to continue to move up, it would impact the head after the pool impact has taken place.

Let's go back to the other article where a small body impacts a big body. The upper core structure is supported. As soon as it feels the pressure, it transfers over to the big body?

MR. THEOFANOUS: No, no, no.

MR. ZUDANS: No?

MR. THEOFANOUS: This upper core structure is a, you know, is the fission -- it is a very porous thing. It's supported, but you cannot -- you cannot fail the columns here by crashing the fission gas plenum.

MR. ZUDANS: I am not saying you can. I am only against the argument that the small body hits the big body. There is no such physical process.

MR. THEOFANOUS: That's why I think we got the thing with this problem in this context.

(Slide.)

This really is the 340 number. We did that, by the way, only to account for the total, and maybe it wasn't a good idea.

MR. ZUDANS: It certainly wasn't.

MR. THEOFANOUS: But I did talk about it.

MR. ZUDANS: Yes.

MR. THEOFANOUS: -- Is that this is as you say, is in contact.

MR. ZUDANS: All right.

MR. THEOFANOUS: However, this is a flimsy structure compared to this. This is a very high load. You cannot keep this on the form, and this to start filling the loads. Really what happens is that this crashes here. It crashes only to the extent that it can transmit forces, but its own very flimsy nature.

MR. ZUDANS: Right.

MR. THEOFANOUS: And that is not until all of the walls were squeezed out of it and it and becomes a compact mass. Therefore, we -- and in this number there is no portrayal, of course, of energy absorbed in crashing that. That is why it is wrong to think of a body with 340 megajoules and hits another body here.

What happens is that during this transmission process, 20 milliseconds, this is crashed continuously. That energy is the equivalent of 340 megajoules. That is being absorbed here, and it will reach the fully crashed state not with a zero velocity, and that is the kinetic energy that is concerned with from the point of view of adding up to the top of this.

There is a loading, and this additional kinetic energy of that structure will be negligible. We said forget about all of this to satisfy your question.

Now, what is the most we can get? The most is 80, and that can appear -- the velocity of that, since it fails, but meanwhile remember that that is doing work. It fails because of this loading, and that hit will be moving upward; and typically it will have been displaced to something like maybe 3 feet, like 2 to 3 feet by the time we are in this time frame, so now you figure this out.

The upper internal structure moves with this velocity, but only 3 feet away from its original position. The head -- the sodium already has hit the head at that point and hits -- and it bounces from that. Not all of it will go in the head and comes out and hits it, displacing the UIS and comes back a wave. I don't think one should waste more time on this problem.

MR. ZUDANS: If that is the way things work out, that makes sense because you could observe it in the sodium and never see the head.

MR. THEOFANOUS: Sure.

MR. ZUDANS: Okay.

MR. THEOFANOUS: I don't see why it would work any differently.

MR. ZUDANS: Well, because of your statement. It says 340 megajoules in kinetic energy of that mass, and it has to go some place.

MR. THEOFANOUS: I am accounting for it.

MR. ZUDANS: Now, you are accounting for it? Let's see. The little piece that you showed there, it's connected to the top piece that you -- that is closed.

MR. THEOFANOUS: Yes.

MR. ZUDANS: There is no gap, so you begin to expand your interfering volume. There is a little bit of gap. I don't know what you consider the end of that, but, in fact, the end -- they are not smack up against the big plate, so that will translate -- that will move a little bit before it begins to stop, therefore begin to crash.

MR. ZUDANS: And in the process of this first phase that you described, the blue piece which is in the upper core structure collapsed. It's most of the time supported against the upper part and in the part some kinetic energy on that -- some of that will show up there. But I am buying your explanation that as it goes up in the sodium, doesn't move too far before the sodium begins to come back, and it will be in that fluid. That's okay.

MR. THEOFANOUS: All right. This interface, initiating phase phenomenon. The important physical processes here are sodium voiding; that is followed by clad relocation and eventually fuel motion.

You will remember from the last presentation we made that there is no way that one can precede energetic events by moving additional fluids. On the other hand, they do have important consequences. They affect the power and they are important because they set the stage as far as the fuel.

There are two different aspects that have to do with those processes which are the result of radial boiling incoherence, and because of the tilt and because of the presence of the walls. That causes a known one-dimensional boiling process which translates into a known one-dimensional clad melting and clad process.

We can calculate those things, and I talked about that. I didn't want to take time and talk about it here except the bottom line, that is on the boiling process. We feel that the 1D compilation correctly compilates the voiding, the microscopic voiding, both in time and in rate and, number two, the cladding location problem is going to be much, much accentuated.

As a result of that, we do cite compilations, take into account the melting coherency, and we see the effect of this plus the plenum fission gas. We believe that in the UC-3 and UC-4 cases there is some pressure for fission gas in the plenum. It will produce no net cladding location at all, and I really agree. That means that the core is going to go into the fuel motion stage with a total incomplete cladding.

Now, if ever of these two aspects -- that is, the -- if the fission -- if there is no fission gas to speak of, there could be some upward location, but that would happen only in a very, very early part of it, and it would be very, very minimal. And I don't want to go through the arguments because they really don't even pertain too much to the story because we don't want to count on anything like that in this stage anyway, but that it is important to keep that in perspective.

Now, for the fuel motion, if we are interested to look at what possible energetic events we can get from that, we have to look for forceful motion, not just normal melting and that kind of running around. And the only thing that, of course, happens in a core like this is very low sodium voiding activity and not very high power to start with, is to look at fission gas pressures.

These pressures can manifest themselves in two ways. One is for the fission gases that are retained and they produce pressure as the fuel melts inside the pin, and that can only happen in the early part of conditions. And in this core, again, we don't expect that to happen under loss.

However, the other way that they can manifest themselves is to during the early disruption stage of the fuel, let's say there's pressure on the top, it is accumulated, so those are the areas in which these considerations enter, so it's because of this then that one is interested to know the fuel failure mode and how is actually the fuel going to fail in any certain phase.

Well, one can obtain this result by carrying out system calculations, and to cover the range of the activity effects, we have gone all the way from very, very accentuated feedbacks like saying the sodium --

(Slide.)

-- Then taking the least possible doppler and all of those things into account, and you can make it slow but -- very slow activity and increase the negative ones.

Here is an example of a slow loss of flow accident for the end of Cycle 4 configuration. This is for the purpose of determining the potential for separating out the steel from the fuel. The slower the accident becomes, the more time there is between the clad melting and fuel disruption, and as long as this time becomes larger and larger, there is more opportunity for it to -- well, I made the statement before that because of radial incoherencies and because of the plenum fission gas blowdown that interferes, that we don't accept. Even the calculations we wanted to really -- could be conservative, and really there are several points, and they are not appropriate for a presentation, but we have them all in the report. We just thought that we would allow the calculation to move the cladding, that some of the things are reasonable in the upward direction, although we don't expect that to be the case.

We find even if we did that, because of the activity it needs and the power, very little time for the cladding goes to move even if you allow it move, and that is the process whereby the power increases enough to disrupt the fuel, and as the cladding mixed up well enough with it to cause a core disruption.

We referred to that as a core disruption of fuel and cladding. That is a result of the power increases, and it is observed in phases where the activity feedbacks were chosen so as to make the overall accident progression slow.

This figure contains all the important aspects of the initiating phase. We have plotted here the time, and here is a group of subassemblies grouped together from the point of view of doing thermal hydraulics, and the yellow line is sodium boiling. The green is clad melting. This is clad motion, and this is fuel motion.

Looking at the lines straight up this way, you can find out at what time boiling started and how much later cladding melted, how much later cladding began to move, and how much later the fuel began to disrupt.

By looking at a line across this way, at any time you can actually have a visual effect of what the core is doing. You find out that some of them haven't boiled yet. You can look at 17 seconds. This part up here just began to boil. This has boiled earlier, about a second and a half earlier, and it begins to move the cladding, and there is no fuel flowing anywhere.

What is the state of the core during this phase? I have shown here with arrows the way by which you take these terms of blockage. There is enough time. There is enough time between the melting of the load and the disruption of the fuel so that the cladding can separate by motion.

To figure out at what parts of the core this is possible, basically you are looking at differences between the fuel disruption and clad melting of the order of more than a quarter of a second. If there is more than a quarter of a second time there for -- conservatively, one might claim there is blockage. Of course, it will be incomplete. They are self-limiting because it's like a self-limiting situation.

The stream that causes the upward location also is cut off by the blockage formation, so before you have a complete blockage, a completely zero streaming, zero loss of the vapor, the flow is going to be reduced to the point where it is insufficient to levitate the cladding, and that will happen well before the core exit of the subassembly is blocked. So at worst you have some parts of blockages, and you can visualize in terms of the timing to form that.

sc 13

So you can see here that suddenly you have -- here is 6 -- this is the first to enter this disruption phase, so here you say well, maybe there is some blockage here. Maybe some -- a little bit more, a half a second, but there is no blockage here and not in this other -- where the cladding just melted every time that whole core disruption because of fuel motion occurs.

So this core disruption is promoted by increased power and by fuel motion that results from that, and it is from that point on the whole accident is controlled by the fuel motion process itself, so that from the moment the fuel begins to move, the cladding activities play a relatively minor role.

I want to show you a picture of what that core disruption looks like, and this is a schematic.

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beg tp 2-B

(Slide.)

We have a result of a computation here, and you can see that this is slanted lines. This represents liquid fuel. This is the actual condition, and here is fractions of material that are in different states, and those states are -- the key for those states is given here.

The important thing is that for the active core we have essentially all the fuel melting at this time. All this is melted steel also, so you can see there is a real intimate mixture of melted steel, molten steel. The boiling point of steel and the determination will provide higher vapor temperatures, so the presence of the two together, the steel produces the vapor pressures, so the steel around. If no blockage is formed, then you can see here the cladding did not have time to move up there before it melted completely, before the fuel melted.

Now, we have that fixture really -- as is shown here. It is not operated steel. It is a mixture. Now, even if that mixture which is going to try to exit were to block, the blockages would not be in the blockages. They would be head-producing blockages, so the next time there is a power increase, the blockages will melt and open up.

MR. KASTENBERG: The ordinate on the bottom?

MR. THEOFANOUS: There is the mil fractions, fraction of material. For each of those things, if you look at a fuel element or a pool, you also might think in terms of a fuel pin or subassembly. It will be the same.

Here is the picture of the end of Cycle 4, increasing the limits of the activities for clad worth and so on. What you see here is only channel 6, and it allows maybe half a second. Under these conditions we didn't think so, but that is all.

See, in all those cases, in all those, more than half of the core, there is just a very small fraction of a second available between the melting of the cladding and the fuel disruption, so that all this clearly is going to core disrupt and mix all together.

MR. KASTENBERG: Six is a high-powered channel?

MR. THEOFANOUS: That is true in all the even-cycle cores. The odd cycle, they have replaced it by blanket, so in that case that is not.

MR. KASTENBERG: I think you said before that in a high-powered channel you expected core disruption.

MR. THEOFANOUS: No, no, no. I didn't say that. I said because of the high power of the core, we bring core disruption, because as the power increases, this interval becomes smaller and smaller.

(Slide.)

Now, going back to looking for energetic events in this general framework of fuel motion, we identify two possible mechanisms. One is portrayed here mechanically. That is referred to as the LG-driven TOB. In the previous homogenous core it boosts the power early enough so that the very significant fraction of the core had not voided yet at a time in which the inner pin melting occurred.

Now, if the core is irradiated, this inner pin melting will produce high pressures. That is going to produce pin failure, and then that will produce an ejection of molten fuel into the sodium.

Now, depending where the fuel failure occurs, that may be a good or a bad event. If this failure occurs outside the mid-range of the core, this ejection of the fuel and the motion of the pin will produce motion away from the high flexation and have negative effects, and therefore a shutdown situation.

Now, we have axial motion of the fuel inside the pin. That was the mid-plane, and that, of course, is a strong positive effect. Of course, as soon as the fuel comes out, it will have to get away from the assembly somehow; but this process has to happen first before that happens.

It is important also for the timing that this process happen between the different subassemblies in the compilation, but anyway, you can see that after it beings to happen, that will increase the power in the activity, and that will accelerate the time that the other unfailed subassemblies might enter this kind of picture. So you can see the tendency.

Again, if the failure is to happen in the core mid-plane or in that immediate vicinity there. Now, for the heterogeneous core, we don't have the sodium void activity strong enough to accomplish this kind of thing by itself. However, we have also clad motion and maybe some fuel motion.

You see that channel 6, and if that solid was to melt and collapse, that in itself might have given enough of an activity boost to produce this kind of situation before completely voiding the core. So we assessed that situation by picking all, in every part picking the worst possible situation.

The sodium activity, that is the maximum. That is the normal plus the sigma. We took the fuel to be -- we took the lowest possible. All right. On the fuel we assumed that it was not very expensive, which would offer experiments when fuel is subjected to high power, which you expect if you approach this condition. It is very -- we took it to be very mild.

Even with all of that, one on top of the other, we find that that that situation, it is not out of the books, and you are not concerned with it. We thought that it was very important to establish that limit because that was one of the sticky points with the previous core; so we are very careful to kind of clear away from that.

Now, with that out of the way, we have no other mechanism for producing energetics in this case except for this mechanism of the plenum fission gases pushing on the fuel column, and therefore introducing fuel into the high flex area.

This happens because in an irradiated core the pressures can be very high, 30, 40 atmospheres. And as the accident progresses and the gases hit, they can become 50, 60; so they are very, very high pressures. And as long as the column is integral, of course, the pressures are balanced.

If you take away suddenly the center part, basically by melting it, and you see that this pressure will be unbalanced, this cladding cannot go upward, and it will push it upwards. If that process was to happen unimpeded by the interaction between the pellets and the cladding, one is concerned by channel 6 doing that, and all of the rest of the channels will be joining in the process.

And so when we approached this problem -- and the Applicant has considered this probably -- we were very concerned about how to have catalytic behavior because of this phenomenon. As it turns out, it takes time before pellets can be accelerated, and even if we assume that there is no interference between the cladding and the pellets -- and many people really disagree with that, and they think -- I agree with them that it would be hard to push a lot of pellets, if you have seen some operation, through a very, very tight clearance cladding.

We do not have any actual evidence to say they will be subject there or it will not move at all, or that they will move at some small velocity. Therefore, we take the approach let us see how bad that situation can get. We carry out the compilation as before for the reference cases at the time of fuel failure. We allow the acceleration of the pellets downwards, and from this it allows it to do that. Not all the while at the same time, and maybe like $20 per second.

MR. KASTENBERG: Is this consistent with the picture of core disruption that you showed us that I asked the question on the previous vu-graph from the appearance that you have coherent melting in a channel over a large portion of the active core, and here you are postulating somthing which looks more like what you would expect in a TOT where you have cool clad because you have coolant there?

MR. THEOFANOUS: No, no. This doesn't -- this is not done to scale, and the time frames are different. In the case here, the initial melting of the fuel will be maybe just a few centimeters, just showing here, just compress this compressible region here and the possibility of getting pressure from above, right?

The other picture was shown on a different time scale which is many tenths of a second, and this is only a fraction of a second. Obviously, you don't expect to have melting of the fuel across the -- in the core pin instantaneously. There will be some small area disruption first. So the rest of it is going to be compacted, and after this process begins, then you are talking about going to millisecond time scale. And what the rest of the fuel will be doing of its own collision is a different story. From that point on the problem is dominated by this process and not by anything else.

MR. KASTENBERG: Yes, I understand that. That is why I made the comment that if you were to believe that this were to occur.

MR. THEOFANOUS: Yes.

MR. KASTENBERG: This picture.

MR. THEOFANOUS: This picture?

MR. KASTENBERG: Then you may never get to this other state.

MR. THEOFANOUS: Yes, sure.

MR. ZUDANS: It starts to act like a TOP.

MR. THEOFANOUS: If it happens, you will never enter the state. In fact, you will probably have an energetic behavior. Again, if the failure -- if the pellets were allowed to move again, I think that is an important qualification of that, and we only assume that because we have other evidence.

MR. KASTENBERG: Do you have a picture in your packet or in the report which shows the temperature profile as you approach melting?

MR. THEOFANOUS: The temperature profile along the pin?

MR. KASTENBERG: Right.

MR. THEOFANOUS: That gives you a feeling that you can have such a vocalized phenomena, because I tend to think of it as you have more uniform heat operation. That's true.

MR. KASTENBERG: And this tends to say that you don't, that you have more of a gradient along there, and that space in the middle of the core you melt and start a little disruption, and it leads you on this path.

MR. THEOFANOUS: Well, I am only at this point. I am looking at this picture here, Bill.

MR. KASTENBERG: That's what I am saying. Is this consistent with what?

MR. THEOFANOUS: It is consistent because it never fails the whole pin at the same time. It will be some portion of the pin that is going to fail. It is not coherent over the whole length of the pin, and I don't care how much of it fails at the same time. At a very short time it begins to become soft. There is no cladding. As soon as you have that fuel becoming soft at some spot, it will begin to fill in compaction, and it is controlled by this process only.

Of course, the other important aspect of it is that there is a power distribution across the floor, the intersubassembly. We don't expect that to happen all at the same time. If it were to happen all together, it would have been, of course, a different situation, a different story. But because of this intersubassembly coherencies, because there is not enough time for different failures to come into the picture and have time to accelerate and add their activity to that whole core, begins to act coherently, so only a few of the subassemblies undergoing that. And before the other end of the picture there is never vapor pressure developed that we have this assembly and the whole thing is over. However, there is one --

Yes?

MR. MARK: You said the cladding was already?

MR. THEOFANOUS: In this particular tunnel.

MR. MARK: Only in the place that red is?

MR. THEOFANOUS: No. Only --

MR. MARK: If the cladding is gone and you don't have any friction moving the pellets?

MR. THEOFANOUS: Well, you have friction in the blanket area. The cladding is only going in the active core region.

MR. MARK: Okay.

MR. THEOFANOUS: This gas pressure has to transmit itself -- has to be transmitted by pushing the blanket pellets. So just like a piston, the blanket pellets come in and push the rest of it down.

MR. CARBON: In your calculation do you only allow for the initial?

MR. THEOFANOUS: Right. We asked the question to the project, and they told us that in fact it is possible that some of the fission rods that have migrated might even make like an easy sliding condition.

MR. CARBON: So you assume that the cladding is gone in the entire active region?

MR. THEOFANOUS: We assume that cladding is gone in the active region. We don't assume anything. We are just calculating according to the calculations, and they tell us what is gone.

MR. CARBON: But when it comes to calculating the rate at which the pellets are pushed into the center to give you your reactivity experience --

MR. THEOFANOUS: Only on the basis of the pellets that have not disrupted. In reality, there will be significant interaction here in the blanket period. So you won't be able to transmit this force in the velocity downward. We look at it in a very pessimistic, limiting kind of condition.

MR. CARBON: When you calculate the pellets in the upper part of the active region moving toward the center do you allow them to move out into the channel, or do you assume that they stay in the state column? And my reason for asking is it seems that it has been moved out into the channel. Then you would squeeze more of it in a hurry.

MR. THEOFANOUS: But now the whole calculation is in a one-dimensional sense. We can't do that in two dimensions, so whatever pellets we have, they can only move downwards, not in a radial direction. However, as you try to visualize subassembly, is a very, very tight diameter, there isn't much of a space for things to move over in radial directions. I think the real -- if you ask me, the real conservatism here, if you wish, is not allowing any interaction between the blanket pellets and the cladding, which is true, and that is going to be -- many people feel it will be significant, and many people in fact don't like the idea of analyzing this, and see the pellets can never move, but we like to see somebody prove that in a reasonable way, and that we don't see because there are no experiments at all with this kind of -- for this situation.

MR. CARBON: You spoke of the different times at which from pin to pin or subassembly to subassembly. What kind of time differences is there? You haven't given any numbers. What do you mean? What are the magnitudes of time?

MR. THEOFANOUS: I think you are asking about corewide incoherencies, and that gives you an idea of the difference in times. If you look at it this way, you can see by how much different parts of the core lacking other parts. That is for the fast case. That is aggregating all positive effects and degrading all negative effects. You could find that between the channel 6 when this reaches a point of failure of the fuel, and that would be 50 percent melt fraction.

So the changes were time, and it becomes more and more coherent. However, the process of concern also becomes shorter and shorter in time, so that coherence throughout the scenario is important.

MR. ZUDANS: On this section, how long is time compared to the core where you have one that is no longer existing in the core? By the time you reach that boundary, it will discharge?

MR. THEOFANOUS: What you are thinking here, yes, I understand the question. This blanket area is 30 centimeters, and you are talking about movement here. They are a fraction of a centimeter.

MR. ZUDANS: I see.

MR. THEOFANOUS: Before the thing disassembled. That is why it is limited.

MR. CARBON: Theo, let me go back to the question of forcing the pellets toward the center. Am I understanding this correctly? The pellets may be broken up into bits and pieces, is that so?

MR. THEOFANOUS: Not the blanket pellets. They might break. I think it is doubtful in the time frame all the way observed to the very, very end of the core they will be all broken up, because first they are going to disrupt in the high power ridges.

MR. CARBON: Let me ask a question. If the pellets are considerably broken up, could you get a higher reactive rate from pellets not only going straight down in a column but bits and pieces of pellets going out and filling up the channel so as to give you greater compaction and more fuel in the matter of the core?

MR. THEOFANOUS: No, Max, I don't think so. But the time there is expansion associated with the heating that is required. There isn't much room there.

Now, that activity rate is really controlled by the flux of fuel coming in times the worth grading, so if you allow any movement in this direction because of continuity, and your flux is going to drop, so it's already radial motions are mitigated. We have them anyway. I don't follow your explanation. If you move material in the one direction, in one direction, whatever you push in, the top must come out on the bottom. Therefore, you have the maximum flex.

MR. CARBON: Wait a minute.

MR. THEOFANOUS: I am assuming you will not only push it downwards but that some of the bits and pieces move radially outward into the channel. I mean this way.

MR. CARBON: Yes. And could you get more fuel moved into the center of the core than you did get?

MR. THEOFANOUS: No. Any of the mass from that one additional motion by radial motion, that is not available to move downwards. If you look at the sample fuel that is disrupted, cut out the core, say where can that go? All the gradients are for it to go upward because it is controlled by the fission gases that are being released in the active part of it, and the gradient is for the fuel to move out. In fact, that is exactly what happens. If this thing is going to come in, this part here actually is dispersed. It goes out in both directions. In fact, that is what we are doing in the calculations, and that is what all the experiments show, so that any disruptive fuel, the natural tendency is to move outwards from the core.

MR. CARBON: Well, it appears by the same token at the top of the active core region you assume that the cladding is gone, your fission gas pressure is going to apply throughout, not only going to apply at the top of the column of fuel pellets, but the fission gas pressure would go out into the channel.

end 2-A &
end 2-B

flws 2B

MR. THEOFANOUS: I know where you are coming from. The problem here with this logic is that the pressure -- the high pressure is up here. Now, in order for this pressure to manifest out here, it has to go through some very, very tiny clearances between the blanket pellets and the cladding, so you talk about a very effect cladding. In fact, that is the reason that we have a problem. Really, if this clearance was somewhat larger, this gas would have vented well before the fuel disrupted, so because of the tight clearances here, we retain the pressure at high.

This retained pressure is effective in pushing the blanket pellet, but not effective in pressurizing the vessel because of the larger volume out here. Much larger space. So all the gradients, then, all the pressure gradients in the channel itself are from the core out, from the center out. The gradients are from the core out, and only inside the pins and only because this pressure here is basically kept there by this blanket pellet.

MR. CARBON: So we have two reverse pressure gradients?

MR. THEOFANOUS: One is inside downwards, and the other is on the other side and is upwards.

MR. CARBON: I understand what you are saying, but I guess in my own mind I am not really convinced that there couldn't be more fuel pushed in there.

MR. THEOFANOUS: Well,let me give you an example that may be of help to you, because this process is only very appropriate for the cladding location. The pressurization of the tunnel by gas coming out is important in the cladding location picture. What do we have here? We have another -- we all it something like a 14, 15 psi, and that is the velocity that causes the upward cladding position, and considered one dimensional. And there is no bypass around the molten fuel.

Another question is, in other words, the story when nobody considers these releasable stresses. Now, we did that. You remember in the November meeting a very detailed discussion in the report, and we are saying this gas there, of course, is going to push the pellets, but even before that is going to start venting into the coolant tunnel, and maybe now it can push the cladding downwards. The same reasoning that you are thinking of. And, therefore, it will interfere very drastically with the cladding location process.

Well, we took the gas out. We compared that against the experimental data which was done with the pressurized plenum on the top, so we did a very thorough, very detailed study of that, and we found out that even in the beginning the first time the cladding fails, and that is typically about .6seconds to .2 seconds earlier from when this happens here. That means you have the highest pressures and the highest blowdowns. Even then, there is not enough flow that they come out to even reverse the pressure gradient, and that is only a pressure gradient of only a few psi.

Do you feel better about that? So it cannot interfere with the cladding under much more benign conditions. In this case, we have lower pressures pushing in. We have much higher pressure here because of the distribution and the fission gases being released, and there is no way in the world that could have anything -- that the released pressure from here can interfere with the overall pressure gradient itself.

The two processes are so -- you know, going to the cladding that is much more. I guess one would expect, if this was the case, what you say was correct, that we should have a very clear picture of cladding moving downwards under this effect, and we can grant that is going to happen.

MR. CARBON: I suspect you are right. Let's move on.

MR. ZUDANS: Could I ask a quick question, Theo? What process creates this delta t in the core?

MR. THEOFANOUS: In the tunnel?

MR. ZUDANS: That's correct.

MR. THEOFANOUS: The process I was just talking about, or what generates that adverse pressure?

MR. ZUDANS: Just the buoyancy, because you don't have any driving power there, do you?

MR. THEOFANOUS: Well, you have very little. The flow is about 20 percent down in the pump. There is only 10 seconds after the thing starts.

MR. ZUDANS: The other thing is if you apply that full pressure base of g that you have on the figure, what happens to those pellets at the point where they get out of the core? They still move the same amount?

MR. THEOFANOUS: That's right.

MR. ZUDANS: So it's really immaterial.

MR. THEOFANOUS: Because the motions are so little that can affect that.

So with this, then, we satisfied ourselves that there was no auto catalytic problem even under the very, very worst kind of assumption, but another problem came up as a result, and we did really bring this up in the previous meeting in which you remember that we had the vessel failures from all events, and we had one going from -- in fact, it was stretched out and was made to be 3 over 1000 because we were concerned about that, and since that time, we have been able to put some numbers to this, and it does turn out that because of the channel fix and the next channel to fail coming in early enough, and because of the whole accident progressing slow enough, and because this is like a slow accident progression, so everything is slow.

Fuel motion, voiding of the core is slow, and now this brings us back to the whole problem that we have happening because the primary reason being sodium voiding. Now, it happens because sodium voiding is not fast enough so to speak, and because we have the additional kick that comes in and generates an over-power condition, so I want to emphasize, so that this is not taken out of context, that this is putting too -- I really don't want to call it pessimistic, but put two bad things one on top of the other.

Well, the question now is can that be auto catalytic. By the way, this was driven at a much slower rate because this may be driven by as much as 20 dollars per second, and we are concerned about that because there isn't much known about this situation.

This is with sodium fuel tunnels, and we feel it has been irradiated. How does it fail, and how much activity can one get by this? So we are very hesitant to try to cope with that in the old ways of the homogeneous review. In fact, this was an attempt to mitigate this situation here, but if there was anything accomplished from that or learned, you can get any numbers you like by just changing things in the order if one wants new certification one way or the other.

We want to calculate this. We have done some calculations, but we don't report it because we don't think it's meaningful. It can be a bad situation, and then, it also has the ability -- we have the ability, or the project has the ability to eliminate it altogether if this pressure vent is made to act on the blanket, and that looks like it was the best solution of all, and brings us to the plenum fission gas composition resolution, which is to eliminate it by design.

(Slide.)

We have been talking to the project, to the applicant and they have agreed to consider changing their design and are writing us a letter to that effect, also.

MR. KASTENBERG: Can you give us a hint as to what the design solution will be?

MR. THEOFANOUS: One of the possible ways that one can do that --

(Slide.)

-- between the moment that the cladding fails, of course, there will be some time between the failure of the cladding and disruption of the fuel, especially in this core. It is typically half a second or more, so that the gas would have come out well before it's relevant if there were a small volume. We have a large volume and it stays there -- the pressure stays up at the time this comes. So the thought, then, was if we take a volume and break it into two volumes, one volume -- that is, the two volumes separated from each by a very, very small clearance.

Well, if that is the case, the upper volume being the big volume, when this disruption process happens, only the lower volume will be really effective in pushing that, and that lower volume can expand very quickly because of the normal blowdown while the upper volume will decay much slower, in a longer period of time, and because of that, because of the clearance, will not be able to apply any pressure, like putting another impedance up here. That means this cannot be pressurized.

So during normal operations and because of the small clearance -- and it will be a calculation and enough space for the gases to flow, but under the rapid conditions there will be no way by which this pressure here can manifest itself on the top of the fuel.

MR. CARBON: You will effectively put a little orifice there?

MR. THEOFANOUS: Yes.

MR. KASTENBERG: Do you know if there is anything in terms of other accident scenarios or normal operations?

MR. THEOFANOUS: I think the project might like to take this question.

MR. DICKSON: Maybe I could explain that a little more clearly. At the top of the axial blanket is a spring about 7 inches long. It rests at its top on what we call a plenum spacer which is -- think of it as a closed tube except that it has a tenth of an inch hole in the top and the bottom just to provide a landing at the bottom and the top, which is almost to the top of the whole pin. Four inches away, a space for the tag gas capsule. We could close that up and make it hermetically sealed and make some very fine holes in it like a couple of two-mil holes in the bottom and a couple of two-mil holes in the top. It would absorb, as Theo said, all the fission gases slowly, but would let them out only slowly.

We have actually built such pins for experimental purposes; not for this reason whatsoever. We regarded it as undesirable to do that. Obviously, you have to tailor it so that you will get the tag gas out when you want if you have a failed pin, for normal operation. You don't want to get it out so slowly that it impacts your failed fuel monitoring system. And secondly, it is going to cost something to do that.

You realize that there are some 50,000 pins in every core, so it doesn't have to cost very much to have an impact. And third, the only reason all that junk is in there is for shipping. The objective of the plenum is to give you a lot of room . The advanced fuel are trying to save the cladding from]5 mils down to 12, and here is a whole mil inside there that if we follow this path, we never get out.

There are a lot of other ways you can think of to keep those -- this pellet back from moving during shipping, besides putting in plenum spacers that take up space and use much more as a gas plenum, reducing the height of the thing, reducing the cost of the fuel and the capital cost of the plant. So we do regard it as undesirable, but can do it if necessary.

MR. THEOFANOUS· So with this conclusion, then, we feel that the energy, particularly, the energetic behavior, is physically unreasonable.

MR. KASTENBERG: Just as a follow-up, when one has a bulletin that says that applicant has agreed to consider it, does that mean that this will be revisited at sometime later, or does it just mean --

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MR. THEOFANOUS: I think this is important for the NRC Staff to answer. With the way you are approaching it, is this going to be fixed one way or another and therefore we don't have to consider it any further from our point of view?

MR. DICKSON: Let me add that we haven't had a chance to evaluate these results in detail. The project has heard of it but we haven't been able to evaluate it. Since we know of the design fix, we can commit to yes, if we agree, and see that that does need to be done. We can do such a thing, but we have not yet agreed that that mechanism is really operative, considering that Theo himself says that's a very conservative basis.

We reserve the right to look at it first, and if we can convince the NRC that maybe we don't need to do that.

MR. THEOFANOUS: Yes.

MR. CARBON: Along the same lines, how strongly do you feel the need for this? I think we have certainly -- we obviously have concern about safety, but I don't think we are pushing for something that is grossly overconservative.

MR. THEOFANOUS: Well, our own feeling is that we have taken the approach throughout this study here that we don't become so entangled with the details that we start really chasing an imaginary event. I think based on what we have seen in the calculations, we think that it is a reasonable concern that we would like to see it fixed.

On the other hand, it is not something that we can say that you are going to get all the time, but we certainly see -- and I think this is a good picture to show you that -- you see that you become susceptible to detail. If you grind it down to the point where you try to separate a few tenths of a second between the core voiding or 20 or 30 percent left over, you begin to have a problem.

Like here, for example, you can see that this shows that the time that the fuel disrupts, essentially all the core is voided except for the very low-powered parts which have just begun to boil.

(Slide)

In fact, boiling has started everywhere. It is not the classical one that you have seen before where maybe 50 percent of the core had not even come even close to such. That in itself is important because it affects the failure location. If you have time to heat up the core, the failure mechanisms become different, so there are several aspects of the problem.

What we are saying here is they are of detailed nature and they have to do with processes that we don't know how to characterize yet, so taking this plus the fact that we are saying it in calculations and also taking into account that our knowledge of it, or our direct experimental evidence on the effect of this pressure on the fuel pellets is non-existent and not likely to be discovered in the next one or two years, it is the kind of calculation that led us to the conclusion that we would like to see the core fixed in a different way. But what I am saying is if you sharpen your pencil, you can see the way out of it, but the question is as you sharpen your pencil, you lose a lot of the confidence that we have -- that doesn't allow you anymore to make the statement that I made before, that you strictly cannot see.

MR. DICKSON: Two more points I would like to make if I could. One is that we have a significant amount of time before we order the fuel to resolve this problem. The second is the probability exists that we would choose to make the design fix rather than attempt the analysis because it might be cheaper to fix it than to go through the analysis.

This pencil-sharpening costs a lot of money, so we have left that open yet, as I say. We haven't had an opportunity to relook at it, but since we could identify a fix, we could go forward with the NRC that we can fix this if necessary.

MR. STARK· This aspect matured very late, and the applicant found out about it quite late, so what they did, they sent a letter to us, which I have here, dated March 8th, saying that they are aware of the aspects that we are looking at but they haven't had time to analyze it. In addition, they indicate that the design change that we suggest to them is j-2d- 4

feasible, and since the fuel won't be constructed for several more years, what they have done in the SER is saying if you want some more time to study and analyze it, you can always come back with another way of solving the problem.

We know of one fix that exists, and unless they could show us another way, which we are always open to, we are not the designers. We just kind of set the regulations and the criteria. We know of one fix, so therefore, since a known fix is technically feasible and that exists, we indicate our criteria or equivalent, and they will have to come back and convince the Staff, whatever the equivalent might be, which happens all the time, but it's possible. So I think that is why we have handled it in the way we have.

MR. CARBON: Theo, it's 12:00. How much time will your next topic take? Is this a good point to break for lunch?

MR. THEOFANOUS: I think it is a good time. I am in between topics now, so it would be a good time. None of the topics, as you see, none of the topics are very big but there are a number of them. I guess if you wanted to go over the next topic, it could take maybe an hour.

MR. CARBON: Let's break for lunch, then.

(Whereupon, at 12:00 noon, the meeting was recessed, to reconvene at 1:00 p.m. the same day.)

END 2d

AFTERNOON SESSION

(1:00 p.m.)

MR. CARBON: Let's resume our activities.

Theo, go ahead.

MR. THEOFANOUS: We will continue with Mr. Bell, taking a bit of the time to kind of break him in here.

MR. DICKSON: Mr. Chairman, could I take this opportunity to ask a question that was asked yesterday and we said we would try to get an answer?

MR. CARBON: Yes, go ahead.

MR. DICKSON: Yesterday the question was asked if we accounted for the lack of symmetry in the loads in the bolts because of the assymetric head. We have checked. The answer is yes, we do. It is not as strong an effect as you might see. The variation from the mean is only about plus or minus 10 percent while they are in the elastic mode.

MR. ZUDANS: I would expect larger radiation in the elastic mode than in a plastic mode. Integration related more to the sheer key load. The large sheer key that you had -- sheer range.

MR. DICKSON: That too has a variation around it, yes.

MR. ZUDANS: About the same?

MR. DICKSON: I didn't check that number but we did calculate it.

MR. ZUDANS: Because I think when you do go out you find there is a significant difference, maybe 50 percent minimum to maximum load. Then you would have to model the flange in the plant test scale rather than put some plant that kind of hides this effect. If it is 10 percent, I don't care.

MR. DICKSON: Fine, thank you.

MR. CARBON: Go ahead.

VOICE: I am going to pick up, then, where Theo left off. He just finished the discussion of the initiating phase and the energetics potential for that phase of the accident, and what we would like to do now is move on into what we have called the disruption phase, which is roughly that part of the accident that continues from the early disruption in the initiating phase on out to the termination, either by fuel removal, which we have termed dispersal, or by energetic events, which we call disassembly.

So a lot of the perspective that we have looked for in the initiating phase was to gain a handle on what the conditions for this disruption phase legitimately might be. To do that, we have looked at the other end of the spectrum, so to speak, of the uncertainties and initial conditions of the initiating phase to look for that behavior which would tend to make the disruption phase more prone to energetic events. Therefore, we are particularly sensitive to anything that might change the extent of blocking, for example, of the

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normal coolant channels and the extent of disruption that They talked about. These things will play a role later on in the fuel removal processes and recriticalities to come.

In the disruption phase, as we move into that, I will review just briefly what are the major aspects of this phase of the accident that we are concerned with.

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As Theo was pointing out, what we find at the end of the initiating phase is that most of the instantaneous neutronic activity is controlled by the fuel motions. The voiding and the cladding tend to control the state of the reactor in terms of how far subcritical or how near critical it may be, but the instantaneous reactivity effects are primarily controlled by the fuel motion.

Now, what we will do is follow those fuel motions on as we progress in time along this sequence to see how they further control the overall neutronic behavior of the system. What we generally will be looking for is the neutronic activity both from the standpoint of how it manifests pressures for fuel removal and also from the standpoint of what the energetic potential is. We have laid the groundwork now in terms of what the system appears to be able to take in terms of specific ramp rate events in a two-phase disassembly. The remainder of our discussion will be to look at this scenario itself and see to what extent we develop ramp rates that even come close

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to those limiting situations.

So neutronic activity, of course, is a fundamental part of that, and as I mentioned, we will be looking for the transient pressures in terms of fuel removal and how that influences the scenario, and we will be keeping an eye out for this progressive disruption as we go through these different identified phases of subassembly disruption, and in a large scale, annular disruption and eventually to the whole cylindrical pool.

We will want to watch as we ago along to see what kind of fuel removal paths are becoming available as we go through that sequence. We will also want to be looking for the other major aspect that tends to control the magnitude of the recriticalities or their severity, and that is the way in which this extended fuel motion becomes more coherent as the physical structures within the core are broken down through this progressive disruption process.

So we will go through this phase by phase, starting with the subassembly disruption phase, and look at some of the main features of that and particularly try to finally come to grips with what the energetics potential of this phase of the accident is.

A typical type of activity which goes on in a subassembly phase following the initiating phase, where we may have fuel still largely distributed in the subassembly structure, still at a high fuel inventory state at this point, and therefore the fuel cannot really be in a gross slumping mode because it would have been critical long before that. In general the fuel tends to be distributed. It may be bunched up more on the ends.

There is a lot of coherence between channels, but on the average it is still roughly a distributed core in terms of the fuel location.

We found in every case from the SAS analysis of the initiating phase that we began this disruption with a highly neutronically active system. It was not a quasi-steady state but a system in which power transients are continuing.

In that process what that means in terms of fluid motions is the potential, at least, even in the subassembly phase, for these multiple neutronic events, which can act across the structural boundaries not impeded by them, put power into the different subassemblies in essentially the same time frame, causing a progressive increase in the coherent behavior of the fluid.

One typical subassembly, if we follow through a sequence, might look like this,where we have the fuel largely distributed but over a longer time frame of a second or more. This fuel will -- much of the fission gas will be de-entrained. It will begin to slump. With the high inventory, it will tend to go recritical into this active neutronic mode, and with the more or less classic but mild disassembly -- not really disassembly. We will have to watch that terminology. We are using disassembly to be a neutronic terminating event. This would be mod recriticality, with the power and the pressure tending to be centered about the mid-plane, the result being that the materials are pushed axially towards the ends of the subassembly, creating a highly subcritical state.

Now, at this point it is not likely that massive fuel removal would have occurred. It is still very early in the transient. We haven't even failed subassembly walls. The thing that will happen will be that the system will try to obtain more energy. It does not have enough energy at this point to overcome all the heat sinks available. It cannot maintain a steady dispersed state, and since it can't do that, it has no choice but to go recritical and try to obtain a higher energy state.

It does that in one of a couple of ways. We have defined two possible modes in which this state breaks down to achieve a second recriticality state. One is what we call a drainback mode. If you imagine the configuration looking like this, that material will not be stable there at the top. If sometime over at your kitchen sink you take a glass of water and turn it upside down with your hand over it and pull your hand away real quick, you will see this mode of fluid dynamic drainback occurring, where a bubble will grow up through the

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liquid slug and the material will drain down around the outside of it. That is a well-known solution.

Through that mode one can get the mass reflux into the puddle at the bottom, and as the puddle grows, the criticality state will be reestablished and this drainback rate would give a ramp rate and thereby will define a second recriticality.

If we do that and if we arbitrarily say that every subassembly in the core is undergoing this process coherently, that is, in the same time frame, and add all these mass reflux rates up together, and if we take then what the differential reactivity worth of this puddle increasing per centimeter really is and multiply those two things together, we find that we would have a second recriticality of about $30 or $35 per second.

MR. ZUDANS: Is this physically possible? You have both walls to contain.

MR. BELL: At this stage, yes. In fact, any other way would be really rather difficult to explain, I think. We are still at a state where these walls have not had time to be heated to their failure point.

MR. LIPINSKI: These are the subassembly walls?

MR. BELL: Yes. Remember they are starting out roughly at the sodium boiling temperature significantly below their melting temperature.

MR. CARBON: What is the temperature of the molten fuel inside the subassembly?

MR. BELL: A few hundred degrees, it is, melting point.

MR. CARBON: Which is what?

MR. BELL: That would be around 3230 degrees Kelvin.

MR. CARBON: It seems that is so high that in heat transfer to the stainless steel subassembly walls, it doesn't seem possible that the subassembly walls would exist there.

MR. BELL: It wouldn't except that what happens is the material on contact with the cold wall freezes the crust of material. The uranium dioxide has a very low conductivity, one-tenth of that of the stainless steel. So that crust of material that forms on the wall is basically an insulator, and even a crust a millimeter thick will require nearly 2 seconds of time in order to melt that wall.

MR. ZUDANS: But looking at this, your column number 2, when that happens the wall would dry out on the outside.

MR. BELL: No, not necessarily. You won't necessarily strip this the way I have got it shown.

MR. ZUDANS: If it doesn't dry out, it will not happen. It will remove the heat and it will --

MR. BELL: No, there is a film -- even if there is

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a liquid film on here, the fuel just does not have the ability to reject heat to the wall because of its low conductivity. It just cannot reject it that fast, particularly at these low temperature states. It will always form a solid crust on the wall, with the wall temperature what it is, yes.

MR. THEOFANOUS: There is no disagreement here. We are not saying that this transient is going to last for a very long time. It is going to be maybe a matter of a second or a second and a half. Now, if the walls were to fail, we would go to the next stage. What we are doing here is taking snapshots, and this is the first snapshot. It is not long-lived by any stretch of the imagination. In a second it will be over,

MR. ZUDANS: Well you see, for the fuel to melt, you would have to dry out the outside surface.

MR. BELL: No, no.

MR. THEOFANOUS: That is the wall of the subassembly. The fuel melts inside.

MR. ZUDANS: This is the outside can. Then I don't disagree with you.

MR. BELL: I'm sorry, we didn't clarify. We are at a state where the pins are completely disrupted and we are into a subassembly scale of fluid motion.

MR. ZUDANS: And you say you might have a second or two of this.

MR. BELL: That's right.

MR. MARK: So there is a fair amount of steel in that red solid stuff.

MR. BELL: If the cladding still has not been relocated into blockages, then this would be roughly one-third steel; that's right.

MR. KASTENBERG: Charlie, you mentioned at the beginning that your power is moving dynamically during this period, or at the beginning of this period. Are you at a critical situation? Are you at low power, high power, where are you power and reactivity wise?

MR. BELL: All over the map, literally. I mean you are coming into here (indicating). Typically when you are finished with the fission gas control dispersal phase and so on, you may be at most a few dollars subcritical. It doesn't take very much fuel slumping to bring you back up to a critical state.

MR. KASTENBERG: My experience has been at these stages and calculations, any little change makes you diverge either up, down, any little motion, very sensitive reactivity changes here.

MR. BELL: That is exactly right. You don't necessarily have high ramp rates.

MR. KASTENBERG: Right.

MR. BELL: But when you are near that critical point -- in fact, that's one thing that keeps the ramp rates from being very high, because you are never in a position to get momentum built up before you are recritical again.

MR. KASTENBERG: I guess the next thing is how do you justify these very clear pictures from one bar to the next?

MR. BELL: What we are trying to do is to establish, if you will, a bounding situation. This is as big as it can get even in the worst circumstances, and if that bounding situation -- and we agree that it is a bound, and if it is of this order, then we are still not challenging the system.

MR. KASTENBERG: This is really not a snapshot -- it is not a progression as you look across the picture, then.

MR. BELL: No. This is a representation of what could be going on in a given subassembly. I think that is real. The thing that every subassembly in thecore is doing this simultaneously is simply a way to get to a bounding situation. If you wanted to be concerned in the limit of this neutronic tuning, having everything going up and down together, and we don't think that's really possible because this state breaks down before about two or three neutronic cycles and therefore that is not enough to tune the whole system and therefore clearly this is a bounding type number. It cannot get any bigger than that.

Now, you can get a somewhat bigger number if you don't take what I would consider to be a more realistic type of drainback or reassembly of the puddle. If one were to postulate that this upper half of the material on its way up were to completely uniformly distribute itself and not get too much momentum such that it would free agglomerate at the top like it did here but just enough till it gravity acted on it, then it turns out that the reflux rates are a bit higher in that mode. And again on this whole core basis, it is around $82 per second.

MR. KASTENBERG: Why do you rule out a slump flow? Why can't that whole --

MR. BELL: For the same reason that I can't get it to fall out of my glass of water when I turn it upside down. It is unphysical. You would be pulling a vacuum up here at the top in order for that to do that. It would not fall. In order for it to fall, a gas bubble has to grope through it.

MR. KASTENBERG: But you are not closed on top of the channel, are you?

MR. BELL: If we weren't closed, we would remove the fuel here and we wouldn't have the problem to begin with. This channel is what I would call a leak-tight channel where you would expect some degree of blocking.

MR. ZUDANS: I am just following Bill's comment. You could begin to slow down and form bubbles.

MR. BELL: Well, I have never seen that physically.

MR. ZUDANS: A glass never falls along the wall; it always falls in the middle.

MR. BELL: I think at this point these details don't matter.

(Laughter)

MR. BELL: Yes.

MR. THEOFANOUS: I want to clarify this point that Bill brought up. The process of having liquid fall down is one in which the acceleration record is the record from the light phase to the heavy phase. That is known as the classical stability. There is no way, though, which you can have it fall down independently, regardless of what is on the top or the bottom, just looking at the interface.

Now, the size of this thing is such that at the most it will generate one or two wavelengths, and that is why the picture of this first thing that you see there kind of big like a bubble. So it is inherent that the slug would break out because of the instabilities.

MR. BELL: Now, what this does for us in this stage of the accident where we are talking about the subassembly scale fluid flows, what we are led to is that unless some mechanism exists to induce higher ramp rates than these that we are getting from this oscillatory recriticality behavior, we will not have a challenge to the system, the structural system. So the only potential challenge will come if we get into phenomena that we discovered as we were looking at some postulated configurations, primarily from the standpoint of seeing how recriticality or disassembly yields would be dependent on these configurations. Obviously, you could have a whole range of configurations here, depending on what the fuel inventory was and when it would go recritical.

So, in that process we came across something which we have termed a disassembly or recriticality best phenomenon, and it has to do with this kind of a picture here where the material is puddling in the bottom with the flux peak because part of the reactor may not have disrupted yet and be in a configuration more like this. If you take the reactor as a whole, the flux peak might be somewhat above the top of this puddle.

If that's true and now you put a mild burst on that puddle, the thermal expansion of this puddle would drive material up the flux gradient and actually give you a positive reactivity effect right at the peak power. In other words, you get a disassembly or recriticality-type boost.

That worried us for a while, but it is only this phenomenon. If this phenomenon does not exist in any significant way, then I think we are prepared to say that this early phase of subassembly behavior cannot challenge the structural part of the system.

So we have investigated that to some extent. I will not try to go into all the details of it at this point, but basically what we find is this: that if you only had this occurring on a very local scale -- in other words, the rest of the reactor is not disrupted and not coherently doing this, then the ramp rates, because only a small fraction of the core is involved, are relatively small. The ramp rates coming into this recriticality are small, and if one does have any of this phenomenon going on, you are amplifying something that is already small and therefore you never get again to the challenging energetics level.

If, on the other hand, a large part of the system has been tuned or coherent so that a lot of it has begun to puddle all at once, we have to remember that the worth curve tends to follow the mass centroid of the material.

Now, starting out with half of it at the bottom, roughly, that means the mass centroid is very close. In this case it is up here somewhere. But as the material moves down, what will happen is that the peak of the flux will actually move into the puddle, and now the recriticality, rather than boosting, is actually mitigated.

So the only possible place this can happen is if you are in the very early stages of disruption in the subassembly phase, in which time the initial ramp rates are going to be very small in this mode and therefore the boost is never powerful enough to achieve a threat to the system.

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There are many, many conditions that have to be satisfied in order to get the boost in the first place, some of which I have listed down here at the bottom. Obviously, if there was radial compliance in the system, any radial expansion, which typically generates quite pressures, might just as easily move sideways. If there is any void in here to start with, that local compliance will observe the thermal expansion and therefore you won't get the boost.

There are a number of things that you have to have just right in order for this boost process to come about. Obviously, in just postulating configurations we managed to find some that were very, very capable of producing these boosts, but we, I think, as our perspective has matured a little bit, we find out that those idealizations really have no place in this at all. They just cannot come about.

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(Slide.)

If I try to put the entire sequence into perspective and in the framework of the system continually losing fuel, what we find is that the boost mode will typically have a fairly narrow range in which it can occur; either because of the high inventories and therefore low reductions of inventory, you just can't get the ramp rates. That's what I was talking about. You don't have the ramp rates. You don't get a significant boost effect.

On the other hand, if you've lost nothing but you are such that you have to puddle the reactor extensively in order to go critical again, then the flux peak moves into the puddle, and you very quickly cause disassembly or mitigation of any recriticalities from that point on. So at best it can only possibly happen in a very narrow range of inventory and for a very specific set of conditions.

And furthermore, the only way you can get that is through this ideal rainback mode of reassembly rather than in the drainback mode that we showed on those two situations. So, therefore, we are very comfortable with the conclusion here that we cannot get a significant boost, and therefore, we cannot get a significant energetics threat during this early disruption phase. We just cannot see that happening.

Let's move on then to the next stage, generic stage of disruption where we have now seen the can walls between the driver subassemblies disrupted due to this rapid heatup from the fuel material. But because the internal blankets are starting from a colder state and they have no internal power generation, at least of the same magnitude they have, which is roughly one-quarter as much in terms of specific power, that their disruption will lag behind the disruption of the can walls, the subassembly walls, and the drivers themselves.

So we can see that the generation of the pool or the progression of the pool becoming larger as different driver subassemblies merge into this annualar pool. I've got this displayed here as again we look for in a generic way the kind of bounds on ramp rates that one might expect in this kind of configuration.

Again we start with a gravity-controlled fuel motion as the primary motivator of the neutronic activity. At this point just about everything else is gone in terms of reactivity effect. If you visualize this as a particular starting point for one of these neutronic cycles -- and of course this is not unique; there's nothing terribly generic or unique about this, but it is simply meant to show a tendency for puddling at the bottom because typically you will have lost some fuel by this time, so you have to have some degree of slumping to achieve a criticality state.

But beyond that, this is a relatively chaotic distribution material. Now, it goes recritical. I have a power shape that looks something like this. Again, it's peaked down at the mass centroid, and it's also peaked at the radial inside of this annulus simply because the rest of the core is over in here tending to pull the flux up, whereas at the outside you have nothing but blanket, and you have a leak flux going on there. So the power leak radial shape will tend to be peaked in here and die off on the outside. Therefore, when I have this next recriticality, my hot point will be right in here at the inner radius at roughly the axial mass centroid.

The result then if you look at the flow dynamics of that kind of a bubble growth through heating a region here that's preferentially expanding just like a disassembly, that vaporization and pressurization at this point will cause these fluids to want to move in a typical type of motion that we calculate and observe experimentally, because we've actually set up some experiments nearly full-scale with water with introducing gaseous sources to represent the disassembly here to try to follow this fluid dynamics. And indeed it does appear to have these kinds of characteristics where the bubble grows and because of the inertia being less at the top, there is a bias in the growth of that bubble upward and outward.

Momentum in this fluid here is given in the early expansion phase, and it's given a momentum upward and outward.

And as that momentum continues to drive the fluid, the fluid tends to collect out of the wall, and its vertical component tends to carry it on towards the top as the bubble breaks through.

That momentum, depending on the strength of this recriticality, if it's very mild this momentum will not be enough to carry it all the way around the circulation. It will tend to climb the wall, turn around and fall back down. That's a very low ramp rate as a result of that.

On the other hand, if the momentum is high enough, it tends to have the circulation pattern. Now, what that does effectively is if you look at this picture, half of the mess is down here, the other half is up here, but this half that's up here is really distributed over two lengths of the system. So, therefore, as it circulates and comes back again, it will have roughly one-half the reflux rate as it would have if it were all just draining down together.

Consequently, you calculate in a circulating mode ramp rates of roughly $35, $36 per second -- again, no real threat to the system. Again, if on the other hand you know there may be some subassembly wall stubs sticking down here at the top or other dispersion sources here at the top such that this climbing sheet is dispersed into a rain here at the top, I think that's very ideal in the sense that it's perfectly distributed and then rained back.

What we would visualize as a real upper limit on this would be like $72 per second -- again, well below the structural threshold for vessel head failure.

MR. MARK: Don't put the other slide back on. But in a slide or two ago you had that thing that Bill Kastenberg was asking about where half the fuel is condensed at the top and half at the bottom. And you pointed out -- and I don't argue against that -- that you can't think of that top thing coming down as a slug in real life. It will break up like water out of a bucket, agreed; but nevertheless, I think if you imagine that slug coming down under gravity, you would break it off these other, more complicated variants you have shown of rain, et cetera, and have a ramp rate that's probably higher than any of the ones you put on the slides.

Is it so that that ramp rate even is not so high as to cause you a problem, or is it too high?

MR. BELL: I don't know that magnitude. Do you, Theo?

MR. THEOFANOUS: That would be about three times higher, so 36 times 3 is a little bit over 100. And as you remember from the last concluding slide of my structural presentation, it removed that problem. If you applied the factor of 3 on the rainback on the $70, then you were pressing the limits.

MR. MARK: I was wondering if you knew a number to go with that, because that's a geometrically simple set of assumptions.

MR. BELL: I guess what you're saying, this would be roughly a factor of 3 larger than the numbers we have.

MR. MARK: Three on top of one of the numbers you have.

MR. BELL: Of which one, the rainback number?

MR. MARK: If it's 3 times 36, then you would have said well, there's a limiting case, and everything is still fine.

MR. THEOFANOUS: Yes.

MR. MARK: If it's 3 times 70 or 80 cents, then it's not so immediately obvious.

MR. THEOFANOUS: You mean $70 or $80.

For the rainback we have about $70 times 3. That's about $200. And what we're saying is in that extreme limit we are approaching the threshold.

MR. MARK: Is that dollars or --

MR. THEOFANOUS: It's dollars, dollars per second.

MR. MARK: Oh, you said it's $82. You'd get $240, and that would be a little high.

MR. THEOFANOUS: A little high, yes.

MR. BELL: But that would in fact require you to stretch your imagination even further, because now if you're bringing things back in a given subassembly more rapidly, that means I have to require a closer coherence.

MR. MARK: I wasn't arguing for it as probable event.

It seemed to me it had to be limiting, and if it were also tolerable, then, of course, you could save some of these drawings.

(Laughter.)

MR. BELL: That's right.

MR. LIPINSKI: I think that last slide of your annular pool -- has this phenomenon been verified through analysis with the code?

MR. BELL: These patterns are actually calculated by the code.

MR. LIPINSKI: What are you using and what are the assumptions in terms of boundary conditions?

MR. BELL: We did this with the SIMMER code, the same one we are using all the way through this, and seeing a lot more of it through here. These are rigid boundaries all the way around here.

MR. LIPINSKI: Are they six-sided?

MR. BELL: These are annular.

MR. LIPINSKI: If you don't have a six-sided can, if you assume a cylinder in order to make the analysis --

MR. BELL: There are no cans at this point, right. We are out in advanced disruption stage.

MR. LIPINSKI: Okay, I see. I'm with you now.

MR. BELL: This outer boundary might in fact be jagged if you chased it around.

MR. LIPINSKI: Now you've smoothed it out.

MR. KASTENBERG: The line on the left in each figure which looks like a center line --

MR. BELL: That's really the inner radius of the annulus.

MR. KASTENBERG: Okay. And that is solid still.

MR. BELL: It's porous.

(Laughter.)

MR. KASTENBERG: I guess that's what you want it to be.

MR. BELL: Well, you can imagine it as being -- there are subassembly structures there, but the gaps between subassemblies are open, so there can be some small passage of material back and forth between this region. And those cans are being melted, so you know at one instance in time it may look like subassemblies, and the next instance it may look like a half a subassembly and on and on.

(Slide.)

Now, we move on to the whole core pool, and I think not surprisingly that if there's a threat to be had to the system it is in this phase that it would come from. And I think we probably all were able to forecast that months and months ago, but perhaps there are some new things here that we have not up until now really been concerned about. But they turn out to be very major aspects of this early whole core pool behavior.

I'm going to make the distinction here in the whole core cylindrical pool between its early behavior, which I will also call its unhomogenized state, and its longer term behavior in which the internal blankets, which I have shown here in purple, it's simply a collapsed rubble form. This long-term phase would be when these blankets are homogenized into the system. It turns out that the whole-core pool has a very fundamentally different behavior in those two arrangements.

MR. LIPINSKI: Is that core center line on the left?

MR. BELL: This is true core center line, so this is roughly to scale, roughly a meter radius and a meter high. All the structure within the core boundary has been disrupted. It's all in a mobile, largely fluid state at this point. And again, as we look at some of these idealized calculations, we can think of these boundaries as being solid.

The other reason that the early behavior of the pool or the unhomogenized behavior of the pool is different is because all of this blanket material in the middle does one fundamental thing, and that is to shift the power peak outward. Instead of one peak at the center line in a normal fundamental mode, it is peaked out here in this annulus region.

Now, that means that whenever I have a recriticality event, my peak energy deposition is going to be out here, and a slushing mode, that's the one we typically worry about the most in terms of getting larger ramp rate events. It's not the classical slushing mode that starts at the middle where material sloshes out and then runs back in a bowl-shaped slosh. It's going to be more of a confused slosh now where material is going to tend to split at this radial location and go both ways as well as upward.

Now, it turns out that is a very, very fundamental change to the way the ramp rates are developed and tends to keep the pool in a more or less confused state rather than in this highly organized state where you can slosh back in with high ramp rates.

So the early behavior is not a central sloshing. And furthermore, the ability to homogenize this material, if you're tending to generate the sloshes out in this region, there's not very much of a homogenization potential here. If you were centering your power in here, then obviously you'd be driving all this material out and mixing it up. But if you're centering your power out here, and it's actually more dramatic than what I've got it shown here, then you're tending to, if anything, keep the material bunched in the middle.

Now, what this does for the whole scenario is that it allows a time period beyond the initial formation of this pool for additional fuel removal. When you get to this state you've got enormous fuel removal paths along this outer boundary. If there's time available to move material into those paths and remove it from the core, you can literally have a very high assurance of reaching permanent neutronic subcriticality before this homogenization process is completed. And that's the theme then that we will want to try to walk through as we go on down through this disruption scenario.

MR. MARK: In this picture you're not assuming that any fuel has already penetrated down into the lower blanket.

MR. BELL: It need not. In reality it really has.

MR. MARK: You say it might freeze, so we'll assume it does.

MR. BELL: When we talk about the actual analysis of the scenario, these are just kind of postulated configurations at this point, but the actual analysis, we are in fact having fuel move out of these regions continously all the way up to this point in time. And what we find is that with this material in this general configuration -- in other words, nonhomogenized -- that the threshold for subcriticality has been changed; that we really only need to remove something on the order of 20, 25 percent of the inventory to reach a subcritical state, continuously subcritical state, even with the material completely slumped.

Now, without that or in other words if this is all homogenized, then you have to remove something like 35 percent of the inventory to achieve that permanently subcritical state, so the lack of homogenization not only keeps the neutronic oscillations under control, keeps them to a lower ramp rate, but it also tends to put you to an arrest, if you will, to an arrest period in this whole scenario where you are temporarily subcritical and maybe even more than that because obviously if you're subcritical, how's this stuff ever mix.

Let's go on now then --

(Slide.)

-- And explore this a little bit further in kind of a bounding way again. To simply say what the situation would be if I went all the way to the limit of having things completely homogenized, we have all had some concern about these sloshing modes and what kind of ramp rates could result from them. So we decided that we would in fact actually attempt to do some sloshing-type calculations with full coupled neutronics to see just what the feedbacks really were from these things.

Here is an example. If we imagine again some partially slumped initial state with everything homogenized and then assume that there is a perturbation applied to this system, either mild recriticality or some sort of pressurization source that is centrally located, now a neutronic event will tend to be centrally located; that's one reason we have been worried about it in the past.

Pressurization events if they can occur -- there's nothing that says they need to be at the center. They could be anywhere. But the most organized sloshing behavior and therefore the highest potential for a high magnitude ramp rate must come from a centrally located slosh.

So now we apply our perturbation here to center line and again at the axial mass centroid of this. What you find is again that the material that tends to be driven to the outside -- this is the classical disassembly, only it's going way beyond disassembly. Normal disassemble calculations move material a centimeter or two, and you have neutronic shutdown, but you put momentum in the system.

What we're doing is following that momentum on out in time and asking the question what happens then. If I haven't removed fuel, I temporarily disassemble. This is a disassembled configuration. It might be $20 subcritical, but the accident is not over if there is still a high inventory of fuel within that reactive core region.

So now what happens at this state in time which might be several tenths of a second after the original neutronic event, which, by the way, might be a very large event; this might be just a few dollars per second type event initially -- we have a system here now that's pretty much pressure equilibrated, nothing but gravity to act on it. Most of the momentum has been disspiated, and gravity simply begins now to pull this. It would go down to here. This material at the top falls down. The material at the edges will tend to drain down and then have to drain in as it wants to reach a common level here. And that's what I have depicted at this point. The top material has fallen down and formed a region in here. The other material is simply draining down the wall. Remember, there is a radial convergence here. So even though it doesn't look like there's much material here, by the time you start moving it across the bottom, it starts to look bigger and bigger.

MR. LIPINSKI: What provides that top boundary?

MR. BELL: Here?

MR. LIPINSKI: No, the next one.

MR. BELL: This top boundary here would be normally the upper axial blanket which has been plugged up with either fuel trying to escape and freeze in that location, or from prior steel blockages from the initiating phase. Typically it is not an absolute boundary.

MR. KASTENBERG: These are SIMMER calculations again?

MR. BELL: This is a pictorial representation of the actual SIMMER calculation.

Now, from these calculations what we see is that when the pool is reconfiguring in about this state -- in other words, the mass of the material really has not reached the center line yet; it's what we call a partial in-slosh or inward slosh. What you find if you simply do some K calculations in a configuration like this and simply advance its state, pull this down and move this down a little bit, you can actually get a progression of ramp rates if you know the velocity of material here, or you can construct a radial differential worth curve.

What you find out is that typical velocities across here are like a meter per second, and the differential worths are on the order of a centimeter again. That's a very magical number. It seems like it crops up all the time.

Consequently, in this configuration one can envision for this organized axisymmetric centrally located slosh ramp rates on the order of $100 per second. If I let that slush progress under this state where it's come on down all the way in and starts to overslosh and build up in the center, which again is not an unreasonable thing to expect since this momentum has to be dissipated somehow, and the only way to do it really is to turn the corner and let gravity work on it, so it bulges up in the middle.

What we find again from doing the differential K calculations is that the minute this material reaches the center line and starts moving up, we find that the differential worth changes dramatically. You're going into more of almost a hemispherical type of reactor, and that's saying the inward velocity when it's in this configuration starts to look like $300 per second. And it's the worrisome-type numbers that we've been confronted with in the past.

But let me point out one very significant point here. This configuration at best must have a fairly low void, and in fact if does have a low void, that $300 per second is nearly meaningless. It does not produce a significant energy yield. In fact, the actual calculation that we did had one that went recritical at about this configuration with $100 per second and another one that went recritical at $300 per second. This one produced twice the energy of this one, and it's simply because to get that high ramp rates in the filling of the central region and this turning of the momentum, it has to be single phase.

end AR 2

MR. MARK: This has to depend upon the power level from which you start. If you are at fairly high power you will reverse that thing on the lower right before it ever gets there.

MR. BELL: Well, you're coming from a highly subcritical state, so typically you will be coming in there at relatively low powers.

MR. MARK: You will start to vaporize junk in the middle, and as soon as it starts that--

MR. BELL: That will eventually crash around, but what I'm saying is that with these high ramp rates, we're only talking, what, three more seconds of time between critical and prompt critical, and it's a very, very short time to overcome any momentum, and if the powers in that range are 10, 20 times nominal, you are only heating material up, you know, a few tenths of degrees. The calculations show --and I think that kind of a fairly simplistic reasoning also confirms that you really won't get much of that happening with these high ramp rates. At low ramp rates you will tend to get that mitigating feedback coming in due to vaporization on the way to prompt critical. But here you get there so fast it really doesn't do much.

MR. KASTENBERG: Charlie, could you go back one viewgraph?

MR. BELL: Yes. To understand what differentiates this case or this kind of a scenario is the heterogeneous core giving you that initial power shake.

MR. BELL: Exactly.

MR. KASTENBERG: Did you do some sensitivity calculations to see what would happen if you got some redistribution of the blanket materials and you changed that?

MR. BELL: In the whole core calculation; these are just spatial effects, calculations I've talked about so far where we've started with idealized initial conditions.

Now, what I'm going to show you next are a few results of an attempt to mechanistically calculate all the way through this thing from the SAS conditions, and there, of course, you have a continually changing configuration. I don't think I have enough details here to really eliminate that too well for you, but we will see some of the effects of it, nevertheless, in some of the overall results here. But indeed, that is continually changing. And what you find is that every time you have a mild burst, this whole power shape changes dramatically.

MR. KASTENBERG: That's what I was wondering. Once you get into some movement it will forget what the initial condition would be.

MR. BELL: Exactly.

MR. KASTENBERG: Then I wouldn't see why the heterogeneous core would be different than the old homogeneous core.

MR. BELL: When you have a mild event and you move this material, typically you're moving it into a higher leakage condition. What that does, if you take this material, for example, here, that's on the average much higher enrichment than this material, the flux tends to be peaked out here. The power burst then is centrally located here and will tend to move this material up on a sheet on the side in some confused motion back in here. This material on the side will become a very high leakage configuration, and what you will find is that this flux will peak and go back to the fundamental mode kind of thing. The coincidence with that is the fact that your systems is 10 dollars sub-critical. Then when it reconfigures again into a critical state, you're right back to the same over-enriched region out here, and it comes right back to that same kind of general power profile.

And what we find is that after a number of these events, this will tend to start to be mixed, and what you find is that you'll go recritical with this thing being essentially flat, for example, and eventually as it's mixed more you go back to the fundamental mode and you'll see that whole progression changes.

MR. KASTENBERG: But all along you're assuming this is all bottled up, right?

MR. BELL: No. In the actual calculations you're not assuming that. In fact, that is the key thing you're asking;

this is what we would call the -- in the development of the clearance in the system, going hand in hand with that is the loss of fuel from the system which is trying to take it subcritical, and these are two very important features that one tries to consider, and we will talk about this whole business of dispersal or removal here in a little bit, because we've tried to tag it, also, in these different stages in kind of a generic way, using this whole core calculation as sort of a background perspective to give us an orientation of what's going on.

In this by the way, we basically ran across this whole behavior as a result of that whole core perspective.

(Slide.)

Let me go on and just say a few words about the type of results that one gets when you try to make a stab at analyzing this thing all the way through. There haven't been very many of these kinds of calculations done, and they are literally of a project nature to turn in and do them. So you don't expect to do a whole lot of sensitivies in these kinds of things, and, therefore, we have chosen to use them not in a mode of saying this is the answer,but more in a mode of establishing perspective for us, and then we'll go off and do the separate effects and even idealize things a little bit. But at least we have this background which is a kind of benchmark for idealizing. We just don't pick things out of the air.

Well, you see a very complicated picture here. What we have is the green lines are the reactivity state of the system as a function of time. Times 0 here is at 19.757 on the SAS calculation that preceded it. It happened to be the SAS calculation that Theo was terming as the slow development of that transient. We chose that one because it would tend to maximize the blocking of the system prior to this whole phase, and we were trying to edge towards the conservative side of things, so we chose it that way.

The SAS calculation up to this point had already manifested some neutronic activity. There had already been several swings up and down in the reactivity rates. The SAS calculation was, in fact, run out to this point. It had run through this burst. We did it purposely so that there was an overlap in the transient, and we can check the SIMMER calculation of this power burst versus the SAS calculation to see if we have made the transition in a reasonable fashion.

Indeed, in this case, the power burst occurs at very nearly the same time and is of almost identical magnitude. We feel like we have preserved the system reasonably well in making the jump, but what we see here is the very cyclic neutronic behavior during this first 1 1/2 seconds. And it turns out to be synonomous with the subassembly phase of disruption.

Now, what is going on there is literally the simple up and down motion that we were looking at earlier in the cartoon fashion where the neutronic event burps this material up into the air, gravity pulls it back down. You look at the timeframe and it's exactly the time you would expect for gravity to reassemble the material. It also manifests a very gassy phase of the exit of fission gas and there was some cushioning going on. In the reassembly process you have these nice single-phase puddles at the bottom.

Then there is a very distinct change in behavior here at this point. Also notice I have tried to plot on here -- the red curve is the fuel inventory in the active core as a function of time. It starts at about 16/100 kilograms of driver material, so what's happening is that every one of these little bursts you can see there is a change of slope here on the inventory, and it's simply the material being burped up against the top. Its momentum is actually carrying some of it into the axial blanket. There's also some fuel removal going on here through the gaps between the subassemblies of the internal blankets, and it turns out in this calculation that those were not very effective; they do not add very much to the fuel removal, primarily because of the way we've modeled them, and we intentionally did that because we wanted, again, this perspective to tilt on the conservative side of things. We didn't want in fuel removal that couldn't clearly be defended.

So what you see here is a reduction during the subassembly phase of nearly 13 percent of the inventory. Roughly speaking, 1 percent of inventory is almost 1 dollar in reactivity, so you would expect that at that point I have not removed enough material to keep the system sub-critical if indeed it puddles at the bottom.

This minimal change in behavior here comes about because at this point, the internal blankets have begun to break down, become mobile. In other words, the annular pool space is deteriorated at this point and we're going into the cylindrical pool phase.

This last recriticality actually was able to cause radial movement of material. Therefore, it does not reassemble in the same timeframe as it was in the simple up and down motion.

The other thing that we see going on here is the gradual puddling of the core in the reactivity state, coming back. Now we are basically in this phase, and here to here we are in this non-homogenized whole core pool situation, and again, it comes back to critical. And then what happens here is exactly what we expected would happen. You start with a very small reactivity event and the thing grows with time. This has been seen in a number of these kinds of calculations and it's simply a manifestation of the fact that you've disturbed the puddle a little bit and it comes back slowly. You disturb it a little more and the next time it comes back a little faster and then more and more. And obviously, the thing is growing. This is a log scale so this one last one doesn't show it very much, but it also would manifest a very large swing. This is 30 dollars sub-critical at this point; very large reactivity swings because these motions are very coherent and on a core-wide basis. But the interesting thing that we see here in these very mild events, this last one was sufficient again to drive a lot of material in the axial blanket. Let me point out that this calculation was done assuming that this outer circumferential boundary of the whole core pool was 100 percent solid. No fuel escaped past at all.

We will see here in a minute that the gaps that come in from that boundary -- in other words, the gaps between subassemblies of the radial blanket and radial reflector region constitute a very large fuel-removal area. Those gaps are guaranteed to be open; there is no mechanism to close them. They do have sodium in them. The sodium has to be ejected, but all during this period, basically from here to here (indicating) those gaps would have been opened had we allowed them to be open.

This calculation shows that even without those large gaps and without control rod fuel removal we have driven the system down to something like 23, 25 dollars sub-critical at this point, and note that it does not come back critical again. We have gone below the criticality threshold of the system.

Now, that is completely in line with what we spoke about earlier. When completely slumped, the non-homogenized pool -- it has a criticality threshold which is higher than the homogenized pool. It's around that 20, 25 percent level, so what this simply shows is that we've achieved neutronic shutdown here. If there were some mechanism to rehomogenize and get about 10 or 15 dollars of homogenization reactivity coming in, then it would bring it back again. Our perspective at this point is that even this is pessimistic, and if this had actually been open it would have been clearly shut down by this point.

MR. MARK: In all of these considerations there is no control rod material, no absorber?

MR. BELL: The control rod for this particular core state, which is into cycle 4 -- six control rods are only roughly one-sixth of the way in from the top. In other words, it's basically at the end of the burnup cycle, so they're nearly all out when the accident starts.

MR. MARK: This is what you call an unprotected accident?

MR. BELL: Yes.

MR. MARK: But that is not an LOFS.

MR. BELL: This is an unprotected loss of flow accident.

(Slide.)

So even from this unhomogenized pool phase, what we tend to see is a damped type of neutronic activity. We see a large window for fuel removal and, therefore, we don't see any credible potential for threatening energetics, even during that part.

Here's the point of the instantaneous ramp rates through that same sequence, and as you can see, they are pretty volatile. These large negative ramp rates are characteristic of disassembly ramp rates. But in general, what I want to point out is that the general magnitude of these things is technically in this 40, 50 dollar range, except out here at the end, where we see this oscillation building up and then we see an upward trend in the magnitude of the ramps when it goes critical again, and that, again, is perfectly to be expected, that is what would happen.

This last one gives gets up to around 70 dollars, and, of course, we were subcritical by this time so it never had a chance to do anything.

(Slide.)

I'll point out briefly the significance of this flux tilt. If I look at an RC map of the specific power which is proportional to the flux, here is the center line of the core. We are going radially outward. This is a situation of three seconds in that calculation right before the centerline began, and that is the actual flux peaking. A very dramatic peaking out there in the outer region. In fact, it begins to look very much like a spherical reactor -- more like a toroidal reactor, I guess. But this is the kind of power peaking that has a sort of fundamental damping built into it, because you can move materials literally in four directions away from the power peak and get very efficient neutronic feedback from the fuel motion.

Now, just a little bit later in time -- we just happened to pull this one off after a power burst where the material had spread out, and you see the complete shift in the flux shape. There's a very dyanamic system when you're moving materials around to this degree. It's very important --

MR. KASTENBERG: R is in the radial direction. What is C?

MR. BELL: This is the bottom of the core and this is the top axial.

MR. KASTENBERG: Okay.

(Slide.)

MR. BELL: Let's summarize ramp rates, and then we'll go into the fuel dispersal part of it, and I want to try to walk through with you what we really think the fuel removal or fuel dispersal characteristics and possibilities at least are for this core, even though we, in this calculation, took it to be very, very conservative in terms of fuel removal by simply not allowing it to take place. So by summarizing here in the subassembly pool phase, we clearly see energetics less than 100 dollars per second. In fact, I guess it's only in the very idealized situations that we see anything even approaching 100 dollars per second.

In the annular pool phase this situation is essentially identical, recalling again that the 100 dollars per second threshold was the structural threshold at which you just begin to get loads in the primary system on the head structure. You just fail the UIS at about this condition, and in both these phases we see the situation has been well below that, well below that. So it's only in the whole core pool phase then that one sees any possibilities for threats at all.

In the non-homogenized pool, from that calculation we just showed we have several opportunities there for the ramp rates to grow, and they did, in fact, grow, but still never even came close to 100 dollars per second. It's only in the homogenized pool phase that we have even identified ramp rates that would get above the 100 dollar threshold. And in fact, in the one very idealized calculation, we actually did get to the 300 dollar threshold, but it's non-energetic. So in terms of energy threats, we see the potential being maximized out here in the homogenous phase, the pool phase, which is certainly no startling revelation. But what is interesting is that we believe from those calculations we have done, it seems to be fairly clearly indicated now that the only way you take the leap, if you will, from this class of ramp rate up to this class is by that central compaction into the middle into the centerline region, and that fundamentally has to be connected with a low void fraction configuration of material which, in turn, is non-energetic and basically immune to ramp rate.

So if I were to put a bottom line on that, I guess what I'm saying is that we simply see no critical threat to the structure of the system, the head structure, as we have defined its capability to withstand events up to 200 dollars per second in the two-phased disassembly mode.

MR. KASTENBERG: Charlies, in breaking up the calculation, or your approach, by looking at these three phases -- which I think is an interesting and reasonable way to go -- did you check to see that during the transition from your subassembly phase to your annular pool phase that you couldn't introduce something which would give you a high ramp rate?

For example, melt-through cans? Are there any phenomena that you may have overlooked, or are you convinced these are the places where if you would have a higher ramp rate, it would be these places?

MR. BELL: We tried to go to the upper limit of coherence in each phase and see what kind of ramp rates could possibly be manifested from that. I don't know if there's anything significant there. Nothing has come to mind that we clearly understand to be a major problem. I don't know -- again, keeping it in perspective, trying to keep fairly close to reality -- I'm sure we could dream up something, you know, postulate something that could, in fact, drive us into a bad situation.

MR. THEOFANOUS: Bill, in the transition from the subassembly to the annular, you enter that through reduction in the walls, and that tends to be kind of like a quenching. Is that so? If anything, during that stage I don't believe you will see the potential for developing any big things but axial things. And really, you have the same problem there.

The only suggestion would be if you go from the annular pool stage and you introduce a pressure source, you would --

MR. BELL: Yes.

MR. CARBON: Theo told us the last time that your analyses are based on not needing to rely on large computer codes such as SIMMER. How much of this conclusion are you really basing on SIMMER calculations of which I'm inherently suspicious, but how much can you base on reproducing them, so to speak, back of the envelope or anything that you can check that's fairly simple, that you can't have much doubt about?

MR. BELL: I think that's precisely why we wanted to investigate some of the simple gravity slump configuration for example, I think that's something we can agree on there's nothing very mysterious about.

The SIMMER calculations indeed have played a big role in the understanding of the expansion process, but what we have done -- the material that Theo talked about earlier -- we have intentionally driven it into a mode where it is purely fluid dynamics and not taking into account any of the fine details of heat transfer and those kinds of physics; they are difficult to quantify.

Some areas we are using it, we're using it primarily for perspective and then we're backing it up with some actual experience in these regimes where things are sensitive, like the annular pool and the whole core slushing pool. We're actually doing experiments and comparing the code to those.

If we had time we could actually show you some movies of the calculation and the experiment side by side, and you could see the report itself does speak to that issue.

What we are trying to quantify in particular are these ramp rates from sloshes. We have a section in here where we tried to benchmark [illegible] analysis of the fluid dyamics directly against these experiments. Even that alone doesn't need to stand by itself because one can go through these differential K calculations to see how the reactivity of the system is changing in a static mode. We have done that, too.

We have come at this from several different angles, and I think are viewing it very much on a first principle kind of basis, so in that sense I feel very confident.

MR. MARK: I was going to say next, you should probably withhold your suspicion of the SIMMER calculation as used. When SIMMER is trying to discuss drops in fuel pumping out or not pumping out of a little hole, that is doing a thing which I think everybody deserves to be suspicious. But these are rather large-scale things. You do have to do a complicated calculation to get this two-dimensional fluid mechanism in neutronics, but they're not -- they're sort of kinetic calculations, and you use this big machine because it's the easy way to get the integral of the ramp rate while the thing is swishing back and forth. You can't do that easily, you can't do it on the back of an envelope. All you can do is say well, it will swish back and forth. I wonder how high it will get.

It's not involving the things that I think must involve your suspicion about the SIMMER framework.

MR. BELL: I think in many ways the whole core calculation does involve those kinds of things that one should be suspicious about. And in fact, I think we have been duly suspicious and that's fine. We're not saying that calculation, as it stands, is the answer. It's simply a perspective on the kinds of things that --

MR. MARK: Now that you've assumed a starting condition and you're suspicious as to whether it applies, but once you --

MR. BELL: I think what we have learned, if anything, here is that without some kind of perspective of that type you can idealize. You're into a very difficult situation simply because you don't have the perspective that you need to have to simply integrate all this high, non-linear mess together. You just cannot generate that perspective in the head.

MR. CARBON: Well, it isn't clear to me that a computer code that I don't fully appreciate how much has been checked and benchmarked and so cn, couldn't give you the incorrect perspective, so I'm going to ask the question: have your experiments of which you speak -- are there enough of those that by themselves they put you on pretty firm ground in terms of perspective, and are they meaningful experiments?

MR. BELL: I think the answer is yes. Theo has been itching to respond back there.

MR. THEOFANOUS: On this question, we take the approach that SIMMER is a very, very general tool. You can do a lot of different things with it. Therefore, for such a thermal tool, to think in terms of it as a final tool thing is-- We would use it for a specific task that is very well identified, and then we try to make sure that for each particular task we check it either through other analysis or through particular experiments to make sure that the calculations for this particular task is done correctly. So it's in this light, for example, that SIMMER has shown that in this annular pool, the bubble was breaking up very quickly and producing the sloshing action. While recognizing that SIMMER does not take into account all the instabilities that are present there, we were suspicious as to whether it was calculated correctly. But in fact, we found very good agreement in the whole process that gives us a very good feeling of what we can do with it.

We have not covered yet, but we have a calculation now that we can apply to a particular experiment in this prototypic material and actually looking at this particular problem. We calculate -- make sure the calculation is correctly reflected in that. So in that sense, we are taking a step-by-step approach and making sure that every step of the way, SIMMER is doing a good job.

One aspect, however, I must say everybody seems to be emphasizing, and correctly so, is the sensitivity to detail of some of the dynamics here, but one aspect I think -- in fact, we rely a lot on it -- we rely on SIMMER.

MR. CARBON: In which aspect?

MR. BELL: In the neutronics aspect. If you cannot do it in your head, you certainly cannot do it with codes any better, because SIMMER is the state of the art. When you get it slushing back in, it's not only the motion that's coming back into the picture; also, you need to know what is the reactivity rate, and what ramp rates are as a result of that. We think this is the best way we know how to do it. We don't know of any other method to do it better.

MR. LIPINSKI: You've got an unhomogenous mixture, you're keeping track of fuel and blanket materials, you're moving around in three-dimensional geometry. You're bringing us back and you're calculating ramp rates. How do you know that's being done right? You've got to know where the material distribution is in the three-dimensional geometry.

MR. THEOFANOUS: I'm saying that the controlling aspect that has given -- I'm saying that SIMMER does the best job we know of any code in that area.

MR. BELL: Actually, there has been some benchmarking done. NRC actually ran it through the critical experiment some years back where they took the core and slumped it altogether at the middle and slumped it out top and bottom, several different drastic configurations like that, and then various codes and various methods were used in an attempt to calculate those various configurations. And if I recall, I think SIMMER probably came out better than any other tool around in calculating those criticals.

You can obviously do some benchmarking against other codes that in some ways are neutronically different, constructed different, but ultimately, you go back to the same fundamental cross-sections and fundamental data. Whatever residual uncertainties are in that data, of course, are in here, also.

MR. CURTIS: SIMMER did very well against those experiments, but I would say that any transport code that had a proper group structure in terms of the neutron energy and enough resolution of the spatial dependence also gives very good answers. And SIMMER relies very heavily on TwoTran and the established transport codes that have been in use for many years.

MR. BELL: Characteristically, we've had more confidence in neutronics over the years than we have in the ability to predict precisely where this fluid is and I think that's probably still true.

If there are no other questions about the energetics of the ramp rates that we see in the system, I will move on, then, to the area of fuel removal.

MR. CARBON: One question. Back there on your last slide, in the way you presented it you jumped rather rapidly from 100 dollars per second phase to 300 dollars per second phase. You just sort of jumped one to the other as if there were nothing in between. I don't think you're saying that here, but it's not clear.

MR. BELL: I think what I would really like to do is let this stand as a general perspective on where the real bounds are out here, but as I go through this next section

I think what you're going to see is that this is a state at which we never expect to arrive, and, therefore, we don't really need a high level resolution of what's going on.

End AR-3

(Slide.)

Let's look now at this area of fuel dispersement. If you remember Theo's early introductory chart of this continuous disruption process, there were two processes that were competing simultaneously for control of the accident, one of them being disassemblies off to the right side, which could bring about neutronic termination, and off to the left side was dispersal or fuel removal. And the dispersal or fuel removal process is a more or less continuous process that begins clear back in the initiating phase and continues all the way along, and ultimately the termination mode is not satisfied with one big discharge of material, but it's the accumulated effect of that discharge occurring all during this time and with some final point then where sufficient inventory has been removed that it renders a system subcritical.

So in dealing with this dispersal problem in that termination mode, there are really three fundamental aspects that have to be dealt with. The first one being that you have to have a fuel removal path available.

Now, what we will do is go through -- as we go through the disruption sequence, we will identify which paths are available, how many of them there are, and what their general characteristics are for fuel removal.

Now once you have a path, you have to be satisfied that indeed you can move material through those paths with a sufficient throughput to make a difference.

In other words, if we put through one gram per second, it takes us 10 years to get the inventory out, that won't do us any good. We've got to get it out in the time-frame between the start of disruption and the achieving of this homogenous cylindrical pool. That's what we're taking as kind of the cut-off time window to see if indeed we can see the fuel removal taking charge before that occurs.

Now this then is fundamentally dependent on the mechanics of freezing and plugging of materials in this passage, and it is also dependent on pressure. We are in an environment that is energy-starved in the beginning. In other words, we have not integrated enough energy in the system to bring the bulk mass of the core up to a temperature that would sustain a discharge pressure.

MR. THEOFANOS: Any time you want me, I'm here.

MR. BELL: Come on. Fortunately, we are interchangeable.

MR. MARK: This is the opposite of one of those movies where one guy plays six different characters in the same role.

(Laughter.)

MR. THEOFANOS: All right. Going on to the dispersal, then, next we want to look into each one of those aspects before we integrate all of this together and see as a function of the core disruption how much fuel gets out of the core.

(Slide.)

The first thing is the pressure, and here is the result of the calculation for the whole-core integral calculation that Charlie mentioned before. This is a function of time, and you see the spikes and those correspond to the activity spikes that we showed before for the power spikes.

What is interesting is not so much the spikes as that the pressure never gets below 5 bar. As soon as the pressure tries to go over the material comes back together and gives it another boost and keeps up the pressure level at this 5 bar. So from the point of view of obtaining a perspective as far as how much fuel you can push out of this core, as the different paths become available, it is important to know that the pressure does not go all the way down to zero.

In addition, we want to remember that there are those pressure spikes that, of course, are very effective in themselves in pushing fuel out.

(Slide.)

Now something about the bundle availability. In the beginning, of course, the subassembly walls are intact, and during the tail end of the initiating phase and early stages of the subassembly pool phase, the only paths available are the coolant passages.

Here I have portrayed the pins. You see the cladding and these are the coolant passages, and that's the only thing available for the fuel to get out.

Now we can make some very good arguments, we think, for claiming that for the case of irradiated cores where we have pressure coming in to interfere with the sodium, and together with the radial incoherency of melting of this cladding, we can make some very good arguments that no part of the core is going to be blocked in these areas here because of plugged locations. Even independent of core disruption, for example, for people who don't like to buy these kinds of arguments. So that as soon as the fuel within the core disrupts and the cladding in it reaches the temperature of the fuel and therefore provides the pressures, we would expect to have an axial motion of this core disrupted mixture into these areas here.

Now even under the worst conditions, however, at least half of the upper axial blankets is going to be unblocked, and three quarters of the lower axial blankets also is going to be unblocked. So even under the worst conditions we have a lot of space there.

(Slide.)

ar4-5

Here is a calculation of penetration of this molten core disruptive material into the upper axial blanket. This is a result of the calculation which has been benchmarked against the experiments. This is where it is pushed, under pressure, into a structure similar to blankets. In this penetration in here, we have the picture of the pins. This is the blanket area, this is the spring area you heard about before. Penetration and distance, that's roughly 32 centimeters on the blanket. This is a function of the injection pressure.

Now the experiments we have are at high pressures, so we benchmark around here, and then we are able to go back by writing the calculations to see how much penetration we get at lower pressures.

Well, what you find even under 5 bar pressure, which is this .5 mega Pascal, we have a complete penetration of the upper axial blanket.

Another interesting thing that you see in this calculation is that the degree of penetration is independent of the amount of superheat. Somebody might have thought the more superheat it is, the more it will penetrate, but you see the two results are very close together. For that, of course, the cladding is so thin that it very quickly gets hot, so the amount of superheat plays no great role.

bu3

Now in this distance of the length of the upper axial blanket, by the time you melt the cladding in that area, and therefore you produce more space, you can accommodate essentially half of the core fuel in that area. So you think in terms of the 20 percent you need to become temporarily subcritical and the 40 percent you need to become permanent subcritical. You can render the system subcritical.

MR. MARK: What is the delta TS?

MR. THEOFANOS: This is how much the temperature is higher for the melting temperature, so this is, for example, the fuel coming in just at its melting point, while this one is 200 degrees above that.

MR. MARK: And this is running into cold areas?

MR. THEOFANOS: Right.

(Slide)

Now let's assume that at this point there is still neutronic activity as you had before, so the next time that we are going to have some -- obviously the thing continues, that means somehow the exits were plugged. The next time around we have some more possibility when the subassembly walls begin to fall apart.

This is a cross section in the core, and we have taken a small part and blown it up. What we are showing here is the drivers with the disrupted fuel, and the green area here is the internal blankets. Now because the subassembly walls are going to become heated, and as they become heated, they become more pliable, they will probably be pushing against each other so that even when they melt, that's going to cause merging rather than an escape path.

When you look at this junction over here, you find that indeed because the blankets are cold, there is a gap there, and as soon as the subassemblies crack at the edges, there will be a path that is open for fuel to go into this gap, travel axially downwards, and here we are looking at the lower axial blanket area. Also the driver subassemblies are not very hot. Therefore, the fuel can go not only axially but can start going around in a radial motion. So the fuel goes first down along the interblanket gaps, but then by the time it goes to the colder areas, it begins to spread off in all directions.

MR. KASTENBERG: Theo, why doesn't it freeze on the cold green surfaces before it drains down?

MR. THEOFANOS: It can freeze, and that is part of the analysis that we want to show you next. Here I am trying to show you the paths, and I want you to remember this number. There are 90 gaps, if we count only the gaps in the internal area, and we will consider that next --

MR. KASTENBERG: At what stage of life are you looking at of the core?

MR. THEOFANOS: If you are thinking in terms of swelling, yes, sure, the swelling is going to be more at the center of the core, but eventually the subassemblies also are going to melt in areas. So that's really not a very big problem.

MR. CARBON: Would you go back to the preceding slide for a moment?

(Slide.)

If I understand this correctly, it's penetration vertically upward as a function of injection pressure and temperature and so on?

MR. THEOFANOS: Yes.

MR. CARBON: Really, with zero injection pressure, I presume that penetration upward would be zero?

MR. THEOFANOS: Yes.

MR. CARBON: And those curves don't indicate anything like that?

MR. THEOFANOS: That's why they didn't go all the way to zero, Max.

MR. CARBON: I know, but is there some explanation for why it would have almost a vertical slope there at the start? Or is it that these curves are numerically off and this isn't correct?

MR. THEOFANOS: No. All it shows here is that you need a very, very small amount of pressure to actually go through the process here of the cladding or the blankets and then heat loss makes this freeze up; unless you are unable to do that, you are not able to plug up the channel. It takes some penetration to be able to do that. So the tendency here is not just to go in and freeze right away, but in the beginning you are going to see a very high sensitivity to the pressure because even if you have small pressures, you penetrate a lot. But then what you show here is a rather great sensitivity to plugging out of the disruption process.

There are two ways to look at that. From here to here (indicating) this thing shows very abrupt, because this one is very slow. If we plot it differently, this thing could show like this, and then showing almost like a horizontal line. See, it is kind of like portraying this. The reason this looks very abrupt is because that is very sensitive.

MR. DICKSON: What is the unit on the vertical access?

MR. THEOFANOS: This is centimeters. That is 30, 40.

MR. MARK: It says meters.

MR. THEOFANOS: I know it says meters. That is a mistake. I am sorry.

MR. CARBON: So it should be centimeters?

MR. THEOFANOS: Right. That is what we have here.

This is the upper axial blanket, and that is about a foot, 30 centimeters.

(Slide.)

Here we will go to the freezing dynamics.

What we have done here is to put in a pressure, three pressures and the temperature, and considered how much mass is going through those gaps as a function of time.

What you see here -- let us take the green one first. It increases all the way out to more than 22 and then very quickly goes down as a result of this still coming in from the wall of the subassembly and making a particulate that actually slows down the motion.

Now at this time you see that almost complete plugging has occurred except that it did not quite stop. It just oozed out and eventually the thing picked up again, and after this particulate comes out of that oozing stage, actually the passage has become now bigger and you have got a passageway much longer for fuel to go through that.

The blue line at 3.4 atmosphere and 3400 degrees Kelvin gives us up to about 10 or 15, and then slows down, again tails off a little bit, and then takes off like that. The 3.4 atmospheres and 3100 K goes up to 10 kilograms per second, and it just gradually slows down asymptotically.

The point here is that within one second or half a second, you might want to claim 20 kilograms or 15 kilograms per second. You read those things for pressures of 3.4 atmospheres.

MR. MARKS: These kilograms, 20 per second --

MR. THEOFANOS: Per gap. So we have to multiply those numbers times 90, times the total amount of kilograms. Just to do it very roughly, that's why I told you about this 15 kilograms per second.

MR. BELL: I just want to point out that the red curve is sort of the classical fuel freezing and occlusion problem where it is pure fuel at its melting point going into the gap channel. That calculation was actually checked against a theoretical solution which it can attain for that situation and matches very, very well.

MR. THEOFANOS: Also, the other calculations, these other calculations are also benchmarked against the gap data at Argonne and they also -- in fact, that's how the numbers were fixed.

MR. LIPINSKI: How long are the gaps?

MR. THEOFANOS: Well, half of the core length; about half a meter. That's it. Most of the injection point is in the center. Like Bill said, if there is some swelling there, then it would be just maybe 10 or 20 centimeters.

(Slide.)

Further into the core disruption stage, we have one more class of gaps. so to speak, coming in and they are coming in because of radial blankets. These are radial blankets all around the core and there is a lot of warmth associated with them.

This is a driver that opens up between these radial blankets and now the flow can go radially out this way, but of course also can axially down now into those areas through those gaps axially down. So you get a three-dimensional flow that quickly develops as soon as the corners open up here.

In fact, what we think is that by that point the gap areas become so many that the problem is completely overwhelmed by the number of passages that are available for the fuel.

This is indicated here by saying that the discharge area is 10 times greater than the area associated with the opening up of the internal blankets.

Now I also want to point out that the availability of those paths will be right in the beginning at the annular pool stage as soon as the subassembly walls are falling apart, and the annular pool phase begins to form. That is when those become available.

(Slide.)

Here is a summary. Because we not only wanted you to know how many gaps or how many parts are available, but also we need to know how much the coolant volumes can hold, and here this is summarized for you, together with the times at which we have access to those. The time zero right at the beginning of the core disruption phase following the initiating phase, upper and lower axial blankets within the pin structure, we are talking about capacity in terms of percent, how much is core pool, 12 percent in the upper and 25 percent in the lower. The percent of removal now under 5 bar pressure is on this list here, and you said that in order to remove these 12 percent, you need about two seconds to do it; to remove 25 percent, you need about two seconds. That's about the amount of time you need to begin to fall apart -- for the walls of the subassembly to fall apart, so the internal blanket gaps begin to open up.

Excuse me. I think I made a mistake. This capacity is consistent with this timing up here. The total capacity is more like 50 percent.

MR. BELL: I just want to clarify the difference between the upper and lower axial blankets. You notice that one is just twice the other and we purposely derated the upper axial blanket by a factor of two because in the long term that fuel could fall back into the core. When it falls back in with the blanket material coming with it, it has the neutronic effect of having half of that fuel removed. In other words, that dilution effect of the blanket is the same as removing half of that fuel. So that is simply derated to take that into account.

MR. THEOFANOS: In the internal blanket areas, we have the time of access on the order of 2 seconds, and in the lower axial blanket area we can accommodate about 10 percent of the fuel, and in a matter of this time we can put in 21 percent under 5 bar pressure, which is always going to be there. And the radial blanket gaps, the rate of removal and percent per second is 200 percent over here. In both cases, the access time is 2 to 4 seconds, and the capacity of the homogenous fuel that we can remove in the time that is available is about more than 10 to 40 percent.

Now remembering that we need only 25 percent at this stage here to keep it temporarily subcritical, and you need 40 percent at this stage here for permanent subcritical, you realize that any combination of those numbers is going to give us -- in those numbers over the periods that are indicated here -- to give us the room that we need in order to assure there will be permanent subcriticality at the time of entering the heterogenous pool.

(Slide.)

Here is one example of how we have tried to do that. Just to show you the bottom line in the upper axial blanket, an adjusted rate of 3 kilograms per second, taking into account the effects you just heard, the discharge time of 5 seconds, we can remove in the lower axial blanket 6 kilograms per second times the time of 5 seconds. The lower blanket is the only one that counts. About 10 percent in the radial blankets, 10 percent in the reflector, 40 percent in the control center. You're hoping it's more like 100 percent.

That kind of gives you an idea of how much margin is there for removing fuel from the volumes that we are looking for.

MR. ZUDANS: In that case, up there where you list 40 percent --

MR. THEOFANOS: Here. This is the radial reflector.

MR. ZUDANS: You can leave that much to the gap, but if you bring that material to that location, that is associated with another tortorous path, so this is not the real number. I mean you have to get it sitting there to be discharged at the gap. It has to come to that location. That's half a core.

MR. THEOFANOS: That's only about that far apart.

MR. ZUDANS: Can you get there at the same time?

MR. THEOFANOS: Sure. Why not?

MR. ZUDANS: If you say so.

MR. BELL: Typically these discharge velocities -- the material can move the distance of a meter in a second, and

and we are talking about a fraction of a meter, too.

MR. ZUDANS: But does it have the passage open to do that? In order for it to be open you would have to have the whole core, the whole thing.

MR. BELL: No, these gaps that he is talking about are in the radial blanket reflector which are not being heated. They are in fact open.

MR. ZUDANS: But to discharge 40 percent of your core through there, you have to bring that 40 percent tothe gap?

MR. BELL: Oh, yes, it's sitting right there all ready to go, if you remember our presentation.

MR. THEOFANOS: It's about so far apart.

MR. ZUDANS: There's no further distinctions in the core?

MR. THEOFANOS: Maybe I can put the picture back on.

MR. BELL: The only thing is the blanket gaps, and the blanket gaps are also cold, and they are going to be filling up also first, and that is the first number that you saw there.

(Slide.)

MR. THEOFANOS: Now we have an overlay here.

MR. CARBON: If you feel this is the start of a new topic, perhaps it would be a good time for a break.

MR. THEOFANOS: No, this is the summing up of what we have got covered up to now, and then we are going to get to a new topic.

(Laughter.)

The completion of this detailed discussion. Now we want to go back to the origin, where we started from. This is the picture I showed you, and now we want to go and put those numbers that we talked about, the probability numbers, and since everything is fresh, we should do that before the break.

MR. KASTENBERG: Ours doesn't overlay, Theo.

(Laughter.)

MR. THEOFANOS: Next time I will remember.

MR. KASTENBERG: It looks like a "follow the dots."

(Laughter.)

MR. THEOFANOS: I have a lot of confidence in your imagination.

I will start from the beginning. The initiating phase, the disruption phase, as you have already -- we expect zero energetics. There is no way of producing energetics from that, and we therefore put on this probability 10^{-3} because of the fact that we believe energetics in this stage are physically unreasonable. In attaining a permanent subcriticality, that is discharging 40 percent of the fuel, we also consider that to be an unlikely event, not so much because we cannot see ourselves with the core not plugging up, but because we can see some situation under which some exits of the core might be plugged up. That is the reason that we give a number of 10^{-1}. That means the main path will be going now from the initiating disruption of the pin structure to the subassembly pools.

Now, as you heard from Charlie a minute ago, the likelihood of obtaining significant energetics is very low. We assigned a 10^{-1} number mainly because we think -- we might have factored in energetics in this assembly, but this path only indicates the potential for attaining a disassembly, not of failing the vessel. This over here is -- this is the age of spectrum condition, conservatively also under this step, the age of spectrum condition again, because we can see that maybe it will take more than this 1 second that is available from this point to that point before you fail all the walls to discharge 40 percent of the fuel. So that seems to be 10^{-1}, so again the main path would be to go to the annular phase.

Now at this stage again the energetic potential is very similar to that, we will say maybe slightly higher, and again that is a spectrum. However, now, in going from this stage to this stage, we have the availability of the radial blankets. As you saw the problem, it was becoming completely overwhelmed by these openings, and that is the reason for the 10^{-2}, assuming it is considered to be outside the spectrum.

So the main path, then, the way we see it, is going to go from the initial disruption of the pin into subassembly pools into the annular pool and exit into a milder specialty from there, and that is conservative, with less likelihood you could do, and with even less likelihood it would do that. Assuming that we got in a whole-core homogenous pool, still again, of course, we would have the ability to do the paths and the probability would be mainly this one, as a spectrum condition, to attain it for this stage.

Now I want to follow each of these energetic paths and see what the probability is and the corresponding outcome of the disenergetic assembly.

This part here is almost not here. Similarly this part here is considered to be physically unreasonable. The subassembly pool was all very well bounded by $100 per second. We have all the additional probability of another $100 per second, and we think it is unreasonable physically to fail the vessel. Similar things apply for the annular pool. Again, the outlays were bounded and 10^{-3} was for this path over here.

Finally, for the whole-core pool stage, as you saw, we had bounds. There is a level required to fail the reactor head.

On the other hand, we recognize that really this is a rather involved kind of situation. Max, you mentioned that we only explored the one case that was two-phase. It is a multi-dimensional kind of a problem. We cannot really have the confidence that one has with those two stages for not failing the vessel, and it is for this reason that we gave the number of 10^{-4} for the probability of failing the vessel for this kind of an event.

So this has completed this story. Obviously we have to multiply this 10^{-6}, multiply this path 10^{-4}, multiply this path 10^{-4} and finally multiply this path here another 10^{-4} and sum them up, and the total is 3×10^{-4}. That is well below the 10^{-3} that you considered physically reasonable, and therefore we come to the conclusion that for loss of flow accident, the conditional probability to fail the head is 10^{-3}, but then converting it back to physical reasoning is physically unreasonable and not expected.

MR. DICKSON: Question, please Were those numbers made with the fuel pins in their present configuration?

MR. THEOFANOS: If you're asking me with respect to the gas compaction, you see, we put a 10^{-3} up here. That means physically we wouldn't get any energetics in that stage. So that is assuming that we didn't have the plenum gas fission problem. If we did have this problem, we would have some problem knowing what to put here with a very high degree of confidence, and the way we want to bring this across is that we have a very high confidence level for all those numbers in the conservative side, and we can claim we do that in every step of the way here. If we had that problem, we might have some difficulty really assigning any numbers. We just wouldn't know what to put.

MR. KASTENBERG: Theo, just a couple of questions. Let me see if I can phrase this correctly:

If you end up in the disassembly box, you have to go somewhere, so do I interpret the green line as being one minus?

MR. THEOFANOS: Yes, that's what it is, but I didn't put the numbers on it to avoid the complication.

MR. KASTENBERG: So it is basically one?

MR. THEOFANOS: This plus that should be. That's what I did here. Those are all the leftovers from those parts. So it would be very close, even if it got through that path.

MR. KASTENBERG: Two other things. This assumes that if you, for example, went on the gamma gamma prime path, that those probabilities are not dependent on one another, yet they are in a sense? I mean they are end of spectrum and if you are going on that path, you'd have to complete that path, wouldn't you? In other words, you wouldn't come across gamma and go down on the green line, you would go down on the red line which says gamma prime? No?

MR. THEOFANOS: No, no, no. Remember, this is only there for processes of qualifying for an exact determination. An exact determination means we have a high pressure developed because of an event, and as a result of that, the fuel somehow gets out of there and that is the end of it. Now having gone through this -- that's why this is in the process now.

Now at this point we can ask the question, is this a necessary event sufficient to fail the reactor vessel head? And if it is, we end up with this fact? Or is it not? And if it is not, we are not in this box which means the fuel is some place inside the reactor vessel and dispersed.

So in fact you go through this, then you split into two parts, depending how energetic the event was.

MR. KASTENBERG: Well, I have to think about it all, because it just seemed that once you embark on one of those Greek letters, your process becomes dependent upon what you have assumed to get to that stage and become independent.

In other words, if you're end of spectrum, you are end of spectrum all the way? No?

MR. THEOFANOS: No, no, no, because these numbers here, Bill, they are not -- well, those numbers really should be distribution probability. If they were, what we are saying is you could associate one part of the distribution with another part of the other. But those are boundings, so what we are saying is that out of all these events that can get us into this state, only we could have end of spectrum events to get us there. That means we count all the processes that actually qualify for hydrodynamic disassemblies.

Now that's all of them there.

Now we have to go there and pick out some of them, if there are any that have failed the vessel, and we have to count those against the previous ones to find out what is the likelihood of going through that. And all we aresaying is that even though we see some edge of spectrum situations bringing us here, we think that we're not going to find any among all those disassemblies that can cause vessel failure.

So it is dependent, but we do that dependency on the basis of the technical material that was provided before we look at the whole picture and say, "Can we find any situation that can get us to that," and assigned a number.

In other words, we don't try to do that in a very detailed fashion because in almost all cases we couldn't find any cases, so we couldn't limit it or identify one.

Any other questions on this?

MR. ZUDANS: You say youcouldn't find any, but how do you dispose of that $300 insertion?

MR. THEOFANOS: Because it's single phase and disassembles quickly. It's much less energetic.

MR. ZUDANS: In other words, single phase?

MR. THEOFANOS: Yes. In fact, that was much less energetic than some of those cases up here.

MR. ZUDANS: Then the highest number you got on the energetics was 18, something like that?

MR. THEOFANOS: We got about 80 or less, and really 80 is ridiculously conservative. I think the numbers here should be more like 30 or 40, and I think the closest thing that we came to producing significant energetics was taking the whole-core pool homogenized, perfect symmetry, putting a source in the center, perfectly symmetric, then allowed to have a perfect symmetry of slosh, and on that gave us $100 per second. It's really putting a lot of things on top of the other, and even that one only gave us the equivalent of what would be required on the basis of our first discussion this morning to only fail and still be very far from doing anything to the vessel head.

MR. KASTENBERG: I don't want to harp on this, but I think it's important to understand something, because it was your second major bullet, of certain things that were physically unreasonable by this thought process and I want to make sure I understand it.

If I go through and I look at what your best estimate, so to speak, I go one and one and I am at an annular pool, and your best estimate is that you will get dispersal with the annular pool, tell me again what does your gamma mean when you get into the minus one?

MR. THEOFANOS: That is the conservative best estimate. I'm bounding it along the way.

MR. KASTENBERG: But what does the gamma mean, going the other way?

MR. THEOFANOS: It means we expect it is not totally impossible that we might have an initiating event from this stage.

MR. KASTENBERG: What is the physical process that would move you along the 10^{-1} gamma path? What is the end of spectrum process?

MR. THEOFANOS: That would be some criticality of the type you heard previously from Charlie. Any of those are criticalities. This \$50 to \$80 per second bounding estimates, they will be doing that, and I do believe there is some possibility for those things to happen.

MR. KASTENBERG: Suppose that did happen, then what is your physical process that would atke me along the delta prime?

MR. THEOFANOS: You mean the gamma prime?

MR. KASTENBERG: What is the physical process that would take me that way?

MR. THEOFANOS: That will take you here? The physically unreasonable process would be -- well, you know, physically unreasonable doesn't exist. That's why I can't tell you what the process is.

(Laughter.)

MR. KASTENBERG: This is what bothers me about the schematic is that I see the --

MR. THEOFANOS: If I had a way, Bill, of telling you that I had to assume this and that, and I will get the vessel to fail and we have told you what that was, but we don't have that. Now we say -- in fact, we have seen the whole-core pool coming in, giving us reasonable energetics, not very high or not very low, and we can see how one can maybe go back in the calculation and can push a few things back and forth and make energy levels come to this level that we need to fill the head. But in those cases we really don't see any physical process to cause that, and we have to represent that in some numerical fashion, so we assigned a number of 10^{-3} for the process which we cannot see how this can happen. That is what we defined as probability level to it.

MR. DICKSON: Could I try to paraphrase it and see if I've got it right? When you are going the horizontal gamma line at 10^{-1}, that's the \$40-\$80 ramp rate that you say probably won't happen, but is within the spectrum? When you get on down the vertical gamma line, you'd have to have a couple of hundred dollars a second, and you don't see how that could happen.

MR. THEOFANOS: That's right. It's outside of the spectrum because I have bounded it before at this point, and in order to get \$200, I must do something outside of the real possibility.

MR. KASTENBERG: And what is the physical process, going back to your annular pool again? Because that's where you are most likely going to end up dropping that 10^{-2} to complete disruption. What is the physical process that might take you down that line?

MR. THEOFANOS: The physical process would be if somehow it was possible for these radial blankets to not open up. We cannot see how this can happen because the same process that is homogenizing the pool and melting all the internal blankets, not only the walls, but also the solid materials, and that is why we are putting outside the spectrum, because we can't see how this can happen. But you see what the process would be, that I postulate all the outside walls remain intact --

MR. KASTENBERG: Even with that, you feel you would go to the dispersal?

MR. THEOFANOS: Oh, yes. That would mean more time available, because every time you are pushing your time limit, you must push harder and harder for getting material out.

By the way, as you saw from the whole core calculation, the transition from this stage to that stage is not a very instantaneous thing. It takes some time before you homogenize everything, and if you ask me, that's one of the most interesting things learned, was that part of it.

MR. BELL: Theo was pointing out one possible way to defeat the paths to the left. It would be those gaps where available to come down to the next stage, and to defeat that you'd have to also say, for example, the control rod removal wasn't available, or other modes of removal were not available. In addition, you're compounding the sort of incredible nature of the whole thing.

MR. THEOFANOUS: Any other questions?

MR. CARBON: Let's take a break.

(A short recess was taken.)

MR. CARBON: Move right on, Theo.

MR. THEOFANOUS: We have completed the discussion on this, and now we want to cover two more accidents, the transient overpower or the TOP, and the loss of heat sink.

The TOP, of all the accidents, it is looked at, and in fact the applicant has spent a little time on it. The loss of heat sink accident is all the more moderate, somewhat more neglected in the past, probably not for bad reasons. But we thought we would take a look at that accident also.

I think I will give you the bottom line from now that we see no energetic behavior in any of those accidents, but maybe you are still interested to know what the story is or what story we are putting together.

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Now, then, this is the accident that is characterized by very, very low heating rates. Now, remember, about 1 degree per second, and because of that there is a lot of time for recovery, and this is something that is not really covered in the probabilistic risk assessment studies adequately, in our opinion. There is not enough credit taken for recovery from this accident. Therefore, maybe the probability of them getting into very extended core disruption is slightly overexaggerated. In this case, if one postulates, extends an unmitigated loss of heat sink, eventually the core will disrupt and the way that this is going to happen will be only following uncovering of the core sodium. There is no way it can start melting while it is covered with sodium. And that is very similar to what is happening in the reactors.

Yes?

MR. ZUDANS: Could you quickly explain to me what is the difference between loss of flow and loss of heat sink?

MR. THEOFANOUS: Well, the loss of heat sink is in the -- going back to the first vuegraph I gave you -- is a protected accident. Remember, no power generation, very quickly within a few seconds is down to a few percent. And somehow you lost capability of removing heat from the system. That can happen because of a large earthquake and you sheared off the pipes or it could be a particular problem with the heat exchanger.

MR. ZUDANS: And loss of flow also as a reactor shutdown?

MR. THEOFANOUS: No. The loss of flow we have been discussing here is an unprotected accident. The pumps were closed down, shut down, the reactor protection system, the control system, has failed to act, so the power is at a high level. So that is a very, very unique, very, very different situation.

So as long as sodium is in the core, the core will remain intact, but that sodium will -- that will happen in a matter of a few hours or more like close to 10 hours. And then if the primary system holds together at this very, very high temperature, I will take quite a long time to vaporize all the sodium in the upper plenum before the core actually begins to get uncovered. And that is 100 hours to do that.

However, at these high temperatures, other things happen structurally in the system that might cause the emptying of the vessel of the sodium before it has an opportunity, all of it, to vaporize. In the beginning -- and I think I brought up in the discussion in November -- one of the things we were concerned with is maybe the high temperature creep of the vessel wall might cause creeping and vessels fall off. We examined that, considering some recent data at the high temperatures for creep. And I found that the times were much, much longer than the time frames that other things would be happening.

On the other hand, there is something else that is of similar nature. As the vessel wall heats up, it will expand downwards because it is so far from the top. Similarly, the guard vessel is going to be heated up, and that will be expanded upward because it is so far from the bottom. There is not enough clearance here as this nozzle goes into the vessel, so the one vessel is going to be interfering with the other. And some preliminary assessments -- and I think the applicant has done work here,and we have come to the conclusion there will be some structural failure associated with this interference between those two structures.

Well, if that is going to happen, the vessel is going to empty of sodium and the core will remain on the body. If that were not going to happen, there is not any difference because 100 hours later there is enough sodium. You talk about you get to 45 minutes and it does not affect the conclusions or the way that you go about analyzing the core disruption independently of what happens.

In any case, this is the range of the heating range that we can expect to be present at the time of core -- beginning of core disruption.

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Now, I would like to follow and see how that core study begins to fall apart,and our interest is to see if there is any potential way. Obviously, in the absence of coolant, there is no way that one can associate any of that initiating energetics that were talked about before with this kind of scenario here, so here we have pictured inside the case, the structures, because of the slow heating up of the fuel, there is ample time for the cladding to keep pace with it and because of the 1,000-degree temperaure difference between the melting of the fuel and the melting of the cladding, there is ample time for the cladding to melt and just drain out.

There is no forces to move it upwards, so all the cladding from the core is going to melt, is going to fall down, and is going to form a very massive steel blockage in the entrance to the core. And in fact, if it's going to block up here, in fact, some of the cladding is going to make it all the way, and some of it will be inside the core region itself.

The other interesting aspect of this is that the upper axial plant cladding, because of the proximity to the active core region itself, is going to melt quickly and fall down. So the upper axial pellets, they are going to fall on the top of the rest of the core.

Now, the core at that point is very hot. The pellets are cindered together. They might stay together. But even if they don't, if they shake up a little bit, they might make a random rubble bed. But in any case, we have a solid packed configuration. It is like a packed bed and is far from being fully compacted.

We have typically a worth fraction of 40 to 50 percent.

Then the next question that we had was will the upper core structure melt? Again, because of the long time duration of this thing and the very, very small heat, obviously, there will be radiation, heat transfer from the core into this melted -- into this steel area here.

Well, our conclusion was that this will not melt, because of two reasons: Number one, this layer of insulating pellets or the blanket that is not heat producing and has very poor thermal properties. Then we did some radiation calculations and found out, in order to be able to produce the heat fluxes to melt the area here, one would have to have temperatures of the order of 5,000-6,000 degreees in the center.

Of course, that goes well beyond the fuel melting point. Therefore, the picture is one in which the steel melts out. That is why we call this steel melt-out. The steel heats up slowly. The upper axial blanket falls and forms a rubble bed on top of that. And the upper core structure remains, or even maybe might be resting on the top of this bed. We will show it here for clarity. But the whole thing might be resting on it.

And, of course, the UIS is still sitting up there. Now, then, the temperature continues to increase at this low rate, and the fuel will begin to approach the melting point, and it will begin to stack. So it will start becoming smaller and smaller, being absorbed by the siding of that oozing fluid, pellets. And all this time it will remain -- well, it's subcritical, but it will approach criticality because, as you know, we know that something of the order 10 centimeters of total reduction in height of the core would be sufficient to cause criticality with all of the control rods being inside the core, which will be the course in this accident. So that is one of the ways in which you approach criticality is by this gradual siding.

The criticality approach will begin to produce heat, and you have already approached the melting point. Right around the flux area. The fuel begins to melt. And of course, this melting begins. The weight of all that bed is going to try to push its weight downward. The weight that we are picturing in this situation, the weight we picture is as follows.

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Just like suddenly the center of this core becomes molten and it cannot carry the weight anymore, and all the rest will fall. It falls under gravity. Similiarly, the fact that in going from critical to prompt critical we require only 1 or 2 centimeters' displacement. Therefore, there is only so much acceleration that you can achieve. And by figuring out the upper limit, the whole thing falling under gravity in this kind of a distance, and that by the worth curve which is about $1 per centimeter, we come up with a maximum to operate of $60 per second.

Now, as you know, this would not have any concern even were the sodium melted. There is no sodium to transmit any of the forces. Of course, that is of total neglect. Really is nothing about that at all.

If this was to happen, it obviously would be enough to dispense the core, and one can claim this would lead to a termination. On the other hand, remembering that the upper core structure is still sitting on top and the UIS also on the top, and remembering that this melting is not likely to happen across the core altogether but more like in the center of the core because of the radial peaking, one would expect more like maybe a fraction of that materializing. Something like maybe $10, $20 per second. And if that was the case, one would not obtain termination at this point. And then the question is, as you go through this burst, the question is -- and of course, as you go through that, the whole thing becomes molten, so you have the whole core cool situation.

Can one obtain from that the higher activity rate than before? Well, the immediate reaction of one is that, as was ours, well, this is a case already for loss of flow case and so we can forget about that. Well, this is not the case. There is quite a bit better than the other one, because now we have quite hot radial blankets.

Now, the upper blankets are sitting up here, and because of this, the tendency to homogenize the radial blankets into this molten situation is much easier. And this will be enough to produce permanent criticality because from that -- and I want to show that just from that.

So in the event we did not obtain permanent recriticality from this initial burst, there is a bubble.

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It will tend to push things up for a time and then bring them back down. In doing so, there will be a tendency for this blanket to come in and homogenize and the other radial blankets to come in and homogenize. To give you a little account that Dr. Carbon was asking about, here is the activity balance. If we homogenize the upper axial blanket, they are worth about minus $20.

If you homogenize the internal blankets, they are worth about $20. So if those two were to come in together, the effect will be not really very high activity state in the system. To homogenize half of the radial blanket, it will have the effect of minus $40. To remove the control material has the effect of plus $30. These two things will tend to happen at about the same time scale. The radial blankets will come in at the time that the other material is being vaporized. And to completely puddle the core, we require plus $10.

As you see here, the sum is zero, and all of those processes happen together, and we cannot see that core achieving configuration stage in which the potential for higher activities is present following that initial process.

So the way that we see the process going on is following this series of mild recriticalities we can mix all the blankets in and put all the material together and being eventually all of the steel up there that, of course, will have to be melting and coming in and end up with a permanent subcritical puddle simply because radial blanket material can mix well and doesn't separate, of course.

So the best-estimate discussion here, the best-estimate result is, of course, no significant energetics at all and permanent subcriticality by dilution with other materials coming into the core.

MR. ZUDANS: Theo.

MR. THEOFANOUS: Yes?

MR. ZUDANS: Do you assume that the sodium was lost completely?

MR. THEOFANOUS: Well, that is a long time ago. It has to be before anything melts.

MR. ZUDANS: Did the guard vessel go?

MR. THEOFANOUS: Regardless of what?

MR. ZUDANS: Did the guard vessel, did it go too? Because it is supposed to keep the sodium level about the core.

MR. THEOFANOUS: Do you remember, that was the interaction between the two? That is one of the possibilities is either the sodium gets out because of the failure, because of the interaction of the guard vessel and the vessel itself or the other one is if it doesn't happen, the core is going to sit there until all the sodium vaporizes and gets into the containment.

MR. ZUDANS: I am just wondering, is there a sufficient way to get it out of the guard vessel so that it ceases to flood the reactor?

MR. THEOFANOUS: If the guard vessel fails, you can get it out.

MR. LIPINSKI: You didn't fail it?

MR. ZUDANS: It still leaves sodium in the reactor tank. That's what I understood now.

MR. THEOFANOUS: Let me show you the picture here.

MR. LIPINSKI: All you did was dump sodium in the guard vessel.

MR. MARK: It's a place where sodium can run out. He will boil it off.

MR. LIPINSKI: Where is he boiling it to?

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MR. THEOFANOUS: I think the first thing that we need to understand is unless sodium gets out of the core, we don't have to worry about core disruption. It doesn't melt. That is the point that you needed to remember.

MR. ZUDANS: Yes.

MR. THEOFANOUS: Point number two is, somehow that sodium isn't going to sit there forever. It will have to get out one way or the other. If it doesn't get out because of structural failure, it will boil off. When it boils off, it will boil over to the containment. If the loss of heat sink was caused by earthquake, already we have a broken part in the system that causes leakage.

The seals can fail because of the high temperatures because you build very high pressures and you will get sodium vapor out of that, but it has to get out and only after it gets out then the core can melt.

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Okay. We are done with that. Then, of course, we like to also ask the what-if questions, and here is one for you: What if we like to postulate an energetic? Of course, the thought here is that we don't have the sodium pool and therefore there is no direct means by which we can couple the high-pressure means to the head. And just as a matter of curiosity more than anything else, we carried out of this compilation -- and Professor Hawkins in November was curious about this situation, so he might like to see this result -- from the same kind of compilation, $200 per second compilation, that we ran to assess the vessel with sodium in. We ran it again except now we don't put the sodium in. So we get out of the expansion as before, and then we let the UIS displace upwards, and we let the venting process go on its way.

By doing that, we calculated the pressure history on the top of the UIS. By taking the pressure history of the UIS, we calculate, neglecting the effect of those because maybe they are warm and somebody might say that they are strengthless, everybody to accelerate under this kind of pressure. And we found that this UIS will be given total kinetic energy of 45 megajoules, and considering the head capability that this kind of energy is, from the point of view of the pressure to the head, direct pressure to the head, in the case of sodium in, you know that at the time of impact you have a hammer effect, and it goes to very, very high pressures, and they are shortlived.

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In this case, the pressures are of the order of 20 bar, and I don't want anybody to get the idea to expect that 20 bar pressures there. There is just the hypothetical compilation to show that margins available from the point of view of failing the head.

MR. ZUDANS: The 20 atmosphere, of course, the vessel couldn't take?

MR. THEOFANOUS: Of course, I don't want anybody to say that we expect this kind of pressure, but it is part of the hypothetical computation.

MR. MARK: Will 5 megajoules left of the UIS even up as far as the head?

MR. THEOFANOUS: The 5 megajoules are in the UIS. That is a free body.

MR. MARK: Yes.

MR. THEOFANOUS: Whether it will work against gravity up to this distance -- I have a feeling that it would.

MR. MARK: I see.

MR. THEOFANOUS: Yes.

MR. MARK: It might, because there is going to be gas ahead of it which will exert a downward pressure as well as gravity.

MR. THEOFANOUS: Yes. And to summarize the loss of heat sink, because of the long recovery times on it in hours, we really think the likelihood of getting into these situations up to core disruption is considerably lower than one normally says.

In any case, the loss of heat sink event is nonenergetic. And even if it was, there is a very high tolerance for energetics because of the sodium pool.

And that summarizes our discussion of the loss of heat sink. Any questions on that?

MR. KASTENBERG: Could you put that back? We don't have that.

MR. BOEHNERT: Yes, you do.

MR. KASTENBERG: Yes.

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MR. THEOFANOUS: By the way, maybe I should make a remark that there were a number of people in fact that were very much concerned about the loss of heat sink accident for some time now and the reason being is that they could see themselves how they could get into a whole core pool situation and then extracting from the literature about what was existing about the core pool situation and compiling that together with the higher probability expected for such accidents to happen, they felt maybe that could dominate even core disruption. And I think that we want to make a very strong point here that we really don't think that this is the case or should not be the case.

The last initiator that we considered is the power accident. That has been cited by many, many people. And here I want to give you the essence of our argument for claiming that -- for the kind of TOP that we consider not completely incredible.

We don't expect to have energetic behavior. And, of course, the interesting question is how much of an operator do you expect in a situation. And we had a little bit of that this morning in our discussion with Walt Lipinski, who wanted to put some numbers from the TOP itself. We did not try to quantify the TOP. We do not consider it was part of the scope of our study. Rather, what we did was we said let's consider the most probable TOP that we can have there.

Now, let's consider higher or lower than that and see what is the relative probability. How much more additional systems have to fail in order to achieve this higher transient overpower condition. And what we found is the most probable between the -- if I remember correctly -- between 5 and 6 cents. That is the most probable.

Now, from that, we go down to 2 cents or go up to 10 cents. You need another loss of probability for a factor of 2. And then from there to go on to something like 15 cents per second, you need another -- additional systems have to fail to produce that situation.

We decided therefore that this 3 order of margin between the most probable TOP, that the people consider all the time, and this higher range would be adequate. And we put our thrust at 10 to 12 cents per second.

All right. That is the maximum to operate that we considered here. Now, what is the concern with the TOP is that if one postulates and if the beams were to fail in the center line, the same process that we were discussing before, in-fuel motion pushed by the gases can potentially cause prototypic behavior.

Now, the mechanics of the failure here are not very well understood. And that is why we are almost pushed to the limit of having to pursue it. We have no real reason for excluding it. On the other hand, it must be said that we have no real hope for expecting them to happen. The fact is interesting, because as one goes through the analyses, one is interested to know what is the rate of sweep-out of the fuel. And then one takes a look at the experiments and what is difficult to see is the experiments don't provide midplane failures, so you can follow the fuel for a long period of time, and that gives you a little bit of the discrepancies of conditions that we have to assume in order to get into this problem here.

On the other hand, it must be recognized that the operation increases, there is more of a tendency for the fuel to melt and pressurize more and more to the center of the core and therefore it becomes more and more likely for the failure to happen at this point.

So that is the reason that we put a discrimination there between what we want to consider and what we consider outside of the spectrum with reasonable consideration.

So now then, having done that much, we like to know at this upper rate of 10 to 12 cents per second whether there is any potential for -- because of this mechanism here, and what are the processes as follows. The operator is introduced at full flow. Another part is produced and melting happens, and then the subsequent importance tells us which way the accident is going to go and depends on in-pin fuel motion at the failure point at the midplane. And the activity will be augmented.

If it is outside or farther from it, the activity will be reduced. This process of sweep-out is the process by which the fuel and the coolant mix and pressures generate and tend to push the fuel out and away from the point of failure.

This is not only the process from the coolant but also they can arise from fission gases that are coming out together with the fuel. This is a sweep-out, and in general, especially if this is the midplane, the effort will be negative.

Of course, if the fuel is going to move out by some pressures, also the sodium can move in the same way; and, and course, if moving sodium is the causative effect, it will be increasing, so in the analysis that you are looking for, has to properly weigh all three of the effects in order to fulfill the outcome.

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The other interesting part here is that not all processes here are coincident. This is a process that has to happen first. Then it has to follow. From that point of view, then, the coherence at which failures take place is important. If all the core was to fail coherently, you would have the whole core, the fuel moving inward, and if I wanted to still postulate, one would have a bad situation before anything else happened. But the other thing happens because of the power distribution.

Some part of the cores would fail first, and some motion is going to take place, and then as soon as the fuel gets into the tunnel, this due process is going to come in before even the rest of it begins to fail. So from that point of view, the timing between success and failure of the core is very crucial because that tells you how much time is available for the sweepout to come in and before the cause and effect.

So we are looking, then, at this process.

MR. CARBON: Question?

MR. THEOFANOUS: Yes.

MR. CARBON: Before you left it, does your plenum gas play a role there in pushing fuel in toward the center?

MR. THEOFANOUS: Well, no, because in this case the cladding is so strong and everything is in place.

MR. CARBON: But you assumed this morning that there was zero friction between the pellets and the --

MR. THEOFANOUS: Yes. You know, this disruption happens with molten material in the center and just rushing out. There is still, in fact, solid fuel around that cavity, so you cannot really compact that very well from that point of view.

Furthermore, the time scheme of this process is slowed up in the projection. By the time there is any chance for anything else to happen, it would be completely out.

MR. THEOFANOUS: Okay. So this here summarizes what are the three aspects that we are looking at. We are looking at coherence. We are looking at failure location and at sweepout. Or the coherence increases with the operation. We have already said, considering 10 to 12 cents per second, we consider this to be adequate. Also, the coherency increases with the power distribution.

Now, the end of Cycle 3 core is much more coherent than the other cycles because in this core the six subassemblies that are driven fuel are refueled and replaced by blanket fuel, that is, with closer power and therefore more coherence for more support.

In the beginning of our work, in fact, the data for the analysis for this core were not available. We requested from the Applicant and we got them in good time, and in fact we were able to do the analysis as the Applicant was able to do the analysis. On the failure locations, again, it goes more to the midplane with a higher ramp and affected by burnup, and there are a lot of questions associated with that. If you remember, in November I brought up the discussion about the W2 test. That was showing, temporarily, at least, that the midplane failures are more likely to happen, but since that time the Applicant has taken a closer look at this test and found out that there were a very large number -- in fact, they make this test completely worthless from the point of telling us anything when the pins are going to fail.

MR. CARBON: Question. If that test is non-productive and you don't know it until afterwards, how many of the others are that way?

MR. THEOFANOUS: Well, why we even take a look at the test after it is run? First, the person wants to do a good job before he runs the test and decide whether the test is close enough reality, and then after running the test, I doubt that anybody can see any results. One doesn't take it at face value when cross-examining him.

MR. CARBON: Take it at face value if they are good results?

MR. THEOFANOUS: No. I think that may be -- I doubt that anybody takes any results at face value. At least in my experience I don't know of anybody that does that, but there are many different forces in this environment, and every j-4c-4

force is looking together from its own perspective, and I don't think there is any chance at all there will be, but they are sitting there and all of these different people are going to look at it, and they can forget about some important and --

MR. CARBON: Usually the things that were wrong on the W2 weren't wrong on the other tests.

MR. THEOFANOUS: Yes, Paul?

MR. DICKSON: There wasn't anything wrong. That test wasn't meant to be prototypical of Clinch River fuel. You had a lot of experimental fuel elements. It was the first one in which we saw failure at the center line, and a lot of people got excited. Then when people looked at it, what does that mean relative to our fuel? There werea large number of non-prototypicalities. It wasn't because they didn't want to achieve their goal. They weren't trying to make it prototypical.

END 4c

MR. THEOFANOUS: Well, yes. I think this is a little bit maybe why you go off on a tangent from what we are discussing, but I do want to say the obvious question to why does anybody do tests that are completely outside the range of interest here, why you were doing tests that do not support or are not relevant to the case at interest, so I can really make it both ways. But let's not pursue that any further.

The fact of the matter is we don't have any good tests to tell where the failure location is going to be under these kinds of conditions, and as a result of that, we are forced to assume mid-plane failures which might be a very conservative assumption.

On the sweepout we have the forces produced by fission gases as well as by hydraulic pressure. Remember that the flow is going full-blast through the core and that is -- so if you were to come in a tunnel, it would be carried away with the sodium, and we refer to that as hydraulic pressure, hydraulic force.

Now, on this one we have experiments, and we can make use of those experiments, as the Applicant has done, to quantify the sweepout. It is very important to quantify sweepout correctly, because as you see, the time margins vary process here, after we have assumed these processes are not that great. So one does do a good job in qualifying the timing of the sweepout. The quality of the fuel involved in this is relatively small. We are about ten percent of the pin.

What happens is there is a molten fuel that builds up inside the cavity up to the failure point and then opens up, and all of it just comes out, and a little bit more melting you get after that, but it is not anything significant. So that kind of an operation is really limiting the amount of fuel that makes it out into the tunnel. So from the point of view of sweepout it is not as detailed and difficult and anything to examine as, for example, if you were going to melt half of a pin coming out, where you can visualize the first maybe 5 percent coming in or 10 percent, interact, produce pressures, and get sweepout, and all the rest of the 40 percent coming in and get nothing except going out and not being able to move away there.

So because of this small quantity of molten fuel available, one can really be quite a bit more comfortable about this whole sweepout.

(Slide.)

That is the point I was trying to bring across, and here is what kind of a -- here is what you can do in calculating the experimental sweepout activity, and this is specific sweepout activity expressed in cents, subassembly per gram injected per peak, and the experiment is done in the reactor, and it was a high -- it was on the order of $7 to $8 per second, and the calculation is done with a PLUTO-2 code with a set of assumptions concerning very detailed physical processes. So I don't want you to get hung up with all -- begin verification and all that the -- all those numbers forget the important point is that we are trying to match the experimental sweepout and the fact one could have done in this just as good a job on the back of the envelope or just by taking the experimental data and applying these to the reactor condition.

And here is the important point why one can use the PLUTO-2 tests to make those judgments. In the early part of the ejection of the fuel, the ejection is very, very similar between the L-8, which was at $7 per second and this 10 cents per second, that if that was to be done either late or if it ever was going to happen in the reactor, and there is -- and the reason is that as soon as the rupture happens, whatever molten material was there will just run out very quickly. And to the extent that the rupture is going to be a function of how much molten material is there, you see about the same quantity of material will be available to come out, and that is why it is about the same in all three cases.

What happened in this case is because it operates slower, the rate at which you are melting more material is much slower, of course, so you are not putting much more in while here you are continuing on to melt very rapidly, and you are ejecting much more.

On the other hand, the process of sweepout is more related, especially for this water bridge, is more related to this small, up to 20 milliseconds on the time frame, and small quantities to 20 grams per pin.

MR. KASTENBERG: Theo, do you really mean written in the green PLUTO-2 L-8 or do you mean PLUTO-2 Clinch River?

MR. THEOFANOUS: No, no. L-8, we took the PLUTO-2 code and applied to L-8, because another question might have been how does the L-8 --

MR. KASTENBERG: But that is what you read once.

MR. THEOFANOUS: No. That is $7 per second.

MR. KASTENBERG: L-8 was $7?

MR. THEOFANOUS: This is the L-8 experiment itself. It granted $7 per second and analyzed with PLUTO-2 code. This green line is the PLUTO-2 compilation applied to a hypothetical L-8 experiment but ran $7 per second rather than 10 cents per second, while this blue line is assessed for a PLUTO-2 calculation. Together, the two together form the 10 cents per second.

There is some difference in the lengths of life and so on, and here is it. There is the most to say for the end of Cycle 3 with the new data that came in, which we used to generate independent inputs and to run the calculations with, a new tape, and this was done very successfully, which has the possibility -- capability of very, very fine. As you see, the number is much more than the numbers in Article 15 that are being used, for example. Here in parentheses is the number of subassemblies, so as you see, the group is very small.

Here is the fuel temperature versus time. Now, one might say we have a lot of series about the location for -- were serious about the time of failure, and therefore, one might say how does this translate to your knowledge of the coherence of failures?

Well, the point there is that whatever it is has to be related, and in fact, in the report we have some good arguments that the failure cause or failure mechanism is the -- can be related to the FOP in the fuel, and as long as that happens on a similar temperature, just by looking at the relative position of the curve we can learn about the coherence without necessarily being too much dependent if the failure happened at this point or this point or that point.

As you see, this is of the order of 100 milliseconds. As you see, we have something of the order between this group and this group here of more than 100 milliseconds of time available for sweepout to come in and to begin initial in-fuel motion that happened because of the failure of those two.

To relate you to that, here is again the experiment.

(Slide.)

A hundred milliseconds is way up here, and you see that the experiment gives us a specific sweepout of the order of .15 cents per the subassembly for 13 within 30 milliseconds, so there is ample time.

So what you do in the report is with this activity you know how much fuel is coming out and very quickly calculate what is the negative effect, and you can actually see and visualize the result that also one obtains from an actual analysis, multi-tunnel analysis of the all the effects going on together. And what you see here is one such analysis for a total activity feedback. Here is the input. It comes in and then this is the second ejection that you saw before. This is the sodium which is just under 1 cent per second per subassembly.

This should be per subassembly, and this is the sweepout activity, and the net comes in in this picture here. You see within 28 milliseconds sweepout was able to cancel the sodium, and the in-pin fuel motion, and from them on it keeps on being negative because it is very strong and brings another activity to -2 cents per second and a little later even more, clearly saying that the action terminates by this process before the next group fails to come in and gives its portion.

So based on the results then, we conclude that the -- operates less than 10 to 12 cents per second. There is negligible autocatalyst potential, and that TOPs of this range or less are nonenergetic.

Operates greater than 15 cents per second. Maybe there is some potential autocatalyst. I think after one learns more about failure location and failure mechanisms, one can answer more definitely this question. But in any case, the probability of this type event is three times lower than this event, and in fact, it is in the range where you consider it is a negligible level.

MR. MARK: You said three times lower?

MR. THEOFANOUS: Three orders among the lower, thank you. And I think with that I have concluded what we had prepared here. Do you have any more questions?

MR. KASTENBERG: Yes. There are two questions I would like to raise. Last year, I guess back in May, I had asked the question about one configuration representative of a class, and I don't remember exactly which one it was, where following the initial TOP you looked back at the picture. Let's put it up.

(Slide.)

You ended up in a situation where you had, say, one range of assemblies failing and sweeping the fuel out.

MR. THEOFANOUS: Yes.

MR. KASTENBERG: But ending up in a situation where you were at some low power level below steady state power, and you were a little subcritical or somewhat subcritical, but what it was that was causing the reactivity insertion still had a ways to go, and that you might start on a second TOP.

MR. THEOFANOUS: Yes.

MR. KASTENBERG: And at that time I guess we heard that someone was going to be looking at that, and I wonder if you have looked at that or if the Applicant has looked at it.

MR. THEOFANOUS: I think that the Applicants have not done any more in this area. This work was done for us by Harry Humble at Argonne, and I repeatedly questioned him about it, and he tells me it is clearly shut down and that there is no concern for really building up again to a situation where we have to consider even more coherence. But I think that you will find probably in more detail.

And, Bill, I think that is a good question, but I do want to give you a more specific answer to that. We don't have much of that in the report, but Harry Humble has an assembly of his own based on which we abstracted the important things in the report, and that summary is going into a computer which is all the additional information that we can't put in that big thing, and I will let you have that, and maybe that will answer the question for all those detailed analyses that we discussed. But I know that Harry has looked into it.

End 4-D

MR. KASTENBERG: A second point that has been raised also last year, I guess it was May, when we discussed this with respect to TOP. That was long-term cool bit, and there is some school of thought that when you do have sweep-up, the particles will cool as they get swept up and freeze and plug, blocking some channels, and this could conceivably lead to a secondary event, a secondary melt. Again, I wonder whether you have pursued that at all.

MR. THEOFANOUS: I know the Applicant talked to us about that and had some feeling of when they might be in trouble, but I wonder if you have looked at that independently. We have looked into that but not in a great detail, and the reason is that we feel we don't expect to have a very extensive luggage -- because of the small amount again. If it was $8 per second, take the L8, I will give you a different answer. But if it is only 10 cents per second, I don't expect to have a lot of fuel moved out in the first place. Then even if that fuel was moved out, I don't expect it to form enough blockage to stop the flow.

I want to take it one step further. Even if the flow was going to stop, I don't think I can get very excited about local multi-ridges in the core, especially when I see I can move fuel all around the whole core and still somehow being able to get it out.

(Pause)

MR. THEOFANOUS: I know it's not a direct answer, but I know it's a very, very detailed kind of a thing, and if you really try to approach it on the best estimate basis, it is a simple thing to follow wherever part of the fuel has gone. But in view of the small amount of fuel involved, I don't think we are very concerned about that.

VOICE: Talking about 4 or 5 percent fuel to shut it down?

MR. THEOFANOUS: Yes, sir. Very small.

MR. MARK: I don't have a question. Maybe I will later.

MR. THEOFANOUS: You don't have a question?

MR. MARK: I don't mean later this afternoon. I do have a slight comment. For example, I find in the summary at the end of Section 0.4 the expression "such events are of sufficiently low probability that they can be excluded from consideration." I don't believe that is the only place that a phrase of that quality is in the summary.

Now, this morning you explained -- and I think from my point of view rather satisfactorily -- what you intended by using that phrase. If I try to say what I carried away from that, I think of an event like an ATWS or an event like pump stopping, and you think of it as having some low level of probability, and that that actually happens -- perhaps 10^{-3} or 10^{-5}, and when you use that phrase, you mean following that event you must have enough sufficient unlikely failures or sequels that you feel, judge, that you could put on that first probability, which you don't here concentrate on -- another factor like 10^{-3}.

MR. THEOFANOUS: Right.

MR. MARK: I know this is probably the final time that you ever hope to see this report, certainly to carry it anywhere.

(Laughter)

MR. MARK: If you were ever to come back to it, it would seem to be useful to put in there very visibly what you mean by using that phrase. I can just hear Professor Okrent running on that phrase.

MR. THEOFANOUS: Yes. You can't imagine how much we sweated in choosing that phrase.

MR. MARK: I am not saying I think it is easy. You did it to my satisfaction this morning pretty well, and I don't find that in the report. It may be there somewhere else.

MR. THEOFANOUS: The way I said it early this morning is in the bulk of the report. My original inclination would be in the summary to use something like "incredible" Instead of saying sufficiently low, saying incredible.

MR. MARK: You put it in a scale of incredibility. If you did, that would be much more satisfying than just using some words like "it's so small we don't care," or "it's incredible."

MR. THEOFANOUS: Yes.

MR. MARK: Those phrases don't sell well, and I don't mean only to Okrent, but to whoever reads it.

MR. THEOFANOUS: That is a good point. I think we will consider that.

Any other questions?

(Pause)

MR. KASTENBERG: Just a point of information. There is considered a final draft now --

MR. THEOFANOUS: This is the very final report, yes.

MR. MARK: See, I was assuming that maybe I should change it. I knew I was talking in a vacuum.

(Laughter)

MR. CARBON: Are you intending to say anything here today on peer review and agreement with this?

MR. THEOFANOUS: I can give you some off-the-cuff remarks if you like on this. As you know, we have been operating on a very tight time schedule. On the other hand, we had an extensive review. We had really two reviews, pretty much official reviews. One was done at Los Alamos about two weeks ago or one week ago -- I think it was two weeks ago -- in which this review was conducted by the Los Alamos management, represented by McDowell, Mike Stevenson, Jim Scott, and Laron

Smith. And as part of that, it was the first time that we actually presented the results of this study to a body of people. At that time we actually saw after the report and also this meeting was attended by Curtis Allen, Bill Morris, and Nelson Grace from the NRC.

Then following this meeting we went through and made some -- based on their comments, made some changes. Then we had a meeting last week which was like a review meeting conducted for the NRR at large and the Office of Nuclear Regulatory Service, so this was attended by Hal Denton, and the Office of Research was Denny Ross, Charles Killberg, Bob Curtis, Bob Wright, and a number of other people, and Phil Wood.

We gave basically a similar presentation to the meeting. So that was like a peer review, and in particular Denton encouraged everybody up to 6:00 or 7:00 or 8:00 to bring up any comments or reservations they had, and we did not hear anything negative. In fact, everything that we heard from that group as well as from the previous group was very positive.

Now, we also had, as you probably gathered from the cover page -- the main part of our team is from the Los Alamos Laboratory, and to the extent that these people are there and available, in fact I think they have read most of this in court over the last two weeks, and again, individual workers were familiar with what we were working, but it was the first time they saw the whole thing, and the comments I got were that everybody didn't have any problems with what we were saying here.

That leaves out only the group at Argonne, and that is Harry and Phil and leaves out the Sandia Laboratory. We have one member who acted as a consultant, Dick Bast, who has not seen it. I think that is about it.

Now, what we intend to do is get this report to Sandia and also to get it to the people that -- like was suggested in the previous presentation. We did send our document at that time around to the laboratories and asked for comments, and we did get some comments like that, and those letters are available. We can make a copy of them any time and get them to you.

We intend as soon as we finish this business here to respond to those comments. Some of the comments, of course, we took them and we incorporated them or did some work and put in the report. Some other ones we didn't feel they apply, and those we are going to respond to by letter at some later time. So the next thing we are going to do is follow the same procedure, take this report and send it around to the national laboratories in the community and ask them for a letter from them with their comments, hopefully before the 10th of the month so we can have their input by everybody before the next committee meeting. Maybe Charley wants to add anything to that.

MR. BELL: I was trying to recall who else was at the NRR review last week. I knew Professor Reynolds was there.

MR. THEOFANOUS: Curtis Pierce was there.

MR. BELL: Alan Walter. I guess he is a consultant, but well-known in this area. Dick Ireland, I guess, was there.

MR. CARBON: Could you, Mr. Stark, go a step further and say are all the NRC people that Charley just mentioned in agreement with this?

MR. STARK: We don't know of anyone that has a problem. We certainly made several opportunities recently available to encourage as many people, and as Theo is saying, Harold in particular was trying to encourage people to dig down and ask questions, get them out now, don't be bashful. We made a very conscious and serious effort to do that, and I know of no one within the NRC right now.

MR. THEOFANOUS: But I do want to say, though, that we don't want to oversell the agreement now at this point because I think that it is not unreasonable to expect that somebody will go to a whole-day meeting and listen to this complicated story, and by just not voicing big disagreements, to take it for granted that that means a lot of agreement.

I would like to see that everybody had a copy of that and actually had it with them for two weeks, and if then I don't hear anything, only then I will say that there are no problems.

MR. CARBON: So you will follow up?

MR. THEOFANOUS: We are following up by sending out and requesting comments from the next two to three weeks, and we will hope to be able to summarize the comments for you.

MR. CARBON: Bob Wright.

MR. WRIGHT: Bob Wright. I might just comment in that regard, speaking essentially for myself, I think I am the only one from Research here at the moment. I think the questions were did we have real objections with the conclusions, and I think all the Research people that were there went along and thought that the scoping analysis and all the conclusions followed well, and we so expressed that. There were some details, as one would expect, in such a substantive piece of work with which one could have some questions, and several of us, including myself, did have such, but I don't think they affected the conclusions particularly. That is the point I want to make in general.

The thrust of the report was very well-conceived.

MR. CARBON: Any more questions of Dr. Theofanous? If not, thank you very much.

MR. THEOFANOUS: Thank you.

MR. CARBON: The Applicant is on the agenda to comment. Dr. Dickson?

MR. DICKSON: We have no comments. Thank you.

MR. CARBON: May we ask, are you in agreement with what has been said here today except for the one point that you have only committed yourself to consider the plenum gas question that came up this morning?

MR. DICKSON: Well, of course we haven't had the opportunity that the Staff has to review this document. We just saw it. But I don't believe anybody has any basis for disagreement at this time.

MR. CARBON: Dr. Fauske represents you or is a consultant to you, is he not?

MR. DICKSON: He is.

MR. CARBON: Could he comment on his views of this?

MR. FAUSKE: I think generally speaking, as Paul Dickson pointed out, I think we agreed with what we have heard. I would just like to reemphasize that all day we addressed probability events, and in addressing such events, one should apply reasonable assumptions, and I think on the basis of applying reasonable assumptions, the project came to the conclusion that energetics are very benign, and I think listening to Theo and Charley, they apply reasonable assumptions, and it is my understanding that energetics is very benign indeed, and in fact, in order to develop energetic events, you have to stretch physical reality, and even if they stretch it pretty far, so to speak, the energetics still are within the capability of the machine.

So in summary, I think that we generally agree. I think in the particular area of compaction of the fuel by fission gas, I think we want to look at it a little more, and my only personal view on that is that I think that is of potential concern. It is a means by which you could get into energetics, and I think you want to look at the analysis. I think analysis of this regard may be more expensive than making the design change. That is something we will be addressing in the next year or so, so in summary, I would like to take the opportunity to congratulate Theo and Charley and all the rest of the consultants for, I think, very outstanding and a very independent piece of work in this most difficult area.

MR. THEOFANOUS: Thank you.

MR. CARBON: Does anyone have any more questions?

MR. THEOFANOUS: This morning you brought up the problem of yesterday having to do with the expansion process, that there was some confusion about how we are doing it and how the Applicant is doing it. Then he was hoping that my presentation will qualify this point. Is there any point of confusion left there as far as how the Applicant is counting energy and how he is doing this versus how we are doing it?

Since we are all here, maybe we can inquire.

MR. ZUDANS: I don't think at this time I have any further problems.

MR. THEOFANOUS: Okay.

MR. ZUDANS: I think that you added to -- the question is -- I am a very primitive person. Unless I can do it, I am not satisfied. I think you have done a magnificent job.

MR. THEOFANOUS: Thank you.

MR. CARBON: I would also comment to you and to Charley, it appears to be a real fine job. We thank you. We thank all for the presentation.

END 4E

(Whereupon, at 4:30 p.m. the meeting was concluded.)