MEMBER LEITCH: Okay. Thank you.

MR. TREGONING: So more than true ruptures. The other thing is it's just not Class one. It's Class one, two and three as well as the support piping. It's a fairly comprehensive look at the balance of plant piping within the system. Primarily right now the database is heavily U.S. biased for many reasons. That's actually good for our intended benefits because that's certainly what we want to focus on.

We'll also be pulling in, because again it's just not the pipe break but there are other potential things that can lead to LOCAs, current PRA estimates for some of these other more traditional LOCA initiators; valves, pump seals, IS LOCAs, and generator tubes. The idea is to combine these with the pipe database efforts to develop what we're going to call our service history baseline. These would be the numbers or the frequency distributions that we would be updating through the elicitation.

Again I use the word bounding here in a deterministic sense. We also want to pull from recent information from other industries; commercial fossil plants, petrochemicals, oil and gas transmission, not to use the numbers themselves, but just to provide us with a sanity check to make sure where we're going makes sense relative to the information contained within other industries.

Again, like I said we'll be eliciting to determine if any modifications to the service history baseline are required. If modifications are required for the longer term, over the next 35 years if the expectations are that those modifications will lead to increases or decreases. So we'll certainly probe the full spectrum of possibilities for LOCA type rate frequencies.

This is some more motherhood statement of things we want to keep in mind. We plan on using some modelling as we've talked about, utilize to base some of the expectations on the future changes in LOCA frequencies resulting from aging, not only aging but then mitigation of aging mechanisms. This is along the lines that we talked about earlier where you use the elicitation to provide input to the models. So you consider that and then you consider your model uncertainty, the fact that two models can give two very different answers in determining what your final estimates are.

We're still in the embryonic stages of planning on this. We're really envisioning two elicitation processes. One is where we do a more traditional elicitation like we did in the near term where we essentially query the panel members within their areas of expertise and develop the numbers for the LOCA frequencies from that query. So that's one parallel path. The other parallel path is it's more the group approach, to have the experts provide the input to the models and let the models provide the answers themselves.

So we really at this point we're envisioning at least two parallel paths, maybe a third if we talk about breaking into small groups and having each of the small groups make estimates. The idea behind that again is to find some sort of sensitivity analysis or sanity check I like to call it on the numbers that we're getting.

We talked about this, the effect of unique events. These would certainly be unique events in the future; things like Davis Besse, maybe things like hydrogen combustion, and the emergence of additional mechanisms that maybe we haven't considered historically. We'll also probe within the group what the group consensus about the effect of what these events are.

Again, the idea is to also factor in ISI mitigation strategies so that we're not just looking at aging and potential degradation, but then the effect that the response would have on decreasing any increases due to that. So we've talked a lot about this, but the idea is to consider as many factors as possible and be as balanced as possible to try to come up at the end of the day with updated numbers that again are balanced and seem to include as much as we can. We'll break things down in the elicitation so we'll have to recombine everything to determine the final frequencies that come out of this elicitation.

MEMBER FORD: I'd like to just make a remark. We were talking about this before lunch. On that particular item, I do encourage you to bin things according to not just the reactor types, BWR, but also how the reactors have been operated and the materials of construction.

MR. TREGONING: Right.

MEMBER FORD: The -- set to 304 versus 316 but also water chemistry control.

MR. TREGONING: One of the good things about the SKI pipe database different from the earlier databases is it tends to be much more comprehensive in terms of the things that are in there. There's root cause analysis associated with not all but a good percentage of the pipe rate numbers that are in there.

As I mentioned earlier, that can allow you to go back and probe frequencies due to certain mechanisms, and it can provide you a way. You can isolate on just a certain mechanism and just take that mechanism through saying this is what this mechanism has provided historically. What do my PFM models say should be any additional adjustment for future considerations for just that mechanisms alone? We certainly need to do that binning and that consideration.

Finally, we have a longer term effort to actually redefine the spectrum --

CHAIRMAN STACK: Rob, can I just hold you for a second here?

MR. TREGONING: Sure.

CHAIRMAN STACK: We have someone from Westinghouse who is going to talk a little bit about their model.

MR. AUSTRATER: This is Bob Austrater from Westinghouse and representing Westinghouse Owners Group. I just wanted to make a comment. There's been a few comments made about the fact that we were going to do a bunch of work related to frequencies and bring it in, and it would be different than the work going on here.

We have done a bunch of work in the Westinghouse Owners Group. We've had discussions with different staff members in Mike Mayfield's area. The issues that keep coming up are the issues we expect this elicitation panel to deal with. We have made it known that we'd like to be involved in this process. What we'd like to do is come and have one set of numbers that everybody results in and not come in with two separate ones and then we dicker about the details.

That also injects us into the process. If we have any process issues, we can get those on the table and try to address those. So we're intending to work together certainly from the Westinghouse Owners Group, and I think that's pretty much true from the industry. I just wanted to make that point.

MR. TREGONING: That's certainly the intent of that study. The intent is to like you say head off the two estimate approach at the pass so to speak. How successful we'll be remains to be seen. That's certainly as we stand now the effort.

Okay. Shifting gears a little bit to talk about the longer term work. Again, this is really a framework at this point because there's been no real work going forward done. I just wanted to talk about the goals, the general approach, the objective, and some of the technical hurdles that we're going to need to overcome to accomplish this task.

As we talked about, we're going to be potentially determining a maximum pipe break size to serve as a surrogate for a design basis or the traditional design basis accident of the double ended guillotine break of the largest pipe in the plant. That will be the objective, to look at the feasibility of replacing that --

MEMBER APOSTOLAKIS: Maybe I'm missing something. Is it only the size that you will determine? I mean, the current design basis accident is not just based on size. It's plus loss of power. You may be defining the context within this particular size will serve as a design basis.

MR. TREGONING: Yes. I believe and I'll defer to somebody that's more experienced than me in terms of capacity, the capacities for the systems are defined based on this.

MEMBER APOSTOLAKIS: The current 50.46 has all kinds of requirements. Right?

MR. KURITZKY: The size and location are the two things that 50.46 gets at.

MEMBER APOSTOLAKIS: No, the reliability calculation would not be affected by this? The size of the LOCA? Sure.

MR. KURITZKY: If you go with the existing GDC 35, it says whatever the spectrum is you need to consider the loss of off-site power and the single additional failure.

MEMBER APOSTOLAKIS: So you mean that will be used in that context.

MR. KURITZKY: Right. It's the size and location that's being addressed here especially in breaks.

MR. TREGONING: Right.

CHAIRMAN STACK: The redefinition could in fact be combined with the work you're doing to come up with --

MEMBER APOSTOLAKIS: That's what I'm asking.

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: Yes.

MR. KURITZKY: It's just that the time scale --

MR. TREGONING: Is different, yes.

CHAIRMAN STACK: Eventually, yes, they will --

MEMBER APOSTOLAKIS: Eventually meaning beyond the two years.

MR. TREGONING: We're in the hope for this by 2004.

MR. KURITZKY: That's a couple years behind the other stuff we've been discussing today.

MEMBER APOSTOLAKIS: But the eventual marriage of this with your approach, the risk informed approach, that will happen before 2004?

MR. TREGONING: The marriage will occur as part of this.

MEMBER APOSTOLAKIS: As part of it.

MR. TREGONING: Yes.

MEMBER APOSTOLAKIS: Okay.

MR. KURITZKY: Theoretically the other stuff we already have in the books by then.

MEMBER APOSTOLAKIS: Again, the max pipe break size, one can take that and go to the current 50.46 and implement it as is with this new size. Is that correct?

MR. TREGONING: You could do that.

MR. KURITZKY: You could in the future potentially.

MEMBER APOSTOLAKIS: You could in August 2004.

MR. TREGONING: Maybe.

MR. KURITZKY: Well that's technical work.

MEMBER APOSTOLAKIS: July?

MR. TREGONING: We might not support --

MEMBER APOSTOLAKIS: Why conflict? I could also take that and use it in my risk informed approach that can be site specific or the generic approach. Is that correct too?

MR. KURITZKY: What would happen then is the work that we're doing right now is based on these LOCA frequencies that will hopefully come from the interim intermediate term.

MR. TREGONING: It's the unyielding term.

MR. KURITZKY: Now when this work is done, theoretically we could have better, more confident LOCA frequency numbers that we then use for the same purpose. The one thing we have to look for in the risk is if this is done and it comes up with less uncertain numbers and they are substantially higher than what we came up with in the intermediate --

MR. TREGONING: Here's how I would maybe try to think of it. You have your LB LOCA frequency. LB LOCA covers the whole spectrum of pipe sizes from six inches potentially up through 31 inches. Pieces of that distribution are attributed to different pipe sizes. Essentially what we'd be trying to do here is evaluate the dependency between LOCA frequency and pipe diameter.

If we say for instance just for argument that 99 percent of the LB LOCA frequencies happens from contributions from pipes that are ten inches to six inches, then that perhaps, and I'll say perhaps because it's not clear, provides a rationale for looking at a potential design change. I'm saying if that's the case, then maybe you don't need to consider a 31 inch pipe break. Maybe you only need to consider the ten inch pipe break or if you want to have some margin, I don't know, a 15 inch pipe break. That's the thinking at least, that we would try to more definitively evaluate how pipe size relates to LOCA frequency, determine if we can make an assessment if there are certain pipe sizes that are just so unlikely to fail that they can be eliminated.

MEMBER KRESS: Do you have a list of what you think would be the resulting design changes as a function of the pipe size you choose for the maximum size?

MR. KURITZKY: As far as what types of applications industry would be nursing?

MEMBER KRESS: Yes. Because in order to

do that second sub-bullet I think you need that. You need to know what changes are going to result when you choose a different pipe size as your large break LOCA.

MR. KURITZKY: Through a number of public meetings, we've been meeting with stakeholders on this topic for probably a couple of years now. Early on, we had lists of potential applications and benefits that industry --

MEMBER KRESS: Okay. You're getting an idea of what they would do if you change that.

MR. KURITZKY: Right.

MR. TREGONING: You're right. That has to be factored back in at the end of the day to make sure your risk guidelines aren't being violated.

MEMBER KRESS: That's right.

MR. KURITZKY: The Westinghouse Owners Group has supplied us with some information that any IS --

MEMBER KRESS: Is it a function of the pipe size you choose? If you choose a real low one, will that do a lot of things?

MR. KURITZKY: The information we had was looking in general about getting rid of large break LOCAs --

MEMBER KRESS: For the diesel start

problem?

MR. KURITZKY: For a lot of different things. It wasn't categorized by if you got to this size, you could do this.

MEMBER KRESS: Okay. You don't have it according to size.

MR. KURITZKY: I think if you got all the way down to six inches, you could do all of these things. Now, if you don't get that far down, you get ten or 12 or 14, there's some subset that would probably --

MEMBER KRESS: That's what I was thinking.

MR. TREGONING: But that feedback on down the road is going to be important. If we're able to be successful in saying pipe size versus LOCA frequency and we come up with some graph, at that point you would want to bounce that off of what changes in the plan and how would the risk based guidelines change due to potential leak plant changes. This bullet is just to say that determining LOCA frequencies or LOCA probabilities is just one piece of the puzzle. You need to combine that in the entire risk space to make sure at the end of the day you're satisfying your original intent of those guidelines.

MEMBER APOSTOLAKIS: But the work that

Alan presented earlier this morning on the ECCS reliability requirements. Until this work is done, you will go with the current definition of the maximum size. Right?

MR. KURITZKY: Yes.

MEMBER APOSTOLAKIS: Okay. So all this work will be done with that. Now in July 2004, some utilities may decide to go back and revisit the reliability requirements with a new maximum pipe break size. Is that correct?

MR. TREGONING: It's possible.

MEMBER APOSTOLAKIS: It's possible, yes.

MR. KURITZKY: Yes. The work we discussed earlier was dealing with the probability or the frequency of the LOCAs.

MEMBER APOSTOLAKIS: Which LOCAs?

MR. KURITZKY: It can be any of them. But let's say large break LOCAs. The PRA doesn't distinguish between a six to ten inch break, or ten to 14, or 14 to 18 or whatever. Large break LOCA is a category. Different plants have different --

MEMBER APOSTOLAKIS: So this is insensitive to the size of the break.

MR. KURITZKY: It's somewhat.

MR. TREGONING: Somewhat.

MEMBER APOSTOLAKIS: Somewhat.

MR. KURITZKY: The frequency may change also. This long term effort --

MEMBER APOSTOLAKIS: Because of all this work.

MR. KURITZKY: Right.

MEMBER APOSTOLAKIS: Okay. Not because of the definition of the maximum pipe setting.

MR. KURITZKY: Right.

MR. TREGONING: Right.

MEMBER ROSEN: I thought that you had exercised risk based things from here. There it is again. Do you mean risk based or risk informed?

MEMBER APOSTOLAKIS: Informed.

MR. TREGONING: I apologize if I'm not using the correct vernacular.

MEMBER APOSTOLAKIS: It may be risk -- soon. Now it's informed.

MR. TREGONING: I apologize. I'm never sure what to put down for something like that.

MEMBER APOSTOLAKIS: Yes. We have to change the terms every five years.

MR. TREGONING: I just can't keep up.

MEMBER APOSTOLAKIS: It's risk informed.

CHAIRMAN STACK: Okay. Onward.

MR. TREGONING: Okay. Next slide please. I'm just going to finish with two slides that talk about some of the technical advances that will be needed. The first thing that we'll be doing is evaluating and updating current codes and models to make sure that we're adequately modelling these pipe failure mechanisms. We'll be drawing off of work at Argonne and other places for the latest modelling and crack rates and things like that under some of these various relatively severe environments.

The other big change from most of the PFM work is where possible we want to utilize realistic loading histories and frequencies for these various pipes, not code allowables and things like that. We're going to try to make it as realistic as possible. Again, whenever you do this type of analysis, you have to combine your loading with your residual stress distribution and your pipe boundary conditions.

This I would argue is really the crux of the problem. There's so much variability. There's so much variety in these input parameters. As Bill said and certainly Dr. Ford knows the order of magnitude and difference of your answer that you get from your model really lies in the assumptions that are made here. This is going to be very critical to go through this in a rigorous way, as rigorous as possible. Like I said, we want to incorporate up to date as much as possible, material aging and environmental effects models.

MR. SCHROCK: What is realistic/conservative?

MR. TREGONING: I don't want to prejudge how we decide to deal with residual stress distributions.

MR. SCHROCK: No, I mean what do you mean realistic/conservative?

MR. TREGONING: It could be either/or. There's a couple of different ways to deal with any of these inputs. Historically what's been done is you say I'm going to take the worst case I can imagine for residual stresses and apply that. That leads to an answer. Sometimes that lone assumption can really drive the outcome.

One of the things we'll need to be doing will be looking at the sensitivity to the result to these types of assumptions. Again, there's a lot of unknown in here. It may end up being perfectly appropriate to make this assumption. The goal is and the hope is that we'll be able to focus in on something, on assumptions that are as realistic as possible.

But again, there are so many input parameters and there's so much variability that sometimes you can't get there for any given parameter. If you really can't get there in the past what's been done is people have made conservative assumptions realizing that there's some margin which may be quantified or not and then move on.

Again, the goal is going to be to use realistic inputs across the board, not just for residual stress distributions, but across the board. There may be instances when we have to fall back on essentially conservative or bounding type analysis to provide us with inputs. I don't think that's unique to this problem. A lot of problems are forced to make those assumptions.

Finally, we talked about this a lot. We want to also incorporate this into our redefinition. We need to develop some sort of scheme to incorporate potential or surprise future mechanisms. We'd like to base that on what we've seen from service history.

MEMBER APOSTOLAKIS: This is the structuralist interpretation of defense in depth. That's how you anticipate surprises.

MR. TREGONING: Yes. So we realize that we need to add something.

MEMBER APOSTOLAKIS: Take extra measures. That's what it is.

MR. TREGONING: Right. We need to account for this. How we do this is really up in the open. There's going to be a lot of contention in this issue understandably. It's something that we're going to strive to do within the effort.

We'll be considering effects from normal operating loads, but then also certainly transients and by transients, earthquakes, certainly well known, but then also thermal transients which are actually more prominent and in some systems can lead to much more damage. We need to look at considering updating our flaw distributions again. We've done this effort for RPBs but certainly fabrication differences and piping could lead to differences in these distributions.

The other thing that we have to consider that we don't have to consider for PTS is the effect of flow initiation; flaws that are not there or maybe there on a microscopic sense initially, essentially initiate and grow due to these aging or degradation mechanisms. The other thing we need to do since we're talking about LB LOCA is to consider LB LOCA frequencies from internal pipe failures but then also external failures because they also potentially contribute to the LB LOCA frequency.

So just considering pipe break frequencies is not quite enough especially when you're looking at potentially removing that consideration from your design basis accident. You need to think, at least consider the potential effects from LB LOCAs occurring from external events. That's it. It wasn't quite 15 minutes, but not bad.

MR. KURITZKY: Let me make one point. I think Rob began to mention on the long term effort that nothing was ongoing right now.

MR. TREGONING: Right.

MR. KURITZKY: Correct me if I'm wrong. There's a lot of code work that is being pursued, modifications to the PFM codes, *et cetera*.

MR. TREGONING: We don't have a contract in place. I would say that nothing substinative is really happening. There's certainly been a lot of thought put into the approach. We haven't really sat down and rolled our sleeves up yet and dive into it.

MR. CHOKSHI: I think in part like with respect to CRDM activities and other things, we are making some modifications to the codes which would fit into this project.

MR. TREGONING: That's true. So modifications for that, yes. Those things will feed in. So they're corollary efforts that will have an indirect role in this. But there's been no direct work per se. Thanks for clarifying that. That's an important point. Any other questions?

CHAIRMAN STACK: If we can move on to the acceptance model. Just remember, Steve, 5:00.

MR. BAJOREK: Got it, 5:00. I'll promise to make up a half an hour here even if it takes an hour to do so. The package that's coming along is not as onerous as it looks. We're not going to try to go through every one of those. There is a little bit of background information. With the extra time that I've had, I'm going to try to throw out a few that I think we've already covered.

What we're going to do the rest of the afternoon is talk about the 10 CFR 50.46 acceptance criteria and Appendix K. My name is Steve Bajorek. I'm here with Norm Lauben. He'll be talking about the decay. Ralph Meyer has done a lot of the work on the acceptance criteria. He could not be here today, so I'm going to go over his information.

Just by brief background, what the three of us have been working us stems from SECY-01-0133 that has asked us to try to go through, look at the feasibility of risk informing the acceptance criteria, look at Appendix K, in specific look at the decay heat model and three other models in there for potential relaxation, but also to take a look at what might be some of the problems of Appendix K. What are some of the shortcomings that could lead to nonconservatisms? If we're going to go through rule making, the idea is we'll take the bad with the good and fix everything at the same time.

We have been working in those three areas with regards to the acceptance criteria. Most of the work has been really a history lesson in where the 2200 and the 17 percent embrittlement criteria came from and the other models in Appendix K. We'll go through each of those. By way of outline, what I hope to accomplish this afternoon is we put up recommendations that we'll get to at the end of the day and we'll follow through most of these in order.

First, we'll talk about the acceptance criteria. Our conclusion at this point is yes, the remaining ones which are not risk informed we should be able to do so. We think that in the case of the decay heat and the other Appendix K models it's feasible to come up with a new Appendix K which we might call and Appendix K prime that would make use of better science, replace the decay heat, and replace some of the other models that have been suggested for change. We'll talk about each of those.

Then we'll move into some of the nonconservatisms that we feel need to be incorporated, not necessarily in rule making. But because they're also applicable to today's Appendix K, they should be pursued outside of rule making. When a new Appendix K and acceptance criteria do come to pass, they need to be corrected on that type of a time frame.

MR. SCHROCK: Steve, here you're saying replace the old decay heat standard with the new one. In Appendix K, it seems inconsistent with what was said earlier. It's an option added to the existing.

MR. BAJOREK: It would be an option. The way that we're thinking of this and have an overhead to try to -- As I did hear those questions early, the way we're thinking about this is in the generation of a new option that would give the Applicant the liberty to use a more up to date decay standard, the '94 standard in this case.

We would preserve as grandfathered options the current Appendix K where you would be required to use ANS plus 20 percent and the best estimate rule where the intent would have been to use the most recent decay heat model, but in reg guide 1.157 they do specify the '79 model. At the end of all the additional documentation that would take place, we would anticipate correcting the best estimate guide 1.157 to allow it to use the most modern decay heat standard as well.

What I'd like to talk about first is the acceptance criteria and along with that the metal water reaction correlations that are used in Appendix K because they're closely tied together. As I mentioned Ralph Meyer has done most of the work in that area, but he can't be here today. I'm going to try to go through a lot of the information that he's generated.

One of the things to keep in mind is out of the five acceptance criteria that are a part of 50.46, two are already performance based. The third, the one percent hydrogen generation can be very easily be made into a performance based because it's effectively now covered under 10 CFR 50.44. The hydrogen generation and what would have to be done to containment in order to cover that was already picked up elsewhere. We're looking at that particular limit as being redundant and not necessary.

We would have to do work to modify the other two which are the Appendix K limit of 2200 and the maximum cladding oxidation of 17 percent. Both are currently prescriptive, but they're closely related. We have to look at these as a pair. The relationship and the specification of these two numbers arose from the commission rule making in 1973 that basically was intended to ensure that the core still looked like a core following a LOCA. If we had a LOCA, the core will still remain essentially intact.

Our goal and guidelines for the risk informing of 50.46 is to continue to go after that intent. When the regulations are complete and we can risk inform them after a LOCA, the core still essentially looks like a core. In addition in 50.46 right now, it is specific to two types of clad; ZIRLO and ZIRC-4. In some of the information I'm going to show you, we think that it's feasible to eliminate that need to get an exemption for other types of cladding material.

The two things that we need to take a look at are the 2200 limit and the 17 percent oxidation. We'll break this into two different categories at this point. When the commission went through the rule making, the two numbers were specifically derived from clad embrittlement tests that were done to ensure that you had margin to fragmentation of the cladding.

The 2200 degree limit was also considered in light of the possibility that it would be a runaway temperature excursion due to a very high metal water reaction. So we've tried to take this issue and break it into two at this point to try to consider what would be the temperature effect on runaway type reactions and what would be the alloy effect and would that have anything to do with that specific criteria.

This is the statement out of the commission opinion regarding the runaway reaction. If you just take a quick look at the numbers on there, their concern was primarily at temperatures 2,300, 2,400, up to 2,700. The information at the time from which the Baker-Just equation was developed showed that there would be an exponentially increasing reaction rate with temperature up in this region.

The commission using that data and essentially that correlation wanted to stay away from those temperatures at which you would have this very rapid increase. The feeling at the time was that as long as you stay closer to 2,300 or 2,400, the temperature criteria would be satisfied. I want to leave you with the message that for as far as the -- for runaway cladding reaction, there was a comfort level that something much higher than 2200 would have been used if they had sufficient information at the time.

The Baker-Just and correlations that have been developed since are generally of the form shown here; exponentially increasing erroneous type function, where the oxidation rate and the reaction increases very rapidly with temperature. Since then it's been shown fairly conclusively that the data that was used to justify the Baker-Just is overly conservative in that temperature range between about 2,000 and 2,300 degrees Fahrenheit.

In very high temperatures, it doesn't a reasonable job. At low temperatures, it seems to do a fairly reasonable job. In the middle, it's been found to be rather conservative. Newer data shows that there's much less scatter in the oxidation data then there had been in that which was used to develop the Baker-Just correlation. What we feel that it's possible to do at this time --

MR. SCHROCK: Are you going to show us the data or are you just going to show us the curve?

MR. BAJOREK: I'll show you some of the curves in a couple of figures. I wanted to try to get the data, but we couldn't get that in the right form in time. We'll do that in terms of the alloys.

If the intent was to stay away from the knee of this curve and we can replace using better information that the Cathcart-Pawel is a better representation of the oxidation and heat release data in this temperature range of interest, we can retain the same amount of conservatism by looking at the heat release that we would get from Cathcart-Pawel compared to Baker-Just. Whatever that heat release is, we can show that Cathcart-Pawel would give that same heat release at 2,307 F as Baker-Just would at 2200 degrees.

MEMBER WALLIS: What's the criterion for runaway? The curves don't give you any criterion for runaways.

MR. BAJOREK: There wasn't one in the Appendix. We haven't specified one in terms of looking at the curves and the slopes. It's really an energy balance. That's part of the problem.

MEMBER WALLIS: If it gets hotter, then it gets hotter. This is a feedback thing. It's stability.

MR. BAJOREK: At some point, you generate more energy within the clad that you can remove. So it's not simply a particular temperature. It's not indexed with --

MEMBER WALLIS: It was dependent on the environment.

MR. BAJOREK: That's correct. So you have to take a look at the energy supply from decay heat and what you could remove in steam cooling.

MEMBER WALLIS: In steam cooling the required mechanism --

MR. BAJOREK: Right now at very low flooding rates it would be. One of the very difficult things that you do see in calculations and we have done code calculations to try to see where is this runaway. It's one that's very subject to the conditions that the code is predicting in the odd assembly which gives us the concern of if you start to use a code to try to come up with that number, you're starting to base your belief on what that code can predict for -- boiling. We don't want to really do that.

MEMBER WALLIS: Something must give you this dashed line that you've drawn up there.

MR. BAJOREK: This is a representation of

--

MEMBER WALLIS: That green dashed line.

MR. BAJOREK: This green one?

MEMBER WALLIS: That came from a code or something.

MR. LAUBEN: Excuse me.

MR. BAJOREK: This is representing some energy generation rings. We don't know.

MR. LAUBEN: No. Yes we do. Norm Lauben, research. If you look at the two equations for metal water reaction and you assume the same thickness of oxide, they will give you equivalent heat release at those two temperatures exactly. In other words, all you're doing is equating two equations.

MEMBER WALLIS: I understand that. You don't know your Q delta triple prime very well.

MR. LAUBEN: It doesn't matter. You're assuming 2200 to Baker-Just and you're solving for T with Cathcart-Pawel to get the same heat release.

MEMBER WALLIS: -- uncertainly maybe will be in this.

MR. LAUBEN: Yes.

MR. BAJOREK: -- all try to represent that the same energy generation based on better information could allow us the same energy generation at a higher temperature now. We would preserve that same margin whatever it was to a run-away reaction keeping in mind that there are a whole flock of things that affect that point that would cause the cladding to increase rapidly in temperature.

MR. BANERJEE: Is the rate of increase the same?

MR. BAJOREK: This rate out here?

MR. BANERJEE: Yes, usually in a run-away reaction chemical reaction it's not the absolute value that matters but the rate of change with temperature that matters. They are the same?

MR. BAJOREK: They're different between those two correlations.

MR. BANERJEE: Because when you look at heat balance, the worst condition is clearly the condition where you are feeding just enough steam not to cool but to do the reaction in which case all of these will run away. It doesn't really matter which.

MR. BAJOREK: That's right.

MR. LAUBEN: This is just a comparison of the amount of heat generated with the two equations.

MR. BANERJEE: I understand.

MR. LAUBEN: Okay.

MR. BANERJEE: But it's the rate of change that really matters. It's not the amount of heat.

MR. LAUBEN: Well, this is a comparison of heat generation. It doesn't tell you anything about the heat removal.

MR. BANERJEE: I understand but what about the slope?

MR. LAUBEN: All right. This also doesn't tell you anything about the slope either.

MR. BANERJEE: Right. That's what it looks like.

MEMBER WALLIS: Well, Sanjoy is right. If you cool and heat something it will come to some equilibrium temperature but it won't come to an equilibrium temperature if when it departs from an equilibrium temperature it runs away because the rate at which heat supply is bigger than the rate at which --

MR. LAUBEN: All this is saying is if have an ideal world in which you want to know what the heat generation due to metal-water reaction is and all other things are the same including the thickness of oxide because the thickness of oxide is one of the parameters in the rate equation you just --

MEMBER WALLIS: I think what we're saying is a graph like this equating Q triple prime from CP to some mythical Q triple prime tells you nothing about --

MR. LAUBEN: Yes, that is correct. If you look at rates that I think Sanjoy was asking about you can see for instances that because the temperature change with temperature is so much greater with Baker-Just that you can't get to quite as high a temperature if you are using the Baker-Just because of that very reason. It reaches a run-away at a much lower temperature than Cathcart-Pawel does because of the rate of change of the rate with temperature. Is that what you are asking, Sanjoy?

MR. BANERJEE: Yes, probably if I look to the curves in more detail you may end up having the same conclusion but it's not just the temperature and the rate of heat generation that matters but the rate of change of the heat generation.

MR. LAUBEN: Yes, and what I'm saying --

MR. BAJOREK: You're right. What you find in an evaluation model is once you get up to these temperatures on either one of those the increase in heat generation and the increase in temperature can not be alleviated by the HΔT that you probably see at that part of the logo. You melt in the calculation. So all we are saying is that in terms of coming up with a limit newer information will justify a higher temperature.

MEMBER WALLIS: I think when you come back and talk about run-away to this committee you better have a criterion for run-away and not this sort of vagueness about heat transfer --

MR. BANERJEE: Well, there's a classical chemical engineering formula which is T reaction minus T coolant is equal to TR^2 into the activation energy divided by the constant or something. I can send it to you. So that tells you why it will run away or why it won't.

MR. LAUBEN: Well, the problem though as Steve said is that if you're trying to relate this to real calculations which is an oxymoron I realize. (Laughter.) But if you are trying to look at calculations none of them give smooth behavior of heat removal especially during reflood in LOCA. You get water splashing up there and the heat removal will be somewhat erratic as well.

You can never really have two identical situations with two different calculations. You will find that as much as you may try to get these things as close as possible they never will be quite as close as possible. And because of this uncertainty and variability I did about 100 calculations of this sort of thing and could never quite narrow it down.

But you could see a trend almost a probablistic type of thing that over a series of many, many calculations that the run-away will change. It will change from one minor set of -- You may be only changing the power by something in the fourth decimal point and you still won't get smooth behavior from one condition to another. You do see that in a large measure because of the nature of Baker-Just and the nature of Cathcart-Pawel that you reach run-away in general much sooner for Baker-Just because of the rapid change in temperature and the slope is very different.

MEMBER WALLIS: The slope is more important than these --

MR. LAUBEN: That's it. Sure. No. That's an easy and quantifiable way to compare it. It just gives you a minimum measure because what's really true because of the slope changes so much is that you can see a much bigger difference. In general I would say I could never achieve turn-around much above 2300 in the limited 100 calculations I did with Baker-Just but I could reach something as close to 2800 with Cathcart-Pawel. Now that's --

MR. BAJOREK: It's an energy balance.

MR. LAUBEN: It's an energy balance.

(Inaudible.)

MR. LAUBEN: Excuse me.

MEMBER WALLIS: Maybe you need to show these calculations. Something more convincing than what we heard today --

MR. LAUBEN: I think what we want to do is see and Steve has given this some thought, actually quite a bit of thought, is can we possibly do these calculations in a more controlled way than I did them because that's really the course of this? How can you really control these calculations so that when you make an incremental change you can see where this goes?

MR. BAJOREK: Let us take the action to try to put some better numbers on run-away which was not the find of the original (Inaudible.) so we haven't done that.

MR. LAUBEN: It's hard to control the uncontrollable.

MR. BAJOREK: Let's move on because this really is not the issue on setting the 2200. It's really in the mechanical integrity. That's where the 2200 came from. This is just an example of something else that was considered.

Now before we close it out the other question has to do with what's the difference between alloys and how does it factor into things in order to try to see is there a temperature effect that could be aggravated by changing from zirc-4 to some other alloy? We went through a fair amount of experimental information that included zirc-4, zirc-2, newer clads such as ZIRLO and M5.

Note by the way Baker-Just in some of the earlier data was based on zirconium data only. It was not from an alloy which might be one of the reasons why it tends to stand out in relation to the other sets of data. I would preferred to have shown the experimental data but instead we can make comparison and contrast the alloys by the correlations that have been developed at the same type of format, in a Cathcart-Pawel type of relation or Baker-Just.

You see that Baker-Just gives a significantly higher oxidation growth rate, higher energy release. I think it's about 50 or 60 percent higher when you are at 2200. Regardless of whether you are looking zirc-4 or M5 or some of the other ones which aren't shown you see there is very little difference as you go from one specific alloy to the next. The reason for that is that the dominant rate controlling step is a diffusion of oxygen through this growing oxide layer to get where the zirconium is.

The presence of tin or niobium or some of the other elements that make up the alloy almost as trace elements have very little effect on that oxidation growth rate and therefore the energy released due to the metal-water reaction.

MEMBER WALLIS: So it's the diffusion limited reaction by the oxide layer?

MR. BAJOREK: It's a diffusion limited reaction based on its ability to diffuse through the zirc oxide that is growing on the outer surface. Now the more --

MEMBER WALLIS: How do you calculate run-away for a diffusion limited reaction?

MR. BANERJEE: It's still expedientially growing. The heat loss is linear. Temperential difference. So at some point it will always take off.

MR. BAJOREK: At some point your oxidation and energy will slow down based on the growth of that layer so that comes in. I think it's more of a second order of fact in relation to the actual temperature of you're oxidizing.

MR. BANERJEE: I think you'll find this

thing will run away eventually and it's clear that the activation energy for those two reactions is very different. So you are right it will run away later with Cathcart-Pawel.

MR. LAUBEN: Right.

MR. BAJOREK: The real crux of the 2200 and 17 percent however came from the belief of the commission that the cladding following a LOCA should still remain essentially intact. They looked at several different ways of doing that. They considered thermal shock test. They looked at calculations that had been performed by the vendors. They looked at blow-down loads and deformation of the assembly during the accident.

They concluded none of those were completely satisfactory and that the only really good way was to perform experimental tests on samples of cladding that had been put through a LOCA type of transient environment and do mechanical tests on there to insure that this cladding still had ductility remaining following the preparation of the sample.

Now the difficulty that they had was that there were two things that the strength of the material was very much dependent on. First the extent of the oxidation and how much oxidation you had built up on the cladding itself. Secondly what was that oxidizing temperature that the specimen was held at before you did the test.

If we go back and think about the Cathcart-Pawel versus Baker-Just, the difference that we see in alloys is more dependent on the time it was held at the oxidizing temperature because this gives the opportunity for oxygen to diffuse completely through the oxide layer and deep into the prior beta phase of the cladding. That's what really affects the strength, the toughness and the ductility.

What had been decided upon in the original commission work was to perform mechanical strength tests on pieces of clad that would be exposed to steam at varying high temperatures. A piece of that clad would be cut and the so-called ring tests would determine what would be the oxidation at which that little ring would fail.

Based on those tests and one thing to keep in mind, the oxidation was not something that was measured in these early initial tests. There wasn't enough information but rather it was calculated using Baker-Just. The relation between failure, ductility and oxidation, that was developed from these tests found that at 17 percent it could survive some load.

I don't know exactly how that load had been determined. But those pieces that had survived the shock and retained ductility would pass this test. When there was more than 17 percent as calculated it would shatter at that point.

MEMBER WALLIS: It says nothing about the burn-up level or anything. It has nothing to do with fuels at all.

MR. BAJOREK: No, this was fresh. I believe this was all fresh.

MEMBER WALLIS: Like a very academic sperm.

MR. BAJOREK: Yes. But it also --

MR. BANERJEE: So at 17 percent calculated by Baker-Just at some temperature? The 17 percent you said?

MR. BAJOREK: You calculate using Baker-Just.

MR. BANERJEE: At some temperature, right?

MR. BAJOREK: It depends at what you oxidized it at.

MR. BANERJEE: So if Baker-Just was too high then it was less than 17 percent. Right?

MR. BAJOREK: That's correct. But the actual oxidation of those tests was probably closer to

13 percent.

MR. BANERJEE: The brittleness occurred at 13 percent rather than 17 percent.

MR. BAJOREK: Right. If you use more accurate information based on something like a Cathcart-Pawel which you would believe for this type of temperature range. Now we go back and look at the PCT limit that became a function of what had been the oxidizing temperatures.

MEMBER WALLIS: For how long?

MR. BAJOREK: Well, that would depend on how much oxidation you would grow on there? So what they were faced with was the lack of experimental information between 2200 and 2400 degrees.

MEMBER WALLIS: It would be very helpful if you used "C" or "F" consistently instead of jumping between the two.

MR. BAJOREK: Okay. Which would you prefer?

MEMBER WALLIS: Whatever you like.

MR. BAJOREK: Okay. Between 2200 degrees F and --

MEMBER WALLIS: What's this 1000 to 1200 degrees mean there?

MR. BAJOREK: That's means 1800 to 2200.

MEMBER WALLIS: Good enough.

MR. BAJOREK: They found that in considering the data they had a gap above 2200 and there was no reliable data up until much higher temperatures.

MEMBER WALLIS: Accept the criterion based on the limited data you have?

MR. BAJOREK: That's correct. So even though the available information suggested a higher PCT based on run-away temperature whatever that may have been defined as they could not find sufficient experimental information to allow the ductility to be specified for temperatures greater than 2200 degrees. That's what became the limit.

Now as somebody has pointed out, there were some problems with this. Perhaps most notable was that's not what the clad looks following a LOCA. At some places on the cladding, it will swell, balloon and rupture. Later about 1980 it was realized that the hydrogen content inside the swollen and ballooned region was significantly higher than it would be on the outside of the clad where it would be swept away. This enhanced the embrittlement in this region near the balloon.

In order to understand that better and to resolve the embrittlement question additional tests were run. I believe it was both Oakridge and Argonne at the time where they conducted similar tests. But when they examined the clad, they took pieces of clad from the balloon region, did tests and found that sure enough when you did the calculation with Baker-Just and you reached 17 percent you were starting to fail in this region.

These tests were then looked at in some additional detail. They realized that up in this region it was very difficult to perform this type of test based on where they were cutting the ring. To meet the intent of the '73 rule, they went to an impact test where you would balloon the clad, take a calibrated weight with a tip, swing this down and in the type of a Sharpie toughness test determine at what point the clad which had been embrittled now due to the swelling, rupture and the extra hydrogen would start to fragment.

They determine through a toughness test and by measuring the clad oxidation that the clad would not embrittle at 17 percent based on the results of these toughness tests. That after 1980 really formed the basis of why 17 percent was really an adequate and conservative number to cover the tests that had been done earlier.

MEMBER WALLIS: It seems to be that both these coursing tests and hitting tests, impact tests and the squeezing tests are not really typical of the loads imposed on the real cladding. You don't take it between two plates and squash it. You don't hit it with a hammer on top of which it's not a radiated material. I keep wondering what the relevance of all these tests are to the real truth.

MR. BAJOREK: I think the relevance as we look at it is (1) there are still questions on how you determine the ductility, the toughness and whether it will survive a LOCA type environment. We think that it is appropriate and feasible to risk inform that type of criteria rather than saying it's 17 percent and 2200 which is varied back on the embrittlement type of studies but rather devise a test to justify that the clad will not shatter to the LOCA type condition.

MEMBER WALLIS: That would be performance based. If you are asking this to be performance based knowing this background they would presume that you would have to do much better tests than any of these to really convince them.

MR. BAJOREK: That may well be the case.

Now we have tests getting ready to go on at Argonne.

MEMBER ROSENTHAL: Steve, can I talk?

MR. BAJOREK: Sure.

MEMBER ROSENTHAL: Jack Rosenthal. I know less than Steve about specific areas but maybe a broader view because Ralph sits next to me if nothing else. At Argonne National Laboratory right now, we're about to perform rod testing. In the course of calibrating the equipment for the tester, we ended up reconfirming Cathcart-Pawel. So that gives us a very nice contemporaneous factual basis for our work.

We have clad from H.B. Robinson and we have clad from Limerick that's high burn-up cladding which we will test. So we will have a factual basis for both fresh clad and high burn-up clad. Let me remind you that the fuel that incurs the peak PCT in a core is likely somewhat burnt clad and not thrice-burnt clad but we will account for that. So in any case we will be able to put this on a factual basis.

Steve started his presentation by saying what we want is the idea to have something that looks like a core standing there when you reflood it. So then you went to the concept of -- Am I taking your thunder?

MR. BAJOREK: No, that's fine. This is

basically a feasibility study to allow us to go ahead with risk informing these criteria. I think some of the questions you've asked should be a ring test, should be a effectiveness test, to be a four-point bending test which is also included. In fact that's still yet to be determined.

MEMBER WALLIS: Well, are any of those tests appropriate?

MEMBER ROSENTHAL: Right. The performance criteria is likely to be something that you should either retain some degree of ductility of some degree of specified toughness post quench (PH). That could form a very nice performance based criteria. You would eliminate the 2200, the 17 percent and the zirc-2 and zirc-4 from the rule itself.

The downside of that and you mentioned it is that if you go to this performance criteria and a future vendor would come up with clad-X then we would expect them to somehow unspecified, unthought out yet, demonstrate that they need that performance criteria and they might likely but not necessarily have to do tests.

MEMBER WALLIS: -- criterion is that the clad maintains its integrity. In other words it's a barrier that's not breached. Is that it?

MEMBER ROSENTHAL: No, about 1600 degrees F it will burst.

MEMBER WALLIS: Yes, I know.

MEMBER ROSENTHAL: The core should look something like a core post quench. You've reflooded it. It shouldn't shatter.

MEMBER WALLIS: Something like that's very vague. Isn't it?

MR. BAJOREK: It's made of metal. There's some oxide laying in there.

MR. LAUBEN: It's not laying on the floor.

CHAIRMAN SHACK: I mean you don't it to shatter and come apart so you basically require it to have some ductility and you --

MEMBER WALLIS: I can't see how squeezing a ring is going to tell me anything.

CHAIRMAN SHACK: It tells you how much ductility though. How much strain the material can take.

MEMBER BONACA: You mean that as it swells and balloons still it would be together rather than fracturing and falling down.

MR. LAUBEN: Excuse me. Graham, if you look at the whole of these tests that are done you find out that a lot of them fail just on handling. So it isn't all of the cladding samples that get tested because a lot of them have failed on handling. If it fails on handling, it's failed. Only some increment of samples do you even get a chance to do tests in so it isn't as though every piece of cladding that you oxidize hasn't already failed and I think you can --

MR. BAJOREK: And it may be a better criteria than what we have now because I think if Ralph were here he'd point out that they've seen tests where the oxidation is only at 6 percent and it wouldn't pass the ring compression or the toughness test now. But under today's regulations it's less than 17 percent.

MEMBER WALLIS: It's reassuring.

MR. BAJOREK: That's why going to a risk informed type regulation based on some material type of test we think is a prudent thing to do and not just rely on some number.

CHAIRMAN SHACK: Well, I'm not sure that it's risk informed. You're maintaining the same basis for the thing but you're just picking a different criteria to demonstrate that you've maintained that. It's a criterion that probably makes more sense.

MEMBER WALLIS: I think you have to show me that given the results of the test you're now able to predict what will happen to a core. I don't see the connection.

CHAIRMAN SHACK: You can demonstrate if it has that much ductility that it will withstand thermal shock for example when it's reflooded which is a good thing to do.

MEMBER WALLIS: Okay. Then that's what you have to do. You have to do that.

CHAIRMAN SHACK: Well, you can also demonstrate that it will maintain that integrity. Steve had a note somewhere that thermal shock wasn't good enough because one of the things you find is that it's a trickier test than you think it is to do. But again if you have enough ductility you can show for a wide range of thermal shock conditions even though you don't know exactly what it will be that it will hang together.

MEMBER WALLIS: So if squeezing or ring test gives you a property which you can't put into your calculations of what happens with thermal shock then you can predict what will happen.

MR. FORD: But there's still a big jump in faith. You surely must have a correlation between damage by thermal shock and some other surrogate such as strain to fractures coming on a VEN test or a ductility test. You think of this as -- Ductility in and of itself is not the sole criteria to whether you are going to survive the thermal shock.

It's not the only one. You're going to have to do quite a few tests on a radius material of different fluency levels, etc. surely to have a good feeling that using one of these criteria, using different loading rates, etc. that you have the correct specification that you are going to meet.

CHAIRMAN SHACK: You mean how much ductility do I need?

MR. FORD: Yes.

CHAIRMAN SHACK: 1.5 percent?

MR. FORD: Right.

CHAIRMAN SHACK: That's a little trickier number to come up with.

MR. FORD: But isn't that a vital number to come up with if you're going to come up with --

CHAIRMAN SHACK: You can calculate many of those numbers.

MR. FORD: So it's how it's ductile. It's not that the fiber is ductile but how ductile.

CHAIRMAN SHACK: Yes, whether it's 0.1 percent or one percent makes a big difference but they will have to come up with that limit.

MR. SCHROCK: Isn't there some information available from the PVF tests about where you start to lose the geometry of the fuel? I don't know. Is that looked at?

MEMBER CRONENBERG: That's burned up only to 20,000 megawatts days per ton or something. Right? The PVF test.

MR. SCHROCK: It went far enough. They failed the fuel.

MEMBER CRONENBERG: But it was not to today's burn-ups.

MR. SCHROCK: I'm sure that's probably true. But I mean the arguments here seem to be when do you lose the ability to maintain a core like geometry.

MEMBER WALLIS: It hasn't been addressed really at all yet.

PARTICIPANT: Our Bruce facility isn't a leaf flood test, is it?

MR. LAUBEN: I don't remember exactly. All I remember is they did actually fail fuel.

MR. BAJOREK: I don't think it was a reflood facility.

MR. LAUBEN: It was designed more for a spike I guess in the energy that was put into the fuel.

PARTICIPANT: It was a broad assertion --

MR. LAUBEN: Yes.

CHAIRMAN SHACK: There are very few full scale tests of a reflood LOCA load down.

PARTICIPANT: NRU --

MEMBER WALLIS: 2200 has a very iffy basis. The only justification really is that it is worked over 30 or 40 years. If you are going to change it you're going to have to have some really good arguments.

MR. BAJOREK: The basis for what the new criterion and regulation still has to be worked out. Is it toughness? Is it ductility? Is that a sufficient amount? We look at that as a question what will be hopefully be answered out of the on-going test program that's going on at Argonne and really work by some of the people that really understand materials.

CHAIRMAN SHACK: As you go up in temperature, the oxygen also as mentioned goes into metal. You know you're brittle. There are all sorts of reasons not to go above a certain temperature. It's not just the oxidation. You embrittle the hell out of the thing very quickly with relatively low amounts of oxidation.

MR. BAJOREK: As in the case of the one cladding material increasing that temperature to 2250. Maybe be the limit for it.

MEMBER WALLIS: Burn-outs have an effective on this? Does all the radiation and chemical environment somehow have an effect on it?

CHAIRMAN SHACK: I think you're mostly looking at the oxide. Surprising little because it anneals. The bad news is it gets damaged. The good news is that it's going up and gets annealed. So if it makes it up --

MEMBER WALLIS: Up on what? Temperature?

CHAIRMAN SHACK: On temperature. If it can last the ramp-up you are going to lose the radiation damage. Now you are picking up oxidation damage at a fairly furious rate. But again it makes the 17 percent -- There's a debate over the 17 percent includes the prior oxidation or it's just the oxidation during the ramp-up. There are reasons for various sorts of things but again as he said it's a the basic notion which I think that it's really the ductility that you want to maintain and it's the correct one.

MR. BANERJEE: You're not proposing to change the limit to anything then. Right? I mean if you substitute the Cathcart-Pawel in place of the Baker-Just if the intention is just to really keep the fuel ductile enough that it doesn't break up, the test you have shown us suggested that it's really the temperature that matters not the correlation that Baker-Just or something. All you've done is back calculated the oxidation.

MR. BAJOREK: Let me make it clear. The early tests were done using the correlation to estimate what the oxide thickness was.

MR. BANERJEE: Right.

MR. BAJOREK: Tests now that would use the toughness requirement at Argonne they measured the oxide. So we've gotten away from relying on the correlation to determine it.

MR. BANERJEE: And at what temperature do they become brittle?

MR. BAJOREK: So it's what temperature you did the oxidation that has potentially the largest effect on whether you have any ductility once you do one of those tests.

MR. BANERJEE: So substituting Cathcart-Pawel or whatever doesn't really matter. It's just a question of ductility. All that is irrelevant.

MR. BAJOREK: Not on this part. When you

start looking at run-away temperatures, there you have to predict things.

MR. BANERJEE: It must be time and temperature, too. Right? The amount of oxidation.

MR. BAJOREK: For this, yes. Part of the Argonne plan is to take radiated samples, subject them to a time-temperature history where they would be exposed and this is 1200 degrees C that didn't show up, leave it at that temperature for some period of time and then cool it off and quench it at a rate similar to what you might expect during a large break LOCA.

The tests would consider temperatures I guess both higher and lower this in order to establish a wider range. But yes, time at that temperature also makes a big difference because that's what's allowing the oxygen to diffuse deep into the metal.

MR. BANERJEE: So you aren't suggesting any revision to the criteria right now until these tests are done.

MR. BAJOREK: Rule making can proceed at this point.

MR. BANERJEE: To do what?

MR. BAJOREK: To start coming up with new text.

MEMBER WALLIS: You can make any rule you'd like but you better justify it technically.

MR. BANERJEE: But are you suggesting that you change the correlation and change the amount of oxidation and change the peak clad temperature? What is the suggestion on this?

MR. BAJOREK: We're not changing the correlation with respect to 5046. The acceptance criteria would be based on material integrity tests. Those would be such that you would expose the sample to a severe environment. You would oxidize it. You would measure the oxidation.

You would develop a criteria probably in a red guide that would specify how you would do those tests and under what conditions you would assume that the cladding had passed the test based on either a ring compression or a toughness or a four point bend test. That is still yet to be determined. We would hope that we would get information from the Argonne test program to really guide that.

MR. BANERJEE: So that would be alternative route to satisfying this requirement. I mean either you could use Baker-Just 2300 or whatever has worked, 2200, or you could go this way. What are you really proposing? That's what I don't understand.

MEMBER ROSENTHAL: Why don't put up your six option slide again? Remember your matrix slide. Let me point out that Steve is talking about the evaluation.

MR. BAJOREK: Don't confuse Baker-Just when it comes to the acceptance criteria. It was used in the past. It will not be used and has not been used in the present justification for 2200 and 17 percent. Baker-Just versus Cathcart-Pawel or other correlations are going to be recommended for revision. This will be in a new appendix K.

In appendix K presently you are required to use Baker-Just to calculate oxidation and the metal-water heat release in your evaluation model. Our recommendation for appendix K will be to calculate the heat release using Cathcart-Pawel instead of Baker-Just. It's better science in that temperature range that's very important in the LOCA analysis.

Likewise other correlations and we haven't talked about those yet which are specified by appendix K, we look at better information and we'll say yes there is better science. Those don't have to be prescriptive. So that is where the discussion Baker-Just versus Cathcart-Pawel should be. It's not with the acceptance criteria.

MR. BANERJEE: So the Baker-Just was used to determine the heat release.

MR. BAJOREK: In 1973.

MR. BANERJEE: Right. Now the idea was that basically you didn't want this thing to run away. Right?

MR. BAJOREK: That was one of the commission's concerns.

MR. BANERJEE: And what was the other concern? Was it the embrittlement?

MR. BAJOREK: Clad embrittlement. That is what was used to determine 2200 and 17 percent.

MR. BANERJEE: Right. So with regard to the clad embrittlement part, you are going to do some experiments or whatever to handle that part.

MR. BAJOREK: Experiments to define --

MR. BANERJEE: So whether you use Cathcart-Pawel or Baker-Just or whatever is irrelevant there. It doesn't matter.

MR. BAJOREK: Right.

MR. BANERJEE: Okay. With regard to the run-away reaction, Baker-Just or Cathcart-Pawel will give you somewhat different answers.

MR. BAJOREK: Right.

MR. BANERJEE: I guess then one has to

find out really what the difference is going to be between the two. It's not obvious that equating the two heat releases gives you a higher temperature or whatever.

MR. BAJOREK: Right. You're going to have a different thermal time constant of the clad depending on which energy generation term you use. Whether it runs away or not, the temperature-time we haven't put a number on that. Nobody has done that. That depends on an energy balance.

MEMBER WALLIS: Does it run away more every time its balloons delivers an attack?

MEMBER SIEBER: I wouldn't think it would be faster.

MR. BAJOREK: If it's in a balloon?

MEMBER WALLIS: Right.

MR. BAJOREK: If it's in a balloon you will have a double sided reaction.

MEMBER WALLIS: But it will run away faster, wouldn't it?

MR. BAJOREK: But it also acts as a fin.

MEMBER WALLIS: (Inaudible.)

MR. BAJOREK: Yes it is.

MEMBER WALLIS: -- 100 calculations involved.

MR. LAUBEN: Right but there's another thing. This may seem hard to believe. Actually in the range of 2200 to 2400 and in a balloon region, you will actually find out that K heat is still the major heat source. It's only when you get real high does the metal-water reaction become the predominant heat source. So you can't ignore the K heat anyway.

So ballooning helps you as Steve says in terms of heat removal but it also removes you at least for a time from your heat source until the surface of the fuel can rise high enough to radiate -- Excuse me.

MEMBER WALLIS: It affects the cladding by the fuel gets hotter.

MR. LAUBEN: The fuel gets hotter --

MR. BAJOREK: Yes. The ventilation is on the clad temperature.

MR. LAUBEN: Yes, that's right.

PARTICIPANT: How do you know that it might be above the burst node also but not necessarily at the same elevation?

MR. BAJOREK: Which should basically serve -- We have to be very careful about using codes.

MEMBER WALLIS: I'm going to retire before we get to the end of this --

MR. BAJOREK: They're never going to allow you to retire.

MEMBER SIEBER: What about the blockage of the cooling channels when the ballooning occurs?

MR. BAJOREK: Excuse me.

MEMBER SIEBER: That would cause the temperature to go up too. Blockage in the cooling channels.

MR. BAJOREK: That would still be in this new appendix K approach. You would still be required to look at blockage in swelling and its effects on the flow distribution in the hot center.

MEMBER SIEBER: But it will occur at temperatures below 2200.

MR. BAJOREK: Yes. The swelling and blockage occurs at 1500 degrees F.

MEMBER SIEBER: Right. So it all balloons out and goes up there faster.

MR. BAJOREK: Yes. All right to try to move on. The next thing that we would like to go over is the decay heat model and start moving into appendix K. This is where we will start looking at models that came be replaced by better science but at the expense of looking at some non-conservative issues in appendix K.

CHAIRMAN SHACK: Before we start a new

topic let's take a break for 15 minutes so we reconvene at 3:10 p.m. Off the record.

(Whereupon, the foregoing matter went off the record at 2:55 p.m. and went back on the record at 3:10 p.m.)

CHAIRMAN SHACK: On the record. Let's get started.

(Discussion off record.)

MR. LAUBEN: I do have a statement of religious belief that I wanted to start with.

PARTICIPANT: Was Milton connected in some way?

MR. LAUBEN: No, it was just that during all this process. Somebody in research actually had this quote pasted on their door. I thought this is true. He was working on something entirely different. I said this is true no matter what. If we don't know where the baseline is we don't really know much of anything. That's true of your best estimate analysis. It was certainly true of the K heat. If you don't know what reality is how do you know whether something is conservative or what. So I thought this is such a good statement I thought I would put it up here.

MEMBER WALLIS: It wasn't discovered until August 9, 2001.

MR. LAUBEN: Well, it probably was discovered a lot longer ago than that. It's just that I found it in writing.

MR. SCHROCK: I guess what you've told us is that you have a best estimate. Is that right?

MR. LAUBEN: No. You know it's probably true that we never have a best estimate. We have things that are maybe more realistic than they were before and in this case I should say that part of the reason I put this up here was that since the last time we've talked, one of the people in our branch, Tony Ulses, was able to do some Origen calculations.

Until we had the Origen calculations, I really had no good idea of whether the standard was telling us anything that was close to reality or not. The ANS standard. This was some way to check the ANS standard as well as to check any calculations we may have done. Now this doesn't --

MR. SCHROCK: When you read the standard you find that it tells you that Origen calculations are one of the sources and in fact a major source of data upon which the standard was based.

MR. LAUBEN: Okay but we developed a spreadsheet to look at the '79, '71, and '94 standards we had some numbers that came out of it. We compared those to the numbers in the table in the standard. But that's just repeating what's already in the standard. This is just that I put the tables in correctly.

Also if we compared them to some of the examples that they gave in the standard, that's also a help too. But then the question was how close is this to something else. The something else was finally our ability compare the spreadsheet that was supposedly the spreadsheet to the standard with the Origen calculations. We'll show you those in a few minutes. It's nice to have something else to compare with what you've done. Partly I guess that's why I'm saying this.

It is proposed and this is what we are proposing to do. I'll start with this right away. It is proposed that the decay heat requirements in Appendix K and the best estimate guidance in Regulatory Guide 1.157 be replaced with requirements and guidance based on the 1994 ANS decay heat standard. I think that's no surprise. Steve has told you that's one of the things that we are doing.

In other words, as everybody knows, 50.46 has two options: the best estimate option for which guidance is in reg guide 1.157 and the conservative option which Appendix K which virtually everything is specified in Appendix K. To date there are no regulatory guides that describe anything that's in Appendix K. Appendix K is self-contained so far.

The Appendix K option in 50.46 currently requires fission product decay heat be modeled using the draft 1971 ANS standard with a multiplier of 1.2 and the assumption of infinite irradiation. A separate paragraph of Appendix K requires consideration of Actinide decay heat but it doesn't say that you have to use the Actinide equations for neptunium and uranium 239 which are in the '71 standard. They're also in the '79 standard. They're also in the '94 standard. It's almost identical there which is not surprising since you're talking about the same two isotopes.

MR. SCHROCK: Did you just say that it does not say that you must use those?

MR. LAUBEN: Appendix K does not say you must use what in the standard.

MR. SCHROCK: Oh, Appendix K doesn't.

MR. LAUBEN: Appendix K does not.

MR. SCHROCK: I thought you were referring to what the standard says.

MR. LAUBEN: No.

MR. SCHROCK: Okay. It's Appendix K.

MR. LAUBEN: Appendix K does not prescribe the standard when it comes to Actinide decay. It just says account for Actinide. That's what Appendix K says.

MR. SCHROCK: I think it says use the standard plus 20 percent.

MR. LAUBEN: It says use the standard plus 20 percent for fission product decay.

MR. SCHROCK: It doesn't' say for fission product.

MR. LAUBEN: Yes it does. If you look at the standard there are two separate subparagraphs. I'm going to get the standard. Okay. The scribe will read the bible now. But it says for fission product decay use the '71 standard for Actinides. Then the next paragraph says consider Actinide. It doesn't say how. It doesn't use the standard. Are we going to get --

MR. SCHROCK: I agree.

MR. LAUBEN: Okay. An alternative would permit the use of the 1994 ANS decay heat standard and that's the K heat standard, which involves more sophisticated uncertainty methods and a greater number of options left to the user. The '71 standard is very simple.

It is so simple that Appendix K is able to describe everything that you need to know in a few sentences in Appendix K which is not so with the '79 standard and the '94 standard. They also use more recent available data and methods.

MR. SCHROCK: But it's incorrect, Norm, to describe these as options. They are not options. They're statements of the reality of the physics and calling to attention things that the user must do and justify in order to apply the standard.

MR. LAUBEN: Okay.

MR. SCHROCK: These are requirements. These are not options.

MR. LAUBEN: Some of the things in the standard are left to the user. In fact we can look at one through five --

MR. SCHROCK: But there are things which the standard does not specify which must be included in the bottom line.

MR. LAUBEN: They must be considered --

MR. SCHROCK: That's the language of the standard. It says these things must be provided and justified by the user.

MR. LAUBEN: Let me see. If you will bear with me. You've said we've discussed several times and I thought today I would once more go through the standards and the '94 standard in particular. I want to make sure that I had correctly remembered those things which the standard says is left to the user or whatever. I could put that up there but they are the same things as --

For instance, number two is something that's left to the user but you have to obviously include it. There are no values specified for recoverable energy. G(t) it says here of wanting to do it but it also leaves it to the user to decide if they want to do it that way or some other way.

There is Actinide contribution which the standard gives the Actinide equations for neptunium and uranium 239. It also says that there are other Actinides which an improvement to the standard would include those additional Actinides.

MR. SCHROCK: I don't think that's the language used at all.

MR. LAUBEN: Okay. I got the language here.

MR. SCHROCK: What it says is that --

MR. LAUBEN: If you want to talk about the language, excuse me. Get me my book. Okay. I mean the language says "further revisions to the standard are planned to include contributions from Actinides not already included." That's what the language in the standard says.

Otherwise if you look in the standard about Actinides it just talks about the two that we've already mentioned. That's what the standard says. Do you think I've missed something?

MEMBER WALLIS: I don't understand what this bullet says.

MR. LAUBEN: All this bullet really is trying to identify is the things that the standard addresses.

MEMBER WALLIS: Does it come up with an agreed procedure for calculating or a recommended procedure for calculation or does it just say that these are the things you should consider?

MR. LAUBEN: No. In some cases, it's pretty explicit as to how you calculate it. In other cases it says it's up to the user to do something. If you will that's what I was trying to go through just now.

The standard method of the standard which describes fission product decay from four isotopes provides tables for that and it provides equations and it provides methods for calculating, fission product decay from those four isotopes without neutron absorption and that sort of things. But it also says you have address neutron absorption. Here are some equations that might help. But it's up to the user to come up with anything else that might be used to justified. If you want the exact working I can tell you what that is.

MEMBER WALLIS: You said the four isotopes. That's ^{235}U, ^{239}Pu, ^{238}U --

MR. LAUBEN: ^{238}U.

MEMBER WALLIS: Are the plutonium isotopes significantly different from the uranium?

MR. LAUBEN: If you look at the tables they're significantly enough different that it makes a difference.

MEMBER WALLIS: So burn-up makes quite a bit of difference to the K heat.

MR. LAUBEN: Burn-up makes quite a bit of difference. That's right. It's usually conservative to assume ^{235}U only. But that's right. There is a significant enough difference in them. That's why it was included and that's why a whole new standard was put together. Right, Virgil?

MEMBER WALLIS: It was also the

recoverable energy for fission varies depending upon the isotope mix.

MR. SCHROCK: (Inaudible.)

MR. LAUBEN: What's the matter?

MR. SCHROCK: Go ahead and make your case. Then I'll comment when you are finished.

MR. LAUBEN: Okay. Anyway, these are the five things that the standard either in some way very explicitly tells you how to consider it or in some cases it is not as explicit. Anyway these are the things.

MR. SCHROCK: You can look upon these things as being physical realities that are dependencies that the decay power has.

MR. LAUBEN: Yes.

MR. SCHROCK: And a best estimate evaluation will require that these things be taken into account. The standard was devised to provide a best estimate methodology not an Appendix K methodology.

MR. LAUBEN: You're saying that I should -- No, let me just continue. The performance based realistic option in 50.46 would allow use of the 1994 standard today. What I'm saying is that the best estimate in 50.46 is performance based.

I'm saying then that this specification of 1994 standard as an acceptable method in Reg. Guide 1.157 would facilitate its use. In other words, right now Reg. Guide 1.157 which describes acceptable ways to do the best estimate calculations does not specify the 1994 standard. It specifies that the 1979 is an acceptable one.

So it would make sense to update Reg. Guide 1.157 to describe a more modern standard that would acceptable for use. In addition to that it makes sense in that Regulatory Guide to specify things a little bit more clearly than it does with respect to some of the things that are in the Reg. Guide regarding several things.

MEMBER SIEBER: Is the 20 percent margin Ader (PH) still going to be there?

MR. LAUBEN: That has nothing to do with the best estimate option. Twenty percent Ader (PH) is not there. The uncertainties that are provided for the four isotopes in the '94 standard are one sigma uncertainties. Those uncertainties plus any other uncertainties would be assessed in terms of the entire uncertainty of the analysis when you are doing it. That's similar to what holders of the best estimate evaluation models do today. The 20 percent is an

Appendix K requirement only, not the best estimate option.

MEMBER SIEBER: For the purposes of providing margin, right?

MR. LAUBEN: No, originally the 20 percent was something that was to look at the uncertainty in the K heat only and, Virgil, you can correct me of course.

MEMBER SIEBER: Right.

MR. LAUBEN: Then as time went on it was discovered that this was much more than what was needed. People realized that it could be thought of covering other uncertainties as well.

MEMBER SIEBER: Okay.

MR. LAUBEN: But originally and in fact I think there is a curve in the '79 standard that shows the uncertainty in the '79 standard. It compares it to the 1.2 --

MEMBER SIEBER: Yes, it does.

MR. LAUBEN: -- and the '71 standard as well.

MEMBER SIEBER: And the 20 percent is outside of the uncertainty base.

MR. LAUBEN: Certainly right. That's correct.

MR. SCHROCK: But it's conservative.

MR. LAUBEN: Yes.

MR. SCHROCK: The '71, '73 standard had error bars that were placed there simply by eye balling all available data against the selected mean curve and also time dependent. At some time interval the negative uncertainty was much larger than the positive uncertainty and so forth.

In the earliest time, the uncertainty was 20 percent first thousand seconds. After that it changed to a smaller number. Then it changed again at longer times. In writing Appendix K it was selected from the early time because I think of the loss of coolant accident application.

So the 20 percent exists in the rule without any reflection of larger detail which was in that standard. That standard also provided a means of assessing the role of finite operation as opposed to infinitely long operation. Again that provision of the standard was not incorporated into Appendix K. It was implicitly or essentially ignored. So there are differences between Appendix K and that standard. It's not a direct comparison.

But the standard itself had an uncertainty which had no statistical meaning whatsoever. It was based on a very minimum amount of information. As I've said it was simply looking at a graph that had all available data shown and those uncertainty included everything which was then available which was quite inadequate.

MEMBER SIEBER: Right. Thank you.

MR. LAUBEN: Okay. What I'm showing here is a table of information about nine different calculations that are going to shown in the next several graphs having to do with the different ways of calculating in decay heat. I grouped them into three groups.

The first calculation, case has only one member of its group and that's the current Appendix K. It tells you what model it is, ANS73, 1.2 multiplier, infinite operating time, 100 percent U^{235} which is the assumption in '71, '73 standard. So these other things, capture time, Ψ and fission energy are not applicable because they are not written in those terms. Actinide yield, the 0.7 and isotope tables, etc. are not applicable because of the way that the standard is written.

The next four cases are Appendix K proposals that would look at the '94 standard. The first one which is case number two looks at 2σ for the individual isotope uncertainties. It uses the additive technique for uncertainty that is provided in the standard.

Number three is the same thing but instead of using the additive technique for uncertainties it's in the standard that uses the root mean squared technique. 3a and why did I use 3a instead of another number was because 3a was sort of the last thing we did but I wanted to show it was closest to case three. That just has 2σ. I didn't say what kind because in all the instances assuming 100 percent U^{235} similar to what's done in the '71 --

MR. SCHROCK: That is done because plutonium produces less K heat as a fraction of its fissive heat.

MR. LAUBEN: Yes.

MR. SCHROCK: So you are being conservative. But if you actually looked at the average over the life of the plan with taking real burn-up you'd actually have less.

MR. LAUBEN: You will see in the next set of curves this stuff doesn't make a lot of difference.

MR. SCHROCK: But there is less to K heat from a plutonium fuel.

MR. LAUBEN: Yes.

MR. SCHROCK: So you still have the conservatism in here by assuming this 100 percent uranium.

MR. LAUBEN: Right. That is correct. When you look at the next figure you will see. Case four is not adding any uncertainty associated with it but maintaining the choices that are shown in the rest of table there. That takes care of that group.

The next group is what I would call best estimate calculations. Case five is Origen calculation for 17 X 17 PWR assembly. Case eight is an Origen calculation for a BWR 10 X 10 assembly. I think that's quite a span of different things. I mean one has boron in it and one has veritable poisons of different sorts. They are really very different fuel assemblies and yet you will see that the decay heats that are calculated there with each one are pretty similar.

Also then using the Origen -- Now in the third column operating time in cases five and six they use the same operating time as what was done in the Origen calculation. The cycle average values for the fission fractions. In other words Origen will calculate a continuous fission fraction change of the various isotopes as a function of time. If break it up into cycles and take the average value for each cycle, that's what I did for the ANS94 calculations.

MR. SCHROCK: For the Origen calculations, Norm, did you just input a constant power?

MR. LAUBEN: No. The Origen calculations are for three cycles. Cycle shut-down, back up to full power --

MR. SCHROCK: That's what you input for the power.

MR. LAUBEN: Yes. You input a power history. That's correct. But between shut-downs it's a constant power. It doesn't use --

MR. SCHROCK: But you allow some shut-down time and let things decay away and change.

MR. LAUBEN: I don't have the charts with me but I think it was like a 30 day shut-down or something like that between cycles. It tries to be not untypical of a real reactor.

MEMBER SIEBER: And the power is put in as watts per gram.

MR. LAUBEN: The power is put in as -- I don't think it was an average assembly. Unfortunately Tony is not here but there is a burn-up and there is a power density.

MEMBER SIEBER: Power density. Right.

MR. LAUBEN: In the decay heat calculation in ANS94, you don't have to put that in.

CHAIRMAN SHACK: Norm, can you try to finish up by 3:45 p.m.?

MR. LAUBEN: I can try. If you don't mind let's just go to the next slide then which is a plot of nine calculations. I think the point is that they group together very closely. ANS plus 20 and '71 standard is way up there by itself.

Two, 3, 3a and 4 which are the various proposals which conservative choices that the user might make to bound his operating conditions before he knows what to do. You know the reactor operating -- is going to look like or doesn't want to argue with the NRC about what things are. He could use those choices and the other choices that are shown in that --

MEMBER WALLIS: I don't understand why 4 is so high. Four is without uncertainty and it's not --

MR. LAUBEN: No, four is below.

MEMBER WALLIS: But it's up with two 3's and the 2. The two 3's are with the -sigma so why is 4 up with them not down with the 5, 6, 7, 8?

MR. LAUBEN: No, 4 is lower.

MEMBER WALLIS: Why is it not down with 5, 6, 7, 8?

MR. LAUBEN: Because the uncertainty doesn't make that much difference. If you make those choices the uncertainty in the values in standards doesn't make that much difference.

MEMBER WALLIS: So it's the Origen that makes the difference.

PARTICIPANT: It must be the infinite operating time before.

MEMBER SIEBER: There's something to that.

MR. LAUBEN: I'll tell you what. I have another set of slides but I'll never finish by 3:45 p.m. if I show you those. On the other set of slides, I looked at the individual bases for these things and I could put up those slides or I could just provide them to you.

CHAIRMAN SHACK: Provide them.

MR. LAUBEN: Provide them and you will see. But infinite operating time doesn't make that much difference. I'll provide you the slides.

MEMBER WALLIS: What is the difference between Origen and the others? Origen is down below all the others. What is the reason for that? That's all I'm trying to get at.

MR. LAUBEN: Origen or ANS plus 20 with Origen input. These are all ANS plus 20 with Origen input too. This is saying that the standard in Origen not surprisingly, right, Virgil, are going to look the same if they have similar? It's just that the input values if at operating time was one of them but if you want maybe I can dig out that slide.

CHAIRMAN SHACK: Not in six minutes.

MR. LAUBEN: Not in six minutes. Okay. I'll provide it.

MEMBER BONACA: Looking at this it seems that ANS71 without multiplier is very close to number 4, to the ANS94 model without uncertainty. You don't have the ANS71 by itself.

PARTICIPANT: He wants to take the 1.2 off.

MEMBER BONACA: If I take the 1.2 off.

MR. LAUBEN: Okay.

MEMBER BONACA: I can draw on the top of the --

MR. LAUBEN: I think if I show you the next slide. We'll forget the next slide in fact and go to the next one after that. Here is where you will see I hope, Mario, what you were asking about.

What we are doing here is dividing the

different values on a curve by ANS71. So ANS71 with a 1.0 is this straight line here. ANS with 1.2 like we showed before is way up there by itself. Here are the various Case 2, Case 3, Case 3a, Case 4 divided by ANS71. They group in there together. Here are all the best estimate ones here with Origen or ANS94 with Origen.

The thing that was somewhat troubling about this curve by the way, and this is the segue of the next set of curves, is the fact that here is ANS 94 with Origen input and it's lower. Eight and five are the Origen divided by ANS71. So for what reason is the Origen higher than the ANS94. Over here they are pretty close together. Now they seem to diverge --

MEMBER LEITCH: Why are there so many giggles? Is it because of all the isotopes behaving differently? I would have expected a smoother curve.

MR. SCHROCK: Are you using in your origin calculation the same N diff data that were used in generating the values in the 94 standard?

MR. LAUBEN: I think so.

MR. SCHROCK: You could check that. That could be what --

MR. LAUBEN: But I'll tell you as you can see from the report the attachment one in the report that I gave you when we asked Oakridge about this, they said the reason that ANS94 doesn't account for all the Actinides as you go out in time.

MR. SCHROCK: This is where you have difficulty in understanding the language of the standard. If the standard calculation was done correctly, you would be including the other Actinides and justifying how you got them.

MR. LAUBEN: That's right. I think it's a great idea by the way. In fact, I would do that except I don't think I need to do that for this Appendix K stuff that I'm talking about now. The reason is because with all these choices that I have here whichever one of these terms I want to choose I'm still well above the Origen calculations. So why add something else on to it for now?

The best way like you say is to really account for all the Actinides in the best way that you can. That is true. That's really what my next set of slides is all about. I probably don't want to do them --

CHAIRMAN SHACK: One minute.

MR. LAUBEN: -- because I understand in one minute I can't do that. Let me just say that this curve was provided to us by Oakridge. There's a bounding line that I put which shows the contribution from ours according to what Oakridge says. This is the contribution of the other Actinides other than U^{279} and Np^{279} which are already accounted for in a standard.

This is the contribution percentage of Actinide components that are not already taken account of. As you can see it grows as a function of time until out here 10^9 seconds which we certainly don't care about. These other Actinides are 80 plus percent of the entire total. For our purposes we're really down here at somewhere between 50 and 10^4 seconds for -- analysis.

MEMBER ROSEN: Let me ask a question and make sure I get the message.

MR. LAUBEN: Yes.

MEMBER ROSEN: Go back one slide to this one. (Indicating.)

MR. LAUBEN: Okay.

MEMBER ROSEN: Right. Slide 27. The message you started with was use the ANS94.

MR. LAUBEN: Right.

MEMBER ROSEN: And the reason you did that is because it's less than 1.2 X ANS71 but it's not all the way down at the Origen.

MR. LAUBEN: No.

MEMBER ROSEN: On the four cases you showed there are about not very much different.

MR. LAUBEN: Right.

MEMBER ROSEN: So it's part way to the right answer let's say. A step in the right direction kind of thing. It makes sense and it's still conservative.

MR. LAUBEN: It's still conservative and it's --

MEMBER KRESS: But it's conservative only because you used a 2-Sigma there.

MEMBER ROSEN: Right.

MR. LAUBEN: No. Because four has no sigma.

MEMBER KRESS: Oh, that was the mean.

MR. LAUBEN: Right.

(Discussion.)

MEMBER ROSENTHAL: Get rid of the 1.2. Go to some reasonable compromise. Specify some of the parameters to keep life simple. Reduce the unnecessary conservatism. You know that you still have left yourself some margin. It's a reasonable compromise.

MR. LAUBEN: Can you see this? At one time we were looking at what I'll call the simplified technique in the '79 standard which was like the 1979 times a factor of 1.1 very close to that would be. That's going to be about half way between here and there. (Indicating.) So the '94 standard whether you have 2-Sigma or not is still getting you more advantage than you had with the '79 standard which is not unexpected. You have better data, better methods.

MEMBER ROSEN: Well, what you're proposing here is instead of using the standard that is 31 years old, use a standard that is only eight years old.

MR. LAUBEN: Right. If you make some choices I don't even careful you account for all the Actinides if you make certain conservative choices between here and here. (Indicating.)

CHAIRMAN SHACK: Okay. I think we're going to have to stop here so we give Steve a shot at it for the rest of the presentation.

MR. LAUBEN: Okay. I guess this one is enough. You probably don't need that other chart unless somebody wants it.

MEMBER ROSEN: That's as far as I'm going to need.

MEMBER CRONENBERG: Just let me point out.

They only received this package from Farouk this morning. I only got it Tuesday.

MR. LAUBEN: That's right.

MEMBER CRONENBERG: All your stuff is in a package that I gave them this morning.

MR. LAUBEN: All the stuff is in a package. I don't have to give you that because they are all in that package.

MEMBER CRONENBERG: Dated the 23rd which was given to me the 25th. So it wasn't mailed out.

MR. LAUBEN: But you got this.

MEMBER CRONENBERG: It's all in here.

MEMBER WALLIS: This looks like a case where you are ready to make a recommendation based on some good information. The question is just where you should draw the line for regulatory purposes. It's very straight forward.

MEMBER ROSEN: 3A is the one I like.

CHAIRMAN SHACK: There's no question in their mind where to draw the line. They've told us. 94 standard.

MEMBER ROSEN: Well, is it 94 or is it the 2 Sigma?

CHAIRMAN SHACK: We can read the recommendation.

MR. BAJOREK: Okay. What I'd like to do then is pick it up with Appendix K and the other model revisions, decay heat being the most important one. We've done that one. Three others have been suggested for revision as part of the SECY 1133. These were replacing Baker-Just with another correlation Cathcart-Pawel, eliminating the requirement for steam cooling only below one inch per second and perhaps leading the requirement not to allow the return to nucleate boiling during blow-down.

Looking at the acceptance criteria, we've already gone through and looked at the correlations. Our conclusion in taking a look at alloys and newer experimental information is coming out of Argonne is that Cathcart-Pawel does a much better job than Baker-Just especially in this range near 2200 degrees Fahrenheit. So our recommendation is going to be for a revised optional Appendix K that Baker-Just be replaced with Cathcart-Pawel and it not be restricted to any particular alloy.

There is a caveat though that we need to be concerned about. This figure showed up earlier in your package. It's in smaller form on page 34. But what it shows is a pressure dependence in the zirc oxide growth. The black solid line here is Baker-

Just. Now experimental data by Pawel and presumably his correlation which matches his data very well was done primarily at low pressure. This information is down here. (Indicating.)

If you follow the experimental data as you go up higher in pressure typically it would increase. If it is used at high pressures, typical of small breaks, 600 to 800 p.s.i., and you have to look very carefully at the data, that's 40 to 75-bar in the units here, you do see that the experimental data starts to creep back closer to Baker-Just. So in a risk informed Appendix K you would Cathcart-Pawel is acceptable at low pressure. If it is used at high pressure, some type of a correction would need to be applied in order to insure that it does not become non-conservative.

MEMBER ROSEN: What do you mean "if it is used"? It's going to be used for any accident that hangs up in high pressure. Right? Where you have damage at high pressure?

MR. BAJOREK: Well, this would be up to the stakeholder to revise his evaluation model --

MEMBER ROSEN: I'm saying if it is used. But all stakeholders have to analyze those kinds of breaks.

MR. BAJOREK: They all have to analyze those.

MEMBER ROSEN: So I don't know why you are being so permissive. I would have expected you to say something like for accidents that hang up at high pressure where core damage occurs at high pressure you have to understand and use this data.

MR. BAJOREK: Okay, that might be a better way of phrasing it.

MEMBER ROSEN: Unless you can rule those accidents out in your plant. It's a plant specific situation but I don't think you can.

MEMBER SIEBER: No, because you can get a bubble. Have high pressure but no cooling.

MR. BAJOREK: It may be with the amount of relaxation you get with decay heat you may not care about replacement. You can adjust with Cathcart-Pawel. So the option would be there to stay with Baker-Just for a small break calculation.

MEMBER ROSEN: Yes, I think some places may just replace the decay heat term and not this.

MR. BAJOREK: If you are limited for large break at low pressure then it's a fairly simple change then.

MEMBER WALLIS: Let me ask you. This

pressure dependence you said that what's happening here is that of a defusion limited reaction. Is the pressure dependence that is seen here modeled by taking into account the effective pressure on density and defusivity and so on?

MR. BAJOREK: No, Cathcart-Pawel does not account for that.

MEMBER WALLIS: It's the intelligible thing to do only if you have the mechanism which says it's a diffusion limited reaction. Then you ought to be able to figure out is that reflected by this trend with pressure or not. If it's not then change your idea about it being a diffusion limited reaction.

MR. SCOTT: Steve, could I interrupt? This is Harold Scott. Some of the literature suggests that maybe it's the formation the way this oxide layer forms and whether it cracks or not at higher pressure. So it's still a diffusion but if the layer's really not as thick as you think it is because it's cracked, it's easier to diffuse and therefore the same phenomenon doesn't occur.

I'd also mention that this chart shows 900 C and we've been talking 1000, 1100, 1200. The pressure effect doesn't appear at higher temperatures.

MEMBER WALLIS: So it's not so simple.

MR. SCOTT: It's not so simple. Right.

MR. BANERJEE: Is the oxidation limits still going to be left at 17 percent?

MR. BAJOREK: That would be in the acceptance criteria.

MR. BANERJEE: But if you are changing this are you going to change that to 13 percent or whatever?

MR. BAJOREK: Not necessarily, no.

MR. BANERJEE: Because you told us that the oxidation limit was being set by the embrittlement experiments which were based on Baker-Just. So if you get rid of Baker-Just and replace it then you should change the oxidation limit as well.

MR. BAJOREK: That was the use of Baker-Just in the original --

MR. BANERJEE: To calculate the limit. Right?

MR. BAJOREK: To calculate the limit back in '73.

MR. BANERJEE: Right. But you are changing that now.

MR. BAJOREK: In '80 they went to measurements of that zirc oxide thickness. They got away from the use of Baker-Just --

MR. BANERJEE: And did they find 13 percent or what?

MR. BAJOREK: No, they found 17 percent. They were able to justify 17 percent using the tests of argon which went to more of a toughness --

MR. BANERJEE: I'm puzzled now. You told us that the experiments that were done found 17 percent on the basis of the Baker-Just. Correct me if I'm wrong.

MR. BAJOREK: Say it again so I'm sure.

MR. BANERJEE: Okay. You showed us some experiments and you said that in the first experiments --

MR. BAJOREK: The ring compression tests.

MR. BANERJEE: Yes. You said that they calculated that 17 percent in that temperature range based on the Baker-Just correlation.

MR. BAJOREK: Right. When they did those tests they calculated the oxidation.

MR. BANERJEE: There is something inconsistent which I don't understand. I'm just asking for clarification.

MEMBER ROSENTHAL: Can I try? I believe and of course we are back on the criteria. Let me just say that at least my mental model is that we will get rid of the 17 percent. We will get rid of the 2200. We will go to some material property because our vision is and I know it's fuzzy that you will have a free standing core. You wouldn't want a debris bed. You'd want it still standing which is some material property. We would just plain get rid of the 2200 and 17 percent.

Having said that, when the original work was done we didn't know about hydrogen embrittlement. In about 1980 we knew about heightened embrittlement which made things worse. The Japanese adopted a slightly different standard. In the U.S. it was Gunam (PH) He Chung and company at Argonne who went ahead and looked and said okay if I account for hydrogen embrittlement and if I do a impact covenance test will I have integrity of this.

They concluded that although the 17 percent, 2200 might not be quite right it's okay. It insured safety even though we now knew about the heightened embrittlement that we didn't know at the original time. So the story just gets more and more complex the deeper and deeper you look.

MR. BANERJEE: I get more and more confused at that moment.

MR. BAJOREK: Wait a second. Now if we

were to go to this that would be one way of specifying material but still have to calculate what the cladding does and whether the core temperatures in that when you are doing an evaluation model and you need a correlation now to predict that energy generation due to the metal-water reaction.

We would recommend then going to Cathcart-Pawel. What it's calculating with respect to oxidation may not matter anymore because it may be other criteria that are used to gauge whether that clad survives to the quench.

MR. BANERJEE: If you change everything consistently that's fine. But if you change one thing and leave the other then it's not consistent. So if you use Cathcart-Pawel then you should change the oxidation criteria.

It seems to me that if you are basing it on those tests then they would have to change because you just said that Baker-Just was used to calculate the amount of oxidation in the first set of tests where they were hitting it with a hammer. I don't remember where it was but something or other they were hitting it with.

So to be consistent you must change everything consistently or you completely disassociate the oxidation calculation from Cathcart-Pawel. It has nothing to do with the oxidation calculation then.

MR. BAJOREK: Steam cooling below 1 inch per second is one of the other. During refill and reflood right now in Appendix K if that flooding rate drops below one inch per second, you need to ignore the entrainment, any droplet interaction, and have to go to a convective cooling only type of correlation.

MEMBER ROSEN: Could you show us physically for those of us who don't understand the whole history?

MR. BAJOREK: I'm sorry.

MEMBER ROSEN: Why do you have to just neglect it if you're filling this thing up less than one inch per second? Is that what it is? You can't take credit for steam cooling if you are reflooding at one inch per second.

MR. BAJOREK: Let's just think of the physics for a second.

MEMBER ROSEN: Maybe I have it wrong.

MR. BAJOREK: You have the core sitting there very hot. When water hits the bottom or very close to the bottom of the rods there is a lot of energy released, quenching. The vapor generation and vapor velocities are sufficient to entrain droplets and bring those up through the core. Those droplets can strike the clad. You can get radiation to the droplets. You have other mechanisms for cooling that are available with those droplets in your flow field.

In Appendix K when they envision this they thought when you get down to a very low flooding rate you may not have the steam velocity sufficient to entrain those droplets. So ignore their effects and assume that your heat transfer is solely by convection from the wall to the steam that is flowing through the hot assembly.

MEMBER ROSEN: That's obviously why that's true, why you wouldn't still have the velocity but that's just history.

MR. BAJOREK: That was the assumption that was made.

MEMBER ROSEN: Dr. Wallis could tell me right away but he's gone.

MR. BANERJEE: The velocity has to be roughly 1,000 inches per second if the steam is an inch per second.

MR. BAJOREK: Our experimental tests have universally shown that you almost always get droplets entrained even from very low flooding rates. This figure which is not in your package shows the carryover fraction meaning how much of that liquid that's brought into the bottom of the bundle is entrained and carried all the way through up to the upper plenum of that test facility.

There are various flooding rates here. The two lowest curves here are an inch per second and eight-tenths of a inch per second respectively. (Indicating.) But notice once you get out into the transient even those very low flooding rates are entraining better than half of the fluid that is coming in at the bottom of the bundle so restricting the calculation artificially to convective cooling only just because it's an inch per second doesn't make sense looking at the data.

Now the only time you do have a period where you would say that it's steam cooling only is this part very, very early when your quench front, your water, is moving over those parts of the rods which are so cold they can't vaporize enough of the water. But that's very short and if we look at other tests at even lower flooding rates, you see the same effect.

We see it in some more modern tests. We were up at Penn State for the rod bundle heat transfer tests watching some of those and with better instrumentation now than what these were run with, laser camera and much faster optics. It's obvious that as soon as the water hits that bundle that there's a very high fraction. In fact, it's even greater in those tests than what were in Stein and Flecht. So our recommendation is that this steam cooling requirement is invalidated by the data we've seen. There's is really no sense in keeping it.

MEMBER KRESS: The carryover amount is the full story because if the effectiveness of the droplets depend on their size.

MR. BAJOREK: That's right.

MEMBER KRESS: Which will depend on this velocity to some extent.

MR. BAJOREK: Yes.

MEMBER KRESS: And I don't see that reflected in what you say.

MR. BAJOREK: We see this as a way of not having to require the steam cooling requirement. Now modeling the process will take more work.

MEMBER KRESS: So you would have to maybe consider the size of these droplets when you do the modeling.

MR. BAJOREK: In your evaluation now are you predicting entrainment correctly? Are you predicting the right droplet size? Is there droplet impaction and interaction with the rods? You would be allowed to do those but you would have to use experimental data to justify those models that you would want to have.

MEMBER ROSEN: Is this a big thing now? How big a deal? Is this a big positive --

MR. BAJOREK: It depends on the transient length. Unfortunately we do not have any good numbers on what this is. I would venture that it's 100 or 200 degrees F over a course of a large break transient. I'm going to show you some numbers for some of these effects in a second.

MR. SCHROCK: I have one comment related to that. Maybe Professor Schrock of Energy might want to comment on that too. I think a lot of the reason for the steam cooling only was that was a regime where the heat transfer coefficient is not very well known. As a result of that it was a conservative assumption to assume steam only. Now I don't believe that the heat transfer coefficient is much better characterized today in the post CHF regime. So I'd say how are you going to take this into account?

MR. BAJOREK: This is where I start looking at best estimate models as really having a distinct advantage. Because to answer that question, we would have to do simulations against Flecht other experimental data, characterize the performance of the code with an uncertainty and then use that uncertainty in your evaluation model. That's the way I was involved in having done it in the past and was approved. How this would get incorporated into an Appendix K model is something that still has to be determined. We're not making a recommendation that this is necessarily easy.

MR. SCHROCK: And a lot would say it's not easy from what I know.

MR. BAJOREK: I would agree.

MR. SCHROCK: They still can't predict the progress of a reflood front because of the inability to predict this precursory cooling that occurs above the front. So you would be involved in significant uncertainty there I think.

MR. BAJOREK: I agree.

MR. SCHROCK: Sanjoy, are you going to comment on that?

MR. BANERJEE: Well, one of the problems also with LOFT was that there were external thermocouples that were preferentially -- which is why you can't take any credit for rewetting after the dryout has occurred.

MR. BAJOREK: Right.

MR. BANERJEE: The same problem would occur here. It would be very difficult to justify credit for droplet heat transfer I would think.

MR. BAJOREK: Yes, so it doesn't necessarily make easier.

MR. SCHROCK: But it would take it out of the realm of a prescriptive requirement and put it in the realm where engineers could do the best experiments, the best analysis and make their case.

MR. BANERJEE: The problem is the case is never clear so they can lash together always a case which convinces some people. I remember we spent years over LOFT because some people would maintain that yes this was a real effect and some people would say that no it isn't.

It is the external thermocouples. So the conservative approach in that case was to say no. You don't get a secondary rewetting. Probably here it's conservative to say you just get steam cooling. It will close a can of worms. Now you can open one. It will take you a long time to settle it.

MEMBER ROSEN: Now wait a minute. If I think what Jack is saying that if licensee meaning the fuel vendor wants to do experiments or whatever --

MEMBER ROSENTHAL: If he thinks the value of this is high enough that he would like to set up a loop and do some good experiments with modern instrumentation and data acquisition and bring that in and show it to the staff he might actually advance the science. Then if you were convinced.

So it would become a commercial advantage for some vendor perhaps to do this sort of thing. I think it seems to me a good thing to do to at least set up a playing field in which vendors might be tempted to do that.

MR. BAJOREK: That's basically the approach that was adopted for the best estimate rule. This was eliminated. You didn't have to assume steam cooling but taking advantage of it meant many simulations, characterization of the code, models and correlations in ways that was very difficult to get approval.

MEMBER ROSEN: So if a licensee has some kind of problem with ECCS in some future time he can go to his vendor and say I need some more help. He can say well if you want to pay for this or join me in paying for these tests it's possible that we can show etc. As long as the regulations allow the showing to be made, you have provided some flexibility to the industry to move ahead.

One of the things that I've been arguing before with Peter, we were thinking about research. I've been arguing that we in the ACRS need to encourage things that looks a little bit more to the future. We're not always getting there. The day we get there we have to say something like we can't do that because we don't have the research. Instead of that we do the research and maybe that enables some things. Here's a case of that.

MEMBER KRESS: Could you just spend one minute on the rod bundle heat transfer experiments that we are now doing?

MR. BAJOREK: Yes, the figure I showed you earlier was from the Flecht series of experiments. It was done in the mid '70s to the early '80s. This took a look at a full height bundle up to 161 rods, a fairly large bundle well instrumented with thermocouples and DP cells. Essentially it's the basis right now for developing your models for heat transfer for any of these evaluation models. There are other tests but these are the ones which are principally referenced.

Well, there were some shortcomings in

Flecht. The DP cells were very far apart which made it very difficult to determine what was the void fraction. They had windows on there and the only way you could an idea of the droplet size was to take some rapid movies which were good for only a few seconds. You let a technician go in there and count the droplets and measure them. I knew the guy who did that. He quit.

You had very limited ways of getting the information from Flecht but it was very useful and demonstrated the conservative in many of the Appendix K models that we are looking at now. It was realized that in order to get better models, best estimate, more realistic models for droplet breakup, grid effects and heat transfer, film boiling and things, we needed tests with better instrumentation.

Several years ago a bundle was constructed at Penn State that was making use of much more detailed instrumentation, more thermal couples, more DP cells. They have several windows and a laser camera that we saw a couple of weeks ago. We're still three feet off the ground because when they ran a test with the visual cameras we saw the entrainment.

With the laser camera, they had essentially a real time measurement of the droplet size and distribution above and below the grids. The grid by the way were again demonstrated to have an enormous effect by breaking up the droplets, stripping away the boundary layer causing it to be reestablished and you see the rods red hot, a grid and cold. It goes up the bundle that way.

PARTICIPANT: It gets hot again.

MR. BAJOREK: Hot again and cold. Hot again and cold.

PARTICIPANT: Above the grid.

MEMBER KRESS: The major effect is the heat transfer between the steam and the droplets. They lag. They have a higher MCCP (PH) and that this steam gets heated from rods and passes that heat on to the droplets and the whole thing just cools down.

MR. BAJOREK: There are several effects.

MEMBER KRESS: Yes, there's radiation and then there's droplet impingement. That may be calculations. It was mostly the droplets and the steam interaction.

MR. BAJOREK: Yes, and you saw in the --

MEMBER KRESS: That's a strong function of the droplet size. When you go by those grids and break it up it really makes a big difference.

MR. LAUBEN: Break up the properties.

MEMBER KRESS: And grids just breaking up in really small droplets.

CHAIRMAN SHACK: We're running out of time, gentlemen.

MR. BAJOREK: Okay. Anyway we're getting more information that adds to the support of these conclusions especially for reflood. Let's move on. I think I just heard Dr. Banerjee also read my next overhead here.

When it comes to allowing rewet during blowdown, here we don't feel there's a real strong case not that we don't think it will occur. But the tests that have been run like LOFT with the external thermocouples, semi-scale which had some questions on its scaling, other tests which have been run with Ink and L (PH) as the cladding as opposed to zircaloy and knowing that there's a major material effect and minimum film boiling leads us to the recommendation that leave this one go and pursue the other first. There may be better information to change this or to do it under a best estimate context. But doing it right now under Appendix K we think would be wasting people's time.

The final thing that I want to go over is with what we call the Appendix K Non-Conservatisms.

First let me define what that is. We see three different ways of something being non-conservative in Appendix K. The first and what we focused on are those physical processes and phenomena that have been identified through experimental programs since 1973. If they didn't know about them, they couldn't put them in the rule. They didn't know enough about them in 1988 so they couldn't have been captured in a rule change then.

In addition we've known for quite some time that these codes have very large calculational uncertainties. We recognize that. It hasn't gone away. We realize that if we take margin out for whatever reason we have to account for the accuracy and uncertainty of the code in addition to these new processes.

Now the processes that we've identified over the last few months which are strong candidates that need to be corrected are downcomer boiling, reflood ECC bypass and fuel relocation. Let me just take a couple of minutes on each one to characterize what they are and I'll show some effects of all of these.

Downcomer boiling, you can read that in the interest of time. Typically you assume that after reflood the accumulators and your low head pumps come on and your downcomer fills. New experimental data from CCTF and UPTF however show that after some period of time I call it about 200 seconds enough energy comes out of the vessel wall in the lower internals to start subcooled and saturated boiling in parts of the downcomer.

As the downcomer froths up part of that liquid is pushed off into the break, boiling continues and the net result is a downcomer that is partially voided. This results in a driving head that is much smaller or can be much smaller than what it would be if you did the typical Appendix K assumption that your downcomer is full and you ignore boiling. I'll show some effects on that in a second. Let me just go through the other ones we've identified.

Very closely related to that is downcomer bypass during the reflood period. This is observed in some of the UPTF tests and to a smaller extent in CCTF. If my downcomer is pretty close to being full, sufficiently high steam velocity coming from the intact loops could entrain part of that liquid, carry it off and throw it out the break. Like downcomer boiling this depletes the driving head and reflood. My reflood rate is slower. This is a non-conservatism if you don't at least account for it.

The other one has been around for several years. We hope to get better information in some of the newer tests that are being devised right now. They are going to be running some tests with better instrumentation on the nuclear rods to try to get at fuel relocation which has been observed in tests in Germany, France and the U.S.

When we get this ballooning that occurs in the rod, it's possible that these fragmented pellets due to the vibrations can migrate down into the burst and rupture zone. The typical assumption in Appendix K is that these pellets remain as a concentric stack.

Now I was talking to Dr. Ford who said why is this cladding temperature going down after it swells. It's good because you've swollen the cladding away from its heat source. If you are at low temperatures and zirc-water doesn't make any difference, this is a fin.

It's not a fin if you consider fuel relocation. It becomes much worse if there is a rupture involved and you have zirc-water reaction because now you've relocated the pellets, your local power is increased, you have very good communication now between the pellet fragments and the cladding itself. I have lost that fin effect. You see varying estimates on this. But we are identifying this as something that needs to be accounted for in future models.

We've thrown a lot of different processes and changes at everyone in looking at change to K heat, change to the zirc-water reaction, look at downcomer boiling and what not. What we've tried to do is to go through documented literature, information we see in journals, information that has been submitted to the staff, other information that is publicly available to try to gauge what is it we are giving away. If we say you have to account for these non-conservatism, what's that throw back at you?

In the tables that follow you can see some of these numbers. Decay heat for a large break and I've broken this into a large break and small break table. For large break, typically you see something like 400 degrees as being the benefit by going from ANS71 plus 20 percent down to something realistic. Most of this is with '79. With '94 you would expect that to increase but rule of thumb may be 400 degrees.

Changing from Baker-Just to Cathcart-Pawel provided you keep your core temperatures high, that change is something on the order of 50 degrees. It's worth zero if your peak cladding temperatures are low. So again you would have to have a power increase to get you back up to where that benefit would be important.

Now a bit surprising has been the estimates that we've seen publicly and submitted to us for downcomer boiling. If we take a look at a WattsBar FSAR that's been submitted and we look the peak cladding temperature that you would get before downcomer that you would get before downcomer boiling occurs and a second peak that occurs later in time after downcomer boiling, we're seeing an increase in the PCT 400 degrees.

We've done some other calculations or I should say one of our contractors has done it using RELAP for system 80 plus unit uprated. This exaggerates the effect because the transients are so much longer when you go to an uprated condition. If you look at that, they are looking at 800 degrees. I think that's a problem with the code in that the interfacial drag for that part of the downcomer is too high. I think that's exaggerated.

I do another code for CE plant at much lower power shorter transient, I see a smaller number. If I had to take a pick of these I would probably look at something like this 400 degree as more of an estimate of what this effect is for a unit that has a long reflood and you have a chance for the downcomer to boil.

Short transients you don't see it. The reason you didn't see this before is because with best estimate now people are uprating. Transients are getting longer and you're allowing that 200 to 250 seconds to pass by so that your downcomer can boil.

Other estimates on the table for fuel relocation originally had been estimated at 40 some degrees. But the French have some recent work looking at different filling fractions. Their estimate is higher, 300 degrees.

I've also listed some estimates on code uncertainty. In 1986 when they refused to change decay heat because of large uncertainty they didn't know what they were. At least now we can go through and look at some of the best estimate codes that have been used, WCOBRA/TRAC with Westinghouse, SEMENS has a model, GE has a semi-best estimate approach. Look at a 95 percentile compared to a 50 percentile BCT and we see numbers that are typically on the order of 300 or so degrees between what a realistic 50/50 temperature would be and what happens if you have to account for heat transfer uncertainty. I think that's where a lot of that is really coming from in these models.

Small break is much harder to define because it hasn't been the leading accident for very many plants. We get very large estimates on what the benefit would be in going from '71 to a more realistic, anywhere from 500 to 1000. Metal-water reaction there are a couple of estimates which are very similar to what we see for the large break, less than 100 degrees.

No one has produced a best estimate small break model so we can't assess the uncertainties. But numbers that have been reported to us by people that have played games with nodalization, looked at operator action, looked at models for the LOOP seal clearance in level swell and what not show that you're looking at numbers on several hundred degrees up or down depending on how conservative or non-conservative your model may have been in the first place.

So our recommendations with regards to evaluation model changes or excuse me due to non-conservatisms is that if we go to a performance based Appendix K we feel it's important to include the non-conservative effects of downcomer boiling, ECC bypass during reflood and fuel relocation. Now these will likely be pursued outside of ruling making because these same issues affect plants and their evaluation models now. So that's why it would be separated from the rule making.

But when we start looking at the numbers for code uncertainty, plus 400, minus 400 we feel very strongly that if we go to this optional Appendix K there must be something in the regulatory process that makes people demonstrate that there is sufficient conservatism in that evaluation model.

We don't have this plus 20 percent on decay heat to give everybody the assurance. We can sloppy in some models because it's accounted for somewhere else. As those major models become more realistic it's going to become important that we find a way to demonstrate that there is still the conservative intent that was there in 1973.

MEMBER BONACA: Maybe Appendix K is not conservative is you adopt these numbers regarding core uncertainties and fuel relocation as well as downcomer boiling.

MR. BAJOREK: The curve I would throw at that is you're seeing these numbers. You see of it in the data but it's hard to estimate the PCT because those big numbers are coming from codes. If Dr. Wallis were here, he would say I don't believe those codes.

MEMBER ROSENTHAL: And they are non-additive.

MR. BAJOREK: Yes, they are not additive. The experiments were it didn't look like a big effect in the package that I think you have we note that if you look at the scaling for those tests, those weren't designed to look at these issues. So the amount of energy in the downcomer versus in what you have in a PWR is much smaller in the tests. So you wouldn't expect the tests to predict those effects to anywhere near the magnitude like it's being predicted. Your only conclusion at this point is that maybe the code is right because it's the only thing that we have to try to estimate the magnitude of those effects right now.

MEMBER ROSEN: Steve, comment on this concern. The '94 model, the K heat is the pure physics that hardly anybody argues with. So going ahead with that makes obvious sense. The staff's concern about non-conservatism is also real. But to equate the two somehow doesn't intuitively makes as much sense.

You're saying okay we're going to take credit for this. Let people take credit for this but hardly anybody argues about that. But at the same time you have to factor in all these non-conservatisms. But the state of the art of these non-conservatisms is kind of uncertain compared to the K heat curve. How do you reconcile that?

MR. BAJOREK: I guess my own view on that is that's why you need to have realistic codes assessed to get an uncertainty. The problem that we see with plus 400 and minus 400 is that you get into this game of compensating errors. This is okay because I know I'm high here and I'm low over here. But until you can come up and can quantify your accuracy with the code uncertainty or some other technique the answer is still wanting I think.

MR. SCHROCK: Along those lines --

MEMBER ROSEN: That's not exactly the answer to my question. The positive change for the decay heat there is fairly solid. Whereas all the non-conservatisms all be it that they're there -- I understand the mechanisms that you are worried about and I'm worried about them too. But there is so much uncertainty with respect to all of those. How do you equate those things, something without any uncertainty effectively with something that has tons of uncertainty? I mean even in the models might be completely wrong.

MR. SCHROCK: Can I make a comment on that, Steve? There are some things that aren't even included in the codes for example heat transfer correlations, drag correlations. They're all steady state. They don't have any transient effects in them actually so it's kind of a quasi-steady model of the process. Yet you speak in front of the best estimate point of view. Those effects are not even included in these analytic models.

It seems clear to me that you're going to need some bias however you want to come up and justify it. Appendix K was kind of a gross attempt at putting bias into these calculations so that when you use them to predict a course of an accident in a plant you can be quite assured that it was a conservative calculation.

Now they erred in some directions as you say. There are some things that are non-conservative that say you'd like to take advantage but I believe you will never get rid of some bias. You have to account for things that the code just is not an adequate model for. I don't see any way to get around it. When you talk about best estimate codes as though somehow they're going to be exactly correct or at least in some probablistic sense.

But there are some things that are probablistic about the models that you have in there like correlations for drag, correlations for heat transfer or data error in those and things like that which you could be accounted for in that way. But there are other things that aren't even included or approximations that had to be made for example one dimensional flow in pipes. Well the flow is not one dimensional but that's a reasonable approximation. You will never agree exactly with the physics. So somehow you have to account for these limitations you might say by some kind of bias I would guess as well as some statistical uncertainty.

MEMBER KRESS: I don't think the bias is the right way to go.

MR. SCHROCK: Well, I don't know what you want to call it. Whether you want to call it bias or margin.

MEMBER ROSENTHAL: Let's take advantage of it's been 30 years since we did Appendix K. There are some things for which we have better knowledge. I think that we in general think that we have better codes also. We're talking about large potential reductions and unnecessary burden. I mean big changes which will taken either in operational flexibility like FQ or taken in just plain power outbreaks.

So by taking out the prescriptiveness of Appendix K and at least allowing a K prime, all licensees may choose to go to best estimate models. At least it puts it in a realm where the vendor, the licensee, could come in, take the pluses, take the minuses, take the best story with circuit 2002, let Ralph Caruso reveal it as he would any other submittal and so let the science move forward from where you were locked in 30 years ago.

MEMBER KRESS: I was about to say pretty much what you were saying, Jack. The proper approach for this is to have a best estimate situation and the way to have one is that you have to quantify the uncertainties. Now you don't have best estimate unless you quantify the uncertainties. I think that we ought to equate those two together.

How you quantify the uncertainties is there are as many opinions as there are people probably but you have to somehow do the Monte Carlo or the things you know the distributions for, you have to account for model uncertainties by an expert opinion, you have to do all sorts of things. But the best estimate process should quantify your uncertainties for you.

Then you have a baseline. You have a distribution. Then you say I won't allow an Appendix K type calculations. The way to get the Appendix K is to take your best estimate and say now what can I do in the way of conservatisms so that I can give them an easier way to do it but it accounts for the thing and gives me basically the same nature with perhaps a little bit of conservatism in them because I'm trading off an easier way to calculate.

See we did it just the opposite. All we did is start with the Appendix K and now we're going to best estimate. We're trying to balance these things off. The best way to do it is to start with the best estimate model, quantify your uncertainties, then work backwards to what you want for Appendix K.

MEMBER ROSEN: But the answer is not a zero sum game.

MEMBER KRESS: No.

MEMBER ROSEN: We should have no prejudice about what the outcome is.

MEMBER KRESS: That's what I'm saying.

MEMBER ROSEN: It may be more restrictive than the current techniques or less but we have to let the science decide that not regulatory policy.

MEMBER KRESS: But what I was saying is that it doesn't matter that your decay heat is known much more precisely than some of these heat transfer correlations of the model. You account for that in your uncertainty. You integrate that in your uncertainty analysis and you end up with final product of the uncertainty in the outcome you are trying to calculate. Of course you have to have acceptance criteria there also which means to me I would have some confidence level in the results you want. That's another issue which is how do you arrive at that confidence level.

MEMBER BONACA: I realize I would like to say that I completely agree with that approach. The only thing is that the shift brings you to the point where there is even further burden on the staff to review the proposed models that come in or present certain approaches to address the issue such as downcomer boiling and so on and so forth. Because you are trading off something as you say, Steve, you know pretty well, this conservatism in the K heat for some effect that you are claiming that is being modeled and is going to be hard to demonstrate I'm sure. Some of these are not going to be easy modeling. I don't disagree that it's a better way to do it.

MR. CARUSO: Dr. Bonaca, this is Ralph Caruso. My name has been thrown about a bit. I'm just responding to your question about the burden of doing the review. There is a certain school of thought floating around that not much use will be made of this proposed Appendix K prime because there is a certain school of thought that thinks that best estimate is the best way to go as Dr. Kress says.

Since all three vendors now have best estimate methods and it cost them money to maintain multiple copies of methods, it is not to their best interests that they would prefer to shift everyone over to best estimate models. So I personally am not too worried about the resource impact of this.

MEMBER BONACA: No, I wasn't talking about that.

PARTICIPANT: That's because it won't get used. At least that is an analogy of NFP805 maybe.

CHAIRMAN SHACK: I don't want to cut off the discussion but we're running out of time and we would like to hear from NRR. Can you make this ten minutes, Sam?

MR. LEE: I might be able to do better

than that.

CHAIRMAN SHACK: Okay.

MR. LEE: Good afternoon. My name is Sam Lee. I work for the policy and rule making in NRR. The objective of my briefing is to tell you a little bit about the rule making activities associated with all the proposed changes that were discussed today under this umbrella effort of risk-informing 50.46. What you have before you is a two page table that lists the proposed changes as well as the second column showing some of the industry interest in the rule changes by way of their rule making submittal.

But before I get into this I just want to refer back to the slide that Alan showed you this morning. As you look at where we are with respect to our effort to risk-informed 50.46 and we have reached a major milestone of completing or nearly completing all of the technical studies, we are in the phase here of just beginning rule making effort so I just wanted to let know you know this is where we are.

I just wanted to share a couple of points. I don't know that I will go through the detail of this table but the point I wanted to make is that as we discuss and consider four of the changes, these ECCS evaluation acceptance criteria, the ECCS reliability as well as the long term redefining LOCA, the industry has submitted a related petition for rule making associated with each of these proposed changes. So that really is an indication that the industry is highly interested in what we are proposing.

Where we are with respect to each of these changes (1) with respect to ECCS evaluation model is you've heard today that the technical study is complete or near completion. Upon which NRR and not just NRR but the staff will form a working group which will be composed of both NRR and research representatives to tackle the rule making associated with this proposed change. We as you can see here have one technical report that was delivered and that goes back to the third one that has to do with ECCS reliability.

As Alan pointed out this morning that of the two proposed pieces one being plant specific approach and the other being the generic approach, the plans for the plant specific approach was delivered in early May and the working group has been formed. We are reviewing the report as well as identifying all the milestones that we need to accomplish for really reducing the rule making package associated with that rule.

The other important thing that I didn't point out earlier is that the rule making associated with making these changes is we will have separate rule for each of these proposed changes. So what we will have eventually is basically four working groups each of them dedicated to each of these proposed changes. That's our plans at this point. That's about it. Are there any questions? I will be happy to answer them.

MEMBER ROSEN: Are you going to link them together the whole idea being that? I assume that the staff is saying you can't just create the flexibility because you can't consider K heat without considering the other non-conservatisms.

MR. LEE: Absolutely.

MEMBER ROSEN: So if you don't link these rule makings together you might get out of phase and have permission to use the K heat without considering the non-conservatisms.

MR. LEE: Absolutely. We will have a working group that is dedicated to each of these proposed changes as well as an oversight group that looks at the links between them.

MEMBER ROSENTHAL: If I could. There is just a subtlety as follows: the rule speaks to the

'71 standard. So you can change the standard in the rule. You need some means of addressing what you know. You can not know what you know. You know that there are problem with just taking out the margin without addressing these other matters.

That will be done right now within what I'll call the regulatory framework. The regulatory framework is some combination of our rules, our reg guides, our SRPs, our reviews, topical reviews I mean the greater regulatory framework. The regulatory framework has to take on the down side but it may not be in rule making per se. It is yet to be worked out.

MR. LEE: Does that answer your question?

MEMBER ROSEN: Yes.

MR. LEE: Any other questions?

CHAIRMAN SHACK: So it's envisioned that you wouldn't haul out for example the acceptance criteria separately. There would just only be an Appendix K prime that would link all these things. There's not a K prime and a K double prime.

MR. CARUSO: I don't think we've gotten to that point yet. We're going to have separate working groups working on different parts of them. As you said they are interconnected.

CHAIRMAN SHACK: Some of that criteria

seems a little bit less linked than others. Maybe not.

MR. CARUSO: I had some ideas about how I was going to word that yesterday. Not today.

CHAIRMAN SHACK: You have to be a little bit more clearer.

MR. CARUSO: I have a feeling that we will be working on all four of them and tacking them to one another. They will not be done in isolation.

MEMBER ROSEN: You did start out by saying you didn't want (Inaudible.)

MR. CARUSO: Correct.

MR. LEE: That's correct. And we look forward to having additional sessions like these to inform you of the progress.

CHAIRMAN SHACK: What's the time frame for the next session presumably at the end of July? September?

MR. GRIMES: This is Chris Grimes. We have asked for time on the whole committee calendar in July. At that opportunity I wanted to share what NRR and research view as to the oversight functions that are going to attempt to define some long range outcomes that we want to see from all our rule making and all of our risk-informed performance based risk management program issues. So we expect to come back to the full committee in July and provide you with a status report and hopefully a better picture of where we view the agency going in this whole arena.

MEMBER ROSEN: Will that include the framework and I think that was Jack's word that will keep us out of trouble here?

MR. GRIMES: Yes, what we call this our vision about coherence is where does the rule making fit into the overall regulatory process? We would expect to be able to describe the framework as Jack describes it. There is also the use of the word "framework" in terms of option free framework. We need to evolve that.

It is something that is a more practical picture about where we are going, a road map, and a set of outcomes and program performance measures. How will we know if our rule making is successful? We need to be able to measure that.

MEMBER ROSEN: I would suggest that one of the measures that you might want to think about is if anybody uses any of the new flexibility.

MR. GRIMES: There's the thinking in the sense that says is it worth the investment. Do we have a customer base that's going to take advantage of the rule making? That has to be a part of the vision in the future as well.

CHAIRMAN SHACK: Any comments from any of the other committee members? Questions? We have about five minutes left.

MEMBER BONACA: The only question I have is to what extent I mean I'm sure there is an interaction going on with the industry. There are three petitions from the industry and a fourth one from Performance Technology. The sense I'm getting as we go through is that maybe the expectations when we went after 50.46 were higher than actually the research work that was being done may be able to deliver because all of the conservatisms and so on. I'm sure that there is a dialogue with the industry and the industry with us.

MEMBER ROSENTHAL: We have that with the industry and we were planning a public meeting again sometime in June. There are public meetings planned on the reliability issue at whatever date. We were going to try to organize a public meeting on the stuff you heard this afternoon hopefully in June. We have had meetings with regulated community with the public in the past.

MEMBER ROSEN: Are you talking about June of this year? That starts tomorrow.

MR. LEE: Yes. With regards to the reliability piece we have had as Alan mentioned this morning an on-going meeting with the industry almost on a monthly or bi-monthly basis. We will have another one at the end of June to talk about the condition of -- probability as well as LOCA frequencies. So we have engaged the industry along the process.

MEMBER BONACA: The reason why I am asking that is two years ago it was also a necessity in the industry that for example the 1.2 1971 the K heat curve was like a freebie. It was just there for the taking. We got a different kind of message when Steve presented his presentation and showed the effects of downcomer boiling and other effects that were not accounted for and the trade-offs with that.

Now I hope that already it's sinking out there that the K heat by itself is another freebie, there are other things that are on the table. If you take that you have to pay some other attention to our effects. That's why I had to know where the industry is and I'd like to know if there is open communication and understanding of these issues.

MR. CARUSO: I would like to add that at

a higher level we have a right paper from NEI on their view about a risk-informed performance based regulatory structure for future reactors which we are taking as input to the framework question that Dr. Rosen referred to. That is how are you going to structure all of activities. They expect to have a series of meetings not just on their ECCS but we have a proposed 50.44 on the street now. We also have the future reactor activities.

Sometime this summer we would like to hold a workshop and try to sort out all of these in terms of the sequencing and the timing and the utility. I would hope that the workshop would provide us not only in the industry prospective but also I've engaged our public interest groups. I've asked the public interest groups to prepare to participate in such a workshop.

MEMBER CRONENBERG: Chris, they didn't see the NEI of 0202 yet because I don't know when I get a complete package from you for the July meeting. But that will be mailed out to you I'm sure.

MR. CARUSO: The public interest groups received the NEI 0202 directly from me in an E-mail.

MEMBER CRONENBERG: But these guys didn't.

MR. CARUSO: These guys didn't, that's

correct. The ACLS did not.

MEMBER ROSEN: I think I want to take back something I said a minute ago after thinking about it. We're in an open thinking session here. I said that one of the things that we want to think about is whether anybody would use this stuff implying that if the word gets out that some of the requirements are so injurious let's say that people would just back away from the whole thing and that you shouldn't do that.

I take that back. I think even if it turns out and nobody knows how it will turn out whether or not this will provide more or less flexibility if you went through the end with all of this. I think it's the right thing to do to the extent that we really can better model the actual physical processes.

You shouldn't stick on the 1971 technology. You should move ahead even if it turns out that maybe people won't use the new flexibility. That's really not a good reason to not use better technology and put it in place.

MR. SCHROCK: Another way of looking at that is though the response to this petition could have been that Appendix K is what it is. It was created in a timeframe when knowledge was what it was.

It's different now and we're heading towards a best estimate methodology regulatory process. We don't wish to confuse the two.

We retain the old one for convenience in sustaining licenses that exist for those that think that the burden of best estimate calculation is so onerous that they can't do it but the agency is committed to improvement of scientifically based regulatory process. We won't get there by modifications of an Appendix K type of regulatory process. That would have saved you all of the staff effort and I think it would get you to about the same endpoint. It would insure that you have some progress in best estimate methodology.

MEMBER LEITCH: Well said, Virgil. I second that.

MEMBER BONACA: Yes. I didn't make too many comments. I was thinking how much work has gone into this.

MEMBER SIEBER: I guess it's not obvious that you are for sure going to come out with a benefit because there are things that aren't modeled properly, there may be conservatisms in there but perhaps new test data and new insights would show maybe that the existing Appendix K isn't as good as being conservative as we think we are. To me I think that there is if nothing else a moral obligation to continue to work to make sure that whatever we come up with that's new is acceptable and to verify somehow or other that we haven't been running for the last 30 years in using models that are inappropriate.

MEMBER KRESS: To follow up on that just a little the thing that I see missing from what I've heard so far is this if I want to change the Appendix K process to something with still an Appendix K but have different things in it. What I'm really interested in is reality. What actual peak clad temperature or what kind of result do I get if my ECCS and I need a best estimate model to do that and I need acceptance criteria.

The other thing I need which I haven't heard much about yet if I change my Appendix K by whatever I do, what is that going to do to the ECCS? Are they going to up to power? Are they going to ask for changes? Then I take those changes to the ECCS for the fleet of plants by the way because this is rule. I put it in my best estimate model and say if I make those changes I still meet my acceptance criteria for all the plants or the significant fracture of them or whatever the process is. That's the part I haven't seen. What would be the result of the changes that you might make in the Appendix K in terms of the actual operation of the plant and the ECCS provisions.

MR. BAJOREK: Well when you're doing the best estimate work one of the difficult things in making a comparison of best estimate to Appendix K was that immediately everyone wanted to use that new margin to increase power. Everybody went from Appendix K to best estimate, took I think it was generally 5 percent increase in power, FQs would increase from 2.3 to 2.4 up to 2.5 close to 2.6. Hot channel entalthy (PH) factors from 1.6 to 1.7. Almost immediately eaten up in increased power either to the core itself to the hot assembly to give you some better core management.

To a lesser extent it went into with relief in tech spec to give you a wider window for your accumulator levels and some things like that. But virtually everybody used it to increase the power.

MEMBER SIEBER: Any other questions or comments?

MEMBER BONACA: The only thing I would like to thank the presenters. I think it was an outstanding presentation, a lot of information, a lot of work in it and then clear.

MEMBER KRESS: Yes, I thought it was very well done myself.

PARTICIPANT (PRESENTER): I'll pass that on to those who are not here to hear that.

MEMBER SIEBER: I hate to say thank you when the person isn't here but it's better than not saying thank you at all. I think we've concluded all our business. This meeting is adjourned.

(Whereupon, the above-entitled matter was concluded at 5:00 p.m.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Reactor Safeguards Materials and Metallurgy & Thermal Hydraulic Phenomena & Reliability and Probabilistic Risk Assessment Subcommittees

Docket Number: N/A

Location: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

/s/ Anna-Marie Smith

Anna-Marie Smith
Official Reporter
Neal R. Gross & Co., Inc.

Rulemaking Activities for
Risk-Informing 50.46 and Related Rules
(Samuel Lee, Policy & Rulemaking Program, NRR)

Changes Proposed in SECY-01-133 / SECY-02-0057		Related Industry Petitions and Interest	Staff Actions
1	Voluntary alternative to the technical requirements related to ECCS evaluation model	PRM 50-74 (Sept. 6, 2001): NEI filed petition for rulemaking that requested NRC to amend Appendix K to Part 50 to allow licensees to adopt 1994 revision of the ANS consensus standard for decay heat power, and to give licensees standing option to adopt any future NRC-endorsed revisions to the standard, without NRC having to conduct a separate rulemaking to incorporate by reference each subsequent revision.	Technical studies ongoing. Technical report to support rulemaking expected by July 2002. Upon completion of the technical report, staff plans to initiate rulemaking phase along with drafting relevant regulatory guides.
2	Voluntary alternative to ECCS acceptance criteria	PRM 50-71 (March 14, 2000): NEI filed petition for rulemaking that requested NRC to amend 50.46 to allow licensees to use zironium-based cladding materials other than zircaloy or ZIRLO, provided the cladding materials meet the requirements for fuel cladding performance and have received approval by the NRC staff. (Benefit: eliminate need for licensees to obtain exemptions to use advanced cladding materials that have already been approved by NRC)	Technical studies ongoing. Technical report to support rulemaking expected by July 2002. With respect to making the acceptance criteria performance-based, staff plans to initiate rulemaking phase along with drafting relevant regulatory guides once the related technical studies are completed. In the interim, the staff is considering a direct rulemaking to add fuel "M5" to the list of zirconium-based cladding materials in the current rule.

3	Voluntary risk-informed alternative to ECCS reliability requirements in GDC 35: Two options include (1) generic plant binning according ECCS accident mitigation reliability, (2) plant-specific assessment of ECCS accident mitigation reliability.	PRM 50-77 (May 2, 2002): Performance Technology filed petition for rulemaking that requested NRC to amend 50.46, Appendix A, GDC 17, "Electric Power Systems", to delete the requirement of assuming that offsite electrical power is not available for postulated accidents. (changes also proposed for GDC 35, 38, 41, and 44 to conform to GDC 17). (Stated benefit: Allows required EDG start time to be increased)	Draft technical report for plant-specific approach to assess ECCS safety function reliability was delivered on May 1, 2002. Technical work for generic approach is expected by July 2002. Complete technical work to support development of regulatory guide expected by July 2002. (Note: Completion of technical work is dependent on resolution/determination of LOCA frequency and conditional LOOP probability to be used for the analysis). Staff formed a working group to review the report on plant-specific approach and is developing an implementation plan for rulemaking.
4	LBLOCA redefinition	PRM50-75 (February 6, 2002): NEI filed petition for rulemaking that would allow alternate break size to currently required double ended rupture of largest pipe in RCS. (Stated benefit: "enable" technical discussions on redefining LBLOCA to proceed without being in conflict with current rules. Also may expedite schedule by up to two years).	Technical studies ongoing. Technical studies expected to be completed by July 2004. Staff is currently reviewing the petition. Rulemaking effort will follow accordingly.

Risk-Informed Revision of 10 CFR 50.46 Acceptance Criteria and ECCS Evaluation Model Requirements (Appendix K)

UNITED STATES NUCLEAR REGULATORY COMMISSION

Presentation to the ACRS Subcommittees on Materials and Metallurgy, Thermal-Hydraulic Phenomena, and Reliability & Probabilistic Risk Assessment

May 31, 2002

Stephen M. Bajorek, G. Norman Lauben, Ralph O. Meyer
Safety Margins and Systems Analysis Branch
Division of Systems Analysis and Regulatory Effectiveness
Office of Nuclear Regulatory Research

OBJECTIVES

1. Update the Subcommittees on status of staff efforts related to risk-informing 10 CFR 50.46 acceptance criteria and Appendix K as envisioned by SECY-01-133 / SECY-02-0057.

2. Summarize and discuss near term actions.

BACKGROUND

SECY-01-133 states:

"The staff recommends that rulemaking should be undertaken to change the current 50.46. ....
....
..... In the near term, this revision would involve an update of Appendix K requirements based on more current and realistic information. ....

As part of this update, the staff will also consider the recognized non-conservatisms and model limitations to insure that proper safety focus is incorporated in any new rule.

.......; in summary, the staff will undertake work to:

support removal of unnecessary conservatisms from Appendix K."

The principal focus of this effort has been on:

1. Replacement of the Appendix K requirement to use 1.2 X 1971 ANS decay heat standard with a requirement based on the 1994 ANS decay heat standard.

2. Determining the impact of decay heat & metal-water reaction rate models and effect of accounting for non-conservatisms in existing Appendix K evaluation models.

- **Staff efforts have been in three areas:**

  - **Reviewing basis of exising 10 CFR 50.46 acceptance criteria for:**

    Peak Cladding Temperature (< 2200 ^{o}F),

    Maximum Cladding Oxidation (< 17% of total cladding thickness before oxidation)

  - **Reviewing 1994 Decay Heat Standard for incorporation into Appendix K, and feasibility of revising criteria related to Metal-Water Reaction, Steam Cooling, and Return to Nucleate Boiling During Blowdown**

  - **Evaluating known conservatisms and non-conservatisms in Appendix K EMs**

Outline: Recommendations to be Presented

1. Revise the 10 CFR 50.46 acceptance criteria for PCT and ECR to be "performance-based".
2. Replace 1971 ANS Decay Heat Standard with 1993 Standard
3. Replace the Baker-Just correlation with Cathcart-Pawel for metal-water reaction heat release.
4. Delete the requirement for steam cooling only at reflood rates below 1 inch/sec.
5. Retain the prohibition on assuming a return to nucleate boiling during blowdown.
6. Require that the new Evaluation Models to demonstrate sufficient overall conservatism and that they account for several identified non-conservatisms.

United States Nuclear Regulatory Commission

ACCEPTANCE CRITERIA AND METAL-WATER REACTION CORRELATIONS

Ralph Meyer
Office of Nuclear Regulatory Research

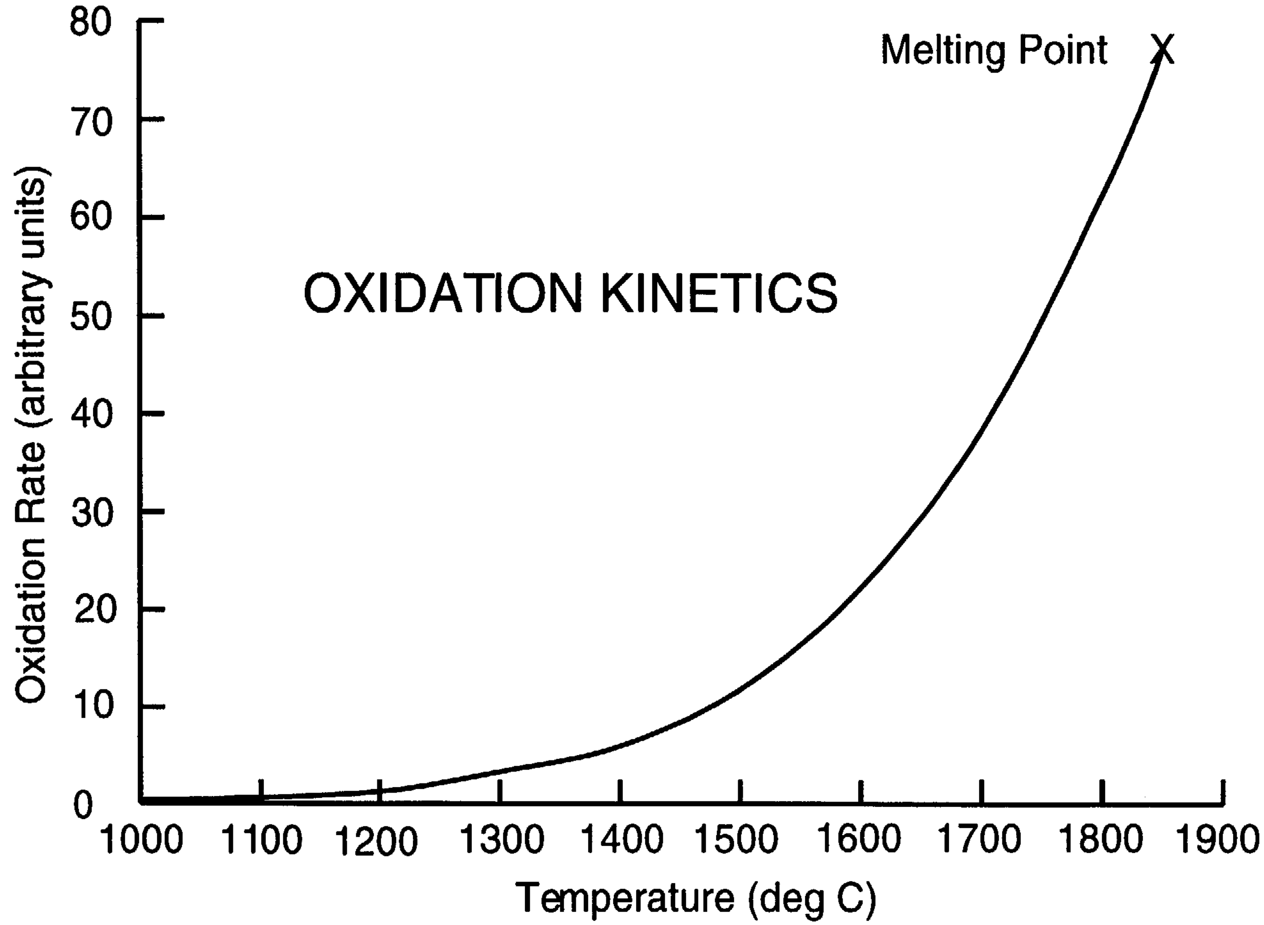
ACRS Meeting
May 31, 2002

ORIGIN OF PEAK CLADDING TEMPERATURE LIMIT

- Comes from temperature at which 17% ECR limit breaks down
- There was a second consideration related to runaway temperature escalation

STATEMENT ON TEMPERATURE LIMITS FROM 1973 HEARING

Westinghouse proposed a maximum calculated temperature limit of at least 2700°F; Combustion Engineering and the Utility Group agreed on 2500°F as the peak allowable calculated temperature on the basis that much of the data on oxidation and its effects stops at 2500°F. Babcock and Wilcox suggested a more conservative 2400°F as the peak calculated temperature to be allowed, presumably because "significant eutectic reaction and an excessive metal-to-water reaction rate would be precluded below 2400°F." General Electric argued strongly that the limit should not be reduced to 2200°F; that 2700°F is really all right as far as embrittlement is concerned, but that the Interim Acceptance Criterion value of 2300°F should be retained. In addition to being consistent with their expressed desire not to change any of the criteria, the GE recommendation of retaining the 2300°F limit is intended to ensure that the core never "gets into regions where the metal-water reaction becomes a serious concern."
(USAEC, Opinion of the Commission....., CLI-73-39, Dec. 28,1973, p. 1097)


OXIDATION KINETICS
Melting Point X
Oxidation Rate (arbitrary units)
0
10
20
30
40
50
60
70
80
1000
1100
1200
1300
1400
1500
1600
1700
1800
1900
Temperature (deg C)

HEAT GENERATION RATE

- When reaction heat becomes a significant part of total, positive feedback causes runaway

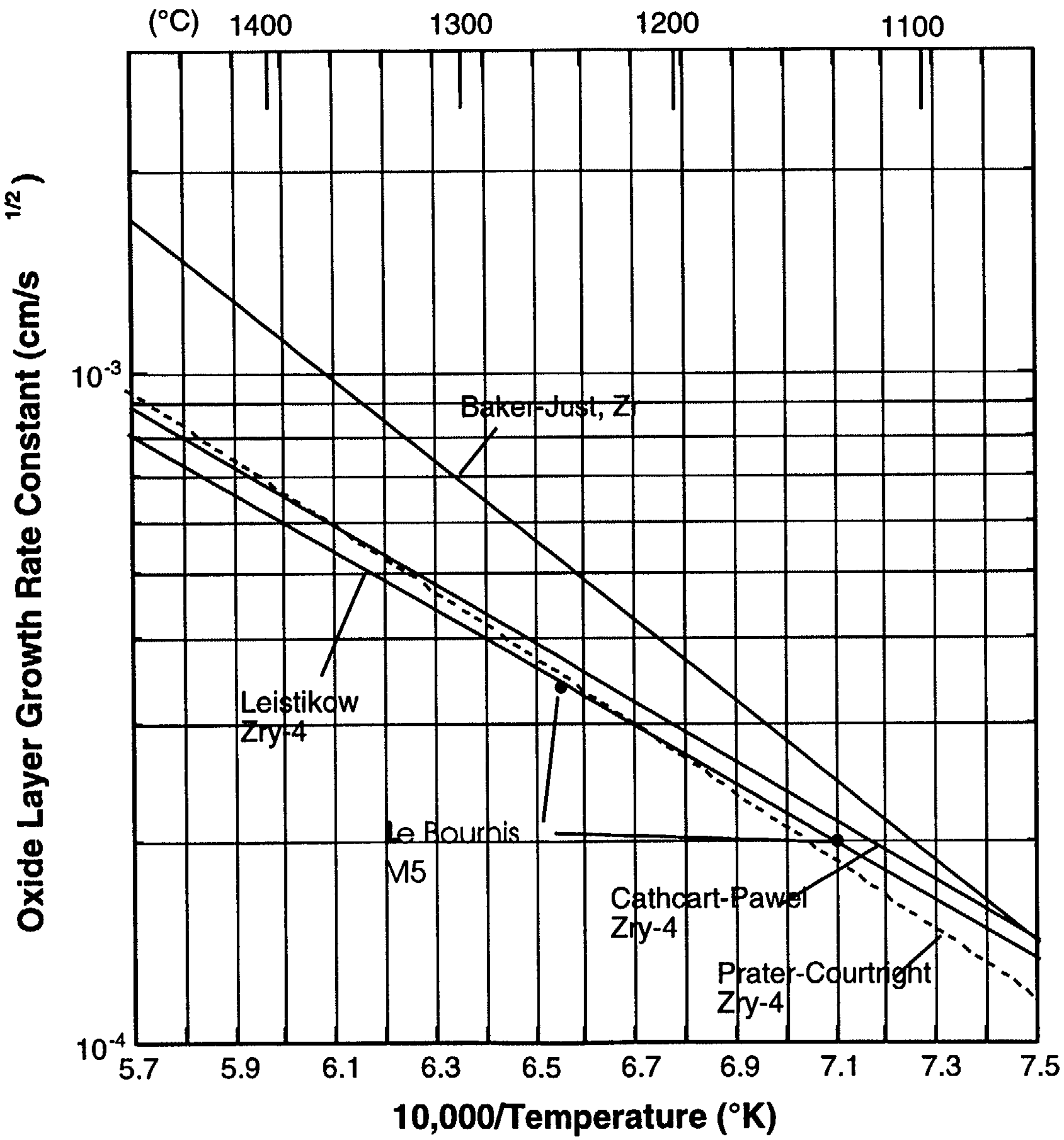
$$\text{Heat Rate}_{B\text{-}J}(2200^{\circ}F) = \text{Heat Rate}_{C\text{-}P}(2307^{\circ}F)$$

- Because Cathcart-Pawel is accurate, PCT could be increased to 2300°F with same margin to runaway as perceived in 1973

HIGH-TEMPERATURE OXIDATION MEASUREMENTS

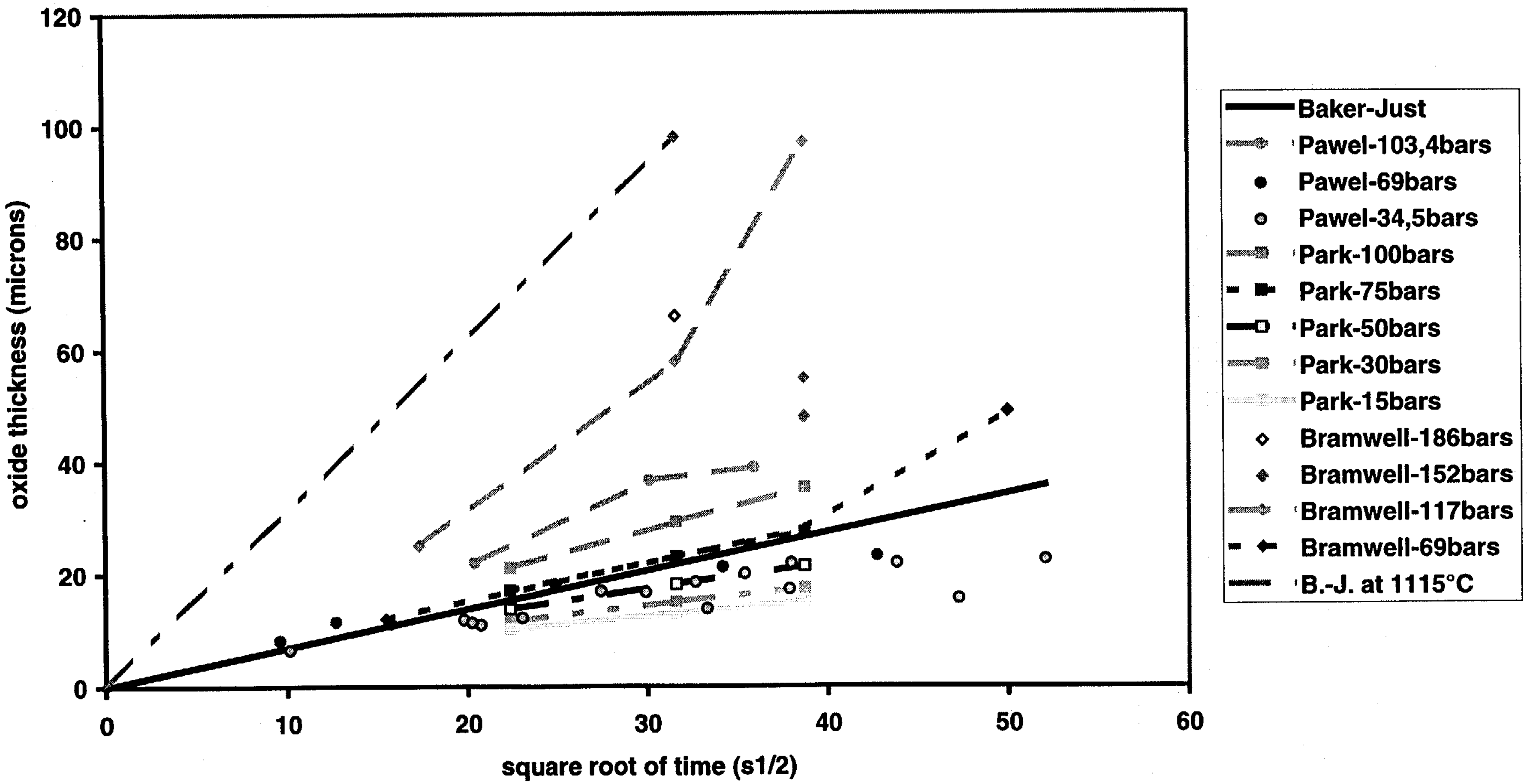
(Approximately the same rate around 2200°F)

Investigators	Metal
Baker and Just	Zr
Lemmon	Zr
White	Valoy (Zr-1.3Cr-0.1Fe)
Urbanic	Zircaloy-2, Zircaloy-4, Zr-2.5Nb
Cathcart et al.	Zircaloy-4
Chung and Kassner	Zircaloy-4
Grandjean et al.	Zircaloy-4
Yan et al.	Zircaloy-2
Waeckel and Jacques	Zircaloy-2
Le Bourhis	M5
Leech	ZIRLO
Yegorova et al.	E110 (Zr-1Nb)



(°C) 1400
1300
1200
1100
Oxide Layer Growth Rate Constant (cm/s^1/2)
10^-3
10^-4
Baker-Just, Zr
Leistikow Zry-4
Le Bourhis M5
Cathcart-Pawel Zry-4
Prater-Courtright Zry-4
5.7 5.9 6.1 6.3 6.5 6.7 6.9 7.1 7.3 7.5
10,000/Temperature (°K)

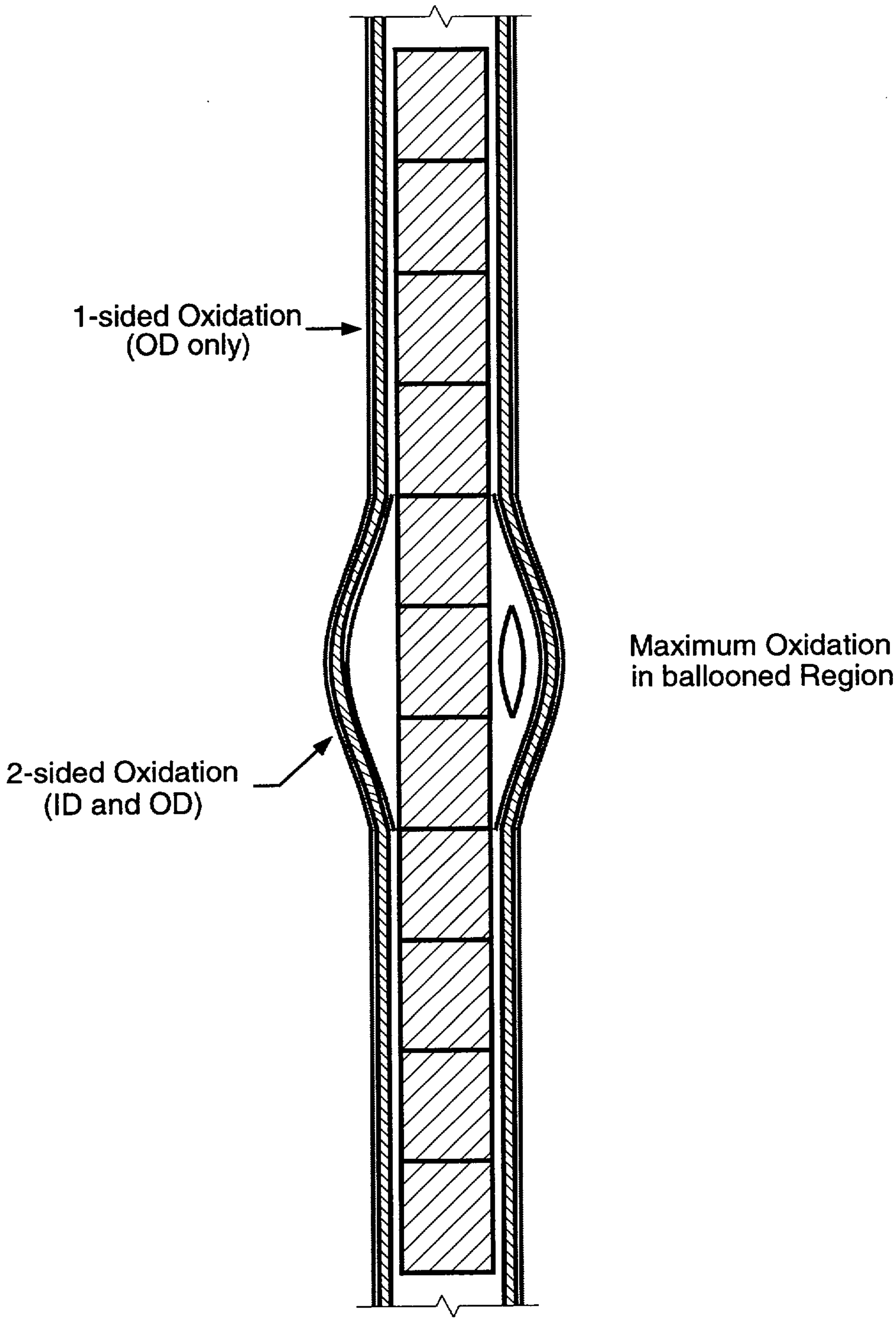
Zry - oxide thickness at 900°C as a function of square root of time and steam pressure



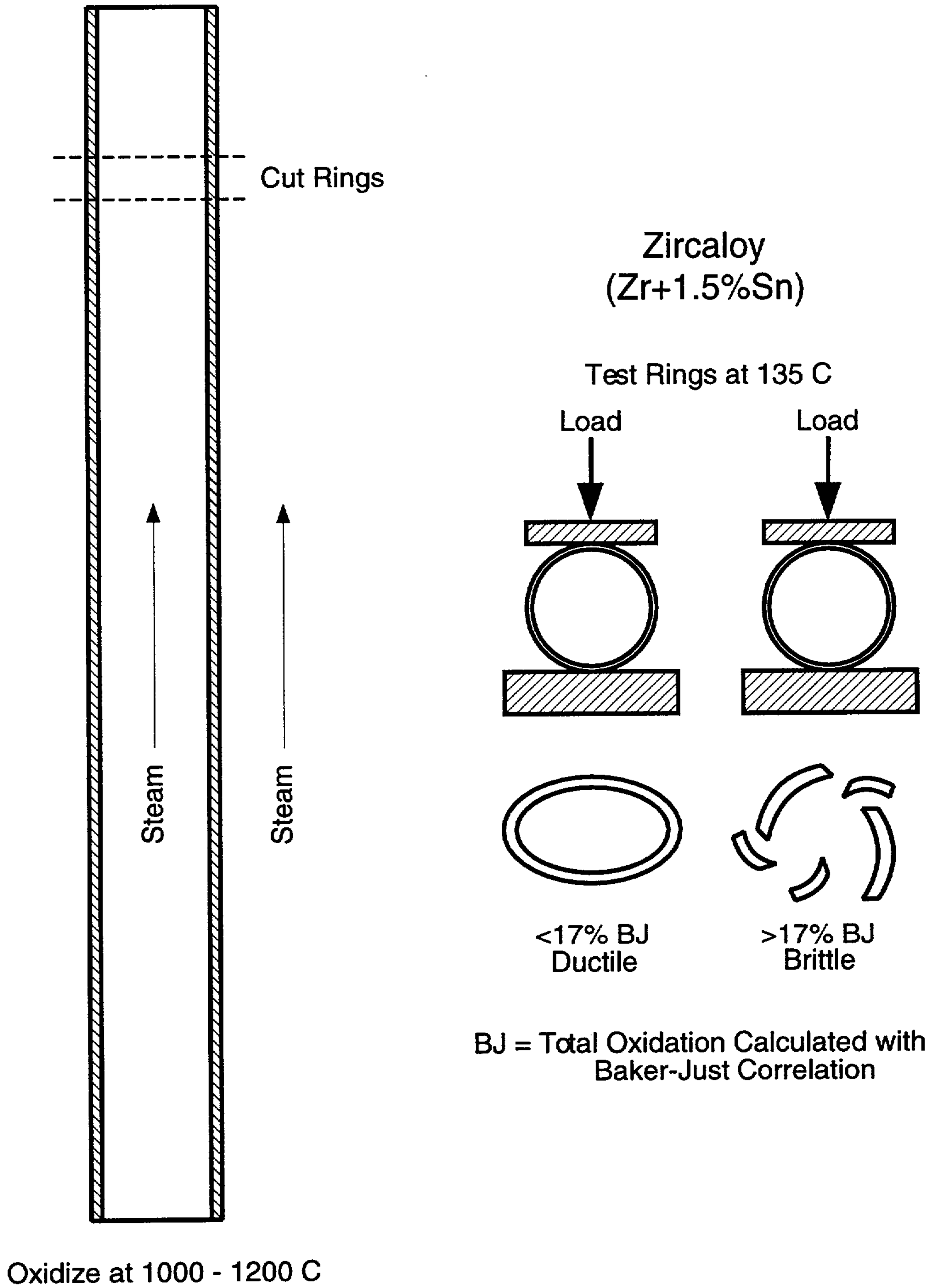
THERMAL SHOCK TESTS

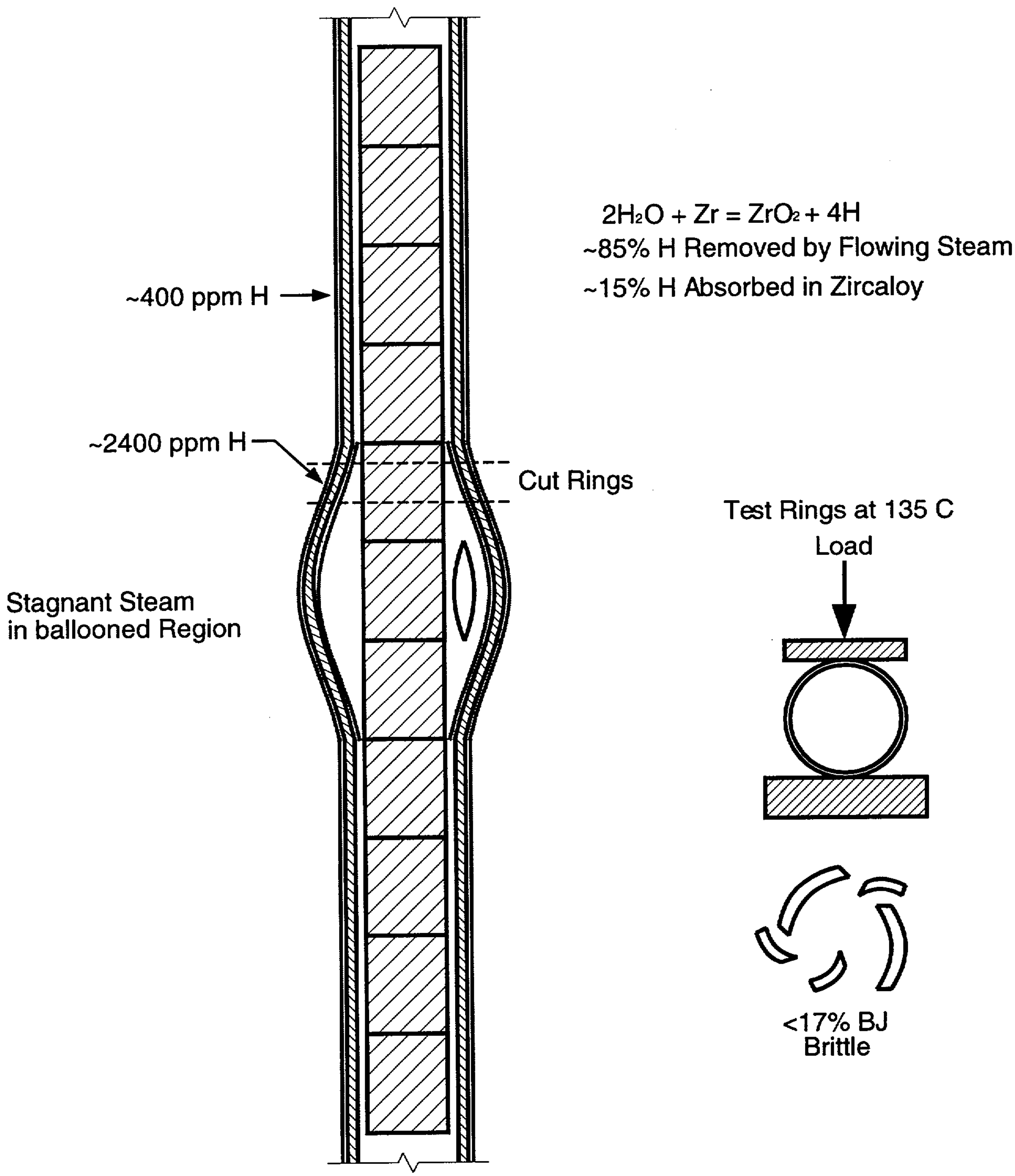
Not adequate according to U.S. AEC Commissioners in 1973

"Our selection of the 2200°F limit results primarily from our belief that retention of ductility in the zircaloy is the best guarantee of its remaining intact during the hypothetical LOCA. The stress calculations, the measurements of strength and flexibility of oxidized rods, and the thermal shock tests all are reassuring, but their use for licensing purposes would involve an assumption of knowledge of the detailed process taking place in the core during a LOCA that we do not believe is justified."

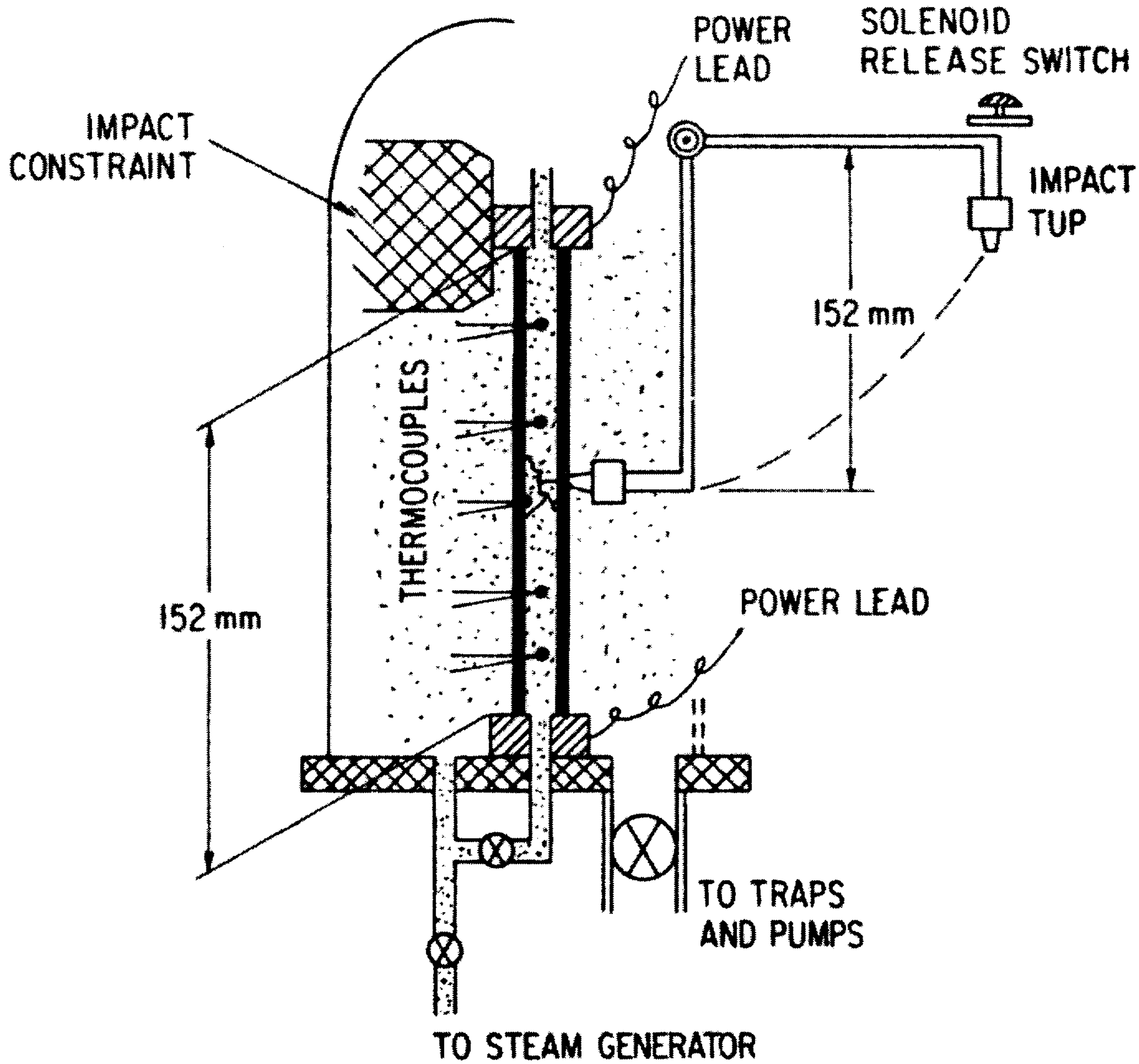


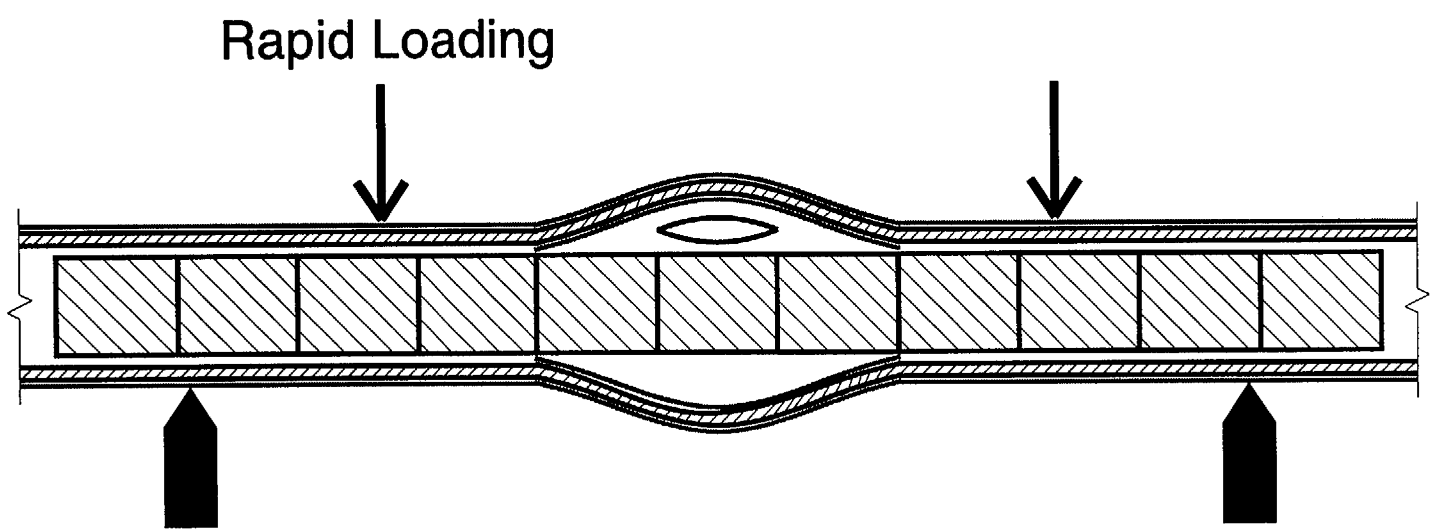
Significant Oxidation 1000 - 1200 C
(2200 F = 1204 C)


Cut Rings
Steam
Steam
Oxidize at 1000 - 1200 C
Zircaloy
(Zr+1.5%Sn)
Test Rings at 135 C
Load
Load
<17% BJ
Ductile
>17% BJ
Brittle
BJ = Total Oxidation Calculated with
Baker-Just Correlation



Hydrogen Effect Discovered ~1980


POWER LEAD
SOLENOID RELEASE SWITCH
IMPACT CONSTRAINT
IMPACT TUP
152 mm
THERMOCOUPLES
152 mm
POWER LEAD
TO TRAPS AND PUMPS
TO STEAM GENERATOR



4-Point Bend Test

CONCLUSIONS

- New PCT and ECR limits can be derived from mechanical property tests for all burnups and different alloys
- Simple ductility test (ring compression) may be adequate, as shown for unirradiated Zircaloy
- Confirmation of ductility test to be investigated with 4-point bend or pendulum impact test
- PCT should not exceed 2300°F to retain margin to avoid runaway temperatures
- Cathcart-Pawel may work adequately for all alloys and burnups (TBD) provided pressure enhancement is added for SBLOCA analysis

Decay Heat Changes to 50.46 and Appendix K

Joint Meeting of the ACRS Subcommittees on Materials & Metallurgy, Thermal-Hydraulic Phenomena, and Reliability & Probabilistic Risk Assessment

May 31, 2002

G. Norman Lauben
Safety Margins and Systems Analysis Branch
Division of Systems Analysis and Regulatory Effectiveness
Office of Nuclear Regulatory Research

"IF YOU DON'T HAVE A BEST ESTIMATE, HOW DO YOU KNOW WHAT IS LABELED AS CONSERVATIVE IS CONSERVATIVE?"

MILTON LEVINSON - ACNW
AUGUST 28, 2001

THE 1994 ANS DECAY HEAT STANDARD

- It is proposed that the decay heat requirements in Appendix K and the best estimate guidance in Regulatory Guide 1.157 be replaced with requirements and guidance based on the 1994 ANS decay heat standard.

- The Appendix K option in 50.46 currently requires fission product decay heat be modeled using the draft 1971 ANS standard with a multiplier of 1.2 and the assumption of infinite irradiation. A separate paragraph in Appendix K requires consideration of Actinide decay heat.

  - An alternative would permit the use of the 1994 ANS decay heat standard, which involves more sophisticated uncertainty methods and a greater number of options left to the user.

  - The 1994 ANS standard considers more recent available data and methods.

  - Model options in the 1994 standard have been identified and studied. They are:

    1. Whether the reactor operating history should be represented by a histogram of multiple irradiation intervals and multiple fissile isotopes or can be modeled as a single interval and a single fissile isotope, ^{235}U,
    2. Values of the recoverable energy per fission (Q_i) for ^{235}U, ^{239}Pu, and ^{238}U, and ^{241}Pu,
    3. Calculation of the correction factor G(t) for neutron capture in fission products,
    4. The actinide contribution to decay heat power
    5. The effect of various uncertainty methods and parameters.

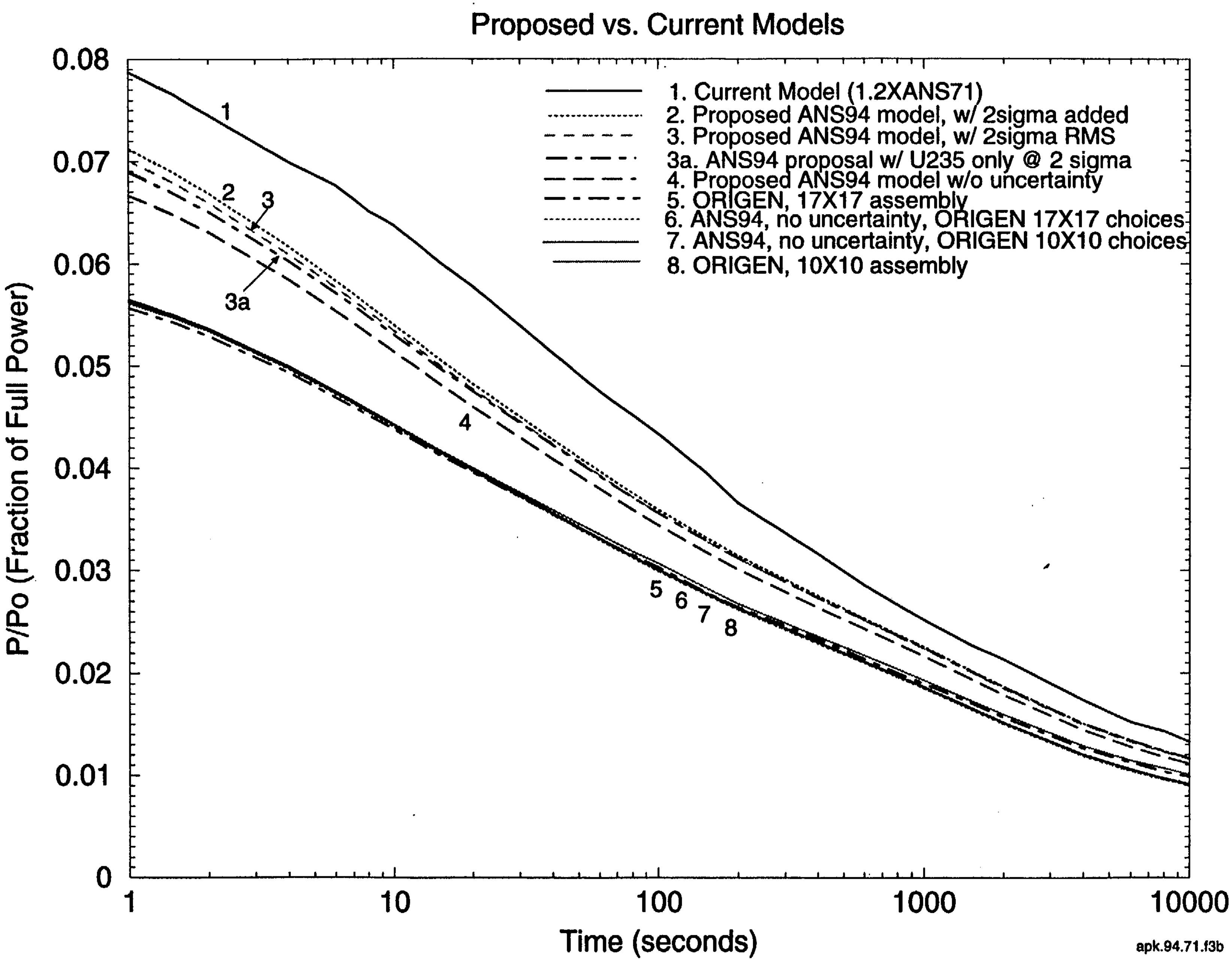
- The performance based realistic option in 50.46 would allow use of the 1994 standard today. Specification of the 1994 standard as an acceptable method in Reg. Guide 1.157 would facilitate its use.

ASSUMPTIONS FOR NINE DIFFERENT DECAY HEAT CALCULATIONS

Case No.	Model	Multiplier	Operating Time	Fiss. Fractions	Capture Time (Sec.)	Ψ	Fission Energy MeV/f.	Actinide Yield	Isotope Tables	Isotopic Uncertainties
Current Appendix K										
1	ANS73	1.2	∞	100% ^{235}U	N/A	N/A	N/A	0.7	N/A	N/A
Appendix K Proposals										
2	ANS94	2σ,add	∞	Note 3	2.e8	1.0	200	0.7	Note 7	Note 8
3	ANS94	2σ,RMS	∞	Note 3	2.e8	1.0	200	0.7	Note 7	Note 8
3a	ANS94	2σ	∞	100% ^{235}U	2.e8	1.0	200	0.7	Note 7	Note 9
4	ANS94	mean	∞	Note 3	2.e8	1.0	200	0.7	Note 7	N/A
Best Estimate										
5	ORIGEN[1]	mean	Calc.	Calc.	Calc.	Calc.	Calc.	Calc.	Calc.	N/A
6	ANS94	mean	ORIGEN[5]	Note 4	1.2e8[5]	1.0	ORIGEN[5]	.514[5]	Note 7	N/A
7	ANS94	mean	ORIGEN[6]	Note 4	1.2e8[6]	1.0	ORIGEN[6]	.508[6]	Note 7	N/A
8	ORIGEN[2]	mean	Calc.	Calc.	Calc.	Calc.	Calc.	Calc.	Calc.	N/A

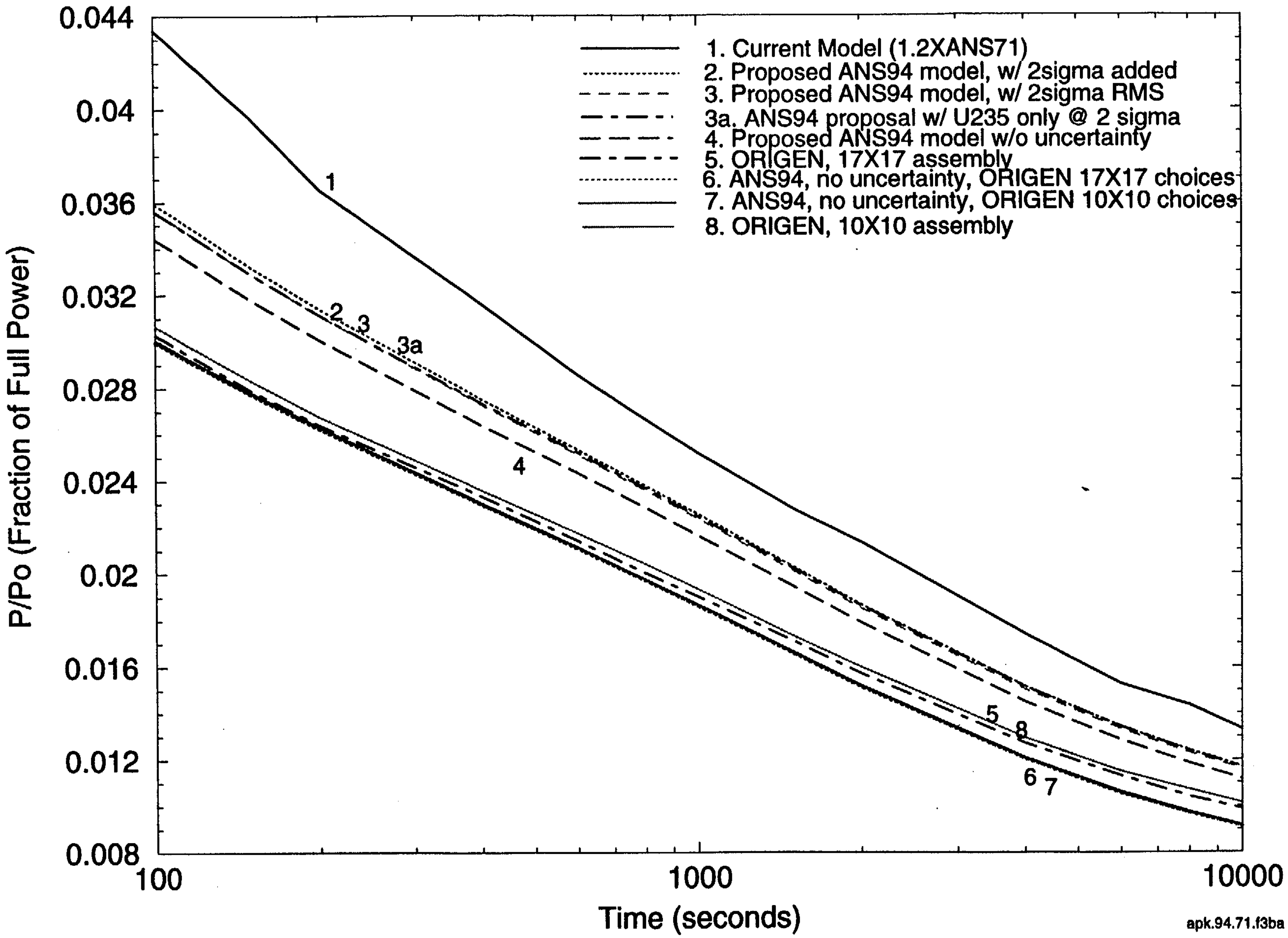
Note 1 17X17 PWR assembly
Note 2 10X10 BWR assembly
Note 3 Assumes fissioning fractions are 90% ^{235}U and 10% ^{238}U
Note 4 Cycle average values from ORIGEN calculations for four isotopes
Note 5 From 17X17 PWR ORIGEN calculation
Note 6 From 10X10 BWR ORIGEN calculation
Note 7 23 decay group exponential fits for $F(t,\infty)$ in ANS94 standard
Note 8 Used curve fits from Figures 1 and 2
Note 9 Used curve fit from Figure 1

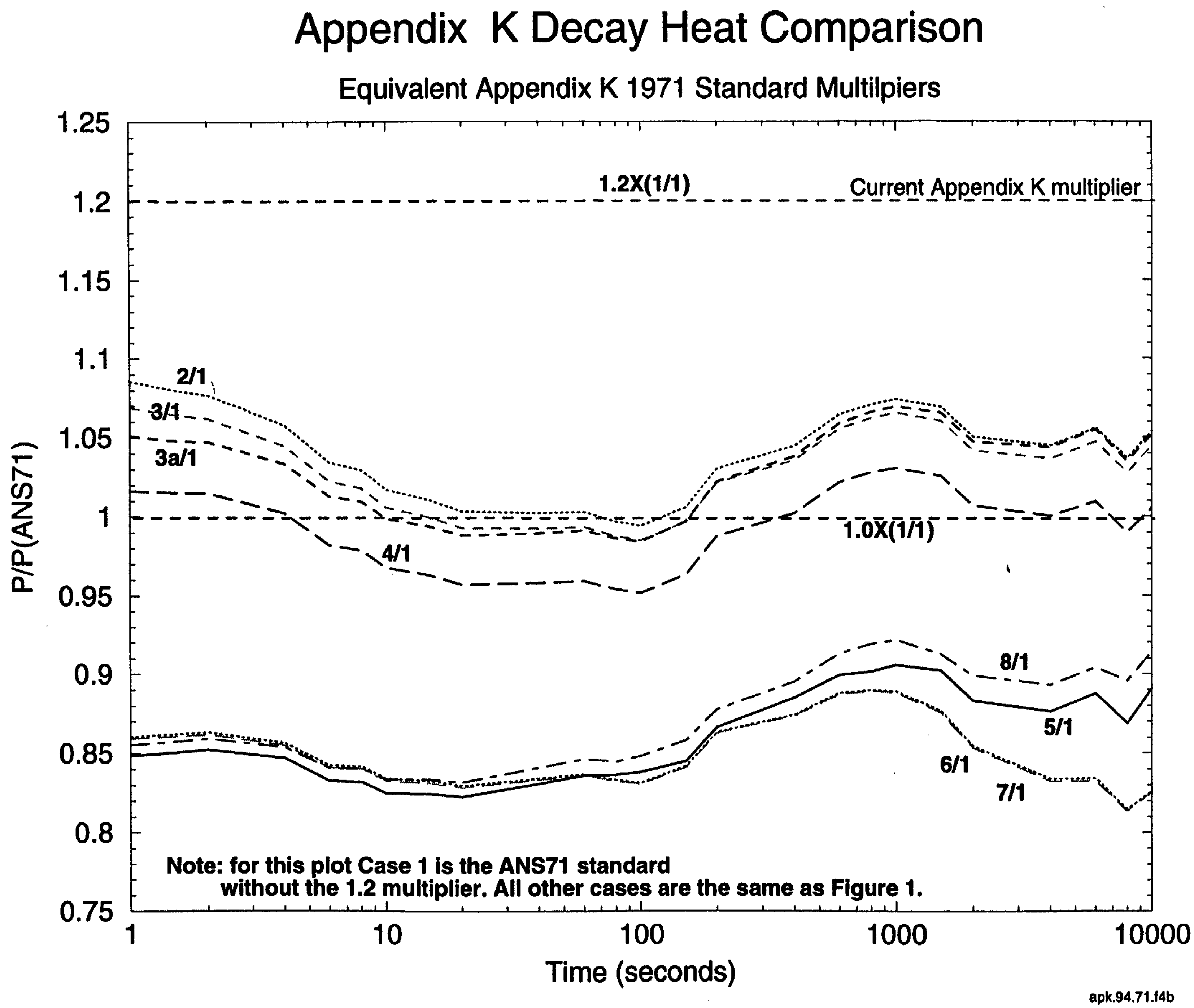
Appendix K Decay Heat Comparison



Appendix K Decay Heat Comparison

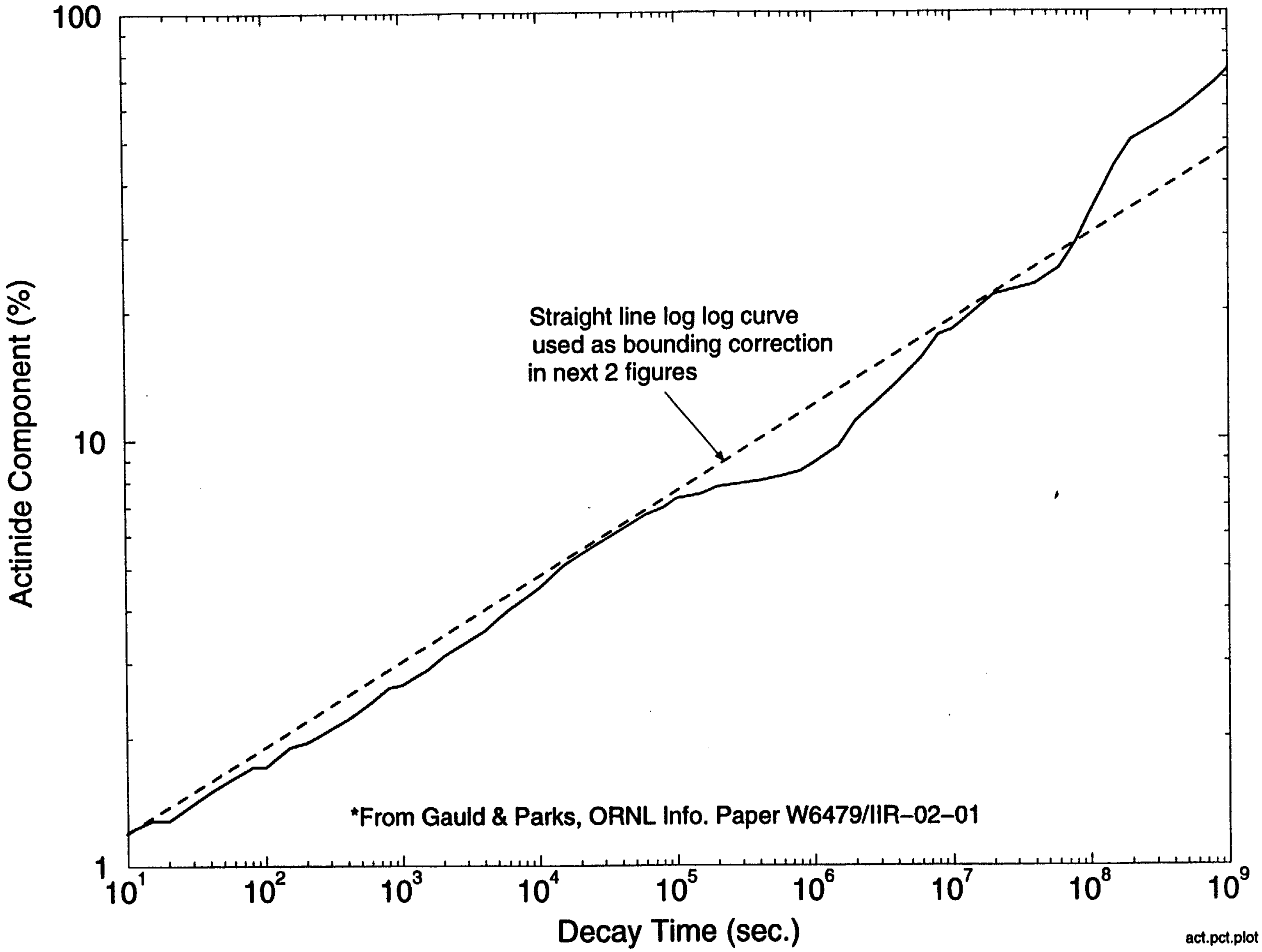
Proposed vs. Current Models

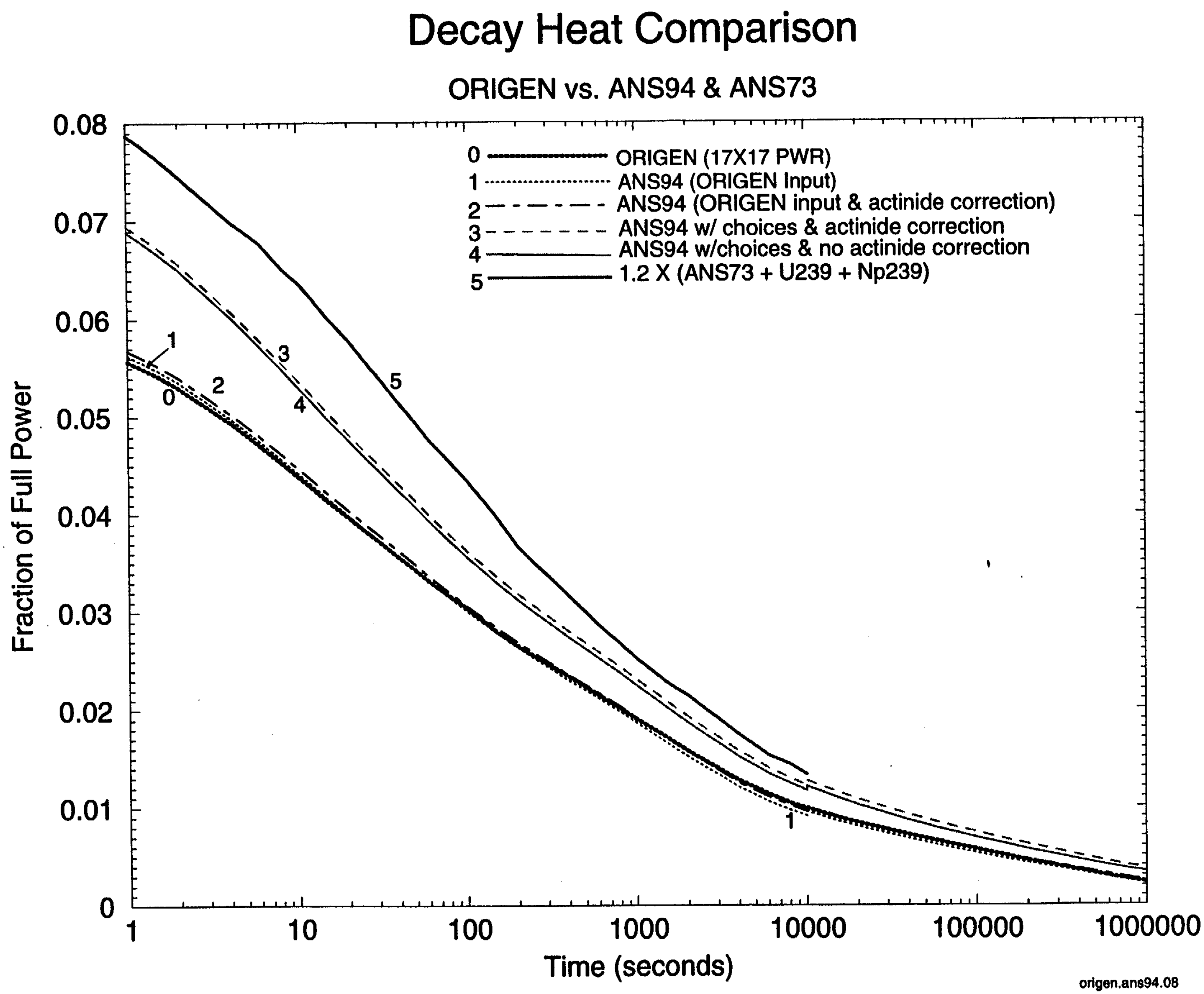



Appendix K Decay Heat Comparison
Equivalent Appendix K 1971 Standard Multilpiers
1.25
1.2
1.15
1.1
1.05
1
0.95
0.9
0.85
0.8
0.75
P/P(ANS71)
1.2X(1/1)
Current Appendix K multiplier
2/1
3/1
3a/1
4/1
1.0X(1/1)
8/1
5/1
6/1
7/1
Note: for this plot Case 1 is the ANS71 standard
without the 1.2 multiplier. All other cases are the same as Figure 1.
1
10
100
1000
10000
Time (seconds)
apk.94.71.f4b

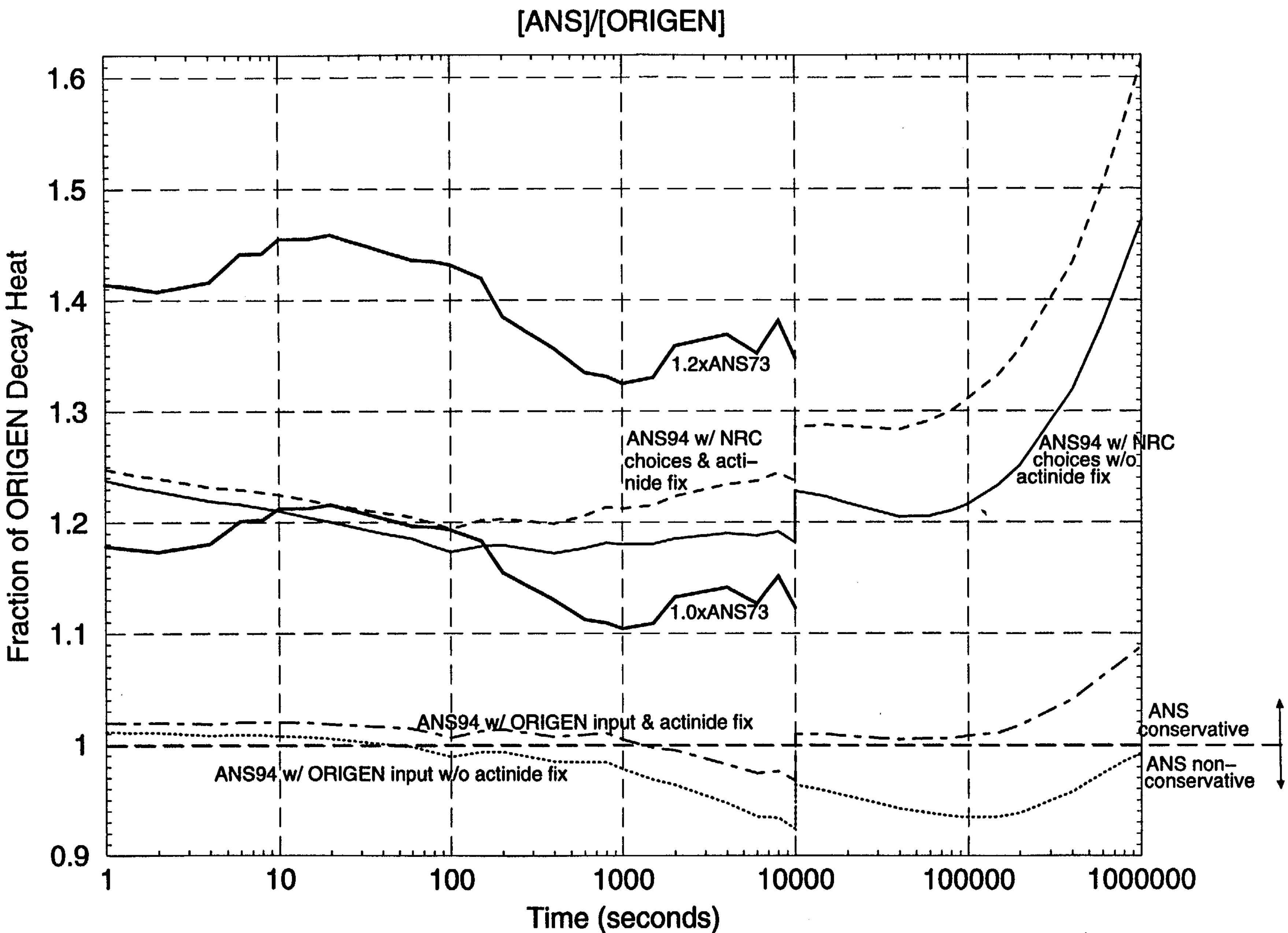
Percent of Decay Heat from Actinides*

(Excluding U239 and Np239)




Decay Heat Comparison
ORIGEN vs. ANS94 & ANS73
0 ORIGEN (17X17 PWR)
1 ANS94 (ORIGEN Input)
2 ANS94 (ORIGEN input & actinide correction)
3 ANS94 w/ choices & actinide correction
4 ANS94 w/choices & no actinide correction
5 1.2 X (ANS73 + U239 + Np239)
Fraction of Full Power
0.08
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
1
10
100
1000
10000
100000
1000000
Time (seconds)
origen.ans94.08

Decay Heat Comparison



RECOMMENDATIONS

- "Grandfather" the current Appendix K decay heat requirements.

- Add an Appendix K option to use the 1994 ANS standard with the following pre-selected choices, which are equivalent to Case 3a in Figures 3 and 4:

  1. Assume ^{235}U is the only fissioning isotope.
  2. Assume infinite operating time.
  3. Assume 200 MEV/fission recoverable energy.
  4. Use Equation 11 in the standard for neutron capture effect for shutdown times less than 10^4 seconds. Use 2.e8 seconds operating time for this equation. Use 1.0 as the value for Ψ.
  5. Use Table 13 in the standard for neutron capture for shutdown times greater than or equal to 10^4 seconds.
  6. Apply Section 4 in the standard for the decay heat contribution for ^{239}U and ^{239}Pu. Use a value of 0.7 for R.
  7. Use a 2σ value of uncertainty for ^{235}U based on the bounding curve of Figure 1. Along with options 1 and 2, this obviates the need to consider methods to combine uncertainties.

- Add another Appendix K option to allow use of a subsequent consensus standard and/or selection of user choices other than those shown above.

- Use of the new Appendix K options would be subject to a model review as required in 50.46. A model review is prudent to assure retention of sufficient remaining conservatism in any revised Appendix K model in which a substantial amount of conservatism has been removed. This subject is discussed in more detail by Steve Bajorek.

- Allow use of the 1994 ANS standard in best estimate Reg. Guide 1.157

Risk-Informed Revision of ECCS Evaluation Model Requirements (Appendix K)

Presentation to the ACRS Subcommittees on Materials and Metallurgy, Thermal-Hydraulic Phenomena, and Reliability & Probabilistic Risk Assessment

May 31, 2002

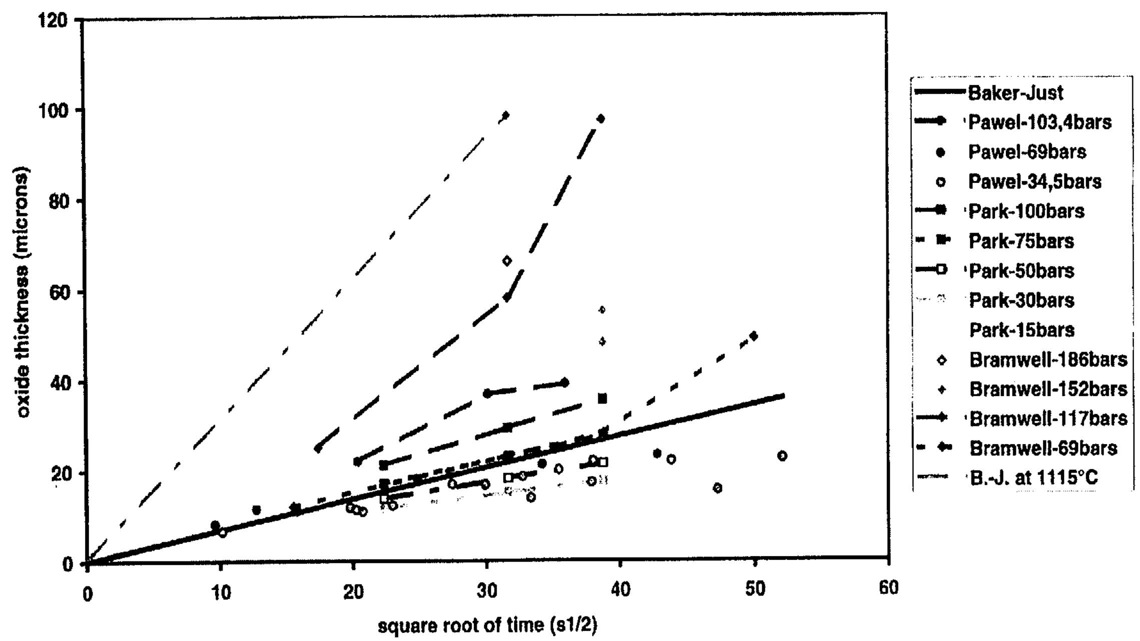
Stephen M. Bajorek
Safety Margins and Systems Analysis Branch
Division of Systems Analysis and Regulatory Effectiveness
Office of Nuclear Regulatory Research

Appendix K Modeling Requirements
Metal-Reaction Heat Release

- Original rulemaking assumed Baker-Just was conservative at 2000 ^{o}F, but was approximately correct at 2200 ^{o}F.

- Baker-Just equation based on pure Zr data - not alloys. Review of more recent data covering several different Zr based alloys shows low experimental data scatter and good agreement with Cathcart-Pawel.

- All Zr-based alloys exhibit about the same oxidation kinetics. Reason: Dominant rate-controlling step at high temperatures is diffusion of oxygen through ZrO_2 surface layer.



- **Experimental data however, exhibits enhanced oxidation rates at high pressure. Cathcart-Pawel correlation is non-conservative for heat release at high pressure.**

Recommendation:

The Baker-Just correlation for exothermic heat release can be replaced with the Cathcart-Pawel (at low pressures) or with a suitable realistic correlation shown applicable to a specific alloy. An adjustment to Cathcart-Pawel or other correlation if used at high pressure.

Appendix K Modeling Requirements
Steam Cooling Below 1 inch/sec

- Paragraph I.D.5.b. of Appendix K states that:

"During refill and during reflood when reflood rates are less than one inch per second, heat transfer calculations shall be based on the assumption that cooling is only by steam, ...

- Experimental data from FLECHT series of tests demonstrated high rates of entrainment & carryover, even for VIN < 1 ips.

Recommendation:

Delete the requirement for steam cooling only at reflood rates below 1 inch/sec.

Appendix K Modeling Requirements
Return to Nucleate Boiling During Blowdown

- Paragraph I.C.4.e. in Appendix K prohibits the return to nucleate boiling heat transfer even if the fluid and surface conditions apparently justify the return.

- Rewet during blowdown supported by LOFT experiments. However, overall database demonstrating blowdown rewet is sparse for Zr cladding and Tmin can be predicted only with very high uncertainty.

Recommendation:

Retain the prohibition on assuming a return to nucleate boiling during blowdown.

Appendix K "Non-Conservatisms"

Sources of potential non-conservatism:

1. **Thermal-hydraulic processes and fuel behavior that have been observed in experimental programs since 1973, but are not specifically addressed by Appendix K.**

2. **Large calculational uncertainties that are on the order of the overall conservatism of the EM. This was a main concern of SECY-86-318, ("Revision of the ECCS Rule Contained in Appendix K and Section 50.46 of 10 CFR Part 50) which recommended that the Appendix K decay heat guidelines not be revised unless model uncertainties were accounted for.**

Non-Conservative Processes Identified:

- **Downcomer Boiling**
- **Reflood ECC (Downcomer) Bypass**
- **Fuel Relocation**

◆ Downcomer Boiling

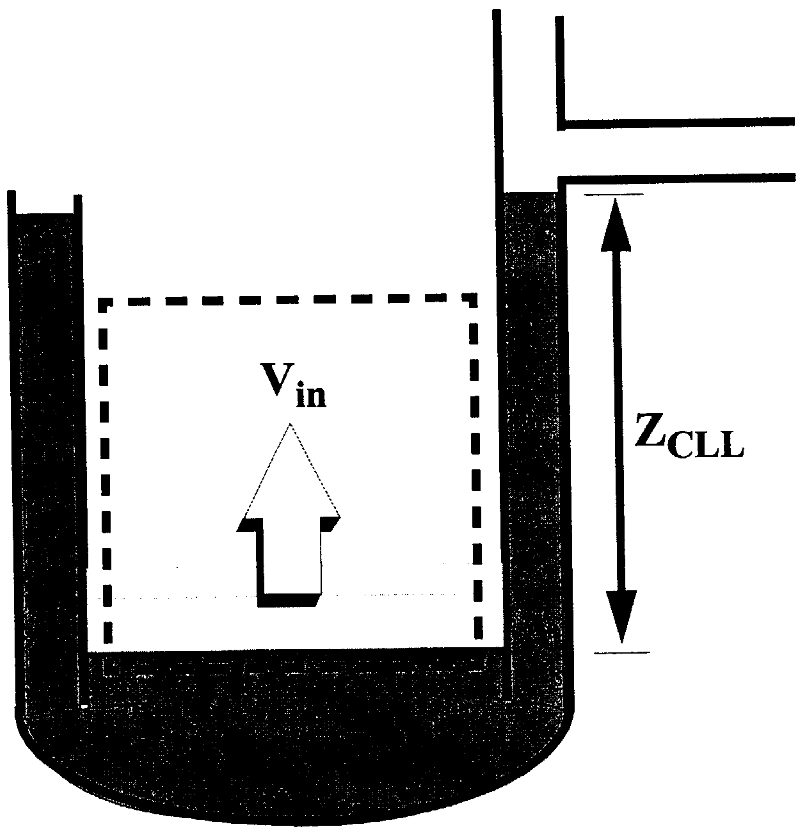
- Experimental data from several facilities, and simulations using "Best Estimate" thermal-hydraulic codes show that stored heat in vessel walls, core barrel and lower plenum structures can cause coolant in the downcomer to boil during reflood.

- Voiding in the downcomer can result in a significant reduction in downcomer head. This reduces the flooding rate and increases the PCT.

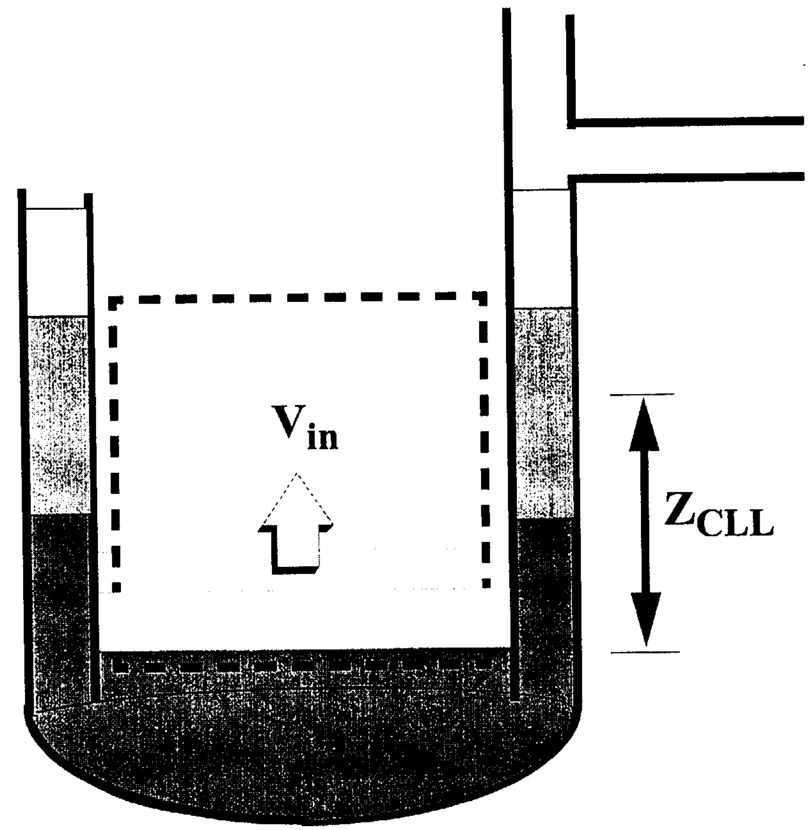
- PWR Appendix K reflood models do not model downcomer boiling. Yet, for at least some plants in all three PWR vendor designs, the existence of downcomer boiling has at least been acknowledged.

DOWNCOMER BOILING

Early in Reflood:
DC Fluid Subcooled

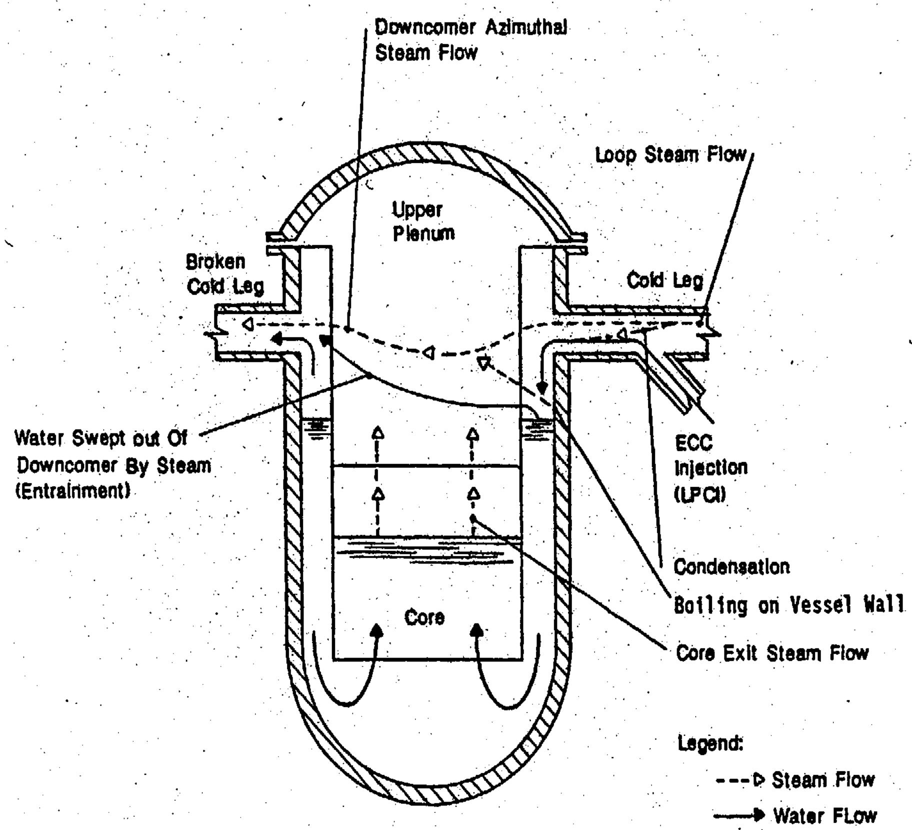


Late in Reflood:
Downcomer Boiling



Downcomer Boiling: Causes Net Loss of Driving Head & Reduces Reflood Rate

◆ Reflood ECC (Downcomer) Bypass



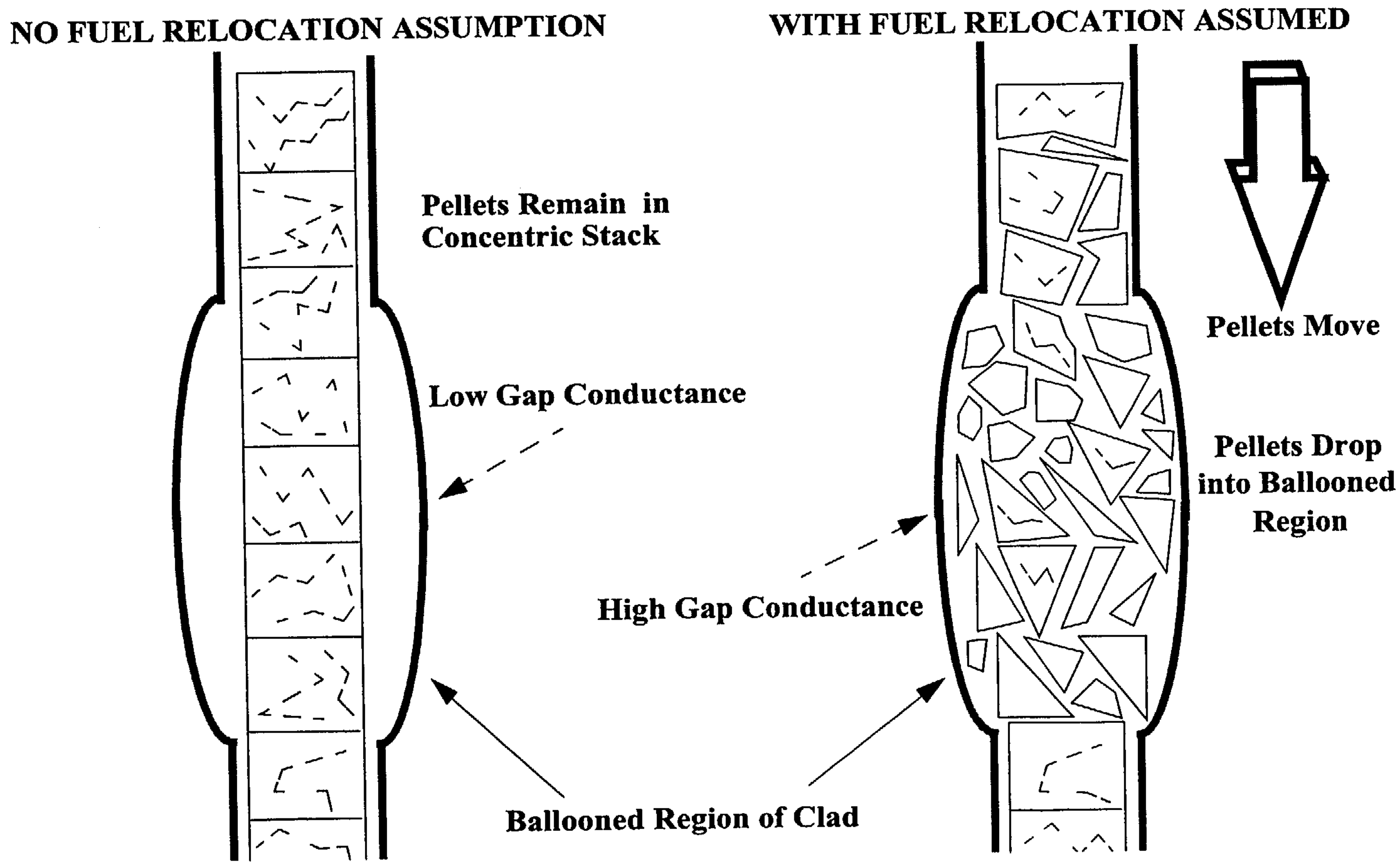
- Experimental tests in the full scale UPTF facility showed that steam from intact loops could entrain significant amounts of water from the downcomer during reflood.

- High entrainment and carryover to the break reduced the downcomer water level and can result in a reduction in downcomer head. This reduces the flooding rate and increases the PCT.

- Process is a strong function of the downcomer water level and oscillations.

◆ Fuel Relocation

- Experiments in PBF-LOC, FR2 (Germany) and FLASH5 (France) showed significant fuel movement in regions where clad has ballooned.
- Relocation of additional fuel into ballooned region increases local power and increases conductance between pellets and clad.



Estimation of Evaluation Model Significance

- Proposed revisions to Appendix K requirements will have major impact on predicted peak cladding temperature (PCT) and equivalent cladding reacted (ECR).

- Since large break LOCA is generally the most limiting accident scenario, more information is available on effect of changing decay heat, metal-water heat release etc. for that accident. Very little information is available for SBLOCA. Results are plant dependent.

- The following tables list documented sensitivities for various models & assumptions. Reference numbers are identified in the "Research Information Letter."

Table 1: Large Break LOCA ΔPCT Estimates

Process	ΔPCT	Basis/Comments
Decay Heat	-260 to -450 °F	Recent Westinghouse estimate based on App. K EM calculations [2]. ANS 1971 + 1.20 replaced with ANS 1979 + 2σ. Calculations performed using BASH-EM.
Decay Heat	-372 °F	NRC contractor RELAP calculations for CE 2700 MWt (Millstone 2) plant [3]. ANS 1971 + 1.20 replaced with ANS 1979 + 2σ.
Decay Heat	-460 °F	1984 Westinghouse study on Appendix K relaxation [4] .
Metal Water Reaction	-45 to -55 °F	Recent Westinghouse estimate assuming the Baker-Just correlation is replaced with Cathcart-Pawel for metal-water reaction heat [2]. Calculations performed using BASH-EM.
Metal Water Reaction	-75 °F	NRC contractor RELAP calculations with Baker-Just replaced by Cathcart-Pawel [3].
Metal Water Reaction	-65 °F	1984 Westinghouse study on Appendix K relaxation [4] .
Downcomer Boiling	+400 °F	Westinghouse estimate from Best Estimate EM calculations for W 4-loop PWR [5].
Downcomer Boiling	+810 °F	NRC contractor calculations using RELAP5 for a CE System 80+ (3800 MWt) unit [6].
Downcomer Boiling + Reflood Bypass	+63 °F	Estimate based on WCOBRA/TRAC calculations for an uprated CE System 80+ unit [7]. Both downcomer boiling and ECC bypass during reflood were found to be important and contribute to increases in PCT.

Table 1: Large Break LOCA ΔPCT Estimates

Process	ΔPCT	Basis/Comments
Fuel Relocation	+46 °F	EG&G estimate based on experimental tests in PBF (Power Burst Facility) to address Generic Safety Issue (GSI) 92 [8].
Fuel Relocation	+313 °F	Results reported in technical paper by IPSN [9] using CATHARE for a Framatome PWR (similar to a Westinghouse 3-loop PWR). A burst zone 70% filling fraction assumed.
Code Uncertainty	+340 °F	W ΔPCT between 95th and 50th percentile uncertainty in a W 4-loop PWR for WCOBRA/TRAC calculation [10].
Code Uncertainty	+300 °F	Difference between the 95th and 50th percentile PCTs for a Westinghouse RESAR-3S plant using TRAC-PF1/MOD1 [11].
Code Uncertainty	> +275 °F	Framatome ANP large break code uncertainty using realistic version of RELAP [12]
Code Uncertainty	> +400 °F	GE code uncertainty using SAFER/GESTER [13]

Table 2: Small Break LOCA ΔPCT Estimates

Process	ΔPCT	Basis
Decay Heat	- 1000 °F	NRC contractor citation of CE sensitivity to decay heat using CE EM for CE 2700 MWt (Millstone 2) plant [3].
Decay Heat	- 859 °F	NRC contractor citation of W sensitivity EM to decay heat standard for CE 2700 MWt (Millstone 2) plant [3].
Decay Heat	-500 to -1000 °F	NRC contractor estimate based on RELAP5 calculations for typical plants [3].
Decay Heat + Metal Water Reaction	-500 °F	Calculations performed using a SBLOCA version of WCOBRA/TRAC for Indian Point Unit 2 [14]. The ΔPCT is the difference between the limiting SBLOCA case in the paper and current plant (Appendix K based) analysis of record.
Metal Water Reaction	-11 to -76 °F	NRC calculations using RELAP with Baker-Just replaced by Cathcart-Pawel.
Fuel Relocation	Not known	Clad swell and rupture and fuel relocation may occur in SBLOCA. However, no calculations have been found documenting the effect.
Nodalization	+600 °F	NRC RELAP calculations w and w/o crossflow for CE 2700 MWt plant.
Operator Action	+ several 100 °F	Pump trip with off site power available depends on operator recognition and adherence to EOPs. This is a known post-TMI pump trip issue. Trip at inopportune time can cause deep uncovery.
Level Swell Uncertainty	+ several 100 °F	NRC contractor (verbal) estimate. Mixture level swell (code interfacial drag) is highly ranked PIRT process.
Loop Seal Clearance	+/- several 100 °F	Affects pressure drop through loop(s) and core level depression.

Recommendations:

A. Evaluation Models making use of a new, optional Appendix K should account for the non-conservatisms of downcomer boiling, downcomer ECC bypass, and fuel relocation.

B. These new Evaluation Models must demonstrate sufficient overall conservatism in their results.

Conclusions & Recommendations

1. Revise the 10 CFR 50.46 acceptance criteria for PCT and ECR to be "performance-based".
2. Replace 1971 ANS Decay Heat Standard with 1993 Standard
3. Replace the Baker-Just correlation with Cathcart-Pawel for metal-water reaction heat release.
4. Delete the requirement for steam cooling only at reflood rates below 1 inch/sec.
5. Retain the prohibition on assuming a return to nucleate boiling during blowdown.
6. Require that the new Evaluation Models to demonstrate sufficient overall conservatism and that they account for several identified non-conservatisms.

Planned Actions

1. Information presented will be documented in a "Research Information Letter" to NRR to provide a basis for pursuing new rulemaking consistent with SECY-01-0133 and SECY-02-0057.

2. RES is continuing to work with NRR to identify appropriate paths for removing unnecessary conservatisms in a new, optional Appendix K while insuring Appendix K retains sufficient conservative.

3. Public meeting is planned to discuss findings.

4. Continue the high burnup fuel research program, which is expected to provide supporting information on fuel relocation, cladding oxidation and cladding ductility/toughness following quench.

5. Resolve technical issues associated with 1994 ANS Decay Heat Standard uncertainty and user selected parameters.

Existing & Potential Analysis Options

Analysis Option	Is Approach "Performance Based" ?	How is Conservatism Assured?	EM Uncertainty
Appendix K (1973) (Grandfathered)	No - Appendix K is specific on: 1971 ANS + 20% Baker-Just Moody break flow Steam Cooling for VIN < 1 ips, etc... Several other models cited as acceptable, without specific requirement.	Several models and assumptions included in Appendix K to insure conservatism. (Pg. 1093 of Commission Opinion)	Not required.
Appendix K' (200x) (New)	TBD - Goal would be not to make specific requirements, but identify acceptable correlations. Some prescriptive features (Moody break flow, no return to nucleate boiling) may remain.	TBD	TBD. Staff to address per SECY-02-0057
Best Estimate (1988) (Grandfathered)	Yes - Existing BE rule is not prescriptive. RG 1.157 discusses "acceptable" models and data for correlation assessment without making a specific requirement.	50.46 requirement to demonstrate there is a "high probability that acceptance criteria would not be exceeded".	Required by 50.46 (a)(1)(i)

RISK-INFORMING 10 CFR 50.46

Presented to:
ACRS Subcommittees on Materials and Metallurgy, Thermal-Hydraulic Phenomena, and Reliability and Probabilistic Risk Assessement

Presented by:
Mark Cunningham, Mary Drouin, Alan Kuritzky, RobTregoning, Lee Abramson, Steve Bajorek, Norm Lauben and Sam Lee
U.S. Nuclear Regulatory Commission

May 31, 2002

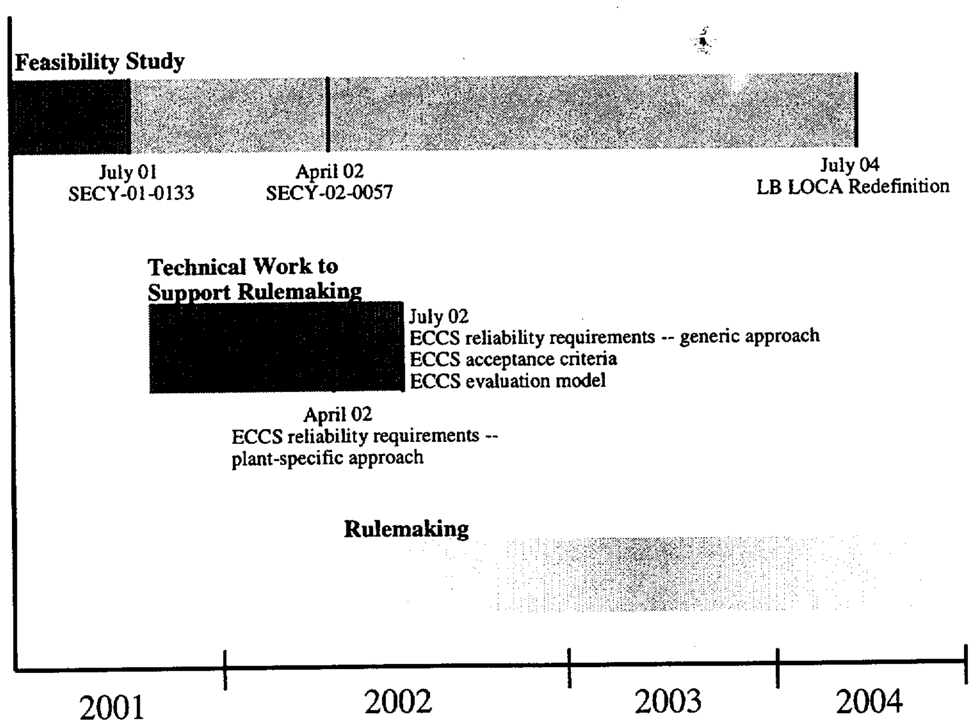
OUTLINE

- Purpose/goal of meeting
- Status and schedule
- Proposed changes to 10 CFR 50.46 (including Appendix K and GDC 35)
- Technical work to support rulemaking for changes to 10 CFR 50.46

PURPOSE/GOAL OF MEETING

- Provide status on staff's efforts to risk-inform 10 CFR 50.46
- Solicit feedback and comments from ACRS
- No letter requested (at this time)

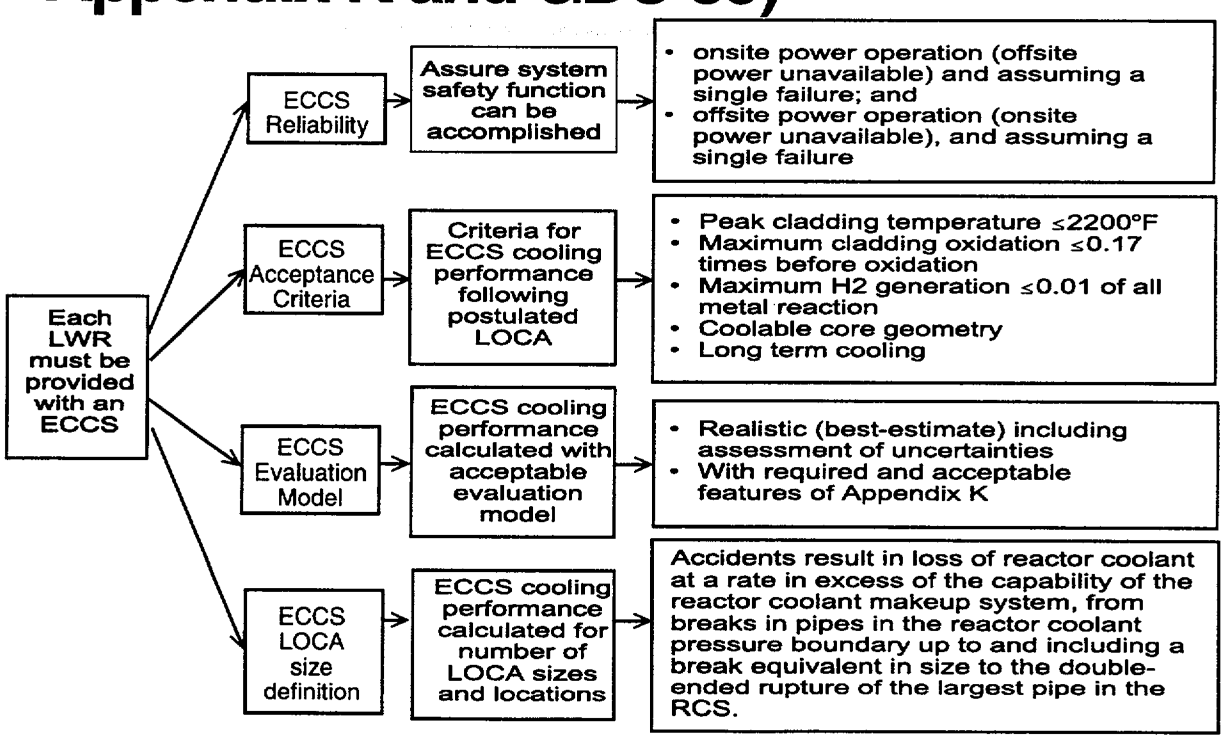
STATUS



JULY 2002 DELIVERABLE

- Memo from A. Thadani (RES) to S. Collins (NRR) will document technical work to support rulemaking for changes to:
  - ECCS reliability requirements (GDC 35)
  - ECCS acceptance criteria
  - ECCS evaluation model requirements

OVERVIEW OF 50.46 (including Appendix K and GDC 35)



PROPOSED CHANGES TO 50.46, APP. K AND GDC 35 (SECY-02-0057)

Staff recommendations on:

- ECCS reliability: Provide two voluntary performance-based options (one generic, one plant-specific) that would demonstrate reliable ECCS safety function without assuming LOOP and single additional failure in GDC 35
- ECCS acceptance criteria: Change current prescriptive ECCS acceptance criteria in 50.46 to add a performance-based option

PROPOSED CHANGES TO 50.46, APP. K AND GDC 35 (SECY-02-0057) (Cont'd)

Staff recommendations on:

- ECCS evaluation model: Add an option to App. K decay heat requirement to permit use of 1994 ANS standard
  - Staff intends to address subject of uncertainty and conservatism in App. K models separate from rulemaking activity
- ECCS spectrum of break sizes and locations: Continue the feasibility study of redefining the maximum pipe break size required to be considered as part of the ECCS performance evaluation

TECHNICAL WORK TO SUPPORT RULEMAKING FOR CHANGES TO 10 CFR 50.46

- ECCS reliability requirements
- ECCS spectrum of break sizes and locations
- ECCS acceptance criteria
- ECCS evaluation model requirements

TECHNICAL WORK TO SUPPORT RULEMAKING FOR CHANGES TO 10 CFR 50.46

- ***ECCS reliability requirements***
- ECCS spectrum of break sizes and locations
- ECCS acceptance criteria
- ECCS evaluation model requirements

PROPOSED CHANGES TO ECCS RELIABILITY REQUIREMENTS

Risk-Informed Alternative to GDC 35

- As part of proposed rulemaking, current approach of GDC 35 would be changed
- Revised approach would permit ECCS to be designed, operated or evaluated based on quantitative reliability considerations instead of prescriptive assumptions on loss of offsite power (LOOP) and additional single failure

PROPOSED CHANGES TO ECCS RELIABILITY REQUIREMENTS

Scope and Limitations

- Proposed changes apply only to ECCS requirements (GDC 35)
  - No changes proposed to requirements for containment design or equipment qualification
  - Changes to single failure criterion not generically extended to other systems
  - E.g., no changes to GDCs 17, 34, 38, 41 and 44
- Performance monitoring and corrective action strategies may need to be developed for specific applications

PROPOSED CHANGES TO ECCS RELIABILITY REQUIREMENTS

Approaches for Risk-Informed Alternative to GDC 35

- In place of loss of offsite power and additional single failure assumptions in current GDC 35, two options would be offered in a Regulatory Guide to ensure ECCS safety function reliability:

  1. Plant-specific approach where licensees, with appropriate consideration of uncertainties, demonstrate compliance with NRC-established acceptance guidelines, ***OR***

  2. Generic approach where a minimal set of ECCS equipment required to meet NRC-established acceptance guidelines would be specified by the NRC, by generic plant group.

- Approaches based on Option 3 framework

PLANT-SPECIFIC APPROACH FOR RISK-INFORMED ALTERNATIVE TO GDC 35

- Technical work included:
  - Determining proposed CDF and LERF acceptance guidelines
  - Determining acceptable LOCA frequencies (ongoing)
  - Developing possible method for plant-specific calculation of conditional probability of LOOP given LOCA

PROPOSED CDF AND LERF ACCEPTANCE GUIDELINES

- Two types of licensee-proposed ECCS-related changes envisioned:
  - Changes in ECCS design or operation (e.g., removal of a piece of equipment or relaxation of technical specifications)
  - Changes in the ECCS design basis (e.g., removal of an accident from the ECCS design basis analyses)

PROPOSED CDF AND LERF ACCEPTANCE GUIDELINES

Design/Operational Changes

- Need to demonstrate that ECCS functional reliability is commensurate with frequency of accidents for which ECCS success would prevent core damage or large early release
- Can be accomplished by demonstrating that the following acceptance guidelines are met:

  (1) Baseline total plant CDF and LERF meet quantitative guidelines specified in Option 3 framework, AND
  (2) Resulting change in risk from proposed ECCS-related change does not represent a significant risk increase

PROPOSED CDF AND LERF ACCEPTANCE GUIDELINES

Design/Operational Changes (Cont'd)

- Option 3 framework specifies CDF and LERF thresholds of 1E-4/yr and 1E-5/yr, respectively
  - Since values apply to full-scope PRA, total plant CDF and LERF need to be determined or addressed
  - Thresholds are flexible, consistent with RG 1.174
- RG 1.174 acceptance criteria used to limit change in risk, since Option 3 framework only specifies absolute risk guidelines
- Consistent with Option 3 framework, quantitative guidelines are only one part of risk-informed defense-in-depth approach
  - Defense-in-depth principles cannot be violated

PROPOSED CDF AND LERF ACCEPTANCE GUIDELINES

Design Basis Changes

- Proposed change must meet Option 3 framework and RG 1.174 criteria, same as for other types of changes
- Change in CDF and LERF are determined by assuming plant can no longer respond to the subject accident (i.e., subject accident assumed to lead directly to core damage)

PROPOSED CDF AND LERF ACCEPTANCE GUIDELINES

PRA Scope and Uncertainty Analysis

- Acceptance guidelines are intended for comparison with results of full-scope PRA
- Significance of out-of-scope items needs to be addressed
- Consistent with RG 1.174, mean values should be used to compare with the acceptance guidelines
- Formal propagation of uncertainties should be performed, where possible
  - Supplement with sensitivity studies or qualitative arguments, where necessary

LOCA FREQUENCY ESTIMATION

- Need to consider LOCA initiating events and transient-induced (or consequential) LOCAs
- LOCA initiating events include pipe-break LOCAs and non-pipe-break LOCAs (e.g., SG manway failure)
- Causes and frequencies of transient-induced LOCAs and very small LOCA initiating events are relatively well understood
- Causes and frequencies of medium and large LOCA initiating events (>~2 in.) are not as well understood

LOCA FREQUENCY ESTIMATION (Cont'd)

- Sources of medium/large LOCA frequencies in PRAs
  - WASH-1400/NUREG-1150
    - ❒ Based on old data, most not applicable to nuclear power plants
  - NUREG/CR-5750
    - ❒ Based on recent operating experience, some technical issues raised
- Several concurrent studies to evaluate LOCA distributions
  - Short-term: quick, in-house elicitation
  - Intermediate-term: formal expert elicitation
  - Longer term: redefine spectrum of pipe break sizes

CONDITIONAL PROBABILITY OF LOOP GIVEN LOCA

- In typical PRAs, occurrence of LOOP following a LOCA is assumed to be random, independent event

- More recent analysis (NUREG/CR-6538) concludes that a dependency exists

- Extremely limited data for consequential LOOP following a LOCA or major ECCS actuation (surrogate for LOCA)

CONDITIONAL PROBABILITY OF LOOP GIVEN LOCA (Cont'd)

- Plant-specific method for assessing conditional probability of LOOP given a LOCA provided in RES report (App. D)
- Continuing to work with industry on alternative approaches for quantifying conditional probability of LOOP given a LOCA
  - Industry expert elicitation
  - Staff review

GENERIC APPROACH FOR RISK-INFORMED ALTERNATIVE TO GDC 35

- Technical work includes:
  - Formulating plant groups
  - Performing reliability/risk calculations
    - PRA scope and quality issues
- List of minimum required ECCS equipment and need to consider LOCA-LOOP would likely appear in regulatory guide
- Plant equipment in excess of the minimum determined above, would be candidates for design or operational changes

TECHNICAL WORK TO SUPPORT RULEMAKING FOR CHANGES TO 10 CFR 50.46

- ECCS reliability requirements
- ***ECCS spectrum of break sizes and locations***
- ECCS acceptance criteria
- ECCS evaluation model requirements

LOCA FREQUENCY AND LB LOCA BREAK SIZE REDEFINITION

Presented to the ACRS Subcommittees on Materials & Metallurgy, Thermal-Hydraulic Phenomena, and Reliability & Probabilistic Risk Assessment

Presented by
Rob Tregoning, Lee Abramson
U.S. Nuclear Regulatory Commission

May 31, 2002

LOCA REEVALUATION: PROGRESS SINCE PREVIOUS ACRS BRIEFINGS

- Previous ACRS briefings
  - March, 2001: Last substantive briefing on LOCA technical issues which initiated reevaluation.
  - June, July, November, 2001: Only overviews of LOCA effort provided to outline its importance within 10 CFR 50.46 revision framework.
- Progress Since March 2001
  - Developed technical position paper documenting issues to address for LOCA reevaluation.
  - Formulated approach for realizing near-term and long-term goals outlined within SECY-01-0133 (later SECY-02-0057).
  - Completed near-term elicitation to develop interim LOCA frequencies.
  - Public interaction with stakeholders: August 2001, October 2001, and March 2002.

LOCA REEVALUATION: EXECUTIVE SUMMARY

- Historical LOCA estimates have been based on service history experience.
- There are several potential LOCA initiating failure events which were not part of the service history based estimates (e.g. VC Summer, Oconee, and Davis Besse).
- MEB has initiated several concurrent studies to evaluate LOCA frequencies.
  - Near-Term (Complete) : developed interim LOCA frequency distributions by staff expert panel. Results were 2 to 4 times higher than NUREG/CR-5750 estimates.
  - Intermediate-Term (within one year): develop final LOCA frequency distributions through formal elicitation process using a panel of academic, industry, and government experts.
  - Longer-Term (2 years): redefine the spectrum of pipe break sizes to consider ECCS capability changes within existing RI-ISI framework.

LOCA REEVALUATION: MOTIVATION

- NRC is investigating risk informed changes to the following ECCS areas within 10 CFR 50.46:
  - ECCS Reliability.
  - ECCS acceptance criteria.
  - ECCS evaluation model.
  - ECCS spectrum of break sizes and locations.
- **LOCA frequency distribution impacts ECCS reliability (near-term effort) and the ECCS spectrum of break sizes and locations (longer-term effort).**

LOCA REEVALUATION: OVERVIEW

Several concurrent studies initiated to evaluate LOCA frequencies.

- **Near-term elicitation (by April 30, 2002): support ECCS reliability revision and initiation of rulemaking (SECY-02-0057).**
- Intermediate-term elicitation (within one year): support final rulemaking decisions.
- Longer-term (by June 2004): redefine the spectrum of break pipe sizes.

LOCA REEVALUATION: NEAR-TERM ELICITATION

- Eleven staff (5 NRR, 6 RES) chosen to obtain broad expertise in relevant technical areas: probabilistic risk assessment; the ASME code; structural mechanics; thermo-hydraulics; piping systems; seismic, thermal and vibrational loading; environmentally assisted cracking; thermal aging; and alternative LOCA mechanisms
- Objectives:
  - Adjust NUREG/CR-5750, Appendix J frequency distributions to account for other LOCA contributions not considered in original study.
  - Prioritize issues and questions which potentially provide the greatest additional contributions to LOCA frequency estimates. These issues will be considered during the formal elicitation process.

NEAR-TERM ELICITATION: APPROACH

- Kick-off meeting.
  - Provide background for NUREG/CR-5750, Appendix J LOCA estimates
  - Present technical concerns and motivation for updating frequencies.
- Issue development meeting.
  - Develop definitions and baseline case.
  - Identify important initiating mechanisms, systems, and components.
  - Identify important factors affecting future LOCA frequencies.
- Elicitation questionnaire.
  - Decompose technical issues.
  - Evaluate expected changes up through license renewal.
  - Obtain rationale for quantitative responses.
- Wrap-up meeting.
  - Present results and summarize important findings.
  - Obtain feedback for intermediate-term elicitation.

NEAR-TERM ELICITATION: ISSUE DEVELOPMENT

BWR LOCA Contributing Systems

ID	LOCA Initiating System	Materials	Failure Mechanisms	(S, M, L) LOCA Contributions
B1	Jet Pump Risers	IN 182/600	IGSCC	S, M, L
		SS Weld	IGSCC	
		Wrought SS	IGSCC	
B2	Recirculation Loops	IN 182/600	IGSCC	S, M, L
		SS Weld	IGSCC	
		Wrought SS	IGSCC	
B3	Core Spray	IN 182/600	IGSCC	S, M, L
		SS Weld	IGSCC	
		Wrought SS	IGSCC	
B4	RHR/LPI	SS Weld	IGSCC, THFAT	S, M, L
		Wrought SS	IGSCC, THFAT	
B5	Feedwater	Carbon Steel	THFAT, FAC	S, M, L
B6	Drain Lines	SS Weld	IGSCC, THFAT, MEFAT	S, M
		Wrought SS	IGSCC, THFAT, MEFAT	
		Carbon Steel	THFAT, MEFAT	
B7	RWCU	IN 182/600	IGSCC, THFAT	S, M
		SS Weld	IGSCC, THFAT	
		Wrought SS	IGSCC, THFAT	
		Carbon Steel	THFAT, FAC	
B8	Instrument Lines	SS Weld	IGSCC, MEFAT, TGSCC	S
		Wrought SS	IGSCC, MEFAT, TGSCC	
B9	SRV		Stuck Open Relief Valves	S, M
B10	External Events		Failure caused by human error (bumping) instrument lines	S
B11	ISLOCA		Failure of Class I interfacing system	S, M, L

- Bin piping systems by functionality, material, potential degradation mechanisms, loading history, and transient similitude.
- Discus LOCA potential of other (non piping) components.
- Examine global issues which influence all systems equally (RI-ISI, leak detection threshold, future degradation mechanisms, and mitigation).

NEAR-TERM ELICITATION: QUESTIONNAIRE

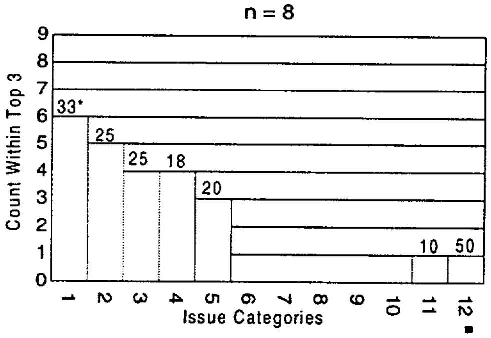
BWR LOCA "Relative Change" Table

ID	System or Components	SBLOCA (%)	MBLOCA (%)	LBLOCA (%)	Rationale and Comments
B1	Jet Pump Risers				
B2	Recirculation Loops				
B3	Core Spray				
B4	RHR/LPI				
B5	Feedwater				
B6	Drain Lines			NA	
B7	RWCU			NA	
B8	Instrument Lines		NA	NA	
B9	SRV			NA	
B10	External Events		NA	NA	
B11	ISLOCA				

- Each panel member completed an individual questionnaire.
- Evaluated relative changes in frequencies over next 35 years.
- Separately considered SB, MB, and LB LOCA changes.
- Utilized quantitative responses and rationale to determine most important LOCA contributors and LOCA frequencies.
- Combined responses in several ways (absolute changes, ratios, global changes) to conduct sensitivity analysis.

NEAR-TERM ELICITATION RESULTS: BWR LB LOCA CONTRIBUTORS

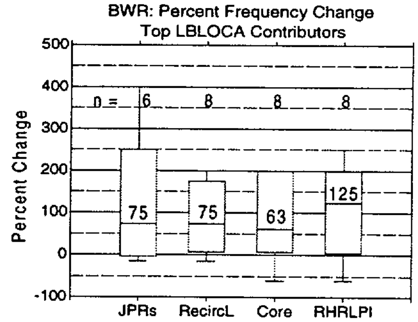
BWR: LBLOCA Contributing Factors
n = 8



B1: Jet Pump Risers
B2: Recirculation Loops
B3: Core Spray
B4: RHR/LPI
B5: Feedwater
B6: Drain Lines (NA)
B7: RWCU (NA)
B8: Instrument Lines (NA)
B9: SRV (NA)
B10: External Events (NA)
B11: ISLOCA
B12: Stub Tubes

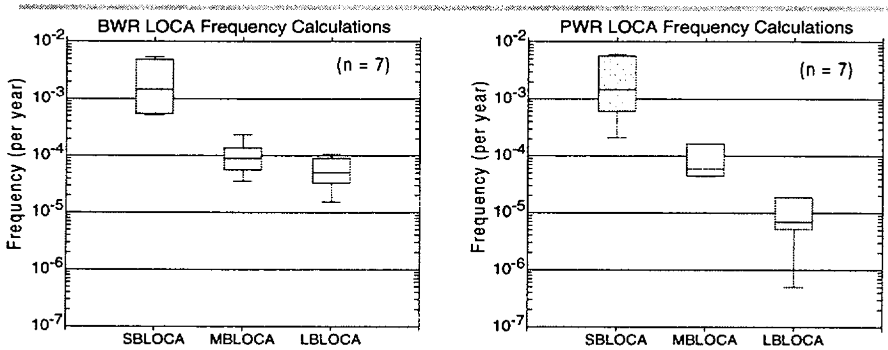
*median of non-zero respondents

BWR: Percent Frequency Change
Top LBLOCA Contributors



- Large class-1 diameter piping failures expected to dominate LB LOCA freqs.
- Frequency increases expected to be relatively independent of system.
- More variability than MB LOCA changes; similar to SB LOCA.

NEAR-TERM ELICITATION RESULTS: INTERIM LOCA FREQUENCIES



- Larger variability in SBLOCA numbers driven by the non-piping initiating components.
- Variability among estimates is generally less than an order of magnitude.
- Initial 5750 differences between the BWR and PWR MB and LB LOCA frequencies are retained.

NEAR-TERM ELICITATION RESULTS: INTERIM LOCA FREQUENCIES

Comparison of Mean Results with Previous Studies (per Reactor Calendar Years)				
Reactor Type	**Analysis**	**SBLOCA**	**MBLOCA**	**LBLOCA**
BWR	**Current**	**15×10^{-4}**	**9×10^{-5}**	**5×10^{-5}**
	NUREG-5750	4×10^{-4}	3×10^{-5}	2×10^{-5}
	WASH-1400	30×10^{-4}	80×10^{-5}	30×10^{-5}
PWR	**Current**	**15×10^{-4}**	**6×10^{-5}**	**7×10^{-6}**
	NUREG-5750	4×10^{-4}	3×10^{-5}	4×10^{-6}
	WASH-1400	30×10^{-4}	80×10^{-5}	30×10^{-5}
Comparative Increase in 5750 Results				
BWR		3.7	3.0	2.6
PWR		3.7	2.0	1.7

- Interim results fall between NUREG/CR-5750 and WASH-1400 estimates.
- MB and LB LOCA frequencies are closer to the NUREG/CR-5750 estimates.

NEAR-TERM ELICITATION: CONCLUSIONS

- LB LOCA frequency < MB LOCA frequency < SB LOCA frequency.
- Dominant initiators are apparent for SB and LB LOCA frequencies for both BWR and PWR systems.
- The effect of other (non-piping) component failure is important for SB LOCAs, and to a lesser extent for MB LOCAs.
- The effects of the global issues explicitly considered was not significant in terms of the median update. However, there was substantial difference of opinion about the role of future mechanisms & mitigation, ISI, and hydrogen combustion.

NEAR-TERM ELICITATION: CONCLUSIONS, cont.

- The SB LOCA 5750 frequencies are expected to change to the greatest extent. This is a direct reflection of the addition of the failure of non-piping components.
- Failure of piping components is expected to increase in the future to a greater extent than non-piping components.
- Aging mechanisms are expected to substantially affect the LOCA frequencies in the future.
- Failure without a precursor event is a significant consideration.

LOCA REEVALUATION: OVERVIEW

Several concurrent studies initiated to evaluate LOCA frequencies.

- Near-term elicitation (by April 30, 2002): support ECCS reliability revision and initiation of rulemaking (SECY-02-0057).
- **Intermediate-term elicitation (within one year): support final rulemaking decisions.**
- Longer-term (by June 2004): redefine the spectrum of break pipe sizes.

LOCA REEVALUATION: INTERMEDIATE-TERM ELICITATION

Process

- Designed and implemented by NRC elicitation team with contractual support provided by Battelle and Emc^2.
- Panel to be solicited from non-NRC participants from industry, academia, contracting agencies, other government agencies, and international agencies.
- Panel members to represent the full range of relevant technical specialties.
- The elicitation process utilized in the flaw distribution determination for 50.61 (PTS) reevaluation will be used as a model.

LOCA REEVALUATION: INTERMEDIATE-TERM ELICITATION

Service History Baseline

- The SKI-pipe database will serve as pipe break baseline.
- This database will be updated through the CSNI-sponsored OPDE project.
- PRA estimates for other LOCA initiating failures and components (e.g. SRV/PORV, pump seal, ISLOCA, steam generator tube) will be combined.
- Relevant information from other industries (e.g. commercial fossil plants, petrochemical plants, oil and gas transmission) can be utilized to provide bounding estimates.
- Elicitation will be utilized to determine if any modifications to the service history baseline are required.

LOCA REEVALUATION: INTERMEDIATE-TERM ELICITATION

Updated LOCA Frequency Development

- Probabilistic fracture mechanics modeling will be utilized to base expectations on future changes in the LOCA frequencies resulting from aging mechanisms.
- ISI and mitigation strategies will be factored into the final result based on historical strategies and effectiveness.
- The effect of unique events and the emergence of additional mechanisms will also be considered.
- All decomposed contributors will be analytically recombined to determine the final LOCA frequencies from the elicitation process.

LOCA REEVALUATION: OVERVIEW

Several concurrent studies initiated to evaluate LOCA frequencies.

- Near-term elicitation (by April 30, 2002): support ECCS reliability revision and initiation of rulemaking (SECY-02-0057).
- Intermediate-term elicitation (within one year): support final rulemaking decisions.
- **Longer-term (by June 2004): redefine the spectrum of break pipe sizes.**

LOCA REEVALUATION: PIPE BREAK SIZE REDEFINITION

- **Objective:** determine the maximum pipe break size to use as design basis accident.
- **General Approach:**
  - Couple state-of-the-art fracture mechanics modeling with understanding of historical, recent, and potential degradation mechanisms to determine the likelihood of a double ended guillotine break in the largest primary system pipes.
  - Utilize philosophy consistent within current risk-based guidelines to determine the maximum allowable pipe break size.
- **Support:** Contract to be initiated with Battelle/Emc^2 (May or June 2002).
- **Goal:** completion by June 2004 as outlined in SECY-02-0057.

LOCA REEVALUATION: PIPE BREAK SIZE REDEFINITION

Necessary Technical Advancements

- Evaluate and update as needed probabilistic fracture mechanics (PFM) models and codes to include latest deterministic models for accurately modeling pipe-failure mechanisms.
- Utilize, where possible, realistic loading histories and frequencies. Also combine these loads with realistic/conservative residual stress distributions and pipe boundary conditions.
- Incorporate up-to-date material aging and environmental effect models to account for material degradation.

LOCA REEVALUATION: PIPE BREAK SIZE REDEFINITION

Necessary Technical Advancements

- Develop scheme to incorporate potential/surprise future failure mechanisms based on service history experience.
- Consider the effect of failure from transients (earthquake, thermal) and their event frequencies as well as from normal operating loads.
- Update fabrication flaw distributions developed for RPVs to reflect expected differences in piping manufacture. Also consider flaw initiation for relevant mechanisms.
- Assess likelihood of LB LOCA from other initiating failure modes and combine with LB LOCA frequencies from pipe failures.

TECHNICAL WORK TO SUPPORT RULEMAKING FOR CHANGES TO 10 CFR 50.46

- ECCS reliability requirements
- ECCS spectrum of break sizes and locations
- ***ECCS acceptance criteria***
- ECCS evaluation model requirements

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