UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

REACTOR FUEL SUBCOMMITTEE MEETING

Nuclear Regulatory Commission
Room 1130
1717 H Street, N.W.
Washington, D. C.

Wednesday, September 3, 1980

The meeting of the subcommittee was convened, pursuant to notice, at 1:00 p.m.

ACRS STAFF PRESENT:

PAUL G. SHEWMON, Chairman
J. CARSON MARK
STEPHEN LAWROSKI
WILLIAM M. MATHIS
PAUL BOEHNERT

NRC STAFF:

W. JOHNSTON
N. LAUBEN
R. MEYER
M. PICKLESIMER

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ALSO PRESENT:

A. STRASSER, Consultant
V. ESPOSITO, Westinghouse
D. BURMAN, Westinghouse
L. HOCHREITER, Westinghouse
R. MUENCH, Westinghouse

PROCEEDINGS

(1:00 p.m.)

MR. SHEWMON: This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Reactor Fuel. I am Paul Shewmon, subcommittee chairman. The other members present today are Messrs. Mark and Mathis, Lawroski may well make it up here. We have in attendance a consultant, Alan Strasser.

The purpose of this meeting is to complete review of NUREG 0630, clad, swelling and rupture models for LOCA analysis. The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and Government Sunshine Act. Mr. Paul Boehnert is the designated federal employee for the meeting.

The rules for participation in today's meeting have been announced as part of the notice for the meeting in the Federal Register, August 19. A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that each of you speak loudly enough and identify yourself so that we can get your words for posterity.

We have received no written comments or requests for time to make oral statements from members of the public.

This is the second meeting at least that we have had on this. It has to do with the criteria that will be used to design against or provide against the blockage of a core by the ballooning of clad. The criteria will certainly make things neater for the staff. Whether they will make it significantly harder for the industry is a problem, and what additional conservatisms it would or wouldn't bring in will be questions for the subcommittee to look at.

Also, there is a substantial extrapolation from a lot of single pin data to worrying about subassemblies and cores which I am looking forward to hearing about again.

I think that is probably my introductory comments. I guess W. Johnston will start and partly tell us what staff proposes to do and what they would like out of it.

MR. JOHNSTON: Thank you, Dr. Shewmon and members of the subcommittee. I am William Johnston, Chief of the Core Performance Branch in NRR.

For the committee as part of the continuing discussion of NUREG 0630, we have several presentations. The schedule that you have before you suggests that Norm Lauben will speak first. However, he has not made the transit yet down from Bethesda, and we don't know where he is. So we will make a change in schedule.

We do wish to discuss the general procedures by which one utilizes how one makes the ECCS calculations and put the cladding calculations in the total context of these calculations, and we were going to lead off with Norm doing that. However, the change is going to be that Ralph Meyer will speak first and talk about the new cladding models that are in 0630. He will comment on the comments that have been made since the original version of 630 came out and which have come in since the February meeting that was held with this subcommittee.

Following Ralph's discussion, if Norm has come in, Norm Lauben will be on next. If not, Dr. Picklesimer from Research, who has been also looking into this and considering some different ways of arriving at the conversion of single rods to bundle blockages, will present an alternative model which he has recently come up with and make some additional comments on some other methods of doing this.

But basically he is going to be speaking mostly to only one aspect of the three models that are incorporated in 0630.

Finally, following that, and not appearing on your thing, we do expect Norm Lauben again to give a summary of our implementation schedule and the ways that we will approach these things.

I would like to emphasize in the beginning that there is no intention on our part to be proscriptive as to precisely how one must arrive at a suitable estimate of the blockage, but what has been contained in 0630 is one possible way to do it and it gives a kind of a point of reference that the staff can use. But we do wish to encourage the industry and other people to come up with other ways of accomplishing the same thing.

The only criteria I think that we would apply, that I want to emphasize right now, is that the result has to result in an aerial flow blockage that looks similar to the data plot on Figures 14 and 15 in 0630, which is really drawing a data point of a curve through the existing bundle blockage experimental information.

So basically what we are saying is that the final result, however arrived at, should be consistent with the world's existing bundle blockage data. And that would be our fundamental point. And with that that really concludes what I wished to say by way of introduction. I don't believe Norm Lauben has arrived, so --

MR. SHEWMON: Would you give me a short lecture for a minute? I am slowly but too slowly learning the difference between staff technical positions and Reg Guides and rules. If this gets enunciated, it would be in what form?

MR. JOHNSTON: I am sorry, I did leave out a portion, didn't I? I may well ask Dr. Rubenstein to comment on that in particular because he is more familiar with that aspect of things than I am, since I have only been doing this for a couple of months.

MR. RUBENSTEIN: I believe you are getting to the very bottom line of how we would implement our policy. We propose to write a letter from probably Paul Check, Assistant Director for Plant Systems, to the vendors, specifying some constraints on how the evaluation models should be done in the particular area of swelling and rupture.

We would then notify licensees that effective in a given date, and we will come back to this at the end of the meeting. The calculations should be dealt with in the new and approved evaluation model. And I will give you the punch line; we are talking about an implementation schedule which starts evolving through early 1982.

I would expect that if the staff's model were adopted, and we would want a letter from you expressing your views on this matter, we would then shortly thereafter publish a letter saying that effective January 1st, 1981 we would expect a revised evaluation model from each of the vendors taking into account the critical elements of NUREG 0630 and offering to the staff for review countervailing analytical models in the thermohydraulics area, which would be compensating for any changes which would be perhaps in a more conservative area, which we will discuss.

MR. SHEWMON: Well, let me come back. I offered you three options, and you didn't take any of them. I said staff technical position, reg guide or rules, and you are saying we put an --

MR. RUBENSTEIN: No, a staff technical position.

MR. SHEWMON: Okay, that is a staff technical position, and you can say that when new things -- new core loadings for example -- come in for review they must meet the staff technical position?

MR. RUBENSTEIN: Right. That would be the third step of course. We would say effective January 1st, 1981 we would like you to come in with your new models, taking into account a sort of open season on thermal hydraulics, compensating changes; the staff would commit the resources to review this through the 1981 chronological year, expecting that the review would be completed by the end of 1981, that we would have another balanced overall, appropriately conservative evaluation model. We would then expect on an implementation schedule wherein each plant, as it came to the staff, starting in say January 1st, 1982, for review and approval would have been calculated with the new vendors' evaluation models, and the staff would have approved them by then and the plants in a rather reasonable manner, for many reasons which we will discuss today, which talk to the lack of urgency or substantive safety impact, or immediate safety impact because there are other compensating measures being taken now in the reactors through power distribution techniques, we feel that we can do this in a deliberate process.

MR. LAWROSKI: What happens to the matter that a new core loading is only partial?

MR. RUBENSTEIN: Well, that is automatically incorporated in the ECCS analysis. It is essentially the new third or quarter of the core which has the peak -- -- or the power.

We do have a presentation, as Dr. Johnston said, which Mr. Lauben will present, which shows in the appropriate context in the evaluation models the role of each of the three principal fuel-related models. We want to review that for you and I apologize for him not being here on time.

MR. SHEWMON: Fine, okay. Are you on, Ralph?

MR. MEYER: I am Ralph Meyer, Section Leader of the Reactor Fuel Section in the Office of Nuclear Reactor Regulation.

Three correlations were described in NUREG 0630 for cladding rupture temperature, burst strain and assembly flow blockage. We have discussed these three correlations and our intended use of them with the ACRS previously and several questions were raised.

I believe that my first slide summarizes the most important questions that have come up, and I intend to address these five questions during the next half an hour or so. So instead of simply starting from scratch and describing step by step the contents of NUREG 0630 I am going to address the main questions that have come up.

I am not even going to do that right now because Norm Lauben has just come in the door, and --

MR. SHEWMON: Are you ready to go to bat, or do you want to collect your wits for a minute?

MR. MEYER: -- so I will interrupt this premature presentation and we will get back on schedule with Norm Lauben.

MR. SHEWMON: Okay.

MR. LAUBEN: Ralph has probably already told you that he is going to follow me.

What I do want to discuss today, and I think it might help to clarify some of the fuels-related issues, is to discuss the ECCS licensing calculations in the cladding model and how they interact. I will discuss the related features of Appendix K, the swelling and rupture effects; that is, what the swelling and rupture do in the calculation. I will discuss how the computer models have evolved in this respect. I will discuss the effect of strain and incidence and what the effect -- in this respect

I mean is it important, what does it do and so forth -- and the same with blockage.

And then of course Ralph will discuss the specific features of the cladding models which he started to do before I came in.

On your handout there is an additional slide, the very last slide, which will discuss implementation and schedules for the NUREG 0630 models.

Now, whenever we talk about licensing calculations for ECCS we must of necessity talk about 10 CFR 5046 and Appendix K, and whatever its faults may be or its strong points it is the law, and it is what we in licensing have to measure. It is the standard for ECCS calculations by which to measure.

So what, where in Appendix K are the things related to swelling and rupture addressed? The first one is the one that Ralph has mentioned to you several times in the past, paragraph 1(b), and it is simply entitled "Swelling and Rupture of Cladding and Fuel Rod Thermal Parameters." What does it tell us?

Well, I think it wouldn't be a bad idea to go through and read word for word what it does say. (reading) Each evaluation model shall include a provision for predicting cladding swelling, and rupture from consideration of the axial temperature distribution of the cladding and from the difference in pressure between the inside and the outside of the cladding, both as functions of time.

To be acceptable the swelling and rupture calculations shall be based on applicable data.

I guess in my slide I have that underlined -- based on applicable data -- in such a way that the degree of swelling and incidence of rupture are not underestimated. The degree of swelling and rupture shall be taken into account in calculations of gap conductance, cladding oxidation and embrittlement and hydrogen generation.

The calculations of fuel and cladding temperatures as functions of time shall use values for gap conductance and other thermal parameters as functions of temperature and other applicable time dependent variables.

The gap conductance shall be varied in accordance with changes in gap dimensions and any other applicable variables.

One thing that is saying then is be mechanistic. Any parameters that are affected by changes in dimensions or changes in the character of the cladding should be accounted for in the ECCS calculations.

What else does it say in that paragraph? Well, based on applicable data, allows a certain amount of flexibility in the data. It is not specifying particular data. It is saying applicable data.

The other thing of course is it says the intent is do not be nonconservative. In the statement it says that incidence of rupture and degree of swelling are not to be underestimated.

MR. SHEWMON: Blockage was not a concern at that time?

MR. LAUBEN: Well, we have to decide what we mean by swelling. Swelling means blockage too. Swelling, and I think we will get into this when we get into the next few slides, but swelling does not just mean strain, but the results of strain are also blockage.

MR. SHEWMON: No, but it is thinner, so you oxidize more area, the gas conductance is poorer.

MR. LAUBEN: Yes.

MR. SHEWMON: And the hydrogen generation rate would go up because the clad --

MR. LAUBEN: But blockage is addressed in the next slide.

MR. SHEWMON: Okay.

MR. LAUBEN: In fact, I could well have entitled this second slide, the next slide, "Blockage," because these are the paragraphs in Appendix K that deal specifically with blockage. And there really are two.

The first one has to do with PWR core flow distribution in paragraph 1(c)(7), and it says the hot region chosen shall not be greater than the size of one fuel assembly. Calculation of the average flow and the flow in the hot region shall take into account crossflow between regions and any flow blockage as a result of cladding swelling and rupture.

So during blowdown that is the only place where the admonition to account for blockage as a result of swelling and rupture is made.

Now the specification of the size of a hot region, and we will show some slides of what a hot region looks like in the calculation, but that was really based on sensitivity calculations that were made around the time of the ECCS hearing. And since that time there hasn't been anything that would cause us to change our perception of the selection of the size of the hot region during blowdown.

In actual fact, blockage during the blowdown of a loss of coolant calculation, I must stress once again these are licensing type calculations, in which the most severe restrictions always have to do with the heat source. That is probably the most unique hallmark of any of the licensing calculations, in the fact that they specify certain hot sources -- decay heat, metal-water reaction, power level in a reactor and so forth, the tech spec peaking factors and what not, and to maximize the amount of heat that is generated in the calculation.

So but what I wanted to say about that was that generally speaking, with very few exceptions, blockage during blowdown is not an important consideration. It turns out that there are generally enough blowdown flow or the pressure is high enough when the flow is low enough that the cladding simply doesn't strain very much during most of the blowdown calculation.

We will look at some stylized plots in a minute.

The next place that blockage is addressed in Appendix K is paragraph 1(d)(5), which is somewhat prescriptive and frankly has given us a certain amount of difficulty, but let me read it.

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 and shall take into account any flow blockage calculated to occur as a result of cladding swelling and rupture, as such blockage might affect both local steamflow and heat transfer.

Now once again notice that this is only a PWR prescription. A later slide will discuss implicitly how the BWR flow blockage is accounted for during the ECCS refill, reflood period of time.

Now, once again the other two paragraphs, the one on the previous page and the one at the top of this page, I was a little bit apologetic about this because first of all, these first two were something that in large measure in 1972 the staff proposed as part of the rule and the commissioners adopted it. This one on reflood heat transfer was somewhat of a surprise to us. There wasn't a lot of guidance in the Commission opinion or anywhere else as to how this was to be implemented.

It was not therefore based on any regulatory or analytical experience that we could draw upon to help us interpret what we were supposed to do in this case. In particular, if you are not going to base it directly on the data that is available, such as a FLECHT experiment for low flooding rates, what do you base it on?

It says steam coolant. What does that mean you are supposed to do? Well, we had to develop our own guidance and our own interpretation as the models came in. And at least as far as the experimental evidence that is available today, it runs somewhat contrary to the experiments. And I think Larry Hochreiter will address the fact that --

MR. SHEWMON: What is it that runs contrary to the experiments?

MR. LAUBEN: The idea that there is only steam cooling available during a reflood --

MR. SHEWMON: By steam you take 100 percent quality, if that is the right word, for it, only steam, no water droplets?

MR. LAUBEN: That is right. Now I can give you an example of what I don't think the intention of the Commission was in this. A little later on there is a slide that talks about Commission guidance on this. There was one short sentence that they really gave us as guidance. I can put that up here.

MR. SHEWMON: Why don't you read it to us?

MR. LAUBEN: The guidance was for lower reflood rate blockage would have a deleterious effect and one must resort to calculations of a single phase steam cooling, taking into consideration the effects of blockage on core flow distribution.

So with the idea that they said deleterious, we felt that they wanted steam cooling not to be of benefit in the calculation but rather to be some kind of a penalty. And it appears that with this they are tying steam cooling to blockage. It is not that just when you have blockage you do steam cooling and it is not when you have low flooding rates you have steam cooling, but rather when you have low flooding rates and blockage they want something with steam cooling. And it should exact a penalty of some sort and not a benefit.

So that was our interpretation.

Now let me -- I can go into some of the history as to what has happened, but let me say this first.

MR. SHEWMON: You have taken up half of your time, so why don't you let us develop it by questions?

MR. LAUBEN: Fine. If you don't want to hear any more about it I won't -- okay, fine.

MR. SHEWMON: Yes.

MR. LAUBEN: At any rate, these are the paragraphs that discuss blockage in Appendix K.

Now briefly, I put this slide up to show you what the effects of swelling are. That is, we consider first of all flow blockage as a result of swelling. It affects the surface heat transfer and the coolant enthalpy. Then there are the strain effects; that is, the dimensional effects on the pin which simply affect the surface heat transfer, the film coefficient that is, the film heat transfer, the gap heat transfer, the transfer of the heat from the fuel to the cladding, and the metal-water reaction, inside metal-water reaction, thinning of the clad, more surface area per reaction per unit mass of cladding. This certainly is an extremely important effect.

Now the next slide says the same thing as the last slide, only it tries to show some interrelationships. First of all, I have outlined in heavy outline the three models that were of primary interest to Ralph and Dale when they worked on NUREG 0630; that is, a model that will predict the incidence of rupture or, that is rupture temperature, the amount of cladding strain, in particular the amount of cladding strain at rupture.

They did not go into great detail about the amount of cladding strain at locations other than the rupture location. I think Dr. Picklesimer may address that today to some degree as it relates to blockage.

MR. SHEWMON: A strain is not often found to be maximum where there is rupture, is that part of the problem?

MR. LAUBEN: Well, I am sure he will get into that in some great detail.

MR. PICKLESIMER: The rupture strain is the maximum, but in my opinion it is the maximum blockage.

MR. LAUBEN: And of course the third aspect is flow area blockage. The idea that a reduction in flow area due to swelling is going to lead to some effects that can cause some difficulty.

Now this first box here indicates the basic parameters that affect the internal pin pressure and consequently the stress on the cladding. Primarily we arrived at this list based on a sensitivity study that we made Exxon go through back four years ago and determined that this list has the most important effect on what the internal pin pressure is.

When the internal pin pressure -- the internal pin pressure is then compared to a -- through the correlation to a rupture temperature. If the actual temperature exceeds the rupture temperature, then rupture is said to occur. There is another correlation that Ralph will talk about that relates the strain to the cladding temperature and rupture, and in another one of course it relates to flow area blockage.

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So, in essence, really, pin pressure affects rupture temperature, which, in turn, affects straining and blockage.

Now, on the other side, we do have another, experimental variable that has entered, and that is ramp rate, or the rate of change of a cladding temperature with time. In reality, that's not a primary variable, really, as I understand. The strain rate and time of temperature are more nearly a primary variable. But ramp rate turns out to be the variable that's most often used in the experiments.

Now, what affects ramp rate or what affects the rate of temperature change? Well, briefly, it's the heat generation and the heat transfer. I could, if I were to make this more accurate, draw a lot more arrows in this diagram, but I'm afraid it would be nothing but confusion.

MR. SHEWMON: Are you trying to present the way the codes work at this point? Or just list the relevant variables and show which are interrelated?

MR. LAUBEN: No, this is how the codes work. And I think --

MR. SHEWMON: Okay.

MR. LAUBEN: -- to a large degree --

MR. SHEWMON: So when you say --

MR. LAUBEN: -- yeah --

MR. SHEWMON: -- you could draw more arrows, are you saying you should draw more arrows to show us how the code works or not?

MR. LAUBEN: For completeness, yes. Because --

MR. SHEWMON: Okay.

MR. LAUBEN: But I -- I think it would be more confusing, and they're not as important, in my opinion, as the arrows that have been drawn here.

At any rate, if you look on the left-hand side, then, we get down to the fact that the rupture temperature will determine what the cladding strain is, the cladding dimensions will thus be determined, and from cladding dimensions we get the three primary effects on the cladding heat balance, namely, metal-water reaction, gap heat transfer, and surface heat transfer. In other words, cladding strain affects all of the primary heat balance variables on the cladding.

MR. STRASSER: Where do you factor the properties of the cladding on such as effects of radiation or effects of manufacturing techniques?

MR. LAUBEN: Well, we -- each model, each computer model, has properties as part of the computer model. In other words, they'll have equations or tables or values as a function of the appropriate variables. Those, those property equations and tables, have been evaluated and approved. They're in the computer code; that's what I can say. They have been evaluated. They appear in the reports that have been supplied by all the fuel vendors, all the properties, the (WORDS UNINTELLIGIBLE) capacity.

MR. STRASSER: I'm thinking mostly of mechanical properties.

MR. LAUBEN: Mechanical properties as well.

MR. STRASSER: Particularly whether they were taken care of in your experimental base.

MR. LAUBEN: Well, let me see.

MR. SHEWMON: That will come up --

MR. LAUBEN: Yeah.

MR. SHEWMON: -- some later. And I'm not sure he's going to be happy with the answers, but --

MR. LAUBEN: Remember --

MR. SHEWMON: -- why don't you hold it for a little bit.

MR. LAUBEN: Yeah. What we -- there are -- see if I can separate it out and tell me if this is what you're looking for -- in terms of burst strain, burst temperature, and blockage, of course, that's exactly what Ralph is going to talk about. There are aspects such as pre-rupture plastic strain, for which the vendors have proposed models which have been reviewed and accepted; and there are also models for elastic strain, for which the vendors have proposed models and they have also been reviewed.

Now, NUREG 0630 has not specifically addressed the latter mechanical models that I've discussed. But we are proposing that perhaps the review should be a bit more general. And we may, depending on how ultimately flow blockage is handled, we may very have to perform a very detailed analysis of things like pre-rupture plastic strain, those kind of models.

I just wanted to say on this that flow area blockage, of course, means flow diversion around the blocked area. It's going to affect the heat transfer coefficient, because the flow is going to be reduced. Also, if you're in a steam cooling mode, the fluid enthalpy. In any event, fluid enthalpy is going to be changed due to the flow diversion. And that's going to, those two things are both going to also have an effect on surface heat transfer.

MR. SHEWMON: Now, your models conserve mass? So that Bernoulli's principles, you know --

MR. LAUBEN: Ah.

MR. SHEWMON: -- if the area -- the cross-section goes down, velocity goes up or something?

MR. LAUBEN: Well, you have brought me to the next slide. That was a good lead-in.

MR. SHEWMON: Okay.

MR. LAUBEN: If only it was good.

This is a typical PWR core model. Shown here, the blocks here, the large blocks, are the fluid volumes. This, this large -- these -- this -- the large blocks down this -- on this side represent what we call the average core channel; that is, all the fluid that is in the core, reactor core, except for the one hot assembly channel, which is represented independently, by itself.

Now, excuse me, the average -- there is also a fuel rod, an average fuel rod, that -- it supplies a heat source to this average channel. And as it says over here, this average core fuel rod represents anywhere from 25,000 to maybe 55,000 fuel rods, depending on the reactor design.

In addition -- the -- the hot assembly channel typically will have two fuel rods associated with it, the hot assembly rod, which represents the average fuel rod in the hot assembly, and the hot rod itself.

Now, that, this fluid arrangement, for the blowdown analysis, is typical of all the PWR models except the B&W model, which has an intermediate channel as well, so they have three channels.

Also, some of the calculations have a hot rod, or part of the hot rod, in a separate calculation; that is, it's not included in the -- right along with the hydraulic calculation, the idea being that there wouldn't be very much feedback effect from the one hot rod hydraulically into a channel with 200 other rods, so it can simply use the input from the hot channel fluid calculation, the output of the boundary condition to a separate hot rod calculation.

Now, to answer your question, yes, mass and energy are conserved within the volumes, or, at least, within the limitations of the computer programs, and with the generation of codes today in a one-dimensional fashion, momentum is conserved in these junctions of flow paths represented by the arrows.

Now, in -- that's true of a blowdown calculation. Now, for a reflood calculation, it's a little more prescriptive, something somewhat different. Frequently the hot -- well, frequently the reflood calculation has an implicit average channel, when one is calculating the flow or the flow diversion in the hot channel; some of them do not. The point is that every vendor had a somewhat different way of approaching how you calculate flow behavior or flow diversion in the hot assembly during reflood, when one is attempting to abide by the Commission regulation that says with low reflood rates do it that way.

MR SHEWMON: You have used up almost all of your time.

MR. LAUBEN: Sure.

MR. SHEWMON: You may be telling us more than we care to know about this. At least, my particular interest was on the impact of the new rules and how it can -- or implementation and --

MR. LAUBEN: Okay. Fine.

MR. SHEWMON: -- on what they did. So --

MR. LAUBEN: I'll tell you what. We can skip --

MR. SHEWMON: -- let's get on to that.

MR. LAUBEN: We can start with this slide, then. And in fact, if you want, we can even stop -- we don't even have to look at the next slide.

MR. SHEWMON: Did you have a question?

MR. MARK: I have a question. You don't need to put the slides back on. You mentioned that low reflood rate, an inch per second.

MR. LAUBEN: Yes.

MR. MARK: It doesn't seem frightfully low. That's five feet a minute and --

MR. LAUBEN: Well --

MR. MARK: -- you can hardly boil that much water out of a thing.

MR. LAUBEN: Right. Now, let me show you this.

MR. MARK: Is that -- well, don't explain it further -- is that amongst the things that might be modified in the new branch technical position?

MR. LAUBEN: Well, as a matter of fact, it was one of the things it was suggested that be changed in the rule when we were very actively pursuing a rule change a couple of years ago, before Three Mile Island. I believe, in fact, there was a whole -- I have that slide, I took it out of the presentation that we made -- and the answer to that is yes. Our intention at that time, and I still think it's a good idea, is that we do not make distinctions between less than one inch per second and greater than one inch per second, but that the experimental evidence be evaluated on its merits and that's what the calculation be based on.

MR. MARK: Okay, that answers my question.

MR. LAUBEN: Yeah. And, in fact, that's why the regulatory, NRR, and, I think, the industry, as well, has been very strong in urging Research, EPRI, and the Westinghouse triumvirate in the FLECHT test to proceed with all due speed with the blockage test, because with the proper evidence we feel that that would be the thing to do.

MR. MARK: (WORDS UNINTELLIGIBLE) would be a change in the rule, as opposed to the branch technical position?

MR. LAUBEN: Well, it would have to be a change in the rules, unfortunately. The branch technical position, on this issue there really isn't one; it's the rule that has to be obeyed. It's Appendix K that has to be followed. It's Appendix K that's giving us the difficulty, in part. So we would -- we would need, unfortunately, it would appear, to require a rule change in this area.

And, I tell you, we have --

MR. MARK: And a rule-making proceeding.

MR. LAUBEN: Yeah. Whatever that might entail.

MR. SHEWMON: Okay, that's --

MR. LAUBEN: Yeah.

MR. SHEWMON: (WORDS UNINTELLIGIBLE).

MR. LAUBEN: We have probably bent, in my opinion, we have bent the interpretation of what the Commissioners meant when they said "steam cooling" just about as far as we can bend it and still be within what anyone would consider the law. So, I think, to make -- to change it much more, we would have to have the rule changed.

Now, let's see, I can -- let me -- I have lumped together the effects of rupture strain and incidence of rupture, because, by and large, unless the rupture occurs later in reflood, after the reflooding rate is less than one inch per second, which generally it does not, but sometimes it does, the incidence effects are also effects on the hot rod as far as strain, gap conductance, and that sort of thing is concerned.

And I just want to mention quickly that the effects that are directly calculated in using these parameters is simply geometrical changes and engineered results, anything that is a function of those geometric parameters is going to be affected. All the effects, except possibly -- with the possible exception of surface radiation, are considered for all analyzed pins throughout the entire transient. Surface radiation models if they are applied now have a small effect, less than 30 degrees. The greatest effects of the individual burst strain and incidence of rupture are on the hot pin or, in the case of the BWR, the hot plane. The strain incidence effects can affect the PWR for the rupture elevation up to the nine-foot elevation, roughly speaking, and beyond that, too, to some degree, but primarily in that range. Below that there's not much effect. The greatest single effect -- and I think this is fairly obvious -- is the two-sided reaction. The two-sided reaction makes the calculation extremely sensitive to almost any small change.

MR. SHEWMON: What's a two-sided reaction?

MR. LAUBEN: The fact that when it's ruptured you are required to start the reaction, the metal-water reaction, on the inside of the cladding as well as the outside. That's a requirement of Appendix K, as well, for a distance of three inches from the center of the rupture, one and a half inches in each direction from the center of the rupture.

Now, to answer the question that you asked, the very last point addressed: the effects of the proposed strain/incidence model changes proposed in NUREG 0630 are worth 0 to 800 degrees F. In some cases, some plants, some models, it doesn't have any effect. On others it may be as high as 800 degrees.

Now, I think it's important to put that into context, because most of this effect is on what I've just mentioned, the ruptured node, which is extremely sensitive to metal-water reaction. Therefore, if you put it in terms of F-sub-Q, or over our F-sub-Q, the number is really not that large; it's maybe zero to .05. A small change in overall peaking factor, generally speaking, can account for an 800-degree effect when it's affecting the ruptured node on the pin of interest.

Next we have the blockage effects. As I mentioned before, the blowdown effects are rather small, except for some B&W client calculations.

MR. RUBENSTEIN: Norm, in limited cases, can (WORDS UNINTELLIGIBLE) rise to maybe .2? (WORDS UNINTELLIGIBLE)?

MR. LAUBEN: Beg your pardon?

MR. RUBENSTEIN: In limited cases cannot the F-sub-Q rise up to about .2 (WORDS UNINTELLIGIBLE) reactors?

MR. LAUBEN: Well, are you talking about the compensating benefits, is that what you mean?

MR. RUBENSTEIN: Yes.

MR. LAUBEN: Well, let me -- for the -- yes, I think I can -- I can discuss it. That's a rather specific Westinghouse type of thing that we're talking about. But let me get through this first.

MR. SHEWMON: Is F-sub-Q some critical heat flux or ratio of heat fluxes?

MR. LAUBEN: F-sub-Q is the ratio of the power in the hottest spot in the reactor to the average. F-sub-Qs typically are in a range of 1.9 to 2.7, depending on whose plant you're talking about. Two three is a fairly typical number for an F-sub-Q.

So [illegible] we're saying, from the previous slide, is that if you have an F-sub-Q of 2.3, it doesn't take a very significant reduction in overall peaking factor to account for an 800-degree effect. And all that's saying is that when it comes to ruptured nodes with the two-sided metal-water reaction, they're extremely sensitive. I think that's something we've lived with for eight years, and sometimes it surprises people, but those of us that have done the calculations --

MR. SHEWMON: When you -- you talked about a hot and average pin in each --

MR. LAUBEN: Yes.

MR. SHEWMON: -- in the subassembly.

MR. LAUBEN: Yes.

MR. SHEWMON: Do you -- when the hottest pin ruptures, do you assume that everything in the subassembly ruptures? Or how do you --

MR. LAUBEN: Some models do, and some models don't.

If you're talking about blowdown, the answer is no, because in blowdown all three pins are carried explicitly.

If the hot pin ruptures, that doesn't have a blockage effect.

MR. SHEWMON: I'm talking about a rupture strain. I'm on your last slide --

MR. LAUBEN: Okay. Yeah?

MR. SHEWMON: -- where you say the greatest -- "strain/incidence effects can effect PWR" -- "greatest single effect is two-sided reaction at ruptured node."

MR. LAUBEN: Yes. Yes, at the ruptured node -- I should have said "of the hot pin."

MR. SHEWMON: Well, but if one pins ruptures, it probably doesn't --

MR. LAUBEN: It doesn't affect --

MR. SHEWMON: -- go very fast towards giving you 17 percent.

MR. LAUBEN: Well, remember, 17 percent is a local number. It is -- if you have one spot in the whole core, be it the three-inch node of the hottest pin of the 50,000 pins, if that reaches 17 percent that's still the limit. It may be somewhat restrictive, but that's still the limit. We don't --

MR. SHEWMON: Only somewhat you feel?

MR. LAUBEN: Well, I'll tell you --

MR. SHEWMON: Scratch that. Go ahead.

MR. LAUBEN: Yeah, okay. Right. Compared to the German method of licensing, which does a more probabilistic -- what's this (WORDS UNINTELLIGIBLE)?

VOICE: (UNINTELLIGIBLE)

MR. LAUBEN: Yeah. That's it, the hot pin. It is the hot pin that determines conformance with Appendix K in this regard, with respect to the 2200 and with respect to the 17 percent. Assessments are made of --

VOICE: (WORDS UNINTELLIGIBLE) hottest spot in the hot pin.

MR. LAUBEN: The hottest spot in the hot pin. That's correct. That's correct.

Oh, okay. Let's see -- blockage effects. I think I mentioned number one. Oh, yes -- in the BWR, the post-blowdown blockage effects are implicitly accounted for in the heat transfer derived from the BWR FLECHT program. In that case, they did have typical blockages in the -- in some of the FLECHT experiments, and in that respect the effect of blockage is taken into account in deriving the heat transfer model for the BWRs.

PWR blockage we've discussed of less than one inch per second. And we have discussed the fact that flow diversion -- well, flow diversion and heat transfer are calculated very differently. Each vendor came in with a different model, and we approved those models, sometimes after a good deal of deliberation. And each vendor went through a good deal of deliberation, because we all didn't have a great deal of idea how we were going to attack this problem at the time.

MR. SHEWMON: If we can stop on item three for a minute, it seems to me that's the nub of much of the discussion here.

MR. LAUBEN: Yes.

MR. SHEWMON: Appendix K may require that if one spot on one fuel element gets 17 percent, then you've got to yell uncle or something. But it seems to me that if you're saying that -- that says the "hot rod only" will be considered for flow blockage effect, and it's going from that hot rod to the whole subassembly blockage which is going to be most of our discussion today. So I guess that one I'd like to hear you say more about.

MR. LAUBEN: Well, let me say this. There are some models that carry along a hot assembly pin that is calculated as well as the hot pin. And blockage will not be calculated to occur until the hot assembly pin, the average pin in a hot assembly ruptures. Other models it's the hot pin that ruptures. I don't -- some -- well, I -- of course, to a certain degree, and I don't want to characterize the degree because that, it may be competitive --

MR. SHEWMON: What you're describing here is what the vendors do, not what you require or what is being suggested?

MR. LAUBEN: Well, this is what they do, this is what they have proposed and what we have accepted. We didn't want to overprescribe the hydraulic models so that everyone had the same hydraulic model. We felt that it was in the interest of -- of independence that each vendor prescribe a model that accounted for flow blockage, and everyone derived a different model. And we all felt that each model, albeit they're different and have different effects, there's different degrees of conservatism in each hydraulic model, that still they -- they make a -- a reasonable attempt to abide by the Commission rule on this, on this score, and that they're conservative.

MR. SHEWMON: Now, part of the reason we're gathered together today is to try to get less diversity in, at least, some of this data.

MR. LAUBEN: Yes.

MR. SHEWMON: I don't know, I guess Ralph doesn't have it in his handout. So that you're saying that --

you're talking only about the thermal hydraulic models.

MR. LAUBEN: Yes.

MR. SHEWMON: And there you try to let 100 flowers bloom or something.

MR. LAUBEN: Well, six or seven maybe.

MR. SHEWMON: Okay.

(Laughter)

MR. LAUBEN: It has its advantages and its disadvantges.

MR. SHEWMON: I agree.

MR. LAUBEN: And I don't think there's any simple answer.

The bottom line of this one is that the effects of blockage are about zero to 150 degrees, once again depending on the model, depending on the plant. However, the effects of blockage have a much stronger effect on F_Q or power level compensation. Whereas the other one was .05 for 800 degrees, this is .15 per 150 degrees. So it's much stronger.

MR. SHEWMON: That F_Q is the steady-state power factor?

MR. LAUBEN: That's right, power distribution factor, that's correct.

MR. SHEWMON: But the maximum -- the vendor would like to keep that as flat as he could in order to avoid DNBR in certain accidents or something?

MR. LAUBEN: Well, he would like to be able to have the highest possible F_Q so that he can operate with the greatest amount of flexibility. It allows him in some cases to load file better. It allows him not to have to monitor the core flux distribution as rigorously. It allows him much more flexibility if he is allowed to have a higher value for F_Q.

MR. STRASSER: You're saying that this will leave a .15 F_Q in his margin?

MR. LAUBEN: Exactly. Now, I think that we don't need to discuss that -- let's see. Oh, okay.

MR. SHEWMON: Isn't there a maximum for the worst case?

MR. LAUBEN: For the worst case that we've seen so far in our assessment since November.

MR. SHEWMON: And you've looked at all the numbers?

MR. LAUBEN: Well, yes. We've made -- I think Jack Rosenthal discussed it. I don't know if he discussed it with the Fuel Subcommittee or not. Was that the Fuel Subcommittee? Yes. (Inaudible).

The kind of assessments we went through for each of them, I don't think I want to go through today. I'll tell you what, I won't -- I won't spend much time on these last few slides, simply to note that blockage data, which is very important. We mentioned the Flecht experiments. There are a number of other experiments.

At this point it would be somewhat premature to use these experiments to define more accurate flow diversion models as a result of blockage. But we do expect, within a couple of years, to be able to do that better. Now, I think once again, Larry Hochreiter has some of the recent Flecht tests of blockage.

I think the bottom line is that we do expect, from what information is available from these flow blockage experiments, that we will find that, as we expected and as the Commissioners intended, in the current model (Inaudible) and we would get some relief. But I don't think we can get the relief if we don't have a rule change.

This is a rather busy slide of the two-phase flow effect. In fact, I think I discussed it and I don't want to go over that again, less than one inch per second.

Let me just say about this that the intent of this slide was -- the bottom line was that our two-phase flow effects are a big deal when it comes to swelling and rupture; that is, their influence on swelling and rupture. The point is, I think, probably the key point on this is this point right here, that reflood test data indicates a rupture occurs at 280 degrees above the reflood temperature. That two-phase flow behavior probably has very little effect on strain and blockage. And we believe that the appropriate kind of atmospheres for these tests are steam atmosphere.

And let's see. Okay, the schedule we'll discuss later. And now we're all through.

MR. SHEWMON: I guess the only thing that bothers me a little bit on that is, in this land of Alice in Wonderland, I have difficulty deciding when it is we're trying to develop best estimate models and when it is we're trying to meet things -- use models that meet Appendix K and in some way approximate reality at the same time. And so when you say that that's the way it happens in reality, I agree with you. But when it comes back to living with

Appendix K and the models you're using, I don't know whether you're in left field or in right field.

MR. LAUBEN: Well, we have tried, as far as flow diversion is concerned and heat transfer, with respect to less than one inch per second. I believe from those -- that one-sentence admonition from the Commissioners told us, I think, that they wanted us to be conservative if we landed in this area of less than one inch per second reflooding rates.

Now, I'm not sure -- unfortunately, they didn't talk about it much more than that. We believe that if we -- the problem that existed at the time of the rulemaking hearing was that there wasn't any data that anyone agreed on that was very applicable data. There were some limited plate blockage experiments. The sleeve blockage experiments that were available were very short tubes.

MR. SHEWMON: What will come out of the Flecht results that you think may change the --

MR. LAUBEN: What I would like to come out of that is that as a function you would be able to derive heat transfer models that not only were a function of the other appropriate variables, but would also be a function of blockage; that you could derive some models that could tell you how to treat blockage during reflooding in a more realistic way; and you wouldn't make any artificial distinction between less than one inch per second and greater than one inch per second, but simply what does the data -- what would the data indicate that you ought to be using for reflood heat transfer and reflood flow models.

Also, in addition, the one slide did show that there were FEVA experiments and NRU experiments that are trying to address the same thing. So in a little while we do expect some of this same to be available.

MR. ESPOSITO: Dr. Shewmon, may I clarify one point. I'm Vincent Esposito from Westinghouse.

What we attempt to do with these values is to maximize the licensing (Inaudible), so that we can give the plants flexibility in operation, but not the operating team. We're not trying to maximize that.

MR. SHEWMON: Okay. Thank you.

MR. MEYER: When Norm was talking about the peaking factors and showing what kind of peaking factor adjustments are needed to compensate for various increases in peak cladding temperature, you shouldn't infer from that that one has to change the peaking factor to live with the cladding model changes. Because in fact compensations can be made in the thermal hydraulic models so that there might be no need for any change in an operating parameter as a result of revisions that might be made to the ECCS models.

I just want to note, as I start, I see that I'm 25 minutes behind the appointed time of starting of my talk; and that you've given me only half of the time that you offerred me initially. So I hope you will be understanding if I don't finish on schedule.

MR. SHEWMON: We'll do our best.

MR. MEYER: Okay. To pick up where I left off, my intention today is to address five questions which I think are the essential questions which have been raised with regard to the cladding models. If there are other questions that you want to discuss, you'll have to bring them up separately.

The first question is: Were important data sets overlooked in deriving the rupture-temperature correlation in NUREG 0630? This slide shows the Oak Ridge correlation, shown as the solid line, that was used in NUREG 0630, along with some additional data points. The slide is basically Figure 2 in the report, and we have added Westinghouse data points, which are shown by the little circle with the "W" in it; and KFK data points, which were shown on other figures in the NUREG report.

The KFK data were shown in the NUREG report, but they were not used by Oak Ridge to influence the derivation of the correlation. These Westinghouse data were not shown in NUREG 0630 because they did not meet our basic requirements for typicality for data selection; and they were not used in deriving the correlation by Oak Ridge.

MR. SHEWMON: Are these clusters or single pins?

MR. MEYER: I should point out that most of the points are single -- single pins. The Westinghouse points are experiments which contain anywhere from two to ten individual rods, and they are spread on the Westinghouse data around each of these points, on the order of plus or minus 30 degrees C.

MR. SHEWMON: To cover the rupture of all of the pins; is that right?

MR. MEYER: If this point has ten -- is the mean of ten data points, the ten data points scatter within plus or minus 30 degrees.

MR. SHEWMON: But do they run data -- when they run these, do they run a cluster of pins or one pin?

MR. MEYER: Are these all single-rod? These are all single rod.

MR. SHEWMON: Maybe Westinghouse should answer.

MR. BURMAN: I'd like to ask a question. Are these points the uncorrected temperature points?

MR. MEYER: Yes.

MR. BURMAN: But we established that there was a temperature bias in the measurement and that these were not the correct temperatures to use and the staff accepted that.

MR. SHEWMON: And would that tend to shift them?

MR. BURMAN: It would shift them upwards.

MR. SHEWMON: Okay. So it would be -- you would improve the agreement with the curve?

MR. BURMAN: Yes. Thank you.

MR. STRASSER: Were these the electrically heated rods, the Westinghouse ones?

MR. BURMAN: These were external, radiantly heated, single rod tests, with the pellets, aluminum pellets on the inside.

MR. SHEWMON: Go ahead.

MR. MEYER: Okay. You can see that the Oak Ridge data and the correlation are 25 to 50 degrees more conservative than some of the KFK data. In this case, if the line is below the data it's more conservative.

MR. LAWROSKI: How do you get your 25 to 50 degrees? Do you use all the points?

DR. SHEWMON: No, just the black point, just the solid KFK point.

MR. LAWROSKI: Oh, just the KFK. I thought he was talking about --

MR. MEYER: There are a group of KFK data points that are on the order of 25 to 50 degrees above the line. There are another group of KFK data points that are right on the line. The Westinghouse uncorrected data points are down here.

None of these data points are corrected. They're all actual data.

MR. LAWROSKI: When you talk about corrections, what's the size of the correction? 5 degrees, 50 degrees?

MR. MEYER: It depends --

MR. LAWROSKI: 100 degrees?

MR. MEYER: -- on the care that was taken in the experiment that was performed. In the case of the Oak Ridge data, the temperature error is estimated to range between zero, when they got the thermocouple real close to the burst node, to about 25 degrees.

In the case of the Westinghouse data, which they may want to comment on in more detail, I think the errors were considerably larger because they were tests that used just one thermocouple. They didn't always get it close to the rupture node.

The -- okay, you see a group of KFK data that are above the line. And after Westinghouse corrects this data, they get a correlation, shown here as a dashed line, which is also above the line in a region that's of interest to us. And so the question has been raised about whether the NUREG correlation should be raised in this region.

In replying to this question, several points have been made. Chapman showed that the Oak Ridge correlation also fit data from the Argonne National Laboratory. He presented his analysis to this Subcommittee on February the 14th, and concluded that the Oak Ridge correlation and the Oak Ridge data were not unique.

If you'll recall that presentation, you'll remember that Bob did a regression analysis of the Argonne data, and the results were almost indistinguishable from the curve that is shown on this slide.

MR. SHEWMON: Ralph, remind me, please. Is the engineering hoop stress the average pressure from the -- stress from the pressure at the rupture temperature, or is it the pressure stress before you start heating it, or what?

MR. MEYER: The engineering hoop stress is a measure of the pressure at the time of rupture, simply converted to the dimensions of the undeformed tube.

MR. SHEWMON: So it rises to that point?

MR. MEYER: That's correct, that's correct.

We pointed out in the NUREG report that the KFK data points that you see above the curve here were from isobaric experiments and that they were not prototypical in that respect. In a fuel rod which has a constant number of moles of gas, the internal pressure will drop as swelling and ballooning progress. This will result in lower strain rates at the time of rupture than in an isobaric test.

And recall that deformation in zircalloy depends strongly on strain rate. It's like silly putty.

And the Westinghouse data are also from isobaric tests. Oak Ridge, on the other hand, usually uses the more prototypical constant gas inventory as a standard test. But they recently performed a couple of comparative tests at constant pressure. At a given stress, the ruptures occurred at a higher temperature by about 25 to 50 degrees, roughly the same as this discrepancy.

Now, I've been cautioned not to make too much out of this; two tests aren't conclusive. But they do seem to show that the isobaric conditions raise the burst temperature. We know that the isobaric conditions will affect the burst strains. It also appears that they will affect the rupture temperatures.

Dr. Chapman is here today and if you want to talk about these Oak Ridge tests, I'm sure he'd be glad to comment on those.

So we discovered something else about the KFK data. When we look closely, if you will recall, we had grouped data by ramp rate, sometimes calling it slow ramp and sometimes calling it fast ramp, and using various ranges of ramp rate to include in those categories.

It turns out that all of the KFK data points that are above the line are for ramp rates that are significantly higher than 28 degrees C. per second; they're in the range of 25 to 40. We had thought that ramp rate effects saturated at about 25 to 30 degrees per second.

The KFK data points that fall right on the line were very close to 28 degrees per second. Again, I don't think there's any conclusive message here. But it is possible that the discrepancies that you see here are residual ramp rate effect and not related to any other.

MR. SHEWMON: Why don't you go on. At least I'm more interested in the blockage part, and I think that's where the discussion is leading.

MR. MEYER: Well, I'll move on to the burst strain. But I'm really --

MR. SHEWMON: Fine.

MR. MEYER: The burst strain, I think, is probably more important for our discussion than the blockage per se, because the burst strain -- the selection of the burst strain correlation drives the blockage model.

So the question about the burst strain data is, were the data selectively used to produce larger strain, large strain in the burst strain correlation in the NUREG report. Selection of data is very important, because experimental conditions can affect the results dramatically.

The central issue is whether or not local temperatures in a fuel rod during a LOCA would be more or less uniform than in the experiments we've chosen to rely upon, because temperature uniformity appears to be a very important variable controlling burst strain.

We believe that the best approach to capturing temperature gradient effects is to mock up experiments as close as possible to real fuel conditions and accept the results as they come out. Other approaches presume to know what local temperature gradients are in a LOCA, and we believe that it's unknowable with any reasonable accuracy to know that.

So therefore, our approach has been to rely on prototypical experiments and to get proper heat flow, simulating K heat, and to include atmospheric conditions for oxidation. We selected only data taken in experiments that utilized indirect internal heaters in aqueous atmospheres. But we did not place any artificial limits on how small or how large the temperature gradients might be.

MR. SHEWMON: Is steam an aqueous atmosphere?

MR. MEYER: Yes.

In retrospect, I believe we should have rejected isobaric data, but it turns out not to confuse the situation very much.

With this as background, there are really two separate questions that were raised, and these were raised at the last meeting we had, on Valentine's Day: One, is the Westinghouse data conservative; and, two, did we improperly weight individual data sets, data within the set we evaluated in NUREG 0630. Stated another way, is the Westinghouse burst correlation adequately conservative and is the NUREG correlation too conservative.

Let me first address the Westinghouse data. We rejected such data because the direct and external heating methods produce incorrect heat fluxes that can lead to extremely uniform local temperatures. Such uniform local temperatures could exaggerate strains and get results that are too conservative.

In the Westinghouse case, however, other atypicalities were present that produced just the opposite effects. The Westinghouse tests were isobaric, as mentioned before. This will produce strain rates that are atypically high, thus resulting in strains that are biased low.

In addition, the Westinghouse bundle test uses spray-coated oxide covering to reduce electrical shorts, but the rods spalled, arced and failed prematurely due to local melt' ,. Therefore, one can only conclude that features that were present in the Westinghouse -- there were features that were present in the Westinghouse test that would both enhance and suppress strains, and on balance the degree of conservatism or nonconservatism is unknown.

To confuse matters a little more, Westinghouse has excluded all the slow ramp data from the strain analysis, because they believe that plant transients were fast transients. We recently have come to realize that that is in fact not the case, that most of the plant transients are slow transients.

We found that the Westinghouse data are somewhat nonconservative in comparison with other data, and therefore we believe that the correlation sometimes underprotects burst strains.

With regard to the weighting of individual data within the data sets evaluated in NUREG 0630, I have the following comments. This is a slide directly out of the report. It's Figure 6, and except for the initial screening of the experiments that didn't use internal heaters or aqueous environments, no selecting of data has been done here.

In deciding where to place the correlation curve, however, we did use judgments that in effect weighted the data differently. For example, these open squares down in the bottom were effectively ignored when we decided where to place the burst strain curve, because these were tests run at Oak Ridge with unheated shrouds. The shroud is put in there to make the fuel rod feel like it's in the presence of other fuel rods, which it can transfer heat to.

These tests were rerun under almost the same conditions, except with the heated shroud being used, resulting in the open diamond-shaped points at the top. So we did indeed selectively use the data in determining the position of the curve in the correlation. And I have a graph that shows --

MR. SHEWMON: Have any tests been run with unheated pins as one of a group of 6 or 36 or something?

MR. MEYER: Yes, there have a couple tests. Either Dale or Bob should answer the question, because I don't know the details.

MR. CHAPMAN: There have been bundle tests in Japan with two or three unheated pins.

MR. SHEWMON: Out of how many?

MR. CHAPMAN: 49. The REBEKA 4 test had one unheated pin, the biggest one, out of 25. The next test we run will have 2 out of 32.

MR. SHEWMON: The reason I ask, as you know, is that there's always some control rod tubes around and usually a water rod or something, and that produces an asymmetry which may operate like this. And what I'm trying to get at it. Was there a marked difference, then, in blockage, when you had a fair amount of rupture? Or can one generalize?

MR. CHAPMAN: That's a difficult question. I think the results from the REBEKA tests were somewhat surprising in that the thing went somewhat to the opposite direction as they were anticipating. There was an effect of the heated or the unheated rods, and it took some time to understand it.

I can't comment much more than that. I don't remember the details.

In the Japanese tests, again there was some effect, but I don't believe a very large effect.

MR. SHEWMON: Why don't you bring it up when you get in and integrate everything with it, if you would. Now I'm speaking to Westinghouse.

Okay. Thank you. Go ahead.

MR. MEYER: We do make an adjustment for the presence of guide thimbles in the bundles, in the model for flow blockage.

Back to the burst strain data, this is the figure in the report. Here is a figure that has been changed in the following way: From this figure we have simply removed all the tests with unheated or without shrouds, that is, removed some of the data down here at the bottom. We have also added on a number of data points that have come out this summer through an ASTM meeting and some exchanges that we've had, that have largely added data points over in the beta phase region, which makes us look kind of nonconservative over there.

However, if you'll focus on the alpha phase region, which is the important region, and if you're not overly influenced by these stragglers down here that all came from one test, I think you will have to admit that we've pretty much best estimated the data.

MR. SHEWMON: I'm not sure you're plotting the right variables, but you may well have put the best curve through those points if you insist on those variables, or if Lauben insists on them, or whatever.

(Laughter.)

MR. STRASSER: Are these all unirradiated rods?

MR. MEYER: Yes. Oh, I wanted to answer the question you asked before, very briefly. We don't account explicitly for fabrication variables such as texture or cold work or things like that; nor do we account for effects of radiation. We assume that all these things anneal out and the tests are run with specimens from commercial tube fabricators.

All the tests are done with -- well, not all of them. There are a couple of -- there were some irradiated rods tested at Battelle-Columbus, so I shouldn't say all of them. But for the most part, the tests involve unirradiated tubes.

MR. STRASSER: How about the variable of dimensions? How do you characterize clad thicknesses?

MR. MEYER: Well, all of the work has been put on the same footing with this engineering hoop stress. So we've eliminated the diameter and thickness design differences from affecting these comparisons.

MR. SHEWMON: My question relates to the direct analysis of the test. Are these well characterized rods? Sometimes clad wall eccentricity can take up ten percent of the clad thickness.

MR. MEYER: Bob, did you hear the question and will you answer it?

MR. CHAPMAN: Yes. In our case, the tubing is well characterized. The tubing is not in fact that far out from uniformity. And we do not believe that the uniformity is significantly -- all these other parameters, the other things are much more important than variations in the cladding.

MR. SHEWMON: Is the variation you're talking about circumferential around one pin?

MR. STRASSER: Yes, clad wall eccentricity.

MR. SHEWMON: You mean if I had a thick wall on one side of my pin and a thin wall on the other, it won't affect the burst strain?

MR. CHAPMAN: Yes, it will. I'm saying that the tubing that we're talking about, that's not important, because it's not a large variation.

MR. SHEWMON: In yours. But the question is, what happens in reactors.

MR. CHAPMAN: Well, I think our fuel is comparable to the reactor specifications.

MR. SHEWMON: Now, you're saying it can be ten or it is ten percent?

MR. STRASSER: Not unusual.

MR. SHEWMON: Well, if you could get it to be standard, maybe they'll make you an allowance for it.

MR. PICKLESIMER: May I answer part of that question? My name is Picklesimer.

I have looked at a good bit of the tubing. I'm on the ASTM committees that set the standards for this nuclear tubing. The manufacturers come in with surprisingly close tolerances on this. And you can examine from one manufacturer to another. They're remarkably good.

So one line of tubing is, for the most part, so far as these tests are concerned, they can be considered completely comparable; and that's whether it's made by the Japanese or the Germans or the Swiss or the Americans. They're that close together.

MR. STRASSER: My basic question was whether it's measured and recorded as part of the test program, so that it can be interpreted.

MR. SHEWMON: The answer is often.

MR. MEYER: Well, let me finish up the bursting point by saying that I don't think there's any question that burst strains can be large, they can be a lot larger than 100 percent. Oak Ridge has seen strains as high as 100 percent.

But based on prototypical experiments, we don't believe that the burst strains, on average, will be more than 90 or 100 percent in the important alpha region. On the other hand, we believe that smaller values would likely underestimate the degree of in-reactor swelling, and that's not allowed by Appendix K.

I'm now ready to turn to the third question, on the flow blockage methodology, and the question is, is the methodology valid for converting burst strains into flow blockage.

#4

The first thing we should do is review the methodology, and this slide is Figure 10 from the NUREG report.

First, and starting from the top of the slide and working down, from three Oak Ridge multi-rod bundle tests we compared burst strains with average strain in a plane, finding the average strain to be about half of the burst strain. This average strain was then converted into a percent area blockage using simple geometrical assumptions that presumed that the fuel rods swell into square shapes after they touch.

Then further adjustments were made to account for nonswelling tubes such as control rod thimbles and instrument tubes.

The question was raised that deals with the first step of the procedure. The ratio between burst strain and average coplanar rod strain was based on only three Oak Ridge tests performed in the alpha phase temperature region.

MR. SHEWMON: Do you have a plot in here that shows an average in plane strain; or to be specific, does that exclude all ruptured elements in that plane?

MR. MEYER: No, it does not.

MR. SHEWMON: Okay.

MR. MEYER: Figure 11 in the reports, on page 27, it shows a cross-section in the first bundle test at the plane of maximum blockage.

MR. SHEWMON: Now to get the strain for those ruptured elements did you inscribe a circle that would represent the unwashed out fuel pellets and take that as the effective diameter, or what did you use as the effective diameter for the average strain?

MR. MEYER: The strain on all of the rods was taken as the ratio of the length of the deformed cladding around the circumference to the -- original circumference, undeformed circumference.

MR. SHEWMON: Why?

MR. MEYER: It is total strain.

MR. SHEWMON: What got us here is the British experiments which showed that if you have elements which do not rupture and they puff up like bananas or balloons or something you can have flow blockage.

MR. MEYER: Yes.

MR. SHEWMON: But I don't see any relevance between the strain of that extended U-tube and flow blockage.

MR. MEYER: Hold on. Because we measured the strain in every rod, whether it ruptured or not --

MR. SHEWMON: Yes.

MR. MEYER: -- and in this case you have a 16-rod bundle. This is the plane where the area, the flow area is smallest by any definition that you might choose to put about the blockage around the ruptured node. And the point I want to make is that the rupture -- I am sorry, the blockage is indeed controlled largely by the nonruptured rods. And there are only four of these ruptured in that plane, and they are not controlling the blockage in the plane. They have --

MR. SHEWMON: Well, if that was true, then why didn't you go back -- it seems to me you can argue that the fuel pellet may always be there.

MR. MEYER: Sure. It may not, but I would be willing to argue that.

MR. SHEWMON: Okay, and that is a conservative position and it may not. But okay, let's assume it always is.

MR. MEYER: Yes.

MR. SHEWMON: Then if you took the fuel pellet diameter, it seems that that is the maximum you can argue is indeed blocking at that point. So why wave your hands and say, gee, it doesn't make much difference by taking this other approach?

MR. MEYER: Can you tell me where you are going with this question because I don't think you are going anywhere that is --

MR. SHEWMON: I am going with this question that -- the basic question to me for this afternoon is the relationship between flow blockage and independent burst strain.

MR. MEYER: Okay.

MR. SHEWMON: And if you take a physically impossible method of calculating an average strain, implying that it implies blockage, then you know I would -- sort of a red flag goes up and I say, gee, I don't know whether I can believe or how much I want to believe anything that comes after this.

MR. MEYER: You are missing the point.

MR. SHEWMON: I am sure I am.

MR. MEYER: And I think half of the people in this room have missed the point from the very beginning.

MR. SHEWMON: Gee, if you got the other half you are doing well. Keep going.

(Chuckles.)

MR. MEYER: There will be a reduction in flow area at every elevation in this bundle because the rods rupture and swell all over the place actually. In any plane that you choose to examine you can define an average rod strain that would produce that amount of blockage.

MR. SHEWMON: What amount of blockage?

MR. MEYER: The amount of flow area reduction that you measure in that plane.

MR. SHEWMON: And you measure where the whole pin or at points along there the flow resistance, is that right?

MR. MEYER: A comparison between these area reductions and pressure drops have been made in the codes, and that is shown that using an area reduction is a legitimate way of describing blockage, because in the codes it does indeed produce the pressure drops that you measure in these very bundles when you test them after the temperature test. You do a flow test, measure the temperature, measure the pressure, which is shown here by the little dots, and you do a code calculation where you have input this measured flow area reduction.

MR. SHEWMON: And the measured flow area reduction is not the flow area reduction; it is what you call average strain, is that right? That is where I am going, because when you say flow area reduction I don't know what you are saying. Whether it is I think is what is physically plausible or credible flow reduction or whether it is some average strain that you calculate this way .

MR. RUBENSTEIN: Ralph, there is a small three-dimensional drawing which shows the blockage just above and below this maximum width you are taking?

MR. MEYER: No, I don't think so. There is indeed an ambiguity about what you call the amount of flow reduction produced by a blocked node. And Bob Chapman has used two definitions: one where he does in fact use the inscribed circle and one where he doesn't. They don't make much difference on the flow area reduction in a plane because there are only a few ruptured nodes in that plane, and their effect is divided by four or five or whatever the ratio is.

We in fact use the least conservative, the most realistic of his definitions.

MR. SHEWMON: It may be least conservative, but if you are talking about something that can't block and say it can block I don't think that is realistic.

MR. MEYER: Suppose that we had found a plane where there were no ruptured nodes at all.

MR. SHEWMON: Okay.

MR. MEYER: Now would you agree that we can measure a flow area reduction in that plane?

MR. SHEWMON: And I would also agree that what you said he did sometimes of the inscribed one is much more physically credible.

MR. MEYER: Okay, we used that one by the way, and when we obtained a flow area reduction for a plane that had a burst node in it we used the inscribed circle method. But you could go actually through this bundle and there would be a lot of planes where you wouldn't have a burst node. And in those cases we wouldn't have any problem discussing the flow area reduction.

In those cases you could then say that if every rod had strain X I would produce the same amount of flow area reduction and calculate an average rod strain in that plane. Okay, that we did, geometrically, simply from taking the flow area reduction and back calculating an average coplanar rod strain.

Then we said if we knew what this average coplanar rod strain is we can work the problem back the other way and calculate the flow blockage.

Now what data did we have available to us? We didn't have measures of average coplanar rod strain. That is a fictitious number in the first place; they are all different. In fact, we didn't have, although some might say we could have gotten them, we didn't have the strains measured at every elevation along the whole rod for all the tests in the world.

What we did have were burst strains. And so we said is there a correlation between burst strain and average strain in a plane. And we looked at bundle tests. The three that we had were good pedigree, and in those three cases when we ratioed the numbers we got nearly a constant. And so we said we will use burst strain as a correlating parameter. It has nothing to do whether mechanistically the burst node is contributing to blockage or not contributing to blockage. All it does is say that if you are in a temperature region where the material is ductile you are going to get a big burst and you are probably going to get big nonburst swelling.

And so we used the burst strain only as a correlated parameter, as a means of getting the average strain.

Now Pic is going to tell you about another way of doing business, where instead of using the burst strain as the correlating parameter, he is going to measure up and down the rod and take an axial average strain on a single rod, and he is going to take the ratio of that to this coplanar average strain and say that this is probably a better way of doing business than using burst strain to correlate on. And I can probably agree that that is a better way of doing business, except that we didn't have the data; the work hasn't been done. So you pay your money and you take your choice. I think the difference is a second order effect.

We chose a parameter to correlate on, and the adequacy of that correlation is what I think is a good question that has been raised about the blockage model. And I have a comment to make about that good question if we are ready to move on to that.

MR. MATHIS: Ralph, your maximum blockage is going to occur just before your burst, when you have got maximum swelling without rupture, right?

MR. MEYER: No, not necessarily, because the burst, axially the burst in different rods occur at different elevations. They are not coplanar.

MR. MATHIS: Well, I realize that.

MR. MEYER: And so you could indeed have --

MR. MATHIS: But you are going to have flow paths in and out though?

MR. MEYER: You could indeed have the most blocked plane not having any bursts in it.

MR. MATHIS: That is right.

MR. MEYER: In this case I think they all did have bursts in them in your three bundles. But it is not preordained.

If they were coplanar, you see, then we would really be stuck on what defines flow blockage if we have got an area that is just full of ruptured nodes. As it stands now, the uncertainty in what is the flow area is -- there is an uncertainty because we don't quite know how to treat those burst nodes, but there are just a few of them on average in this plane. And so we make a reasonable guess. We use the inscribed circle method. And that is how the area reduction is defined in the plane where you have a burst node.

Now the question --

MR. STRASSER: Let me ask you this.

MR. MEYER: Sure.

MR. STRASSER: This ratio of burst strain to average rod strain that you find constant for the Oak Ridge tests, do you feel that has a physical basis or is it a happenstance?

MR. MEYER: No. I think in a very crude sense it has a physical basis, and that is in the alpha and the beta, pure alpha and the pure beta regions where the material is very ductile you would expect both burst strains and prerupture strains to be big, because you are dealing with a ductile material.

In the mixed phase region where the material is fairly brittle you will expect the rupture strain to be small and the rod strains away from the rupture to be small.

MR. STRASSER: Depends on the ramp rate to a certain extent?

MR. MEYER: Yes. But fundamentally there is no mechanistic connection. We don't pretend there is.

MR. STRASSER: How did you find your maximum plane of flow blockage?

MR. MEYER: Well, in the Oak Ridge tests they sectioned them how often. every --

VOICE: One and a half centimeters.

MR. MEYER: Every one and a half centimeters and looked for them.

Okay, the question that was raised was about this ratio of burst strain to coplanar average strains, and the question is you -- or maybe it is an observation -- that we took that ratio from three bundle tests, the only three bundle tests that existed at the time, and all three bundle tests were taken in the alpha phase region. And the comment was that as you move up through the mixed phase and into the beta region that you might expect that deformation in the burst node and deformation away from the burst node might get out of kilter a little bit, so that the ratio wouldn't hold.

Put very simply we are using a correlation, a ratio outside of the range that we had data to derive it. And that is a risky business. And it is a good criticism. There are only two problems. One is that there aren't any data outside of the alpha phase region. So there is no way we can remedy the situation. And in other modeling assumptions like these of average strain or anything else would be subject to the same limitations.

And secondly, the alpha phase region appears to be the most important for the licensing analysis anyway. So we feel fairly comfortable in the region that is most important. It is the region of the biggest strains and where most of the plant transients fall down.

So we do indeed acknowledge limitations of the model. I don't for a minute think that this is the model that is going to stand for 20 years without modification. But at the moment, considering the data that exists, I don't think that you can do much better.

Now there were two final questions that were more philosophic in nature, and they were: is there a need to require cladding model changes and should it be done now?

Let me first flash through the different vendor models for the three cladding correlations. This is a comparison of the vendor models for cladding rupture temperature which gives the incidence of rupture.

The comparison has been made for a ramp rate of 5 degree C. per second, which is a very common ramp rate in the plant transients, although in most cases the vendor models don't have ramp rates as a parameter. So no matter where we would, what ramp rate we would be interested in, most of the vendor models would stay put. The NUREG model labeled NRC is a solid line, which was done at 5 degrees per second. And the Westinghouse model was also a 5 degrees per second. It is the only rupture temperature model that is currently used by the industry that does have ramp rate as a parameter.

Now that is rupture temperature, which gives the temperature in pressure or hoop stress at the time of rupture, and is what you use to decide if you have had a rupture or not.

I think you have seen this one before. This slide compares burst strains for all the vendors. Again, none of the vendor curves account for ramp rate in the burst strain correlation, and this is even true in the Westinghouse case.

Westinghouse uses ramp rate as a parameter in the rupture temperature correlation but not in the burst strain and not in the flow blockage.

Here again, this comparison has been made with our so-called slow ramp curve, which is a curve that was fitted to data with ramp rates between zero and 10 degrees C. per second.

And here is the corresponding flow blockage curve. The NRC flow blockage curve looks just like the NRC burst strain curve, because that conversion process, the model simply gave us a factor to scale the strain curve with.

Now, on this slide I have simply listed three reasons why we believe that changes should be made in the cladding models. The first reason is that there is no physical basis for cladding models to vary significantly from vendor to vendor. The obvious design differences like diameter, thickness, fill pressure are all explicit inputs to the model.

The second reason is that the cladding

correlations in the NUREG report were developed to give the minimum values that would satisfy Appendix K. That was our intention. If you agree that the NUREG curve satisfies that purpose, then you can see from the previous slides that almost all of the licensing curves underestimate the degree of swelling and incidence of rupture at one temperature or another. And even if you don't agree that the curves in the NUREG report satisfy the -- illustrate or show the minimum requirements of Appendix K, I think you still have to recognize that some of the vendor curves are way out of line based on the data that you have seen.

Finally, the cladding model discrepancies shown on the previous slide can produce large changes in peak cladding temperature. Norm talked about this, and of course there are compensating margins and other peaking factor changes that we can use to avoid the consequences of that. But these changes greatly exceed the threshold for action, which is 20 degrees F. as stated in Appendix K.

Therefore, we believe that we have got a problem with part 1(b) of Appendix K that must be dealt with. The question then boils down to should the changes be made now.

MR. SHEWMON: Where did this 20 degree F. come from?

MR. MEYER: Appendix K. Can somebody quote it? Appendix K says if you got a discrepancy of more than 20 degrees you got to redo the analysis.

MR. LAUBEN: Well, actually it says if the model results in a peak cladding temperature change of more than 20 degrees we have to report it.

MR. SHEWMON: Have to what?

MR. LAUBEN: Report it, and it has to be reviewed.

MR. MEYER: You know, if we were talking about changes in peak cladding temperature that were on the order of 20 degrees, then we could simply say that is interesting, but we are not obliged to do anything about that. But we seem to be well above that threshold.

Well, down to the home stretch here, the question whether ECCS evaluation models should be changed now is really broader than the question I am going to address. Norm will address the larger question with the schedule in a few minutes. I will address only the feasibility and desirability of making cladding model changes now in light of ongoing research programs.

The first reason that I would like to give you is that we have learned enough in the past five years to know that the present measure models are not very good. And even if you are not convinced that the research programs in the last five years have given us significant new insights or knowledge about cladding behavior, we are still faced with a rather recent revelation that most of the plant transients are in fact slow transients, not fast transients.

This makes a big difference, and a lot of the early work was predicated on having fast transients.

MR. SHEWMON: When you say most of the transients, are you talking about experience or what is stylish these days to make people calculate and come back at or redoing big LOCA's or what?

MR. MEYER: What I am talking about is when we actually looked last winter at LOCA calculations done for licensing purposes we discovered somewhat to our surprise that almost all of the ramp rates in these calculations were what we would call slow. They were not up in the vicinity of 20, 25.

MR. SHEWMON: This is the full-blown, big pipe, instantaneous, double-ended pipe break?

MR. LAUBEN: When he talks about slow, he is speaking of the rate of temperature change at the time rupture is occurring in a plant. Of course there are times when the clad temperature may be changing a thousand degrees a second or something like that. That is very early in the transient, and the pressure is still high in the system, and there is no danger of rupture.

MR. SHEWMON: Yes.

MR. LAUBEN: But at the point where cladding rupture is occurring, almost without exception -- there are two exceptions. The rate of temperature change is what Ralph has termed slow -- 10 degrees C. per second or less.

MR. SHEWMON: Well, someday we can also discuss the relevance of the instantaneous to the average, but that is a separate ramp rate.

MR. MEYER: Yes. We are, by the way, using a long-term, not an instantaneous ramp rate, both in the analysis and in the experiments themselves.

Well, just to underscore that point one more time, five, six years ago, when a lot of these NRC-funded programs were being started, we just did the 28 degree C. per second test, because we thought that they were all going to be that order of magnitude. And the investigation of the slower ramps is a relatively recent event.

Okay. The second point with regard to the imminence of critical research, I think that some false hopes were raised before this committee back in February. At that meeting a slide was shown by Westinghouse that listed three future research programs, and then the conclusion was drawn that the test schedule in the near future would provide data for development of more definitive cladding models.

None of those tests were designed to measure cladding behavior. They were all thermal hydraulic tests with predetermined fixed amount of ballooning and blockage.

MR. SHEWMON: There is nothing coming out of the NRU experiments on cladding?

MR. MEYER: Now, those NRU were not the three that Westinghouse showed. I am going to talk about the --

MR. SHEWMON: But they are done in the next five years I hope.

MR. MEYER: I am going to talk about the NRU right now.

MR. SHEWMON: All right.

MR. MEYER: We talked about the NRU tests at our February 14 meeting, and that program will indeed contain the cladding behavior tests. The initial tests in the NRU program that will be run later this year are unpressurized nonballooning fuel. So the first tests with ballooning and rupture are scheduled early next year.

I think the present schedule is for about January or February. The NRU program will run for about three years, June 1982, and the blockage data will probably not be available till near the end of the program. We also understand that schedule slippages are right now being discussed on the programs, slippages on the order of a year. And so it may realistically be late 1983, 1984 before the NRU blockage results are really available.

Now if you want to probe into the schedule for that, you will have to ask the Research people.

MR. SHEWMON: I have, thank you.

MR. MEYER: But I wouldn't count on results from NRU that are going to make a big difference in what we have done in less than two or three years. There will also be some more Oak Ridge outer reactor multi-rod tests, and there should be some additional tests from Germany, Japan, and England during this period of two or three years that the NRU program is running.

Most of the most new tests are multi-rod tests, a lot of in-pile tests. They are difficult, expensive. I think the results are going to be controversial just like the results from the first five years have been controversial. We have already heard disagreements about the shape of the shrouds and balloon sections and the number of ballooning rods in upcoming tests.

So I think it would be unwise to think that we will reach another plateau with a useable licensing position in less than five years from now.

The next point has to do with some calculations that were done after November of last year. There were some rough calculations done to show that there were compensating features available in the cladding models so that this whole business was a so-called nonproblem. Those approximate calculations really can't be relied on to satisfy the requirements of 5046, and the review schedule that we favor would upgrade those calculations in several steps as final model revisions are made.

I think there is plenty of indication that there are compensating features available. I don't think as a regulatory policy that we can rely on the approximate evaluations that have been done for an extended period of time. I do think we can rely on them to allow us to make revisions in an orderly and nonhysterical manner.

MR. SHEWMON: Would you explain what you mean by that last point on the slide -- near-term approvals are already needed for several vendor models?

MR. MEYER: Yes. I haven't gotten to that yet. I am going to do that right now.

The last point: it is an interesting point and I want to mention some vendors by name. Combustion Engineering, whose cladding correlations appear most out of line, have acknowledged the need to make changes and they have submitted revised cladding models that now need review.

The point that I am trying to make is that there are -- I am going to go on with some other vendors now -- but the point I am trying to make is that there are actions that are already needed in this area that have been precipitated by one thing or another so that this is, I think, an additional reason for going ahead and doing it now.

CE has some models before us which need review.

Westinghouse a couple of years ago added this ramp rate variable to their small break model. You remember Westinghouse has one correlation that has ramp rate as a variable, and that is the rupture temperature correlation. But it only has ramp rate as a variable in the small break model, not the large break model. And Westinghouse has contacted us after our round of questions last November and said that they really ought to be using the ramp rate dependence in the large break model.

So here is an item that needs an action. I think you could on a technicality wonder whether Westinghouse had an approved large break model at the moment.

Exxon. Now as I understand it, and I haven't confirmed this, but as I understand, Exxon has recently done some planned analysis for reload work, where they have already taken the cladding models from NUREG 0630, plugged them into their code, and done the analyses and submitted them for reload.

Technically, of course, I think that is a good thing to do, but again there is the question of whether that constitutes an approved ECCS evaluation model or not because it hasn't been blessed by the NRC staff and locked up in the safe with the others. And then finally I don't think GE evaluation models have ever been formally approved, but it is less important there because you don't even have this blockage concern.

Okay, that is the end --

MR. SHEWMON: Why is it with GE you don't have it because they are shielded, or clad or --

MR. LAUBEN: Go ahead.

MR. MEYER: No, you, please.

MR. LAUBEN: I was going to address your question, you said about them not having approved models. But I think you asked a different question.

MR. SHEWMON: Yes. I thought it was this blockage can't occur there.

MR. MEYER: I said they don't have one yet, it doesn't matter so much.

MR. SHEWMON: Blockage can't occur there because of the subassembly walls or what?

MR. LAUBEN: No, as I mentioned in my one slide that first of all, the lower pressure that GE tends, and the better heat transfer that you get because you don't have as large a break, causes them to have lower temperatures during blowdown. (inaudible)

MR. SHEWMON: Okay.

MR. LAUBEN: Now they do have blockage, but as I said, blockage is accounted for implicitly in the heat transfer model in GE's computer model, so it isn't as necessary to go through such and such -- --

MR. SHEWMON: Thank you.

MR. MEYER: They don't have a blockage correlation for example.

MR. SHEWMON: The world, as Ralph said, the world in which you live or we live is now set up so that these three correlations are what must be put in to keep the computers operating for these calculations. Is that right?

MR. MEYER: That is right to a first approximation. There are a few other cladding related pieces in the models. For example, prerupture strain and the axial extent of strain on a rod that has a rupture in it.

MR. LAUBEN: I don't think the computer programs are that limited as to what they can -- -- there was a better way to calculate blockage or strain. I am sure you could handle it as long as the variable is something that is a calculable, quantifiable material.

MR. SHEWMON: Okay, are there any other questions for Ralph? All right, why don't we take our ten-minute stretch now?

(A brief recess was taken.)

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MR. SHEWMON: Dr. Picklesimer, could you cover the schedule? He is down for general summary and implementation on my agenda. That was what was given me.

MR. PICKLESIMER: I have a rather large handout. I would appreciate it if you would go through it quickly.

I am going to surprise you. I am going to be sure that you have certain kinds of information available to you for study at your leisure whenever you wish.

MR. SHEWMON: Okay.

MR. PICKLESIMER: I plan on using 15 for 33, and since the pages are numbered we can go through them fairly easily here and quickly.

Let me start out by saying that -- and I speak for the Fuel Behavior Research Branch now, not just PIC. Now we have no problem whatsoever with the rupture temperature pressure correlation that is used in UH-630, nor do we have any problem with the use of the bounding burst strain curve when it is used for the calculation of fighting oxidation room, nor for hydrogen evolution.

Now, what we do have a problem with is the use of this bounding burst strain curve to calculate flow blockage. I am of the opinion that the burst strain has nothing to do with blockage. It has nothing to do with the material parameters that are important in blockage, and that it is a misleading factor.

Now, I intend to show you some of the reasons for that in the slides that I intend to bring up now.

I am going to propose a different approach which uses what I call average rod strain and which I will hopefully define later on satisfactorily, and pose the possibility of developments in the near future which I am prepared to discuss, if you wish, such as the FEVA work, which shows the effect of 90 percent blockage on the temperature of the cladding and the burst region and the simulated blockage region and in the bypass region.

In the Erbacher report, which was given at the Boston Symposium on Zirconium in the first week of August, which gives a new kind of correlation, a new method of correlation for getting at burst strains, calculating from, we think, material properties, but we are not sure; it is very complex.

MR SHEWMON: Is that still available only internally?

MR. PICKLESIMER: No, sir, I have copies. In fact, David has a copy.

This was the ASTM presentation given in Boston. Then finally I am prepared to discuss, if you wish, some of the Sengpiel-Borgwaldt calculations on probabilistic analysis for blockage in bundles, if you wish.

Now, I understand that translation has been sent to me in the mail, but I haven't been able to put my hands on it yet.

That one is, so far as I know, still in German, but the translation should be available shortly.

Now, it is my thesis that the average rod stream and cross section of a bundle can be calculated and can be used to calculate the pressure drops measured there in flow tests. You saw some of that data a little earlier. I will have to show you another --

MR. SHEWMON: Now, I assume that Ralph would completely agree with that first statement, what you have just had on that slide?

MR. PICKLESIMER: Yes.

MR. SHEWMON: I don't know how he massages that data, but it seems to me --

MR. PICKLESIMER: That iswhat I am going to prove.

MR. SHEWMON: You are going to explain what you mean by that statement?

MR. PICKLESIMER: That is what I am going to show you. All right. Now, this is a table showing the sectional sprains in each of the rods for all 16 rods in the bundle. This is the blank by blank sections taken a centimeter and a half on the bundle. I want to consider the strains between one grid and the other grid.

This is what I considerthe test section, right in here. All right. If I take the strains at each of these cross-sections, average them, then I will wind up with this column of average strains.

This column of numbers then is put into COBRA as a single subchannel to calculate this curve right here. These are the experimental pressure drop measurements that were made.

Now, the only ones that went into COBRA were these average strains right here. That tells me then I can use the average strain in the cross-section to calculate the pressure there measured in flow tests.

MR. SHEWMON: How does that average strain differ from Ralph's average strain?

MR. PICKLESIMER: It is no different. It is the same.

MR. LAWROSKI: It includes a rupture?

MR. PICKLESIMER: Where the rupture occurs? Yes, the rupture is closed off so there is no credit taken for the passageway of the rupture.

MR. LAWROSKI: I am not sure that is entirely clear. What you are showing are all the strains, including the strains, rupture elevation and all others.

MR. PICKLESIMER: From the cross-section.

MR. SHEWMON: And what you used were inscribe smears which would correspond roughly to the pellet diameter. Is that right?

MR. PICKLESIMER: No, sir, to the circumference of the first rod. Circumference of the first closed up to make a circle. That calculates a rod strain for that particular point.

MR. STRASSER: Circular or straight line?

MR. PICKLESIMER: The circumference of the first is the circumference of an equivalent circle.

MR. STRASSER: Oh, I see. You push it together.

MR. PICKLESIMER: Push it together; that is right.

Now, Chapman has done two types of measures. He takes a straight line across the lips or you can close it up. It makes only --

MR. STRASSER: Preburst diameter.

MR. PICKLESIMER: Yes. It makes only a small difference, a few percent in any of the calculations you want to make any way with us, so it doesn't really make all that much difference.

Now, the other set of data that are important here is the average rod strength for each of the rods in this bundle. Now I am averaging between these two points only. I am not averaging over the full length of the rod.

Now, if I average the average axial rod strains, I come up with 19.2 for the average rod strain in the bundle in the test section. If I average the cross-sections I come up with 19.2, as I should. I sampled the same body of data, just different ways.

I come up with sigmas for these, 3.5 and 3.4.

I submit that the average of the axial rod strain is as good a measure of the blockage in the bundle as is the

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average of the cross-sections.

Now, the correlation I do not yet have is for the maximum plating of blockage in that. This is stuff I was doing Monday at home without complete references of library or anything else, and I don't have the correlation complete, but I know that this works. I think we can make this one work.

Now, we can --

MR. STRASSER: Is there preference for one over the other for some physical reason?

MR. PICKLESIMER: Yes.

MR. STRASSER: Yes. Measurement reason?

MR. PICKLESIMER: Yes. We have data in one way and not the other.

Now, here was a set of measurements made the same way on that with this bundle, with two different correlations for fraction factors, and that makes a difference in the two different regions of fit. So this part has been fairly thoroughly covered.

Now the correlation that Dr. Meyers and Powers used to get at the blockage is through the burst strain. I contend that the burst strains are not physically related to the average strain in the rods nor to the loss of flow area in the bundles.

Now I want to show you one of the reasons why I think they were misled. Here is a plot of the rod average strain for each of the individual rods in the bundles against the first strains in that rod. Now this is not against the blockage plain. It is not the maximum burst strain, it is for every individual rod using the axial rod strain average.

Now the slope for the best fit of those points is .5, whichis essentially the ratio they came up with, and it is bounded by a slope going from .4 to .65.

I think that this is a fortuitous thing. It just happens to be those three bundles that give it that way. If we go to other bundles it won't happen.

Now one of the reasons I say that is this. I take that same table that I used before to get at this block. I take out all the burst strains and replace each of the burst strains by the nearest neighbor non-burst strain, and some of these are going fro things like 67 percent down to 40 percent.

I do my same averaging everywhere, and I find there is no significant difference. I go from 19.2 to 19.0 for my average strain in the bundle. I can remove all these and it makes no difference in the analysis. My analysis is better than the first one.

Now, with and without those burst strains here -- I have the table comparison of the individual rod strains, and I have underlined only those that have made changes. I am going from those of 19.6 to 19.4; 19.4 to 19.6. The largest one in there is 23.8 to 25.1. I think that is probably the largest difference there is for an individual rod, and it just doesn't make that much difference in the bundle average.

MR. SHEWMON: Tell me again what I am supposed to have in my notes for that?

MR. PICKLESIMER: This is all material that I covered last spring, so you should have a more complete set of notes. This is the table of values with the burst strains. This is the ones without the burst strains for calculating the pressure drop with the covered measurement, and what I am saying is it doesn't make any difference whether you include the bursts themselves or not, if you include a large strain, the next neighbor.

So I can throw the burst strains out and not make a significant difference.

Now I am going to show you here a plot -- this is on Figure 8, or page 8 -- which shows you the start of what I want to propose as an alternate. The bounding curve here is almost directly out here around 630. I am not sure that I got all the points right. I was reading a graph, and I am not sure that I applied it exactly accurately, but it is close enough.

I took all of the rods inthe bundle and a number of the single rod burst tests that Chapman has run recently. I don't have all the data, but I have most of it, and I plotted on here the average strain in the rod, axial average, and the burst

burst strain, and then for most of the single rod tests I connected the average strain with the burst strain for that particular rod.

Now some of these, the average strain drops when the burst strain increases when you start comparing one rod to another, so the conclusion that the average strain is greater when the burst strain is greater is not correct. It may be smaller. You never know.

I have included here the average strains for each of the rods and the burst strains for each of the rods in each of the three bundles. Now I have enveloped this set of average strains with the proposed curve. I am proposing to use this curve to go directly to the full blockage calculation, rather than the burst strain to the average strain for the flow blockage calculation.

MR. SHEWMON: What is that bunch of sparrows between the two lines there?

MR. PICKLESIMER: This one in here?

MR. SHEWMON: Yes.

MR. PICKLESIMER: Those are the first strains for bundle B-1 and B-2.

MR. SHEWMON: Okay, and there are lines over here for --

MR. PICKLESIMER: Yes, for single rod tests. B-3, average strains right in here and first strains right in here.

MR. SHEWMON: I guess you connected some lines and others without lines.

MR. PICKLESIMER: I have connected single rod tests now. Whether they have been run as single rods now, I have connected --

MR. SHEWMON: Okay. We will go back to that set of points I first asked about, B-4, B-2 or B-1, B-2.

MR. PICKLESIMER: B-1, B-2. These are the average strains of the B-1, B-2. These are the burst strains. This is a single rod right here. This is a single rod.

MR. SHEWMON: But B-1 was a group of rods?

MR. PICKLESIMER: That was the first bundle, 16-rod bundle.

MR. SHEWMON: Yes.

MR. PICKLESIMER: You could look upon these really as being replicates of a given test run.

MR. STRASSER: Does the same phenomena hold true in B-1 and 2 as B-3?

MR. PICKLESIMER: Yes. This is B-3 right here for average strains and burst strains. Now this B-3 was a slower test.

MR. STRASSER: I meant does the same hold true for B-1 and B-2?

MR. PICKLESIMER: Yes.

MR. STRASSER: Diverging burst versus average rod strain?

MR. PICKLESIMER: Not likely it does for the individual rod strains here, no. They are much more tightly grouped.

Now, these are individual tests that have special heating rates. Some of them are pretest. Some of them are wrap test. Some are isobaric tests. They are grouped together as an average strain, where the burst strain does not.

Now, the problem that I see here -- I will agree that you can form the ratio for the average rod strains versus the burst strains for the bundle test. You can average that. That gives you a number. I claim that that number cannot be used outside the temperature of your data plots. You can't use it anyplace else because the burst strains, ratio to the average strains does not represent a material parameter. There is no physical basis for that.

Therefore, you can't take an outside temperature range. Now, I show you sme fo that. I have here a set of data points where I have calculated the average ratio of the average strain to the burst strain for individual rod tests. I want to emphasize that now, for the individual rod tests over a large temperature range.

Now, I can classify these into several groups, e the strains are less than 30 percent, circular points, where I have no circle -- another symbol on the points, like these three right here. Their strains lie between 30 and 60 percent, burst strains; diamonds give me strains greater than 60 percent; squares give me strains greater than 90 percent. The larger the strain in general here, the larger the burst strain the smalle is this average.

Now, the correlation that has been used in NUREG-0630 lies a set of points right here about .45 to .5, in this region here, and I say that as the burst strains go up you do not have an increase in average strain. This ratio was found, especially when yougo to another country. Therefore you cannot use that outside the temperature where you have actually made your calculations of data.

Now, I would like to make this particular statement as an emphasis. If a standard flow blockage curve must be establishe now for use in auditing vendor models, then I suggest that it be done based on average strains in the rods, not the burst strains.

We have average rod strain data that is in about as good a shape or as good as the burst strain in the selected data pool that we have to work with. If we don't have all of the numbers, they will be readily available in a few months time. All we have to do is go and pull them out.

I know that the Germans have axial rod profile data on their burst rods. The only data that are reported in the literature are burst. I can ask them for the average. They can give us that. Thatis no great problem within a short period of time. Chapman does not have average rod strain calculated as I wish it to be calculated for all the single rod tests. He does have rod strain profiles. They can pull this out in a few weeks' time. It doesn't take long to get together a suitable set of average strain to use for this, as far as I am concerned.

One of the big points on this is that it is directly applicable to flow blockage calculations, yet if single rod data can be used at all, I am not yet convinced that single rod data can't be.

Now I would like to propose then this as a possible bounding curve for calculation of flow blockage. I have on here the average rod strains for the single rod test. All of the B-1 data points fall within this rectangle. All of the B-2 fall within that, and all of the B-3 fall within that except -- I am sorry. This is the average plus one sigma.

There are a few points that fall outside those, but not very much out. So these blocks represent the average plus one sigma for B-3, B-1 and B-2.

If I then bound that with this curve here -- I am suggesting this as a starting curve for looking at a calculation to flow blockage. Now, we could improve this by simply getting more data points, and I know of at least 25 or 30 more that are available. We just don't have them.

MR. SHEWMON: If we stayed with that, how does that differ from what -- or if you backed the flow blockage out of

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that, how would it differ from a suggested flow blockage curve, or have you had time to do that?

MR. PICKLESIMER: I haven't had time to get at flow blockage itself. The thing is I am substituting this average strain curve for the one that is calculated from burst strains for NUREG 0630.

MR. SHEWMON: Well, go back the other way. You said the correlation that they had was the slope of one-half. If that is right --

MR. PICKLESIMER: Burst average, yes.

MR. SHEWMON: Then what they would say would be the maximum strain for this curve would be 70 percent, and it would decrease monotonically to go to higher temperatures.

It would correspond to a burst strain of 70 percent maximum.

MR. POWERS: It would correspond to a maximum burst strain of 9 percent reduced by .46. It would be the maximum.

MR. PICKLESIMER: You would be in that neighborhood, yes.

MR. SHEWMON: I guess I don't know what .46 is per units.

MR. POWERS: Point four six is the reduction factor which we --

MR. SHEWMON: Are we taking 90 percent and multiplying it by .46 or are we subtracting half a percent?

MR. POWERS: Multiplying it by .46 and claiming that as what is to go into the flow model.

MR. SHEWMON: Thatis the average strain, so 90 times .46 must be about .4.

MR. POWERS: That would be the maximum.

MR. SHEWMON: Okay. So he ends up with five or six percent less maximum strain than you do, average strain.

Okay.

MR. PICKLESIMER: All right. Last strain in our discussion, I presented to you a set of strain profiles of the individual rods in the free bundle test. I simply want to show youhere my conclusion on that. You have in your handout the actual plots that I used.

Now, the point here is that the same profiles of the individual rods in the four by four bundles -- single rods now as far as I can see cannot be used to calculate the bundle data for any single rod. I have not yet been able to find any full rods in there that will allow me to calculate the bundle characteristics from these four rods.

Whether we can go on to nine or not I don't know. I haven't made all of those calculations. The point is that one single rod cannot be used to characterize the bundle properties.

Now, I think that --

MR. SHEWMON: Let me come back to that for a minute. What you showed on your first coupleof slides with all kinds of numbers was that the average -- in my own mind called picture body theorem, but that is a separate story -- that the average across the plain equalled the axial average when you use the whole set.

MR. PICKLESIMER: Yes.

MR. SHEWMON: And now you are telling me that if you take one pin out of that and take an axial average, that doesn't correlate well with what you get for the average of the set.

MR. PICKLESIMER: It does with the average of the set but not for the bundle characteristic. Let me show you.

This is page 19 of the handout, and this is a frequency plot of the times a certain strain -- in the strain increments was encountered within the test section. It is just a standard frequency, increments of strain.

Now if I sum over here on this side the strains at every cross-section here on this, I line up with a certain profile here. There is not one single rod nor any four here in here I can put together that will get me that profile.

What I am submitting is that in single rod cannot be used to calculate that profile. Therefore, I am concluding that I cannot characterize the bundle from a single rod. I have got to have multiple rods, and I don't yet know how many I have to have, nor do I know what I have to have for an average rod property and a one or two sigma. That I don't know yet. We shall have to come up with that.

MR. SHEWMON: How many did you have in your bundle? Sixteen?

MR. PICKLESIMER: Yes.

MR. SHEWMON: But you think 16 works from the first half of your discussion today.

MR. PICKLESIMER: That is all the data we have. I can't characterize a bundle of 16 from a single rod.

MR. SHEWMON: And thus you are saying you are not sure you can't characterize 14 by 14 bundle from a four by four bundle.

MR. PICKLESIMER: That is right. We are not. That is why Chapman has done an eight by eight, because we are in the process of characterizing flow by flow amount.

All right. I feel that a statistical approach must be developed eventually for the estimation of a flow blockage in the bundles. I don't know how this is to be done yet. This still has to be worked out, but I think we are on our way.

There are three recent developments that I want to mention here, and I am prepared to discuss them if you wish. Otherwise I will not discuss them.

Erbacher of PFP presented at the Boston symposium a calculated procedure for determining the time of burst. Knowing the time of burst then was the creep equation, an oxidation equation of burst stress correlation and input heating rate and initial pressure. He can calculate then burst strains, burst stresses, all of the other parameters you want for the burst itself.

Now, I think this same line can be used to calculate the rod average strain, but I haven't had time to work it out yet to see if it can. But I thinkthe same procedure can be used.

Dr. Hagelman at EG&G working on our metro work and track team has been modifying balloon-2 code, which is a subcode of flat-T, to allow calculations on the axial strain profile of ballooning rods during ballooning at any time in the temperature range using statistical variation, appellate dimensions and power, axial and asmysal temperature gradients.

Now, he does this -- he is calibrating this at the present time with some of Chapman's single rod tests where we know the infrared heater scan of the heater. He is using this now as input to see if he can predict the axial strain profile that Chapman observes in the single rod test.

The indications are that he will. His calculation so far has only been made with the sine wave input rather than an infrared scan, but that is what he is doing this week and next, but we will know within a month whether he is successful on that or not.

If this is true, then we have a statistical approach for getting at the axial strain profile of a rod. Therefore, we can get the average strain in the rod at the time of burst.

The more important, I think, approach that has been developed recently was a presentation that Sengpiel and Borgwaldt made at our annual information exchange in Carlsbad last June, where they have done a probabilistic analysis or rod strain and flow blockage in a 15 by 15 bundle.

They used response surface methodology, statistical variation, rod geometry, temperature gradients and some other things, including neighboring cold rods.

Now I will show you a couple of slides from that set of presentations that I think are very important.

Now, this is a 15 by 15 bundle at the plain of maximum blockage which he deliberately forced by a very sharp cosine power profile to occur in a single plain. This is a worst case as far as he is concerned, and these represent that rod strains that he calculated using the Erbacher burst strain correlations.

Now, he then looked at the subchannels in each of these places and he calculated the probabilities for those having less than 30 percent passageway and less than 20 percent passageway. This is a plot of those having less than 20 percent passageway left in the bundle.

Now, he also defines what he calls blockades. In this particular group right in here there are two in there that do not form or have greater than 20 percent passageway left. What he does is take those two out and collapse all of those into one cluster now that has no holes in it. He calls that a blockade.

All right. Now, he looks at three different classes of rods. There is one in here, one a higher power and one still a higher power within that bundle. He took into account full rods, thimbles, neighbors, so on and so forth.

Now, with that they calculate this set of parameters. The plotting here is a distribution, probability distribution for blockades of certain sizes, a cluster such as blocked, say, from greater than 30 percent.

Here are his probabilities for peaking here, a cluster of six subchannels. That has a maximum probability.

A cluster of 10 is almost zero probability. For 20 percent passageway, his maximum probability is for a cluster of four neighbors. That is a three by three array of rods.

A very low probability for a cluster of eight. A cluster of 16 is essentially improbable by his calculations. A cluster of 16 is a four by four array of subchannels and a five by five rotary, and he is saying that that is probably by his calculations the maximum size of clusters you would ever observe in a balloon burst bundle.

Now, this remember is a conservative forced calculation where he is forcing his burst to occur in a given plain, all of them. Now. not all the rods burst.

MR. SHEWMON: How is he getting the variability within that plain?

MR. PICKLESIMER: He has variations on the rod power in the neighborhood, and rod to rod power, and thermal hydraulics and surroundings, like cold rods. He is getting all of these as statistical variations.

MR. STRASSER: Is he using Erbacher's methodology?

MR. PICKLESIMER: For the burst data, yes.

MR. STRASSER: You previously mentioned that you don't think you can use single rod tests to apply to bundles.

MR. PICKLESIMER: Yes.

MR. STRASSER: How can you then use Erbacher's single rod methodology to apply to the bundle here? Is there something different about it?

MR. PICKLESIMER: There is in this way. Erbacher's methodology is an averaging of a number of tests, many tests. Many and some of these are duplicate tests. Others are -- you have 20 data points on a curve and you are averaging a whole bunch, so he is not just using single rod data, but he is calculating on the first data now. We have to still look at this for getting rod average strain. I am not sure we can, but we have to work at it.

All right. Now, the Fuel Behavior Research Branch has the following suggestion to make concerning the licensing actions involved in fuel rod ballooning and flow blockage in bundles.

The first thing is that if a flow blockage audit curve must be established at this time, let us base it on an average rod strain, not on burst strains. I believe it is on a sounder basis technically, and it is much more easily defended, and it can be extrapolated over different temperature ranges, although we don't have all of the data that we would like.

The developments that should occur in the coming year in code analyses of ballooning and flow blockage in fuel bundles should provide a much sounder basis for auditing flow blockage calculations by vendors than will be available from the use of NUREG-0630 correlations.

Proper combinations and modifications of Erbacher's burst criterion, Balloon-2 code, ORNL-MRBT average strain data and the Sengpiel/Borgwaldt probabilitistic approach should permit best estimate pretest predictions to be made for the NRU tests and for the larger bundle tests that may be scheduled in Loft if we can get them to go.

Now, I have already talked to FRAPT people on this, and they don't see any great problem in doing this.

MR. SHEWMON: You talked to what people?

MR. PICKLESIMER: Code people at EG&G.

MR. SHEWMON: Code people?

MR. PICKLESIMER: Yes, people whoaare doing the FRAP-T developments. It is a branch of NRV.

The FRAP-T people at Idaho is what I am talking about.

Now, we can see within the branch that we should have a complete and verified code for best estimate correlations for flow blockage in large bundles available in less than five years, verified by both ex-pile and in-pile large bundle tests. These are the NRU and Loft, as well as out-pile bundle tests running up to 32 rods.

That completes my presentation.

MR. SHEWMON: Is this the first time you have heard this?

MR. MEYER: Some of it as he mentioned, is very recent work.

MR. RUBEN: We have been establishing our requirements. Now, if the measures that are being given here support flow blockage, I have no objection if there is a sufficient amount of data pursuing Dr. Picklesimer's flow of the art approach.

Is this material based rather than a phenomenological approach? Perhaps it will be verified , or in the intermediate term, perhaps a couple of years. However, if someone has any average strain model or sufficient data, we would be happy to consider it.

We also would exercise some constraints that the flow blockage model predict the bundle data and flow blockage data as perhaps represented in the new HE-14 and 15, and I guess --

MR. SHEWMON: Sorry. What report?

MR. RUBEN: -- would suggestthat the strains would not be too much given.

MR. SHEWMON: What is the new report 13 and 15?

MR. BURMAN: Figures 14 and 15 of 0630. Those two figures show actual measures of flow tests. What we are saying is that it is relatively unimportant how you get there if you have different approaches. That is fine. Just make sure that you come out with a blockage model that predicts reasonably well the blockage data.

MR. SHEWMON: Yes, I guess discounting partly the enthusiasm of the research man for how soon he is going to get results, I certainly have -- I would be happy with an average strain than a maximum strain, I guess. It just seems so much more plausible physically to me.

Now, you can say I massage my numbers three times and get my point, and you may be right. That is my interpretaion of what you told me when I asked you that before, but if you can bring it in the first place, then you don't have to assume that when you massage your data once that it is for all temperatures or whatever sorts of things there are.

MR. MEYER: Thereare still some massaging assumptions in here. They are less risky than the ones involved in our method.

MR. SHEWMON: Yes. I am only talking about the first part, not the statistics of the last part, which is an interesting separate exercise.

MR. MEYER: Just in the assumption that the axial average strain equals the coplanar of the strain, I think one needs to look carefully to see that that holds for all strains.

MR. SHEWMON: Yes.

MR. MEYER: For example, the model would absolutely preclude 100 percent flow blockages. You wouldn't have an average axial strain depending on -- you would have a -- I think it is abetter approach. I don't mean to be quibbling about that.

It is relatively unexamined, and I can see that if you pursue the same avenues of critique that have been pursued with our model, that you will run into similar problems. My guess is that they would be more forgiving than ones that we ran up against.

It is a matterof what is in hand and what isn't.

MR. SHEWMAN: Yes. George?

MR. MORINO: I would like to point out that --

MR. SHEWMON: Louder.

MR. MORINO: We will have to have a correlation factor for the average rod strain versus the average of the strain of maximum blockage, and that correlation will be subject also to critique outside the range where we will fund the data.

Now we are hoping that since we have material properties, that zircaloid is non-stable. We want to be able to predict that. But we think it is a better approach.

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MR. PICKLESIMER: A very quick, cursory examination this morning of the bundle data indicates that the maximum blockage is equal approximately to the average rod strain in the bundle, plus two sigma of that rod strain. So it looks like we are not too far away from having a satisfactory usable correlation, but it has to be looked at.

I just think we are on sounder ground for going outside the temperatures where we have data.

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MR. SHEWMON: Very interesting, Dick. Thank you. I don't promise any comp time for Monday but we appreciate it.

Gee, the rest of the afternoon is scheduled for Westinghouse, it says. I hope you don't take all the time you are scheduled for.

MR. ESPOSITO: We will at least give you the hour.

My name is Vincent Esposito. I am the Manager of Safeguards Engineering forWestinghouse. There are a number of items we would like to discuss this afternoon. All of the presentations that are being made, the handouts have already been given to you. We will not go through every one of the s lides there.

We have included the slides for information purposes and for making the points more vivid, other than just some one-line comments. So you have the basis for many of the comments we plan to make.

The items we wouldlike to go through this afternoon are first what the issues are as we see them; namely, what are the differences of opinions between ourselves and the NRC model.

We would like to then look at the fuel aspects. That is what you have been hearing basically today, with the exception of Norm's presentation. How we interpret the data, our overview of the technical issues relative to fuel aspects; and our results of the review of the data that you have been seeing this morning.

The next item we would like to go through is the heat transfer of flow blockage information. Dr. Meyer made a comment earlier that we were using this as a basis for saying that the fuel model or swelling should be. Really what we are doing here is to show the thermal hydraulic effects of blockage in terms of what the experiments are giving us.

Dr. Hochreiter will give an overview of the available data and some recent Flecht-Seaset data which we received over about the last three months or so.

One issue that has been brought up a number of times today is what is the potential impact of NUREG-0630? And we will present peaking factor impacts from evaluations that we have performed to date using 0630, and we will give you that information.

Then finally I will wrap up with some conclusions and recommendations. Some of the conclusions will be technical and some will be philosophical, and the recommendations will be very pointed.

Let me first discuss what the issues are. There are three basic issues. You have been hearing about them all day today. We are talking about burst temperature, strain and blockage. The differences of opinion between Westinghouse and the NRC is the following.

In terms of the burst temperature, Westinghouse does not agree with the NRC's heat uprate dependence on burst temperature. As it was stated earlier, we did put in our small break model a heat uprate dependence on burst temperature. That dependence, that model shows less of a dependence on heat uprate than what the staff shows, especially down in the low heatup range, and that will be discussed in detail by Dennis.

In terms of burst strain or strain information, I think we all agree it is important to use prototypical data. I think the problem is that we all have a different interpretation or a different definition of what prototypicality is. Again, Dennis Burman will cover each of these fuel aspects.

Finally, in terms of going from strain to blockage, the use of a statistically average, not maximum strain to arrive at a flow blockage we believe is appropriate.

Those are the three basic issues as we see them. We will address each one of them in the technical presentations that follow.

Just for giving you a frame of reference again in a little bit more succinct way than may have been done earlier, I want to tell you what the different parts of those models do in terms of the evaluation.

For example, if you look at burst temperature, indeed this determines the incidence of burst. If we use NUREG-0630 we will get earlier burst. This results in one more zirc reaction and two, higher gap conductance after burst. Both of these will give you a peak clad temperature --

MR. SHEWMON: The first one I understand. I guess I don't understand the second one.

MR. ESPOSITO: The reason for the second one is that by bursting earlier, the clad hasn't expanded or hasn't formed away from the pellet, so if you burst earlier your clad is closer to your pellet, so you have better communication in terms of heat transfer between the pellet and the clad.

MR. SHEWMON: You are saying the average strain is lower at first if you burst earlier?

MR. ESPOSITO: That is correct, yes, and that will give you higher heat transfer between the pellet and the clad, and therefore, increased clad temperature. So that is what you see.

MR. SHEWMON: I mean, in essence he is saying that the rputure strain to average strain is higher -- there is less average strain for rupture earlier, even though it bursts someplace.

MR. ESPOSITO: At the non-burst node, the clad will not have moved away as far from the pellet if you burst earlier. This is the non-burst node.

MR. SHEWMON: That must have more to do with your model than data, though.

MR. ESPOSITO: Well, this is the result of what you will get if you use the model that we have in our calculations along with the NRC model.

MR. MUENCH: As I mentioned before --

MR. SHEWMON: Are you from Westinghouse?

MR. MUENCH: Yes.

MR. MUENCH: Rick Muench from Westinghouse.

A valid point made earlier was only three models have changed, burst temperature, blockage and burst strain. Swelling prior to burst model did not change, okay? So we have not changed swelling prior to burst in a manner consistent with burst temperature change, so by bursting earlier all we do is we burst at a point where the plant has swollen on the average actually to a less extent.

That is what gives us the difference between -- it is a function of what you would get if you raise --

MR. SHEWMON: But it is a function of your models, not the NRC models?

MR. MUENCH: The NRC models.

MR. LAUB: Because the NRC models --

MR. SHEWMON: Let's let the NRC speak up.

MR. LAUB: NUREG-0630 concentrates on bursting and in answer to your question earlier, what about other mechanical things. I said NUREG-0630 is not addressing pre-rupture strain, so that is as Rick said, not being affected.

So if you are bursting earlier, your strain on locations other than the burst location is not going to be affected. It is just going to be less.

MR. SHEWMON: But 10 minutes ago Ralph was telling us that he assumed the average strain that he related to flow blockage increased with burst strain, and therefore, he should have a very high average strain. And you just don't talk to him about that part with your model.

Is that what you are telling me?

MR. LAUB: And I think I also said that if we are going to talk about blockage in the future it is going to be related to average strain, it is going to be very important to review pre-rupture strain model.

MR. SHEWMON: Is that a commitment?

MR. LAUB: Well, I guess it has to be, depending on what is being submitted.

MR. SHEWMON: Okay. Touche.

MR. ESPOSITO: These are the two ways in which the burst is -- the next one in terms of burst strain which determines the strain at the burst location; again, with 630 giving more strain, which leaves more surface area for this ZR-   water reaction again. I think that is pretty straightforward

Finally, in terms of blockage which determines the cooling penalty, NUREG-0630 gives more blockage, which will also give us an impact in terms of peak clad again because of what the models are doing.

So this is in general how the three areas affect the ECCS calculation given models that we have today. So I have tried to crisply give you the issues and their impact. The first part of our presentation will be by Dennis Burman on the fuel aspects, and that will be followed by Larry Hochreiter on the heat transfer and flow blockage information.

Dennis?

MR. BURMAN: Before I start on my presentation, I would like to address a few points that were raised earlier. That is, in discussing whether there were cold tubes in any of the multirod burst tests. In the Westinghouse multi-rod burst test, we had two cold tubes in a four by four array, so that we had 14 heated rods and two cold rods.

I have not done an analysis of the strain near the cold tubes versus other places, but I have looked at the direction of the bursts, and I find that with a very high degree of confidence you will find that there is a cold wall effect in that all of the hot spots which had already been burst were pointed away from the unheated rods.

So there is a temperature localization effect there, and I would expect that to carry over into the strain because as we will get into later, the amount of strain is a direct function of the temperature non-uniformity, and all the other effects are second order.

There were a couple of comments made about the Westinghouse multi-rod burst tests that I would like to tear up; first, that we used a spray coating. We did a careful analysis of the use of the coating prior to doing these rods, and we burst rods in a single rod mode with and without the coating, and there was a preparation of the cladding, sand-blasting, roughening of the surface prior to putting the coating on.

We tried it both on prepared rods, rods with the sand blasting, and rods with the sand blasting and coating and found that the coating never resulted in less strain in virgin rods which had received no treatment, so there was no reduction of strain due to the coating.

There were a few rods failed by arcing. There was less than 10 percent of the total rods tested, and therefore, although that is a source of non-conservatism, it is not a large factor. So I like to keep the record straight on that.

In Ralph's curve on maximum strain, he shows some points up quite a ways above his burst strain curve, and as I look at those points -- and I talked to Bob Chapman -- most of those points were points in which the heater rods had a linear power of the order of .3 or .4 kilowatts per meter.

Now, the power in our rods in LOCA is about .7 kilowatts per foot. These very low power levels result in a low heat flux across the gap which results in low circumference of temperature differences and make them non-prototypical tests.

The same criticism, I believe, applies to the Erbacher test, although I don't have a one to one correlation. I recall Franz Erbacher telling me that they should not be used because they were very low powered tests and not appropriate for -- and not prototypical.

MR. SHEWMON: You are saying low power goes to high strain?

MR. BURMAN: I will get into what causes large strains and small strains a little bit, and it is essentially a temperature difference around the cladding, as shown by Argonne, and that can be shown and we will in the future be able to present you data that shows that that is a direct function of the heat flux across the pellet clad gap, and not a function of heat up-rate.

This is just quickly what I propose to cover, that our small burst temperature model shows good agreement with ORNL and other data, and that there is no need for a new correlation, that our burst strain data and correlation show good agreement with the ORNL multirod burst test individual rod burst strains, and the difference between the NRC and the Westinghouse models is the use of maximum versus average strains.

MR. SHEWMON: How do you define average?

MR. BURMAN: What I am talking abouthere is not the raw average, but the average burst strain. In other words, as you recall from some of our previous slides -- and there is one in your handout which shows a block of Westinghouse data that shows some rods with very high strain and some rods with very low strain, depending on the particular temperature distribution that they receive.

I think I included that. It is one I have showed you before anyway.

MR. SHEWMON: I am sorry. The answer to my question is that you have averaged how?

MR. BURMAN: These averages are average burst strains.

MR. SHEWMON: Oh, not on a four by four or some cluster?

MR. BURMAN: No, these are the average burst strains of a whole lot of individual bursts.

MR. SHEWMON: Okay. I thought that is the way Ralph defines his maximum burst strain, isn't it, or he could define it that way.

MR. BURMAN: In that sense Ralph's definition is very similar to ours, but then he applies it to much higher single rod strains than we do because of his use of data with very low circumferential temperature differences.

A quick rundown of the comarison of the data bases: In numbers of single rod burst test data points, Westinghouse has a total of 261 versus the ones reported in NUREG-0630, which were 178 in total.

MR. SHEWMON: I take it you have made all 261 available to the staff?

MR. BURMAN: They have been available to the staff since our early models.

MR. SHEWMON: Okay.

MR. BURMAN: The number of multi-rod burst test data points were 11 in either case, and I think more importantly, the statistical characteristics here in that our tests were run under a single set of conditions with many tests at the same condition in order to get statistical variation, whereas the material in NUREG-0630 seemed to be heterogeneous. They are from many different investigators using different methods, and very few were tested at the same conditions, so they don't have good statistical scatter data.

Therefore, they used essentially an eyeball upper bound.

The question of prototypicality: We have tried to prototype expected inpile temperature differences because that is what is going to cover strains in a reactor. In NUREG-0680, the prototypes are only internal heaters, regardless of whether they had the proper power and steam-cooling on the outside or whatever. They were anything that was done with an internal rod heater; it was considered prototypical.

Here I have a curve of the ORNL single-rod burst test, burst temperature curve, and that is compared with the Westinghouse small grade curve for the same 28 degree C/second data, and you will notice these dark lines here, bound the Westinghouse region of design interest.

This is where we are designing reactors, from here to here. If you will note here, they have a cluster of data here in which our curve fits a group of data as well as theirs does.

Furthermore, since we know that the measured temperature always has to be some amount less than the burst temperature because rods always burst at the highest temperature -- it may be very close, but there is always a delta -- the only point that they have other than here within our data range is this single point here, which is really fitting our curve better than their curve.

So that in the area where our curve is non-conservative with respect to their curve, the data actually fits our data better.

Now, I don't know about the Argonne data base. Do you have any data from Argonne that fills in that area?

MR. SHEWMON: What is your point on this?

MR. BURMAN: The point is that they are saying that we are non-conservative because we are higher than they are, but the data itself supports our curve better than theirs in this region.

MR. SHEWMON: Okay.

MR. BURMAN: Now we don't care really what happens here because we are not designing over here, and we are not in bad agreement over there.

MR. SHEWMON: And that 20 degrees difference is enough to argue about?

MR. BURMAN: That is enough to argue about.

MR. SHEWMON: Okay.

MR. BURMAN: Now, further discussion of the ORNL single-rod --

MR. SHEWMON: While you have that there, one of Esposito's comments was Westinghouse doesn't agree with NRC's heatup rate dependence. Youare going to get to that later?

MR. BURMAN: We will get to that later.

MR. SHEWMON: Okay.

MR. BURMAN: This is a plot taken from one of the ORNL rotary reports which shows the temperature heat-up at an elevation near the burst for four different azimuthal locations, and this is for what is called in NUREG-0630 a 28 degree C per second heatup rate. Now, the 28 degree C per second is a curve with parallels in this lower part.

Up in here where the strain is occurring, you can see that the heatup rate is much slower. Now, this is only representative. Here it is about one-half of the 28 degrees C per second. But we looked at many of these curves, and we found that some of these were negative heatup rates in these last three seconds. Some of them were zero, one, five, whatever.

In the suggested way of implementing 0630, they requested we wait the heatup rate, since it is not a constant in a reaction, towards the time just prior to burst, the highest temperature area. If we do that, thenthis should not have been a 28 degree C per second heatup rate. It should have been something like a 13 degree per second.

MR. SHEWMON: It looks like a negative heatup rate to me.

MR. BURMAN: Well, that is after burst. When the pressure drops off here is the burst time.

MR. SHEWMON: Which one of those is pressure?

MR. BURMAN: This is pressure. I am sorry. The temperature scales were arbitrary, so --

MR. SHEWMON: It seems to me anytime you get into these two parameter curves for something as complex as a LOCA, you have got approximations. Is there any particular resemblance to the annointed LOCA calculations heatup rate and that heatup rate? Was that sort of chosen to get an average, or does that correspond to what is likely to happen during a --

MR. BURMAN: What happens here is a --

MR. SHEWMON: What would the real one look like in a LOCA?

MR. BURMAN:. It may be all over the place.

MR. MUENCH: I had one slide I didn't show. This is an instant computer printout.

I am just going to draw something. I am not sure it is relevant, but it is conceivable it would be.

MR. SHEWMON: You make more points with the chairman if you could -- go ahead. Draw it, and then you can talk about it. You are blocking it all out now.

MR. MUENCH: I am not trying to bias the proceedings. The way the LOCA starts is you have the initial plant heatup, and this is when you are going to close stagnation inthe core, and then there is a cooldown during the flow reversal, and I probably have these a little bit relatively out of whack here.

As we get to -- almost the flow starts diminishing towards the end to blowdown. This cooldown rate diminishes and it actually starts heating up again; go through refill and then reflood. Okay?

So here is approximately end of blowdown, and it is around this range here typically they are reversed. Now, there are variations.

MR. SHEWMON: For the particular high heatup rates that he was talking about?

MR. MUENCH: Well, this heatup rate we are calculating would be in the neighborhood of five to ten degrees per second. That is the heatup right here.

MR. BURMAN: There are variations. The reason for this particular shape here is that about in here the clad goes into alpha plus beta transition. It doesn't in all of the curves, and that is not necessary to see this slowdown. Then as it comes down of it, as the strain starts, then you are getting to very high temperatures; the radiant heat loss, their unheated shroud causes a larger heat transfer, and they have a fixed constant power, so that the heat uprate drops off.

When we correct some of the data for that -- and I apologize for a couple of errors on this slide -- the x's on here represent -- and I don't know whether you can read your handouts better than the slide. They are very small.

The x's here represent what was reported to be a 28 degree heatup rate. The numbers alongside of them is our calculated average heatup rate over the last three seconds prior to burst, and you can see that -- here is one that I pointed out a little bit ago. It tended to support our line better than theirs but it is actually a 10 degree a second rather than 28 degree per second.

As you look over in here, we see a four degree, a

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33 degree, a 10 degree, a 25 degree all grouped together in terms of burst temperature.

Up in here there is a minus 8 here. There's also a one which overlays this that we didn't put on the curve. There is another minus eight. There is a 17, and I am not sure if that is a minus or not. There is a two degree; a zero degree here.

MR. SHERMON: Let me ask a different point. If I take longer to heat a tube up to its first temperature, will I get a higher burst -- if I heat something up along slowly until it burst, and I heat another one up faster, which one will rupture at the lower temperature?

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MR. BURMAN: The one that will rupture at the lower temperature is the one that has a maximum strain localization due to temperature differences around the tubing. And that's a function of the heat flux across the gap. And it's not necessarily a function of the heat-up rate.

MR. SHEWMON: So you're telling me that creep exists in metals but it's irrelevant at this point?

MR. BURMAN: In these, these kind of things, there is some small amount of creep, but argon and -- in the work that they've done have shown that essentially you can -- well, they've shown essentially that you can correlate the circumferential temperature differences to, in their terms, heat-up rate, because they were using a constant cooling, and therefore the higher the heat-up rate, the higher the heat flux.

And Hageman at Idaho Falls has shown that the burst always occurs at the same true stress level for the same temperature.

Okay. Now, the more that you localize the strain and you tend to get the local melting effects, you'll get burst at a lower temperature with higher temperature differences. Which means that anything that gives you a high temperature difference will give you a somewhat lower burst strain.

MR. SHEWMON: And low heating rates give higher temperature differences?

MR. BURMAN: Low heating rates give lower temperature differences for the same steam flows.

MR. SHEWMON: But I thought the lower -- the lower heating rates gave lower burst temperatures, didn't they?

MR. BURMAN: There's no difference in this data --

MR. SHEWMON: I know there's not. But if I look at the --

MR. BURMAN: -- other than -- yeah --

MR. SHEWMON: You have certain -- sometimes you quote from the Oak Ridge data.

MR. BURMAN: Yeah.

MR. SHEWMON: In fact, if we look at that graph that you had on earlier, it showed some of that data, though you chose not to talk about it at that time.

MR. BURMAN: We found -- we found that slower heat-up rates did give lower burst temperatures to some -- but to a lesser degree than we have here.

MR. SHEWMON: But it should give a higher burst temperature, by the reasoning you were giving, wasn't it, that the slow heat-up rate would give more uniform, less temperature difference, and thus --

MR. BURMAN: What we're plotting against is not true stress. But we're plotting against essentially engineering stress. The difference between a true stress and engineering stress gets larger as it -- as the temperature difference gets larger. And therefore for the same true stress --

MR. SHEWMON: How is "truth" defined in this case? What's "true stress"?

MR. BURMAN: That's the local -- the local stress at the rupture versus the average original -- stress based on the original diameter and thickness.

MR. SHEWMON: And so this is -- is the average -- the stress locally while it's still uniform or after we've gotten instability which has started to thin?

MR. BURMAN: The true stress at the point of instability is the one that's important, I believe.

MR. SHEWMON: Where the instability starts?

MR. BURMAN: Yeah.

MR. SHEWMON: Okay. Now let me come back. If I go to your second or third Vu-graph, I find that their burst temperature is lower if I have a low heating rate.

MR. BURMAN: Yeah. When -- when plotted on a engineering stress basis, that's correct. And that --

MR. SHEWMON: Yes.

MR. BURMAN: -- that's because of the greater difference between engineering stress and true stress.

MR. SHEWMON: So it's complex. Well, go ahead, then.

MR. BURMAN: Okay. It's a very complex argument.

MR. SHEWMON: I'm not sure it's correct. But it's complex.

MR. BURMAN: The other point to make here -- first, the error that I mentioned in the graph is that these two solid circles here should be X's. These were also 28 degree per second so-called tests.

The round circles here are -- the dark, round dots are other ramp rate tests, other than 28. Some -- some were listed as 5 degrees, some as 10 degrees, and some as isothermal, 0 degrees.

And the -- as I understand, the way that the C630 model was developed was to use the 28 degree per second curve and the isothermal data in here and, essentially, linearly interpolate between the two. But isothermal data can be put anywhere across here on the curve, depending on how long you want to wait for it to burst. So it doesn't form a valid point down here.

Our curve happens to agree quite well, our 1 degree C per second curve happens to agree quite well with theirs. But we consider that an invalid extrapolation, because we were using a logarithmic extrapolation between 5 degrees and 25 degrees.

MR. SHEWMON: Now, why did you get any difference between 25 degrees and 1 degree? You were extrapolating on what plot?

MR. BURMAN: In our single-rod burst test data, not -- nothing to do with this data, we have data at 25 degree C per second and at 5 degree C per second. We also had faster data. But our slowest rate for 5 degree -- 5 degree F, I'm sorry, 5 degree F and 25 degree F. When we fit all of the data to a curve, using least square fitting techniques, to correlate the ramp rate effects, between the maximum data, which was 200 degrees F per second, and the minimum, which was 5 degree F per second, and it fits that data pretty well. When we extrapolate it down to 1 degree --

MR. SHEWMON: You mean "it fits that data well," does it mean that there is an effect of heating rate?

MR. BURMAN: There is an effect of heating rate when it's plotted against the engineering stress or pressure. I think what Hageman shows is that if you plot it against --

MR. SHEWMON: Okay, go ahead. It's Esposito that has to explain what he means.

MR. ESPOSITO: What I mean by that is that the heat-up rate is less significant, the limit is less significant of a variable than what the staff's heat-up rate is. That's what that comment means.

MR. SHEWMON: Okay. It doesn't mean it's

nonexistent. It's less significant.

MR. ESPOSITO: No, it does not mean that.

MR. SHEWMON: Fine. Thank you.

MR. ESPOSITO: It just means it's less significant.

MR. SHEWMON: Pardon me, then.

MR. BURMAN: I've done some comparisons with other data. This is a comparison of our heat-up rate dependent curve and some data from the French EDGAR tests and their correlation of data. And you can see that there's very good agreement between those.

I also have compared our data with the REBEKA data of Erbacher. And this dashed line is our 1 degree C per second curve, which agrees with his very well. And a -- this line with the X's on it here is a 30 degree C per second line, which compares very well with his 30 degree C per second line.

So these three data sets, ours, the French EDGAR program, and the German REBEKA program, all agree very well on the heat-up rate within themselves.

Our conclusion, then, on the ramp rate effect on burst temperature is that when ramp rate effects are correctly accounted for, the Westinghouse small break burst temperature model is in reasonable agreement with the Oak Ridge data and the French EDGAR data and the German REBEKA data.

And because of that, we don't see any reason for imposing a different model. We do not currently use the small break model in the large break LOCA, but we have said that we would do that.

This is the curve I mentioned a little bit ago, which shows the Westinghouse single-rod burst test data and showing the large scatter in data at essentially the same conditions. And it's simply a function of the randomness of the temperature distribution. In our case, because we had pellets inside of the rod which were unheated and were acting as heat sinks, and they never stay in the center, just as fuel pellets never stay in the center, and we had uniform external heat flux in. You get the same sort of thing, we would expect the same sort of thing with fuel pellets on the inside and a uniform heat sink on the outside.

I want to show here a comparison. The curve I just showed was the Westinghouse single-rod burst test data. This is an upper envelope of that data and a lower envelope of that data. And in here I've plotted the Oak Ridge multi-rod burst test data, the individual burst strains, individual rod burst strains, from the ORNL test. I also have shown our Westinghouse LOCA model for burst strain. And you can see that it captures almost all of the Oak Ridge multi-rod burst test individual rod burst strains. This is meant to be a best-estimate model and was approved by the staff previously as being in compliance with Appendix K, even though it did not -- it was not an upper-bound model. The staff is now saying that that was not valid, that one must use an upper-bound model. This is a difference in opinion between previous NRC personnel and current NRC personnel. And it makes our job very difficult if next month they have a new person who comes in who has yet another interpretation.

But you can see that we essentially, we consider the -- their multi-rod burst test data to be nearly prototypical. And you can see that we envelope most of their points; there's a couple of outliers.

Getting into the effect of temperature distribution, this is ANL's plot where they have plotted the maximum circumferential strain against what they call a "strain localization parameter," which means the -- a sort of max' to average strain around the rod. And you can see that there's a direct correlation, a very steep correlation, between these two factors.

They also show a circumferential strain localization -- or, I'm sorry, the same radial strain localization parameter that was plotted on the last curve is shown here against the circumferential temperature difference at burst, which shows that this is not some mysterious property of zircaloy that makes strain localize on one side; it's the temperature difference around.

MR. SHEWMON: Tell me how you're determining the radial strain localization, how do you define it?

MR. BURMAN: I don't have the exact definition. But, essentially, it's the integral of the thinning of the cladding with reference to the -- either the minimum or the maximum clad thickness.

Do you recall, Bob? Have you gone over that?

MR. PICKLESIMER: No, I don't. (WORDS UNINTELLIGIBLE).

MR. BURMAN: It's written out in their summary report.

MR. SHEWMON: If you had a tube which ballooned completely uniformly, would it be one or zero?

MR. BURMAN: It would be zero.

I'm -- I'm sorry, I'm sorry, I'm sorry, no. No, it would be infinite, I think.

MR. PICKLESIMER: No. No, it doesn't matter. This radial strain localization is a circumferential strain localization.

MR. BURMAN: Yeah, it's --

MR. PICKLESIMER: It's circumferential.

MR. SHEWMON: So at a given elevation it's the uniformity around the thing?

MR. BURMAN: Around --

MR. PICKLESIMER: Related to the uniformity around it, yes.

MR. BURMAN: Yeah, okay.

MR. SHEWMON: What am I supposed to remember out of the slide before this?

MR. BURMAN: The purpose of showing these is to show that it's the temperature difference around the cladding that results in higher or lower strains, not whether the cladding was heated from the outside in or the inside out or whatever.

MR. SHEWMON: It's a hypothesis I'd like to believe. I just don't see how it follows in what you're telling me.

MR. MATHIS: I don't see how you get here from there.

MR. BURMAN: What they did -- and let me go down to a later slide, this is not Argonne's -- and by the way, there is a discussion of that in NUREG/CR 0344 or ANL 77-31, whichever you prefer, which is a summary of that, and they show the mathematical formulation why they're doing it. What they're getting at is that -- these, by the way, are German in-pile tests in FR2 -- and, for instance, in this tube here you can see that there's very little thinning, very little strain on this side, but it's very thin and a lot of strain over on this side. Maybe a better example would be this one, where it's a little less localized and the curve's over a longer distance. And this -- what -- because the -- the thickness reduction here is proportional to the circumferential strain, he's saying that this side of the tubing strained a lot more than this side, and the reason for that is the temperature difference across it; and that's what they're showing in these two slides that I showed back here.

MR. PICKLESIMER: If I can make a comment here, in looking at the report, this radial strain localization factor is a complex function of the ratio of the wall thickness at the rupture versus the maximum wall thickness in that plane effect. It's a complex function of that; it is related to it.

MR. SHEWMON: Okay. Thank you.

MR. BURMAN: This is the data that I mentioned of Hageman's a little while ago, where he took a whole bunch of tests, including Chapman's tests at ORNL, (NAME UNINTELLIGIBLE) at Argonne, Hobson's at ORNL, the German data, et cetera, here, and showed that the burst temperature can be plotted against true hoop stress. And true hoop stress here is the stress considering the clad thickness at the point of the rupture at the time of rupture, rather than original dimensions.

MR. SHEWMON: Now, you said that true hoop stress was just before the instability sets in that leads to rupture?

MR. BURMAN: I think that's the definition used, isn't it?

MR. PICKLESIMER: No, it's -- that is the stress at rupture.

MR. BURMAN: At rupture.

MR. PICKLESIMER: That's the stress at the fracture itself.

MR. BURMAN: Okay. I'm sorry, I --

MR. SHEWMON: So after the instability has developed and it finally pops?

MR. PICKLESIMER: Yes.

MR. BURMAN: So that it's effectively related to the wall thickness at that time and the gas pressure and the heat at that time.

MR. SHEWMON: Then that's a true rupture stress. But I don't see how the Sam Hill you use that for design or calculation.

MR. BURMAN: You can't. But the thing that this shows is that if you can determine a relationship between local to average stress from this strain localization parameter as a function of the circumferential temperature differences, as they have here, then you can show that the difference in heat-up rate is a function, a first-order function, of that temperature difference and a second-order function of anything else.

MR. PICKLESIMER: May I make a point here? Picklesimer.

The purpose of this is to develop a model for use in FRAP-T for calculating the burst strains. It's not for designing. This is strictly for FRAP-T.

MR. SHEWMON: Okay. I understand.

MR. BURMAN: But I think it does explain why we see burst temperature or ramp rate or heat-up rate effects and that they're related to the temperature difference and not to time at temperature or other parameters.

Some evidence that these temperature non-uniformities occur in-pile as well as in out-of-pile tests are these tests from the FR2 -- and I guess I put it up upside down -- which show this difference in thickness which is related to the difference in temperature and strain around the cladding; and these are from in-pile tests with nuclear fuel. And they've -- these were previously unirradiated -- they've also done similar tests on irradiated fuel, and I haven't seen the cross-sectional plots on those, but we talked to Mr. Clark (?) on the phone the other day and he said that he could see no significant difference in the previously irradiated and unirradiated fuel.

Another piece of evidence for non-uniformities, temperature uniformities in pile is some pictures of some Westinghouse fuel rods which have undergone two cycles of irradiation. And what you're seeing here is dark crud patches on the rods. And these rods were running at a temperature and heat flux and coolant chemistry combination which puts us on the boundary between deposition and dissolution of crud on the surfaces of the rods, so that wherever the temperature and heat flux were slightly higher you find a crud spot. And you can see that these are pellet interval lengths here. There's no doubt it's the pellets that's doing it. And you can see that even after two cycles there's a spiral pattern with sudden offsets. There -- I don't know whether there's another figure in your kits which shows a smaller scale or not. But we have other, smaller-scale pictures which show longer lengths and show that these spirals are predominant in here.

This shows the temperature non-uniformity and, essentially, a pellet eccentricity effect which is remaining in this fuel after two cycles, so it's surely there early in life when LOCA is the worst.

MR. LAWROSKI: Where's the spiraling you're referring to now?

MR. BURMAN: Well, for instance, here you see the pellet is against the cladding here. Up here it's over in this side. It sort of spirals around. This is sort of an interrupted spiral here.

In the other pictures that we have, which are smaller-scale and don't show up on here very well but cover a longer length, you can see the spirals more pronounced.

I've run tests where I used transparent tubing and I've tried to stack pellets up in it, and you find that you cannot center a pellet in the tubing; the stack is not dimensionally stable.

MR. SHEWMON: Now, what are your predictions about how the NRU experiments are going to turn out relative to the staff's predictions, then?

MR. BURMAN: Well, I'm not sure what the NRU test matrix is yet. And it is going to be a function of the power level that they use and the steam flow on the outside, the heat transfer; there's a whole lot of things. And I would expect them to be much lower than the staff's upper bound.

MR. ESPOSITO: Dr. Shewmon, just a point about that. We are going to be involved in reviewing of that NRU information and we will provide our comments. (WORDS UNINTELLIGIBLE) test facility and all of the conditions that we're talking about. I believe next, I believe this Monday two of these gentlemen will be involved with that (WORDS UNINTELLIGIBLE).

MR. SHEWMON: It might increase your credibility if you could tell them what's going to happen ahead of time and it happens.

(Laughter)

MR. ESPOSITO: I agree.

MR. BURMAN: In conclusion, then, we believe that our models are in good agreement with ORNL and other data where prototypical temperature differences were used. As a matter of fact, over the last several years, I think Dick will bear me out, I've attended almost every meeting that they've had where they've presented their data as it was being generated. I sat and looked at the presentations of this data and congratulated myself and Westinghouse as to how well our models were fitting the data. And so it was a complete shock to me when someone comes up and says, "Well, this new data is showing a much worse situation."

MR. STRASSER: Your data agrees well with Oak Ridge. And 0630 is based on Oak Ridge data. What parameters in your model, do you feel, caused the difference in agreement between you and NUREG 630?

MR. BURMAN: Oh six three oh is based on other stuff in addition to the Oak Ridge data. It's also based on some fuel rods that Oak Ride ran at very low power levels to get low heat-up rates, at power levels that are non-prototypically low, so that the heat flux across the gap is non-prototypically low and there is very little circumferential temperature differences. Those tests, as one would expect, gave very large strains. But we don't believe that that's possible in the reactor.

MR. SHEWMON: Is this circumferential temperature difference model that you discussed, has that been accepted by the NRC for licensing purposes?

MR. BURMAN: Our licensing model doesn't have a circumferential temperature difference in it. We use a one-directional model. But our data is based on data which had circumferential temperature differences in it.

Just quickly, the difference in between our position and theirs, as I see them right now, is that NRC now claims that upper strain limits should be used for both burst strain calculation and blockage determination. Previously they had agreed with us, and our reports all included the fact that we were using average or best-estimate strain.

MR. SHEWMON: Now, is that average fracture strain or average --

MR. BURMAN: That's average burst strain.

MR. SHEWMON: I'd be interested in your comments on the average strain over the length of the subassembly, as Pic' suggested.

MR. BURMAN: I think Pic' is on the right track.

I think -- and if I can get the data, I'll help him correlate it.

(Laughter)

We didn't have that data. We used something else. And I don't -- actually, Pic's model which he showed up here is a preliminary model and gives a lower blockage than does our model by a small amount.

Best-estimate strains, I believe, are clearly applicable in blockage calculations, because you're getting the averaging effect of a whole lot of rods. It's not fair to use only maximum strains.

In justifying the use of best-estimate strain for burst strain calculations, we justified that, back in the interim criteria days and again in the early Appendix K modeling, based on the very low probability that you would see a maximum strain at the hot spot. In other words, you're getting a large variation in strain; the probability of getting one of those maximum strains at the hot spot is a very low probability. And I don't believe that Appendix K requires us to meet that. It requires us to use a conservative estimate, but I don't believe it requires us to use the world's worst data point.

It's also worthwhile to note that the hottest rod would have the highest heat flux. And because the heat flux out of the rod determines the circumferential temperature difference, it would give a lower clad burst strain, regardless of other statistical variations.

MR. SHEWMON: Yeah. What's that got to do with whether the staff's position is conservative or non-conservative?

MR. BURMAN: Well, we believe the staff's position is conservative, clearly. But we believe it's very much too conservative.

MR. SHEWMON: Yeah, I got that picture. I just didn't see the part about what the highest energy rod having the largest temperature variation had to do with whether or not their position was conservative.

MR. BURMAN: Well, the highest power rod having the highest temperature variation will result in the lowest strain, because strain, as shown by Argonne, is directly correlatable to the temperature distribution. So that if you have a high heat flux and a high circumferential temperature difference, you will get a low strain.

MR. SHEWMON: Okay. Thank you.

Yes?

MR. POWERS: May I make a comment. Powers, NRC.

In the Westinghouse presentation on February 14th, (WORDS UNINTELLIGIBLE) also stated that Franz Erbacher had said that he wished they did not include his high strain data because it was taken at power levels that were too low. If you look in the March 1980 submittal made to the PDR, we included a Telex that was sent to Franz and he returned a response on those heat ratings, which were, if I recall, 1.4 to 1.7 kilowatts per foot; and he's not telling us that his data should be excluded from the data points.

MR. SHEWMON: Well, maybe he's changed his mind. Let's go on.

MR. ESPOSITO: We'd like to now discuss the flow -- the heat transfer and the flow blockage effects, or the flow blockage effect and how it affects the heat transfer, from some experimental data that was available. And this --

MR. SHEWMON: Okay.

MR. ESPOSITO: -- is to give a view of what this all means and, hopefully, some kind of real space, data space.

MR. HOCHREITER: I am Larry Hochreiter of Westinghouse. As Vinny just said, I want to touch on the thermal hydraulic aspects of flow blockage, discuss some of the heat transfer mechanisms during reflood with flow blockage, and review with you some of the flow blockage heat transfer data that we have been able to obtain both in FLECHT-C set and in other locations, and hopefully give you some conclusions.

When we look at the heat transfer mechanisms during reflood with flow blockage, first of all we have got FLECHT tests in the reflood heat transfer tests, and we have run tests down to flooding rates of .4 of an inch a second. Now when we run tests down to these very low flooding rates, we still observe the flow as two-phased. What we have is a flow regime with superheated steam entraining water droplets. And those water droplets constitute a significant heat sink to both the steam, and the heat sink eventually to the rod heat transfer.

We also find that the radiation heat transfer in these dispersed flow situations can account for up to 40 percent of the heat transfer, total heat transfer.

MR. SHEWMON: Now, the first point on there the staff feels they have to ignore because Appendix K came chipped in a marble tablet.

MR. HOCHREITER: That is correct.

and this is really what you are calculate in Appendix K. As you increase the blockage you increase the flow by-pass.

But with the flow being two-phased, you can have droplets which can be atomized by the blockage, you can -- the flow acceleration through the narrow channels caused by the blockage can also shear the drops, giving the small droplet spectrum, which will increase the droplet to steam heat transfer and increase the driving temperature between the wall and the vapor, because it will desuperheat the steam, because it will allow the drops to mix more effectively with the superheated steam. And also with smaller drops you will improve radiation heat transfer of the drops.

Flow blockage will also induce additional mixing. You generate new boundary layers. It is like an entrance problem in a pipe, and you get better heat transfer this way.

Right now, at least as how Appendix K is interpreted, these mechanisms are not allowed, and we are stuck with looking at a flow by-pass effect which decreases heat transfer.

MR. SHEWMON: Why isn't the last one allowed?

MR. HOCHREITER: Well, we tried this and even for single-phase flow it was not allowed. We tried arguments on boundary layer effects and initiating new boundary layers and entrance effect type things, and it simply wasn't allowed.

MR. SHEWMON: I am tempted to say I am a simple metallurgist, and I don't understand this, but go ahead.

MR. LAUBEN: Well, I think it goes back to one sentence of guidance, where the implication is clearly that blockage is supposed to be a deleterious effect.

MR. SHEWMON: Oh, come on. You can take it for dry steam if you want to, but why do you say we have to look at the worst possible things we can think of?

MR. LAUBEN: I don't think we are. I think that is where we stand.

MR. SHEWMON: Well, that is the way I interpret your last statement, to say that anything that is positive we have to even more because we think it is supposed to be a penalty.

Now I don't see why, assuming it is steam requires you to ignore physical reality beyond that.

MR. LAUBEN: I think that what we have interpreted it as is that it is supposed to be what we interpret the admonition -- penalty relative to the unblocked FLECHT. I think that is what it says -- --

MR. SHEWMON: Well, that is between you and your god, but then it seems to me one can be punitive -- conservative, or one can just be conservative or one can be literal, and I would put you someplace on the first part of that instead of the other, I guess.

MR. LAUBEN: I mistakenly used the first person. I don't think that -- --

MR. SHEWMON: You is plural.

MR. HOCHREITER: Well, in reality all these heat transfer mechanisms can occur, flow blockage. And whether you have heat transfer penalty or benefit is going to depend on whether you have more of a penalty to the flow by-passing effects or more of a benefit due to atomization of the entrained water. In FLECHT the worst thing that can happen is that you entrain water out of the bundle, because if you do that then you don't use it as a heat sink. If you vaporize all the water within the bundle, then you have completely used all the water as a heat sink and you get better heat transfers.

In fact, our first model -- I think Norm would remember -- would be that we had to apply steam cooling below one inch a second, so at one inch a second we vaporized all the water that was coming into the core, and we got marvelous heat transfer and blockage was a benefit. That was judged to be not the direction the commissioners wanted us to go in. And so we wound up negotiating the blockage models, which came out to be a penalty.

But in reality --

MR. SHEWMON: Don't tell me about your

experiments. That doesn't fit what the commissioners want.

MR. HOCHREITER: Something like that.

In reality all these heat transfer effects can be significant and if the flow is certainly two-phased the droplets can be a significant contribution to the total heat transfer.

Currently in Appendix K we think like that.

Now looking at different data, Norm mentioned FLECHT, original FLECHT data. This data was generated on plates with plate blockage, and when we did run tests down to one inch a second in fact we blocked 16 rods 100 percent, we still saw a heat transfer improvement. However, that data was not allowed.

Looking at other data, I dug up some KWU-BWR parallel bundle tests, and these were forced flow tests. Plate blockage was used to a coplanar as to plate, one bundle. These were 7x7 bundles. One bundle was blocked either 37 percent or 70 percent, and in the 70 percent case the local subchannel blockage was 80 percent.

And the other bundle was unblocked. Now what this facility looked like, briefly, is something like this, where you had two BWR bundles coupled to a common plenum. Now these bundles are canned so you can have no crossflow between the bundles, and you would block one bundle. It would then force water into the bundle and because there would be a pressure drop between these two the flow could split differently and not be even. And then you examine the heat transfer in the blocked bundle versus the heat transfer in the unblocked bundle.

When we looked at that, the Germans observed that the temperature rises were always lower, thereby indicating a higher heat transfer coefficient for the blocked bundle compared to the unblocked bundle.

So we proceeded to indicate --

MR. SHEWMON: Leave that there for a minute till I am -- lower delta T means that this is across your whole subassembly?

MR. HOCHREITER: A lower delta T for a given rod at a given elevation. There are plots in the --

MR. SHEWMON: Okay, and this is where the same amount of material going -- same amount of water or the same amount of pressure drop?

MR. HOCHREITER: This is for the same pressure drop.

MR. SHEWMON: But different amounts of coolant? Actually it will --

MR. HOCHREITER: It will be different amounts of water going to each bundle.

MR. SHEWMON: Yes.

MR. HOCHREITER: Because there would be in this case a feedback effect of the blockage on the flow into the blocked bundle.

I have included in your handout some copies of the temperature rise data.

Another experiment that we have examined that Pic referred to briefly was the KFK FEBA tests. These are the German tests that are being run over in -- -- and they have been on about a three or four year program that will last for about another two years examining flow blockage. And we have been very close in communication with them and working with them, in many cases providing overlapping tests and exchanging data with them.

They have run tests for the force flow. They have looked at plate blockage, and they have looked at coplanar sleeve blockage. They ran some preliminary experiments with a 1x5 bundle. This would be five rods in a row, about 12 feet long. And they looked at the effect of blocking the same amount of flow area with a plate or with sleeves, smooth sleeves to simulate the ballooning.

What they found was that the sleeves would give lower heat transfer improvement compared to the plate and this is because the sleeves would atomize less of the water that was entrained.

MR. SHEWMON: How did the plates end up?

MR. HOCHREITER: The plate would give you an even higher heat transfer.

MR. SHEWMON: How did the plates get put in the fuel bundle?

MR. HOCHREITER: This was just a 1x5, and they would just insert a plate to cover the rods, a thin plate to cover the rods, almost like an orifice plate.

MR. SHEWMON: Okay.

MR. HOCHREITER: Okay, they ran tests down as low as .8 of an inch a second and still showed a heat transfer improvement for a blocked configuration relative to an unblocked configuration.

Recently they have been running tests, and these are 5x5 tests, 25 rods, with a 3x3 corner section of the rod bundle blocked 90 percent. And that looks something like this. What they have is they have a test section here, 25 rods, and they blocked these 9 rods 90 percent, and then they look at the heat transfer in this blocked region and they compare it to a test where they have no blockage at all.

What I have got plotted on this plot is a ratio of the heat transfer coefficient from the blocked test to the heat transfer coefficient from an unblocked test with the same test conditions as a function in time. And this is for a 10 millimeters downstream. You can see that there is a heat transfer improvement up to the turnaround time, and there is a penalty. And for 300 millimeters downstream there is a slight heat transfer improvement and then a penalty.

Well, what you are really concerned about from a peak clad temperature point of view is what is happening up to and through turnaround. And what they see is a heat transfer improvement.

They also observed when they analyzed their data that they do get steam desuperheating, which means that the droplets which were entrained in the flow are contributing to the heat transfer. They are atomizing, they are reducing the vapor temperature, giving the greater vapor temperature to the rod temperature, driving temperature difference to improve the heat transfer.

And they get lower peak clad temperatures for the blocked cases. This is at 90 percent coplanar blockage.

MR. SHEWMON: If we talk about LOFT or other irrelevant things there --

MR. JOHNSTON: Could I ask a strictly technical question?

MR. SHEWMON: Sure.

MR. JOHNSTON: Larry, I have heard some criticism of these -- -- tests, as essentially there is a rather heavy thermal mass. In other words, this section here is a rather heavy thermal mass by which all of these rods are thermally tied to the corners, and what you have really done is sort of raised the durable heat transfer region in there in that blocked section and so the heat is being pulled out to the edges and therefore you are not getting the heat.

Now I --

MR. HOCHREITER: Well, the Germans went through a whole bunch of calculations to show how their sleeve design would minimize that thermal mass effect.

MR. JOHNSTON: But also the heat flow. Not only the thermal mass that had to be heated up, but it is the heat flow to the corners -- --

MR. HOCHREITER: To the edge of the shroud?

MR. JOHNSTON: Yes.

MR. HOCHREITER: Well, the shroud is almost at the same temperature as the rods when they begin the tests. Okay?

MR. JOHNSTON: And no water or anything around the outside?

MR. HOCHREITER: No, no. And the point that we are looking for, really the data that we are most interested in is really not the data that is going to be like located right or the sleeves, because the sleeves aren't going to be that far atypical anyways.

They were interested in the heat transfer that is downstream of the sleeves, because when we look at an Appendix K calculation that is where we calculate a penalty, if it is downstream of the blockage, for most of our crossflow planes.

MR. JOHNSTON: Yes, but your point is when you say the Germans have checked it --

MR. HOCHREITER: Yes.

MR. JOHNSTON: -- that there is no heat flow, no heat sink, that those blocked sections are effectively connected to which we lose -- --

MR. HOCHREITER: No, not as far as I know, because the test section boundary, the housing is almost at the same temperature of the rods when they start the tests, and they have gone through a whole bunch of calculations that we have looked at that helped them design the proper sleeve because they did have different sized sleeves they looked at.

Now the heat transfer itself relative to reactor would be least prototypical on the sleeve. But where we are looking for the heat transfer is downstream of the sleeves. We are looking for the flow effect of the blockage.

MR. JOHNSTON: Yes, where you are measuring heat transfer that is one thing, but if you are using a temperature as lower --

MR. HOCHREITER: No, the temperatures I am referring to are downstream of the sleeves.

MR. SHEWMON: What happens in the NRC blessed calculations with regard to if there is blockage in the core midplane, is there any crosstalk allowed or how --

MR. HOCHREITER: Sure.

MR. LAUBEN: That was one of the things I said in the slide but I didn't get to because of the time constraint, and that is virtually every flow diversion model that has been offered of a single-phase nature, that any reactor vendor has offered since I think the first task of working on them, has been reviewed acceptably. (inaudible) when clearly an advantage is gained by steam -- -- but all the other flow diversion models --

MR. SHEWMON: Where an advantage was gained by what?

MR. LAUBEN: Steam cooling.

MR. SHEWMON: Steam cooling.

MR. LAUBEN: Compared to the FLECHT data.

MR. SHEWMON: Okay, does that steam cooling mean two-phased?

MR. LAUBEN: Single phase.

MR. HOCHREITER: In other words, what I am saying is this: when we looked at the original Westinghouse model, it showed an advantage, as Larry has explained, compared to the FLECHT experiment. The only -- in other words, FLECHT gives you bad heat transfers because you lose the entrained water of the test sections. But if you vaporize all that water at the quench rate and turn it into saturated steam you get very good heat transfer.

MR. LAUBEN: Very good heat transfer. So as you say, it doesn't make much sense to violate physical reality.

MR. SHEWMON: Unless it is Appendix K.

(Laughter.)

MR. LAUBEN: If the physical reality is such that it is going to vaporize more steam and (inaudible). So there were other models proposed by everybody. Everybody's idea was to key the steam cooling heat transfer in some way to reflect data on blocked, and we have accepted most of those models.

In addition, everyone has at various times proposed modifications to their flow diversion models. Westinghouse proposed one in 1975. Combustion then proposed one in 1978, and Exxon proposed a revised one in about 1976, compared to what they did in 1975.

They have received favorable review by the staff. We have not been son of a bitches just trying to penalize everybody. We have come up with what we thought was a reasonable flow diversion model subsequent to the originals (inaudible)

MR. SHEWMON: Flow diversion is your words for if there is a block here the flow can come around and pull what is downstream?

MR. LAUBEN: That is right.

MR. SHEWMON: But you still bump into a penalty downstream, is what you were saying. Okay, fine, go ahead.

MR. HOCHREITER: You might think of Appendix K steam cooling as setting back technology, because what we were forced to do was to take a situation which we know is two-phased and hammer it into a situation which was single phase.

MR. LAUBEN: As an aside to what the staff is allowed to do, General Electric in 1978, when we were discussing Appendix K modifications, made the point that they felt that no change was needed to Appendix K. Westinghouse (inaudible) current opinion is that I think you will (inaudible) discuss what your opinion is about, and we will finish revising Appendix K today.

But they said no, a change is not required to Appendix K. What is required is the staff should give the fuel vendors more latitude. They should be more forgiving in their interpretation of Appendix K, and Mr. Gossick wrote back to Dr. Sherwood that we couldn't do that, that we were constrained to live by the rules that are here, and we couldn't do that.

MR. SHEWMON: Thank you.

MR. LAUBEN: I mean, I would like to do it too, but I can't.

MR. HOCHREITER: One of the key programs that we think is going to help this situation as Norm has been describing it is the FLECHT-SEASET program. And we have purposely tried to structure part of this program to address the Appendix K steam cooling rule. It is the only reason we are running these tests, and to do this and do this in a joint fashion we have involved Dr Picklesimer, we have had Dale Powers, we have had Norman Lauben. We have tried to get as many people involved in this particular -- the design of this particular program as we can, such that we can provide the data base and people can then go to a rulemaking hearing or whatever is required to be able to assess the thermal hydraulic effect of flow blockage and rod bundles.

In our program we will be looking at different blockage shapes, we will be looking at the alpha burst case and the beta burst case. We will be simulating blockages with thin pieces of steel to simulate the blockage shape itself.

We will be looking at both coplanar and noncoplanar blockage distributions, and we will be testing these in a 21 rod bundle test series and then a 161 rod bundle test series.

In your package I have given you additional information on the program, the blockage distributions that will be tested, a picture of the blockage shape, and the 161 rod bundle with two 21 rod bundle blockage islands.

This is currently what is planned in the program. Right now we are in the middle of the 21 rod bundle blockage test series, and we are designing and building the 161 rod bundle test series.

Now we have been trying to make our program complementary with the work that is being done in Germany and through the NRC and Dr. Tong and Picklesimer, Dr. Picklesimer's help we have set up close communications with the FEBA people and with the REBECCA people over in -- -- and we have been exchanging information quite freely and we have been very profitable in doing that. And we have been able to make the programs complementary.

As Dr. Esposito indicated, we are getting as involved in the NRU tests as much as we can so that we can make that overlap with our program or rather overlap our program with NRU, such that we can provide for the NRC and the ACRS a good technical data base to assess this particular technical problem.

MR. SHEWMON: Pardon me for appearing in person, but this is all interesting, logical, but what has it got to do with 630?

MR. HOCHREITER: We want to show you that we don't think that there is a heat transfer problem with flow blockage. I have shown you two sets of data, and I want to show you another set of data right now.

We calculate in Appendix K --

MR. SHEWMON: Okay, let's say I agree with you and there is no heat transfer loss with flow blockage. So what to the acceptance of this document?

MR. HOCHREITER: If we have to accept this document and we don't change Appendix K, we just calculate a larger penalty.

MR. ESPOSITO: Dr. Shewmon, a comment or two to my conclusion. What we see is this model has bestowed upon us a unilateral impact from penalty point of view without any of the positive points which unfortunately we can't do because of Appendix K, and if the data did not substantiate a benefit in terms of heat clad temperature at the point of turnaround, then we would feel that we couldn't take these two things together and cancel them out, or at least withhold them in a balanced structure.

MR. SHEWMON: Yes.

MR. HOCHREITER: I think Norm has referred to, and I think even Dale Powers has referred to, compensating thermal hydraulic effects would help offset the flow -- the new blockage model, and the compensating thermal hydraulic effects we see is the heat transfer effect that the flow blockage causes in the rod bundle itself, relative to what we calculated with our Appendix K models.

MR. SHEWMON: But to do that you need two-phase flow, is that right?

MR. HOCHREITER: To do that we need two-phase flow; we have got to change the rule.

VOICE: (inaudible)

MR. HOCHREITER: Well, just to confirm that indeed there is a benefit, I must say that this is for a 21 rod bundle test results. Obviously we have got more work to do and we are in the process of doing that. And this confirms the other stuff that the FEBA people have seen.

We will be examining this in larger bundles where we have more by-pass. There is some flow by-pass in this test series. What we have done is we have blocked the inner 9 rods 62 percent. So you do have flow by-pass around it. This is certainly not like a reactor. But you do have both aspects of the problem there. You have the blockage effect, and you do have the by-pass effect.

This is the heat transfer about an inch and a half downstream from the blockage sleeve itself. You can see the peak clad temperature is lower, the heat transfer of earlier time is higher, and this is for a 40 psi, .9 inch a second test, which would be typical of what we would calculate in our licensing calculations.

MR. SHEWMON: .9 inches of reflood rate.

MR. HOCHREITER: Reflood rate. I have looked at data as low as .4 inches, and I see the same trends. And I have looked at data at 20 psi and you see the same trends. So from looking at the data base, the brackets are our licensing calculations, we do see this type of behavior.

I am not going to go through all these slides, but this heat transfer improvement effect occurs more locally downstream --

MR. SHEWMON: Put that one back again and hold my hand for a minute.

MR. HOCHREITER: Yes, sir.

(Laughter.)

MR. SHEWMON: The top is temperature?

MR. HOCHREITER: The top is temperature.

MR. SHEWMON: Of the water-metal interface? Or temperature of what?

MR. HOCHREITER: I went too fast. This is temperature of the rod, the inner rod.

MR. SHEWMON: What temperature? Is the water temperatures in that inner rod.

MR. HOCHREITER: This would be the inner clad temperature.

MR. SHEWMON: At some elevation?

MR. HOCHREITER: Yes, at this elevation, 75.25 inches --

MR. SHEWMON: Up from where the water starts coming in?

MR. HOCHREITER: From the bottom -- --

MR. SHEWMON: Okay, and so it is zero time. You start adding water at zero on that scale, is that correct?

MR. HOCHREITER: That is correct.

MR. SHEWMON: Zero inches on the 75-inch scale down there?

MR. HOCHREITER: That is correct. And you reflood, we flood the water in. We are adding it at .9 inches a second.

MR. SHEWMON: Okay, and at 400 seconds you come to 75 inches, is that right?

MR. HOCHREITER: Yes. You could think of it that way. Actually it can quench --

MR. SHEWMON: Well, a two-phase flow. It has to be either water or steam. So --

MR. HOCHREITER: If that is the case, sir, this would probably quench later.

MR. SHEWMON: Okay, but what you are saying is th at the heat transfer coefficient changes from near zero there all the way up as the water interface approaches?

MR. HOCHREITER: That is correct.

MR. SHEWMON: By order, in order of magnitude if you really want to take extremes?

MR. HOCHREITER: And number 2 here is the blocked case. Number 1 is the unblocked case, and what you see is a significant increase in heat transfer early in time, and we believe that this is due to the atomization effects of the water droplets. Later in time, as you get a lot more water up there, it doesn't seem to make any difference.

Then there is a penalty later in time. And it is supposition right now, but later in time the slope of this temperature curve changes, and this implies a different flow regime.

MR. SHEWMON: Well, what are you allowed out of this range of heat transfer coefficients from 25 to 250?

MR. HOCHREITER: This first value right here.

MR. SHEWMON: Is the only one you are allowed?

MR. HOCHREITER: Right.

MR. SHEWMON: So the conservatism runs from zero to a factor of ten.

MR. HOCHREITER: From the minute you put the water into the bundle.

MR. SHEWMON: Okay, thank you.

MR. HOCHREITER: As you go further up the bundle, this heat transfer difference decreases. Let me just show you one other shot, at 90 inches. Here there is almost no difference. Correspondingly, there is almost no difference in the temperature between an unblocked case and a blocked case. This is 90 inches.

Now if you get up to 10 feet y can actually see that there is a penalty for flow blockage because what has happened in the two-phased case you have used up the water down below just behind the blockage region, you have atomized that water and you have gotten improved heat transfer there. And when you get up to the top of the bundle now you have got less water, compared to the case where you had no blockage at all.

And so you do see heat transfer penalty here, but you don't really care because the clad temperatures are 1300 degrees.

So what you have done in reality with flow blockage is you have utilized the entrained water more effectively. You have utilized it more effectively downstream of the blockage region.

I have also included vapor temperature measurements in the package and you can see that downstream of the blockage region you do desuperheat the vapor more, which implies that you have atomized the water, you have got more liquid surface area which you can desuperheat the steam.

MR. SHEWMON: Is desuperheating the same as undercooling? Two negatives make --

MR. HOCHREITER: I don't think so, because I don't know what undercooling means.

MR. SHEWMON: I am not sure what desuperheating is, but go ahead.

MR. HOCHREITER: Okay.

(Laughter.)

So based on the information that we have been able to dig out to date, and of course this is mostly for small bundles now, what we have observed is that in the flow blockage, in the flow blockage heat transfer mechanisms there are competing heat transfer mechanisms. There is a flow by-pass effect, and then there is an effect of the entrained water. And the effect of the entrained water is not allowed by Appendix K, so we only calculate a penalty due to by-pass.

But the data that we have observed to date is that the atomization of droplet breakup of the water, and the mixing of this water would be superheated steam, will offset the flow by-pass. And so you actually have improved heat transfer.

Now later in time you do see a penalty. We think this is because of a flow regime change, but this is well after turnaround. We don't care. We really don't care at that point.

Further up in the bundle for our particular power shape, our cosign power shape, you can get poor heat transfer at the top of the bundle. But again that is outside the zone of interest, and the peak clad temperatures that you calculated up at 10 feet and 11 feet are like a thousand degrees to 1200 degrees.

MR. SHEWMON: At least if we are staying with the ballooning model, the balloon burst will come in the top half of the core, I would trust?

MR. HOCHREITER: If the balloon bursts, it will come in the top half of the core. The same situation would occur because again you would get a heat transfer improvement now at 10 feet, just downstream of the balloon bursting region. And there would be no difference up to 10 feet.

MR. SHEWMON: So when they --

MR. STRASSER: Presumably they are scattered throughout the core?

MR. HOCHREITER: Well, the stuff that we are looking at, that we have seen both from the Oak Ridge data and from the data in Germany, is that it is usually going to be within the grid span. The blockage and ballooning effects will be within the grid span and we will be testing that.

We have gotten distributions from Oak Ridge. We have gotten blockage distributions from the Germans, and we will be placing these sleeves, these stainless steel sleeves on rods, either to simulate an alpha burst or a beta burst -- we will be placing these sleeves on the heater rods at different axial positions and we will look for the heat transfer effect there.

My own personal belief is that as you would string out these balloons you are probably going to see less of a heat transfer improvement, because there is going to really be more flow area for a given channel and you won't get the local acceleration as the flow snakes through this rod from the balloon tubes you probably won't get as much atomization. But then you don't calculate as much flow by-pass out of that region either.

So you probably would get a lower boundary --

MR. SHEWMON: What sorts of fractions of the cross-section do you fill with these tubes, sleeves?

MR. HOCHREITER: I am not too sure I know what you mean.

MR. SHEWMON: Well, do you get in trouble with the NRC at 10 percent flow blockage, 90 percent flow blockage or someplace in between?

MR. HOCHREITER: Anything greater than zero, because it is always going to be a penalty. It will always be a penalty.

MR. SHEWMON: Well, yes, but you must have lived, learned to live with some penalty.

MR. ESPOSITO: Well, we will show you a range of penalties that you get, depending upon some of the --

MR. SHEWMON: Okay. I am sure it can only be a penalty. I had thought that it really got hard and tough someplace up in the upper reaches of the flow blockage though.

MR. HOCHREITER: Yes, if you increase the amount of blockage, like if we would go with Ralph's model, which would give us much higher blockage, we are going to get much more flow diversion and a much greater penalty. And that will be the source of part of the penalty that --

MR. SHEWMON: Okay. Well, let us get on to these things.

MR. LAUBEN: Rick, can I show you, because the rules are based on, as you called them, blessed calculations. There is something on the order of 1 or 2 degrees per percent (inaudible).

MR. SHEWMON: 1 or 2 degrees what to percent?

MR. LAUBEN: Peak cladding temperature.

MR. SHEWMON: You mean we only get a 100 degree temperature rise with 100 percent blocking?

MR. HOCHREITER: I don't think it is less.

MR. SHEWMON: There must be a nonlinearity someplace here, or else you wouldn't be here today.

MR. LAUBEN: But really the NRC blockage, the NUREG 0630 doesn't --

MR. [illegible]O: And the Westinghouse model that we saw a peak of around 47 percent --

MR. SHEWMON: Does it cut off at 80 percent --

MR. HOCHREITER: 72 percent.

MR. SHEWMON: Why do you stop at 72?

MR. POWER: That is what it looks like.

MR. SHEWMON: Let me hold up these illustrious gentlemen for two minutes more. It seems to me I remember reading in this job one time a report from General Electric that said, gee, blocking subassemblies is hardly any problem at all because even if you block over 95 -- or until you get above 95 percent flow blockage there is still adequate cooling. Was that when they are pushing water through the other end or --

MR. JOHNSTON: That was the inlet --

MR. SHEWMON: Probably isn't anything regard to a LOCA, I don't know.

MR. JOHNSTON: It is inlet flow blockage calculations where you maintain about 45 percent of the power being generated in a bundle, and you say anywhere from 5 percent water flowing through they don't calculate -- the difference is they don't calculate temperatures that are going to cause melting, but they do exceed 2200.

MR. MEYER: This is not during a LOCA.

MR. SHEWMON: But it is not during a LOCA, so we don't --

MR. MEYER: That is not during a LOCA.

MR. JOHNSTON: -- flow blockage with water pressure, and a small amount of water going through. But the concept is right; you don't see -- go up and melt because you blocked no more than 95 or 96 percent. But if you exceed 2200 calculation -- required for a LOCA count.

MR. SHEWMON: Okay.

MR. JOHNSTON: So it wouldn't be permitted anyway in essence.

MR. SHEWMON: Okay.

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MR. ESPOSITO: What we'd like to present now (Inaudible), what happens if you forget about all of this stuff we just told you, the Westinghouse data, and just take the NRC 0630 as we had it back in March and that report, and supply that to our evaluation models.

MR. MUENCH: Now that the scientists are done talking about the real technical items, an analyst is going to get up here and tell you, hopefully, a little bit about what the significance of this discussion really is. I think you lose track a little bit when you get into the nitty-gritty of the technical pros and cons of the models, the technical correctness. You lose track of the significance.

Obviously, the use of NUREG 0630 is not going to have a significant direct impact on plant safety. It's not going to change the probability of loss of coolant accidents. It's probably not going to change the reliability of the operations equipment. And it probably will not change the consequences of a loss of coolant accident.

In most cases it will not change the peaking factor or the peak kilowatt per foot, if you will, that the plant will be operating at the great majority of the time. There are a few exceptions and I'll talk about that.

The impact of NUREG 0630 really is on the operations of the plant, the operations limit. And that's because 0630 -- or I should say the tech specs of the plant are really governed by licensing calculations, like Appendix K. So I'm going to run through a summary of an estimated impact on the operations of the plant due to the use of NUREG 0630.

The methodology I'm going to use in providing the summary was agreed upon with the staff in December when we were going through the exercise in responding to a 60-day request the staff had sent to all of our operating plant customers. The evaluation is based on sensitivity studies where we arbitrarily change burst temperature blockage and burst strain and see what the change in peak clad temperature is. And we apply that using hand calculations.

We have not performed any analyses with NUREG 0630. We have not even programmed that model into our computer codes yet. I want to make it clear at the beginning, these are hand calculations.

The results I'm going to show you and the methodology that I discuss were used in response by all of the applicants under the Westinghouse-designed PWRs, in response to the November 9th, 1980 -- 1979; I put the wrong date here -- letter from the staff. And it's updated every time there's an application for a license amendment that requires a LOCA reanalysis. It's keyed to the FSAR large break LOCA analysis, so we have to redo it every time.

MR. SHEWMON: You're saying this letter was sent out in November of last year; is that right?

MR. MUENCH: Yes. I'm sorry. Yes. I'm good, but I'm not that good.

Okay. One of the observations you're going to make when I show you the results -- let me see here -- is that there is significant variation between plants of the impact of NUREG 0630. Hopefully, this chart will help you understand that a little bit.

Remember, there were three models that were discussed in 0630: the burst temperature model, the burst strain model, and the burst blockage model. And depending on the type of plant, depending on the characteristic of the plant heat-up transient during loss of coolant accident, some of these models will not apply to various plants.

For example, we have some plants which we call burst mode limited plants. Obviously, the peak clad temperature occurs at the burst mode. These are plants that had a less than optimum blowdown transient, okay. So they have a high clad temperature during the blowdown.

The clad temperature normally occurs -- the peak clad temperature normally occurs right after bottoming and full recovery, as you're at the burst mode. Turning clad temperature around is a matter of balancing the heat transfer coefficient and the gas transfer coefficient. At the first mode I have swollen to a greater extent, and I can turn the clad temperature around as soon as I get a little bit of steam generation in entrainment.

Now, because of that type of characteristic, only the burst temperature and the burst strain models apply to a burst mode limited plant. We have discussed before, both of these will give you more zirc-water reaction on the burst mode. As soon as I turn it around, before there's any blockage, the blockage model is not impacted.

Another major category of plant is the reflood mode limited plant, and there are two types of reflood mode limited plants. There are plants where the peak clad temperature occurs before the flooding rate falls below the magic one inch per second that we discussed so many times today; and plants where it occurs after that time.

When it occurs before you go below one inch per second, we call that a Flecht plant because it turns around during the time when we can use our Flecht heat transfer correlation, which implicitly includes two-phase flow and everything else.

Plants where the peak clad temperature occurs in steam cooling and we're using the steam cooling correlation -- in both of these cases, the burst temperature impacts due to the difference in swelling models. We discussed earlier that that's a non-mechanistic sensitivity, because we're not changing the swelling model prior to burst in a consistent fashion with the rest of the model in NUREG 0630. But we're talking about the impact of NUREG 0630, and that's what happens.

Now, the blockage model only impacts the steam cooling model. There's a little X right over here. So the blockage model only impacts upon (Inaudible). Hopefully, this will help you understand why there's a variation of the impact.

I should point out that these (Inaudible) were meant to be not necessarily used in some kind of an (Inaudible) mode. They are bounding calculations.

Now, this chart hopefully will represent to you an estimate of the impact of NUREG 0630. We have not used any plant names, and this is a generic discussion. I have included some information. These do represent calculations that have actually been performed, hand calculations that have actually been performed for each of the owners of Westinghouse PWRs and submitted to the staff.

I have provided information about what size plant this is -- four-loop, three-loop, two-loop -- and what type of clad heatup characteristics we have -- is it burst mode or is it reflood mode.

The fourth column represents the change in peak factor, using that methodology, that we would get using the NUREG 0630 models compared to using our large break model as we do today. And I think you can see that there is a wide variation. But basically, it ranges from .02 to .23 in peaking factor, but it's a very significant delta peaking factor.

To help put that into perspective, the fifth column that should be on the page is the total peaking factor that we would estimate, again in a bounding calculation, a bounding hand calculation, that we would get for each of these plants if we were to apply NUREG 0630 without any model changes.

MR. STRASSER: That's not the tech spec.

MR. MUENCH: That is not the tech spec today. The only calculations that the staff has obtained are these numbers. They were balanced off against other things, such as that the peak numbers did not need to be calculated. These are (Inaudible) tech specs.

But these would be the ones that would go into the tech specs if NUREG 0630 were unilaterally applied tomorrow without any beneficial model changes. Okay.

The reason why I've chosen to show you that peaking factor without the beneficial model changes is that we feel that when you're trying to determine the appropriateness of an arbitrary increase in the conservatism of Appendix K, you should look at that change and that change by itself; how significant is that change to the operation of the plant and to plant safety and so forth.

So I've not tried to show any benefits on this chart.

Now, the real significance is seen in the final column, where I've tried to provide an estimate of what those peaking factors would mean to the various plants in terms of their operation. And you'll see that there's a wide variety.

For example, Plant No. 1 there's a decrease in peaking factor of .13, bringing it down to a peaking factor of 2.12. Typically -- and these numbers, this narrative over here is based on experience. Some cases are three-part numbers, but in most cases you'd have to do detailed nuclear calculation to really put a concrete column 5 -- I'm sorry, column 6 -- up there. They're mostly based on experience.

But in general we have found that a plant, after its first cycle, will offer at a maximum peaking factor of 2.15, in that range, with its load following. So if you get down below 2.15 peaking factor, you start impacting the capability of the plant to load follow to the extent that it was designed to load follow.

So Plant No. 1 would be restricted in its load follow operation potentially. Now, what does that mean, to be restricted in load follow? There are a wide variety of things that customers have done, our customers have done, operators have done, operating plants have done, to meet this kind of a challenge.

Number one, we've sold a thing called automatic power distribution monitoring system to plants, where there's a bell that rings whenever you get above that peak factor in your plant. When that occurs, you reduce power. Some people do that manually. Some people administratively reduce the flexibility of operating the plant, load following factors. Okay.

Plant No. 2, you see the peaking factor of 1.89; Plant No. 3, you see 1.79. Those plants, even in the most restricted load follow case, which is baseload operation, would be impacted by reducing power.

MR. SHEWMON: Is No. 2 at reduced power now?

MR. MUENCH: No. 2 -- I guess I kind of hesitate here because this is a generic discussion.

MR. SHEWMON: (Inaudible). Well, I'm just wondering. That's an extremely small increment. It sure as hell isn't load following.

MR. MUENCH: That plant -- the last time I checked that plant it was. It did have some reduction in power. So I guess it would be more proper to say further reduction in power.

One other item. You see -- well, two other operations. One, you see baseload operation, reduce power early in cycle. Of course, as you burn up you can reduce the peaking factor being measured in the plant. You only have to reduce power early in the cycle. That takes another layer in tech spec changes and changes in the way you operate the plant to do this.

I think we see several here that are baseload operation, reducing power early in cycle, reducing power period. The point is that it is a significant impact. This change in itself is significant relative to the operating margins in the plant.

MR. SHEWMON: Now, you have quite fairly pointed out that this is a hand calculation without any compensating things. What's your gut feeling for how much of that Delta F_Q would remain after you did the first cut of other changes you've asked the NRC for.

MR. MUENCH: I'll address that on the next slide.

MR. SHEWMON: Okay.

MR. MUENCH: I think it's pretty easy to do that.

The next and last slide, I was trying to demonstrate to you the impact of the various components of NUREG 0630 that lead up to that change in peaking factor that I just showed you.

This is a histogram of delta peaking factor due to the NUREG 0630, with the various plants. And in the histogram, if you see a clear part of the histogram, that's just the burst temperature part. If you see a cross-hatched section, that's the burst strain part. The solid section is the burst blockage part.

And let me demonstrate the different types of plants. You notice these first several plants over here on the left-hand side only have burst temperature and burst strain impact. Of course, that's a burst limited plant. This plant here only has a clear section, which means it only has a burst temperature impact. That's a Flecht reflood limited plant.

And last but not least, this column here is a steam cooling plant, and it has both burst temperature and blockage impacts.

The main point I wanted to make in this slide is that blockage, which from all indications we have to date really should be beneficial for you, is a significant contributor to the penalty of NUREG 0630. And we therefore, in our own minds, question the appropriateness of adding this arbitrary conservatism when there doesn't seem to be a need for that. It is significant.

You asked a question about what our first cut, the changes in models, improvements in models, would do. I guess I would characterize the first cut as being that which we're already being given credit for. Obviously, none of our plants are taking these kind of peaking factor penalties today. The staff has given us credit in the interim for using some of the software technology we developed in licensing our plants equipped with upper head injection. And that credit that they gave us -- and this is, by the way, four-loop plants, three-loop plants, two-loop plants, actually organized in this way for this question -- they gave us a credit of .2 for the four-loop plants; .15 for the three-loop plants; and .12 -- is that right?

VOICE: .12.

MR. SHEWMON: That's for overhead injection or upper head injection?

MR. MUENCH: That's for software technology, namely --

MR. SHEWMON: Okay.

MR. MUENCH: -- slipped flow, reflux model changes, that we licensed in our UHI plants.

MR. SHEWMON: So that --

MR. MUENCH: I'm sorry?

MR. SHEWMON: (Inaudible).

MR. MUENCH: (Inaudible). So you can see that just that model change takes care of (Inaudible).

MR. SHEWMON: I guess what I was getting at more was that it sounded like how much strain one assumed was there with a higher average strain to have these people talk to those people, or you change it so that you -- one hand was talking to the other, would be a pretty easy one to implement. But that -- well, I'm not sure I'm being coherent.

MR. HOCHREITER: You mean that gives the average strain that Dick was talking abo9ut?

MR. SHEWMON: No. It was the first point that came up in Esposito's comment, that had to do with the fact that things ruptured earlier and therefore you didn't have as much gas conducting change, or -- well, let it go. I don't find it.

MR. MUENCH: I guess the clear part of the block, if I understand your question, is that the burst temperature effect -- this is in fact, where you see these clear parts of the block, is the estimated impact of bursting earlier without having a measured change in the swelling prior to burst mode. Is that where you're headed?

MR. SHEWMON: Higher gap conductance after burst turned out to be due to the fact that you didn't have a realistic total strain at that point in time. You didn't have the strain there that was being used in the other part of the calculation.

MR. MUENCH: That's correct.

MR. SHEWMON: It seems to me it would be pretty straightforward to make the two parts, the two models agree.

MR. MUENCH: I went back to the scientists for that part.

MR. SHEWMON: Okay.

MR. MUENCH: But it would seem to me that it would at least make sense to do that.

MR. SHEWMON: Go ahead.

MR. BURMAN: Excuse me.

MR. SHEWMON: Yes?

MR. BURMAN: The earlier part of the strain, the creep strain or the creep yielding strain, if you will, that occurs as long as the pressure is on the rod. But what's the reverse? I mean, the pressure is relieved and the rod stops straining. That's what is called strain other than burst.

MR. SHEWMON: Yes.

MR. BURMAN: And that -- that will be less if the rod breaks earlier, because that is --

MR. SHEWMON: Not according to 0630, in which written in at least soft clay is the assumption that average strain for the whole rod and maximum burst strain are linearly related.

MR. BURMAN: But not average strain and burst time.

MR. SHEWMON: Well, if they are then --

MR. BURMAN: We have a little difference there between burst -- burst time and burst strain.

MR. SHEWMON: Well, I'm suggesting that if they'll admit it in one half, you might really go ask them again and they'll probably admit it in the other half of the calculation.

MR. MUENCH: I said we would.

MR. SHEWMON: Okay. That was the simple thing I had. This turns out to be not so simple.

MR. ESPOSITO: Let me try to wrap up --

MR. SHEWMON: Okay.

MR. ESPOSITO: -- with the last couple of slides. And since nobody can find them, I can do this a little bit differently, and that is to look at the conclusions that we have arrived at. And I present a few facts here as we see them. They're very global facts, but nevertheless they are the facts as we see them.

The first one is that all the Westinghouse data was supplied to and reviewed by the NRC.

MR. SHEWMON: Okay.

MR. ESPOSITO: In this model that we're talking about and that Dennis talked about earlier, we used convolution or statistical average, not the maximum strain, as the basis of the model. You've heard that before.

The second fact is that Westinghouse developed the burst temperature model with a heat-up rate dependence -- and you heard this before, again -- with a small break model which we submitted and had approved, I believe it was, in 1978. The importance of this comment here is that this work was back in the '72 to '75 time frame. Here we are in '78. We're still looking at things. We have not made things dark.

I claim that there was not a vacuum, but we are continuing and continuing looking at the data.

The next point that we've arrived at is that the Oak Ridge data complements the Westinghouse data. We see no new findings from that data.

The fourth fact as we see it is that the Flecht-Seaset and the FEBA show that flow blockage is a benefit up to peak clad temperature, peak clad temperature turnaround time, and in the region downstream of the blockage. That's the discussion that you heard from Larry. That's present data as we understand it.

Some conclusions that we reached: The first one is that we do not believe that there is a safety problem. With any of the new data or the data that's come out of Oak Ridge and its interpretation, there's no safety problem.

Our second conclusion is that there's no need to change the basis for determining blockage. I did not say burst temperature, and I'll give you my recommendations in a moment; but I said for determining blockage. Okay. We already said that there was a heatup rate dependence on burst temperature which is presently not in our large break codes, and we would recommend putting that in there. Okay.

Now, before I get to the recommendations, I'd like to go through a few concerns from implementing -- they should be on the back -- some concerns from implementing NUREG Guide 0630. And I think the first one and the second one from the philosophical point of view are the most important.

The first one is that NRC performing both modeling and checking function is a dangerous precedent. What has happened is NUREG Guide 0630 is the development of a model. There is obviously some belief that that model is correct. Trying to change that opinion is very difficult. It becomes the NRC model. It does not become the industry model or the vendor's model.

There is a removal, if you wish, of the check and balances by development of models through the NRC staff. And we feel that that is a very dangerous precedent. This checking is important. If there's data that has been obtained that's questionable and people have a concern about it, by golly, tell us and we'll look at it We will not let it fall on the wayside. And I think that's how models should be developed.

And this is a very serious and a very significant concern to Westinghouse. What it does is it puts us in a very difficult position. From the licensing point of view, you can always make a model more conservative. There's absolutely no doubt in my mind, you can always make a model more conservative. And I think that's something that should at least be guarded against.

The next item is that, since there's no new findings that's been obtained from the Oak Ridge data in particular, we feel that the NRC model is the result of different interpretation by new people. The staff had all of our models, all of our data, back in the '72-'75 time frame. We showed strains as high as 80 percent, and in Dennis' handout you'll see that information. Oak Ridge data complements that data base in our opinion; and we feel that there's no -- the difference in where the NRC model is today is a different interpretation.

The next two lines stand for burst and blockage: and one is that we think that the model is being viewed as a very isolated model. All of the feedback that we've talked about, that has been presented in terms of the heat transfer and the thermal hydraulic model, is being disregarded. It's really looked at as an isolated model. It's not in total context.

And secondly -- this was a comment that was made by Dennis a little bit differently -- I think that the burst and blockage model as proposed by the NRC is super-conservative without balance. And namely what I mean by that is that Appendix K is being used unilaterally.

The next item we have here is peaking factor degradation. What is happening here by the loss of peaking factor, if 0630 is applied, by loss of peaking factor margin -- again, this is large break analysis, Appendix K -- we as the vendors will develop large break models to compensate that. We will take our resources, which are always limited, and place emphasis back on large break instead of on some of the items that it should be placed on, like small break, like procedures. And that's what it will force us to do. And I think that's important.

It seems like if we look at the Kemeny Commission report, if we look at the Rogovin report, et cetera, the preoccupation with large break is driving us in perhaps not a very positive direction.

The results of operating -- re-analysis of operating plants: I tried to give you some feel for a more practical point of view, if you wish. If we have to re-analyze all the plants on a backfit basis -- that means, all the plants have to be redone -- our guess -- and it's a pretty good guess -- it costs about $2 million to perform that activity.

MR. SHEWMON: That's for how many plants?

MR. ESPOSITO: That is for 14 plants.

MR. SHEWMON: Okay.

MR. ESPOSITO: That's about $2 million. And these are rough numbers. There's an "approximate" sign there. Okay, it can go higher and it could possibly be lower.

The other part is, if it's only done on a forward fit basis, one of the concerns we have is that we would have different basis for different plants. So if you only do it as a plant comes up and needs to have a re-analysis for some reason, we have a mishmash of some plants with one model and some with the other. And that just causes difficulties, especially if we have to ever look at potential unreviewed safety issues. What model do we use? It gives us some problem. But that's what forward fit would do.

MR. SHEWMON: What would be different for different plants? I mean, let's say every time you came in with a reload it had to be on this basis. Different plants are different, but I don't understand what you mean by different basis.

MR. ESPOSITO: What I mean by that is that I may have half of my plants who are not reloading, and therefore they are not getting a re-analysis. Therefore, they still have the old bases, whereas the ones that may have gotten a reanalysis have a different basis. And it's just those different bases that can give us some problems.

MR. SHEWMON: And esthetically that bothers you?

MR. ESPOSITO: Yes. It's the -- that's part -- it's a little bit more complex. We've seen burst mode limited, reflood limited, you know, types of plants, and it just adds one more dimension. It may be, in retrospect, more positive than total backfit, okay. So if I put that in the degree point of view, I prefer that to total backfit, okay.

Now, let me give you our recommendations, okay. And these are, perhaps, in what we're asking for, relative to what the staff are asking for. In the short-term, I think we, in particular Westinghouse in this case, and the NRC, should reach agreement on the heat-up rate dependence on the burst temperature. You heard Dennis' discussions. You've heard other discussions. We still have a difference of opinion on the heat-up rate dependence, okay. I think that has to be ironed out.

Secondly, it's our recommendation that we maintain the existing strain and blockage model as we presently have in our evaluation model. Okay, the model that was being discussed earlier. If we have to use anything, we would use it on a forward fit basis, so we don't have to run calculations forever.

I recognize that this -- or please recognize that forward fit is the least -- is a less problem than backfit. Okay.

MR. SHEWMON: I would guess so.

MR. ESPOSITO: In terms of the long-term recommendations, we believe that rulemaking should be used to address any potential blockage concerns, along with the Flecht-Seaset data that's being developed to address Appendix K, specifically. Okay. There have been rulemaking considerations already made by the staff, and in that consideration, if I remember correctly, we've seen cooling was one of the issues which was going to be addressed during the rulemaking hearing.

I think that the last item here can give us more of a balance between some of the things that are positive that we've seen from data and some people's interpretation of other data. And I'd like to try to get that kind of balanced approach.

Thank you.

MR. SHEWMON: Okay. Thank you.

As soon as you finish, we can go to supper. I'm not talking to you.

Are you the clean-up man?

MR. LAUBEN: I think I'm the clean-up man, but I may get some assistance from Les Rubenstein.

I decided I would limit my clean-up simply to our proposed schedule.

MR. SHEWMON: Okay.

MR. LAUBEN: Because I guess Les, before I came, had mentioned to you that what we were looking for was a letter, and I think a letter saying that you approved of the way we planned to go about this. That would be our desire. And --

MR. SHEWMON: I'm not sure how you plan to go about it yet. But then, you'll tell me that eventually.

MR. LAUBEN: And the schedule.

MR. SHEWMON: Okay.

MR. LAUBEN: I think also, let me -- let me, if I can find the schedule --

MR. SHEWMON: It's on the last page of the handout. Do you want to borrow mine?

MR. LAUBEN: Yes.

(Laughter.)

MR. LAUBEN: At least the first item is correct, and we presume that the second item is going to be correct. We're going to discuss this to some degree Friday with the full Committee.

Now, the next part is our part of the schedule, and that is that we would inform the licensees and the applicants the first of next month that ECCS evaluation models must be revised; and then, in a period of three months, we would receive revised evaluation model calculations -- excuse me -- revised evaluation models would be submitted to us for review.

These revised models would include --

MR. SHEWMON: All right, just a minute. You say "revised models" and your handout says "sample calculations."

MR. LAUBEN: Okay.

MR. SHEWMON: Okay. You're at the one before that.

MR. LAUBEN: Yes, I'm at A still.

MR. SHEWMON: All right.

MR. LAUBEN: The revised models would include compensating benefits that the fuel vendors would feel would be appropriate at this time. Rick mentioned that there were -- there was what he called UHI technology which has already been submitted on behalf of certain Westinghouse applicants, which is under review by the staff and for which it would not require them to -- would not require any more difficulty on their part to submit it for a generic model review.

In addition, Combustion Engineering has already submitted compensating benefits in the area of flow distribution, for which they wouldn't be -- there wouldn't be any difficulty for them, either. As a matter of fact, they've already submitted it.

The other vendors, I'm not aware that they feel that they would be required to send anything else in of a compensating nature, but they would be free to do that as well.

So it doesn't appear that that would be too unreasonable a schedule for the vendors to meet.

MR. SHEWMON: All right, that's your opinion, that two months is plenty of time for it?

MR. LAUBEN: Yes, that's right.

MR. SHEWMON: Okay.

MR. LAUBEN: Now, the next part -- in addition and at the same time, we would request sample calculations with this revised model of the NSSS vendors' worst plants, worst breaks, with the revised model, to get interim assurance that all plants meet 10 CFR 1546 with revised ECCS evaluation models.

Now, I think I might want to ask -- well, I might want to ask Ralph at this time, if he hasn't already said --

MR. SHEWMON: Let me ask a minute. You feel that the vendors in two months can gin up a new model, do all their calculations, check it out and get it in to you. And then, working very hard, it takes you a full year to review it.

MR. LAUBEN: Well, first of all let me say -- yeah --

MR. SHEWMON: It doesn't quite sound fair. I mean, I realize Westinghouse has a lot of resources.

MR. LAUBEN: Yeah. I think there's a couple of reasons. We're limited in resources. But more importantly, we believe that there may still be a lot of interchange that's going to go on about these models, and that it's not going to be resolved the minute they send us the new model. There are still going to be things that are going to need to be discussed with the vendors.

MR. SHEWMON: Now, you could get the same symmetry of one-year breaks there if you'd say it was due in on 10/81 and then you had to have your review done 1/1. That way you'd have the one-year break again.

MR. LAUBEN: We would be interested in the Subcommittee and the full Committee's views on it. If you feel that another month or two would help the process, I'm sure we would consider it.

MR. SHEWMON: Okay.

MR. LAUBEN: I'll tell you what. We're -- mostly, we felt it was important to try to do two things here with this on 1/1/81. First, in order to satisfy the legalisms of not running with this patch that we've run with; the idea that Ralph expressed that it's back of the envelope calculation. We're trying to get rid of that as soon as possible.

So we're asking for some sample calculations with what the vendors feel is as close to a full-blown new model as they can get by 1/1/81.

And the other thing is that we do believe that it's not too restrictive, in view of the fact that most of the things that have been discussed that we would envision them coming in with, all the vendors, have been discussed with them before, including NUREG 0630. I think that as far as the most significant aspect, it is in the hot pin calculation. And my experience is that you could put in a NUREG 0630 model in a matter of a couple of weeks.

Now, if there's still going to be some discussion about what's an appropriate model, if it's not going to be exactly NUREG 0630, then I say, okay, let's discuss that after 1/1/81. And that's the reason we have a longer schedule for that.

I'll entertain a question.

MR. ESPOSITO: Just so the statement doesn't go on the record as uncommented on --

MR. LAUBEN: Hey, listen. I bit my tongue a lot when you guys were up there.

(Laughter.)

MR. ESPOSITO: We would be submitting models that (Inaudible) hadn't seen between now and the end of '81. One of the things that Norm is talking about that the staff presently has under review, which was briefly mentioned earlier, is the UHI technology aspect. And the reason for all the work that Westinghouse did there was to be able to compensate for some of the potential penalties we see in some of our products coming downstream, the fuel. We did that work to compensate for that.

So we would still have to go ahead and develop more techniques, more models, to (Inaudible).

MR. MARK: Could I ask: Sample calculations of the vendors' worst plants; does that mean one worst plant from each vendor?

MR. LAUBEN: That means, like for Westinghouse, one -- one worst plant of a plant type.

MR. MARK: They might have to do a two, three, and a four-loop?

MR. LAUBEN: Yes.

MR. MARK: Now, do you have a preferred list of worst plants? Or by the time they put in their mitigating features, the plants you're thinking of might look a lot better.

MR. LAUBEN: Well, it could. But for now we'd be willing to accept -- I think we'd be willing to risk the fact that we'll take the one that has the highest priority.

MR. MARK: On the old model?

MR. LAUBEN: Yes.

MR. MARK: So it would be the worst one on the present model would be looked at in the new model.

MR. LAUBEN: Sure, it's a risk we take.

MR. MARK: I can see it's a risk.

(Laughter.)

MR. LAUBEN: Rick and I have played this peaking factor game before. We played it with the zirc-water error, and I think we guessed conservatively correctly on about 19 out of 21. So I don't think that batting average is too bad. So I'm willing to take the risk.

MR. SHEWMON: That's a lot better than 400, and that's what's (Inaudible).

(Laughter.)

MR. SHEWMON: Okay.

MR. LAUBEN: So anyway, admittedly this schedule is long. We're trying to submit something that looks as close to NUREG 0630 as possible by January 1st, so we can get out of the back of the envelope game; and then take some time if there are still discussions that we need to iron out, with new models that may take into account average strain or, as Vinnie has pointed out, maybe even some new thermal hydraulic models that I haven't seen yet.

Now, let's see. So that -- you're right, there's a whole year in there before we have the final models completed. And then there's another year that looks like a backfitting year. I'd say I'd be willing to entertain approximately --

MR. SHEWMON: How about a forward-fitting year; Esposito would like it better.

MR. LAUBEN: Well, there would be no -- there would be no forward -- there wouldn't be any forward fitting yet. It says analysis would revise models for all plants.

MR. SHEWMON: There'd be some back fitting.

MR. LAUBEN: That would require some back fitting, because not every plant gets reloaded within a year, nor does every plant -- a lot of plants are what they call 50.59 plants and they don't have to be re-analyzed every reload. So this would imply some backfit there.

And I'm open to rethink this one if we have to. After all, that's two years away.

MR. MARK: You left off a day of 1982.

MR. LAUBEN: I did.

(Laughter.)

MR. LAUBEN: Well, I figure we're not going to come in on New Year's Eve.

(Laughter.)

MR. LAUBEN: Do you care to elaborate on anything, Lester?

MR. SHEWMON: I think at this point let me, I think the term is, go into open executive session. Does anybody see any problems with taking this to full Committee on Friday? And if not, do we have any advice on what they bring up, or are there other problems that should be --

MR. LAWROSKI: How much time is scheduled for it?

MR. SHEWMON: Two hours.

MR. MATHIS: About two hours.

Are we as a Subcommittee expected to have a recommendation of sorts?

MR. SHEWMON: Presumably, if we go to the full Committee, we will. And maybe that's another question to bring out. Do you feel that we can write a letter on it or you would like to write a letter on it?

MR. MATHIS: I'd hate to have to write the letter right now. Let's think about it. We've got a day, two.

MR. STRASSER: It depends what the letter says.

MR. MATHIS: When is this to be discussed? Friday?

MR. BOLHUERT: Friday, 4:20 to 5:30.

MR. SHEWMON: I don't know whether we can bring open -- you know, we can talk to Plesset about -- one could write a letter saying, you know, parts of it we like and parts of it we wonder about and, gee whiz, it would be nice if Appendix K could be reconsidered some day. Or we could just say, 630 is great and let's implement it. Or something in between.

MR. LAWROSKI: Well, in view of the discussion of today, I think trying to get that thing in in two hours, that's -- that can get tough. That may influence whether or not you can write a letter.

MR. MATHIS: Well, there's one other part of this, and that is that 630 as such is still going to be two years in the review process, essentially.

MR. SHEWMON: Yes.

MR. MATHIS: So there's a stall option in here. You proceed with a schedule similar to this, to be reviewed when you get all the feedback you asked for.

MR. SHEWMON: Well, "stall option" is what the staff would say. It's not what Westinghouse would say, I suspect. It's in the eye of the beholder.

It seems to me one of the other questions is to what extent this gets cast in concrete. The last time we heard this presentation it was supposed to be good for ten years with no further changes, maybe, we hoped. This time everybody agrees that there will be other results in in a few years, and maybe if the staff's flexible -- they're always reasonable; just ask them.

Who were you pointing at over here?

MR. RUBENSTEIN: I would look for some stability over the next three to five years. I can't promise ten years. There is a lot of activity. But we, the staff, certainly don't want to reopen this in a ten-year term again. One of the things that's driving us to get it behind us is that we want to do other things.

MR. SHEWMON: Do you feel the admonition to make -- not underestimate clad strain or whatever, the ballooning --

MR. RUBENSTEIN: Incident stress.

MR. SHEWMON: -- also requires that you put that into an increased blockage model and not just an increased rate of oxidation and whatever else?

MR. RUBENSTEIN: I think to some degree; I think it does.

MR. SHEWMON: Yeah. One of the things that always sort of bothered me in this business is instantaneous double-ended pipe breaks may be a bounding calculation, but when you get to believe in them so much you worry about asymmetric loads it really gets rather silly. But --

MR. RUBENSTEIN: We wouldn't object to your views on the overall Appendix K.

MR. SHEWMON: Well, do we have any other?

MR. STRASSER: Is there a potential of considering Pic's model, any time to consider it?

MR. SHEWMON: It seems to me --

MR. STRASSER: Because that seems one of the major objectives.

MR. SHEWMON: To me that would come under the general heading of flexibility. It seems to me that's one very interesting thing that has come up. There is data extant to go back and look at this over the next six months or a year and get something out in something much less than five years.

I think the NRU stuff is really not going to get shaken down for several years.

MR. STRASSER: I just meant a revision or modification based on current data, not data from new --

MR. SHEWMON: Well, you're still talking about a reasonable amount of time. I mean, even if he works all his weekends, to get it down, get it shaken down, get other people to look at it, is six months minimum. So I'd be pleased, if we do write a letter, to put in something saying, yeah, they should look at that and if it looks reasonable or continues to look promising, why --

MR. MATHIS: I have one other question, though,, and that is: Is the staff really looking at this as a flexibility kind of thing for 0630, or do you consider 0630 as cast in concrete, essentially?

MR. RUBENSTEIN: Well, let me answer that in two ways. One, we would like not to be prescriptive and say, 0630 is the way you have to do it. But that would be facetious. In point of fact, when you look at the data on the flow blockage curves, for example, and you find that the staff's bounding with the flow blockage curve, while we disagree somewhat with Westinghouse, is not grossly out of line.

However, we would want to obtain a model which used average strain if there were sufficient data. And at this time I'm not sure the data would change the outcome drastically in terms of the ultimate flow blockage. It is a more satisfying way of calculating it than with the ruptures, strains, and it's probably more fundamentally based. But it probably won't change the outcome, and probably that is -- I'm probably not as optimistic as Dick is that somebody's going to throw it together and we're going to review it in nine months.

MR. SHEWMON: In other words, what you're saying is that the concrete is setting mighty fast.

MR. MEYER: The outcome of the NUREG 0630 model on flow blockage just skims across the top of the collection of nine -- eight or nine data points that we have. And however you approach that, you're going to have to skim along those data points. And I don't see that a different approach is going to afford you much variation from what we ended up with.

MR. STRASSER: One different approach apparently may be the differential, temperature differences, circumferential differences in temperature. Is it even feasible to have such a model.

MR. RUBENSTEIN: I would leave that question to Westinghouse.

MR. STRASSER: Pardon?

MR. RUBENSTEIN: I would leave that question to Westinghouse.

MR. STRASSER: (Inaudible). There's Argonne data, and others have also proposed this as a --

MR. RUBENSTEIN: (Inaudible). LOCA space would be rather difficult.

MR. STRASSER: Yes. All right. As Ralph says, to define temperatures in a LOCA is pretty tough. I'm wondering whether it's a feasible test or not.

MR. SHEWMON: It seems to me there's one philosophical point that bothers me a fair amount. And I don't know whether the Committee's willing to take it on or not. But if Westinghouse brought it up and if we are trying to attain truth in some way, if everybody feels that they must be on the conservative side of everything that's reputable, then you know darn well that you don't end up with a best estimate or even a mildly conservative estimate; you end up with an extremely conservative estimate.

And that's what bothers me about this statement here that we must end up with a curve that bounds these nine points; you know, purity would not allow anything else.

MR. BURMAN: I'd like to make a comment on Ralph's comment. All of these multi-rod burst tests that he's saying he skims the boundary of are, I think, 4 by 4 tests. Oak Ridge recently ran one a little bit larger. If there's something different, then we have --

MR. MUENCH: There are 7 by 7's in there.

MR. BURMAN: Or 7 by 7. Appendix K and the model approvals require that we base blockage on a full assembly cross-section. And from Dick's discussion of average burst strain, even though those small bundles show higher peak clad blockages or peak flow blockages in the smaller arrays, when you put those together to make a larger array, which is our model base, they won't have that maximum. They will have more like the average.

MR. MEYER: We accounted for that explicitly on 0630. It was taken account of.

MR. SHEWMON: It may not be accurately, but it's explicitly.

MR. MEYER: There's a difference with smaller blockages based on averages instead of peaks for PWR blockages.

MR. SHEWMON: How do we -- how do we extrapolate from 4 by 4 to 17 by 17?

MR. MEYER: What we do is, for the model as compared to the data, since the data are taken in the plane of maximum blockage, we use a model that's designed to give maximum blockage based on the Oak Ridge tests.

For the large PWR bundles, instead of looking at the plane of maximum blockage in Chapman's test, we average and just look at average blockage and say that our adjustment for bundle size effect will be to back away from maximum blockage as observed in the test to average blockage as observed in the test,

MR. SHEWMON: And you hope and believe that that compensates for going from 4 by 4 to 17 by 17?

MR. MEYER: Well, there are 7 by 7's in there. Yes, we do, basically.

MR. STRASSER: Somewhere in some report I think -- I forget which one now, who commented on it; I think it was Chapman himself. He didn't feel that this was extrapolatable to large bundles.

MR. MEYER: Chapman's here.

MR. CHAPMAN: I think what you have reference to is perhaps pressure drop data per se from the 4 by 4 to the large bundle, because of the lack of radiant restraint. From that viewpoint and also, I guess, (Inaudible) who say that deformation in a larger bundle may produce more blockage, because the outer rods constrain the inner rods and so they can't balloon out nicely. They become -- (Inaudible).

MR. STRASSER: It could be either direction, either more or less than 7 by 7.

MR. CHAPMAN: Yes.

MR. SHEWMON: Okay. Do we have any other questions or --

MR. LAWROSKI: Does anyone care to hazard what they speculate will be the result of the further experiments that were mentioned with NRU? Is the probability high that that will provide further verification of the position? Is it going to load to greater disparity?

MR. SHEWMON: It seems to me there are three points. There's that, when you get into an in-core, whether anything can be made that would convince the staff of the statistical argument that Dick was referring to but we didn't get into, that basically scaling up the smaller units to big units, an awful lot of -- okay.

And I've forgotten the third one because I'm getting hungry. I don't know. The question is open. Would somebody care to speculate?

MR. JOHNSTON: I'd like to speculate a little bit, because part of the sequence of tests we've designed with (Inaudible), the NRU tests were designed to try to pick up the out of pile stuff, the MRBT, and look at the real effects on the thing; and also to reflect doing it with something other than electrically heated rods. Because it's true that the circumferential effect cannot really be simulated in the out of pile kinds of tests. If we have real pellets in there, we begin to find out whether there are circumferential temperature effects and whether they're important or not.

We -- and I expect if we should learn something like that from the NRU tests, or if we should learn that from tests that are conducted in ESSOR in a somewhat later time frame, with bundles of size 36 and thereabouts -- that would be the 200 size.

We had the thing that Chapman referred to. The idea that when you get the larger bundle, the outer rods serve as a constraint, is a possibility, something that we hadn't considered that maybe we should have. That is, if you've got rods on the outside that are not heated or which don't balloon, the rods on the inside expand out and actually occupy more space than they would have if they hadn't been restrained.

That is a possible negative which may in fact get larger (Inaudible), and I think we need to get that information.

MR. SHEWMON: Larger balloonings on certain elements, but not larger average total, is it?

MR. JOHNSTON: The ones in the center could have larger ballooning than you would have otherwise.

MR. SHEWMON: Yes.

MR. JOHNSTON: They have more restraint.

MR. SHEWMON: Okay.

MR. JOHNSTON: (Inaudible) from bowing. That's a factor that may compensate for the circumferential temperature effect.

I guess what I'm saying is I don't know whether it's going to confirm or deny. I think there are two effects that we have yet to learn about, one of which will move things in a smaller ballooning direction, the other of which would tend to move it in the larger ballooning direction. I don't think we know what the balance will be yet.

MR. LAWROSKI: Do you think the situation is 50-50 or is it 60-40? (Inaudible).

MR. MARK: It's 60-40; he doesn't know which way.

(Laughter.)

MR. JOHNSTON: I guess I like to say I'm optimistic. I always like to feel that something --

MR. SHEWMON: (Inaudible).

MR. JOHNSTON: I feel that they will probably be somewhat smaller than were anticipated, because I know more about that kind of estimate.

MR. SHEWMON: I'd like to bring this to a close. Does anybody else have any other questions?

MR. MEYER: Or pearls of wisdom.

MR. SHEWMON: Or pearls of wisdom.

MR. MEYER: I have a very constructive suggestion.

MR. SHEWMON: Aside from adjournment.

MR. MEYER: Aside from adjournment.

There has been some discussion about whether a new decay heat standard could be used without revising the rules. The staff has in the past made some statements on that to the effect that it couldn't be done. I'm not personally convinced that that has been explored fully as an option. I think that would eliminate (Inaudible) if possible and would avoid going through a rulemaking process.

ACRS itself has not encouraged our going into rulemaking hearings. So really, we are in between a rock and a hard place.

MR. SHEWMON: My guess is that somebody will quote scripture to us just about the way Norm did, and that is even more firmly stated than using only dry steam and making it a negative contribution to whatever. But you maybe read it more recently than I have.

(Whereupon, at 6:26 p.m., the Subcommittee was adjourned.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS-REACTOR FUEL SUBCOMMITTEE MEETING

Date of Proceeding: September 3, 1980

Docket Number: ______________________

Place of Proceeding: Washington, D. C.

were held as herein appears, and that this is the original transcri
thereof for the file of the Commission.

Suzanne Babineau

Official Reporter (Typed)

Suzanne Babineau

Official Reporter (Signature)

ACRS REACTOR FUEL SUBCOMMITTEE MEETING
SEPTEMBER 3, 1980
WASHINGTON, D.C

		PRESENTATION* TIME	ACTUAL TIME
I.	INTRODUCTION P. SHEWMON, CHAIRMAN	10 Min	1:00 PM
II.	NRC PRESENTATIONS		
	A. Introductory Remarks W. Johnston, Chief, CPB	15 Min	1:15 PM
	B. ECCS Evaluation Model Methodology N. Lauben, NRR	15 Min	1:20 PM
	C. New Cladding Models for LOCA Analysis - R. Meyer, CPB	30 Min	1:45 PM
	BREAK	10 Min	2:30 PM
	D. Alternate Flow Blockage Model M. Pickleshimer, FBRB/RES	45 Min	2:40 PM
III.	COMMENTS BY WESTINGHOUSE ON 0630 MODEL V. Esposito D. Burman L. Hochreiter R. Muench	60 Min	3:40 PM
IV.	GENERAL SUMMARY	10 Min	5:00 PM
V.	DISCUSSION W. Johnston	15 Min	5:15 PM
VI.	ADJOURN		5:30 PM

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* ADDITIONAL TIME HAS BEEN ALLOWED FOR COMMITTEE QUESTIONS

Topics 1 2 4 5

LICENSING CALCULATIONS
AND CLADDING MODELS

- RELATED FEATURES OF APPENDIX K
- SWELLING AND RUPTURE EFFECTS
- COMPUTER MODELS
- STRAIN AND INCIDENCE EFFECTS
- BLOCKAGE EFFECTS

APPENDIX K, PARA. I.B.

SWELLING AND RUPTURE OF THE CLADDING AND FUEL ROD THERMAL PARAMETERS

EACH EVALUATION MODEL SHALL INCLUDE A PROVISION FOR PREDICTING CLADDING SWELLING AND RUPTURE FROM CONSIDERATION OF THE AXIAL TEMPERATURE DISTRIBUTION OF THE CLADDING AND FROM THE DIFFERENCE IN PRESSURE BETWEEN THE INSIDE AND OUTSIDE OF THE CLADDING, BOTH AS FUNCTIONS OF TIME. TO BE ACCEPTABLE, THE SWELLING AND RUPTURE CALCULATIONS SHALL BE BASED ON APPLICABLE DATA IN SUCH A WAY THAT THE DEGREE OF SWELLING AND INCIDENCE OF RUPTURE ARE NOT UNDERESTIMATED. THE DEGREE OF SWELLING AND RUPTURE SHALL BE TAKEN INTO ACCOUNT IN CALCULATIONS OF GAP CONDUCTANCE, CLADDING OXIDATION AND EMBRITTLEMENT, AND HYDROGEN GENERATION.

THE CALCULATIONS OF FUEL AND CLADDING TEMPERATURES AS A FUNCTION OF TIME SHALL USE VALUES FOR GAP CONDUCTANCE AND OTHER THERMAL PARAMETERS AS FUNCTIONS OF TEMPERATURE AND OTHER APPLICABLE TIME-DEPENDENT VARIABLES. THE GAP CONDUCTANCE SHALL BE VARIED IN ACCORDANCE WITH CHANGES IN GAP DIMENSIONS AND ANY OTHER APPLICABLE VARIABLES.

PARA. I.C.7.

PWR CORE FLOW DISTRIBUTION DURING BLOWDOWN

.....THE HOT REGION CHOSEN SHALL NOT BE GREATER THAN THE SIZE OF ONE FUEL ASSEMBLY. CALCULATIONS OF AVERAGE FLOW AND FLOW IN THE HOT REGION SHALL TAKE INTO ACCOUNT CROSS FLOW BETWEEN REGIONS AND ANY FLOW BLOCKAGE AS A RESULT OF CLADDING SWELLING OR RUPTURE.

PARA. I.D.5.

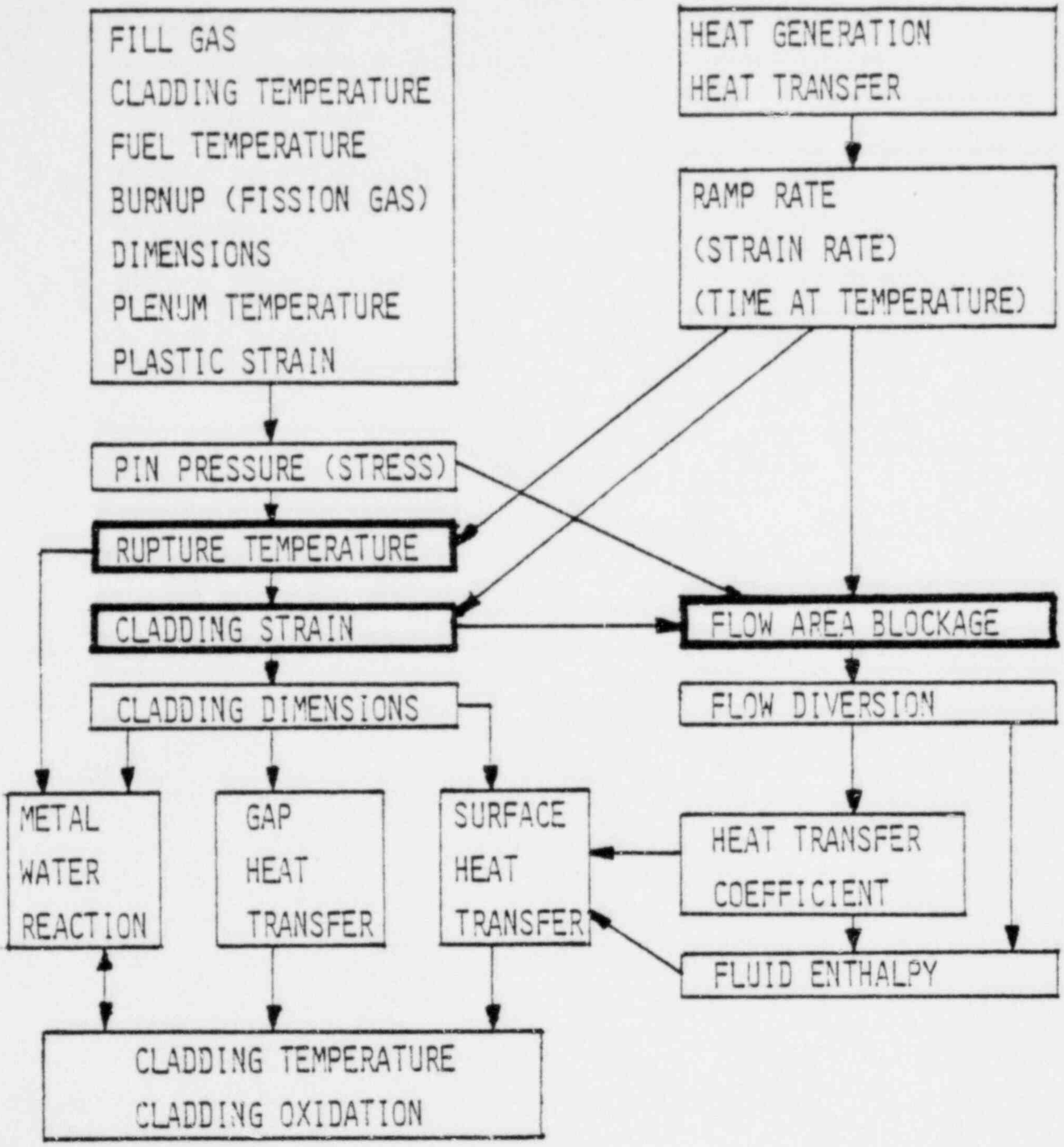
PWR REFILL AND REFLOOD HEAT TRANSFER

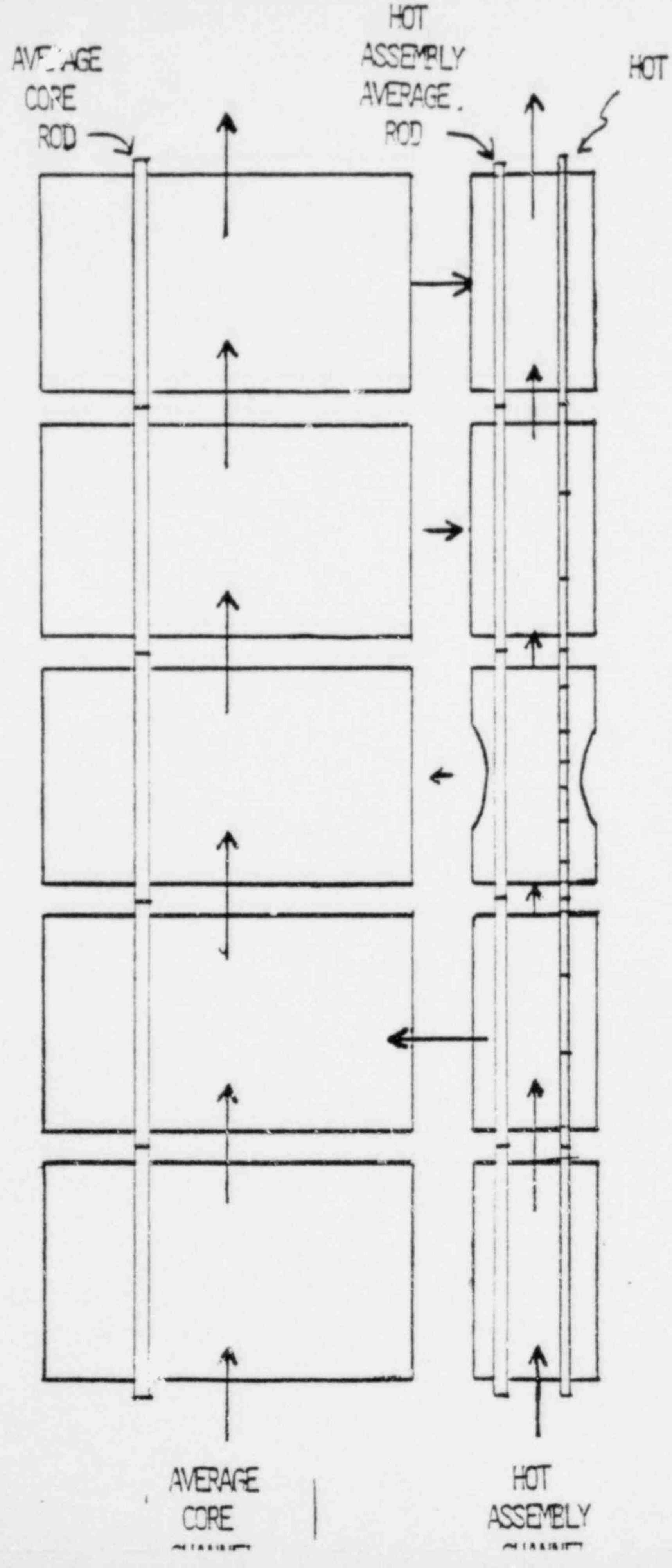
DURING REFILL AND DURING REFLOOD WHEN REFLOOD RATES ARE LESS THAN ONE INCH PER SECOND, HEAT TRANSFER CALCULATIONS SHALL BE BASED ON THE ASSUMPTION THAT COOLING IS ONLY BY STEAM, AND SHALL TAKE INTO ACCOUNT ANY FLOW BLOCKAGE CALCULATED TO OCCUR AS A RESULT OF CLADDING SWELLING OR RUPTURE AS SUCH BLOCKAGE MIGHT AFFECT BOTH LOCAL STEAM FLOW AND HEAT TRANSFER.

SWELLING AND RUPTURE EFFECTS

- FLOW BLOCKAGE EFFECTS
  1. SURFACE HEAT TRANSFER
  2. COOLANT ENTHALPY
- STRAIN EFFECTS (PIN GEOMETRY)
  1. SURFACE HEAT TRANSFER
  2. GAP HEAT TRANSFER
  3. METAL-WATER REACTION

IMPORTANT PARAMETERS



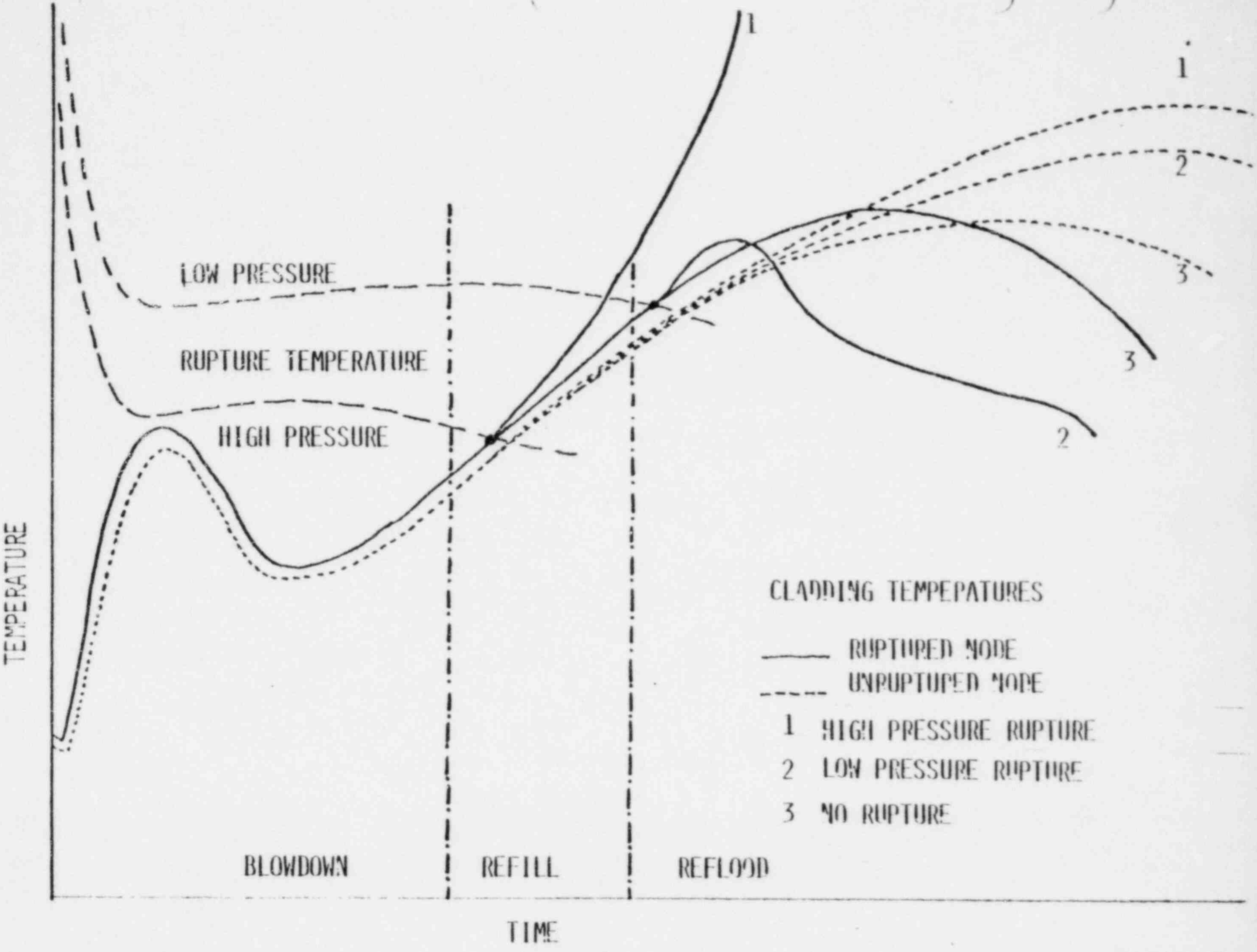


TYPICAL
PWR
CORE
MODEL

AVERAGE CORE ROD
REPRESENTS 25,000
TO 50,000 FUEL RODS

HOT ASSEMBLY
AVERAGE ROD
REPRESENTS ABOUT
200 RODS

THE HOT ROD IS A
SINGLE ROD



1
1
2
LOW PRESSURE
RUPTURE TEMPERATURE
HIGH PRESSURE
3
3
2
TEMPERATURE
CLADDING TEMPERATURES
——— RUPTURED NODE
- - - - UNRUPTURED NODE
1 HIGH PRESSURE RUPTURE
2 LOW PRESSURE RUPTURE
3 NO RUPTURE
BLOWDOWN
REFILL
REFLOOD
TIME

RUPTURE STRAIN AND INCIDENCE OF RUPTURE EFFECTS

- THE EFFECTS ARE DIRECTLY CALCULATED USING THOSE PARAMETERS.

- ALL EFFECTS (EXCEPT SURFACE RADIATION) CONSIDERED FOR ALL ANALYZED PINS THROUGHOUT ENTIRE TRANSIENT.

- SURFACE RADIATION MODELS APPLIED DIFFERENTLY DURING POST-BLOWDOWN PERIODS (PWR EFFECT ∠ 30°F)

- GREATEST EFFECTS ON HOT PIN (PWR) OR HOT PLANE (BWR).

- STRAIN/INCIDENCE EFFECTS CAN EFFECT PWR FROM RUPTURE ELEVATION (6FT.) TO 9FT. GREATEST SINGLE EFFECT IS TWO-SIDED REACTION AT RUPTURED NODE.

- EFFECTS FOR PROPOSED STRAIN/INCIDENCE MODEL CHANGES WORTH 0 - 800°F (0-.05 F_Q).

FLOW BLOCKAGE EFFECTS
(FLOW DIVERSION HEAT TRANSFER)

- BLOWDOWN EFFECTS SMALL FOR MOST REACTORS, (EXCEPT SOME B&W)

- POST-BLOWDOWN BWR EFFECTS ACCOUNTED FOR IMPLICITLY IN HEAT TRANSFER MODEL DERIVED FROM BWR FLECHT.

- PWR BLOCKAGE CONSIDERED ON HOT ROD ONLY WHEN FLOODING RATES LESS THAN 1 IN/SEC. (APPENDIX K STEAM COOLING REQUIREMENT).

- FLOW DIVERSION AND HEAT TRANSFER CALCULATED DIFFERENTLY FOR ALL PWR FUEL VENDORS ($<$1 IN/SEC.)

- EFFECT OF PROPOSED BLOCKAGE MODEL CHANGES WORTH 0 - 150° F (0 - .15 Fq).

COMMISSION OPINION:

FOR LOWER REFLOOD RATES BLOCKAGE WOULD HAVE A DELETERIOUS EFFECT AND ONE MUST RESORT TO CALCULATION WITH SINGLE PHASE STEAM COOLING, TAKING INTO CONSIDERATION THE EFFECTS OF BLOCKAGE ON CORE FLOW DISTRIBUTION.

PWR REFLOOD FLOW BLOCKAGE DATA

- APPENDIX K CONSIDERATIONS BASED ON EARLY PWR FLECHT BLOCKAGE TESTS.

  RESULTS INCONCLUSIVE

  - BLOCKAGE NOT TYPICAL
  - HEAT TRANSFER NOT ADVERSELY AFFECTED EXCEPT AT HIGH BLOCKAGE & LOW FLOODING RATE.
- FEBA, NRU + NEW FLECHT BLOCKAGE TEST RESULTS AVAILABLE IN 1 - 2 YEARS.
- EXPECT RESULTS WILL SHOW EFFECT OF BLOCKAGE NOT AS SEVERE AS PRESENT MODELS SHOW.
- RECOMMENDED APPENDIX K CHANGES:
  - ELIMINATE STEAM COOLING REQUIREMENT
  - CONSIDER BLOCKAGE EFFECTS AT ALL FLOODING RATES

TWO-PHASE FLOW EFFECTS

- MOST HOT-PIN RUPTURES IN PWR LICENSING ANALYSES ARE CALCULATED TO OCCUR SOMETIME BETWEEN LATE BLOWDOWN AND EARLY REFLOOD (INCLUDING REFILL)
- THIS IS THE TIME WHEN THE SYSTEM IS MOST "EMPTY" AND THE CLADDING IS HEATING UP.
- ALSO THE SYSTEM IS CHANGING MOST RAPIDLY FROM HIGH QUALITY STEAM DOWN FLOW, TO AN UNKNOWN BEHAVIOR DURING REFILL, TO REFLOOD FROM THE BOTTOM.
- CURRENTLY AVAILABLE EXPERIMENTS OR ANALYSES CAN NOT ACCURATELY CHARACTERIZE THE TWO-PHASE CORE FLUID BEHAVIOR DURING THIS PERIOD. (EXCEPT EARLY REFLOOD WHERE UPPER CORE ELEVATIONS ARE DRY).

THE "TYPE" OF TWO-PHASE FLOW COULD HAVE AN EFFECT ON STRAIN AND BLOCKAGE.

- HOWEVER, REFLOOD TEST DATA INDICATES THAT RUPTURES OCCUR 200-800 F ABOVE THE "REWET" TEMPERATURE. THUS, TWO-PHASE FLOW BEHAVIOR MAY HAVE VERY LITTLE EFFECT ON STRAIN AND BLOCKAGE.
- BECAUSE OF TWO-PHASE FLOW UNCERTAINTIES, AND HIGH RUPTURE TEMPERATURES RELATIVE TO REWET, NRR BELIEVES RUPTURE EXPERIMENTS IN STEAM ARE MOST APPROPRIATE.
- VENDORS HAVE PROPOSED SEVERAL MODEL CHANGES TO ACCOUNT FOR IMPROVED FLOW DIVERSION AND HEAT TRANSFER. THE MODELS HAVE GENERALLY RECEIVED FAVORABLE REVIEW BY THE STAFF.

PROPOSED SCHEDULE FOR RESOLUTION OF SWELLING AND RUPTURE ISSUE

9-3-80	DISCUSS WITH ACRS SUBCOMMITTEE
9-5-80	DISCUSS WITH ACRS FULL COMMITTEE
10-1-80	INFORM LICENSEES AND APPLICANTS THAT ECCS EVALUATION MODELS MUST BE REVISED.
1-1-81	(A) REVISED ECCS EVALUATION MODELS SUBMITTED TO NRC FOR REVIEW. (B) SAMPLE CALCULATIONS OF NSSS VENDOR'S WORST PLANTS WORST BREAKS WITH REVISED ECCS EVALUATION MODELS TO GIVE INTERIM ASSURANCE THAT ALL PLANTS WILL MEET 10CFR 50.46 WITH REVISED ECCS EVALUATION MODELS
1-1-82	(A) ALL NRC REVIEWS OF ECCS EVALUATION MODELS COMPLETED BY THIS DATE (B) ALL ECCS CALCULATIONS PERFORMED AFTER NRC APPROVALS TO BE DONE WITH REVISED MODELS.
12-30-82	ANALYSES WITH REVISED MODELS FOR ALL PLANTS MUST BE ON FILE BY THIS DATE.

Second Hando
Tape 3,+4

CLADDING SWELLING AND RUPTURE MODELS FOR LOCA ANALYSIS

SEPTEMBER 3, 1980

PRESENTATION TO ACRS
BY
CORE PERFORMANCE BRANCH

UNITED STATES
NUCLEAR REGULATORY COMMISSION
WASHINGTON, D. C. 20555

INTRODUCTION . . . . .W. V. JOHNSTON

METHODOLOGY OF ECCS EVALUATION MODELS. . . .G. N. LAUBEN

DISCUSSION OF COMMENTS ON NUREG-0630 . . .R. O. MEYER

AN ALTERNATE FLOW BLOCKAGE MODEL . . . . .M. L. PICKLESIMER

SCHEDULE FOR IMPLEMENTATION OF CHANGES . .G. N. LAUBEN

#1

MAJOR QUESTIONS ON THE SWELLING AND RUPTURE ISSUE

1. WERE IMPORTANT DATA SETS OVERLOOKED IN DERIVING THE RUPTURE-TEMPERATURE CORRELATION?

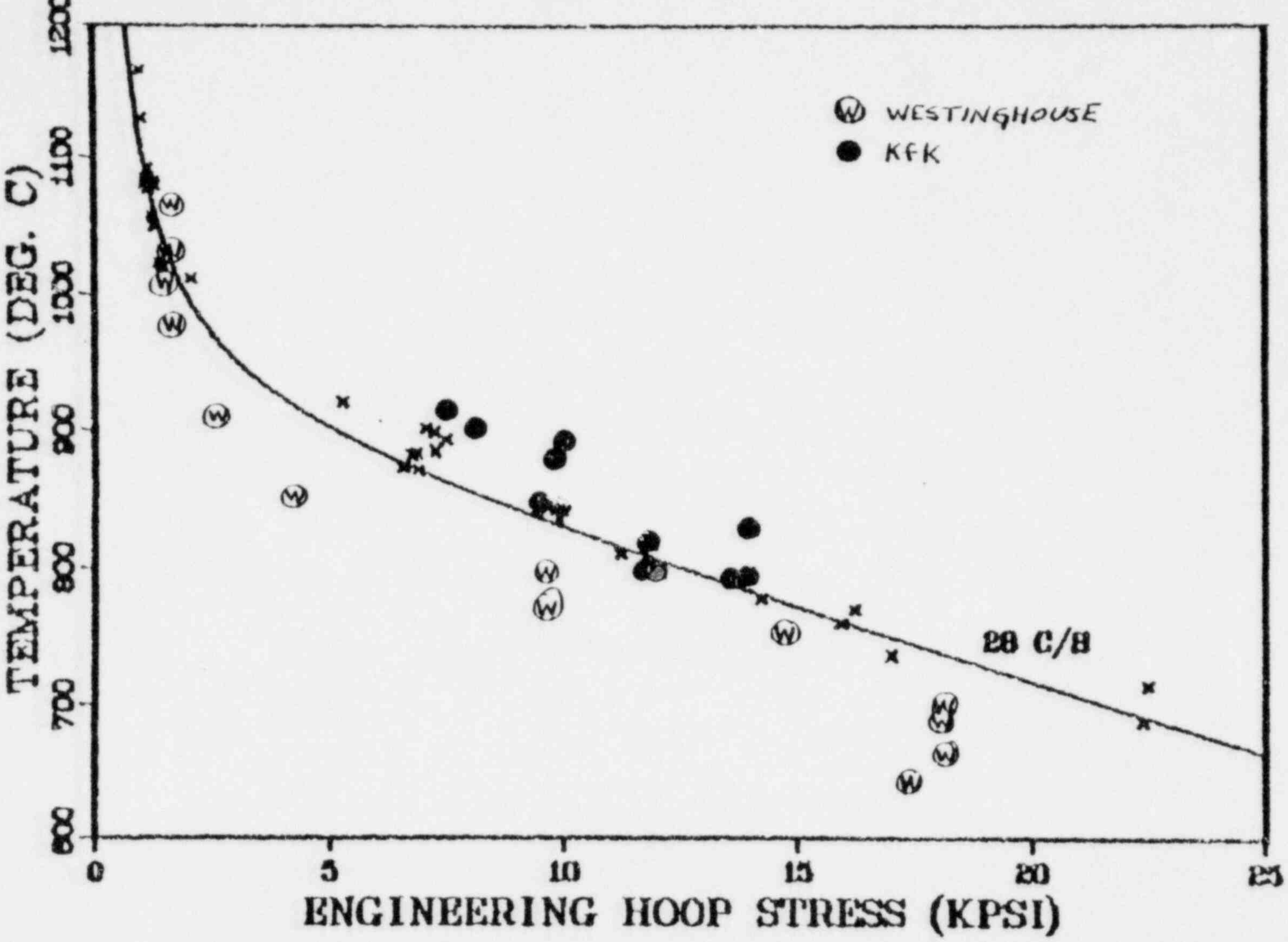
2. WERE DATA SELECTIVELY USED TO PRODUCE LARGE STRAINS IN THE BURST-STRAIN CORRELATION?

3. IS THE METHODOLOGY VALID FOR CONVERTING BURST STRAINS INTO FLOW BLOCKAGE?

4. IS THERE A NEED TO REQUIRE CHANGES IN LICENSING MODELS?

5. SHOULD SUCH CHANGES BE MADE NOW?

RUPTURE TEMP. CURVE & ORNL 28 C/S DATA

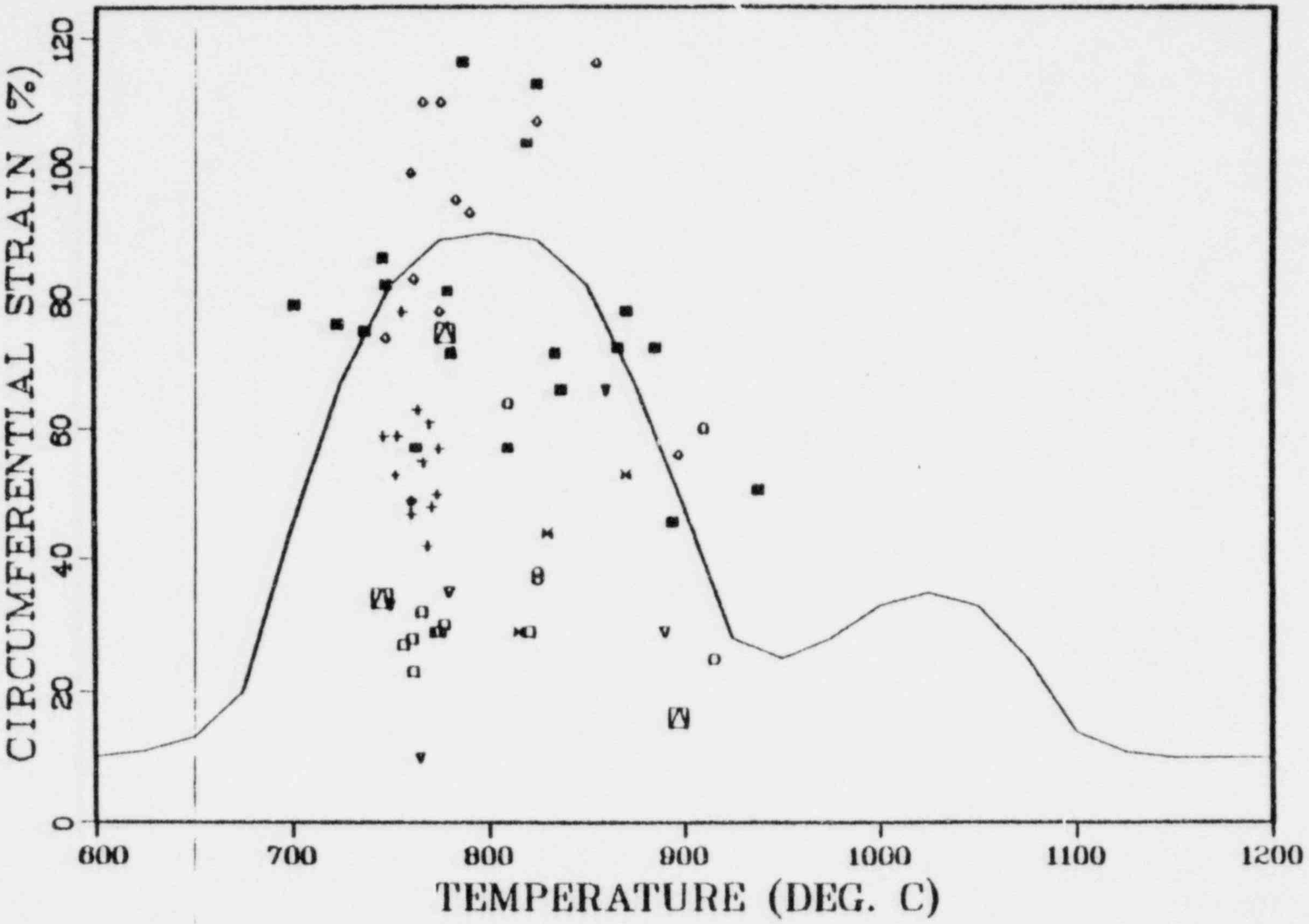


6

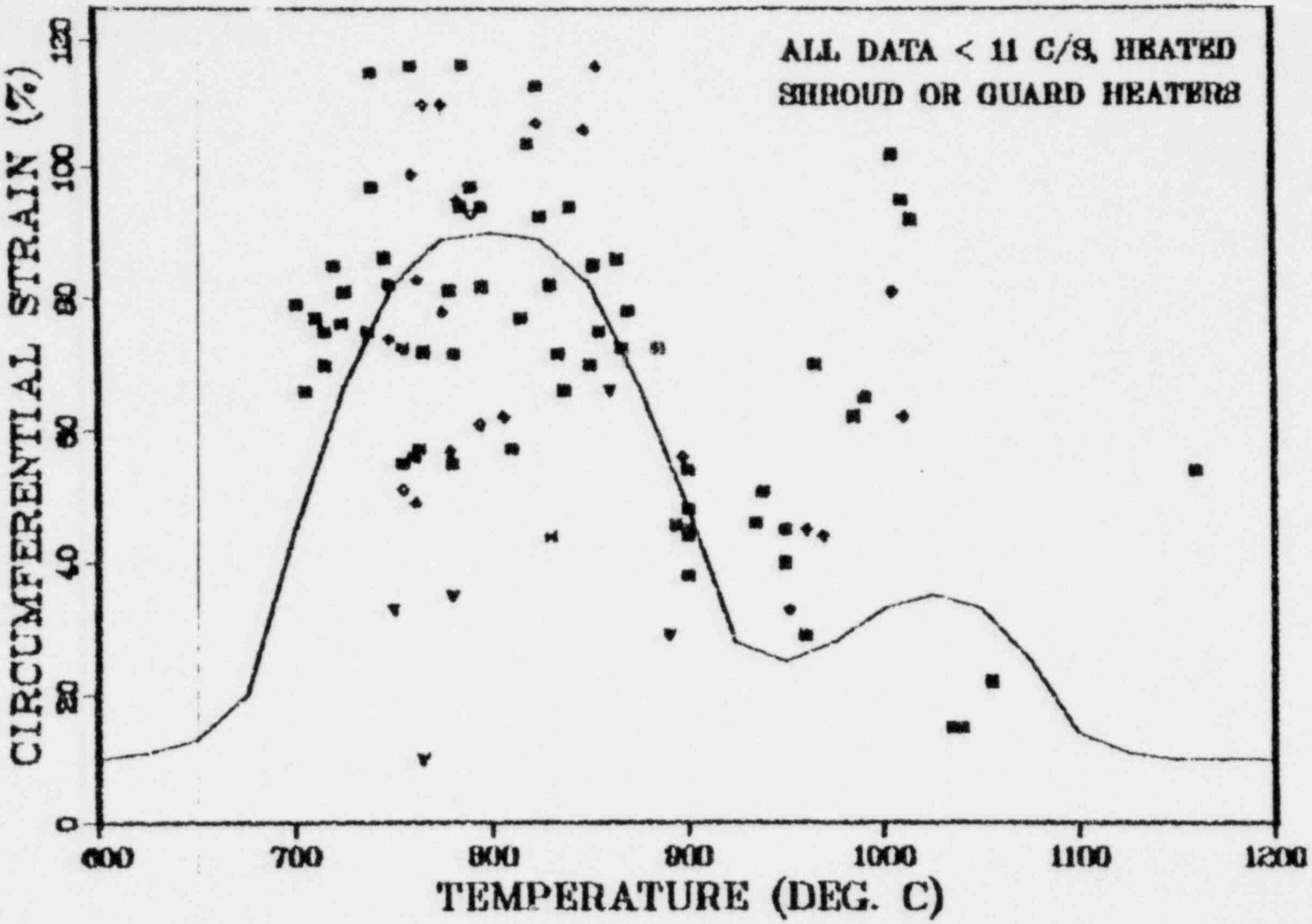
#3

WERE DATA SELECTIVELY USED TO
PRODUCE LARGE STRAINS IN THE
BURST-STRAIN CORRELATION?

SLOW-RAMP BURST STRAIN CURVE & DATA



SLOW-RAMP BURST STRAIN CURVE & DATA



#6

IS THE METHODOLOGY VALID FOR
CONVERTING BURST STRAINS INTO
FLOW BLOCKAGE?

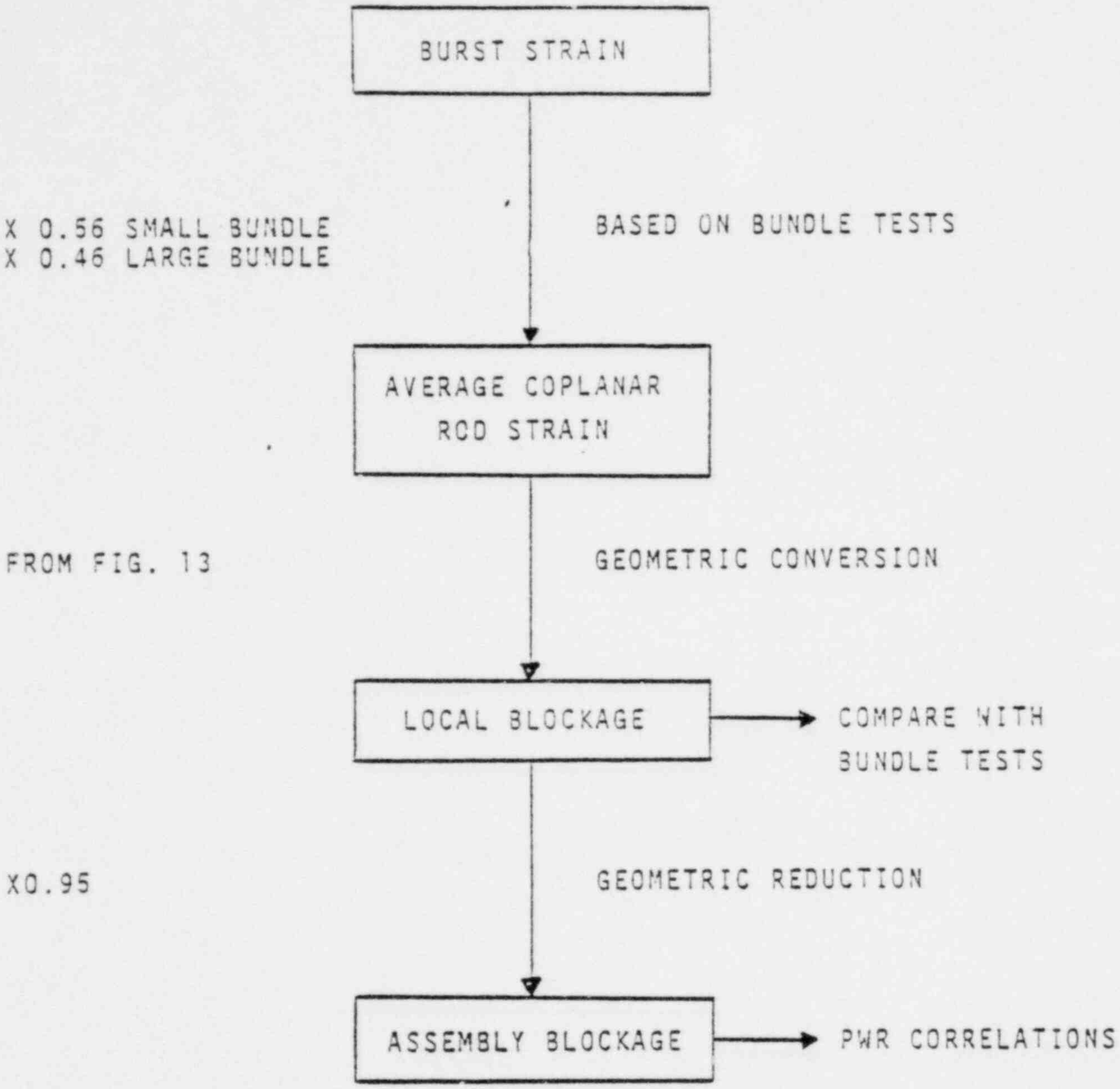
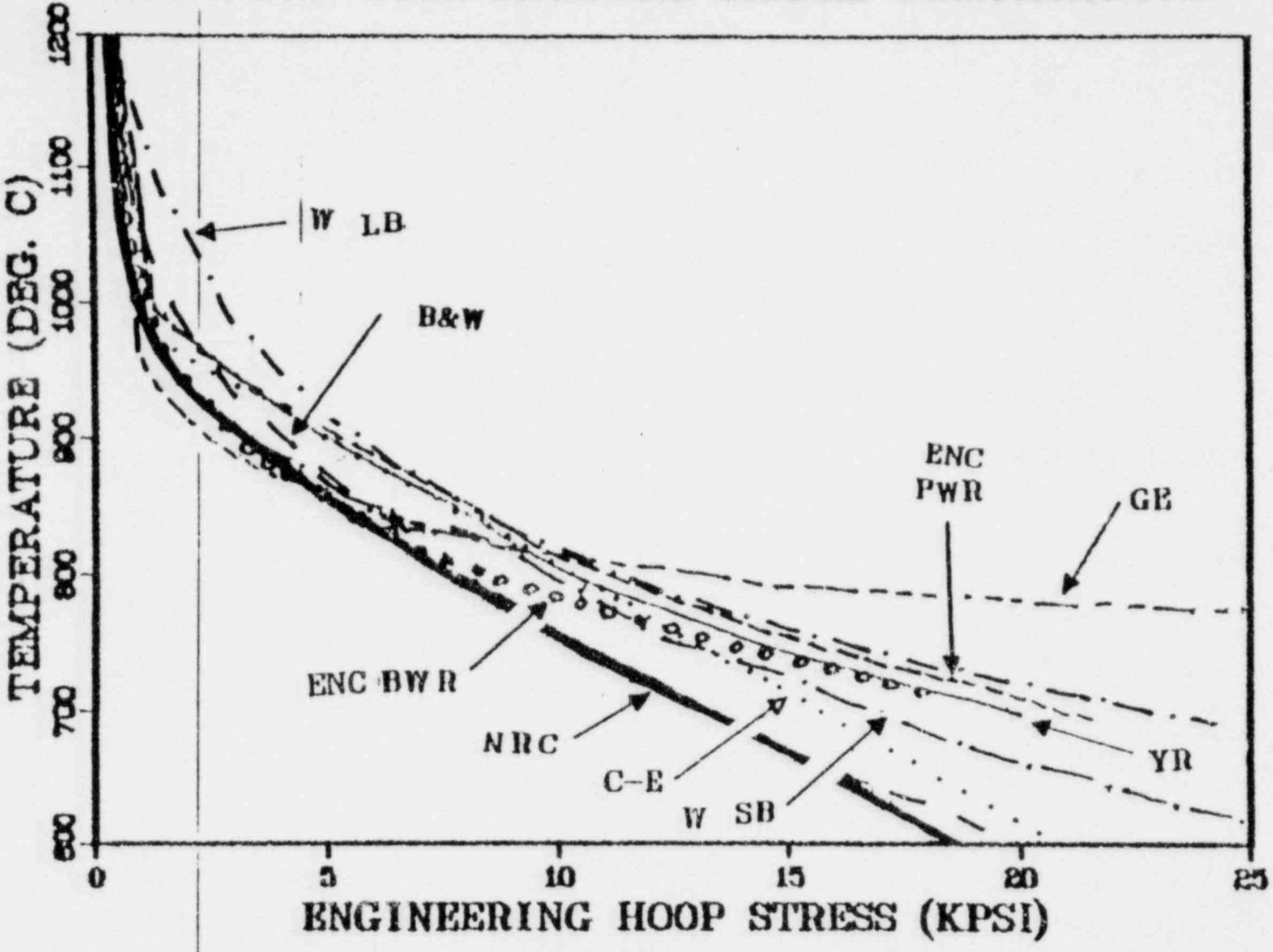
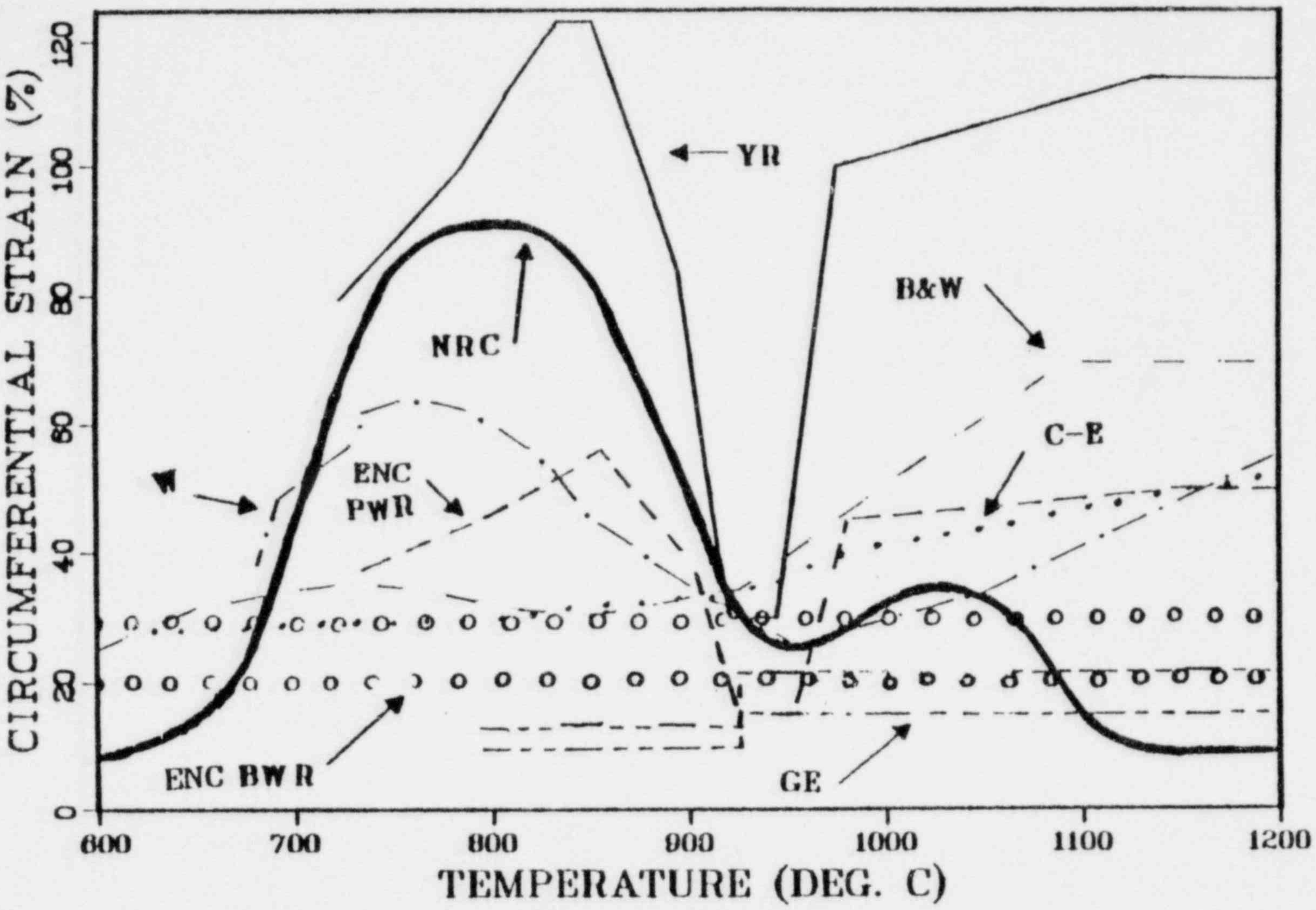


Fig. 10 Outline of flow blockage model.

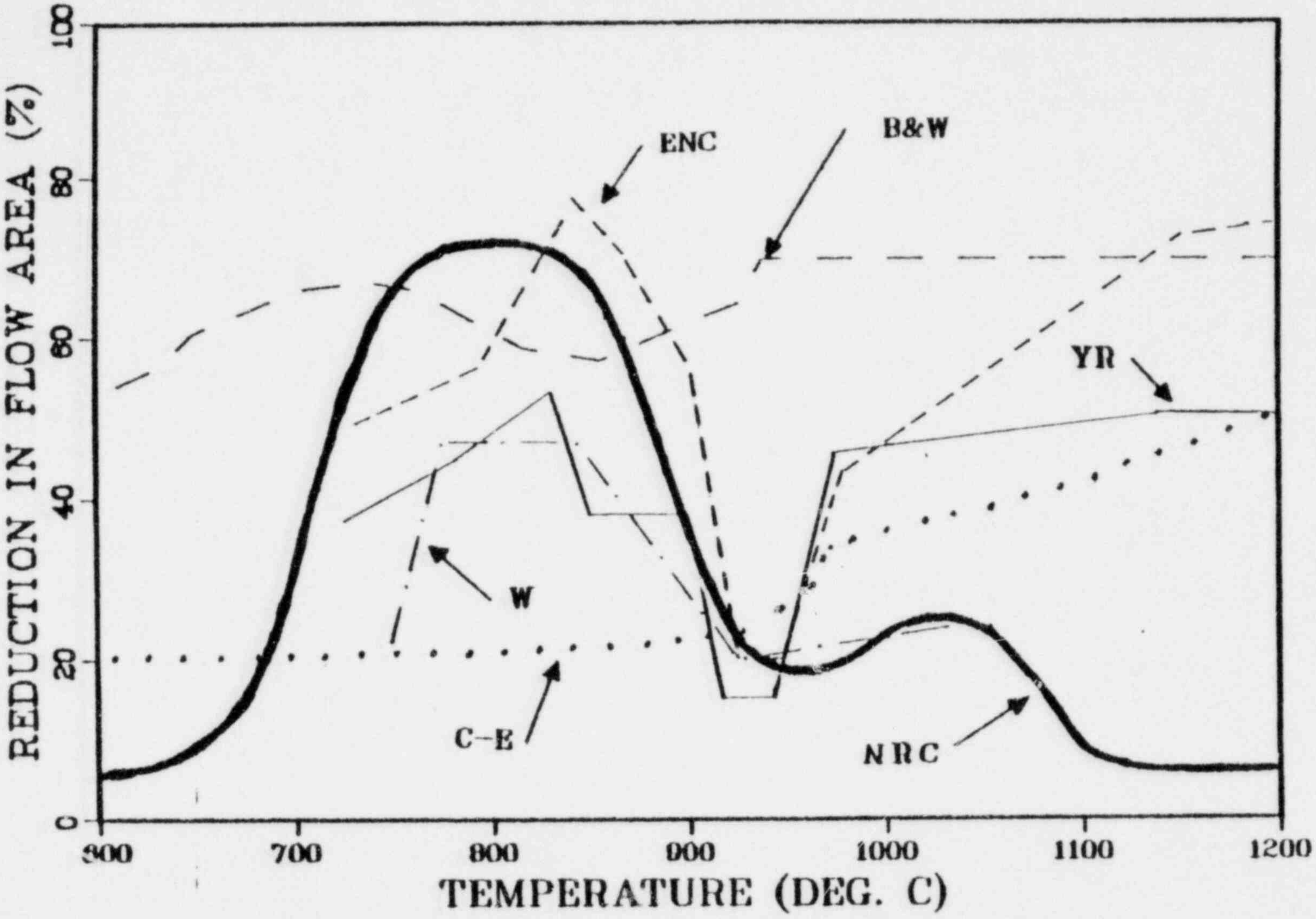
RUPTURE TEMPERATURE MODEL COMPARISONS



BURST STRAIN MODEL COMPARISONS



FLOW BLOCKAGE MODEL COMPARISONS



#11

NEED FOR ECCS CLADDING MODEL REVISIONS

NO JUSTIFICATION FOR MAJOR VARIATIONS FROM VENDOR TO VENDOR.

APPENDIX K REQUIRES THAT CLADDING MODELS NOT UNDERESTIMATE BASED ON APPLICABLE EXPERIMENTAL DATA.

SUCH CHANGES PRODUCE HUNDREDS OF DEGREES INCREASE IN PCT--FAR IN EXCESS OF THE 20°F TOLERANCE OF APPENDIX K.

#12

REASONS FOR MAKING CHANGES NOW

WE HAVE LEARNED ENOUGH IN THE PAST 5 YEARS TO KNOW THAT THE PRESENT VENDOR MODELS ARE NOT VERY GOOD.

IT WILL BE ANOTHER 5 YEARS BEFORE SUBSTANTIAL NEW GAINS ARE MADE FROM RESEARCH.

ROUGH CALCULATIONS SHOULD NOT BE ACCEPTED AS A LONG-TERM BASIS FOR ECCS ADEQUACY.

NEAR-TERM APPROVALS ARE ALREADY NEEDED FOR SEVERAL VENDOR MODELS IN THIS AREA.

Tap 5

COMMENTS ON NUREG-0630 MODELLING OF FLOW BLOCKAGE IN FUEL BUNDLES

M. L. PICKLESIMER
FUEL BEHAVIOR RESEARCH BRANCH, RES

PRESENTATION TO THE ACRS SUBCOMMITTEE ON REACTOR FUELS
SEPTEMBER 3, 1980

COMMENTS ON NURGE-0630 MODELLING OF FLOW BLOCKAGE

1. THE AVERAGE ROD STRAIN IN A CROSS-SECTION OF A BUNDLE CAN BE USED TO CALCULATE THE PRESSURE DROPS MEASURED THERE IN FLOW TESTS.

ORNL/NUREG/TM 3 2

Table 23. Percent circumferential strain in the tubes of the B-2 test

Elevation (cm)	Tube No. 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	$\bar{\varepsilon}_i$	σ_n
0.0	-0.2	-0.2	-0.3	0.3	0.0	-0.4	-0.1	0.3	-0.8	-0.4	-0.4	0.1	-0.9	-0.5	-0.4	0.0		
1.8	4.4	5.7	6.0	5.8	4.1	7.1	7.4	6.9	4.6	5.1	5.1	7.2	4.1	6.3	4.8	4.8		
3.4	10.5	11.3	12.3	10.6	10.5	18.0	14.1	11.6	8.2	10.6	10.9	13.0	8.9	15.0	9.6	7.7		
5.0	10.4	11.5	11.0	11.5	9.9	17.1	12.8	11.1	8.5	10.7	11.0	11.2	9.3	13.2	10.4	8.3		
6.5	8.5	9.7	6.0	7.8	7.6	13.5	10.4	7.5	7.0	9.0	9.0	8.5	7.4	9.8	7.5	6.6		
8.0	4.6	4.9	3.9	4.1	3.9	5.0	4.4	4.2	3.1	3.1	3.9	3.9	3.5	3.7	3.1	3.1		
11.5	3.7	3.9	3.9	4.4	3.7	5.6	4.3	4.5	3.4	3.7	4.8	4.6	3.5	4.2	2.9	5.3		
13.5	8.6	7.7	9.2	7.9	6.7	24.7	12.1	9.8	9.3	9.4	13.2	10.5	8.7	10.3	5.1	6.5	9.3	2.4
15.1	13.3	11.4	11.5	9.5	8.5	20.9	15.1	13.2	10.7	12.4	16.8	15.6	11.6	17.3	6.8	7.1	12.6	3.7
16.8	15.3	16.3	14.6	11.6	11.7	22.9	15.7	16.2	10.9	16.2	18.3	17.5	14.4	20.4	11.2	10.5	15.7	4.5
18.1	15.8	18.2	17.6	13.0	14.3	23.2	15.6	18.7	11.0	16.5	16.9	17.9	15.7	37.8	12.3	11.7	17.3	6.1
19.5	15.2	19.5	19.7	13.7	18.7	22.4	17.9	21.1	4.7	21.5	16.6	20.7	20.9	41.8	12.6	12.9	19.6	6.5
21.4	15.8	17.5	19.9	10.8	18.0	21.6	17.9	22.8	13.8	22.6	15.2	24.7	19.5	37.4	11.1	11.8	18.7	5.4
23.2	15.5	16.1	21.4	9.7	18.3	24.7	18.4	27.0	14.9	23.3	15.1	21.8	18.5	25.8	13.1	13.3	18.8	4.8
25.0	14.2	13.8	24.4	11.0	17.4	23.4	21.0	27.4	15.4	21.0	13.5	18.5	16.0	22.3	15.5	14.7	18.2	4.4
26.9	15.4	11.5	25.6	13.5	14.6	19.8	23.4	20.7	14.5	21.1	13.5	18.7	14.9	15.7	17.3	11.8	17.0	4.1
28.5	16.5	12.7	29.8	16.7	15.1	21.0	24.5	17.4	14.8	23.8	14.3	19.5	14.8	15.5	18.4	10.7	17.8	4.8
30.0	14.7	12.4	32.5	18.6	14.9	21.5	23.3	16.1	13.2	23.3	15.1	18.8	12.4	15.8	17.5	11.7	17.6	5.3
32.0	12.6	15.3	20.0	19.8	14.7	20.8	21.8	16.2	13.3	21.0	14.4	20.2	12.8	17.3	13.7	13.5	17.2	4.2
34.0	11.6	15.4	22.0	17.7	14.8	23.8	19.4	15.1	13.8	18.6	14.8	20.7	16.1	17.7	12.7	13.2	16.7	3.4
35.9	12.9	18.0	21.6	14.0	15.3	44.3	17.4	16.1	13.5	20.1	16.7	18.1	16.5	22.7	13.9	11.7	18.3	7.4
37.7	13.5	18.7	24.0	13.5	17.9	57.8	15.7	20.7	15.0	25.0	16.2	15.5	14.6	30.1	16.2	12.7	20.4	10.7
39.5	12.2	15.0	20.1	13.8	15.6	39.6	18.4	23.4	13.5	32.6	14.4	14.2	12.3	28.9	16.0	13.3	19.0	7.9
41.2	18.8	15.7	23.7	15.6	16.4	35.3	23.1	29.6	16.2	42.8	16.9	18.9	17.5	26.8	17.3	16.9	22.0	7.7
43.3	22.2	15.8	29.5	17.0	15.6	44.3	19.7	37.6	14.2	33.0	16.8	21.0	17.1	21.3	15.3	18.3	22.4	8.7
44.7	23.4	15.0	37.7	18.3	14.9	40.7	17.3	33.0	14.4	27.3	16.0	18.4	15.9	18.9	12.9	21.6	21.6	8.4
46.2	27.8	17.0	38.4	19.7	16.0	36.4	17.2	23.3	17.0	23.2	16.1	18.1	14.7	17.9	11.8	29.0	21.5	7.5
47.7	34.5	18.9	30.8	19.8	19.0	37.3	20.1	18.2	21.1	24.5	17.7	18.8	16.0	18.3	13.5	30.8	22.5	6.8
49.7	28.5	19.6	21.5	18.9	20.7	34.9	20.6	17.4	19.0	22.7	19.9	20.4	14.3	19.2	16.5	22.2	21.0	4.7
51.6	21.0	20.0	14.0	23.4	27.1	28.9	17.3	18.4	17.7	20.5	18.5	22.1	14.5	20.8	17.4	19.6	20.2	3.7
53.5	18.1	19.7	19.3	29.4	35.0	25.9	20.1	17.5	19.5	21.8	18.3	25.7	22.0	23.5	20.6	28.2	22.8	4.7
54.9	15.1	21.9	19.2	27.2	29.0	26.0	22.6	17.6	18.1	22.6	17.6	27.1	29.9	24.2	20.5	37.1	23.7	5.2
56.2	20.7	25.3	21.3	27.2	25.2	25.6	23.6	22.2	17.6	24.8	17.7	28.1	41.5	24.7	23.2	41.7	25.8	6.7
57.6	17.1	22.3	21.4	28.1	21.5	21.3	19.3	24.1	14.7	21.6	13.6	23.4	34.6	25.2	20.4	32.4	22.6	5.5
59.9	12.5	15.7	19.7	22.7	15.0	17.7	16.9	16.8	12.9	17.3	10.9	17.4	26.1	18.2	14.1	17.0	16.3	2.8
61.8	11.0	10.3	12.4	12.7	10.5	13.6	14.8	11.8	11.2	13.6	10.5	14.0	15.1	12.1	10.5	10.8		
63.8	5.1	5.3	4.9	6.1	5.1	5.5	5.7	6.0	3.7	5.4	4.3	5.8	6.4	5.7	5.0	6.4		
66.5	4.8	4.9	5.2	6.5	4.7	5.8	6.3	5.9	4.1	5.1	5.5	5.2	5.5	5.0	4.0	5.1		
68.4	10.2	10.8	13.6	10.8	11.0	17.7	18.1	11.6	12.4	13.7	15.0	11.0	10.9	10.8	8.2	9.3		
70.1	14.6	18.3	19.7	15.9	16.0	27.0	23.1	16.7	17.0	17.8	20.7	16.8	9.7	19.1	12.6	12.6		
71.6	15.0	22.2	21.3	21.5	20.6	27.0	21.8	17.8	19.4	18.1	20.2	16.6	9.7	25.8	12.7	14.3		
73.1	16.1	29.1	24.8	30.5	22.0	25.6	23.1	19.3	23.0	18.1	20.9	17.7	10.8	26.7	13.5	17.6		
74.6	21.7	33.0	37.4	37.8	23.3	30.6	27.7	22.1	29.2	22.2	26.4	19.7	16.5	24.8	17.0	20.1		
76.2	23.2	38.5	40.3	42.0	22.0	35.4	31.9	22.3	23.9	25.3	34.4	23.2	17.3	22.7	22.2	22.5		
78.0	24.5	29.0	35.6	33.2	23.6	28.8	34.2	21.9	20.2	27.6	39.8	26.3	17.3	23.1	31.4	23.1		
79.5	22.9	20.7	22.4	27.1	23.9	22.5	39.7	21.6	19.6	28.0	35.2	29.4	16.7	22.2	35.1	22.0		
81.6	17.3	17.9	16.8	26.5	19.0	23.8	55.6	16.3	19.1	28.3	25.9	38.7	14.3	18.7	25.2	16.2		
83.8	11.4	15.9	15.6	18.9	14.1	24.0	32.7	12.8	16.1	21.1	20.1	21.8	10.6	16.1	14.4	11.6		
86.0	8.0	13.8	12.7	12.9	15.4	14.8	20.7	11.5	13.9	23.0	14.6	11.8	7.5	14.3	9.8	10.8		
88.1	4.5	8.1	4.3	7.5	10.3	9.2	12.2	8.4	7.8	12.1	7.8	7.1	5.2	9.7	6.8	8.4		
89.9	1.2	1.6	1.7	3.0	1.9	1.9	2.4	1.2	1.5	3.5	0.7	3.3	-0.1	2.4	0.3	1.1		
91.5	0.4	0.4	0.8	1.2	1.1	0.1	1.0	-0.1	0.5	1.7	0.5	0.9	0.1	1.5	0.4	0.5		
																	19.2	3.4
σ_n	5.6	3.7	6.8	5.9	5.6	9.9	3.0	6.0	2.7	6.2	2.0	3.7	6.9	7.0	3.9	9.0		3.5
$\bar{\varepsilon}_i$	17.9	16.6	22.9	17.3	17.6	28.5	19.1	20.6	14.8	22.5	15.9	19.8	17.6	22.9	14.9	17.9	19.2	

3

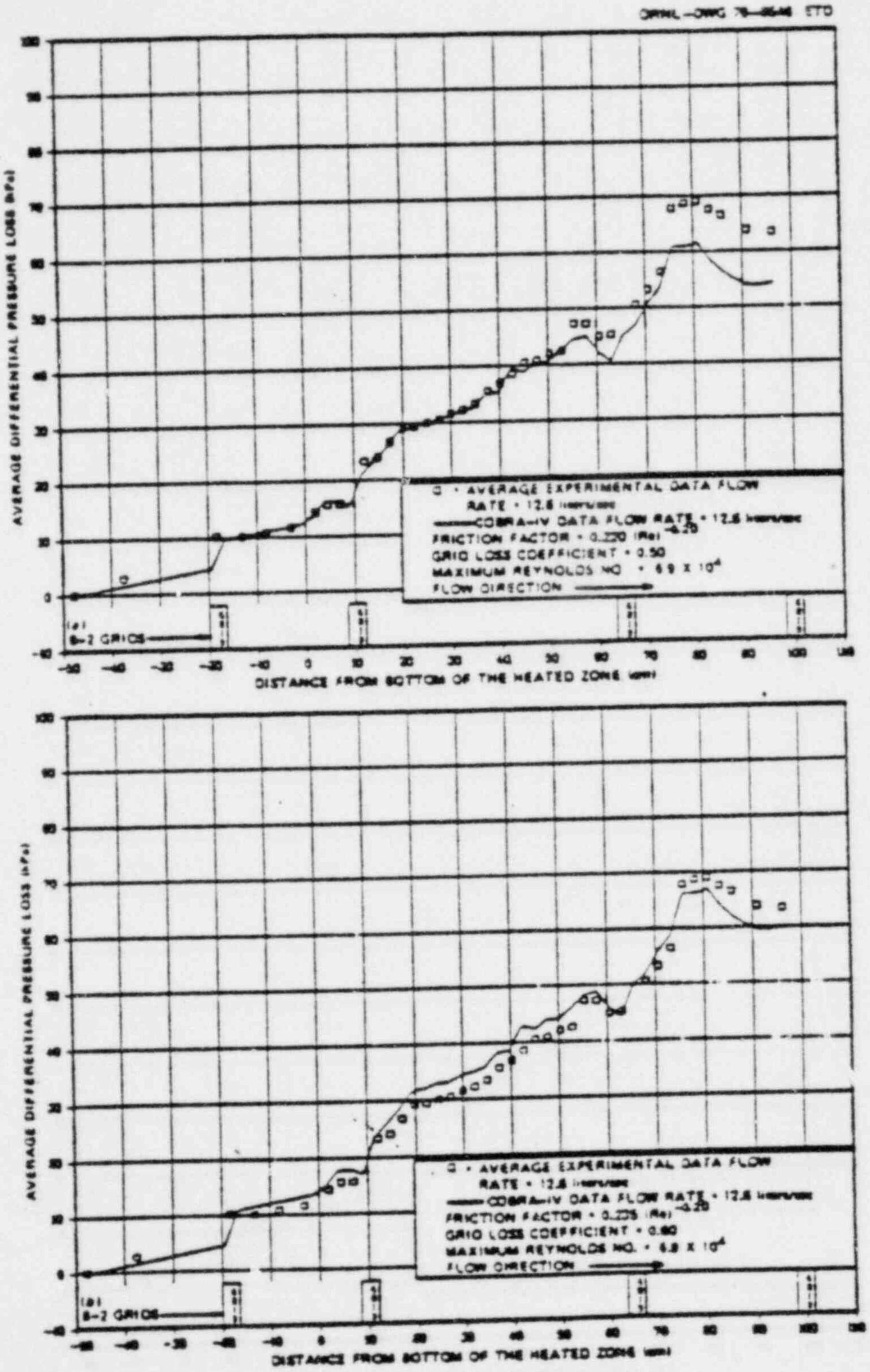
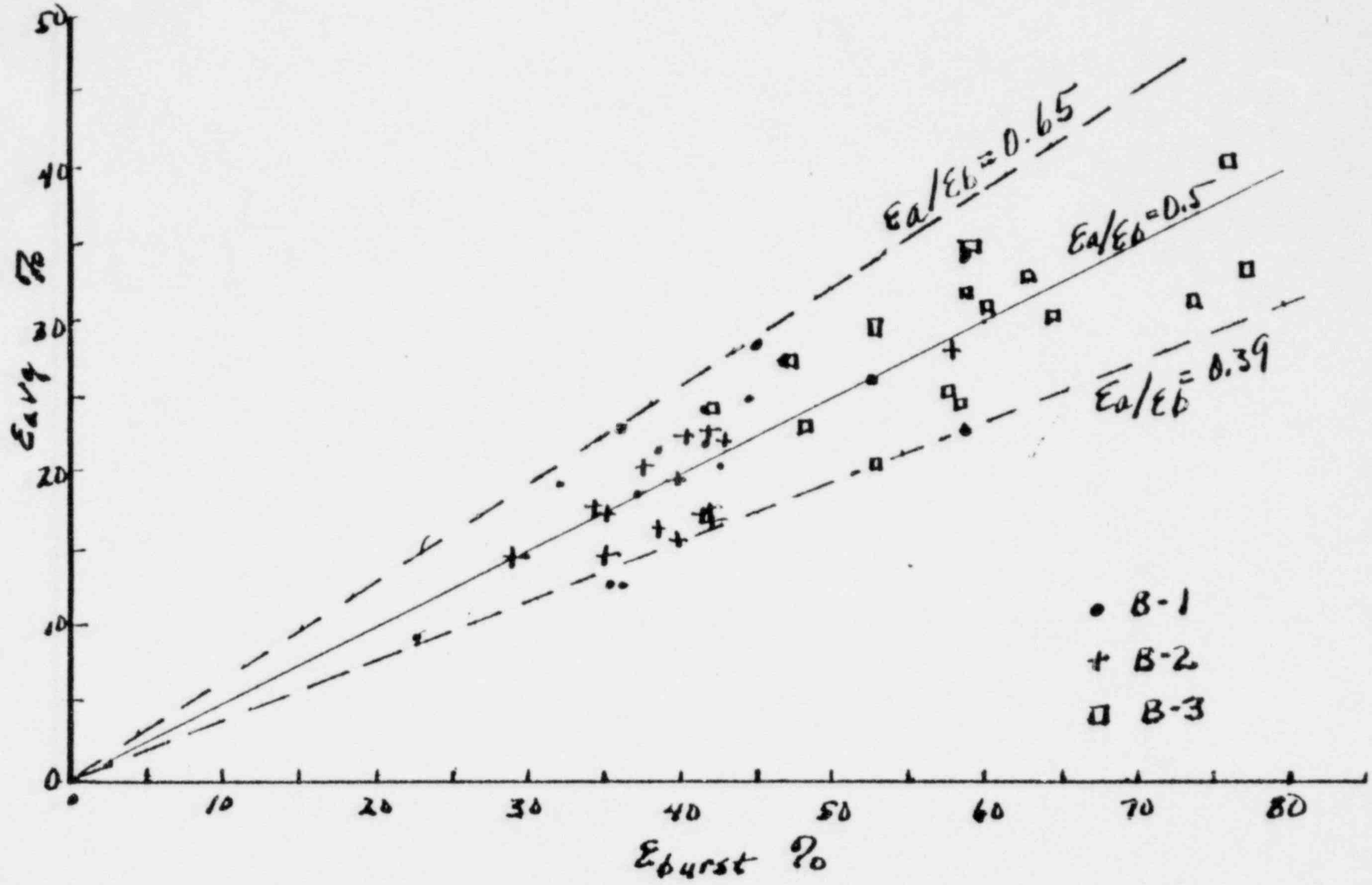


Fig. 5.32. Comparison of B-2/shroud 1 experimental and COBRA-IV axial pressure loss profiles; experimental flow rate = 12.6 liters/sec; maximum restriction definition. (a) Lower-limit; (b) upper-limit correlation values.

W

2. BURST STRAINS ARE NOT PHYSICALLY RELATED TO AVERAGE RODS STRAINS NOR TO THE LOSS OF FLOW AREA IN BUNDLES.



AVERAGE ROD STRAIN VERSUS BURST STRAIN FOR MRBT BUNDLES B-1, B-2, & B-3

6

Percent Circumferential Strains in MR 1-2 with Burst Strains Replaced

Elevation (cm)	Tube No. 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
0.0	-0.2	-0.2	-0.3	0.3	0.0	-0.4	-0.1	0.3	-0.8	-0.6	-0.4	0.1	-0.9	-0.5	-0.4	0.0
1.6	4.4	5.7	6.0	5.8	4.1	7.1	7.4	6.9	4.6	5.1	5.1	7.2	4.1	6.3	4.8	4.8
3.4	10.5	11.3	12.3	10.6	10.5	18.0	14.1	11.6	8.2	10.6	10.9	13.0	8.9	15.0	9.6	7.7
5.0	10.4	11.5	11.0	11.5	9.9	17.1	12.8	11.1	8.5	10.7	11.0	11.2	9.3	13.2	10.4	8.3
6.5	8.5	9.7	8.[illegible]	7.8	7.6	13.5	10.4	7.5	7.0	9.0	9.0	8.5	7.4	9.8	7.5	6.6
8.0	4.6	4.9	3.9	4.1	3.9	5.0	4.4	4.2	3.1	3.1	3.9	3.9	3.5	3.7	3.1	3.1
11.5	3.1	3.9	3.9	4.4	3.7	5.6	4.3	4.5	3.4	3.7	4.8	4.6	3.5	4.2	2.9	4.3
13.5	8.6	7.7	6.2	7.9	6.7	14.7	12.1	9.8	9.3	9.4	13.2	10.5	8.2	10.3	5.1	4.5
15.1	13.3	11.4	11.5	9.5	8.5	20.9	15.1	13.2	10.7	12.4	16.8	15.6	11.6	17.3	6.8	7.1
16.8	15.3	16.3	14.6	11.6	11.7	22.9	15.7	16.2	10.9	16.2	18.3	17.5	14.4	20.4	11.2	10.5
18.1	15.8	18.2	17.6	13.0	14.3	23.2	15.6	18.7	11.0	16.5	16.9	17.9	15.7	37.8	12.3	11.7
19.5	15.2	19.5	19.7	13.7	18.7	22.4	17.9	21.1	14.7	21.5	16.6	20.7	20.9	32.8	12.6	12.9
21.4	16.8	17.5	19.9	10.8	18.0	21.6	17.9	22.8	13.8	22.6	15.2	24.7	19.5	~~32.4~~	11.1	11.8
23.2	15.5	16.3	21.4	9.7	18.3	24.7	18.4	27.0	14.9	23.3	15.1	21.8	18.5	25.8	13.1	13.3
25.0	16.2	13.8	24.4	11.0	17.4	23.4	21.0	27.4	15.4	21.0	13.5	18.5	16.0	22.3	15.5	14.7
26.5	15.4	11.5	25.9	13.5	14.6	19.8	23.4	20.7	14.5	21.1	13.5	18.7	14.9	15.7	17.3	11.8
28.5	16.5	12.7	29.8	16.7	15.1	21.0	24.5	17.4	14.8	23.8	14.3	19.5	14.0	15.5	18.4	10.7
30.0	14.7	12.4	32.5	18.6	14.9	21.5	23.3	16.1	13.2	23.3	15.1	18.8	12.4	15.8	17.5	11.7
32.0	12.6	15.3	28.0	19.8	14.7	20.8	21.8	16.2	13.3	21.0	14.4	20.2	12.8	17.3	13.7	13.5
34.0	11.4	15.4	22.0	17.7	14.0	23.8	19.4	15.1	12.8	18.4	14.8	20.7	16.1	17.7	12.7	13.2
35.9	12.9	18.0	21.6	14.0	15.3	44.3	17.4	14.1	13.5	20.1	16.7	18.1	16.5	22.7	13.9	11.7
37.7	13.5	18.7	24.0	13.5	17.9	44.3	15.7	20.7	15.0	25.0	14.2	15.5	14.6	30.1	16.2	12.7
39.5	12.2	15.0	20.1	13.8	15.6	~~39.6~~	18.4	23.4	13.5	32.6	14.4	14.2	12.3	28.9	16.0	13.3
41.2	18.0	15.7	23.7	15.6	16.4	35.3	23.1	29.6	16.2	31.0	16.9	18.9	17.5	26.8	17.3	16.9
43.3	22.2	15.8	29.5	17.0	15.6	44.3	19.7	31.0	14.2	~~33.0~~	16.8	21.0	17.1	21.3	15.3	18.3
44.7	23.4	15.0	37.3	18.3	14.9	40.7	17.3	~~33.0~~	14.4	27.3	16.0	18.6	15.9	18.9	12.9	21.6
46.2	21.8	17.0	30.4	19.7	16.0	36.4	17.2	23.3	17.0	23.2	16.1	18.1	16.7	17.9	11.8	29.0
47.7	20.9	18.9	30.8	19.8	19.0	37.3	20.1	18.2	21.1	24.5	17.7	18.8	16.0	18.3	13.5	30.8
49.1	~~18.5~~	19.6	21.5	10.9	20.7	34.9	20.4	17.4	19.0	22.2	19.9	20.4	14.3	19.2	16.5	22.2
51.6	21.0	20.0	14.0	23.4	23.1	28.9	17.3	18.4	17.7	20.5	18.5	22.1	14.5	20.8	17.4	19.6
53.5	18.1	19.7	19.3	29.4	24.0	25.9	20.1	17.5	19.5	21.8	18.3	25.7	22.0	23.5	20.6	28.2
54.9	15.1	21.9	19.2	27.2	~~26.0~~	26.0	22.6	17.6	10.1	22.4	17.6	27.1	29.9	24.2	20.5	37.1
56.2	26.7	25.3	21.3	27.2	25.2	25.6	23.6	22.2	17.6	24.8	17.7	28.1	34.6	21.7	23.2	37.1
57.6	17.1	22.3	21.4	28.1	21.5	21.3	19.5	24.1	14.7	21.6	13.6	23.4	~~36.6~~	25.2	20.4	~~32.4~~
59.8	12.4	14.7	18.7	23.7	15.0	17.7	16.9	16.0	12.9	17.3	10.9	17.4	16.1	18.2	14.1	17.0
61.8	11.8	10.3	12.4	12.7	10.5	13.6	14.8	11.8	11.7	13.6	10.5	14.0	13.1	12.1	10.5	10.8
63.8	5.1	5.3	4.9	6.1	5.1	5.5	5.7	6.0	3.7	5.4	5.3	5.8	6.4	5.7	5.0	6.4
64.5	4.8	4.9	5.2	6.5	4.7	5.8	6.3	5.9	4.1	5.1	5.5	5.2	5.5	5.0	4.0	5.1
68.4	10.2	10.8	13.6	10.8	11.0	17.7	18.1	11.6	12.4	15.7	15.0	11.0	10.9	10.8	8.2	9.3
70.1	14.6	18.3	19.7	15.9	16.0	21.0	23.1	16.7	17.0	17.8	20.7	16.8	9.7	19.1	12.6	12.6
71.6	15.0	22.2	21.3	21.5	20.6	27.0	21.8	17.8	19.4	18.1	20.2	16.6	9.7	25.8	12.7	14.3
73.1	16.1	27.1	24.8	30.5	22.0	25.6	23.1	19.3	23.0	18.7	20.9	17.7	10.8	26.7	13.5	17.6
74.6	21.5	33.0	37.4	37.8	25.3	30.6	27.7	22.1	29.2	22.2	26.4	19.7	16.5	24.8	17.0	20.1
76.2	23.2	38.5	40.3	42.0	22.0	35.4	31.9	22.3	23.9	25.3	34.4	23.2	17.3	22.7	22.2	22.5
78.0	24.3	29.0	33.6	33.2	23.6	28.8	34.2	21.9	20.2	27.6	39.8	26.3	17.3	23.1	31.4	23.1
79.5	22.9	20.7	22.4	27.1	23.9	22.5	39.7	21.6	19.6	28.0	35.2	29.4	16.7	22.2	35.6	22.0
81.6	13.3	17.9	16.8	26.5	19.0	23.8	35.6	16.3	19.1	20.3	25.9	32.7	14.3	18.7	25.2	16.2
83.8	11.4	15.9	15.6	18.9	14.1	24.0	32.7	12.8	16.1	21.1	20.1	21.8	10.6	16.1	14.4	11.6
86.0	8.0	13.8	12.7	12.9	15.4	14.8	20.7	11.5	13.9	23.0	14.6	11.8	7.5	14.3	9.8	10.8
88.1	6.5	8.1	6.3	7.5	10.3	9.2	12.2	8.4	7.8	12.1	7.8	7.1	5.2	9.7	6.8	8.4
89.5	1.2	1.6	1.7	3.0	1.9	1.9	2.4	1.2	1.5	3.3	0.7	3.3	-0.1	2.4	0.3	1.1
91.5	0.4	0.4	0.8	1.2	1.1	0.1	1.0	-0.1	0.5	1.1	0.5	0.9	0.1	1.5	0.4	0.5
σ_n	5.0	3.7	6.8	5.9	5.2	8.7	3.0	5.5	2.7	5.3	2.0	3.7	6.1	6.6	3.9	8.6
$\bar{\varepsilon}_c$	17.7	16.6	22.9	17.3	17.4	28.0	19.1	20.5	14.8	22.2	15.9	19.8	17.4	22.8	14.9	17.8

3.4	
19.0	

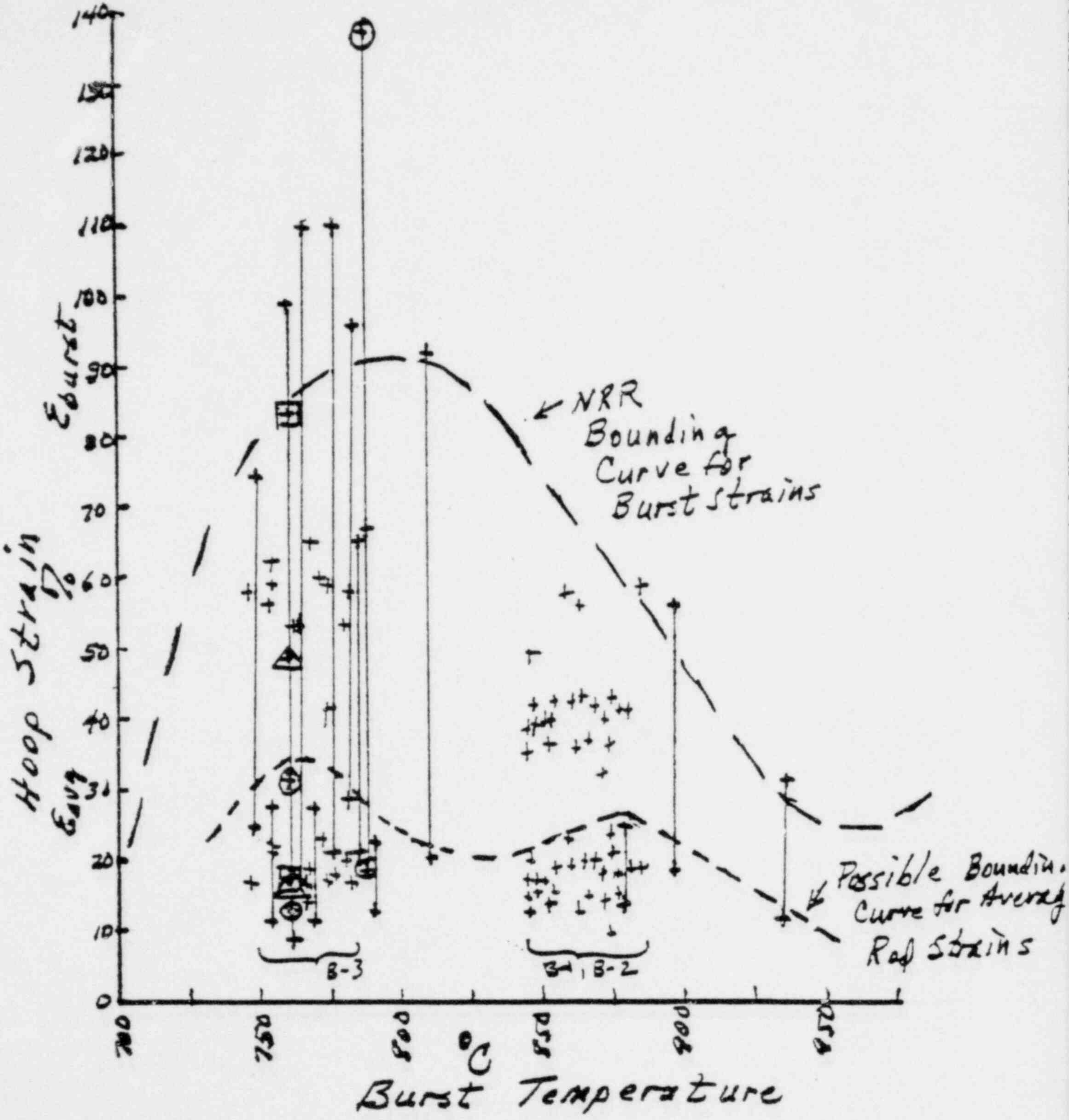
$\bar{\varepsilon}_i$	σ_n
9.4	2.4
12.6	3.7
15.7	4.5
17.3	6.1
19.4	5.7
18.7	5.4
18.8	4.8
18.2	4.4
17.0	4.1
17.8	4.8
17.6	5.3
17.2	4.2
16.7	3.4
18.3	7.4
19.6	7.9
19.0	7.9
21.4	6.3
22.1	8.2
21.9	8.4
21.5	7.5
22.1	6.3
21.0	4.7
20.2	3.7
22.4	3.9
23.7	5.2
25.1	5.1
22.6	5.5
16.3	2.8
19.0	3.3

7

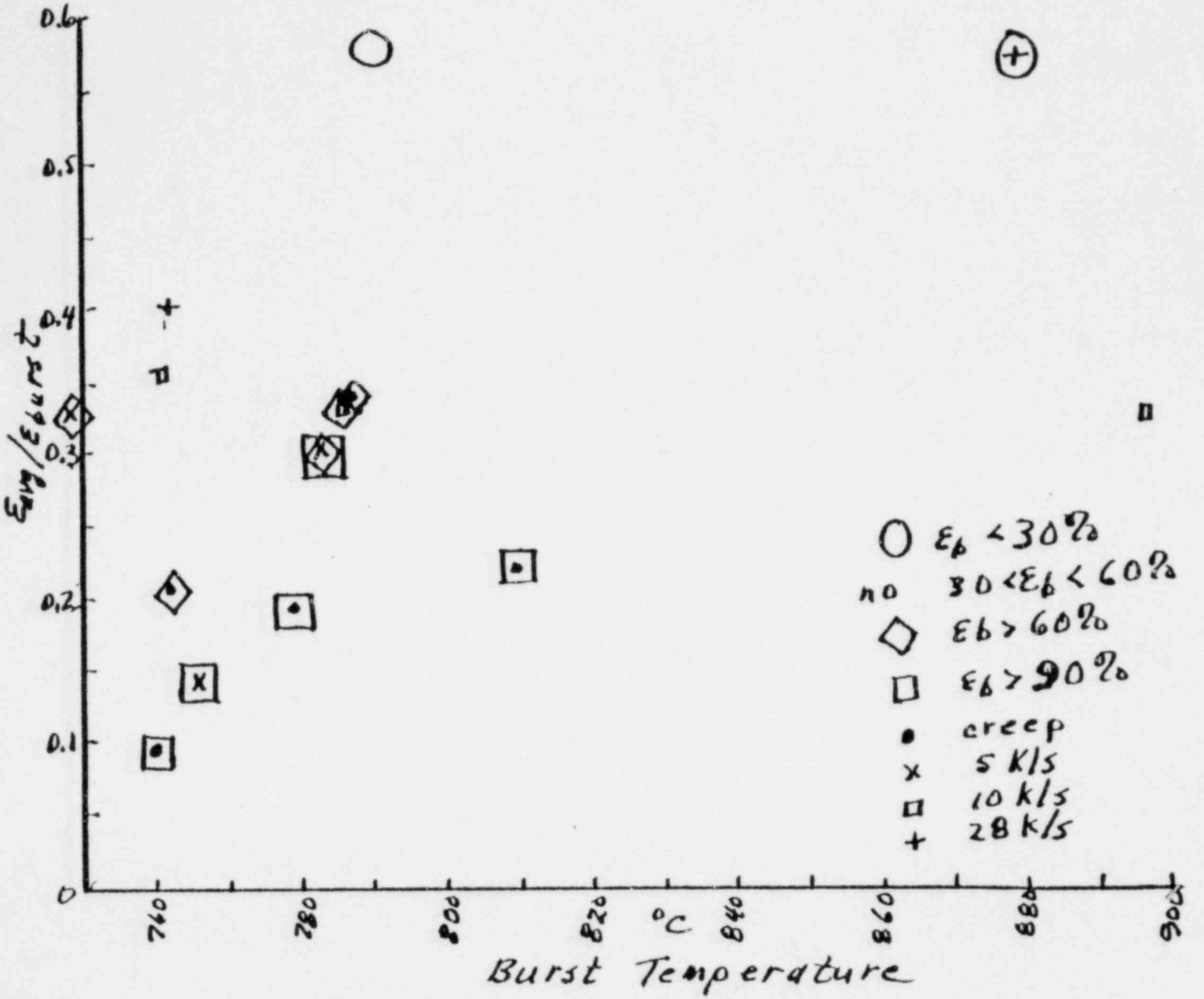
AVERAGE CIRCUMFERENTIAL ROD STRAINS

IN MRBT B-2 TRANSVERSE SECTIONS

WITH BURST STRAINS		WITHOUT BURST STRAINS	
$\bar{\varepsilon}_j$	σ_n	$\bar{\varepsilon}_j$	σ_n
9.3	2.5	9.4	2.4
12.6	3.7	12.6	3.7
15.7	4.5	15.7	4.5
17.3	6.1	17.3	6.1
19.6	6.5	19.4	5.7
18.7	5.4	18.7	5.4
18.8	4.8	18.8	4.8
18.2	4.4	18.2	4.4
17.0	4.1	17.0	4.1
17.8	4.8	17.8	4.8
17.6	5.3	17.6	5.3
17.2	4.2	17.2	4.2
16.7	3.4	16.7	3.4
18.3	7.4	18.3	7.4
20.4	10.7	19.6	7.9
19.0	7.9	19.0	7.9
22.0	7.7	21.4	6.3
22.4	8.7	22.1	8.2
21.6	8.4	21.6	8.4
21.5	7.5	21.5	7.5
22.5	6.8	22.1	6.3
21.0	4.7	21.0	4.7
20.2	3.7	20.2	3.7
22.8	4.7	22.4	3.9
23.7	5.2	23.7	5.2
25.8	6.7	25.1	5.1
22.6	5.5	22.6	5.5
16.3	2.8	16.3	2.8
19.2	3.4	19.0	3.3



AVERAGE ROD AND BURST STRAINS VERSUS BURST TEMPERATURE FOR ORNL MRBT AND SRBT TESTS



RATIO OF AVERAGE ROD STRAIN TO BURST STRAIN VERSUS BURST TEMPERATURE FOR INDIVIDUAL RODS

3. RELATIONSHIP BETWEEN BURST STRAINS AND AVERAGE ROD STRAINS ESTABLISHED AT ONE TEMPERATURE CAN NOT BE USED AT ANOTHER TEMPERATURE.

11

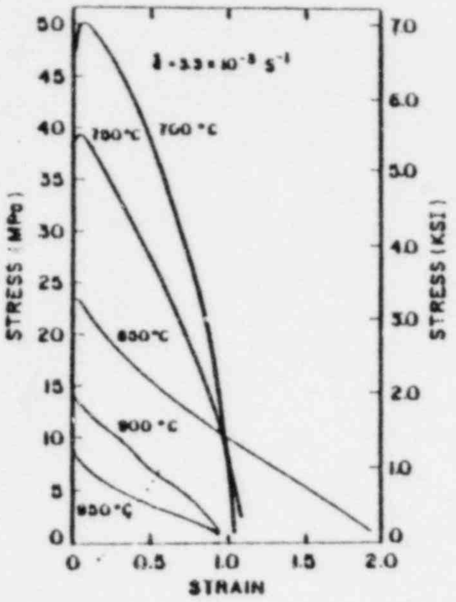


Fig. 16. Engineering-stress/Engineering-strain Curves for Zircaloy-4 Specimens Deformed at 700, 750, 850, 900, and 950°C. Neg. No. MSD-61494.

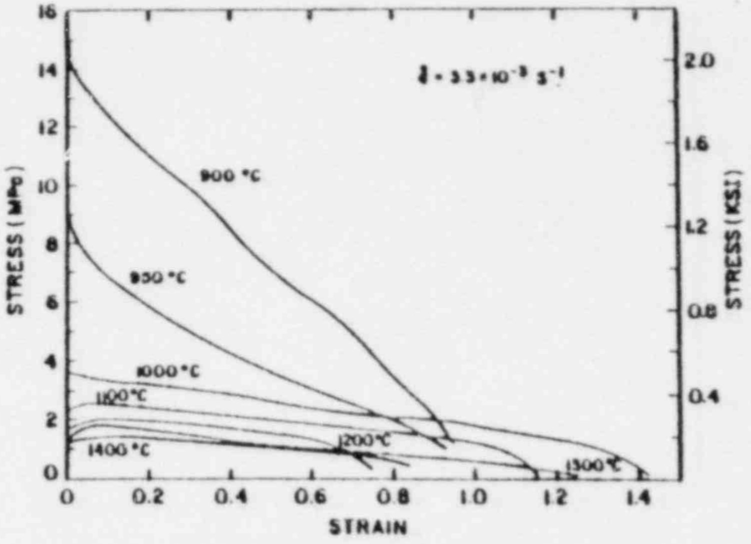


Fig. 17. Engineering-stress/Engineering-strain Curves for Zircaloy-4 Specimens Deformed at 900, 950, 1000, 1100, 1200, 1300, and 1400°C. Neg. No. MSD-61489.

From : UNIAXIAL TENSILE PROPERTIES OF ZIRCALOY CONTAINING OXYGEN: SUMMARY REPORT

ANL 77-30(June 1977) by A.M.Garde, H.M.Chung, and T.F.Kassner

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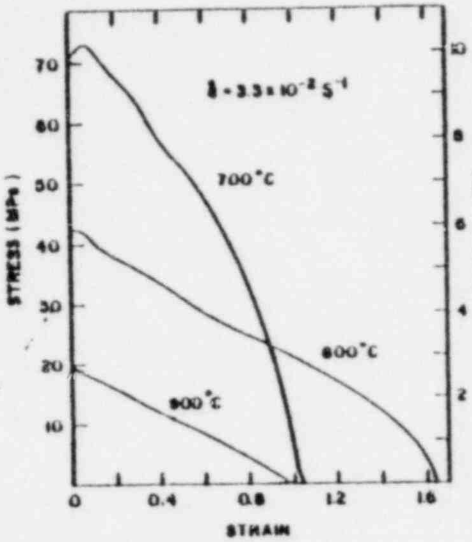


Fig. 18. Engineering-stress/Engineering-strain Curves for Zircaloy-4 Specimens Deformed at 700, 800, and 900°C. Neg. No. MSD-61497.

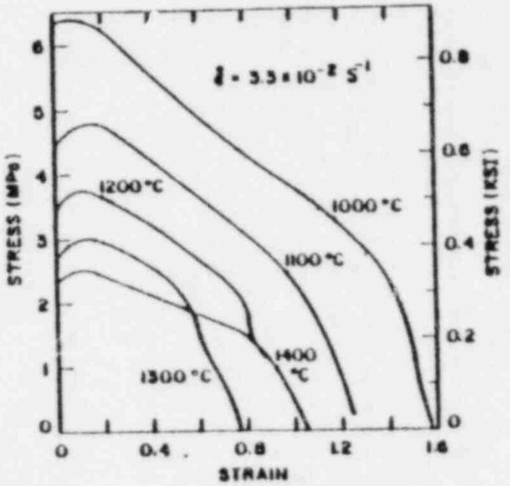
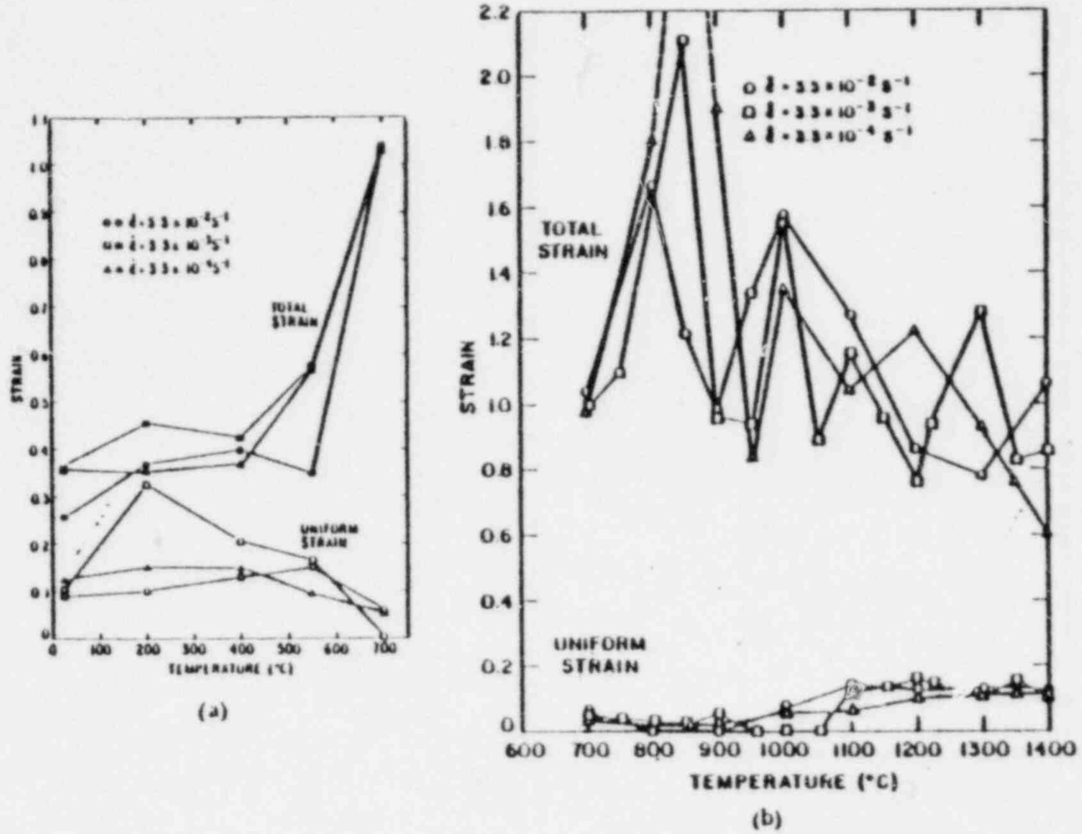


Fig. 19. Engineering-stress/Engineering-strain Curves for Zircaloy-4 Specimens Deformed at 1000, 1100, 1200, 1300, and 1400°C. Neg. No. MSD-61503.

From : UNIA'' TENSILE PROPERTIES OF ZIRCALOY CONTAINING OXYGEN:SUMMARY REPORT"
AN, -30(June 1977)by A.M.Garde, H.M.Chung, and T.F.Kassner



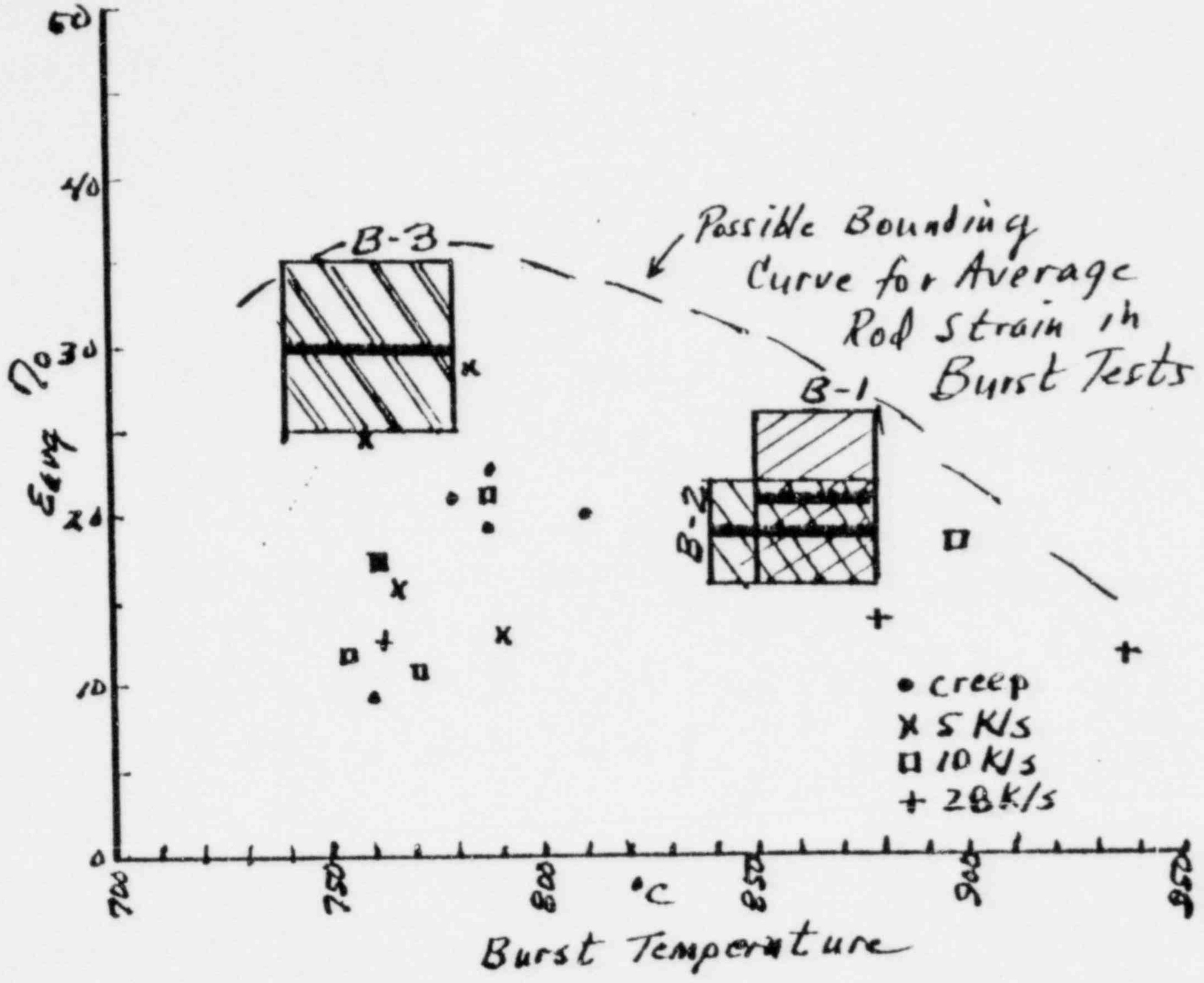
(a)

(b)

Fig. 21. Uniform Strain and Total Strain for Zircaloy-4 Specimens as a Function of Deformation Temperature at Three Strain Rates. (a) 23-700°C and (b) 700-1400°C. ANL Neg. Nos. 306-77-54 Rev. 1 and 306-75-199 Rev. 1.

From: UNIAXIAL TENSILE PROPERTIES OF ZIRCALOY CONTAINING OXYGEN: SUMMARY REPORT ANL 77-30 (June 1977) by A.M. Garde, H.M. Chung, and T.F. Kassner

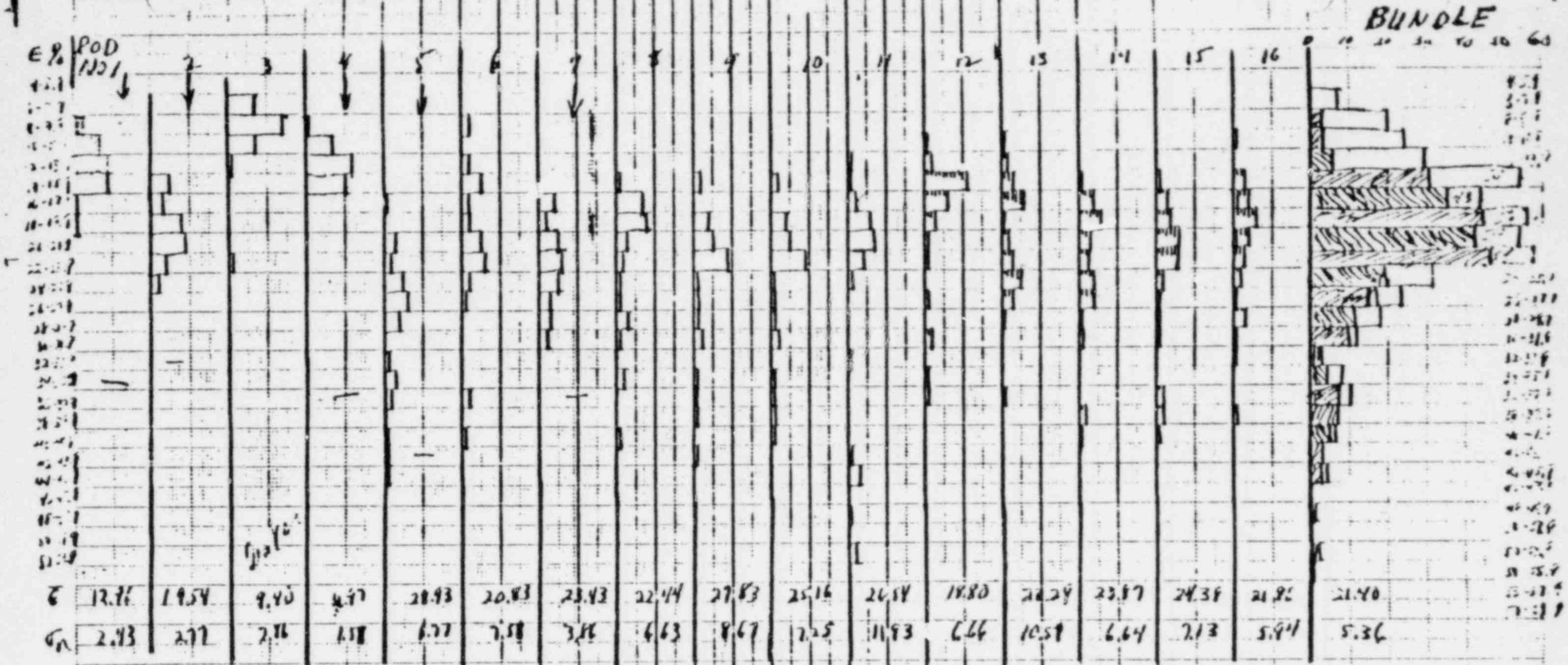
4. IF "STANDARD" FLOW BLOCKAGE CURVE MUST BE ESTABLISHED NOW FOR USE IN AUDITING VENDOR MODELS, THEN LET IT BE DEVELOPED FROM AVERAGE ROD STRAIN DATA DEVELOPED IN SUITABLE BURST TESTS. SINGLE ROD AVERAGE STRAIN DATA IS IN AT LEAST AS GOOD A SHAPE AS THE SINGLE ROD BURST DATA, AND IS DIRECTLY APPLICABLE TO FLOW BLOCKAGE CALCULATIONS (IF SINGLE ROD DATA CAN BE USED AT ALL).



AVERAGE ROD STRAIN VERSUS BURST TEMPERATURE FOR ORNL MRBT AND SRBT

5. STRAIN PROFILES OF INDIVIDUAL RODS IN 4 X 4 BUNDLES SHOW BUDNLE CHARACTERISTICS CAN NOT BE CALCULATED FROM THE STRAIN DATA FROM ANY ONE ROD IN THAT BUDNLE.

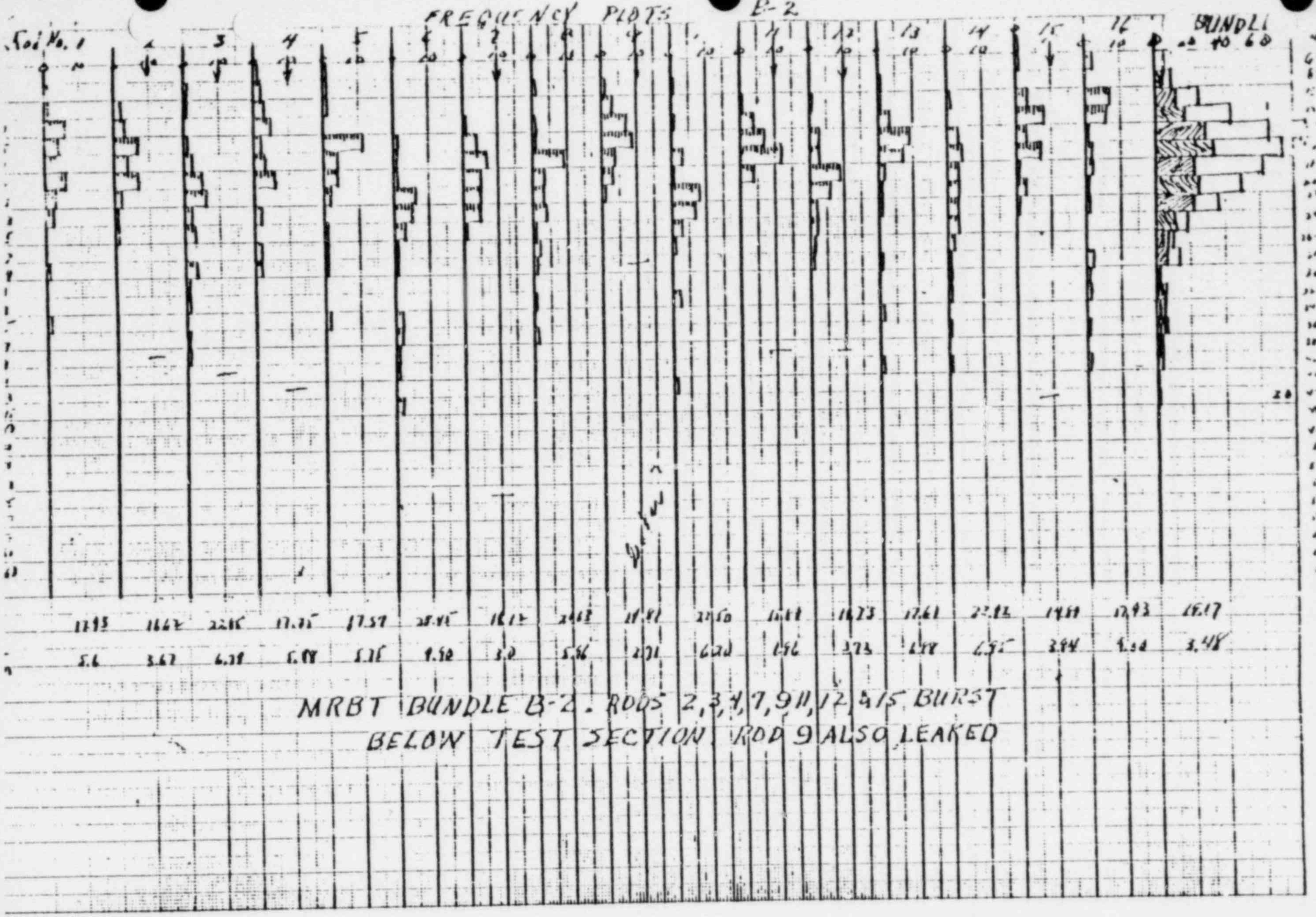
FREQUENCY PLOTS B-1



MRBT BUNDLE B-1. RODS 1,2,4,5,&7 BURST
BELOW TEST SECTION. ROD 3 LEAKED

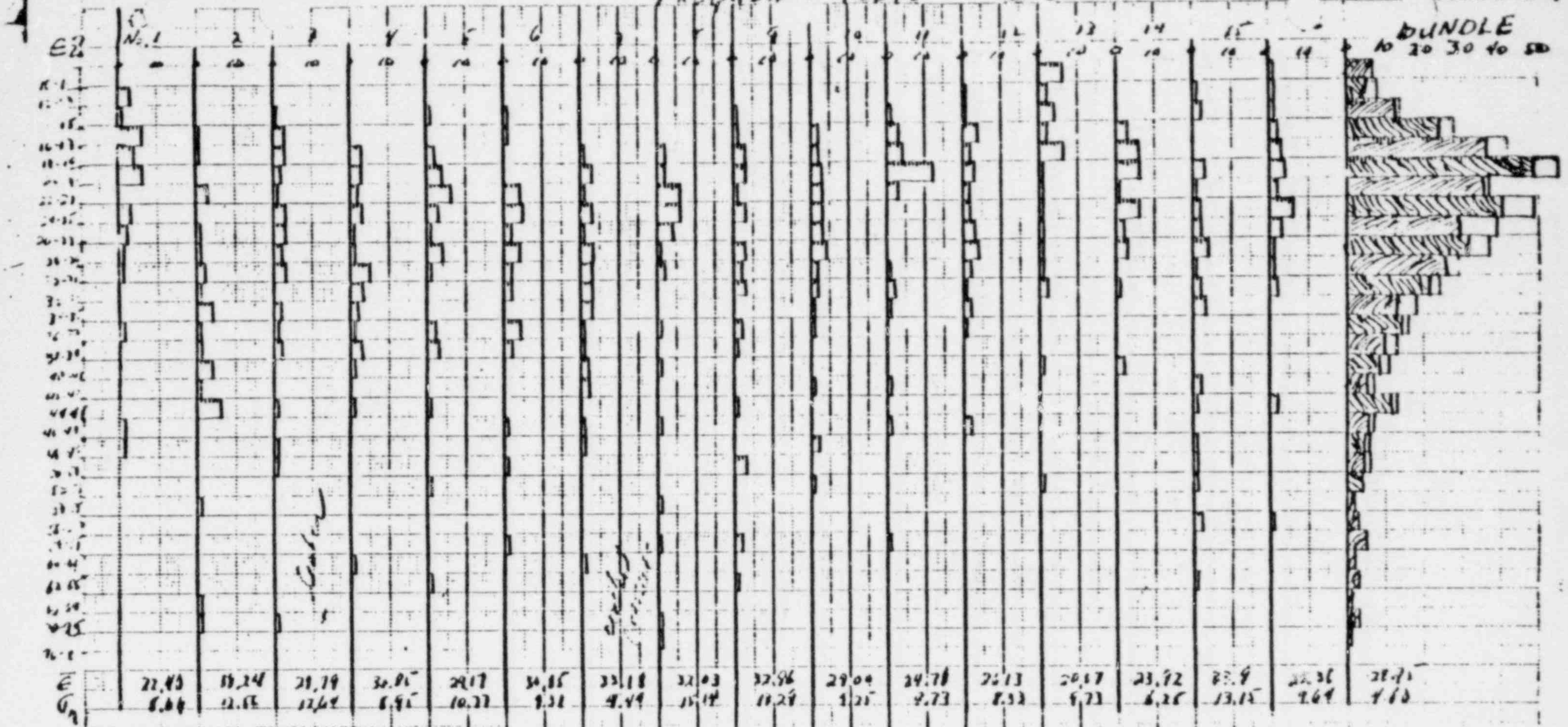
F18

FREQUENCY PLOTS B-2



MRBT BUNDLE B-2 - RODS 2,3,4,7,9,11,12 & 15 BURST
BELOW TEST SECTION. ROD 9 ALSO LEAKED

FREQUENCY PLOTS B-3



MRBT BUNDLE B-3. ALL RODS BURST WITHIN
TEST SECTION. ROD 3 LEAKED AND ROD
7 WAS TESTED AT CONSTANT PRESSURE

6. A STATISTICAL APPROACH MUST BE DEVELOPED FOR ESTIMATION OF FLOW BLOCKAGE IN BUNDLES. THREE RECENT ANALYTICAL DEVELOPMENTS ARE APPLICABLE TO THE PROBLEM:

   A. DEVELOPMENT BY ERBACHER, ET.AL., (KFK) OF A CALCULATIONAL PROCEDURE FOR DETERMINING TIME OF BURST AND THUS BURST STRAINS, USING CREEP EQUATION, OXIDATION KINETIC EQUATION, BURST STRESS CORRELATION, WITH INPUT HEATING RATE AND INITIAL PRESSURE.

   B. BALLOON-2 MODIFICATIONS BY HAGRMAN (EG&G) WILL ALLOW CALCULATION OF AXIAL STRAIN PROFILE OF BALLOONING ROD AT ANY TIME DURING TEMPERATURE RAMP, USING STATISTICAL VARIATIONS OF PELLET DIMENSIONS AND POWER, AXIAL, AND AZIMUTHAL TEMPERATURE GRADIENTS.

   C. DEVELOPMENT BY SENGPIEL AND BORGWALDT (KFK) OF PROBABILISTIC ANALYSIS OF ROD STRAIN AND FLOW BLOCKAGE IN A KWU 15 X 15 BUNDLE USING RESPONSE SURFACE METHODOLOGY AND STATISTICAL VARIATION OF ROD POWER, ROD GEOMETRY, AZIMUTHAL TEMPERATURE GRADIENTS, NEIGHBORING COLD RODS.

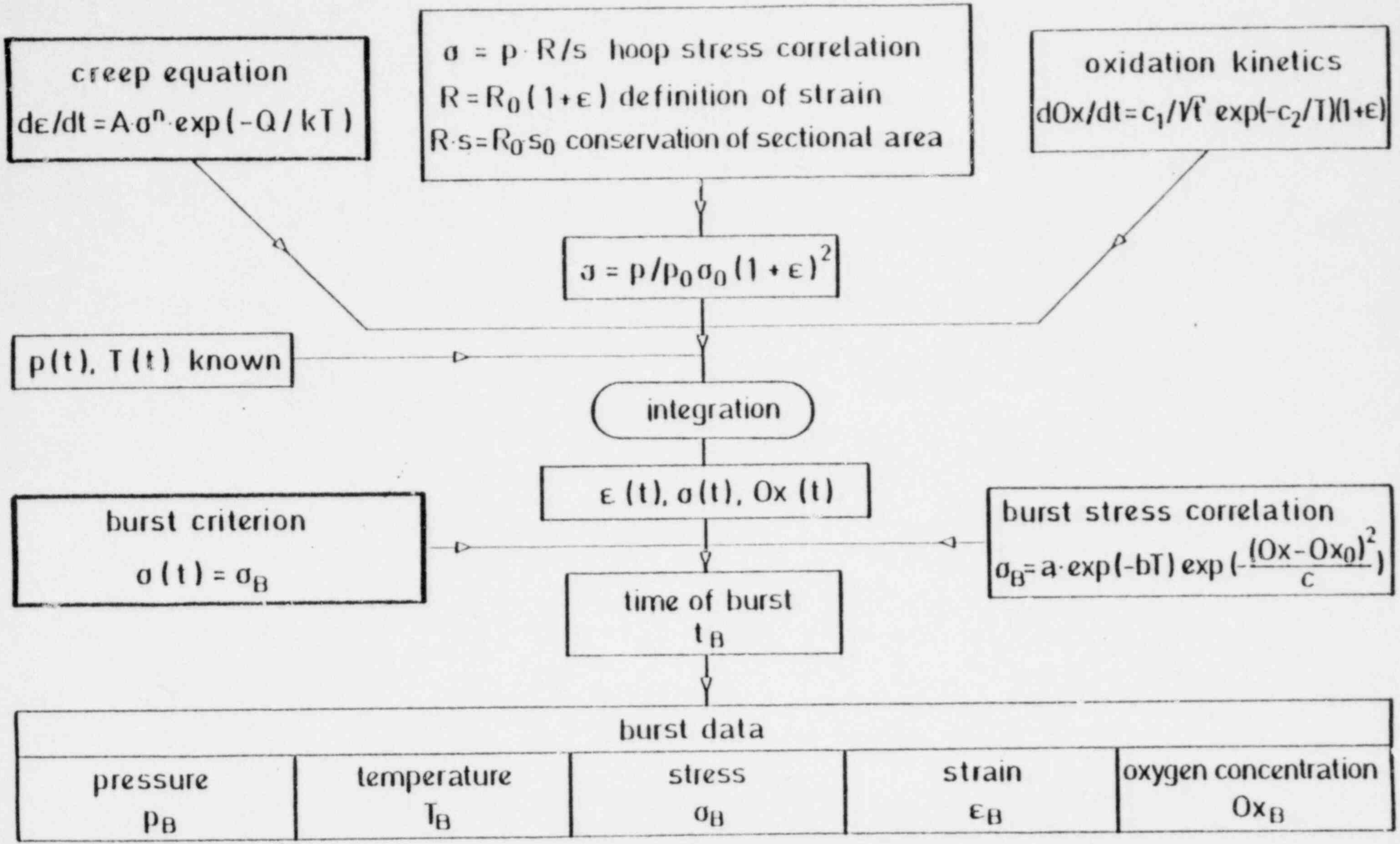
7. ERBACHER, ET.AL., (KFK) PRESENTED AT THE AUGUST 1980 ASTM SYMPOSIUM ON ZIRCONIUM IN THE NUCLEAR INDUSTRY A PAPER, "BURST CRITERION OF ZIRCALOY FUEL CLADDINGS IN A LOCA."

   A. THREE SIMPLE ASSUMPTIONS ARE MADE: (1) CROSS-SECTIONAL AREA OF DEFORMING ROD IS PRESERVED, (2) TIME OF BURST IS REACHED WHEN THE ACTUAL LOCAL STRESS EQUALS THE LIMITING BURST STRESS, (3) BURST STRESS IS A FUNCTION OF TEMPERATURE AND OXYGEN CONCENTRATION.

   B. TWO MATERIAL PROPERTY RELATIONSHIPS ARE KNOWN: (1) CREEP EQUATION OVER RANGE OF TEMPERATURE OF INTEREST, AND (2) EQUATION OF KINETICS OF OXIDATION.

   C. ORIGINAL ROD DIMENSIONS AND PRESSURIZATION ARE INPUT DATA.

   D. OUTPUTS ARE BURST TIME, TEMPERATURE, PRESSURE, STRESS, STRAIN, AND OXYGEN CONTENT.



creep equation
$d\varepsilon/dt = A \cdot \sigma^n \cdot \exp(-Q/kT)$

$\sigma = p \cdot R/s$ hoop stress correlation
$R = R_0(1+\varepsilon)$ definition of strain
$R \cdot s = R_0 \cdot s_0$ conservation of sectional area

oxidation kinetics
$dOx/dt = c_1/\sqrt{t}\ \exp(-c_2/T)(1+\varepsilon)$

$\sigma = p/p_0\, \sigma_0 (1+\varepsilon)^2$

p(t), T(t) known

integration

$\varepsilon(t), \sigma(t), Ox(t)$

burst criterion
$\sigma(t) = \sigma_B$

burst stress correlation
$\sigma_B = a \cdot \exp(-bT)\exp\left(-\frac{(Ox - Ox_0)^2}{c}\right)$

time of burst
t_B

burst data				
pressure p_B	temperature T_B	stress σ_B	strain ε_B	oxygen concentration Ox_B

Development of a Burst Criterion

Fig.1

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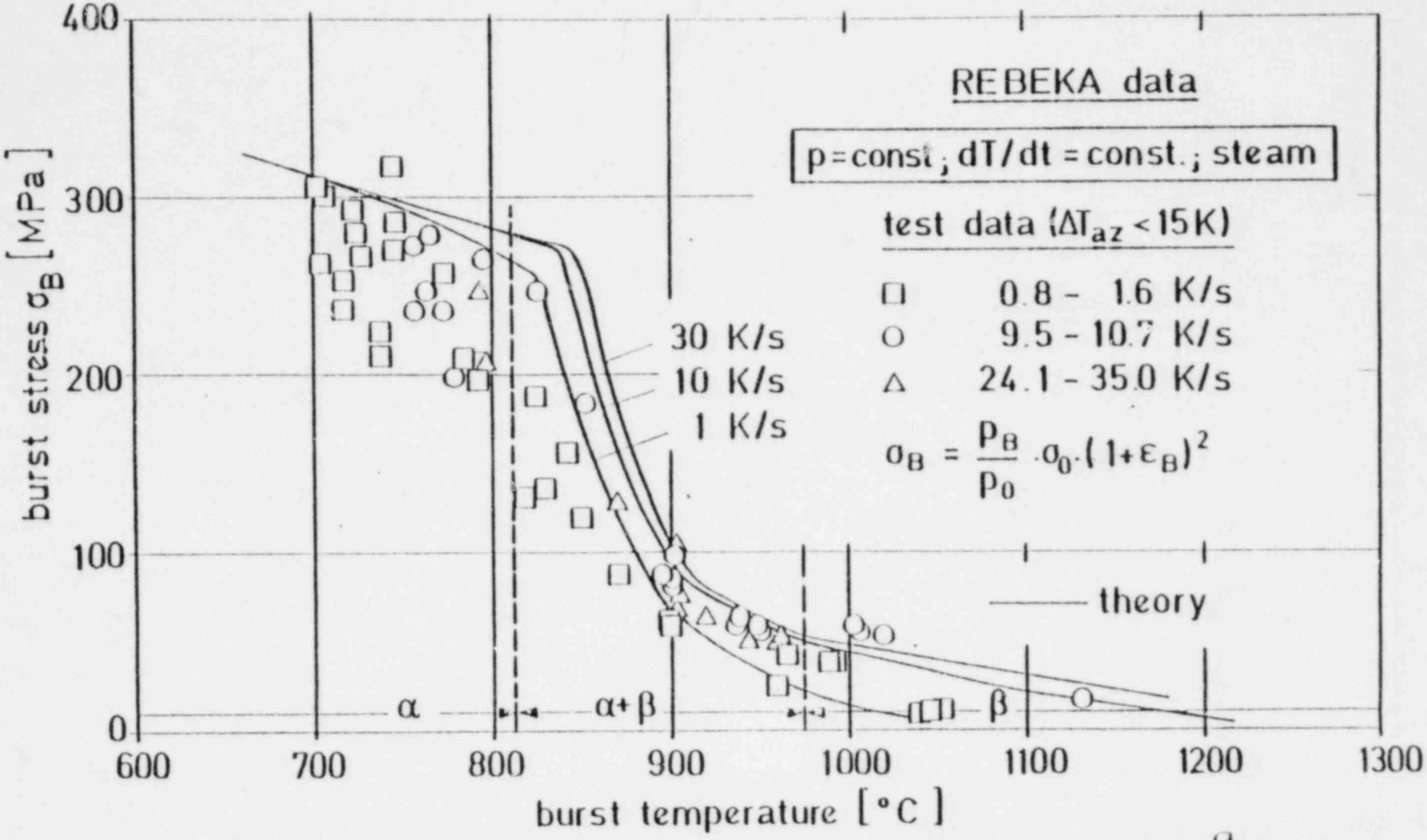
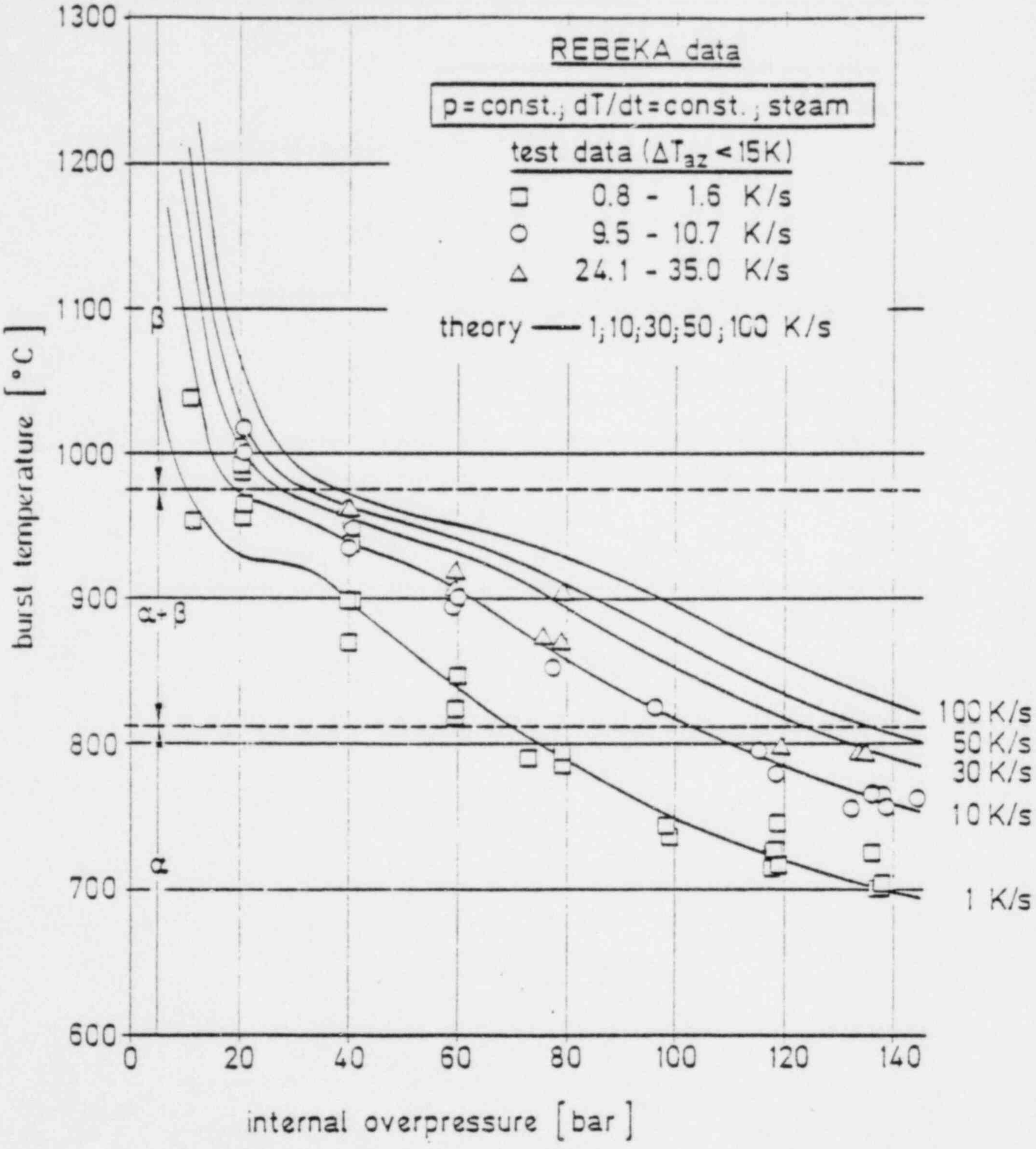


Burst stress vs. burst temperature of Zry claddings

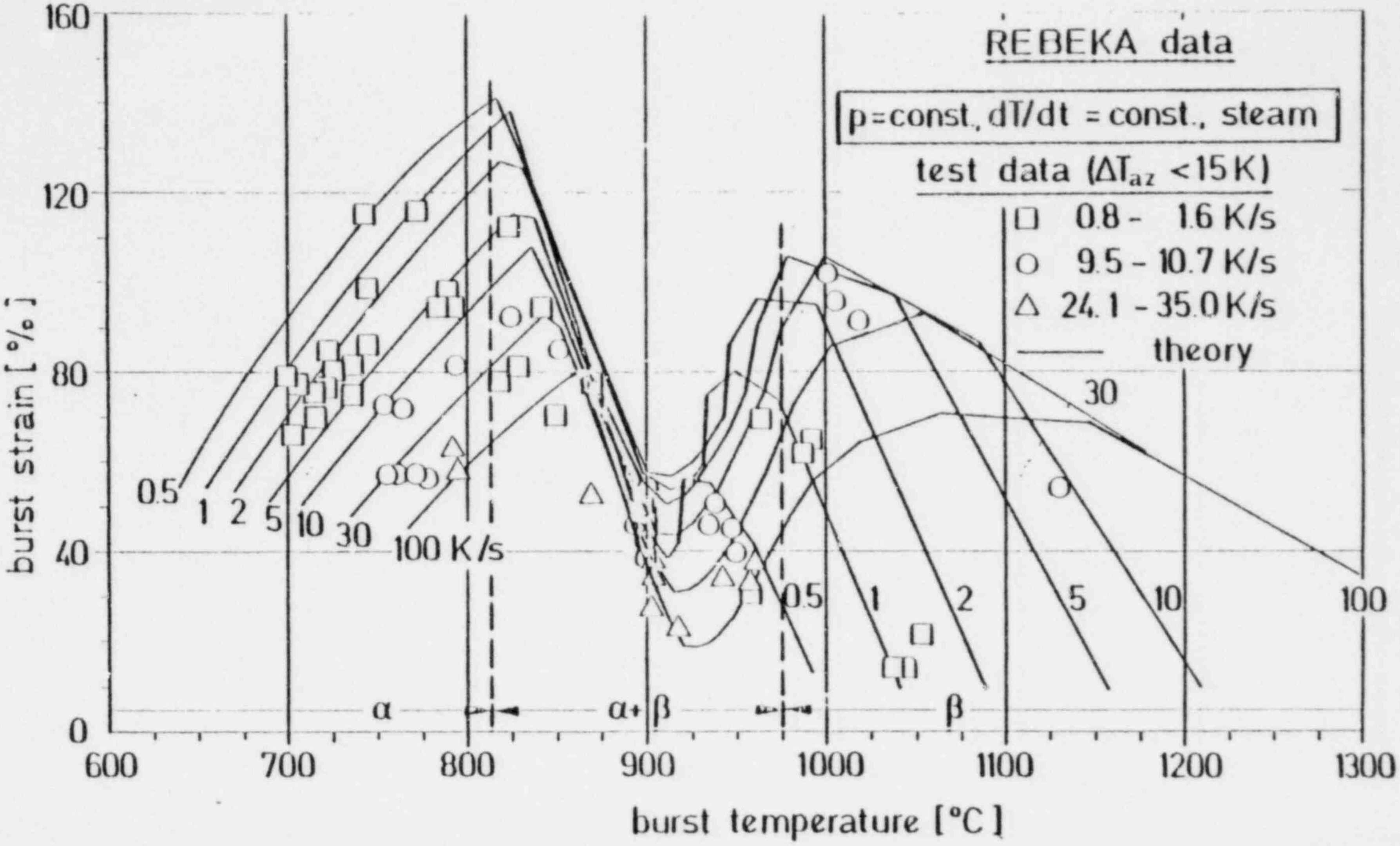
From: "Burst Criterion of Zircaloy Fuel Claddings in a LOCA", F. Erbacher, H.J. Neitzel, H. Rosinger, H. Schmidt, K Wiehr, ASTM Symposium

Fig 3



Burst temperature vs. internal overpressure of Zry claddings

From: "Burst Criterion of Zircaloy Fuel Claddings in a LOCA", F.Erbacher, H.J.Neitzel, H.Rosinger, H.Schmidt, K.Wiehr, ASTM Symposium on Zirconium in the Nuclear Industry, Boston, Aug 1980



Burst strain vs. burst temperature of Zry claddings

From: 'Burst Criterion of Zircaloy Fuel Claddings in a LOCA", F.Erbacher, H.J.Neitzel, H.Rosinger, H.Schmidt, K.Wiehr Fig

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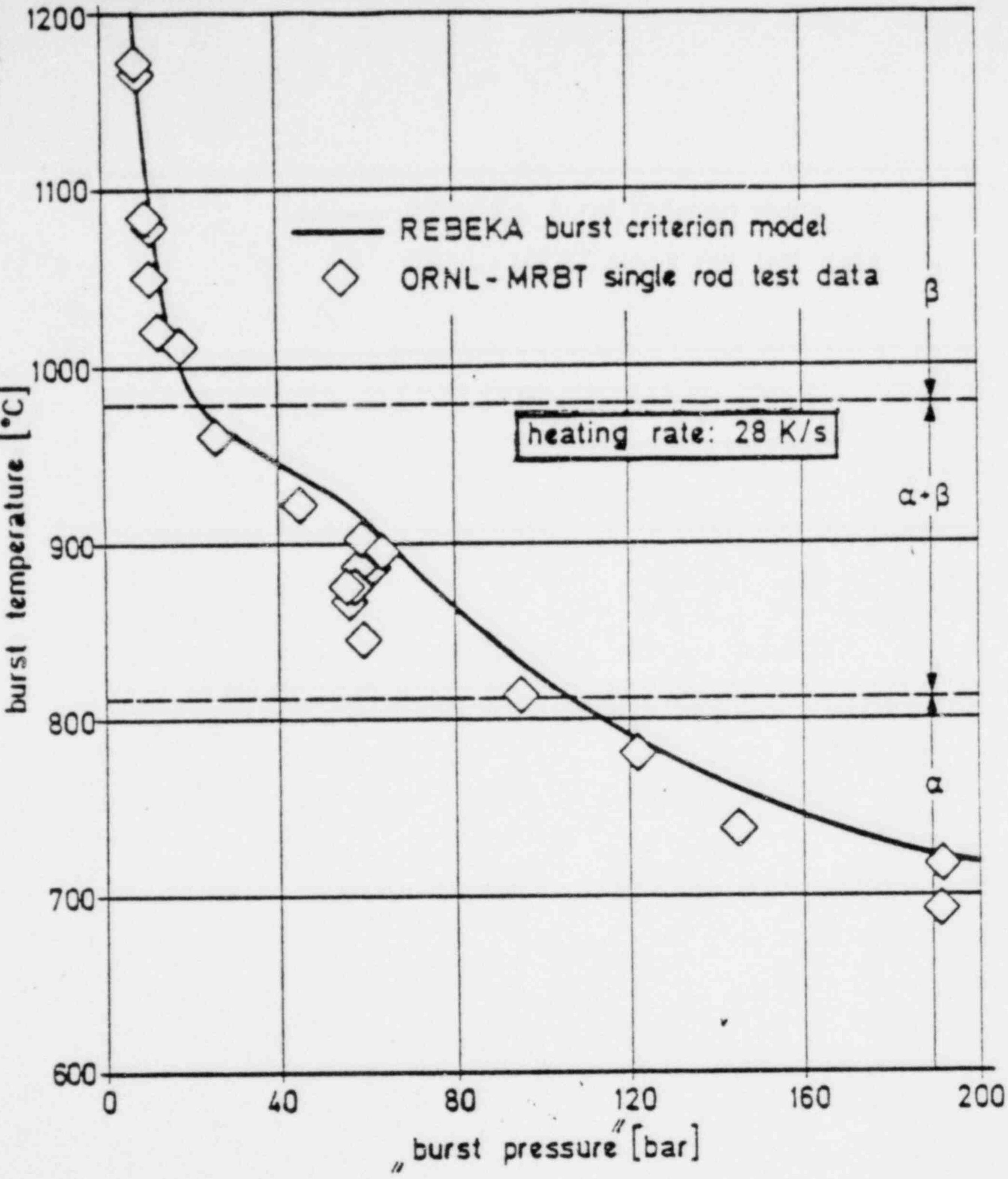

Burst temperature vs. burst pressure

From: F. Erbacher, Kernforschungszentrum Karlsruhe, FRG

8. THE BALLOON-2 CODE USED IN FRAP-T IS BEING MODIFIED BY HAGRMAN (EG&G) TO CALCULATE AXIAL STRAIN PROFILES OF BALLOONING FUEL RODS USING STATISTICALLY VARYING PELLET PROPERTIES AND POWER ALONG THE LENGTH OF THE FUEL ROD. TESTS USING INFRA-RED SCANS OF HEATER PROFILES IN ORNL-SRBT TESTS HAVE ALLOWED CALCULATION OF AXIAL STRAIN PROFILES OBSERVED EXPERIMENTALLY. THIS COULD BE COUPLED WITH ERBACHER'S CALCULATION OF TIME OF BURST TO PERMIT CALCULATION OF AVERAGE ROD STRAIN AT TIME OF BURST.

9. SENGPIEL AND BORGWALDT (KFK) HAVE USED A PROBABILISTIC APPROACH, RESPONSE SURFACE METHODOLOGY, STATISTICAL VARIATION OF PELLET POWER, STATISTICAL VARIATION OF COLD NEIGHBOR RODS, AND SEVERAL THERMAL-HYDRAULIC FACTORS, TO CALCULATE FLOW BLOCKAGE IN A 15 X 15 KWU FUEL BUNDLE. THEY FOUND THAT:

   A. THE MOST PROBABLE "BLOCKADE" OF NEIGHBORING COAXIAL BLOCKED SUBCHANNELS HAVE LESS THAN 20 PERCENT FLOW AREA LEFT IS A CLUSTER OF FOUR.

   B. THE PROBABILITY OF A "BLOCKADE" FORMED OF A CLUSTER OF NINE SUBCHANNELS (3 X 3 ARRAY OF CHANNELS) IS QUITE LOW.

   C. A "BLOCKADE" OF 16 SUBCHANNELS (5 X 5 ARRAY OF RODS) IS QUITE IMPROBABLE.

   D. BALLOONING STRAINS CALCULATED RANGED UP TO AT LEAST 80 PERCENT, 79 OF THE 205 FUEL RODS BALLOONED 20 PERCENT OR MORE, AND 60 SUBCHANNELS HAD LESS THAN 20 PERCENT FLOW AREA REMAINING.

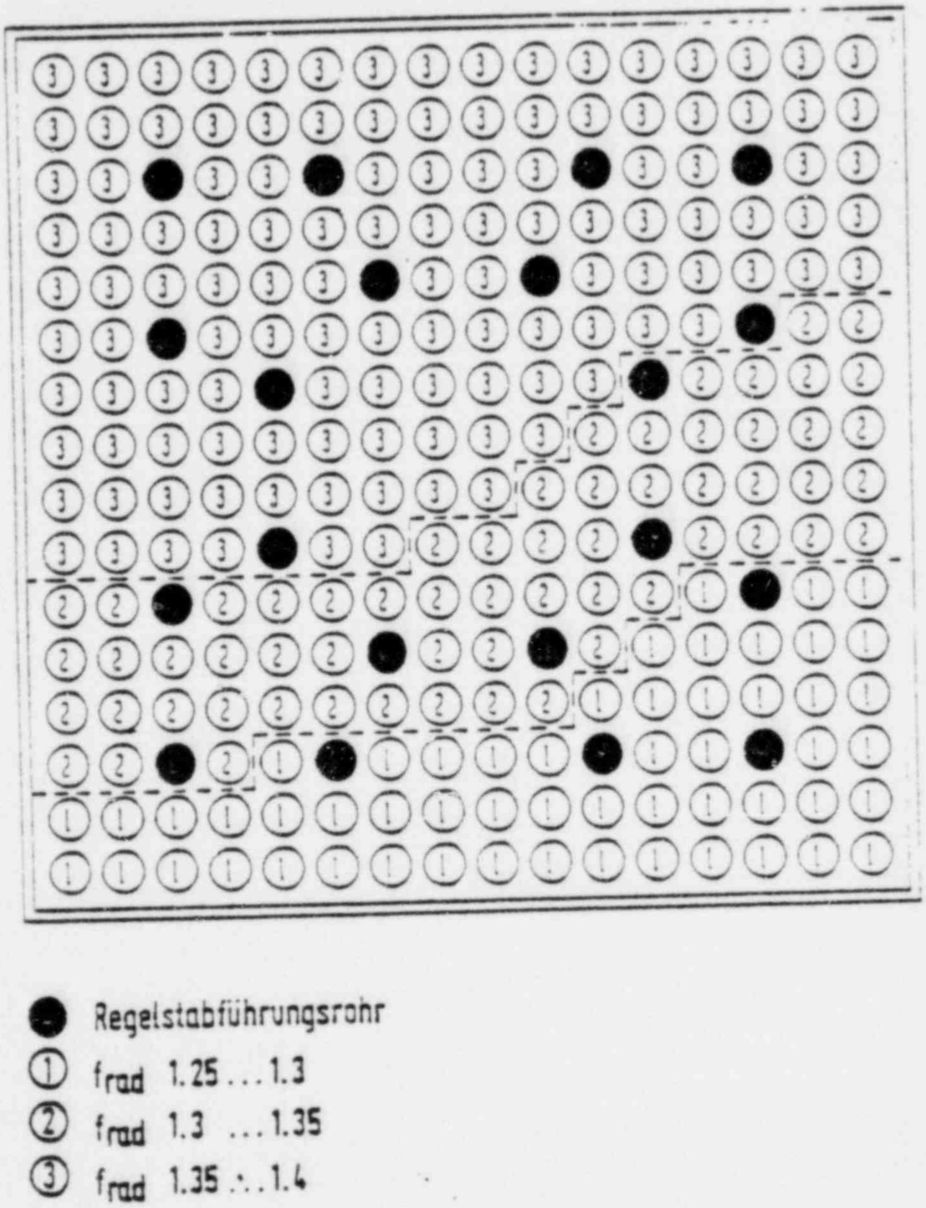

Abb. A-4: Einteilung der Brennstäbe des Referenz-Brennelementes in Leistungsklassen unterschiedlicher nomineller radialer Leistungsfaktoren

From papers by W. Sengpiel and H.Borgwaldt,KfK,presented at the PNS/NRC/JAERI Annual Information Exchange on Cladding and Codes,KfK,Karlsruhe,FRG,June 1980

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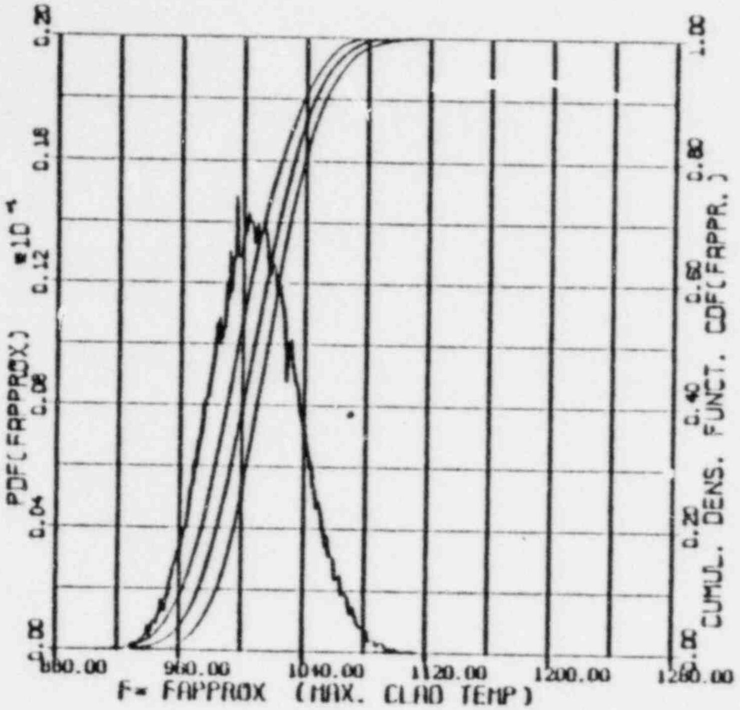


Abb. A-18: Dichtefunktion (PDF) und Verteilungsfunktion (CDF, mit 95%-Konfidenzintervallen) der max. Hüllrohrtemperatur (K) für einen Brennstab mit einem nominellen radialen Leistungsfaktor f_{rad} = 1.3, Stabklasse 1

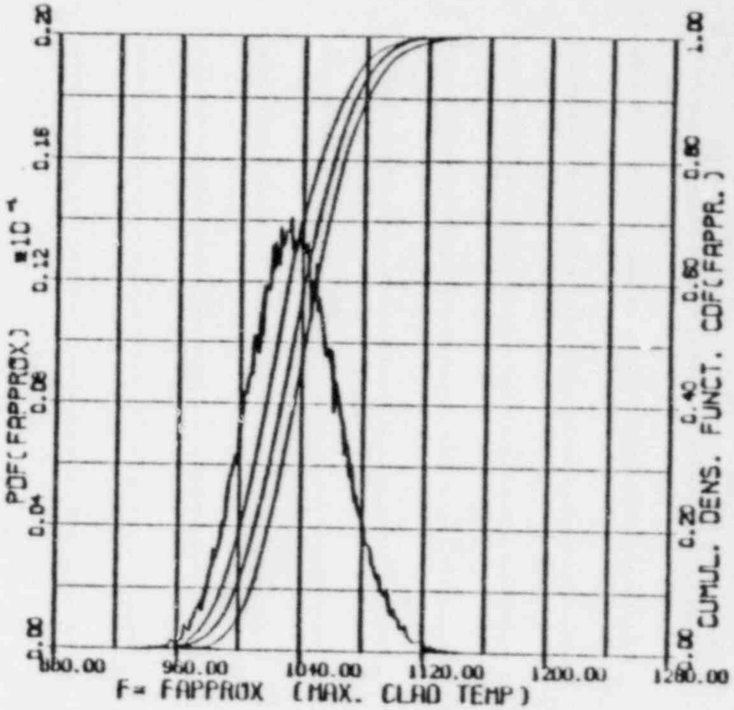


Abb. A-19: Dichtefunktion (PDF) und Verteilungsfunktion (CDF, mit 95%-Konfidenzintervallen) der max. Hüllrohrtemperatur (K) für einen Brennstab mit einem nominellen radialen Leistungsfaktor f_{rad} = 1.4, Stabklasse 3

From papers by W. Sengpiel and H.Borgwaldt,KfK, presented at the PNS/NRC/JAERI Annual Information Exchange on Cladding and Codes, KfK,Karlsruhe,FRG, June 1980

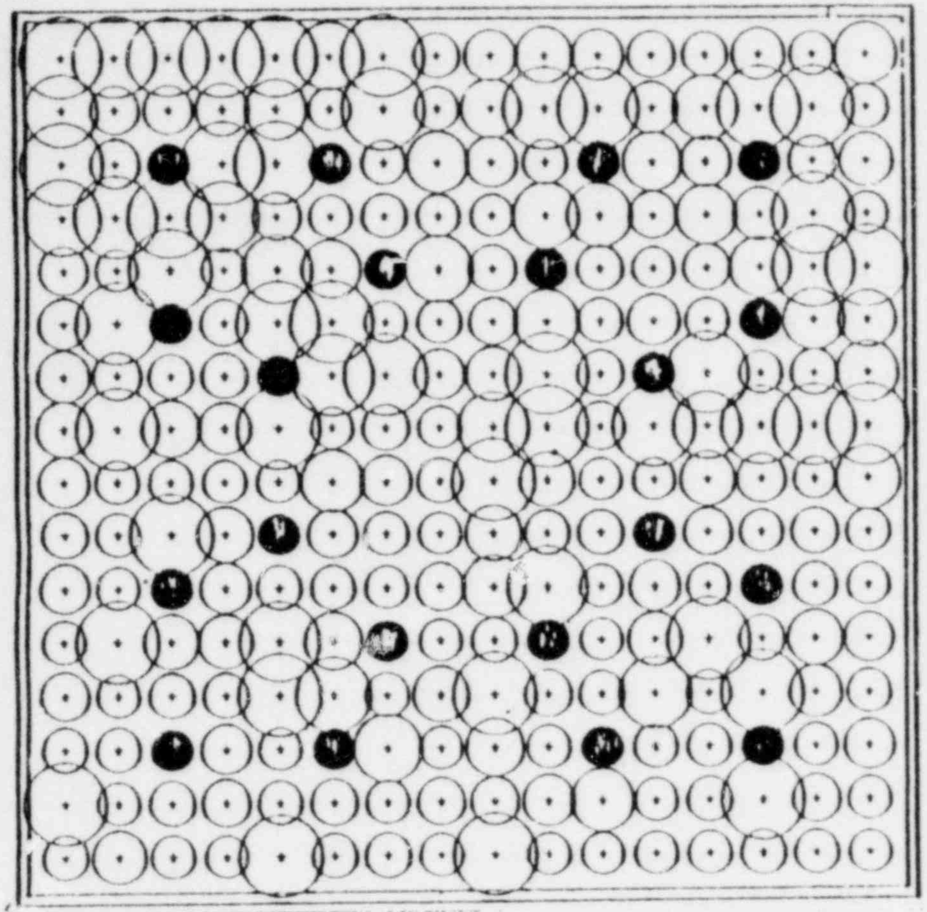


Abb. A-22: Qualitative Darstellung der max. Hüllrohrdehnungen der 236 Brennstäbe im Querschnitt durch die Zone 12 des Referenz-Brennelementes. Ergebnis einer Monte Carlo-Simulation unter pessimistischen Nebenbedingungen

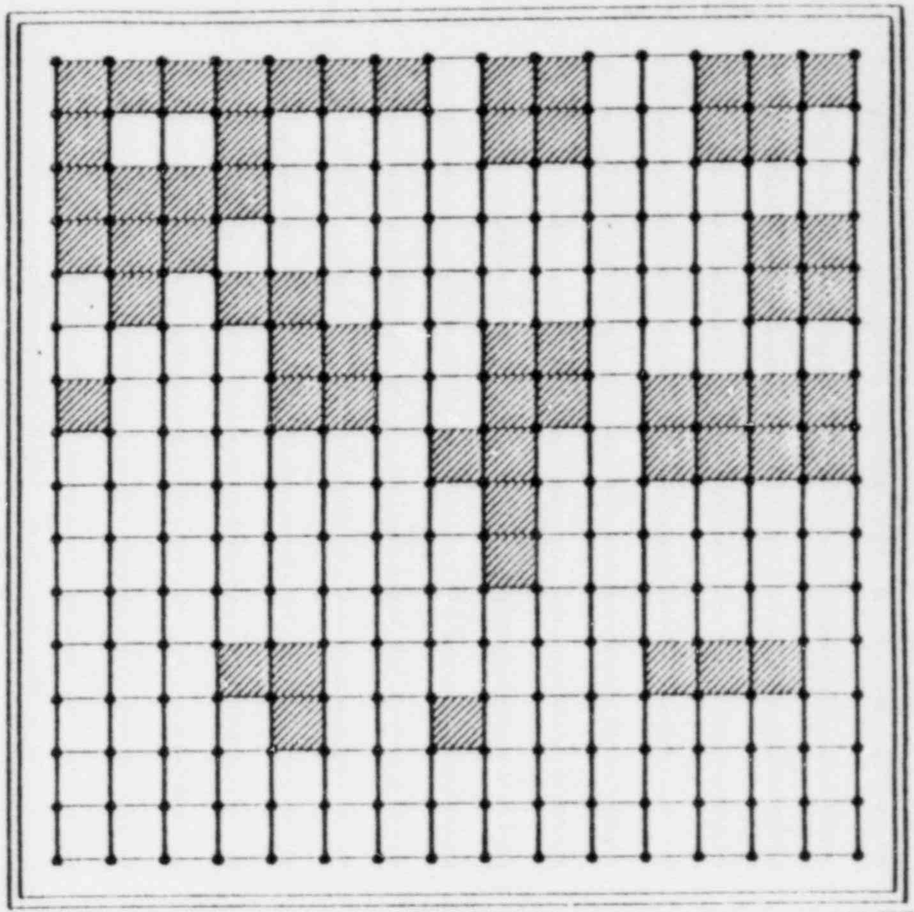
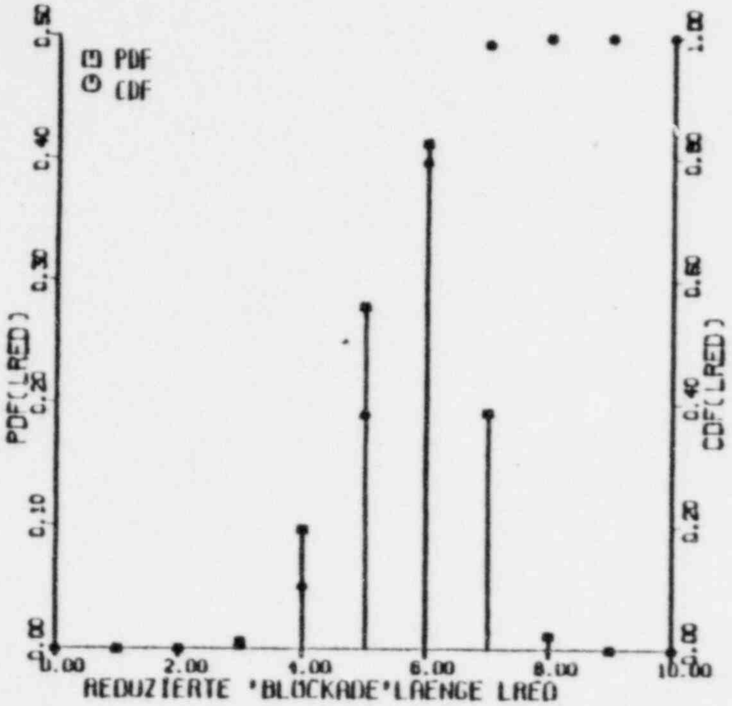


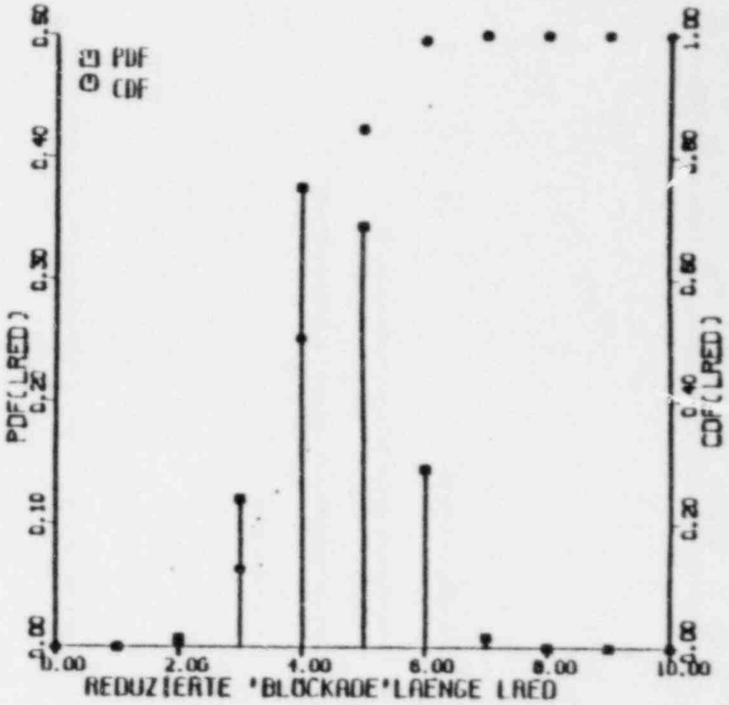
Abb. A-23: Inseln blockierter Kühlkanäle in der axialen Zone 12 des Referenz-Brennelements als Folge der Hüllrohrdehnungen entsprechend Abb. A-22

From papers by W. Sengpiel and H. Borgwaldt, KfK, presented at the PNS/NRC/JAERI Annual Information Exchange on Cladding and Codes, Kfk, Karlsruhe, FRG, June 1980



$F_{RED} < 30$ %

Abb. A-28.1: Dichte- und Verteilungsfunktionen des "relativen äquivalenten Blockadedurchmessers" der größten Blockadeinsel im Referenz-BE, $F_{Red} < 30\%$, pessimistische Nebenbedingungen



$F_{RED} < 20$ %

Abb. A-28.2: Dichte- und Verteilungsfunktionen des "relativen äquivalenten Blockadedurchmessers" der größten Blockadeinsel im Referenz-BE, $F_{Red} < 20\%$, pessimistische Nebenbedingungen

From papers by W. Sengpiel and H.Borgwaldt,KfK, presented at the PNS/NRC/JAERI Annual Information Exchange on Cladding and Codes, KfK,Karlsruhe,FRG,June 1980

10. FBRB HAS THE FOLLOWING SUGGESTIONS TO MAKE CONCERNING LICENSING ACTIONS INVOLVING FUEL ROD BALLOONING AND FLOW BLOCKAGE IN BUNDLES.

    A. IF A FLOW BLOCKAGE AUDIT CURVE MUST BE ESTABLISHED AT THIS TIME, LET IT BE BASED ON AVERAGE ROD STRAINS, NOT BURST STRAINS.

    B. DEVELOPMENTS THAT SHOULD OCCUR IN THE COMING YEAR IN CODE ANALYSES OF BALLOONING AND FLOW BLOCKAGE IN FUEL BUNDLES SHOULD PROVIDE A MUCH SOUNDER BASIS FOR AUDITING FLOW BLOCKAGE CALCULATIONS BY VENDORS THAN WILL BE AVAILABLE FROM THE USE OF NUREG-0630 CORRELATIONS.

    C. PROPER COMBINATION AND MODIFICATIONS OF ERBACHER'S BURST CRITERION, BALLOON-2 CODE, ORNL-MRBT AVERAGE STRAIN DATA, AND THE SENGPIEL/BORGWALDT PROBABILISTIC APPROACH SHOULD PERMIT BEST ESTIMATE PRETEST PREDICTIONS TO BE MADE FOR THE NRU TESTS, AS WELL AS THE LARGER BUNDLES (15 X 15) TEST SCHEDULED IN LOFT.

    D. A COMPLETE AND VERIFIED CODE FOR BEST ESTIMATE CALCULATIONS OF FLOW BLOCKAGE IN LARGE BUNDLES SHOULD BE AVAILABLE IN LESS THAN 5 YEARS, VERIFIED BY BOTH EX-PILE AND IN-PILE BUNDLE DATA.

Tape 6
Esposito

ACRS MEETING
SEPT. 3, 1980

WESTINGHOUSE COMMENTS ON NRC PROPOSED BURST AND BLOCKAGE MODELS

(NUREG-0630)

V. J. ESPOSITO ✓
D. L. BURMAN
L. E. HOCHREITER
R. A. MUENCH

AGENDA

1. ISSUES (V. J. ESPOSITO)
2. FUEL ASPECTS (D. L. BURMAN)
   - OVERVIEW OF TECHNICAL ISSUES
   - RESULTS OF W REVIEW OF DATA
3. HEAT TRANSFER/FLOW BLOCKAGE (L. E. HOCHREITER)
   - OVERVIEW OF AVAILABLE DATA
   - RECENT FLECHT-SEASET DATA
4. POTENTIAL IMPACT OF NUREG-0630 (R. A. MUENCH)
   - PEAKING FACTOR
5. CONCLUSIONS/RECOMMENDATIONS (V. J. ESPOSITO)

ISSUES

BURST TEMPERATURE

- W DOESN'T AGREE WITH THE NRC HEATUP RATE DEPENDENCE ON BURST TEMPERATURE

BURST STRAIN

- IT IS IMPORTANT TO USE PROTOTYPICAL DATA

BLOCKAGE

- THE USE OF STATISTICALLY AVERAGED (NOT MAXIMUM) STRAIN TO ARRIVE AT A FLOW BLOCKAGE IS APPROPRIATE

IMPACT OF NUREG-0630 MODELS

BURST TEMPERATURE: DETERMINE THE INCIDENCE OF BURST

NUREG-0630 GIVES EARLIER BURST

- MORE ZR-H2O REACTION
- HIGHER GAP CONDUCTANCE AFTER BURST

BURST STRAIN: DETERMINE STRAIN AT THE BURST LOCATION

NUREG-0630 GIVES MORE STRAIN WHICH LEADS TO
MORE SURFACE AREA FOR ZR-H2O REACTION

BLOCKAGE: DETERMINE STEAM COOLING PENALTY

NUREG-0630 GIVES MORE BLOCKAGE

Tape 6
& 7

ACRS
COMMITTEE MEETING
SEPTEMBER 3, 1980

WESTINGHOUSE COMMENTS ON NRC PROPOSED BURST AND BLOCKAGE MODELS

D. L. BURMAN

PROPOSE TO SHOW

1. WESTINGHOUSE SMALL BREAK BURST TEMPERATURE MODEL SHOWS GOOD AGREEMENT WITH ORNL AND OTHER DATA: THEREFORE, NO NEED FOR NEW CORRELATION.

2. WESTINGHOUSE BURST STRAIN DATA AND CORRELATION SHOW GOOD AGREEMENT WITH ORNL MRBT INDIVIDUAL ROD BURST STRAINS. DIFFERENCE BETWEEN NRC AND WESTINGHOUSE MODELS IS USE OF MAXIMUM VS. AVERAGE STRAINS.

3. REASON FOR VARIABILITY IN STRAIN IS DUE TO TEMPERATURE NON-UNIFORMITY.

4. EVIDENCE FOR TEMPERATURE NON-UNIFORMITY IN-PILE.

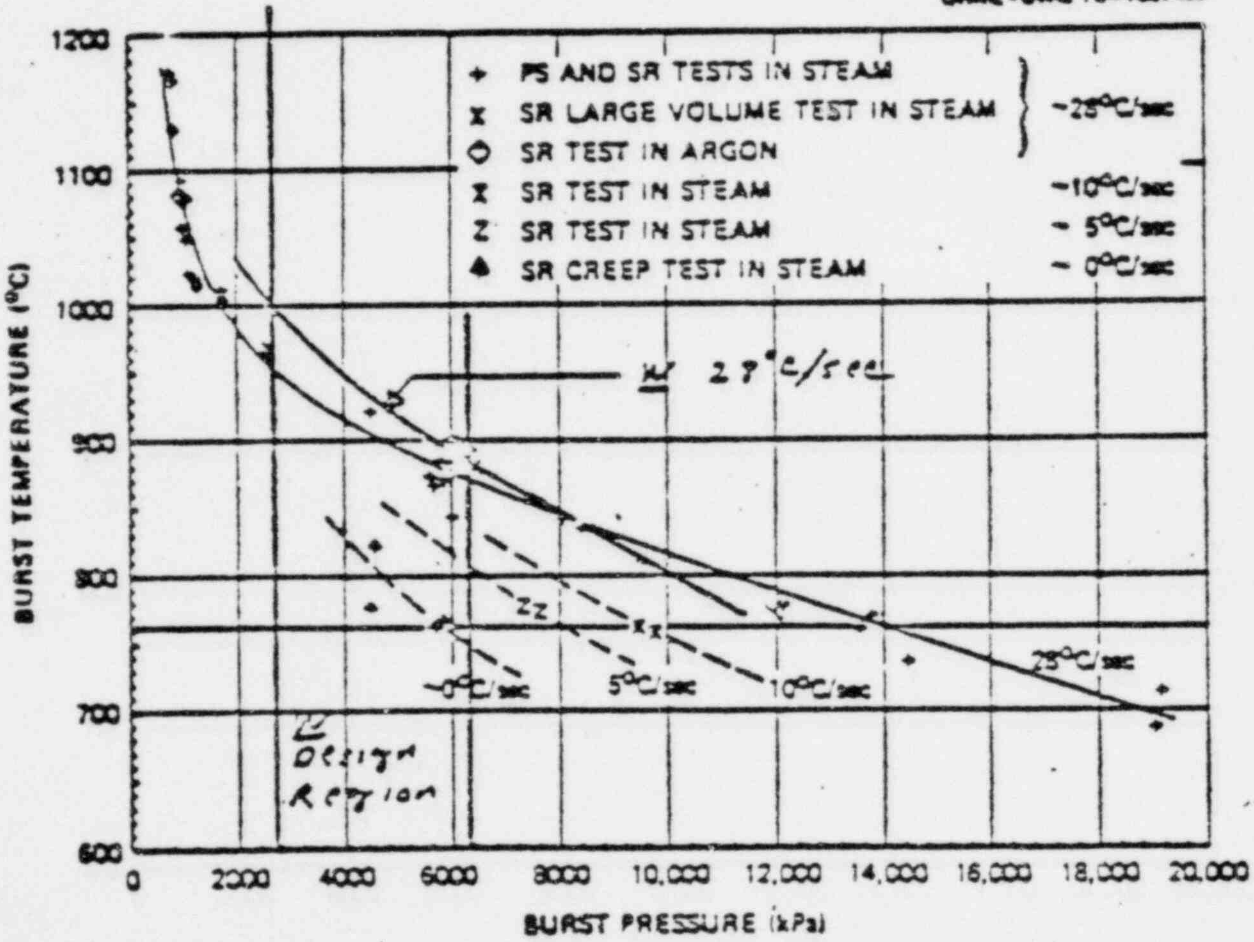
5. FRG IN-PILE TESTS SHOW BURST BEHAVIOR SIMILAR TO OUT-OF-PILE TESTS.

COMPARISON OF DATA BASES

	W	NUREG-0630
NUMBER OF SRBT DATA POINTS	261	178
NUMBER OF MRBT DATA POINTS	11	11
STATISTICAL CHARACTERISTICS	HOMOGENEOUS	HETEROGENEOUS
	MANY TESTS AT SAME CONDITIONS	FEW TESTS AT SAME CONDITIONS
PROTOTYPICALITY	PROTOTYPES EXPECTED INPILE TEMP DIFFERENCES	PROTOTYPES INTERNAL HEATING

BURST TEMPERATURE

ORNL-DWG 78-10874A



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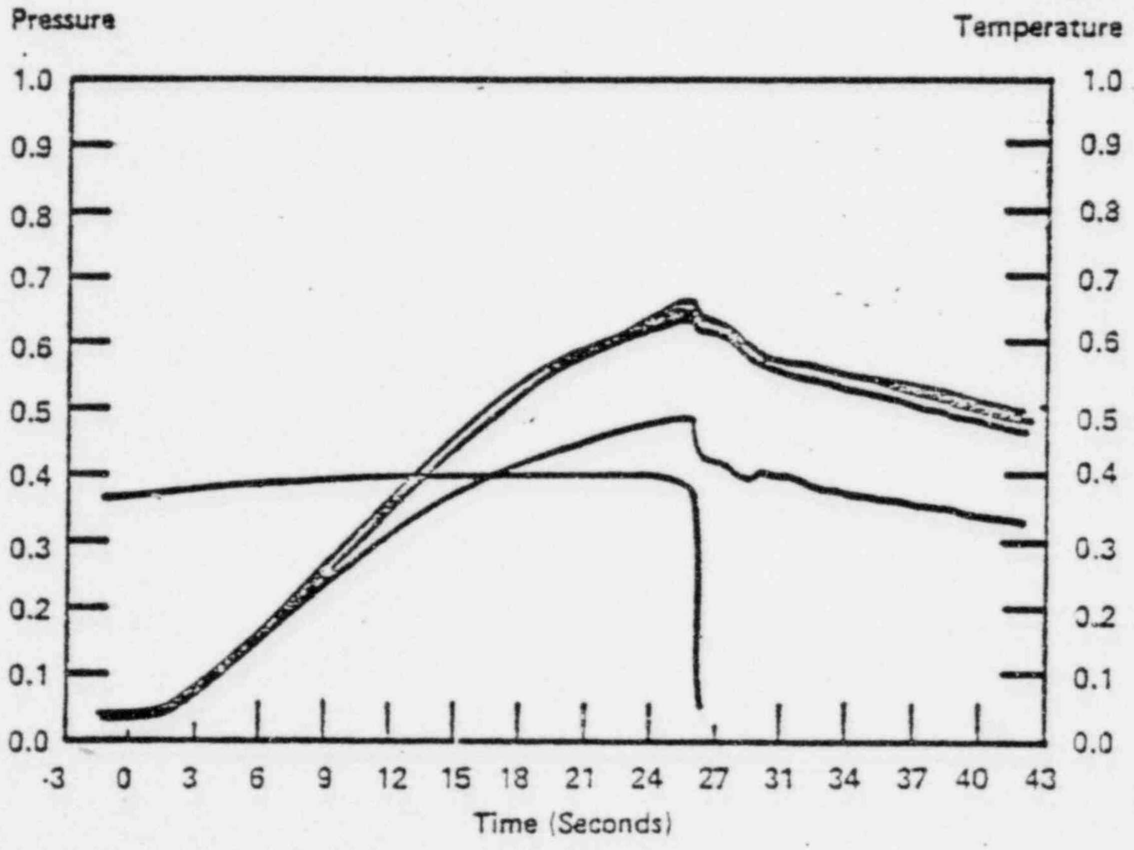
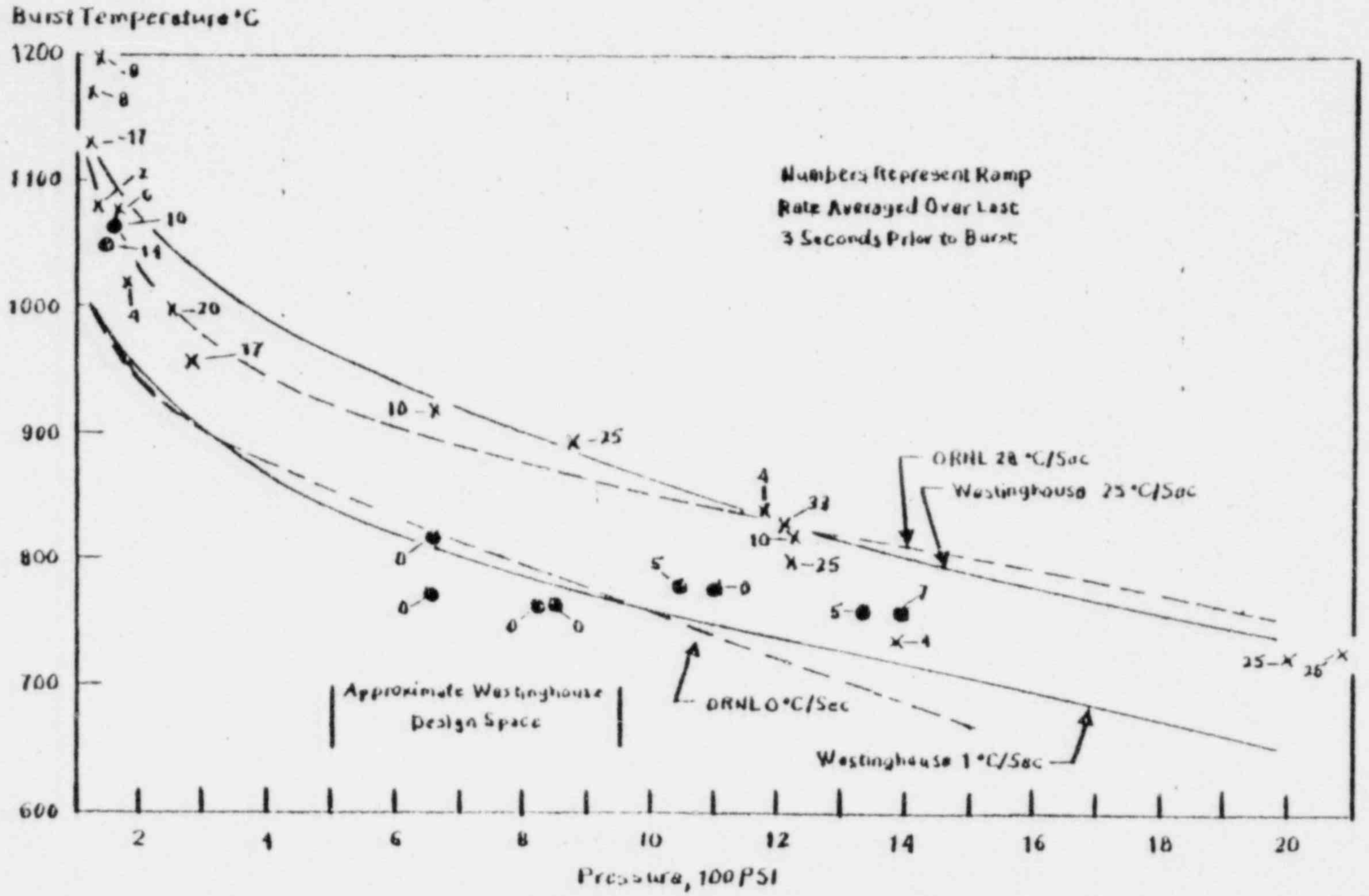


Figure 10. Quick-Look Data Plot of SR-45 Showing Temperatures Measured at 63.3 cm Elevation

5


Burst Temperature °C
1200
1100
1000
900
800
700
600
2
4
6
8
10
12
14
16
18
20
Pressure, 100 PSI
Numbers Represent Ramp
Rate Averaged Over Last
3 Seconds Prior to Burst
ORNL 28 °C/Sec
Westinghouse 25 °C/Sec
ORNL 0 °C/Sec
Westinghouse 1 °C/Sec
Approximate Westinghouse
Design Space
X-9
X-8
X-17
2
6
10
14
X-20
4
17
10-X
X-25
4
33
10-X
X-25
0
0
0
0
5
0
5
7
X-4
25-X
26-X

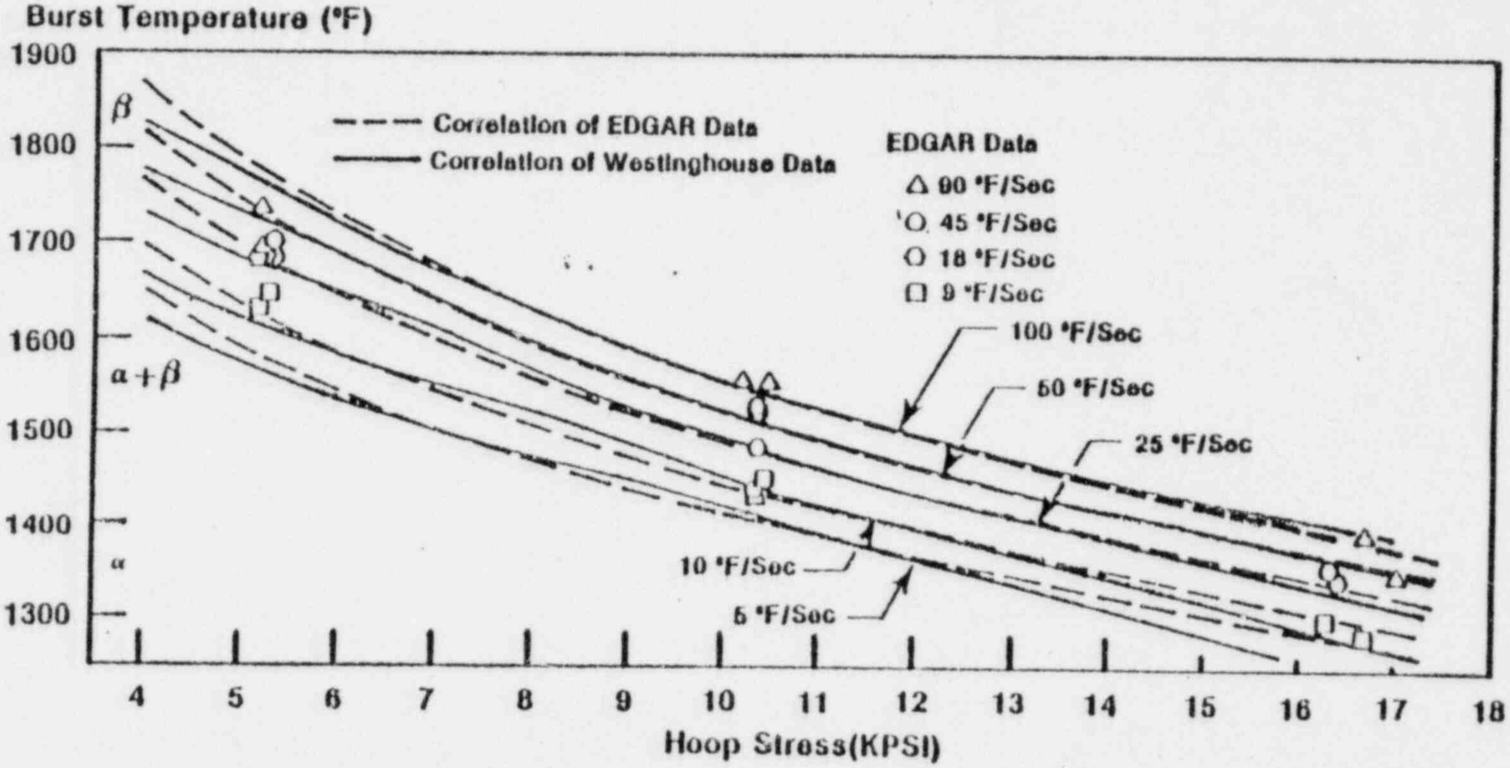
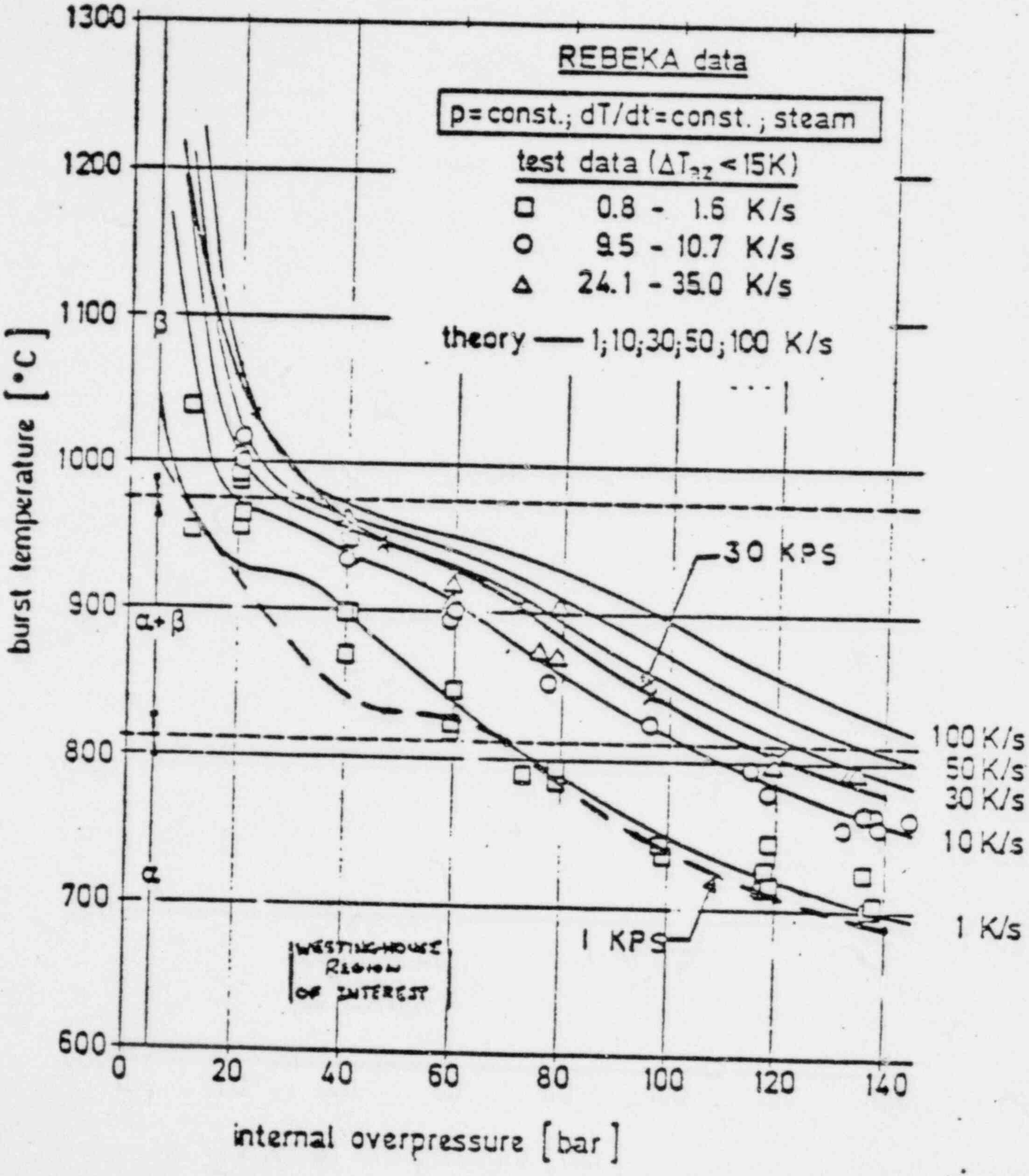


Figure 9. Comparison of EDGAR Program SRBT Correlation and Data vs Westinghouse Correlation



Burst temperature vs. internal overpressure of Zry claddings

CONCLUSION

WHEN RAMP RATE EFFECTS ARE PROPERLY ACCOUNTED FOR, WESTINGHOUSE SMALL BREAK BURST TEMPERATURE MODEL IS IN REASONABLE AGREEMENT WITH ORNL DATA, FRENCH EDGAR DATA, AND FRG REBEKA DATA.

BURST STRAIN

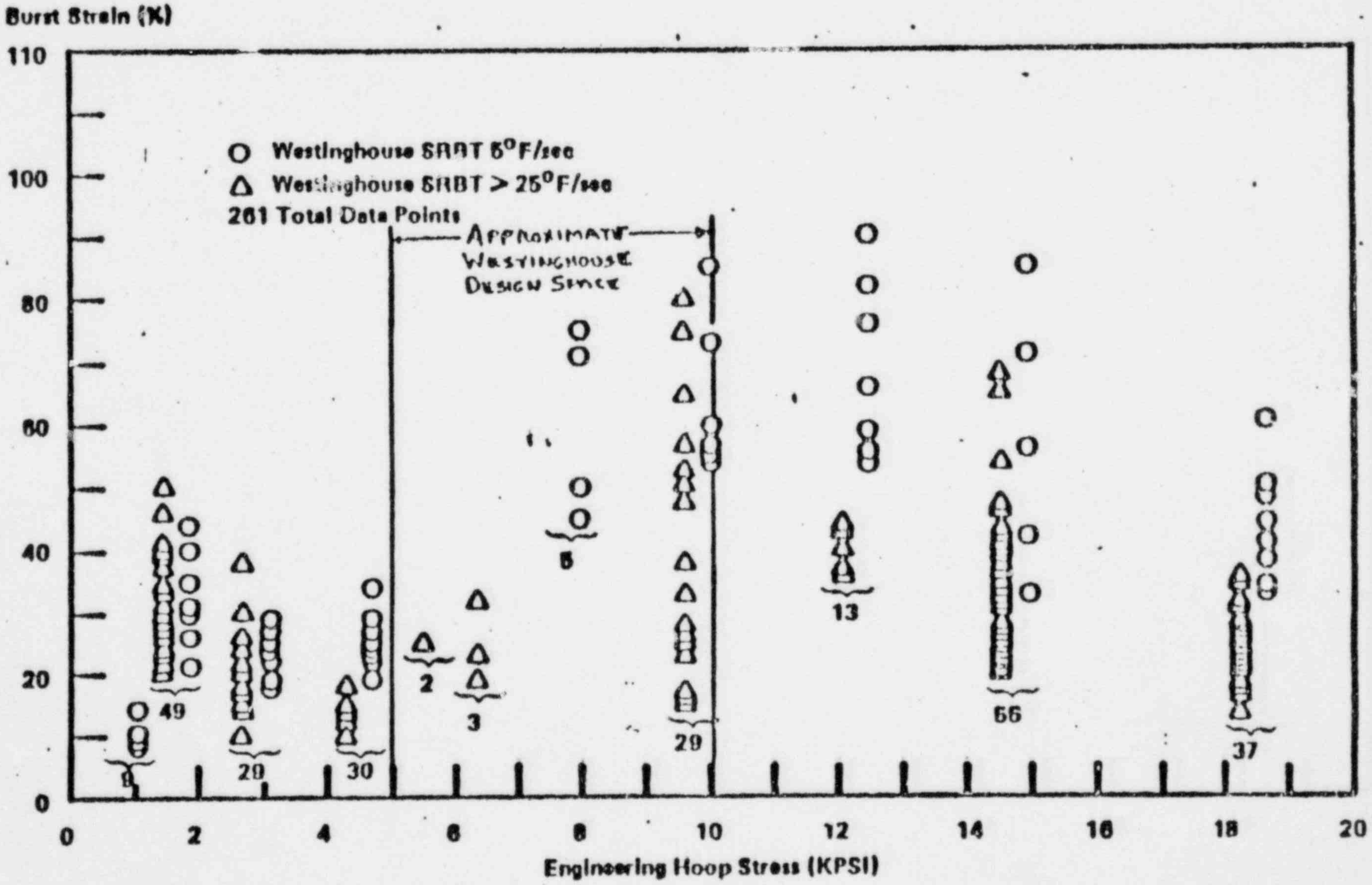
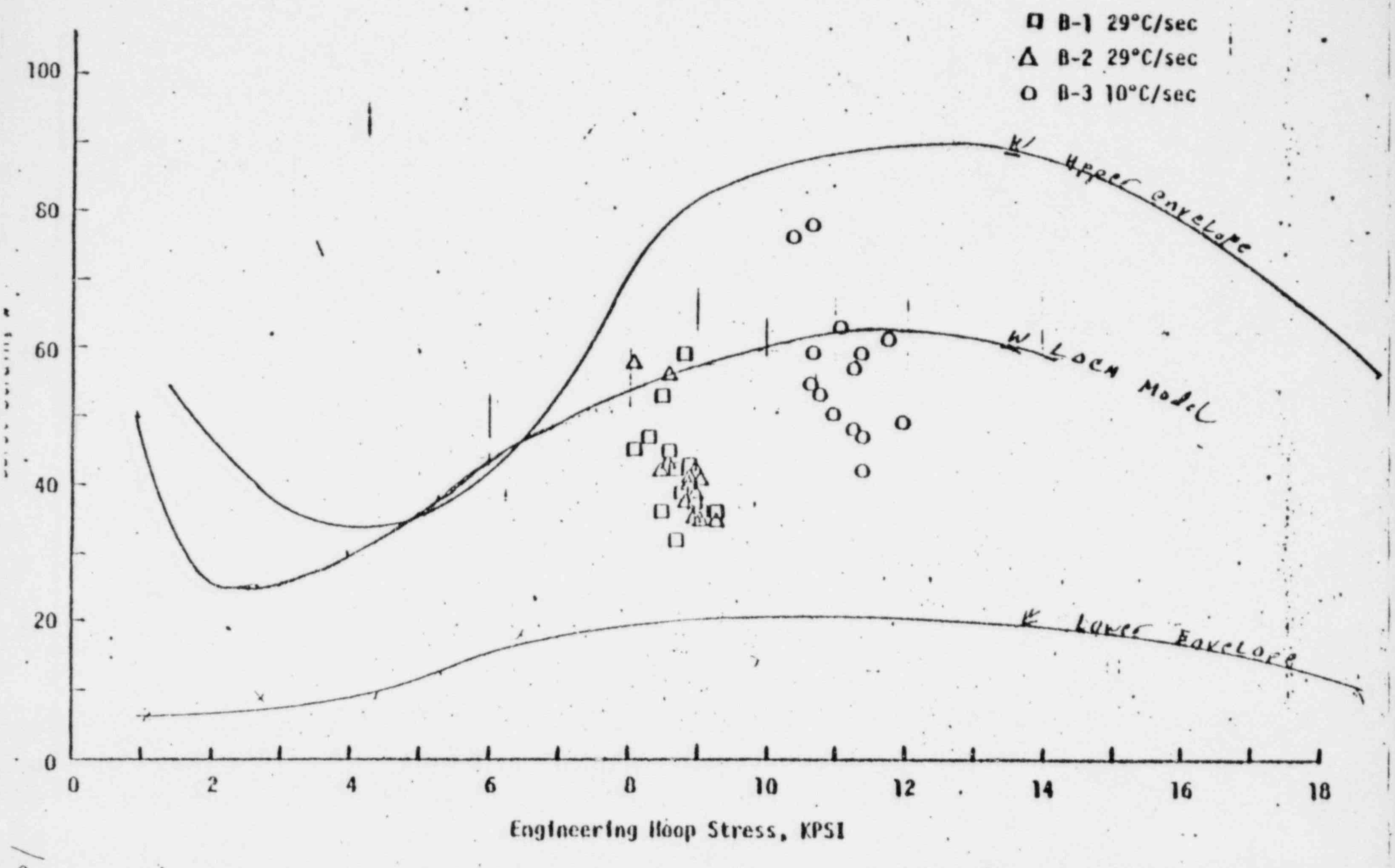


Figure 8. Westinghouse Single Rod Burst Test Results
Burst Strain vs. Engineering Hoop Stress

ORNL MULTI-ROD BURST TEST DATA



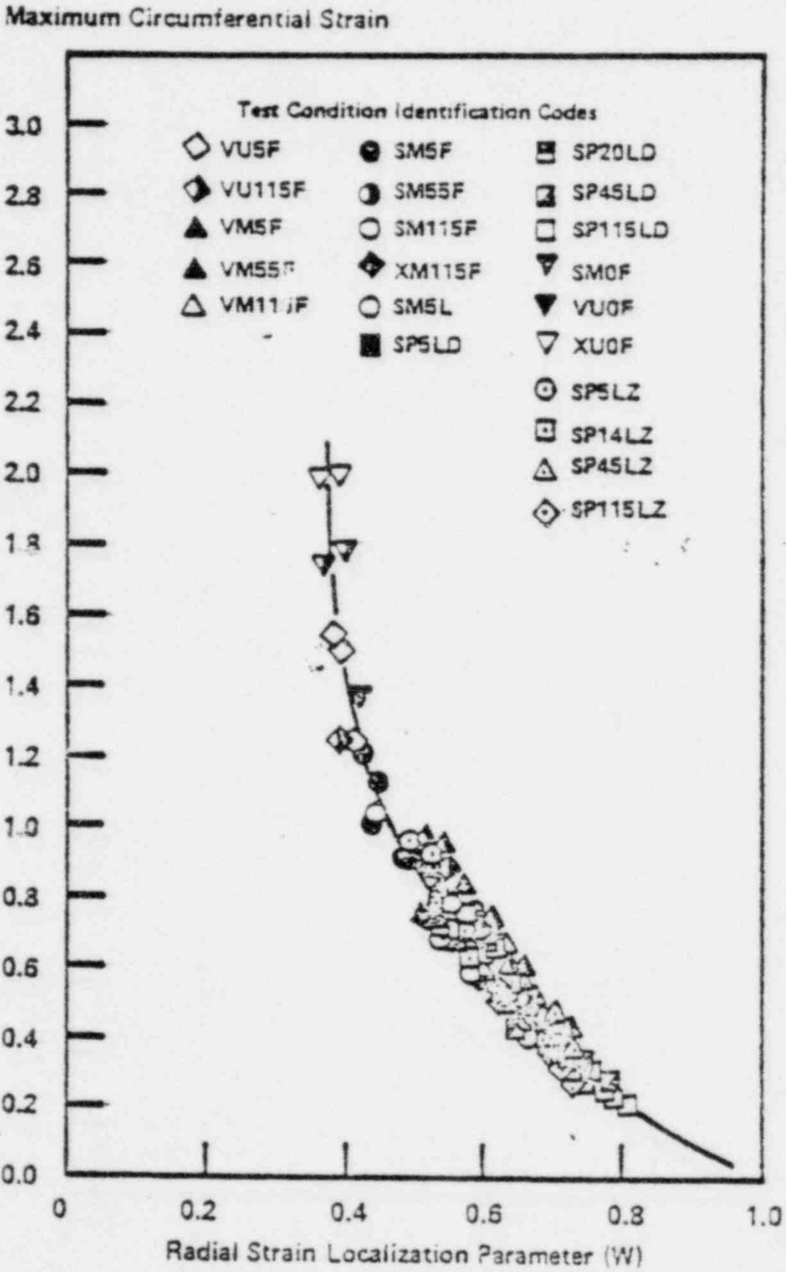


Figure 14. Maximum Circumferential Strain vs Radial Strain Localization Parameter

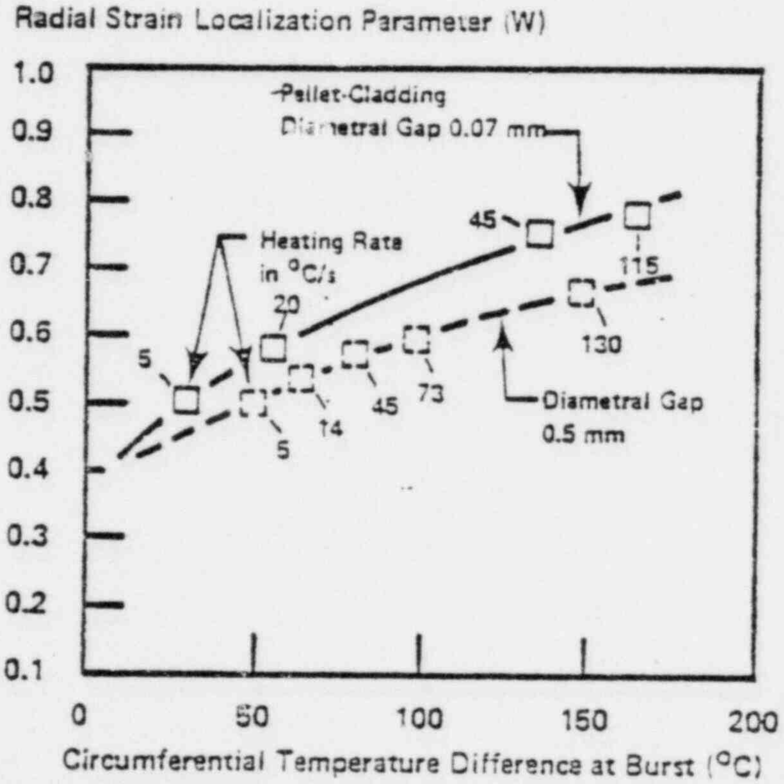


Figure 15. Radial Strain Localization Parameter as a Function of Maximum Circumferential Temperature Difference

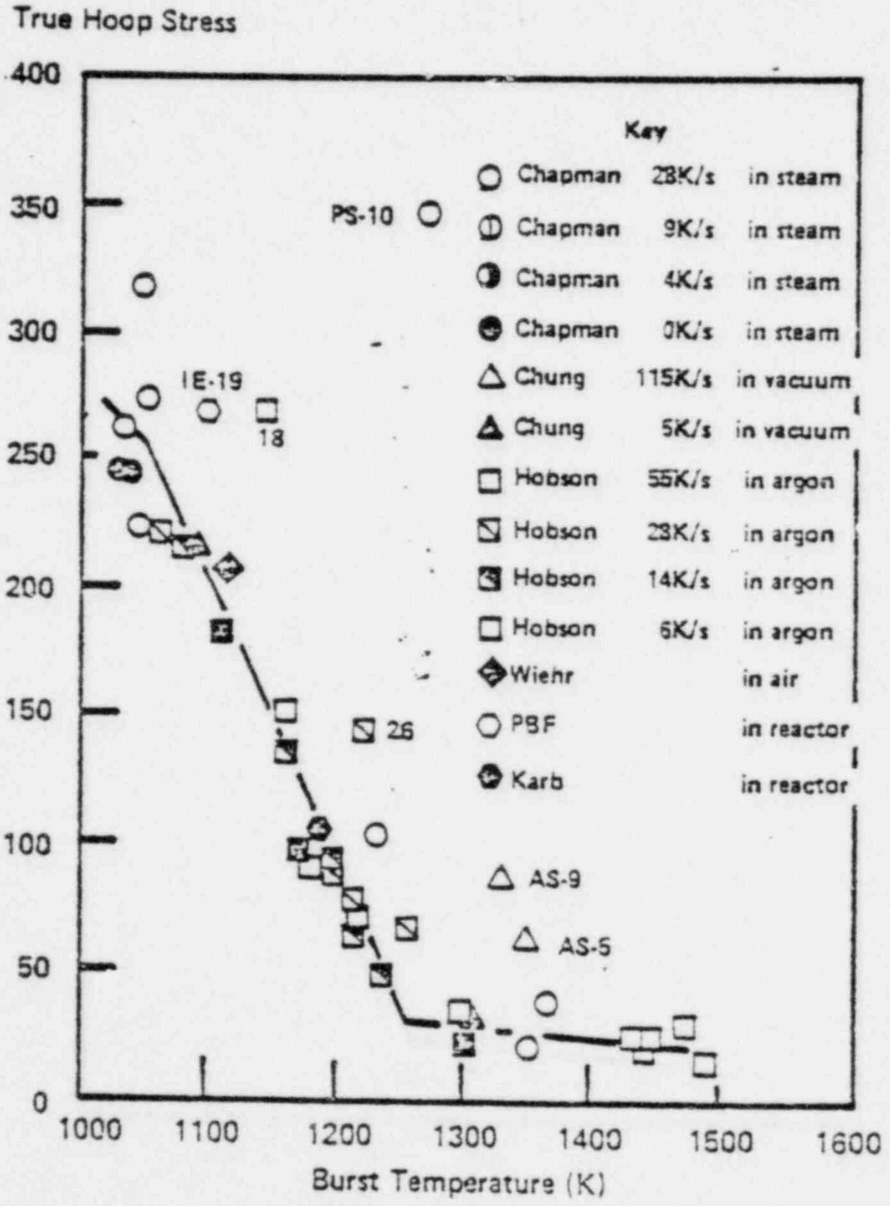
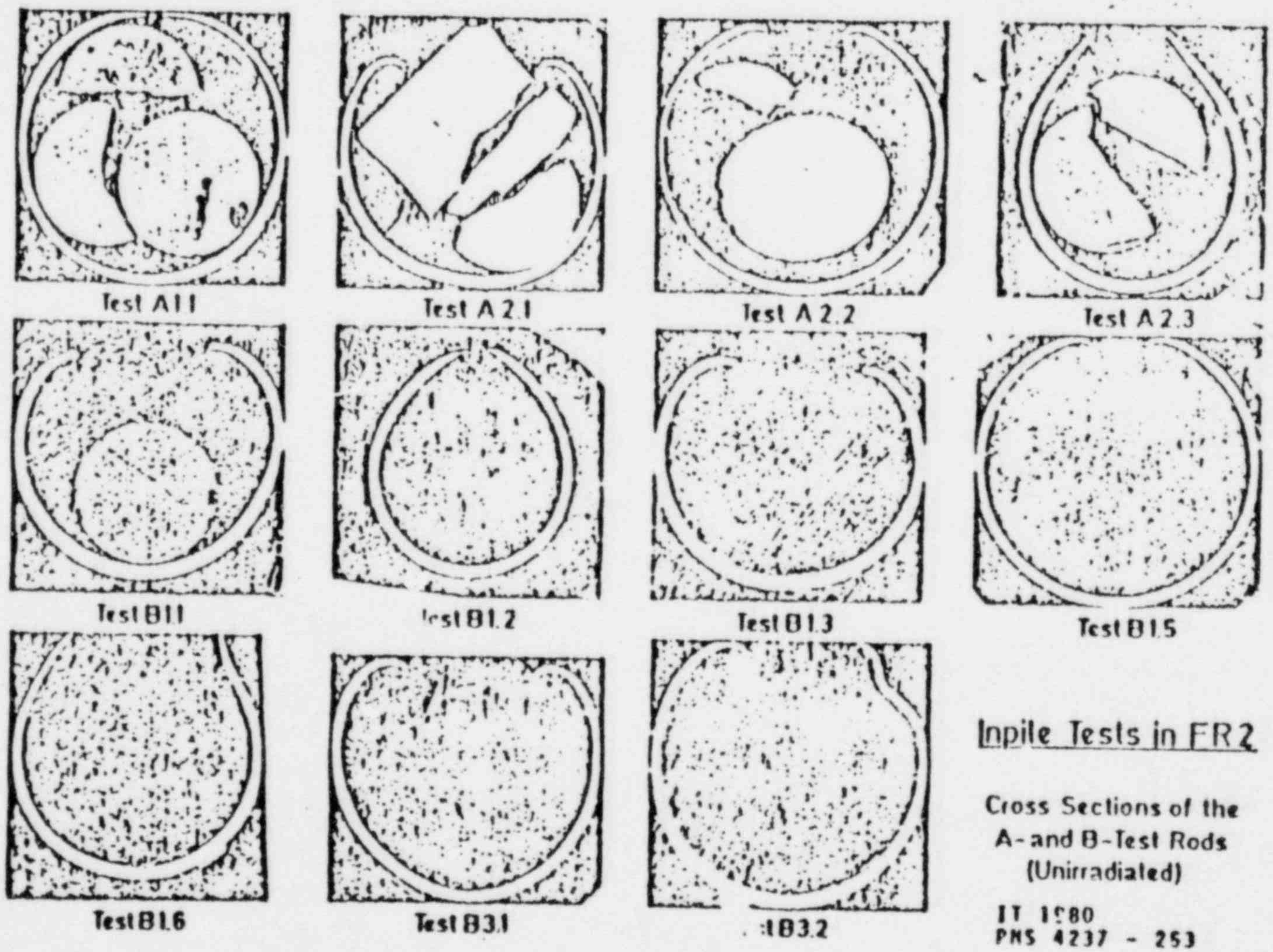


Figure 13. Local Tangential Stress at Failure vs Temperature

15

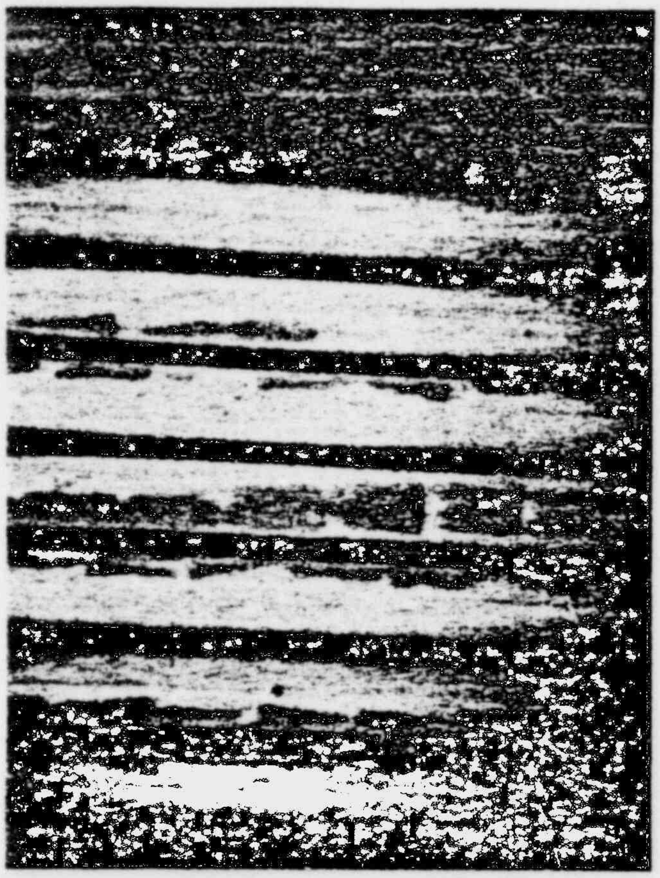
46



Inpile Tests in FR2

Cross Sections of the
A- and B-Test Rods
(Unirradiated)

IT 1980
PNS 4237 - 253



CONCLUSION

WESTINGHOUSE MODELS ARE IN GOOD AGREEMENT WITH ORNL AND OTHER DATA AND NEW MODELS ARE NOT JUSTIFIED.

DIFFERENCES BETWEEN NRC & WESTINGHOUSE POSITIONS

NRC NOW CLAIMS THAT UPPER LIMIT STRAINS SHOULD BE USED FOR BOTH BURST STRAIN CALCULATION AND BLOCKAGE DETERMINATION

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BEST ESTIMATE STRAIN IS JUSTIFIED IN BURST STRAIN CALCULATIONS SINCE IT IS HIGHLY UNLIKELY THAT MAXIMUM STRAIN WOULD OCCUR IN THE HOTTEST ROD (CONVOLUTION IS JUSTIFIED)

Hochreiter
Tape 8

THERMAL-HYDRAULIC ASPECTS OF FLOW BLOCKAGE

- HEAT TRANSFER MECHANISMS DURING REFLOOD WITH FLOW BLOCKAGE
- REVIEW OF DATA ON FLOW BLOCKAGE HEAT TRANSFER
- CONCLUSIONS

HEAT TRANSFER MECHANISMS DURING REFLOOD WITH FLOW BLOCKAGE

- FLECHT TESTS INDICATE THAT THE FLOW IS TWO-PHASE EVEN AT FLOODING RATES OF 0.4"/SEC CONTRARY TO APP K
- RADIATION HEAT TRANSFER CAN ACCOUNT FOR 40% OF TOTAL HEAT TRANSFER
- COMPETING HEAT TRANSFER EFFECTS CAN OCCUR WITH FLOW BLOCKAGE
  - FLOW BYPASS CAN INCREASE LOCAL FLUID TEMPERATURES IN THE BLOCKAGE REGION (H.T. PENALTY)
  - DROPLETS CAN BECOME ATOMIZED BY THE BLOCKAGE OR DUE TO FLOW ACCELERATION RESULTING IN IMPROVED DROPLET/STEAM HEAT TRANSFER DESUPERHEAT THE STEAM (INC. TW-TV), IMPROVED DROPLET RADIATION H.T.
  - FLOW BLOCKAGE WILL INTRODUCE ADDITIONAL MIXING AND TURBULENCE INTO THE FLOW, WILL REQUIRE RE-ESTABLISHMENT OF NEW BOUNDARY LAYERS DOWNSTREAM, FLOW SEPARATION, CROSS-FLOW MIXING, ALL OF WHICH CAN LOCALLY IMPROVE THE ROD HEAT TRANSFER

- LOCAL FLOW BLOCKAGE HEAT TRANSFER EFFECTS AND FLOW BYPASS COUNTERACT EACH OTHER. APP K MAXIMIZES THE HEAT TRANSFER PENALTY FOR BLOCKAGE.

REVIEW OF FLOW BLOCKAGE HEAT TRANSFER DATA AND PROGRAMS

- ORIGINAL FLECHT
  - FORCED FLOW, PLATE BLOCKAGE (COPLANAR)
  - BLOCKED 16 RODS 100%, SAW IMPROVED HEAT TRANSFER RELATIVE TO UNBLOCKED
  - TESTS WITH BY PASS ALSO SHOWED HEAT TRANSFER IMPROVEMENT
  - TESTS WERE CRITICIZED BECAUSE
    - PLATE BLOCKAGE GEOMETRY
    - FORCED FLOW
    - INSUFFICIENT DATA LESS THAN 1"/SEC

- KWU BWR PARALLEL BUNDLE TESTS
  - FORCED FLOW, PLATE BLOCKAGE (COPLANAR)
  - BLOCKED 7X7 BUNDLE 37% AND 70% (LOCALLY 85%), OTHER BUNDLE WAS UNBLOCKED

- BLOCKED BUNDLE DATA SHOWED LOWER ΔT RISE (BETTER H) THAN UNBLOCKED TESTS FOR <u>ALL</u> CASES
- BLOCKED BUNDLE QUENCH TIMES WERE LONGER

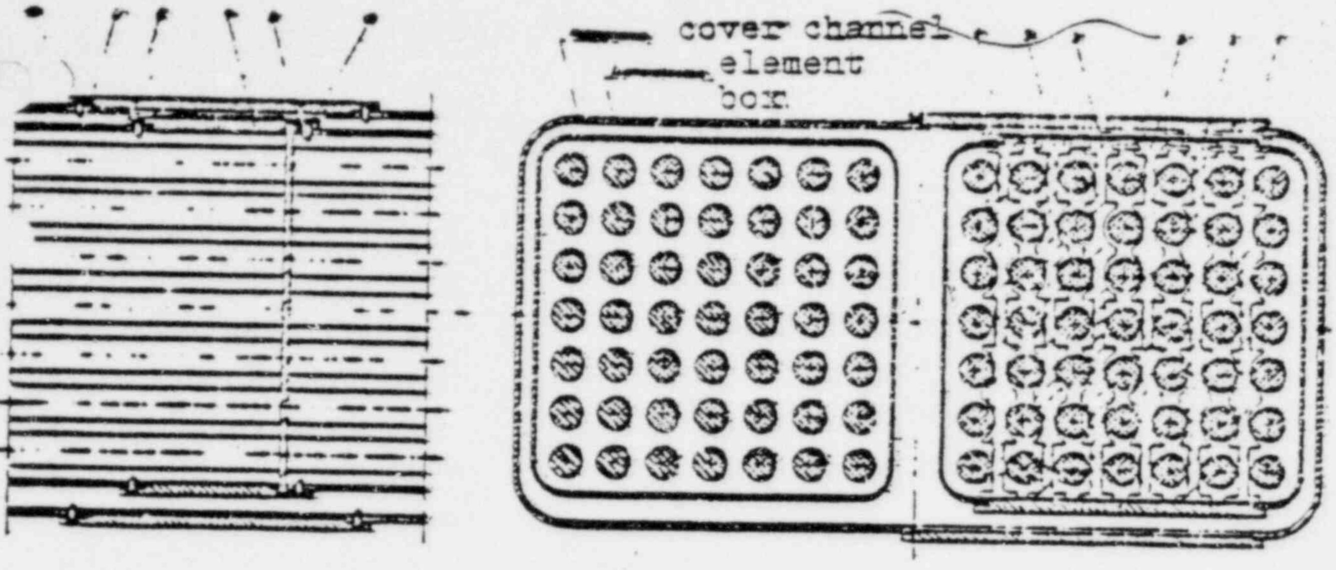


ABB. 3: QUERSCHNITT DER 1. BLOCKAGEFORM (VERSPERRUNG 37 %)

Illus. 3: cross-section of the first form of blockage (obstruction 37%)

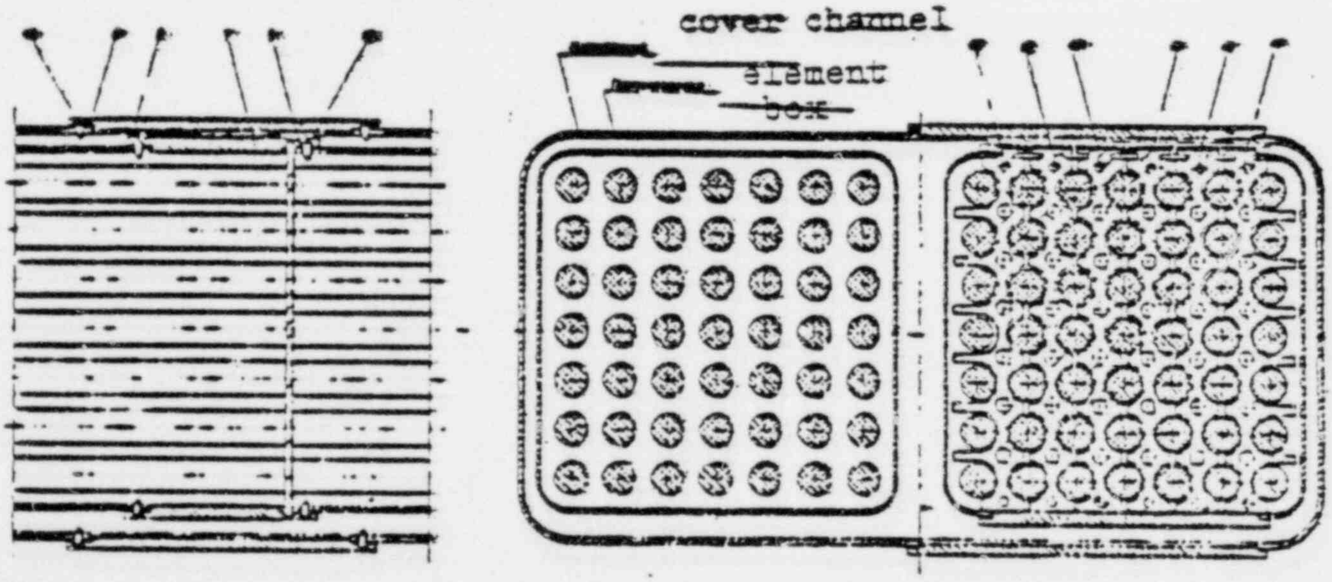


ABB. 4: QUERSCHNITT DER 2. BLOCKAGEFORM (VERSPERRUNG 70 %)

Illus. 4: cross-section of the second form of blockage (obstruction

Illus. 48: comparison of the heating-up ranges of all measuring of heating rod B 18 for three flood experiments with varying degree of blockage

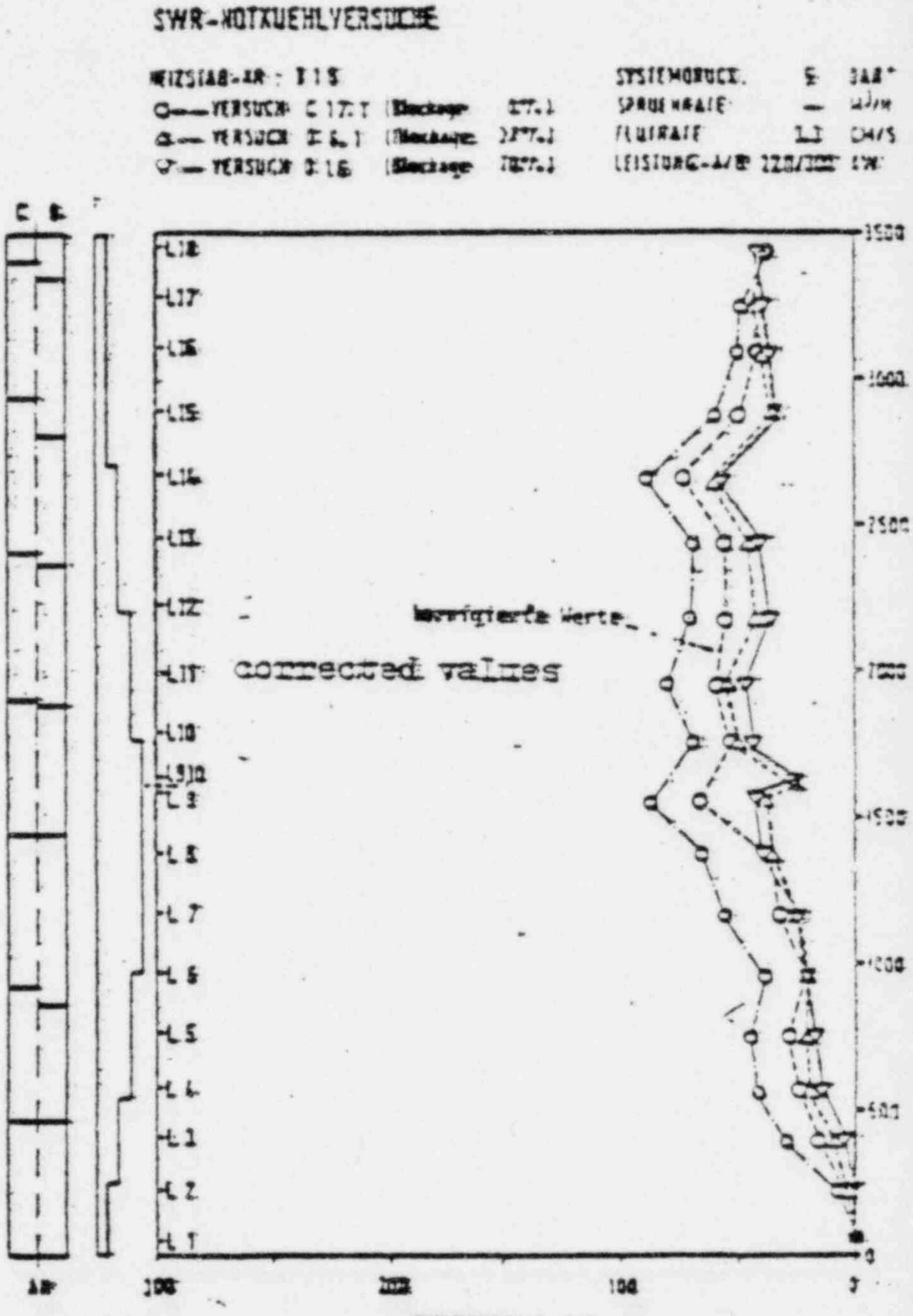


ABB. 48: GEGENÜBERSTELLUNG DER AUFHEIZSPANNEN ALLER MESSEBEN DES HEIZSTABES B 18 FÜR DREI FLUTVERSUCHE MIT UNTERSCHIEDLICHEM BLOCKAGEGRAD

Illus. 49: comparison of the heating-up ranges of all measuring levels of heating rod B 1 for three flood experiments with varying degree of blockage

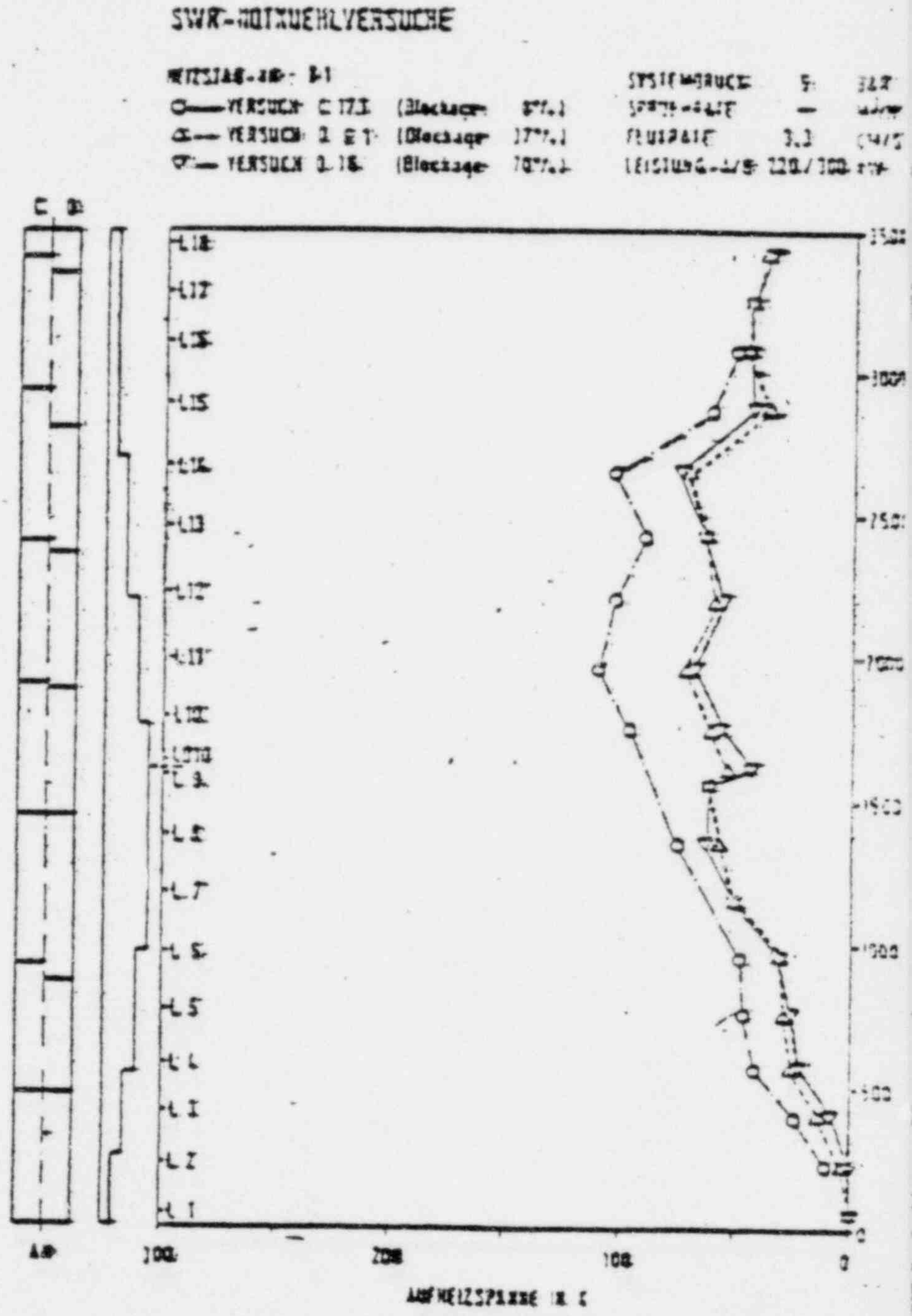


Abb. 49: Gegenüberstellung der Aufheizspannen aller Messebene des Heizstabes B 1 für drei Flutversuche mit unterschiedlichem Blockagegrad

- KFK FEBA TESTS
  - FORCED FLOW, PLATE AND/OR COPLANAR SLEEVE BLOCKAGE
  - 1x5 TESTS SHOWED SLEEVES GIVE LOWER HEAT TRANSFER IMPROVEMENT (OVER UNBLOCKED) THAN PLATE
  - 5x5 TESTS WITH 3x3 CORNER BLOCKED 90% SHOWS:

- IMPROVED HEAT TRANSFER RELATIVE TO UNBLOCKED THROUGH TURN AROUND TIME
- STEAM DESUPERHEATING FOR BLOCKED TESTS
- LOWER PCT'S FOR BLOCKAGE
- LONGER QUENCH TIMES FOR BLOCKED

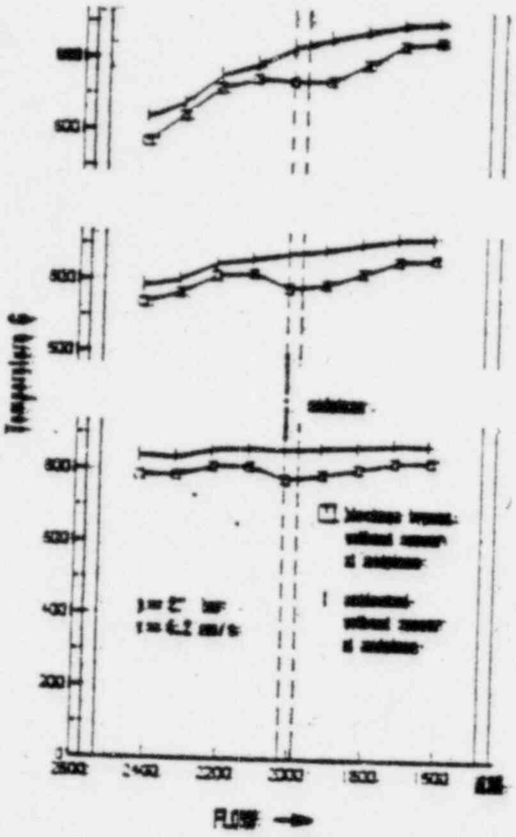
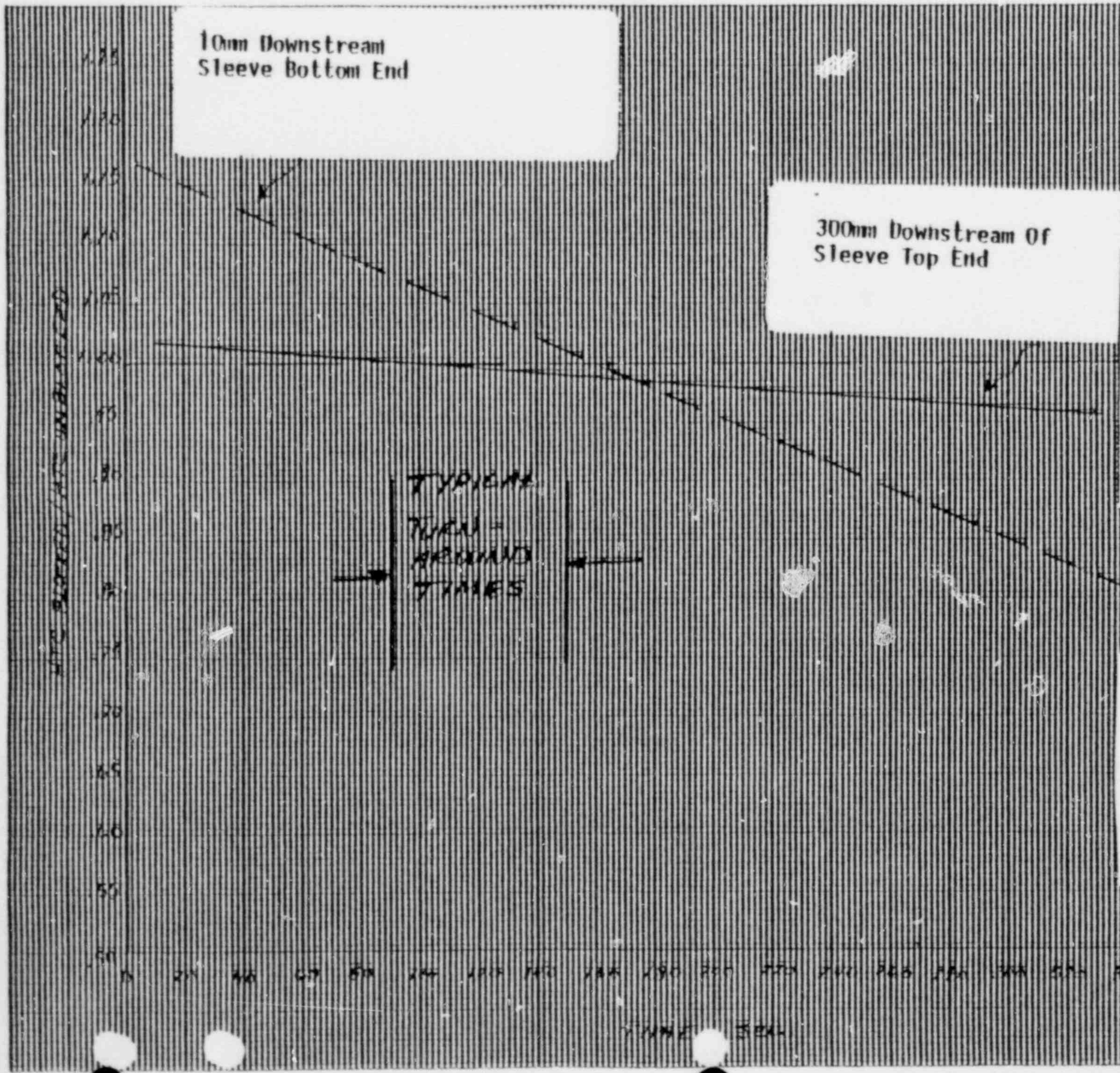


Fig. 4 Comparison of axial temperature profiles, blockage bypass - unblocked without spacer at midplane, series III/II



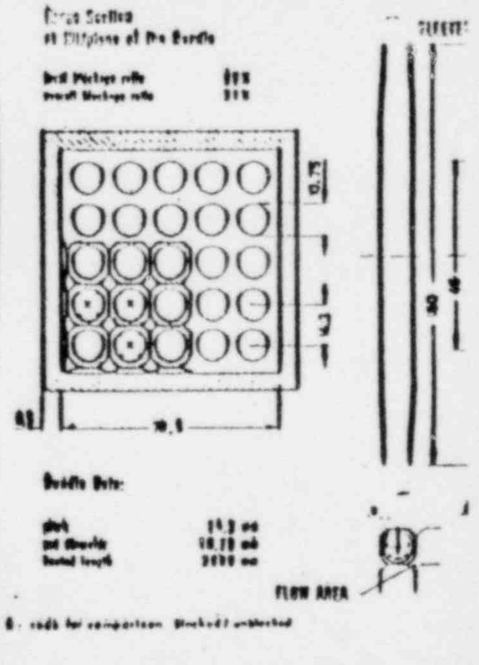


Fig. 1 Array of the 90 % blockage achieved wit
sleeves

TABLE 1

Parameters of the Tests Compared

Series	T0(C)	t_w(C)	v(cm/s)	p(bar)	Test ru
I	773	40	3.4	2.2	223
II	770	40	3.4	2.0	236
III	784	40	3.4	2.0	241
I	760	40	3.2	2.1	218
II	791	40	3.2	2.0	233
III	783	40	3.2	1.9	240
I	799	40	3.4	0.2	220
III	813	40	3.4	0.2	236

- W/NRC/EPRI FLECHT-SEASET FLOW BLOCKAGE PROGRAM
  - PROGRAM HAS BEEN SPECIFICALLY STRUCTURED BY ALL PARTIES TO ADDRESS APP. K STEAM COOLING-FLOW BLOCKAGE BY PROVIDING APPROPRIATE DATA AND ANALYSIS
  - DIFFERENT BLOCKAGE SHAPES CHARACTERISTIC OF α-BURST (LONG NON-CONCENTRIC) AND β-BURST (SHORT CONCENTRIC) WILL BE TESTED
  - BOTH COPLANAR AND NON-COPLANAR BLOCKAGE DISTRIBUTIONS WILL BE TESTED
  - A LARGE (161-ROD) BUNDLE WITH AMPLE FLOW BYPASS WILL ALSO BE TESTED.

- 21-ROD BUNDLE PROGRAM WILL TEST:

A- UNBLOCKED REFERENCE

B- 9 RODS BLOCKED COPLANAR, 62%, SHORT CONCENTRIC SLEEVE

C- 21 RODS BLOCKED COPLANAR, 62%, SHORT CONCENTRIC SLEEVE

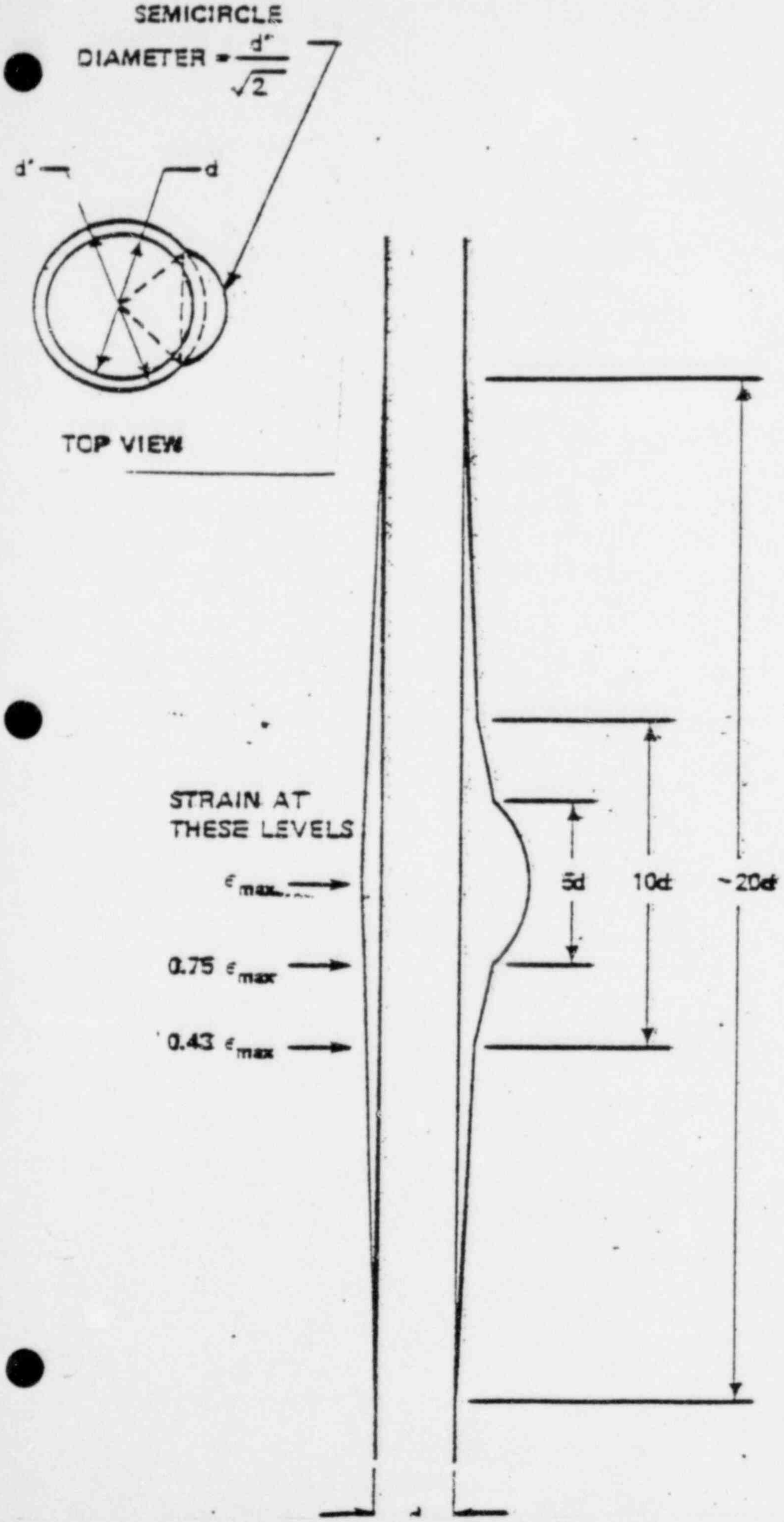
B- 21 RODS BLOCKED NON-COPLANAR, SHORT CONCENTRIC SLEEVE

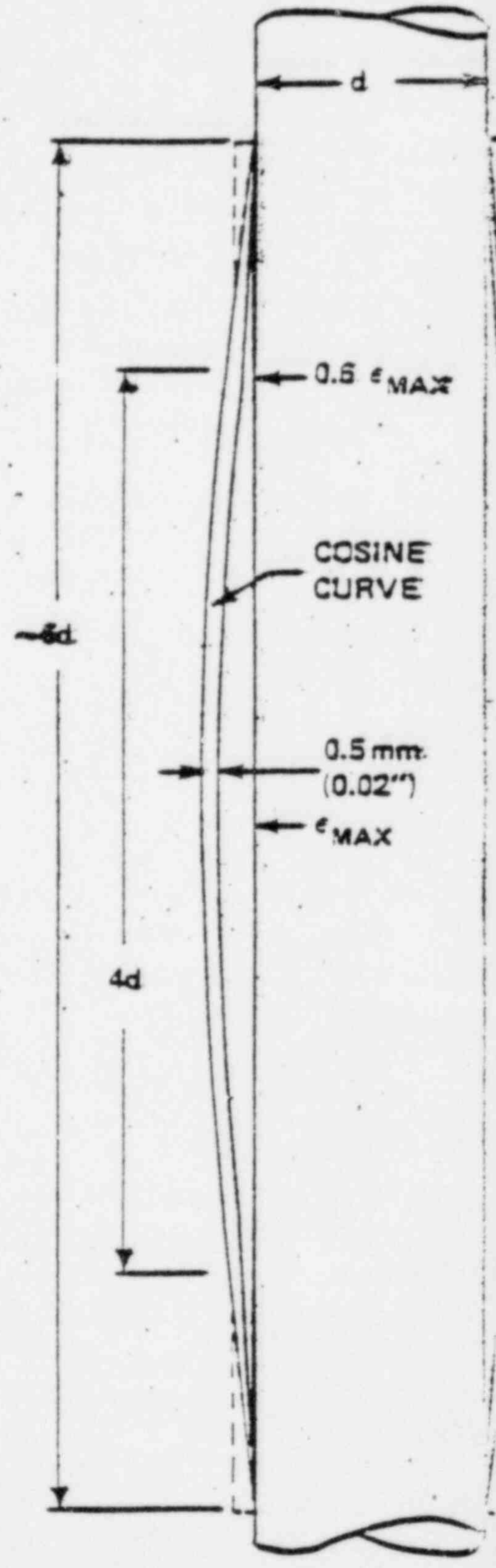
E- 21 RODS BLOCKED NON-COPLANAR, LONG NON-CONCENTRIC SLEEVE

F- THE WORST SHAPE, NON-COPLANAR, MORE STRAIN

G- TO BE DETERMINED

  - 161-ROD BUNDLE WILL BLOCK TWO 21-ROD BUNDLE ISLAND WITH WORST SHAPE DETERMINED FROM 21-ROD BUNDLE. TWO TEST SERIES WILL BE PERFORMED.


SEMICIRCLE
DIAMETER = $\frac{d'}{\sqrt{2}}$
d'
d
TOP VIEW
STRAIN AT THESE LEVELS
ϵ_{max}
0.75 ϵ_{max}
0.43 ϵ_{max}
5d
10d
~20d


d
0.6 ϵ_{MAX}
COSINE CURVE
~6d
0.5 mm (0.02")
ϵ_{MAX}
4d

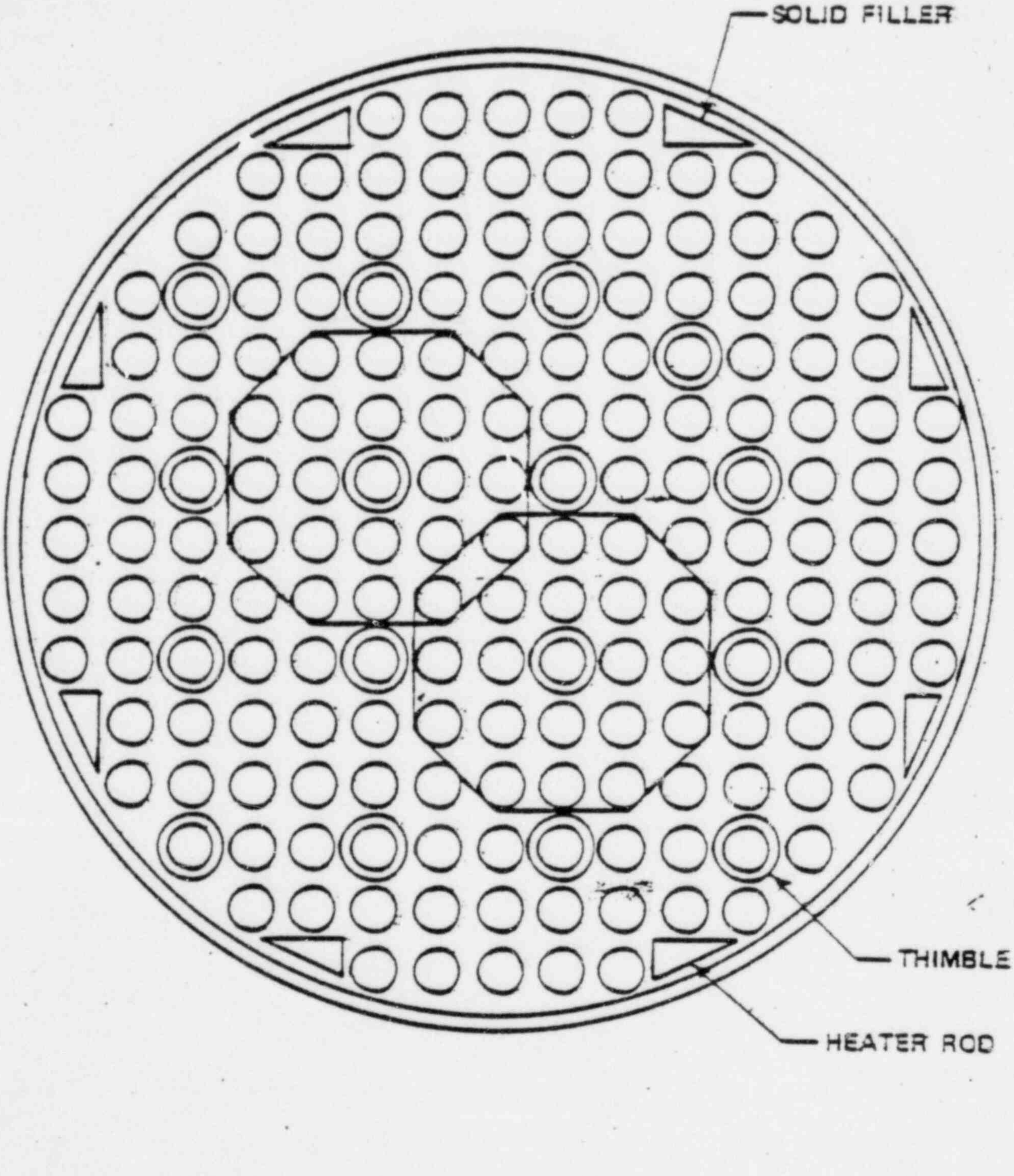


Figure 4-16. 21-Rod Islands in 161-Rod Bundle

14

- FLECHT-SEASET 21 ROD BUNDLE COMPARISONS OF BUNDLE 1(A) UNBLOCKED REFERENCE AND BUNDLE 2(B), CENTER 3X3 BLOCKED 62% WITH SHORT CONCENTRIC SLEEVES.

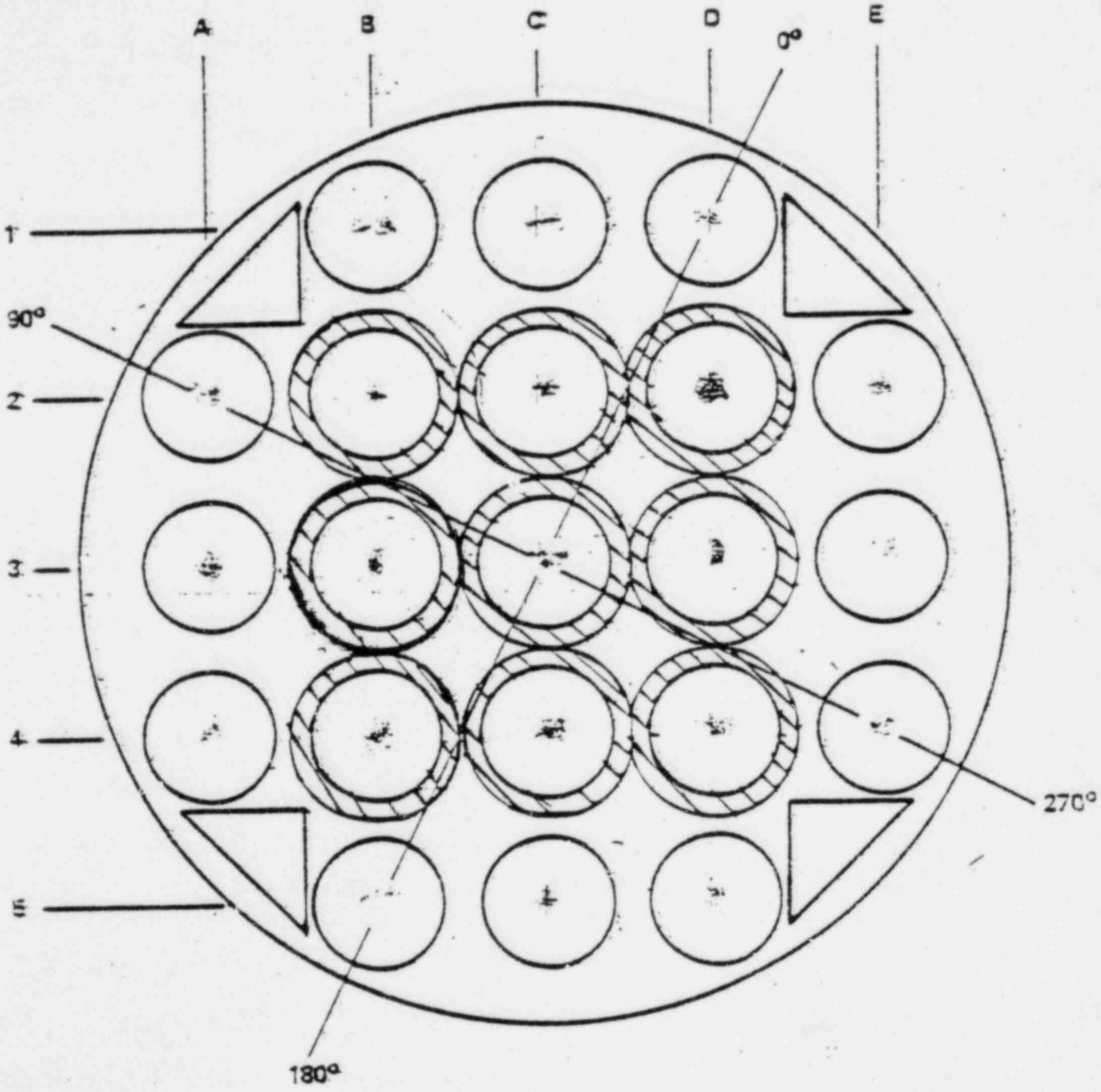
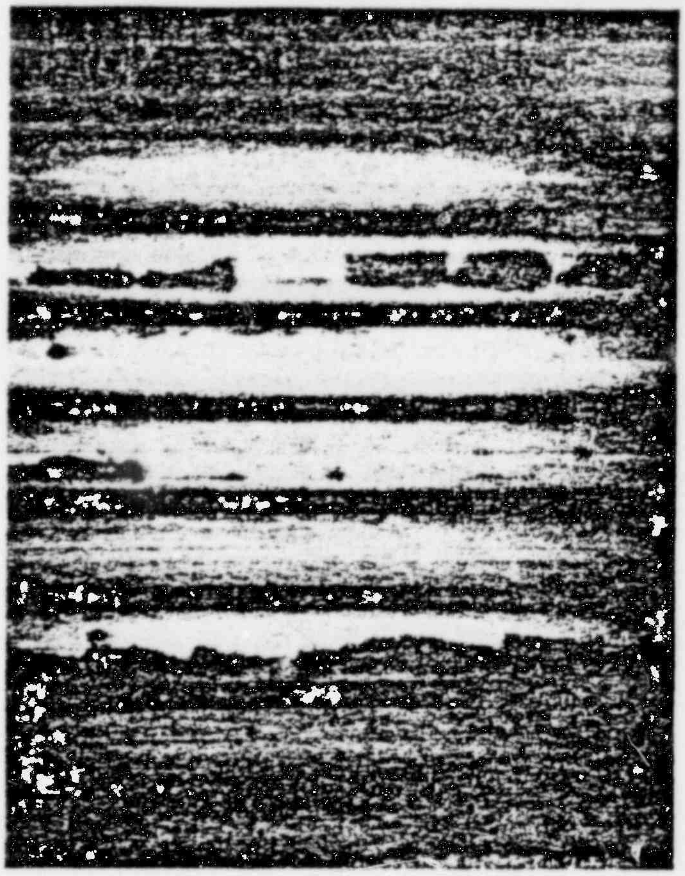
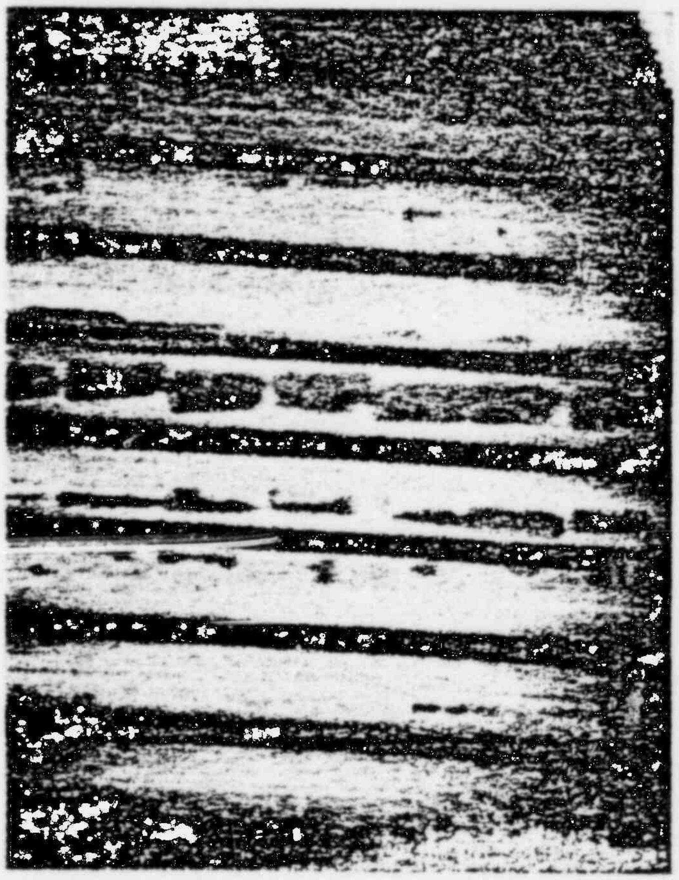


Figure H-1. Radial Distribution of Heater Rods





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Hoekreiter
Tape 8

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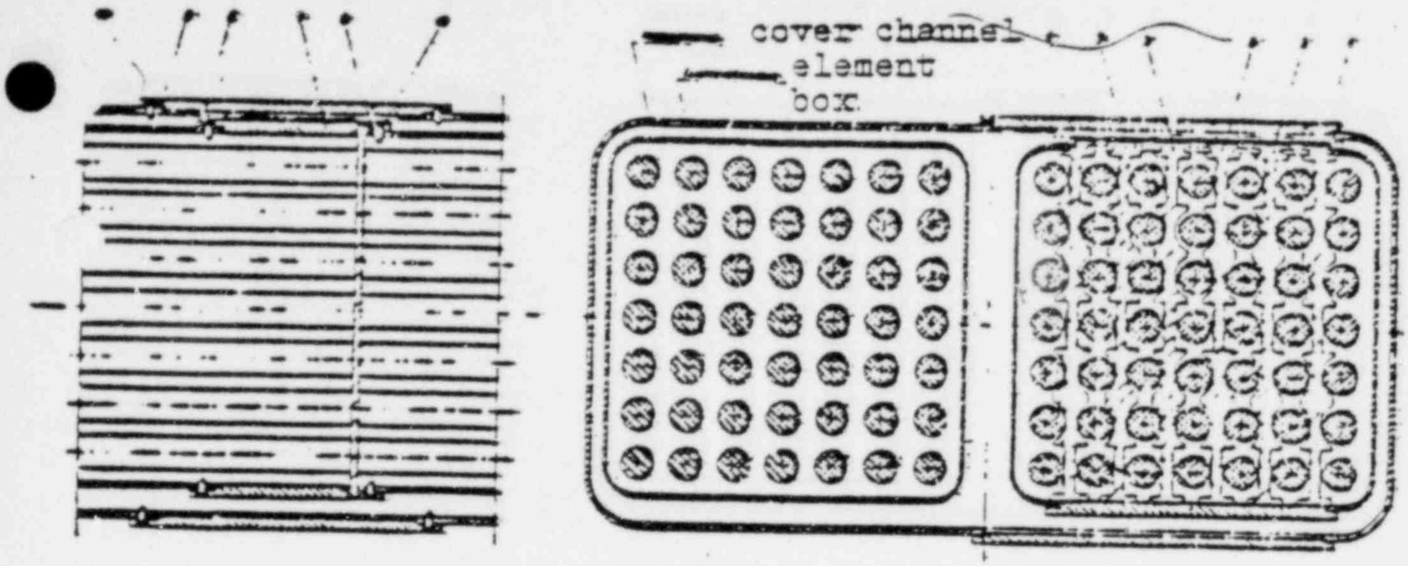


ABB. 3: QUERSCHNITT DER 1. BLOCKAGEFORM (VERSPERRUNG 37 %)

Illus. 3: cross-section of the first form of blockage (obstruction 37%)

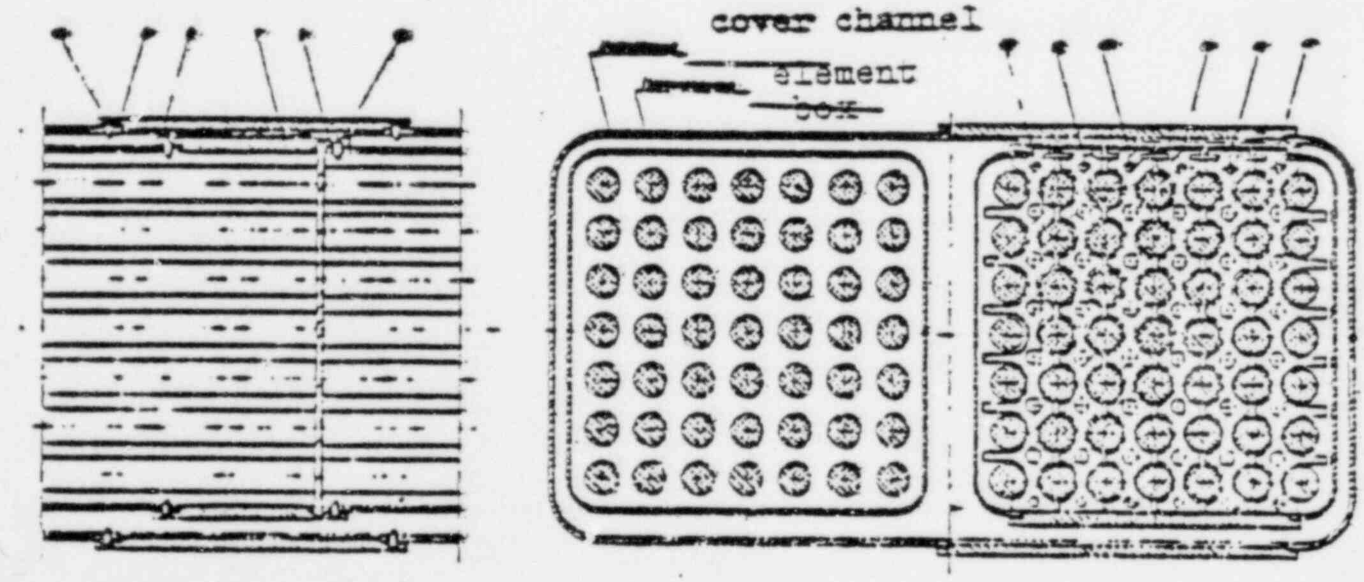


ABB. 4: QUERSCHNITT DER 2. BLOCKAGEFORM (VERSPERRUNG 70 %)

Illus. 4: cross-section of the second form of blockage (obstruction

Illus. 48: comparison of the heating-up ranges of all measuring
of heating rod B 18 for three flood experiments with
varying degree of blockage

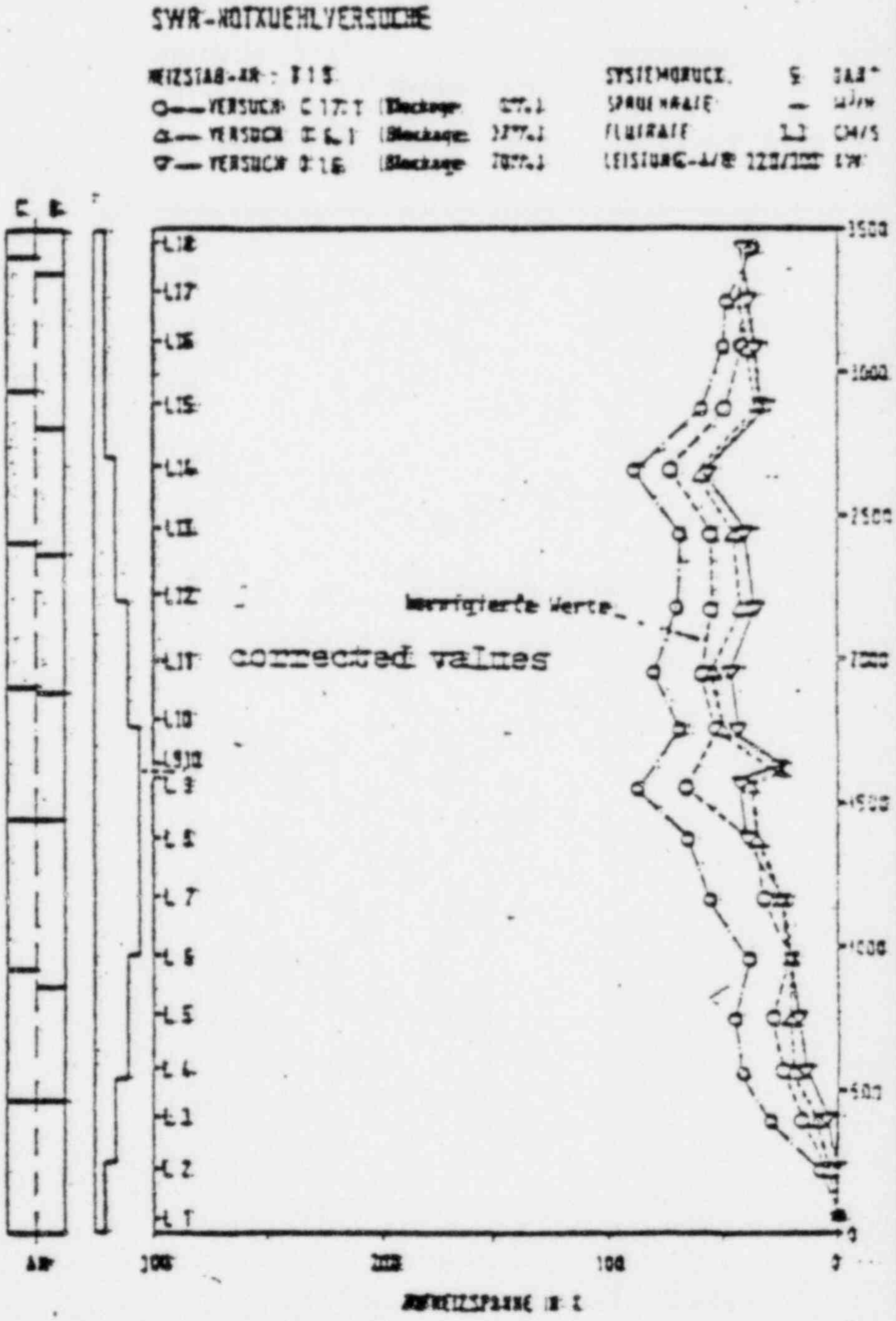


Abb. 48: Gegenüberstellung der Aufheizspannen aller Messeben des Heizstabes B 18 für drei Flutversuche mit unterschiedlichem Blockagegrad

Illus. 49: comparison of the heating-up ranges of all measuring levels of heating rod B 1 for three flood experiments with varying degree of blockage

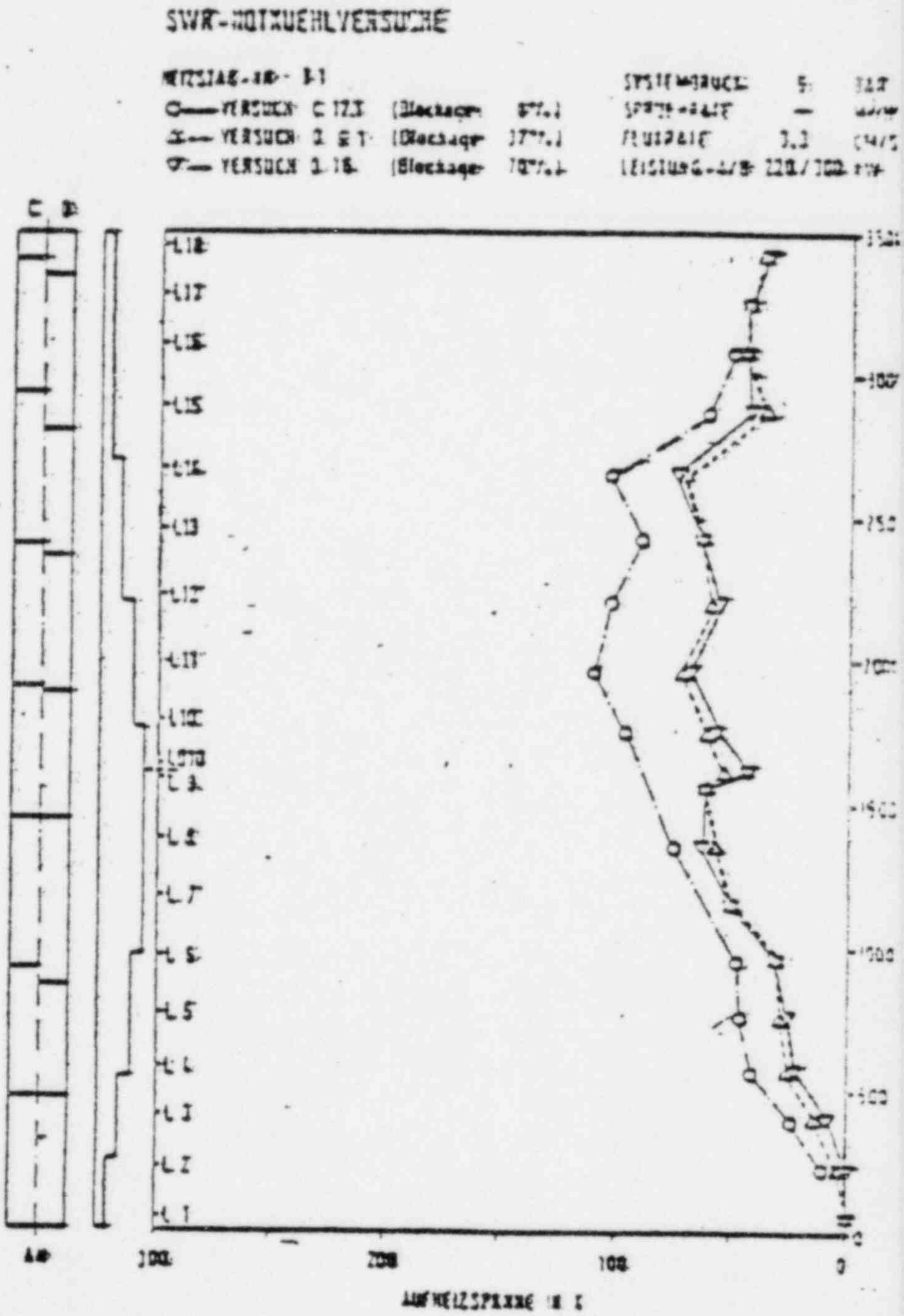


ABB. 49: GEGENUBERSTELLUNG DER AUFHEIZSPANNEN ALLER MESSEB
DES HEIZSTABES B 1 FÜR DREI FLUTVERSUCHE MIT UNTERSCHIEDLICHEM BLOCKAGEGRAD

7

- KFK FEBA TESTS
    - FORCED FLOW, PLATE AND/OR COPLANAR SLEEVE BLOCKAGE
    - 1x5 TESTS SHOWED SLEEVES GIVE <u>LOWER</u> HEAT TRANSFER IMPROVEMENT (OVER UNBLOCKED) THAN PLATE
    - 5x5 TESTS WITH 3x3 CORNER BLOCKED 90% SHOWS:

- IMPROVED HEAT TRANSFER RELATIVE TO UNBLOCKED THROUGH TURN AROUND TIME
- STEAM DESUPERHEATING FOR BLOCKED TESTS
- LOWER PCT'S FOR BLOCKAGE
- LONGER QUENCH TIMES FOR BLOCKED

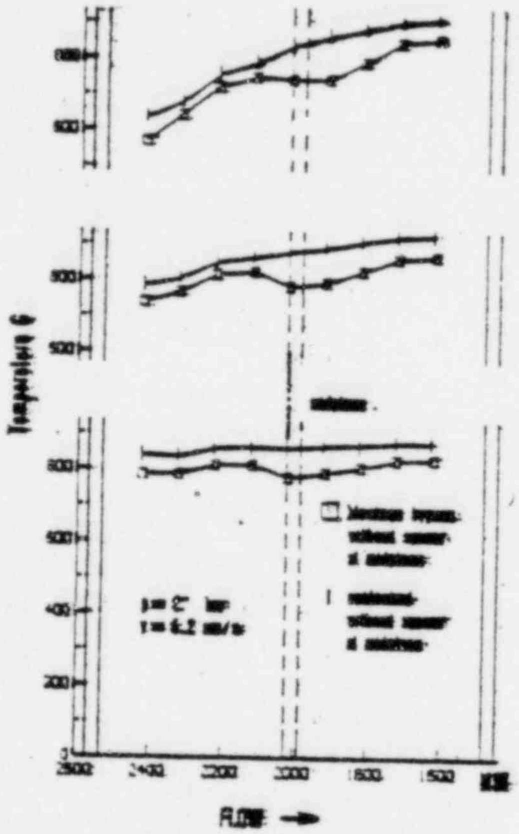
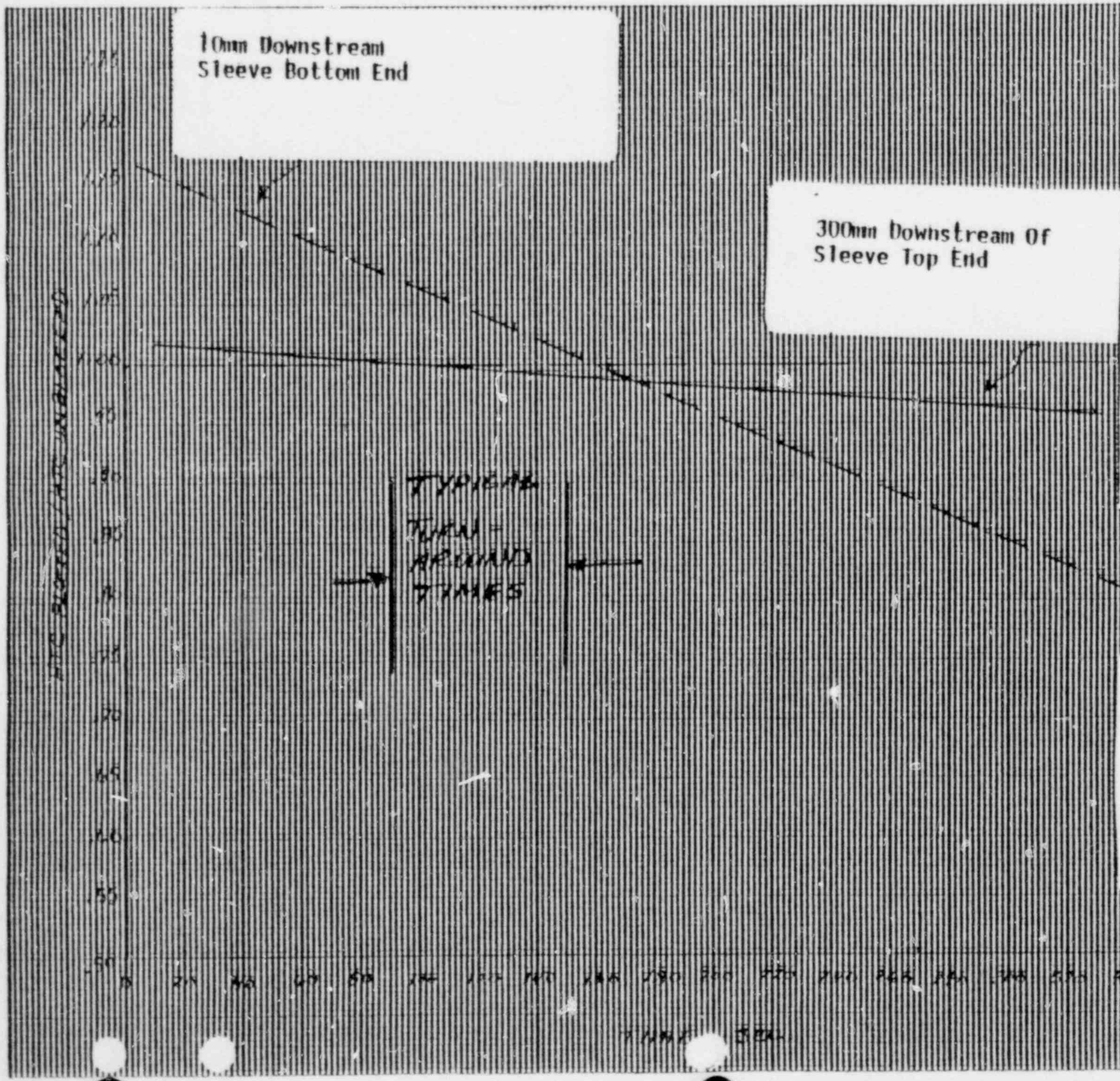
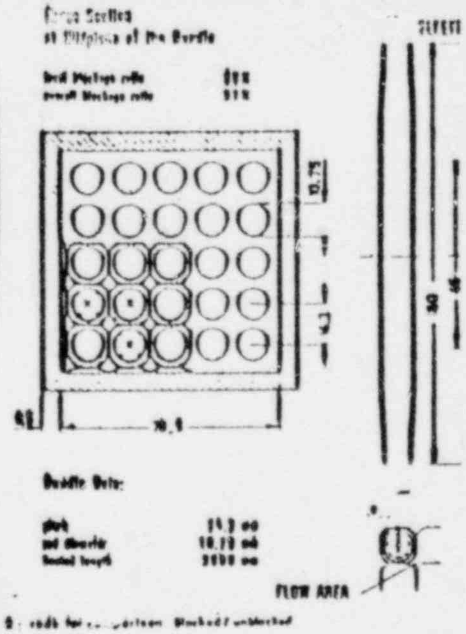


Fig. 4 Comparison of axial temperature profiles, blockage bypass - unblocked without spacer at midplane, series III/II





Bundle Data:

pitch 14.3 mm
rod diameter 10.75 mm
heated length 3900 mm

⊗ : rods for comparison blocked / unblocked

Fig. 1 Array of the 90 % blockage achieved wt sleeves

TABLE 1

Parameters of the Tests Compared

Series	TB(C)	t_w(C)	v(cm/s)	p(bar)	test r
I	773	40	3.4	2.2	223
II	778	40	3.4	2.0	234
III	784	40	3.4	2.0	241
I	750	40	3.2	2.1	218
II	791	40	3.2	2.0	233
III	783	40	3.2	1.9	240
I	799	40	3.4	0.3	220
III	813	40	3.4	0.3	236

- W/NRC/EPRI FLECHT-SEASET FLOW BLOCKAGE PROGRAM
  - PROGRAM HAS BEEN SPECIFICALLY STRUCTURED BY ALL PARTIES TO ADDRESS APP. K STEAM COOLING-FLOW BLOCKAGE BY PROVIDING APPROPRIATE DATA AND ANALYSIS
  - DIFFERENT BLOCKAGE SHAPES CHARACTERISTIC OF α-BURST (LONG NON-CONCENTRIC) AND β-BURST (SHORT CONCENTRIC) WILL BE TESTED
  - BOTH COPLANAR AND NON-COPLANAR BLOCKAGE DISTRIBUTIONS WILL BE TESTED
  - A LARGE (161-ROD) BUNDLE WITH AMPLE FLOW BYPASS WILL ALSO BE TESTED.

- 21-ROD BUNDLE PROGRAM WILL TEST:

A- UNBLOCKED REFERENCE

B- 9 RODS BLOCKED COPLANAR, 62%, SHORT CONCENTRIC SLEEVE

C- 21 RODS BLOCKED COPLANAR, 62%, SHORT CONCENTRIC SLEEVE

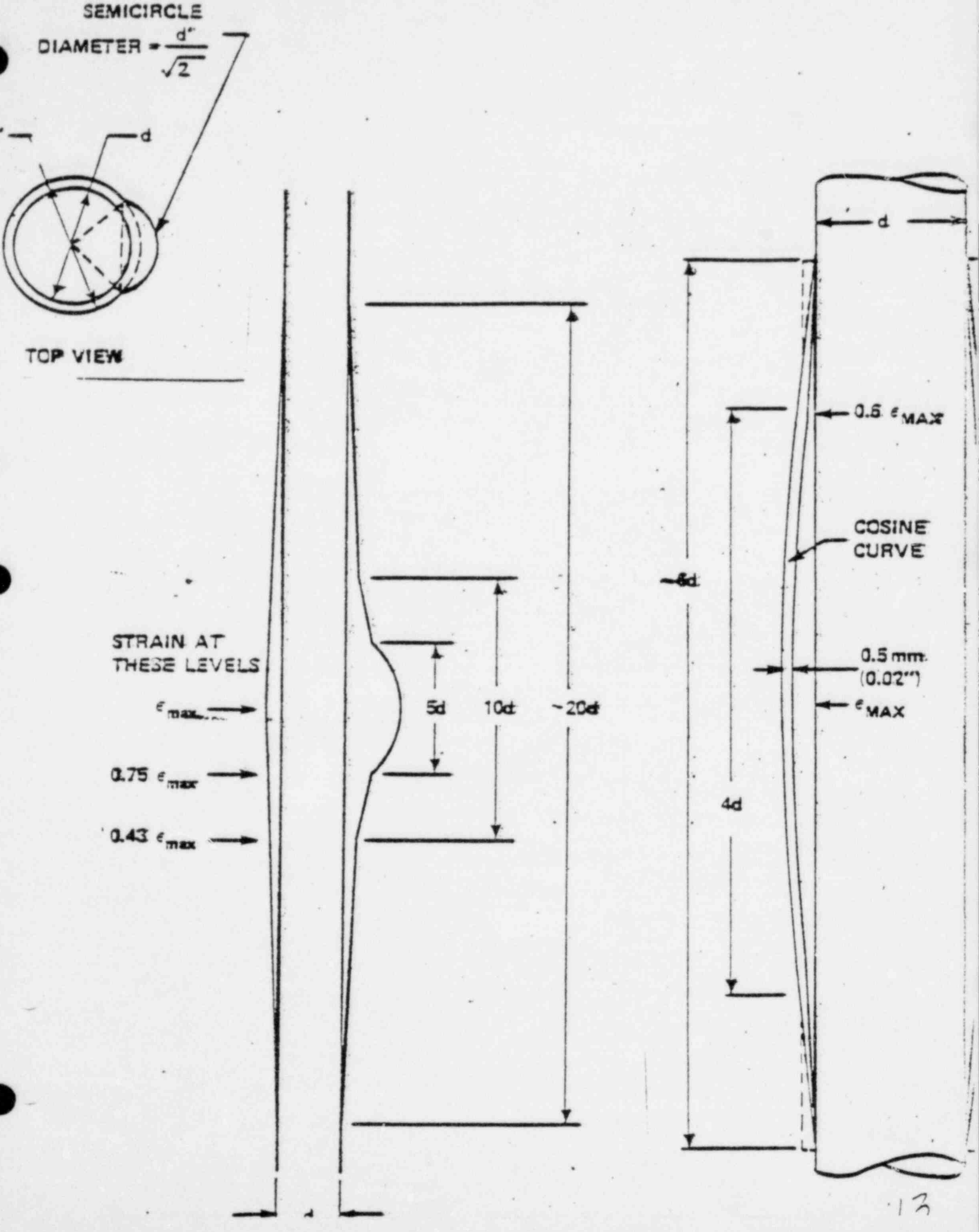
D- 21 RODS BLOCKED NON-COPLANAR, SHORT CONCENTRIC SLEEVE

E- 21 RODS BLOCKED NON-COPLANAR, LONG NON-CONCENTRIC SLEEVE

F- THE WORST SHAPE, NON-COPLANAR, MORE STRAIN

G- TO BE DETERMINED

  - 161-ROD BUNDLE WILL BLOCK TWO 21-ROD BUNDLE ISLAND WITH WORST SHAPE DETERMINED FROM 21-ROD BUNDLE. TWO TEST SERIES WILL BE PERFORMED.


SEMICIRCLE
DIAMETER = $\frac{d'}{\sqrt{2}}$
d'
d
TOP VIEW
STRAIN AT THESE LEVELS
ϵ_{max}
0.75 ϵ_{max}
0.43 ϵ_{max}
5d
10d
~20d
d
d
~6d
0.6 ϵ_{MAX}
COSINE CURVE
0.5 mm (0.02")
ϵ_{MAX}
4d

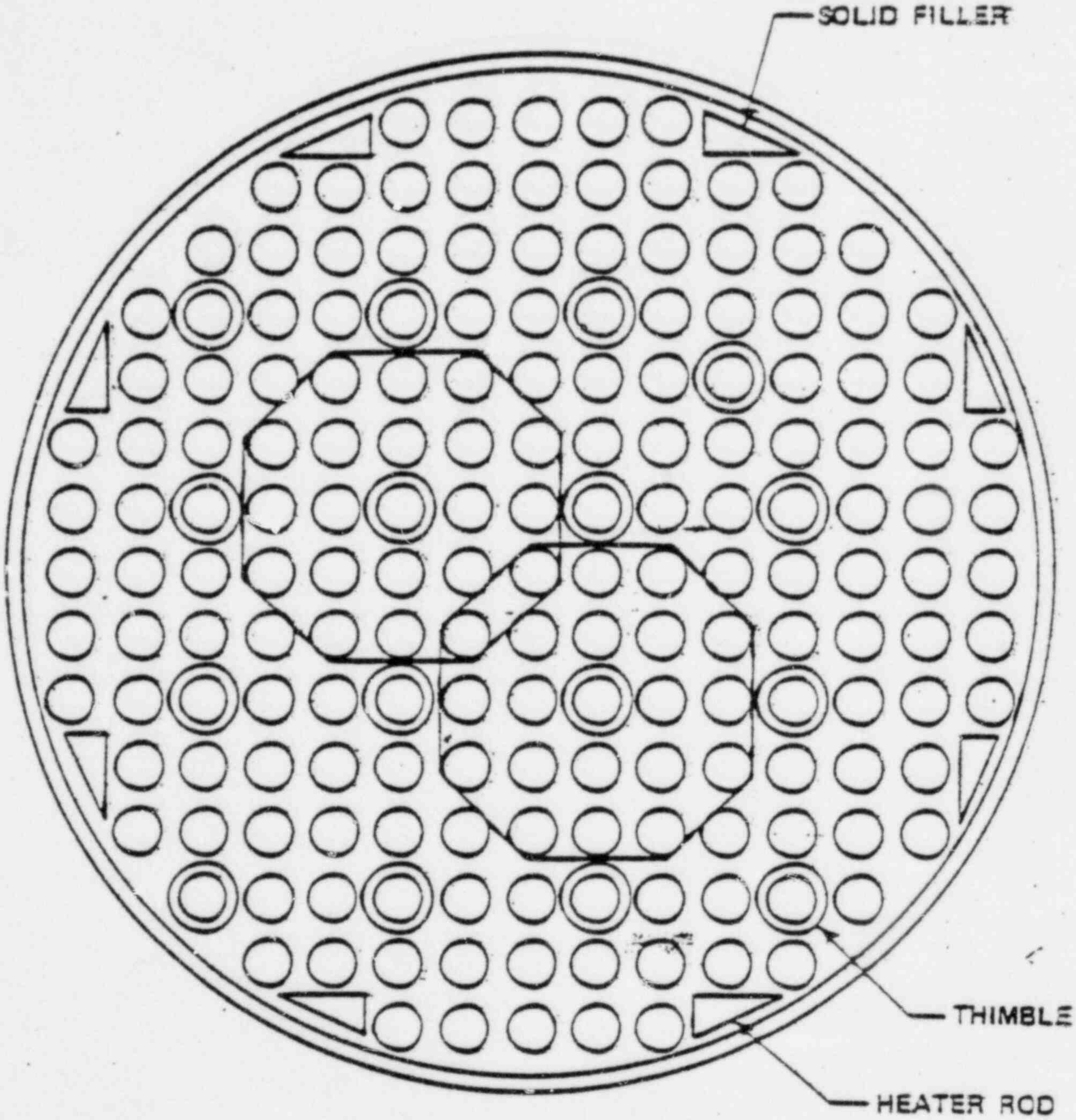


Figure 4-16. 21-Rod Islands in 161-Rod Bundle

- FLECHT-SEASET 21 ROD BUNDLE COMPARISONS OF BUNDLE 1(A) UNBLOCKED REFERENCE AND BUNDLE 2(B), CENTER 3X3 BLOCKED 62% WITH SHORT CONCENTRIC SLEEVES.

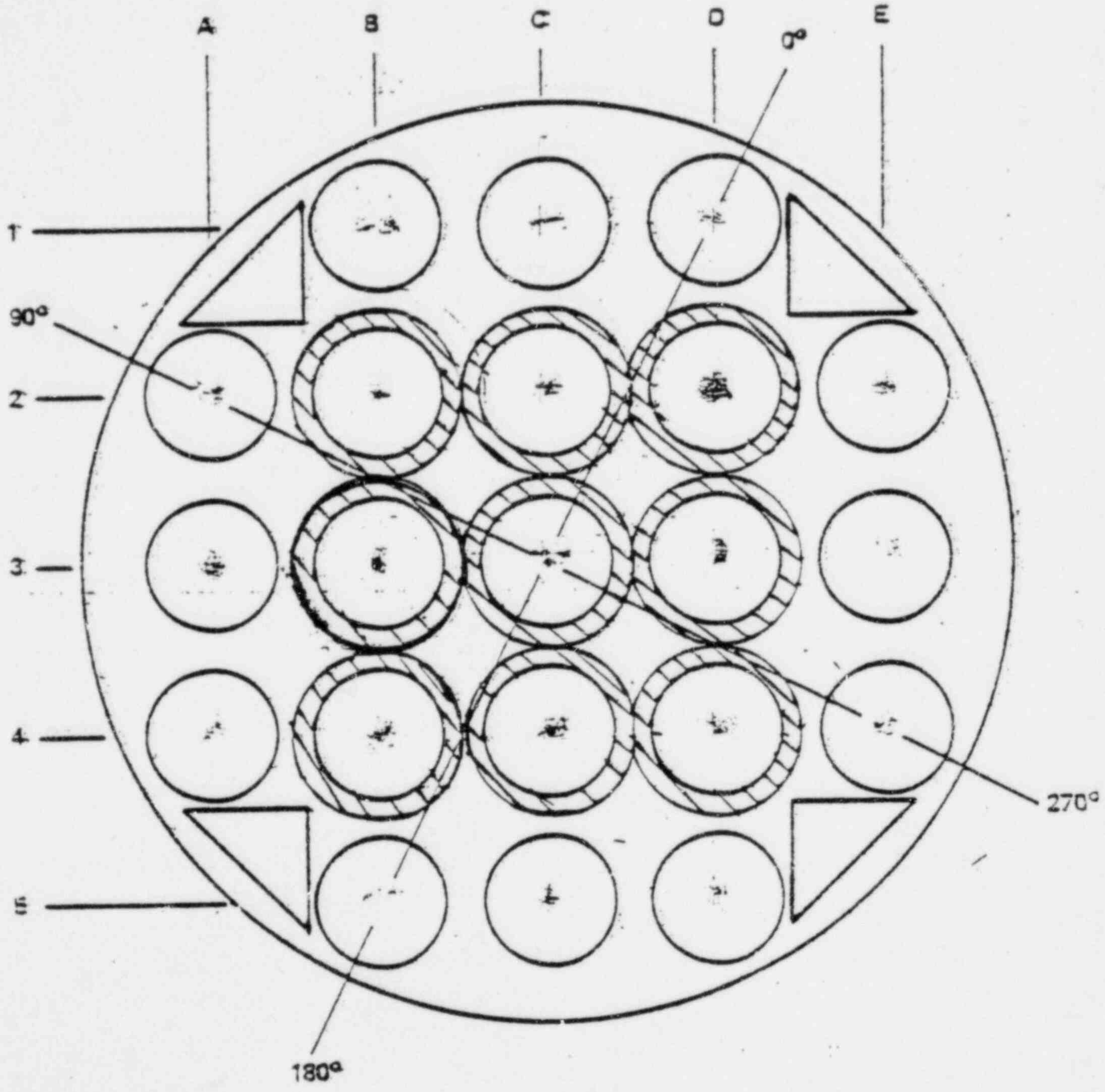
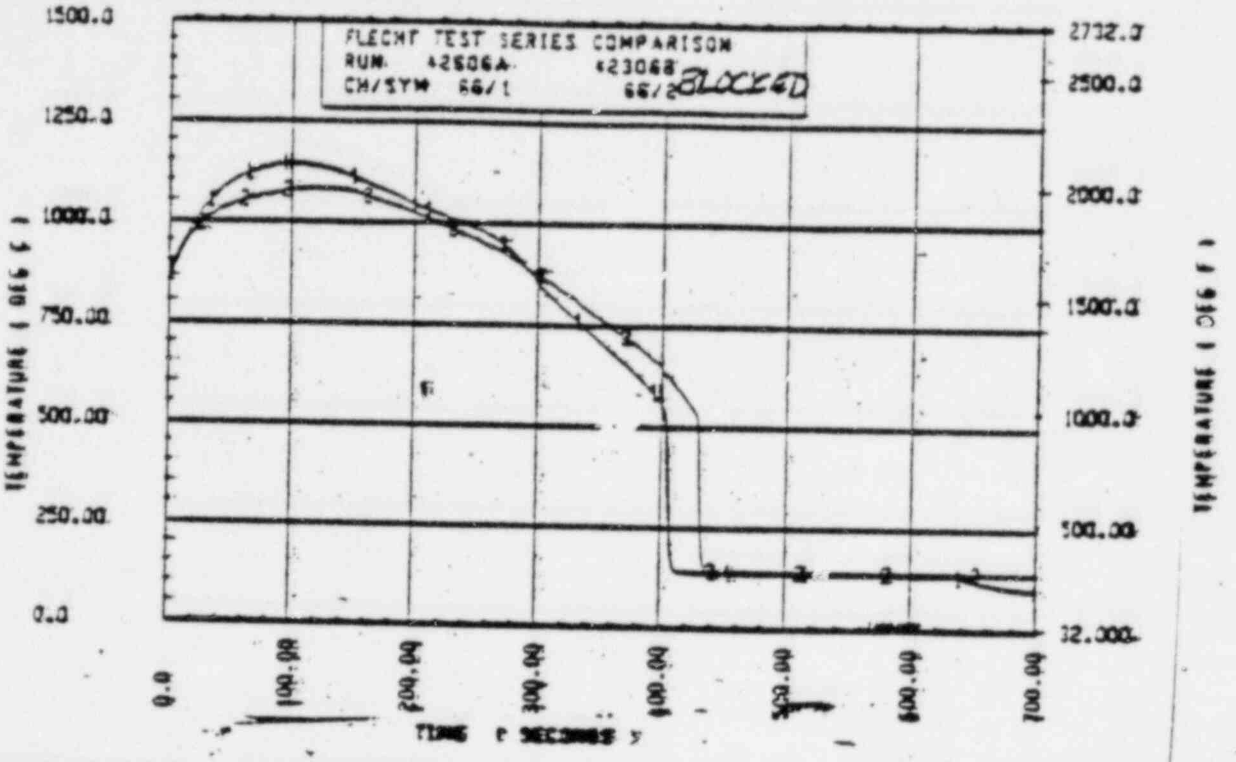
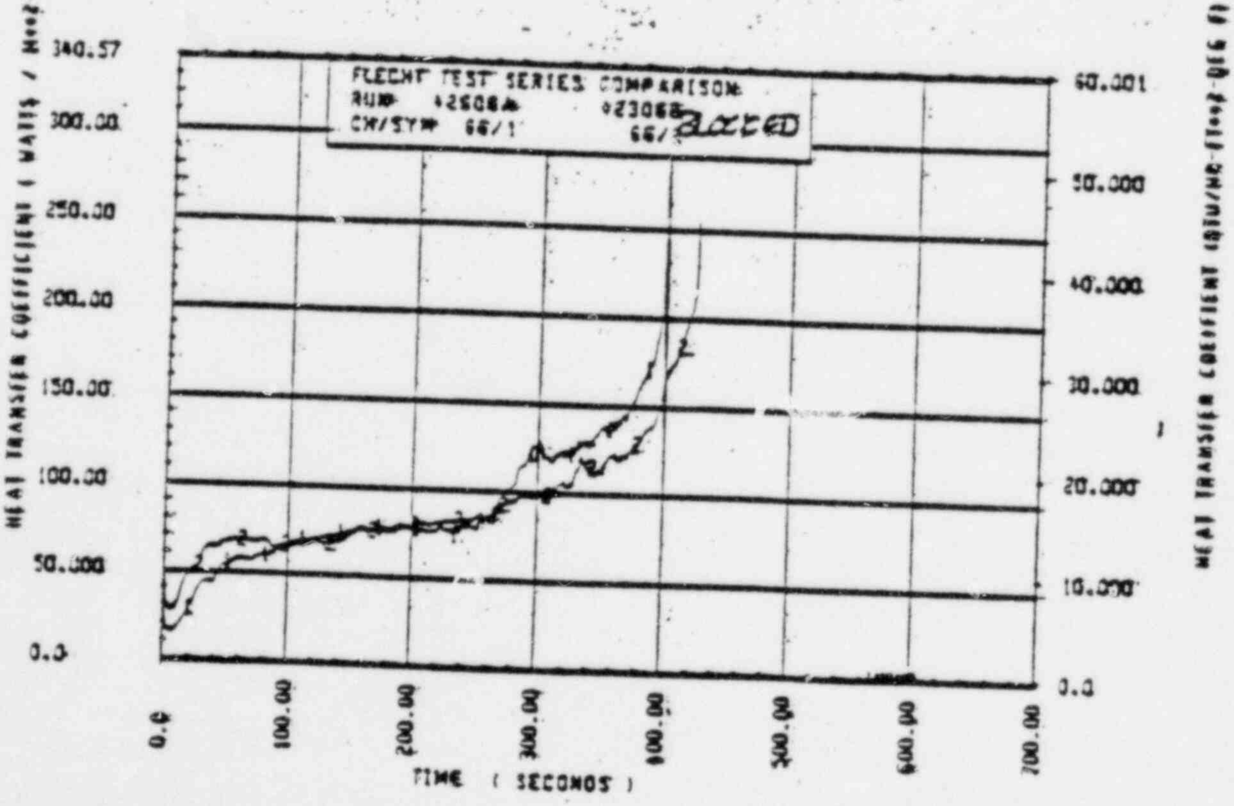


Figure H-1. Radial Distribution of Heater Rods

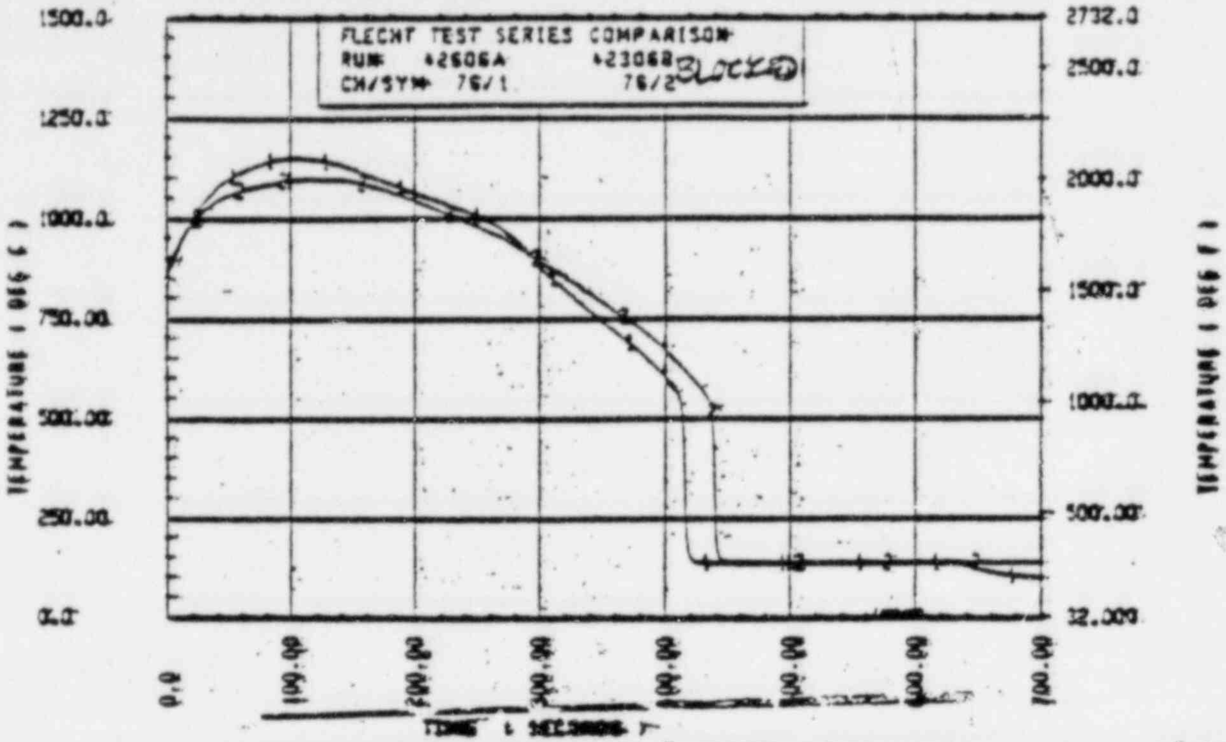
16

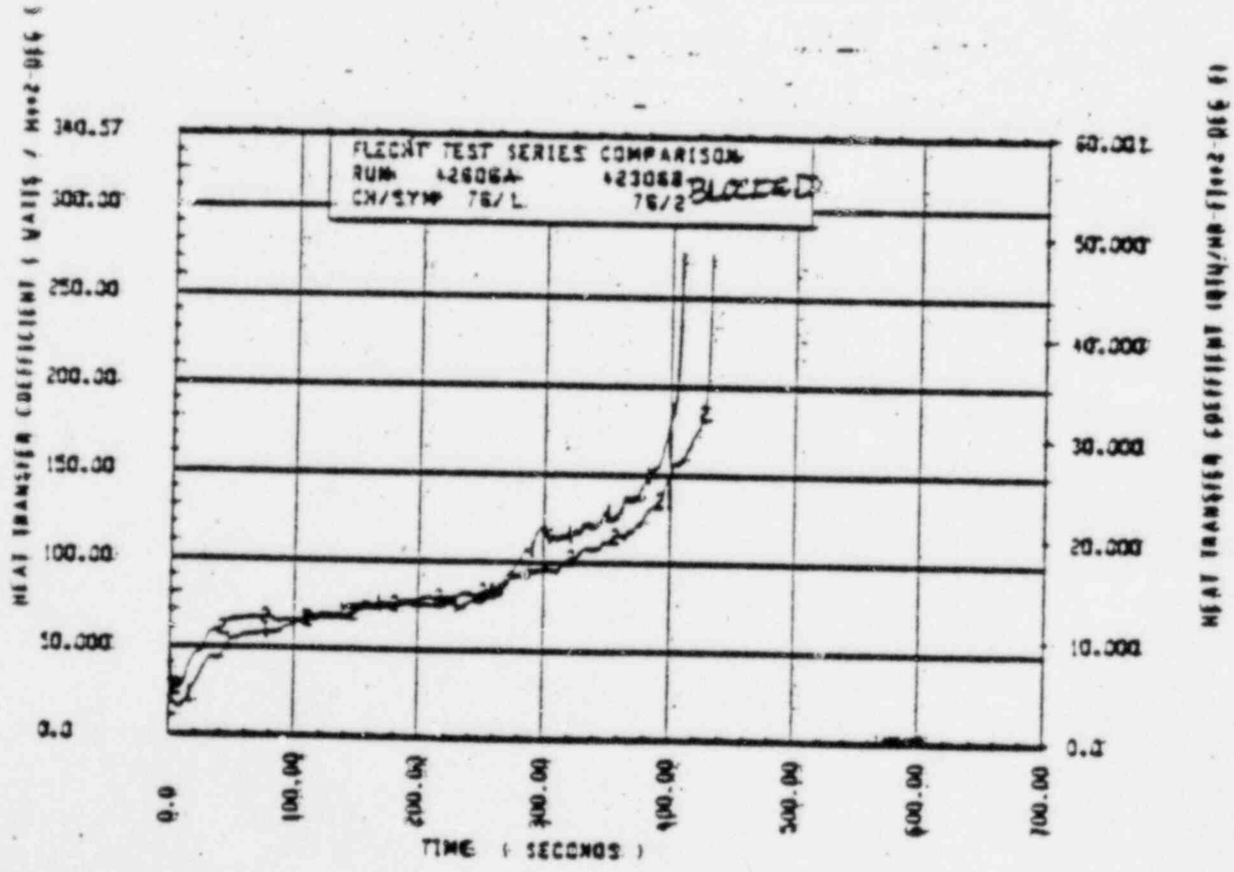


FLECHT TEST SERIES COMPARISON
RUN 42506A 42306B
CH/SYM 66/1 66/2
BLOCKED
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
TEMPERATURE (DEG C)
2732.0
2500.0
2000.0
1500.0
1000.0
500.00
32.000
TEMPERATURE (DEG F)
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)



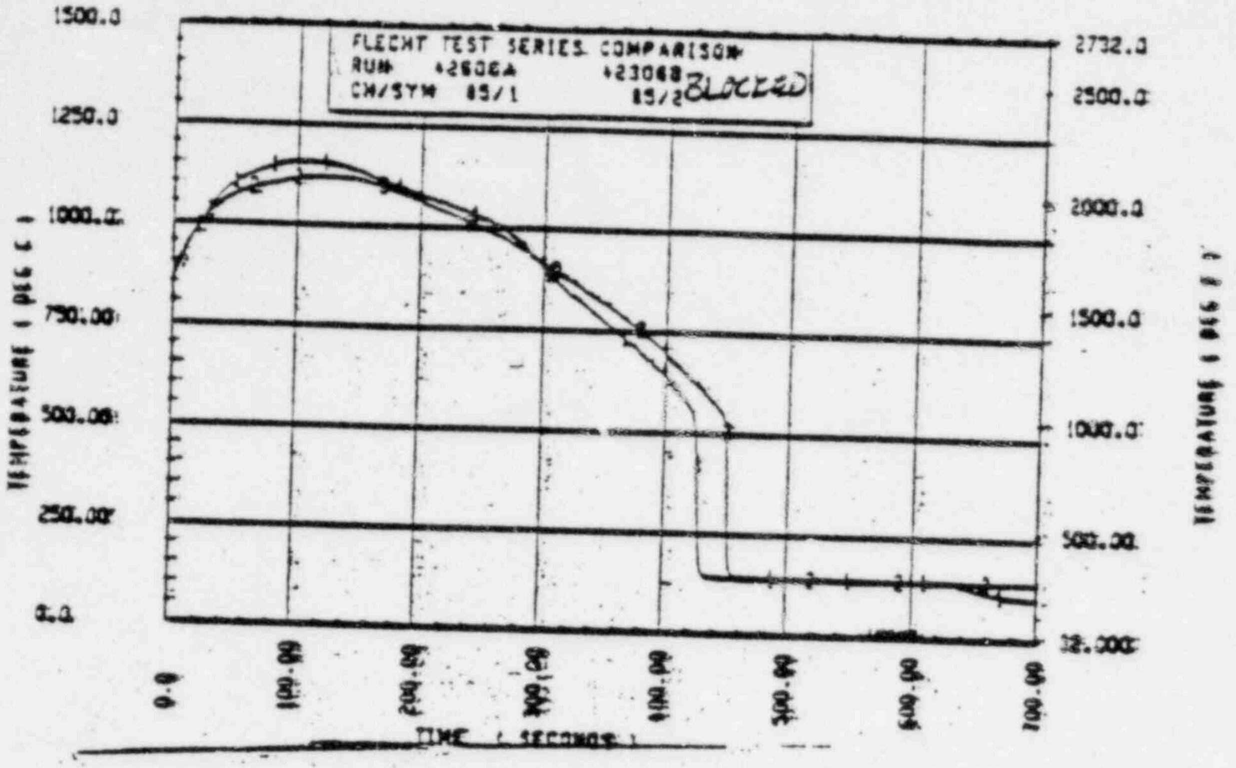
FLECHT TEST SERIES COMPARISON
RUN 42506A 42306B
CH/SYM 66/1 66/2
BLOCKED
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG C)
60.001
50.000
40.000
30.000
20.000
10.000
0.0
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)

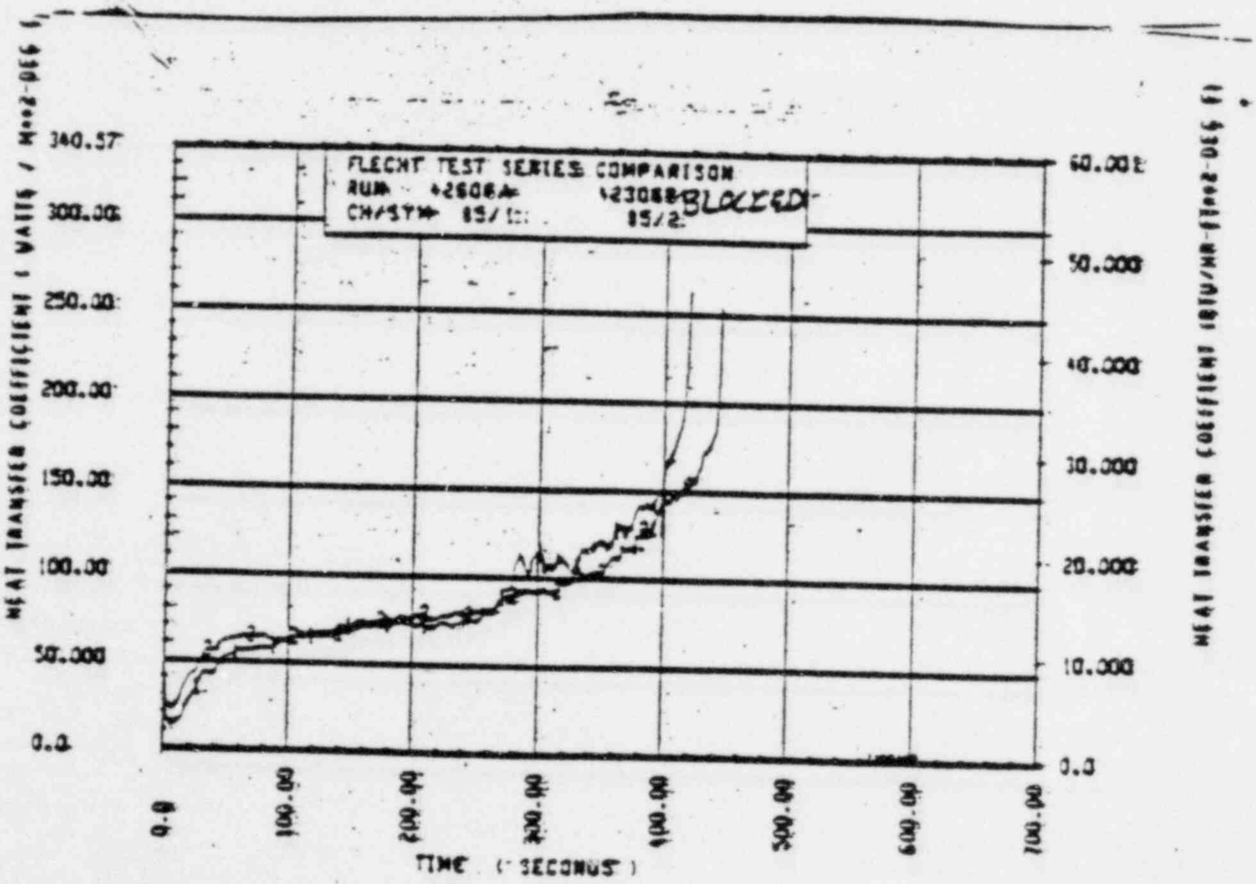

FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B BLOCKED
CH/SYM 76/1 76/2
TEMPERATURE (DEG C)
TEMPERATURE (DEG F)
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
2732.0
2500.0
2000.0
1500.0
1000.0
500.00
32.000
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)

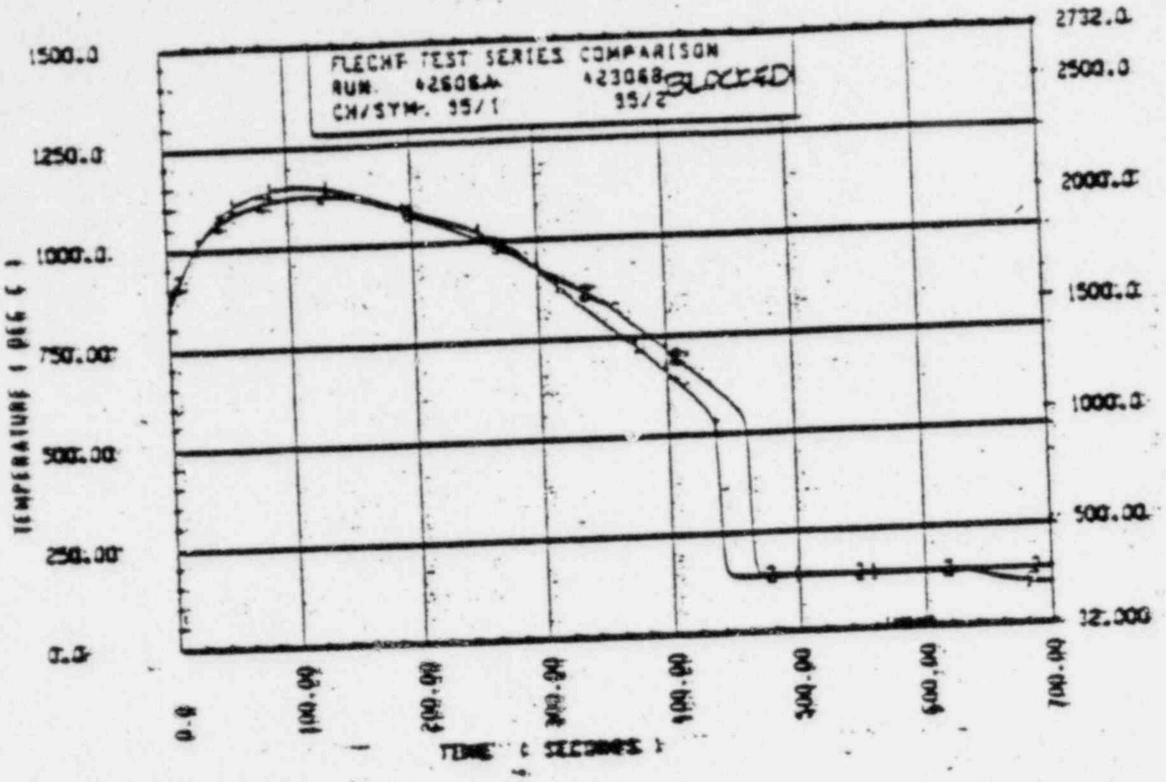

FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B BLOCKED
CH/SYM 76/1 76/2
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG C)
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
60.000
50.000
40.000
30.000
20.000
10.000
0.0
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)

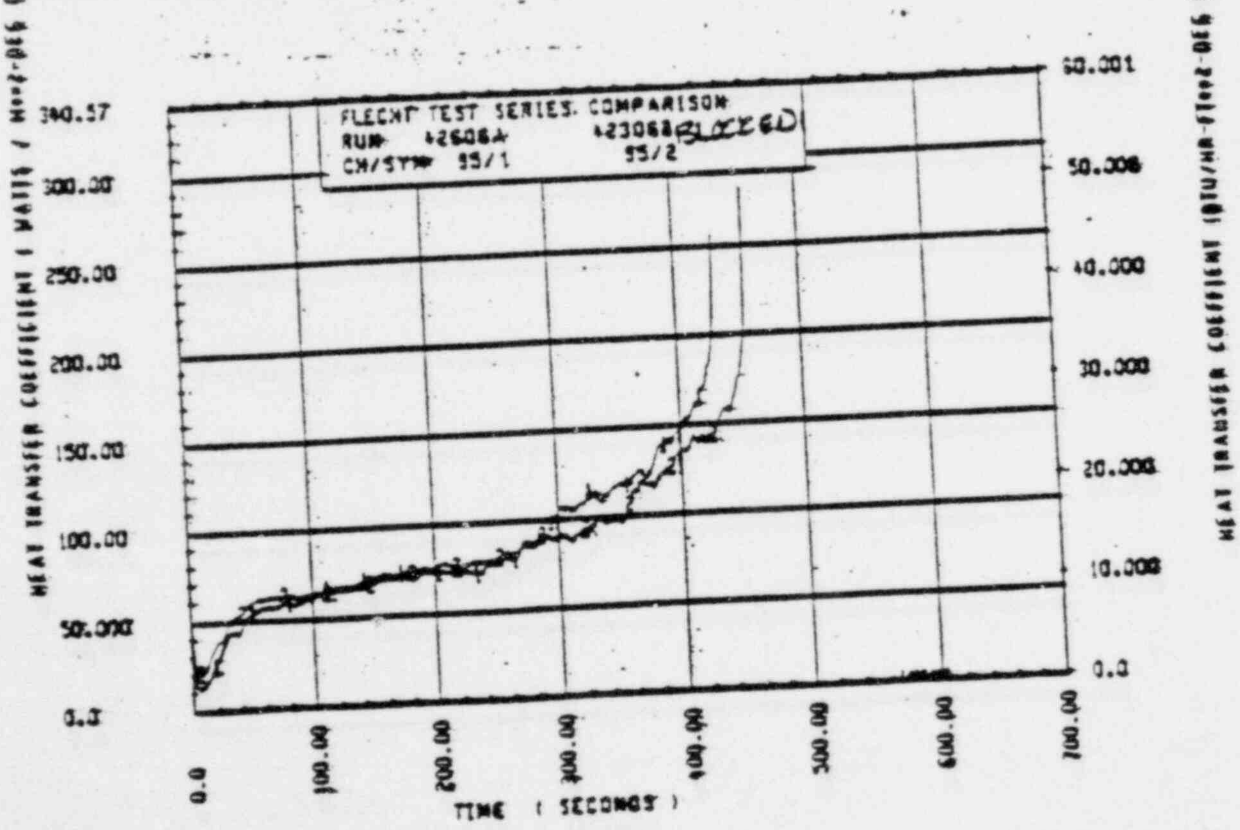
18

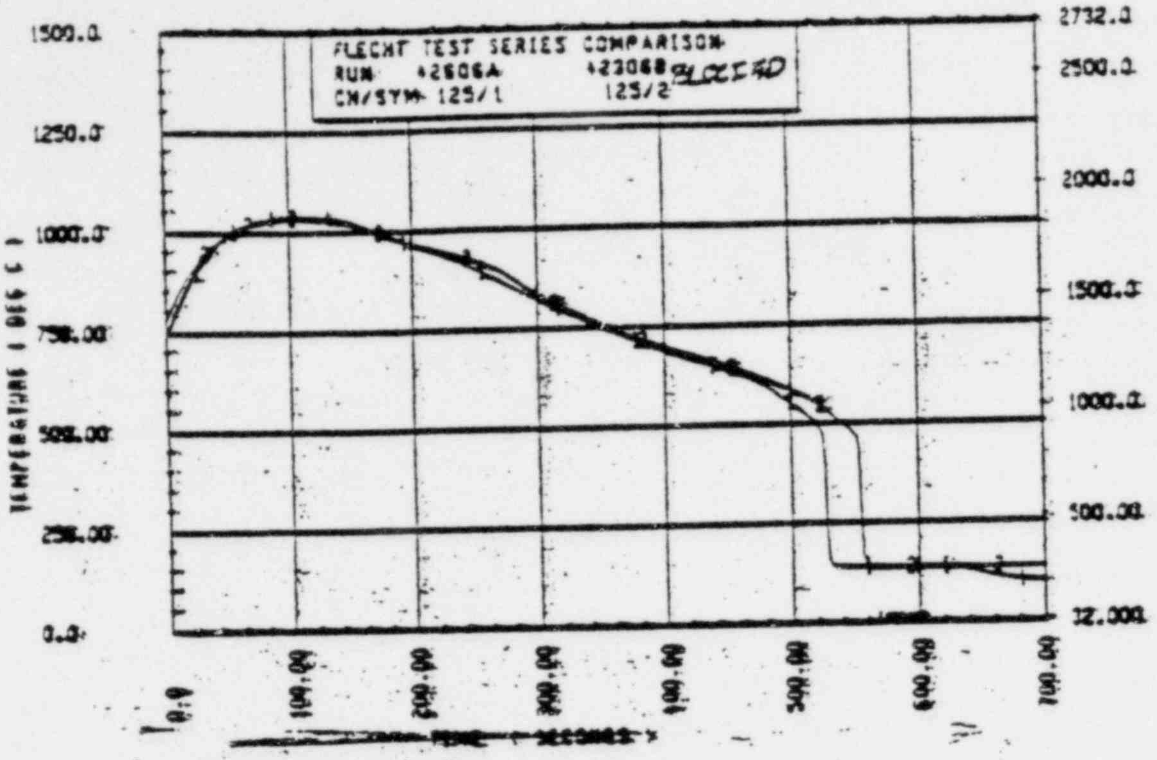
[illegible] 71"

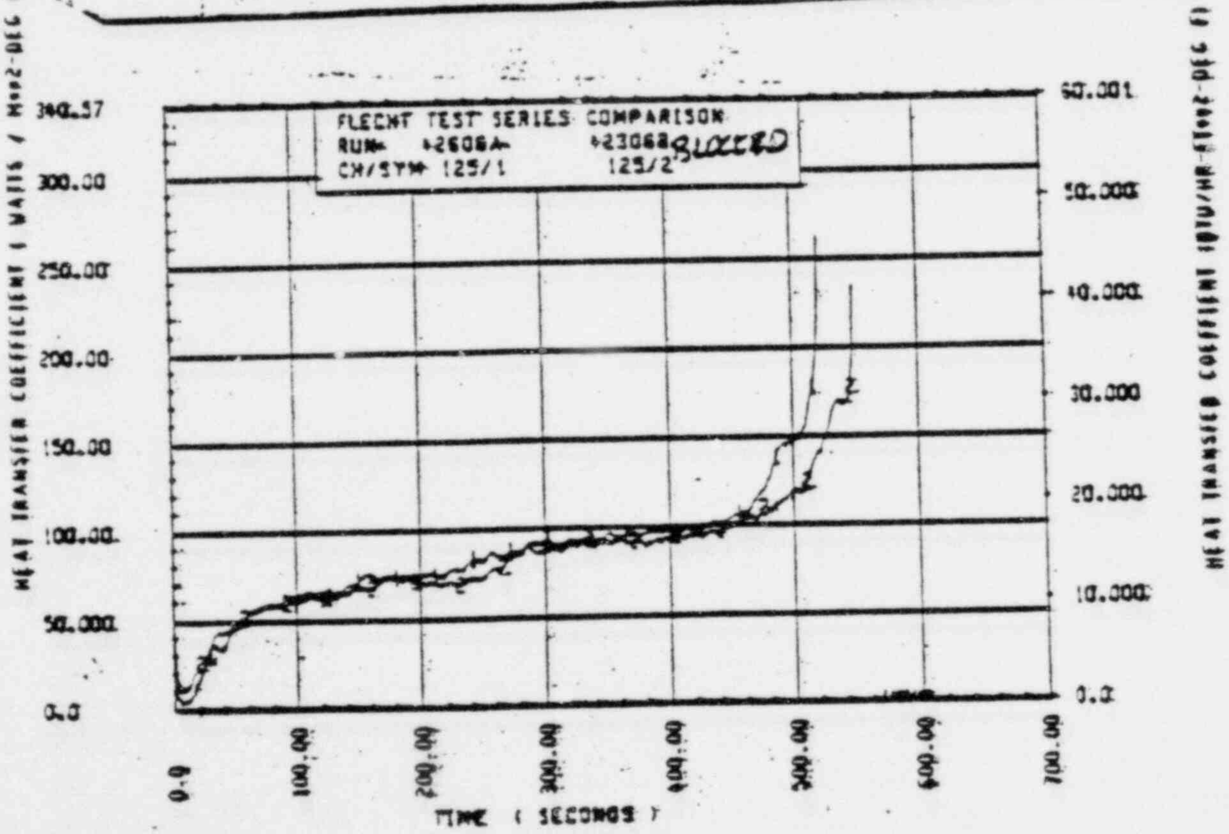

FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B
CH/SYM 85/1 85/2
BLOCKED
TEMPERATURE (DEG C)
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
TEMPERATURE (DEG F)
2732.0
2500.0
2000.0
1500.0
1000.0
500.00
32.000
TIME (SECONDS)
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00

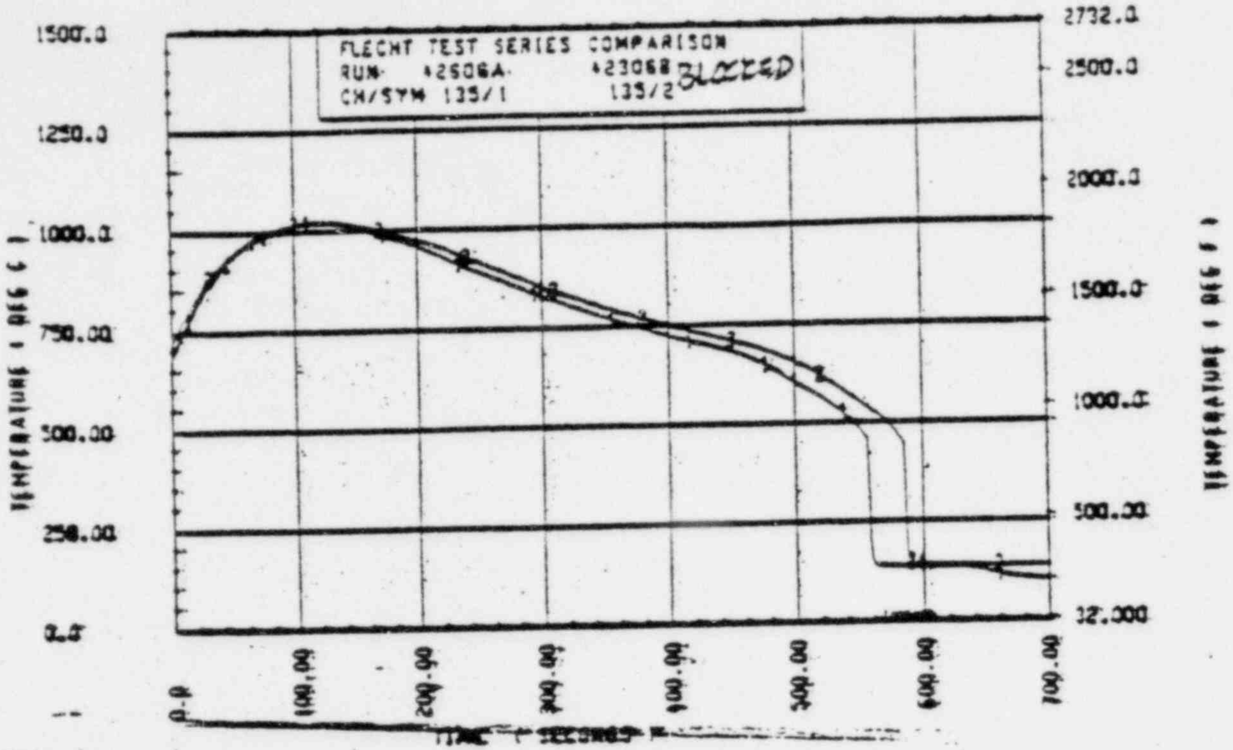

FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B
CH/SYM 85/1 85/2
BLOCKED
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG C)
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
60.000
50.000
40.000
30.000
20.000
10.000
0.0
TIME (SECONDS)
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00

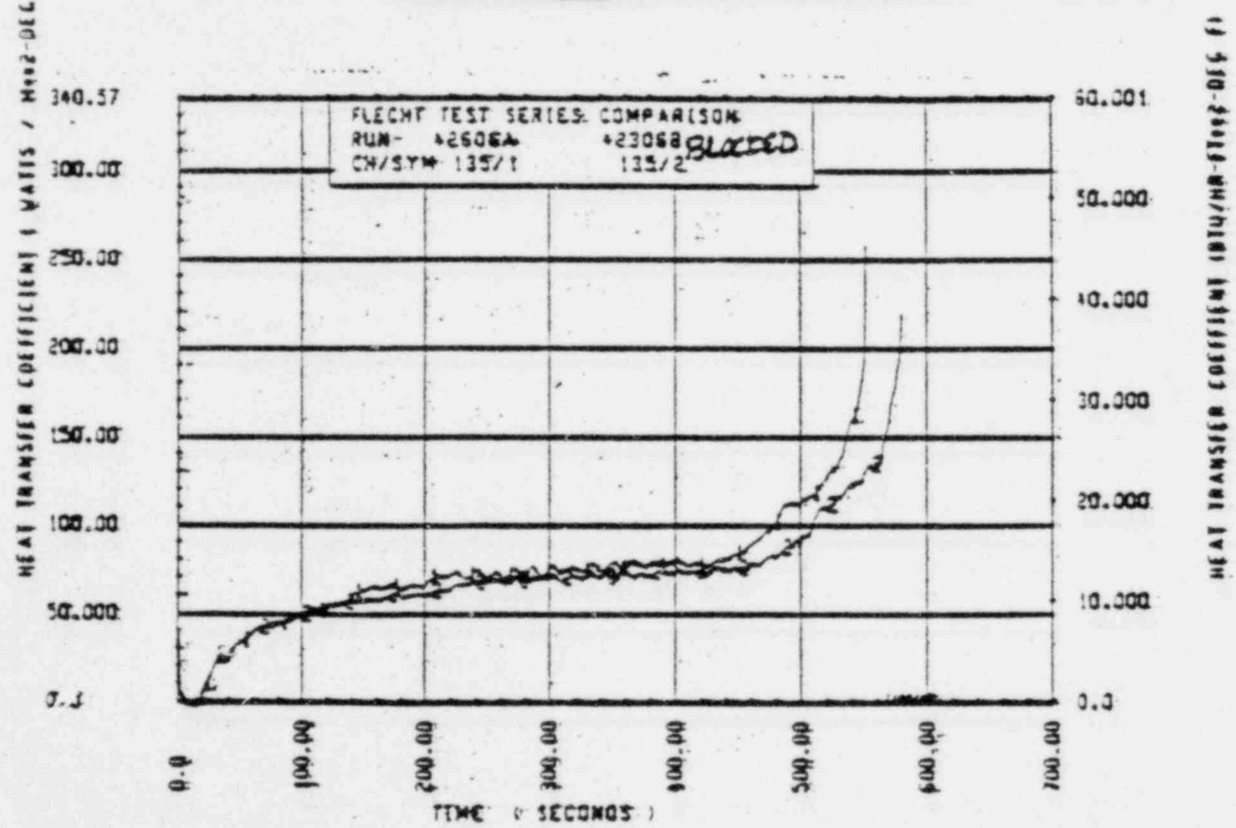

FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B BLOCKED
CH/SYM 35/1 35/2
TEMPERATURE (DEG C)
TEMPERATURE (DEG F)
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
2732.0
2500.0
2000.0
1500.0
1000.0
500.00
32.000
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)


FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B BLOCKED
CH/SYM 35/1 35/2
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG K)
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
60.001
50.000
40.000
30.000
20.000
10.000
0.0
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)


FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B BLOCKED
CH/SYM 125/1 125/2
TEMPERATURE (DEG C)
TEMPERATURE (DEG F)
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
2732.0
2500.0
2000.0
1500.0
1000.0
500.00
32.000
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)


FLECHT TEST SERIES COMPARISON
RUN 42606A 42306B BLOCKED
CH/SYM 125/1 125/2
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG K)
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
60.001
50.000
40.000
30.000
20.000
10.000
0.0
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)


FLECHT TEST SERIES COMPARISON
RUN- 42606A 42306B BLOCKED
CH/SYM 135/1 135/2
TEMPERATURE (DEG C)
TEMPERATURE (DEG F)
1500.0
1250.0
1000.0
750.00
500.00
250.00
0.0
2732.0
2500.0
2000.0
1500.0
1000.0
500.00
32.000
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)


FLECHT TEST SERIES COMPARISON
RUN- 42606A 42306B BLOCKED
CH/SYM 135/1 135/2
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG K)
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
60.001
50.000
40.000
30.000
20.000
10.000
0.0
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)

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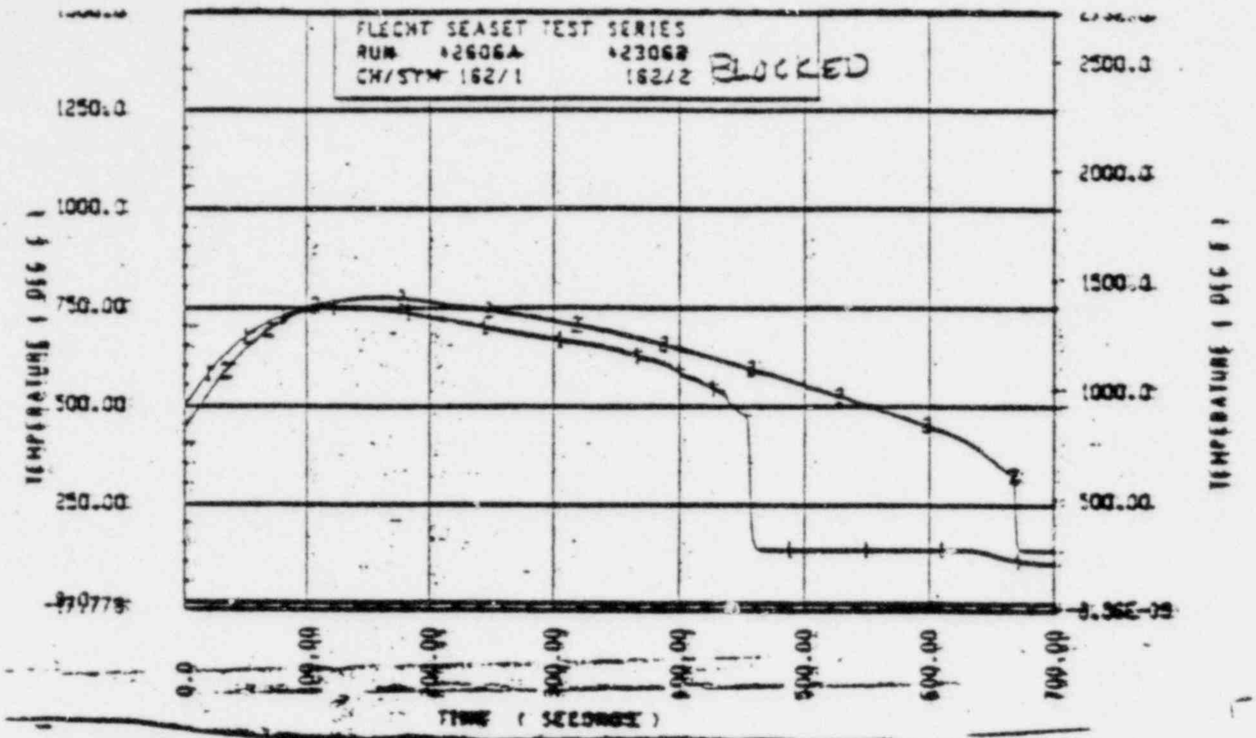
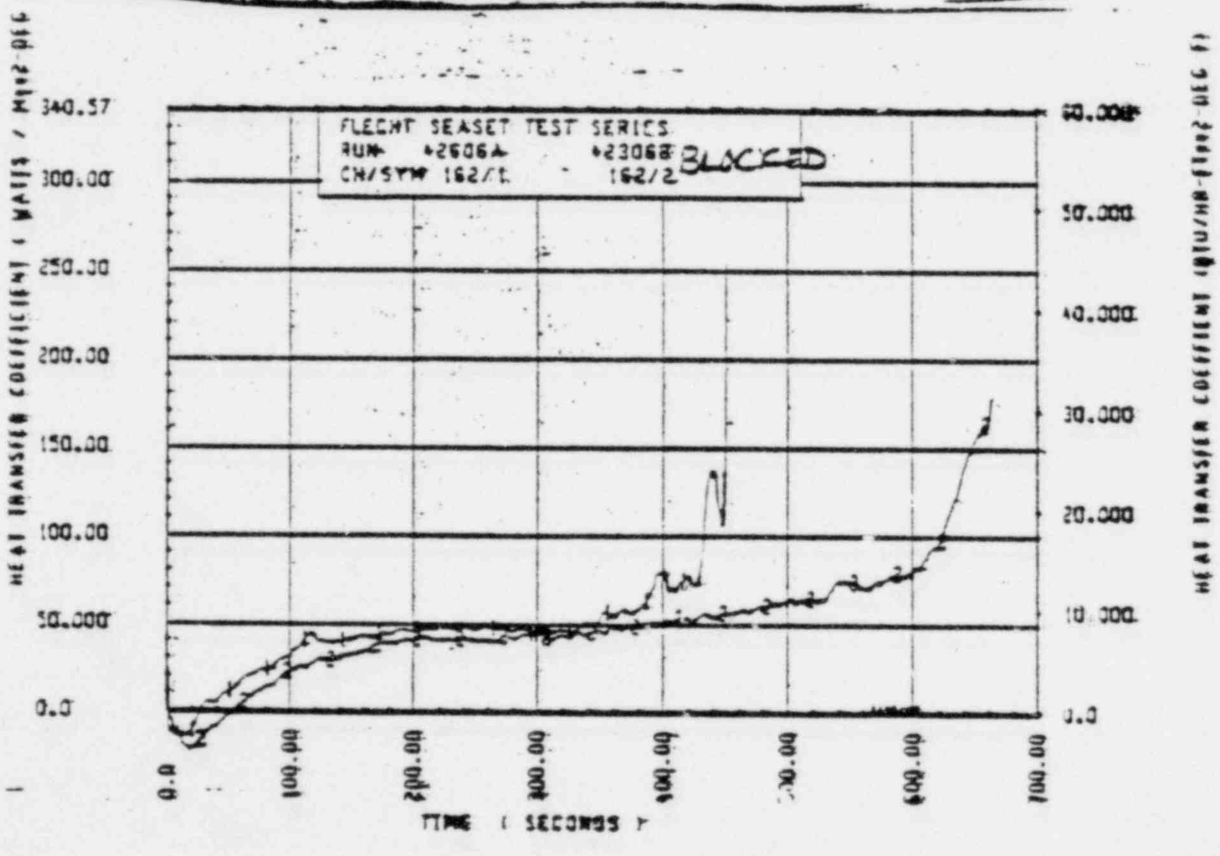
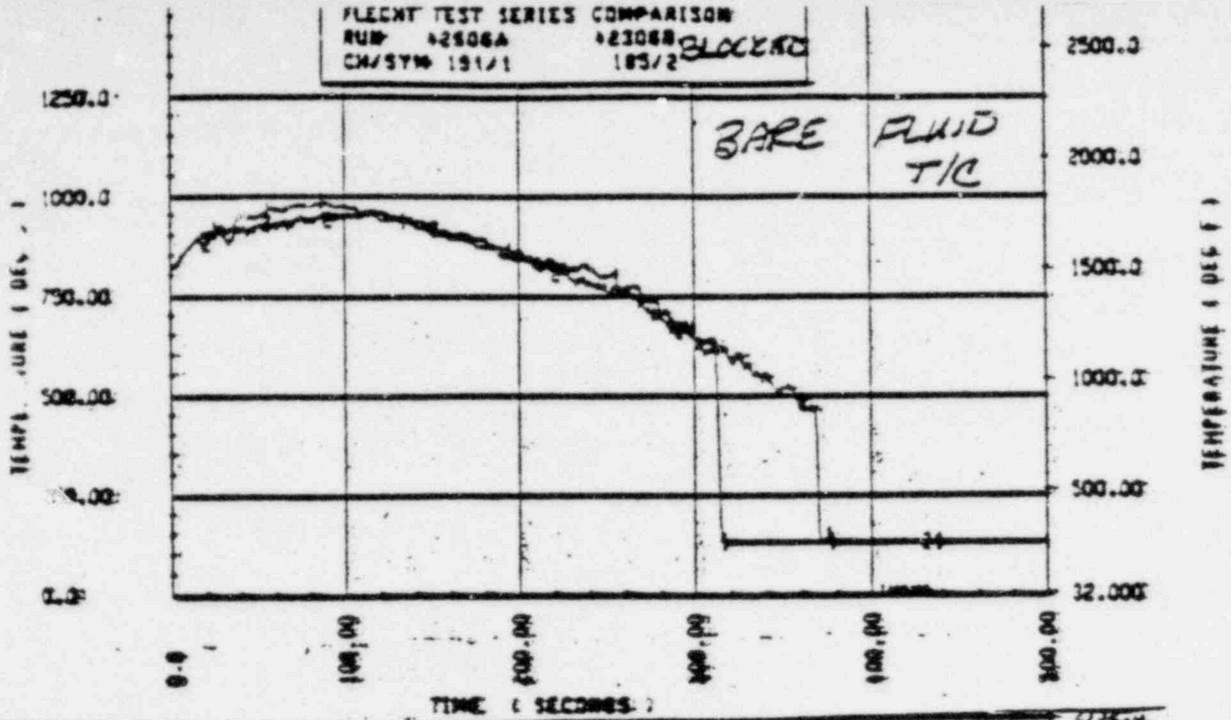
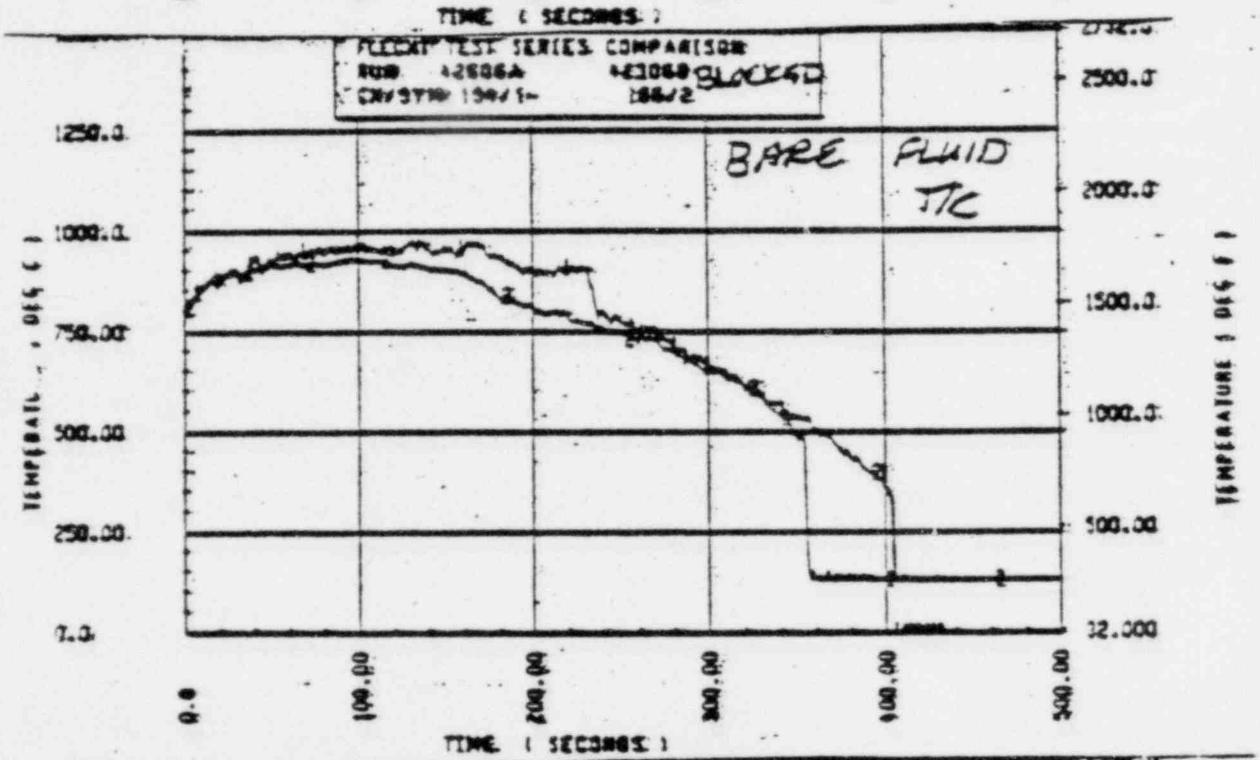

FLECHT SEASET TEST SERIES
RUN #2606A #23068
CH/SYM 162/1 162/2
BLOCKED
TEMPERATURE (DEG C)
TEMPERATURE (DEG F)
1250.0
1000.0
750.00
500.00
250.00
2500.0
2000.0
1500.0
1000.0
500.00
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)

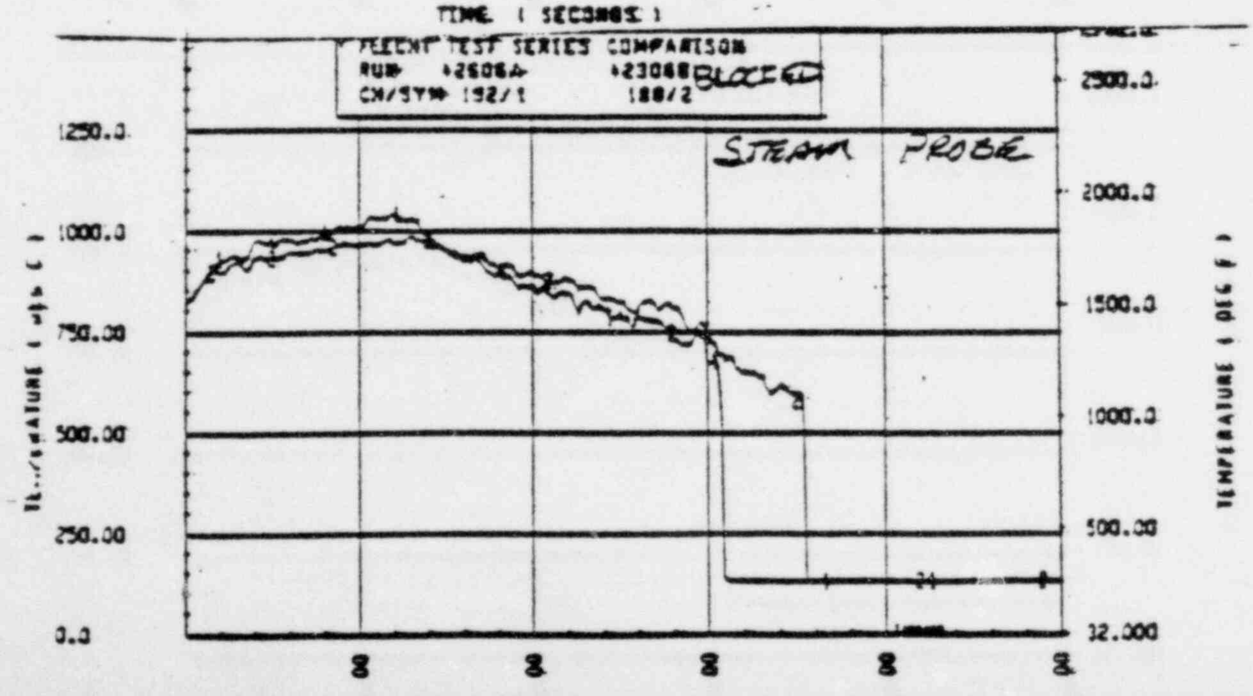

FLECHT SEASET TEST SERIES
RUN #2606A #23068
CH/SYM 162/1 162/2
BLOCKED
HEAT TRANSFER COEFFICIENT (WATTS / M**2-DEG C)
HEAT TRANSFER COEFFICIENT (BTU/HR-FT**2-DEG F)
340.57
300.00
250.00
200.00
150.00
100.00
50.000
0.0
60.000
50.000
40.000
30.000
20.000
10.000
0.0
0.0
100.00
200.00
300.00
400.00
500.00
600.00
700.00
TIME (SECONDS)

ROD 2C - 120"

23

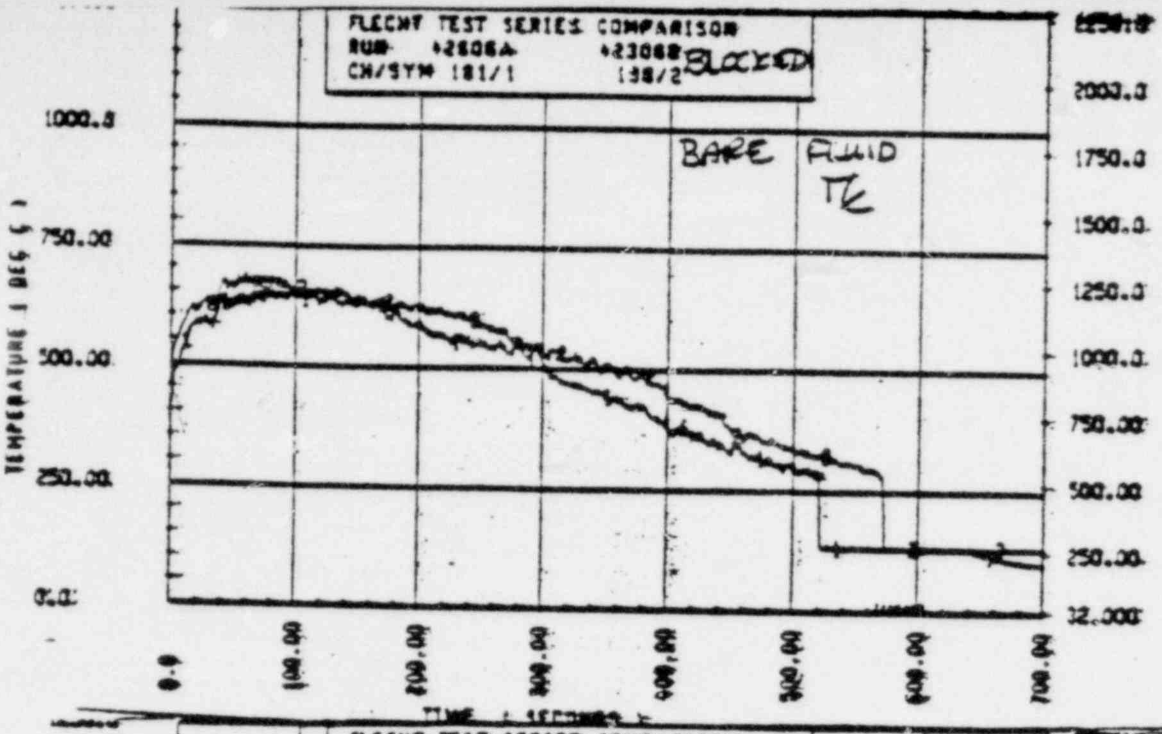




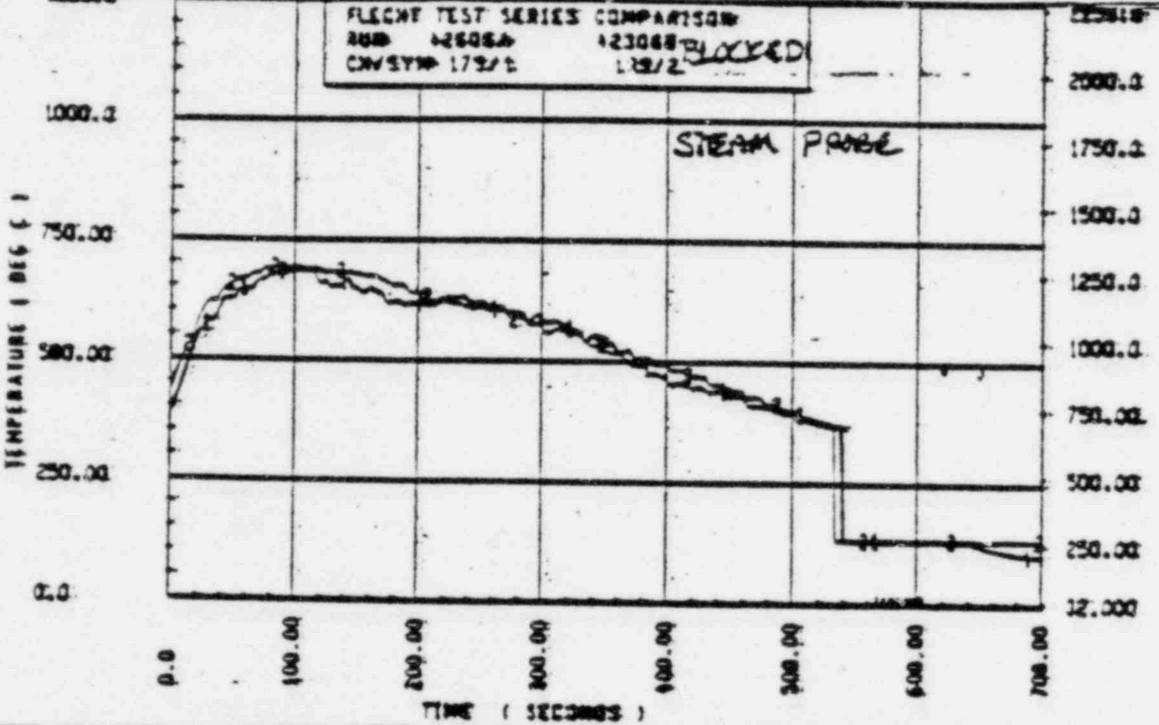


BARE
FLUID
AND
STEAM
PROBES
AT
77"

24

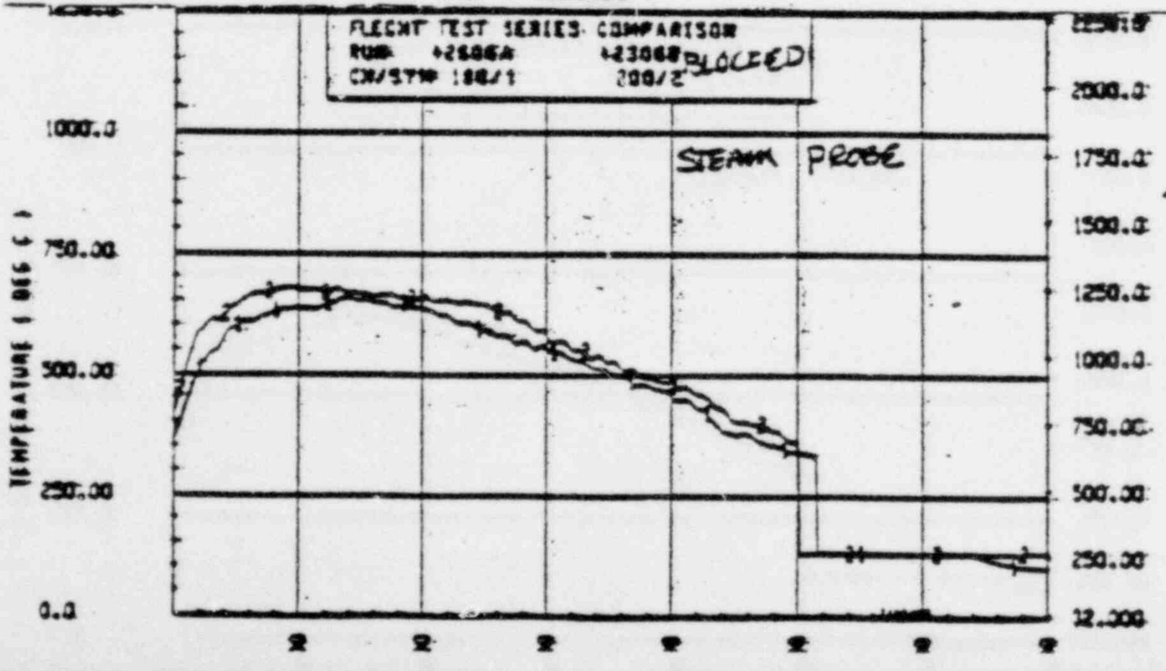


← 120''



← 120''

BARE FL
T/C AND
STEAM
PROBES
AT 120



← 120''

CONTINUING PROGRAMS ON FLOW BLOCKAGE

- FEBA TESTS WILL BE COMPLETED AT THE END OF 1981, BOTH 5x5 AND 5x10 ROD BUNDLE TESTS WITH BYPASS, COPLANAR BLOCKAGE
- FLECHT-SEASET FLOW BLOCKAGE PROGRAM WILL BE COMPLATED AT THE END OF 1982
- US/JAPANSES/GERMAN 2D/3D SLAB CORE BLOCKAGE TESTS WILL BE COMPLETED IN 1983

- CONCLUSIONS

BASED ON THE DATA REVIEWED AND OBTAINED TO DATE, IT APPEARS THAT IN THE BLOCKAGE ZONE:

- THE ATOMIZATION, DROPLET BREAK-UP AND MIXING OF THE ENTRAINED LIQUID IN THE FLOW, LOCALLY INCREASES THE HEAT TRANSFER AND MORE THAN OFF-SETS A FLOW REDISTRIBUTION PENALTY DUE TO BLOCKAGE UP TO AND THROUGH TEMPERATURE TURN AROUND

- LATER IN TIME, AFTER TURN AROUND, BLOCKAGE APPEARS AS A PENALTY BECAUSE (PRELIMINARY) THE BLOCKAGE KEEPS THE FLOW IN THE DISPERSED FILM BOILING PHASE, WHILE UNBLOCKED TESTS APPEAR TO GO INTO A TRANSITION PHASE WITH IMPROVED FILM BOILING HEAT TRANSFER. ALSO, UNBLOCKED TESTS QUENCH EARLIER

- HEAT TRANSFER AT UPPER ELEVATIONS (10 FEET AND HIGHER FOR A COSINE SHAPE) APPEAR TO HAVE POORER HEAT TRANSFER WITH BLOCKAGE. IT IS BELIEVED THAT THIS IS A RESULT OF EVAPORATING A LARGER FRACTION OF THE ENTRAINED DROPS IN THE BLOCKAGE REGION FOR THE BLOCKED TESTS RELATIVE TO THE UNBLOCKED BUNDLE.