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NUCLEAR REGULATORY COMMISSION

IN THE MATTER OF: DOCKET NO: 50-445-OL2
50-446-OL2

TEXAS UTILITIES GENERATING COMPANY, et al.

(Comanche Peak Steam Electric Station,
Units 1 and 2

MEETING: Summary Dispositions:
Upper Lateral Restraint
for Steam Generators

~~PREHEARING BRIEFING~~

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In the Matter of: :

TEXAS UTILITIES GENERATING : Docket Nos. 50-445-OL2
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Room 110
Phillips Building
7920 Norfolk Avenue
Bethesda, Maryland

Tuesday, November 13, 1984

The briefing in the above-entitled matter was convened pursuant to notice at 10:00 a.m.

APPEARANCES:

JACK REDDING, Texas Utilities Electric Company

P. T. KUO, NRC

FRANK RINALDI, NRC

C. J. COSTANTINO, BNL

S. K. SHARMA, BNL

MORRIS REICH, BNL

HSIANG CHANG

SPOTTSWOOD B. BURWELL

ROBERT IOTTI, Ebasco Services Inc.

ANIL M. KENKRE, Gibbs & Hill, Inc.

P R O C E E D I N G S

MR. BURWELL: My name is Spottswood Burwell, I am the project manager for Comanche Peak, and I am employed by the NRC.

The topic this morning relates to the applicant's motion for summary disposition relative to the upper lateral restraint beam. This motion was transmitted to us by a letter dated May 20, 1984.

This particular motion, I believe on this particular one case, is filed in a partial answer to the applicant's statement -- to the applicant's motion on August 29.

MR. REICH: You should have our answer.

MR. BURWELL: Yes. The applicant also filed a response to that, dated October 26th.

With that, perhaps I could ask P.T. Kuo, or Franklin, to pick up on where we are.

MR. RINALDI: I'm Frank Rinaldi, I'm the structural reviewer for NRC.

Basically, in June '84 we had a meeting to address this problem, and following that meeting, our staff and the staff from BNL, acting as our consultants, visited Gibbs and Hill for an audit of the calculations in the middle of August.

And following that audit, the applicant provided some information on the NASTRAN code that was used in the

evaluation of this problem, and also some sample problems related to this.

BNL has reviewed this, and also performed somewhat of an independent check on the specific NASTRAN code problem and they have had discussions with the applicant -- their consultant -- to resolve differences in the code.

At this point the Staff feels that we should address any problem so that we could come to a conclusion in the evaluation of this upper lateral restraint beam.

I turn the floor to --

MR. BURWELL: May I add a further statement on that? I believe our meeting this morning is specifically to address the calculational methods used to predict concrete structural failure, which is one part, only, of our review of the upper lateral restraint beam.

This has more to do with a resolution -- an understanding between the Staff's consultants and the applicant's consultant, relative to the method used to derive some of their conclusions, rather than to the conclusions themselves.

You, I believe, were going to turn the meeting over to --

MR. KUO: If I may, I would like to add another statement. My name is P.T. Kuo.

During the audit, I believe, the Staff and their consultant, BNL, found the calculations that we reviewed

were adequate, with the exception that the modified version of the NASTRAN code was not verified. And discussion today is basically to discuss with the utility, about the verification of this modified version of the NASTRAN code.

MR. IOTTI: I guess I would have to make a statement to that, if I may be permitted? I don't want to imply that we would disagree with the conclusion that today's meeting is to discuss additional verification of the NASTRAN program, but it is my impression that the initial question from the Commission was not whether the code was or was not verified, because the code had been verified against a certain set of problems; but whether the verification was all-encompassing and whether it might not be prudent to have additional verification. This is what we set out to do.

So, I don't want to leave the impression that the Commission characterized the code as being used without initial verification. There were some questions in their consultant's mind that the verification problems used to date may not have been sufficient to totally exercise the code, and we were asked to run additional verification problems.

MR. KUO: You are right. Thank you.

MR. BURWELL: Thank you.

Okay. How would you like to start?

MR. REICH: It's up to you.

MR. KUO: I guess at this time we can turn to Morris Reich and ask them what concerns they might have with regard to the few example problems that were submitted to them.

MR. REICH: Okay. Let me go over the sample problems and how we looked at them and I would ask my colleagues here to come in whenever they feel they should add on or subtract from whatever words I say.

First, four check problems were submitted to BNL. Of these four, we only were able to take one check problem and thoroughly go through it.

This problem I think is classified as CA-29, and it involves a beam which is held rigidly between two walls, and it is loaded in the middle. And it was sent to us, we got the problem sometime the beginning of September.

A finite element grid was constructed in the same manner as the one submitted to us in the problem, with the same elements, number of elements -- we tried to put the steel in the same place and everything.

Similarly, the same material properties were used. And we ran this particular problem.

Now, for low applied roads, that is, let's say we were coming up to 10 or 12 kips; the two runs yielded similar

results. However, when we started going higher, the BNL calculation showed that the beam would fail at between 22 and 24 kips. And we found that the failure would be due to diagonal tension cracks near the fixed ends.

And this value, indeed, was close to a value which was predicted in a paper by S. Sushil -- that's a name. This is an equation which this man brings in a paper, and it is based essentially on a statistical evaluation of many experimental data points.

It is also in the ACI September-October 1984, pages 81 -- I think it's 81-83, by Bazant and Kim. We gave a copy of this report to EBASCO -- so, the predicted failure is verified by tests, physical tests.

Now, the calculations that EBASCO did, in that first run that we had, did not show failure even at 32 kips. It showed cracking but it did not show failure.

Furthermore, the results that we saw did not show any diagonal tension cracking. And at this point we had a meeting, attended -- at BNL -- attended by Bob Iotti, and also Harry Chang. And we presented this differences -- we had these differences between our answer and their answer, and at that point we mentioned that, you know, that perhaps it is possible that there should be more than one crack there, because we didn't see in any of the answers that we had, that there was more than one crack. And we

talked about that a little bit. And it was agreed that EBASCO should go back and look at the problem again; perhaps there was something missing in the input or something in the code -- and they needed some time for that. And I also recommended at the time to NRC that we should allow for EBASCO, two to three weeks, at least.

This is, indeed, what happened. EBASCO returned on November 5th with new runs, and they also gave us a little write-up of that, sort of summarizing the runs.

Now, in this summary -- and let me sort of quote from it -- there are tables. And I will sort of direct you to table 1, where it says that -- first of all, in this new run there were more than single cracks. In other words, there were two cracks -- multiple cracks, let's say. Okay?

However, this table shows that, for instance, for shear interlock -- and that's shear retention; in other words, after you get a crack there's still shear retention. And they changed it. Harry, or whoever ran the code, changed the shear retention factor from .2 to .4. There was virtually no difference in the maximum reenforcement stress, even though that was changed.

Now, this part we do not understand, because it's not supported from other studies, including BNL's work, which we have done in concrete analysis -- it shows it does -- shows that it does have an effect. It is not negligible.

So we didn't understand that.

Also, according to this -- the discussions we had with Harry -- said that they were only able to obtain shear failure when the concrete is assumed to have a low tensile strength of about 120 psi, and then it gets a shear failure close to 24 kips. That's about a quarter of what the strength normally is. If it goes higher, it doesn't get shear failure at all. We can't understand that too clearly.

Another thing which this report says is that -- it states that they calculated large vertical displacements before the diagonal tension failure occurred, and that's another thing we don't understand, because our analysis does not indicate this to be the case. We find it to happen suddenly and the displacements are not that abnormal.

Another thing this table also indicates is that regardless of whether you use the angle of limitation criterion or not, the difference in the answer is very little. Virtually nothing. And this seems to indicate to us that the angle criterion doesn't even come into play when you are talking about the answer that they were getting for this beam.

So, that brings us to the final point that we have on this. The figure in EBASCO's report which shows the crack

pattern -- and again the cracks are shown to be virtually all orthogonal, and the reason for this is not clear to us, too.

These are the things we don't understand and our code gives us different answers. From our experience, from the data that that we have, we feel that we are correct. We don't know why EBASCO is getting this.

MR. BURWELL: That's quite a laundry list, however you want to take them.

MR. IOTTI: Well, I don't want to go over every point of the laundry list because these points are now well known to us. We have discussed them with Brookhaven.

The first thing we looked at is whether this could be a problem with the code, in the terms of, given the theory is correct, there may be some coding input errors that we would have to look at. We found none. And of course that puzzles us quite a bit, because if the code is doing the thing right and the theory is correct, then why aren't we getting the same answers?

All we can conclude so far is that there is a mechanism of possible failure which the EBASCO NASTRAN version does not address.

In-plane, shear-type failures are simply not considered in our code.

A lot of the conclusions presented in the last set of

example problems, for example, the relative lack of effect of the shear interlocking character, were presented for a tensile strength of 546 psi. Those conclusions may or may not be the same if the tensile strength were lower.

Likewise, the angle limitation, that conclusion was valid for 546 psi. It may not be the same conclusion where the tensile strength is lower.

We kind of looked at what the code limitation may be. We ran a simply supported beam. This is something that we had promised Brookhaven we would do, and we didn't have a chance to have it completed before our latest submittal. And on the simplest supported beam, we would suspect, as the load increases at a certain point, at the location of the two supports, you would begin to see cracking under essentially pure shear, and you would expect that crack to be 45 degrees. But the way the code predicts -- and that code, by the way, the paper of -- what's the name -- Bazant and Kim -- would predict to start when your tensile strength or your shear exceeds 120 psi.

Well, the way that our code handles that, the shear is equal to the tensile stress. And with a tensile stress of 546, we simply aren't getting a crack. And the reason that we aren't getting the same result as Brookhaven as we lower the tensile strength is not, perhaps, because the mechanism is the same, but because when you set the

tensile strength equal to 120, you, in fact, are predicting the diagonal shear failure at the ends. Then we would get the corresponding results.

We are puzzled at Brookhaven's results in the sense that the explanation given to us is that their only mechanism for failure is also the check against the tensile strength of the concrete, which is what we are doing.

This is not a question of computer code -- you can simply run this calculation by hand for a simply supported beam. What is the shear at the end and what is the tensile strength and they are going to be equal. You can just run the calculation by hand and you find it's 120 for this particular beam. So how are you going to fail if your figure is 480?

MR. CHANG: On the last item -- let's forget about the code. Whatever the code is saying. Let's talk about a simple support beam.

A simple support beam, a load to the center with 22 to 24 kips, the average stress on the section -- in the section is approximately, average stress is approximately 150 psi.

Now, taking the distribution of the shear -- it's a parabolic shape, on the end of the beam, so it would go about 200 psi, 220 psi, something in that area; 200 to 220

psi.

Under the shear condition, the diagonal tension will be equal to 220 psi.

If we put in the concrete strength of 500 psi, there is just no way to crack this beam. The coding is doing it correctly. And we compute -- we also show that at the end beam section we have about 200 psi. And I also show that coming out, the tensile strength of 500 psi; it will not crack.

From the ACI code or from the paper by Bazant and Jin-Keun Kim, the cracking of the diagonal tension to the shear should initial at approximately 2 square root of FC pi.

This -- for this case it is approximately at 120 psi, the tensile strength.

So, if we input 120 psi in tensile strength in our code, we will get a failure mode of from 22 to 24, as shown by this experiment.

However, if we input 500 psi, tensile strength, we will not get any crack due to shear, because the shear crack -- the shear stress around that area is only about 200, and with the diagonal tension is only about 200, only.

And that, therefore, what we are really saying is this way: if BNL's code, using the same criteria we are using -- because they review our theory correctly, they don't find anything wrong with our theory -- then I expect BNL also

will show up a crack with a lower tensile strength as a higher tensile strength, 500 psi.

Usually it's due to fracture failure of the beam -- the fracture is bending structure of the beam it's usually 1/10 of the compression.

The initial cracking of the shear from all the test results and from ACI code, initiation of the cracking, is around 120 psi.

Therefore, what I said: If we input 120 psi, 20 psi to the code, the code would correctly predict the failure. Therefore the difference --

MR. IOTTI: Let me see if I can summarize this. We are puzzled at this point. We know there's what we perceive to be a limitation in the code that we are using because we are only checking tensile strength caused by flexure. Maybe we should have a dual criterion and also check for diagonal tension. So that we see a potential weakness in the code and we can address whether that has any effect on it -- that has been done.

What is puzzling is, if in fact BNL also has a failure caused by flexure, how they can get results different from ours. That's what is puzzling.

MR. COSTANTINO: When we first got together in August, everyone agreed that failure prediction in concrete is a difficult task. The BNL code is really not

the question. I don't know if we should be --

MR. IOTTI: I don't want to discuss it. We are puzzled why we are not getting the same answer.

MR. COSTANTINO: I'm puzzled, listening to this talk. Everyone agrees that the checking does not agree with the data on shear failure of the beams -- for whatever reason. Aside from the BNL calculation.

The conclusion from that would be that for the kinds of problems to which NASTRAN -- the NASTRAN code was applied in the main -- the steam generator compartment, you would estimate, then, that the response predicted for combined moment, shear, and axial force would probably also be incorrect for predicting the amount and extent of crack.

MR. IOTTI: It would underpredict the extent of cracking and therefore overpredict --

MR. COSTANTINO: Whatever. It seems to me the primary conclusion then is you would question the results of the cracking predictions from the NASTRAN.

MR. IOTTI: Of course. We went back and looked, because the next question is obviously how much difference would it make? And then you have to flag the areas where the differences can arise, how extensively --

MR. COSTANTINO: Then everyone would agree that the results predicted by the NASTRAN for the cracking of the compartment --

MR. IOTTI: For the high tensile strength, yes.

MR. COSTANTINO: I want to make sure we all agree with this.

MR. CHANG: I would --

MR. COSTANTINO: Let me finish my statement first. I want to be sure we are all saying the same thing.

MR. IOTTI: Absolutely. The data conflicts with what is predicted by our code at high tensile strength. We have already gone back and found out how much of a difference. I think where we started straying is, because we couldn't quite reconcile the differences between the two codes, we were asking ourselves how can we go wrong? We know what our limitations are. We think we have found out where our limitations are.

We are puzzled where why, if in fact the theory is identical with Brookhaven, how could we come up with a different answer? So the conclusion is maybe the theory isn't the same. Okay?

MR. COSTANTINO: That's probably the case.

MR. SHARMA: The only thing we mentioned in that meeting is we agreed with the theory EBASCO is using. We didn't say we are using the same theory. In fact --

MR. IOTTI: I wish you had told me that, because I am going crazy. If you have a slightly different criterion it's not surprising.

MR. SHARMA: We have a dual failure criteria model --

MR. IOTTI: Oh, you have a dual failure in; and that's why we differ. That's the answer.

MR. SHARMA: But you implied one is in tension and one is in shear. We have two criteria in the sense that we check the strengths --

MR. REICH: But this is irrelevant. The code is not under review.

MR. SHARMA: This is irrelevant. Given that beam, we know what its failure to shear is and we are not using the ACI equation to get the failure because as Harry Chang said right, that only gives the initiation. We are using the other mode as stated before.

That is a question which gives the failure load -- not the initiation of cracking, but the failure due to diagonal tension. And according to that equation, this beam should fail in shear around 22 kips, assuming a compress strength of 4800 PSI.

MR. IOTTI: What we are saying is that the failure for diagonal tension would occur at the lower value of diagonal tension than it does if you assume a diagonal tension occurs at 480 or 500. And we don't change that fall. We still assume it's 500 and that's why we are not predicting that failure.

MR. SHARMA: Now, if we assume a concrete of 4800 psi compress strength, then the most reasonable value would be 1/10; not 1/40. We disagree here. We would assume the tensile strength of this concrete to be around 480 psi.

MR. IOTTI: For diagonal?

MR. SHARMA: For anything. Now Harry is saying he has to assume 120 psi in value to predict that failure, which in our opinion is too low.

MR. REICH: One more thing, while we are still answering you, Harry -- you mixed in the moment and shear diagrams. That only applies if there's no crack in starting at all. This beam will start cracking in beforehand. You can't tell us the moment of shear diagram theory will stay the same once you have cracking going on. The elastic theory isn't there anymore. We told you this before.

MR. SHARMA: Another point, if I may, is I also gave Harry some references in which people have predicted shear strength of the beam using similar kinds of formulations, and in those papers the tensile strength might assume even more than 1/10, like 12 percent of the compressive strength, and they were able to predict the shear failure at the experimental load level, so it's not that you have to assume 1/40 of the tensile strength. The

value will be 10 percent and one should be able to predict --

MR. REICH: And the other thing, Harry, why do you get large displacement?

MR. CHANG: The large displacement is because we want to show you that observation is after it has failed. We have shown that it is small if you just take starting of the failure. It is reasonable --

MR. COSTANTINO: You get a major increase in load capacity before you call it failure?

MR. CHANG: No. We don't. At 24 kips, when you increase the 24 kips, first you start a diversion solution. A diverging solution in that spacing is small. If there's something is wrong -- you would take the next shot to try to converge again. The next shot -- the stiffness is almost failed already, it is very small. Then you are putting reiteration issue of the large displacement.

MR. COSTANTINO: That's the wrong answer. The shear failure of a beam is a brittle structure-type failure.

MR. CHANG: Yes.

MR. COSTANTINO: So collapse of the beam starts at its normal displacements; displacements of the order of less than 1/10.

MR. CHANG: It has collapsed.

MR. COSTANTINO: If the code requires a large

displacement to enforce collapse --

MR. CHANG: No, it does not.

MR. BURWELL: One at a time, please.

MR. COSTANTINO: I mean, Harry, what you are really saying is under the initial assumed stiffness coefficient you get small displacement which don't satisfy equilibrium, so you need a zero stiffness correction which gives you large displacements. So the conclusion is the code is not predicting experimental behavior. I think you agree with that.

I don't think there's any point in discussing too much else beyond that, except that everyone agrees that the code is not predicting concrete failure behavior properly.

That's the overall assessment. That's all everybody is trying to say.

The implication of that on the -- on the generic stress analysis is something else.

MR. CHANG: Well, one more thing I would like to say is this way: The code is doing correct what we want to do, according to the theory. We may not have a second theory -- second -- I don't know that area. But if we, for this particular application is concerned, if we lower the tensile strength to zero -- okay? -- that would be predicted at much lower failure criteria, failure loads. For this particular upper stress concern, we actually did

run certain runs with zero tensile strengths, certain runs with very high tensile strengths --

MR. COSTANTINO: We are talking about detail.

MR. IOTTI: We are talking about a different issue.

We concur that there is a limitation in the code --

MR. COSTANTINO: When it comes to cracking in concrete --

MR. IOTTI: Right. At the higher loads. Right. And there's a point at which the two solutions obviously depart. I don't know why that point is; we tried to assess it.

MR. COSTANTINO: The cause of that is something else.

MR. IOTTI: We can't find it.

MR. COSTANTINO: If there's no programming error there's an error in formulation. If not, then there's -- but obviously there's a problem someplace.

MR. IOTTI: We are trying to learn also. Given that that's the case, first of all does any of the analysis that has been done for the upper lateral restraints involve the type of loads that cause this vast change in the behavior? That's the question we asked ourselves and we went back and reviewed it and we can assess it later.

Give me one minute, Frank, and then I'll turn it over to you.

The other part is that, since we don't -- well, we think we've found what the limitation of the code is. So, obviously if we ever have to go back and do anything further, we will address it in that fashion. And at this point, I guess, Frank, if we want to get into the discussion of the upper lateral and the analysis and the consequences of the limitation of the code we can do so. If not -- that's up to you.

MR. RINALDI: Basically, the way I see it, you use this code to predict the value at this upper lateral restraint beam. Now we have a question about this code, so apparently we should use another approach to evaluate it.

MR. IOTTI: We used the code with zero tensile strength to assess the values on the concrete. Okay? We used the higher strength to assess how much of a load we will get on the upper lateral. There is a dual approach to this. For the concrete we use the zero tensile strength, if you look at the affidavit. And when you look at the upper lateral in terms of maximizing the effect on the upper lateral, the bounding on the upper lateral so you maximize the stresses on the steel itself, we use this high tensile strength concrete. Let's not forget that. I

think that's the point that Harry wanted to make.

So, clearly, yes. We may be overestimating the resistance of that wall. The point is, by overestimating all you have done is created a worse situation for yourself in terms of stresses on the steel beam.

Obviously nothing worse can happen to the concrete than the concrete having zero tensile strength. So, if you go back to the affidavit -- I'll read it to you, because there is a tendency after you do all of this work to forget why some of the work was done.

On page 11, first we address the impact on the concrete structure; okay? And the test for concrete structure was conducted for zero tensile concrete strength and then later on it was for 450 psi.

MR. COSTANTINO: Where are you reading, Bob?

MR. IOTTI: Page 11. The actual affidavit itself.

MR. COSTANTINO: Oh, the May 20th document?

MR. IOTTI: Yes. But I guess ultimately we would concur, there is a question about the validity of the 450 psi, or not.

MR. COSTANTINO: Are you saying if you assume zero you get a very conservative or very soft response to the concrete and 450, a very hard response?

MR. IOTTI: The concrete at 450 will certainly

provide a much larger resistance to expansion --

MR. COSTANTINO: As far as the evaluation of the upper lateral restraint beam, I'm not sure that that's the proper question. It seems to me the question is: Let's assume the NASTRAN output is wrong. In all cases. Can you conclude that the 450 calculation is conservative? If all you know is the NASTRAN calculation is wrong? As far as had the upper lateral --

MR. IOTTI: From the standpoint of the upper lateral itself.

MR. COSTANTINO: The steel beam.

MR. IOTTI: Yes.

MR. COSTANTINO: It's an opinion. It seems to me that some justification in terms of simplified calculations is needed.

MR. IOTTI: We looked back to see what elements would have load, where the particular application of the code would affect it -- it turns out to be very few of the elements near where the upper lateral impediment is. If they crack as opposed to not crack as they are, what we would get is additional relief. Okay? So it's an educated opinion that there is lower cracking; okay?

Now the next question is whether the zero tensile strength represents the worst case for the concrete. It should be close. It may not be the worst case absolutely

because you also get expansion of the upper beam, but what we found just by comparison, the extent of cracking for zero tensile strength was practically everywhere when we went up to 450 -- we expect the zero tensile strength represents the worst conditions for concrete for the cracking.

Now, we did go back and check everywhere in the elements where the diagonal tensile strength could have been a factor in calculating the initiation of cracking. And we found it was six or seven elements in that region which --

MR. COSTANTINO: Should have been predicted --

MR. IOTTI: Should have been put as cracking in accordance with the test data. Okay? Now we don't know what cracking those elements would have done with the surrounding elements without running it, but we would expect you'd have, if anything, more cracking; not less.

To that extent, we believe the results, however incorrect in a sense of predicting the failure or the cracking in the concrete, are conservative for the analysis.

Understand that the off-shear cracking, that was checked manually. So that part was addressed properly, per ACI.

MR. COSTANTINO: Are you going to submit a piece

of paper on that or have you already done that?

MR. IOTTI: We have already done that. We gave you the manual calculation, off-shear.

What we don't know is what else to do at this point to correct whatever deficiencies there are in the code. In other words, we are a little puzzled about how we are going to go about correcting it. I would like to separate, perhaps if it's possible, the upper lateral restraint analysis from whether the code has limitations and whether the code should be corrected. Okay?

As far as we can determine, the limitation of the code actually imposed, if anything, more of a problem for the upper lateral restraint than otherwise. So whatever results we have would have been conservative, as opposed to not conservative.

Call it an opinion, if you will, but it is an educated opinion.

MR. KUO: See if I can summarize what I understand from Mr. Iotti. I believe everyone here agrees that the code itself may or may not correctly predict the cracks in the concrete.

MR. IOTTI: I believe we agree it does not, at high levels. What is a fact is a fact; okay?

MR. KUO: Okay. And that you tried two ways to approach the problem: One, by using zero tension in

concrete to look at the concrete; and then another, assume 450 psi in the concrete, look at the upper lateral restraint. And supposedly this approach is conservative.

And later on you, I guess you were asked whether this is an educated opinion or you have something to support it.

MR. IOTTI: We think we have something to support it.

MR. KUO: Can you make that available to us?

MR. IOTTI: Yes, we can basically. The list of all of the elements and what the diagonal -- what the shear stress is in all those elements -- the in-plane shear stress, which is, after all, what is in question. And wherever that shear stress is in excess of 120 or 150, we flag that element as being a possible candidate for crack initiation. Whereas right now there's no crack prediction.

You can find the location of the zone and you kind of mentally assess for yourself what would have happened if those elements were also cracked.

Again, it's not a question --

MR. COSTANTINO: What do you mean by "in-plain shear?" I thought we were talking about normal shear, bending and normal shear?

MR. IOTTI: This is a two-dimensional element. The shear through the thickness is assessed manually.

This is a shear through the plane.

MR. COSTANTINO: Okay.

MR. IOTTI: And we looked at 5 elements, really, is all we saw so far. There might be 6. It is not that many. It's not that pervasive, in terms of the analysis. And this is the worst case.

MR. CHANG: Yes. That's the worst.

MR. KUO: Now I can continue? By that approach, if you were able to show that the lateral -- upper lateral support beam is okay, then we have resolved the problem for this lateral support beam. But then, by using the other approach, zero tension, you probably would show a lot of cracking in the concrete.

My question is that: Is there any impact of extensive cracking in the concrete?

MR. IOTTI: I don't know. I guess I'm a little at a loss to answer your question directly, as what is the effect of having extensive cracking in the concrete.

Certainly, when we ran the case of the tensile strength equal to zero, we found that everywhere the rebar stresses were low as compared to the allowable maximum was 22 ksi (sic) versus 24; the compressive strength in the concrete would also be well within the allowable, like a third of the allowable. The strain in the rebar was something in the order of 20 percent of the allowable use strain -- not

the allowable use strain -- the use strain. So, yes, we have cracking in the concrete. But there are no adverse consequences from that cracking.

MR. KUO: That's basically what I'm asking.

MR. IOTTI: Right.

MR. KUO: If you can make an assessment --

MR. IOTTI: That is in table 1 of the affidavit. There is an assessment of the rebar stresses, compress stresses in the concrete, straining the rebar as a function of the tensile strength assumed; for zero psi you have all the values. We don't think there is a problem. I don't know whether you remember this or not -- the first three columns of table 1 give you the results for the tensile strength equal to zero psi.

MR. KUO: Right. These are all numbers. I think what I'm asking is that, if you can provide us an assessment, in words, what the effect of this concrete cracking would be, in terms of the structure.

MR. IOTTI: Oh. None. None. I don't know what words I can give you other than "there is none."

MR. KUO: But why? I think we have to have a little basis.

MR. IOTTI: Okay. There are two things that those walls have to fulfill.

They are not -- these are interior structures of the

containment. There is no requirement for them to be leak tight. They have to be able to carry load. Okay? And they have to be able to distribute the load due to post -- call it post-event, if you will, conditions initially the codes that these structures will seal, which will cause the extensive cracking, will be the mechanical loads. At the time the mechanical loads are imposed on the structures, the concrete has not really cracked very much. Even if it did it would still carry the mechanical load. Bear in mind the temperature load occurs later.

After the temperature load has occurred there is virtually no loading left for these structures to accept other than possibly a seismic event.

They have assessed the capabilities of the structures to accept the coincident seismic event together with the mechanical and thermal load.

If we were not to input the seismic event simultaneously but input it later; number one, we would find there would be less cracking to start with and; number 2, certainly if we accept all three of them simultaneously we can accept one of them separately by itself.

So, as far as I'm concerned, the structures fulfill every single functional requirement expected of them. And there is none other. Those are the words that I would use

to justify there is no adverse consequence of the cracking in the concrete.

It's not a containment. It's not that I have to maintain leak tightness in these structures. All I have to do is maintain structural integrity and the ability to accept loads coming in from environmental and other places.

MR. COSTANTINO: The case really has to be more that the seismic is small?

M.. IOTTI: I don't recall any. Is the seismic really that small?

MR. COSTANTINO: Do you remember -- that was the August -- the numbers were very small.

MR. IOTTI: I just didn't want to say anything because I don't remember.

MR. COSTANTINO: Okay. If you are going to write something down about the seismic cases, they are small rather than --

MR. IOTTI: Right. I recollect that the case was made where the seismic loads per se were small.

MR. COSTANTINO: Because the seismic analysis was so incorrect, because of improper boundary condition.

MR. IOTTI: Was it boundary or asymmetry?

MR. COSTANTINO: Due to the asymmetry, that was assumed. That last thing should be because the seismic analysis was incorrectly performed.

MR. BURWELL: Let's go off the record a minute.

MR. IOTTI: I just want to qualify if we can stay on the record for a minute. The seismic analysis that Professor Costantino is referring to was the seismic analysis of the structures themselves because there was another of the primary system, whose loads come back into the upper lateral, that one is not in question. Or at least it wasn't discussed. It was assumed that it was done right.

MR. BURWELL: Let's go off the record a minute.

(Discussion off the record.)

MR. BURWELL: On the record.

MR. KUO: As we said before, we all recognize there is some limitation in your program and you have provided us some verbal justification a while ago, why you think the approaches that you have taken are conservative.

We basically agree with your conclusion, based on our own independent check. We still are not quite sure what conservatism is inherent in your calculation.

We would still have a few questions to clarify with you. But before we do that, I would like to make a request that you provide a written justification for your approach -- for the approach you have taken, both for the upper lateral restraint beam and for the concrete support. We don't expect any new calculations.

MR. IOTTI: Just words.

MR. KUO: Just words. Put together what you have. With that, I would like to turn, more or less to clarify a few more details in your calculations.

MR. BURWELL: Do you understand what he's asking for when he asks for a verbal justification?

MR. IOTTI: My understanding is that applicants have performed a set of calculations, some of which can be judged to be suspect because of the limitation in the computer codes. So he needs words that places the limitations of the code in terms of result predicted by the code in a proper perspective as to why, even though the prediction may not be correct, it would be conservative. And he wants those words to reflect conservatism for the prediction of the upper lateral restraint itself, stresses; and he would like similar words insofar as the concrete. So I would have to discuss both the calculation performed for zero tensile strength and the 450, without doing any additional calculations. Just place it in perspective.

Is that a fair statement?

MR. KUO: Yes, I think so.

MR. REICH: Let me take you to a couple of questions on your write-up. This pertains to your write-up dated August 21st.

MR. IOTTI: This letter here, Gibbs and Hill, August 21, '84; submittal from Gibbs and Hill to -- actually it's really a --

MR. KUO: It's really a letter from Mr. Ballard of Gibbs and Hill to Mr. Joe B. George, of Texas Utilities, with copies to the Commission.

MR. BURWELL: The letter is identified by GTN-69363.

MR. REICH: I take you to sheet number 55. There is where you take a very conservative case and you say that the stresses in the beam assumed fully constrained at each end. This is for the upper lateral restraint; okay?

Now, in this case here you did this calculation with a delta T 282 minus 70 -- 212; right?

MR. IOTTI: Right.

MR. REICH: Now, there is yet another case. If you look at your letter, page 3, the attachment to the letter, "minutes of the meeting." And there is page 3 on top, you have the main temperature --

MR. IOTTI: It is higher.

MR. REICH: It says over there 370. I want to ask you two questions about that, actually.

I have seen different numbers. In some places you had 340, which you said you calculated. Some places I have seen a number like 352, being used.

MR. KENKRE: 355?

MR. REICH: 355. I'm sorry. 355.

What is the real number for that main steam case; okay? And why did you cnly do this very conservative case? Why didn't you show the other case, too? That's one question.

MR. KENKRE: This calculation was done only because it was requested. Okay?

MR. RINALDI: We were trying to get to the limiting case, I guess.

MR. IOTTI: There may be a misunderstanding. The reason this calculation was done is because, if you go back to the records of the hearings, you will find that this is the calculation that is always appearing because the issue was always the local case. We issued the main steam later.

You have to understand, if you assume this you can have problems. We have always maintained that's a bad thing to assume, the wall is infinitely quick, to begin with.

We essentially reproduced what had been discussed at hearings; okay? The main steam line does give you higher temperature.

MR. REICH: And would give you a higher force?

MR. IOTTI: Certainly.

MR. REICH: What should the number be for the main steam line, 255 --

MR. IOTTI: 355.

MR. REICH: 355? 340? In terms of temperature, or should it be 370?

MR. IOTTI: 370 is not the correct number, I can tell you that much. It was a conservative -- 355, I think, was the calculated --

MR. KENKRE: 71, 340 for the upper and 350 for the lower --

MR. KUO: 355 was referred to. What is the real one?

MR. IOTTI: That's the one reported on the affidavit, I believe.

MR. REICH: Is it 370 you have there?

MR. IOTTI: The temperature on page 10 of the affidavit would state the temperature in both upper and lower lateral supports rises for approximately 355 fahrenheit at about --

MR. REICH: It should be 355.

MR. IOTTI: Approximately 355; right.

MR. COSTANTINO: Then that letter should be changed; right? Bob? Or whoever? On page 3 of the letter from Gibbs and Hill that we are talking about, it implies that the beam is designed for 370. At the top of the page.

MR. IOTTI: I'm not so sure that it is implied.

I think what needs to be changed is the word "as desi gned." It's really "as analyzed." Because we used 370 in our analysis. That beam was never designed for any temperature in terms of the beam itself.

So we need to change is the word there to "as designed."

MR. COSTANTINO: What do you mean "as analyzed"? In the NASTRAN computer run?

MR. IOTTI: Yes.

MR. KUO: What's the real number we should use?

MR. REICH: He said 355.

MR. KUO: I didn't hear it.

MR. IOTTI: My best recollection is when I look at the output of the heating 5 code and the rehab that generated the input, it was about 355.

MR. KUO: So we could go ahead and use that number?

MR. IOTTI: Right. I don't know why this is 340 and 350.

MR. REICH: Let me ask you this, then: When you later on -- let's say, for instance, on page 60, okay? When you deal with the main steam break case, 1-9, and you get these values, let's say F sub BX; and F sub BY, in the beam itself you are getting some sort of bending; am I right?

MR. KENKRE: Yes.

MR. REICH: Now, those values there come because of the computer code. Those forces, actually -- forces that go into bringing you that BX and BY, result because you are using the computer code with zero? Is that with zero shear? Zero tension; I'm sorry.

I'm just asking if the two values of F sub BX and F sub BY; need number 60?

MR. KENKRE: Those are really coming from the fixity of the beam on the wall.

MR. REICH: Yes, but they are coming -- at what condition did you get this? The fixity is due to some sort of force interaction that you are getting between the beam and the wall and it comes from the computer output.

Now, in that computer output you assume zero tensile strength? Or am I wrong? I'm just asking you.

MR. RINALDI: I guess the question is: What do you assume?

MR. REICH: Yes. What do you assume? Let me not give you the words.

MR. KENKRE: Run 9. I'm not sure whether --

MR. COSTANTINO: We are not asking for those answers now. We are just bringing up --

MR. REICH: No. Maybe just add in the letter "what do you use"; that's all.

MR. IOTTI: I can speculate and we can give you

the correct response, but I think it will be 450.

MR. BURWELL: Particularly in case 2.

MR. IOTTI: That's correct, because we are talking about maximum compression and that occurs at maximum compression at the wall.

MR. RINALDI: So that would be the worst case in predicting --

MR. IOTTI: For the upper lateral.

MR. KENKRE: For the maximum compression.

MR. COSTANTINO: I have some more questions.

MR. IOTTI: Wait a minute. I have to make a note. So you need an explanation as to what case sheet 60 corresponds to; and confirm max temperature of main steam line break; B, is what case page 60 concerns. And you want all of this as part of a written reply.

MR. COSTANTINO: I have some more little items.

MR. IOTTI: Sure. I just want to make sure I don't miss any.

MR. COSTANTINO: On page 4 of the letter, if you look at the middle of the page it says, "to support the position that the assumed direction boundary conditions are adequate --" that sentence it seems to me to need change, the S direction boundary conditions are incorrect. The boundary conditions are not adequate.

MR. IOTTI: I don't want to quibble over

semantics.

MR. COSTANTINO: I don't know if that's so much semantics. Boundary conditions are either right or wrong.

MR. IOTTI: I understand. But when you do modeling, you know very well on occasion you make assumptions that may seem to be incorrect and you know that they are not, you know, the proper conditions, but you think they are adequate for the purpose that you want.

MR. COSTANTINO: Well, I agree with you. All I'm saying is the reason why we look at calculations is because everybody agreed the boundary conditions were incorrect for the seismic problem. Okay? So it just seemed that should be -- I guess we agree to that.

And on page 5 of the letter, now --

MR. IOTTI: Do you want that in writing?

MR. COSTANTINO: Something has to modify that statement, it seems to me. Okay?

MR. IOTTI: Okay.

MR. COSTANTINO: On page 5, subparagraph C, "calculations were developed and reviewed based on the assumptions that the beam --" page 5, subparagraph C, which applies to the calculation of upper bound estimates of thermal stresses in the beam.

MR. IOTTI: Right. We don't look at the high case.

MR. COSTANTINO: So that paragraph has to be modified, giving an upper bound.

MR. IOTTI: C on page 5; right?

MR. COSTANTINO: C on page 5. Right.

MR. SHARMA: I have a question. When the concrete -- these walls are fixed at the bottom?

MR. IOTTI: I believe that's correct. Yes.

MR. SHARMA: Now, what of the shear if you have zero strength concrete and you have shear developing at the bottom? What will happen to the shear force?

MR. IOTTI: We'll have to go back to the calculation. There is a shear, obviously.

MR. CHANG: The concrete stress is assumed to be zero but not necessarily assumed the concrete and steel together still can hold up the structure. In the design of concrete structure we always assume the tensile strength of the concrete structure is completely zero. That's the usual assumption.

MR. SHARMA: That's right.

MR. CHANG: So, when we design with zero tensile strength, that steel can hold up the structure because the steel, combined action with the concrete and the portion of the shear which is not cracked -- portion of the shear resistance which is not cracked.

MR. IOTTI: Let me make sure I understand your

question. You want to know what the shear is at the bottom?

MR. SHARMA: The steel will then provide the membrane resistance. Now, if you have shear at the bottom and you assume the strength of the concrete to be zero --

MR. KENKRE: The tensile strength of the concrete is zero. But there's shear strength because of interlocking of everything.

MR. SHARMA: If you put tensile strength to be zero then I assume --

MR. IOTTI: That's the off-plane. That shear will be exhibited as an off-plane shear through the thickness.

MR. SHARMA: What shear do you keep? Do you have any idea?

MR. IOTTI: .2, I think.

MR. CHANG: That shear -- we took a bend -- we talk of bending, that shear would be consistent to the bending moment.

MR. SHARMA: Okay. In my opinion, shear strength and tensile strength are related. All right? If you assume shear strength to be in one plane, then obviously if you take a component, it will have a tensile strength at 45 degrees. So what does it mean to have a zero tensile strength concrete?

MR. CHANG: Zero tensile strength concrete does not mean the concrete cannot have resistance, okay?

It doesn't even address the shear in that direction. The shear is computed inconsistent of the bending moment of the wall. There's an assumption there --

MR. IOTTI: I think you lost them.

MR. COSTANTINO: Am I allowed to use the board? I think all Sushil is saying, Harry, if you look at the failure it's something like this with some kind of a cap someplace. And if this is the tensile capacity, then you have some kind of a shear capacity. If you are saying you have zero, at zero you have zero shear strength. Is that right; Harry?

MR. CHANG: Zero strength.

MR. COSTANTINO: That's all Sushil is saying, he questions that part.

MR. SHARMA: I need some clarification.

MR. IOTTI: What you want to know is what is the shear at the bottom of the model. We'll provide you that. There is a number.

MR. RINALDI: One more item on this response. I'm assuming --

MR. IOTTI: Excuse me a second, Frank. Let me write this down. Go ahead.

MR. RINALDI: We are assuming that this modified

NASTRAN code was only used for the upper lateral beam restraint evaluation.

MR. IOTTI: That's correct.

MR. RINALDI: If, in the reply you could restate that? Maybe you have done it before, but I think we would like to have it restated.

MR. IOTTI: Okay.

MR. COSTANTINO: One point of clarification. The linear seismic run, the SSE run that we looked at, was that a SAP or a STRUDL? Was that SAP?

MR. IOTTI: I think it was STARDYNE.

MR. KUO: What kind of schedule are we talking about?

MR. IOTTI: What is today, Tuesday? I could try and have something in the mail Monday.

MR. BURWELL: Fine. In the meantime, I hope you gentlemen know what you will say, whether or not to proceed to put pencil to paper and give us some results soon -- get this work in reasonably soon.

Okay. I thank you gentlemen. I think that was a useful meeting and I think we have come together.

(Whereupon, at 11:55 a.m., the conference was concluded.)

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This is to certify that the attached proceedings before the UNITED STATES NUCLEAR REGULATORY COMMISSION in the matter of:

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