ORIGINAL

UNITED STATES
NUCLEAR REGULATORY COMMISSION

IN THE MATTER OF: **DOCKET NO:** 50-322-1 (OL)

LONG ISLAND LIGHTING COMPANY

(Shoreham Nuclear Power Station)

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

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In the matter of: :

LONG ISLAND LIGHTING COMPANY : Docket No. 50-322-1 (OL)

(Shoreham Nuclear Power Station): :

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State Office Building,

Veterans Memorial Highway,

Hauppauge, New York.

Wednesday, November 14, 1984.

The hearing in the above-entitled matter was reconvened, pursuant to adjournment, at 9:00 a.m.

BEFORE:

JUDGE LAWRENCE BRENNER, Chairman,
Atomic Safety and Licensing Board.

JUDGE PETER A. MORRIS, Member,
Atomic Safety and Licensing Board.

JUDGE GEORGE A. FERGUSON, Member,
Atomic Safety and Licensing Board.
(Not present.)

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APPEARANCES:

On behalf of the Applicant:

TIM ELLIS, Esq.
Hunton and Williams
700 East Main Street,
Richmond, Virginia 23219

On behalf of the Nuclear Regulatory Commission Staff:

ROBERT G. PERLIS, Esq.
Office of the Executive Legal Director

On behalf of Intervenor Suffolk County:

ALAN ROY DYNNER, Esq.
JOSEPH J. BRIGATI, Esq.,
Kirkpatrick, Lockhart, Hill, Christopher
and Phillips,
1900 M Street, N. W.,
Washington, D. C. 20036

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C O N T E N T S

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		Id.	Evd.
EXHIBITS (Cont'd)			
Suffolk County No. 46:		26556	26557
Attachments 5 through 9 to			
7/25/84 ltr to Am Bureau of Shipping			

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P R O C E E D I N G S

JUDGE BRENNER: Good morning.

Are there any preliminary matters?

MR. DYNNER: Yes.

I would like to address the issue, if I may, of the pistons, to give the Board a status report on where that matter stands, and how it may be resolved.

In the first place, there is testimony, very, very short, on one particular topic which the County would like to put into the record, with a few accompanying exhibits dealing with the peak firing pressures of the EDGs. For technical reasons it would seem this could be done in the context of the pistons or any other components. And it is our understanding that any portions of the litigation evidence can be drawn upon for any component to the extent it is relevant to that component.

The parties have reviewed this testimony and none of the parties have any cross-examination concerning this testimony, so that it would consist of stipulated testimony by the County in this limited area.

Because it is very short, I would like to read that testimony into the record, and have it adopted by one of the County's witnesses who is present as the County's testimony. And then we will furnish to the Board and the parties the exhibits and make them part of the record, that

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are alluded to in this short testimony.

Following that, it is the County's position that based upon the County's consultants' inspections of the pistons -- AE pistons from the 103 engine that were run in the latest test for about 740 hours, that the County's concerns regarding the scuffing, scoring, and other attributes of the piston skirts thought to be a problem have been allayed.

In addition, we have received a copy of an inspection report from LILCO indicating that non-destructive examination disclosed that there are no cracks or other reportable defects in the pistons. It will therefore be the position of the County that, subject to a confirmatory report from the NRC Staff that the pistons, in the Staff's view, are acceptable, the County intends at that point to withdraw the piston contention.

If that approach is acceptable to the Board, I would like to read into the record the short statement which would constitute the only testimony for the record on this matter, which deals really with firing pressures and is relevant in reality to the other components that are at issue.

As you know, the reason why this has to be done is simply that the organization of the County's direct testimony was put in as an interrelated group, and not

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purely in a component-by-component method. Therefore, there is certain evidence and testimony which relates to more than one component. This particular evidence that I would like to read at this point happened to be in the piston section of the direct testimony.

So if I may proceed, I would read into the record--

JUDGE BRENNER: On that you said you would need some exhibits to provide for the record which are not prefiled exhibits, I take it.

MR. DYNNER: No, these are prefiled exhibits, and I will use the same exhibit numbers. It is just that copies will be given to the Court Reporter and they will then, with the Board's approval, be made a part of the record.

JUDGE BRENNER: All right. Let me go to the other parties first, and I will come back to you, Mr. Dynner.

Mr. Ellis?

MR. ELLIS: Judge Brenner, we agree with Mr. Dynner and what he said. There are a couple of clarifications I suppose I would add.

The first is that the confirmatory report by the Staff I assume can be an oral one and is intended to be an oral one, and indeed, I will let Mr. Perlis speak to this. But I expect, so that we can tie this matter up, that that

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may be available orally today from Mr. Perlis once he has had an opportunity to check with Mr. Berlinger or others with whom he must check.

Secondly, with respect to the testimony, as Mr. Dynner said, we have no cross-examination of it, and it would come in as uncross-examined -- or as testimony of the County which LILCO elected not to cross-examine. And I think that's the same for the Staff as well.

JUDGE BRENNER: Wait a minute. Is it stipulated, or is it just something that you have elected not to cross-examine?

MR. ELLIS: It is something we have not cross-examined. There may be other portions of the record that relate to peak firing pressures which Mr. Dynner does not want to be precluding from using and which we do not want to be precluded from using.

JUDGE BRENNER: That wasn't my question. My question is is it correct that everyone agrees that what Mr. Dynner is going to read into the record is true and correct? What we do with it in combination with other facts in the record is a separate question.

MR. ELLIS: The testimony reports what is in the exhibits and to that extent, it is true and correct.

There may be additional information in the record which we would want, and I am sure Mr. Dynner and

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Mr. Perlis may want to be able to refer to, which relates to the same issue.

JUDGE BRENNER: You are not distinguishing between the two questions that I distinguished.

MR. ELLIS: I'm sorry, Judge Brenner.

JUDGE BRENNER: I don't know what he is going to read in, but you do, I assume.

MR. ELLIS: Yes, sir.

JUDGE BRENNER: Is what Mr. Dynner is going to read in accepted by LILCO as being true and correct? You told me it is true and correct that the exhibits contain that information, but that is not the answer to my question.

MR. ELLIS: Well, the reason-- Let me be concrete about it.

If the testimony about to be read in says firing pressures are X and the exhibits reflect that the firing pressures are X, that's what we say is true and correct. There may be other evidence in the record relating to what firing pressures are, and we do not want to be precluded, and Mr. Dynner doesn't want to be precluded and Mr. Perlis doesn't want to be precluded to using that other evidence in the event that it seems to us appropriate to do so.

Does that help explain, Judge Brenner?

JUDGE BRENNER: I think so. When I hear what is being read in and know the exhibits supporting it it might

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help me a little better. I was trying to understand whether you are going to challenge the veracity of the exhibits being relied upon, but I take it you are not.

MR. ELLIS: No, sir.

JUDGE BRENNER: Mr. Perlis?

MR. PERLIS: The Staff also does not plan on challenging either the testimony or the exhibits.

In terms of the Staff's confirmatory analysis, I am going to need to talk to some people in Washington, or have someone here talk to people in Washington. And hopefully I can report to the Board after the first break if the Board needs a report today.

JUDGE BRENNER: Yes. I would like to wrap up the whole issue very quickly, and unless there is a reason why the Staff cannot do its work immediately, I would like it done immediately. I don't want to close the record here and have something like that hanging if we can avoid it.

Did you envision an oral report from the Staff, Mr. Dynner?

MR. DYNNER: I frankly had envisioned a confirmatory written report that the Staff has no problems, but if the Staff is prepared to make the representation on the record of this proceeding, I would have no objection as long as it is not simply somebody telling us something off the record.

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JUDGE BRENNER: Let's see if we can get it done on the record, and then do it both ways, get it done on the record and then get a written report in a more extended time frame.

I take it, speaking of writings, that there will be no written settlement agreement on the pistons, but what we have here on the record is satisfactory to me at least, if it is satisfactory to the parties.

MR. ELLIS: Yes, sir. With the withdrawal of the contention that is satisfactory.

JUDGE BRENNER: Can somebody give us some basic facts as to what the non-destructive examination of the pistons which, according to LILCO, disclosed no cracks consisted of?

MR. ELLIS: Yes, sir.

The skirts of the AE pistons on the 103 engine after the accumulation of a total of 740 hours at or above 3300 Kw were examined by liquid penetrant examination, and this is liquid penetrant examination of the skirt bosses which were the subject of the testimony on the piston contention. On all eight skirts that were examined by liquid penetrant, the results of the examination disclosed no indications.

JUDGE BRENNER: And that tinplating was visually examined?

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MR. ELLIS: Yes, sir, that tinplating on the piston skirts was examined by LILCO, the Staff, and by the County's consultant.

JUDGE BRENNER: Can the Staff tell me who looked at it for the Staff? I guess I have two questions: Who for the Staff looked at the tinplating? And who for the Staff is going to confirm or not confirm, as the case may turn out to be, the reports of the non-destructive examination of the pistons performed by LILCO or its agents?

MR. PERLIS: Adam Hennksen and Paul Louzecky examined the pistons, and I believe it is Larry Van Fleet --all these gentlemen are with Batelle -- and will be looking at the non-destructive examination.

JUDGE BRENNER: To the extent you have any persons involved in looking at the non-destructive examination that have not testified as witnesses before us, we don't know their qualifications. So as a part of the report I guess we would like to hear a little bit about their qualifications.

I assume the Staff will make sure that anybody examining the LILCO work on non-destructive examination will have the appropriate qualifications, and part of the report will be what they did to do their work. Did they do any looking themselves? Did they just look at the paper report, et cetera?

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MR. PERLIS: I understand. Are you speaking about the follow-up written report, or are you speaking about --

JUDGE BRENNER: Orally.

MR. PERLIS: -- orally this morning?

JUDGE BRENNER: Yes. And I assume more details will be in the written report. I don't mean a full statement of professional qualifications, just enough so that we have some insight into their area of expertise.

MR. PERLIS: I understand.

JUDGE BRENNER: That's acceptable to us. I guess once again we will commend the parties for their being able to reach an accommodation on an issue that started off in controversy. And I know the process has been a long one, and I believe not an easy one, given the number of people and the number of issues, and more to the point, subissues in support of the issues involved.

As of this moment we don't know if it is fully settled. I hope it is a fair statement on my part -- well, not a statement on my part. It's a statement of the parties that it will be settled and the contention will be withdrawn, subject to the confirmatory report from the Staff, which we hope to receive orally as a preliminary matter.

And if the oral report confirms matters with the

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bases so that we can accept the conclusions of that oral report, then we will at that point deem the settlement consummated, so to speak, and the contention withdrawn.

If anything later turns up in the written report that is inconsistent with what any party believed, it will be the obligation of that party to come back to us.

MR. PERLIS: Excuse me, Judge Brenner. Some of the people we will need to talk with are in Richland, Washington. With the three-hour time difference, it may not be until after the luncheon break before we will be able to make our report.

JUDGE BRENNER: Okay.

All right, Mr. Dynner. I guess it is your turn to testimony.

MR. DYNNER: Well, obviously I'm not testifying, but this is the uncontroverted testimony concerning the peak firing pressures as follows:

The peak firing pressure as measured by a Keene gage in cylinders of the EDGs at full load (3500 Kw) is known to be in the range of 1560 to 1720 psig, and at overload (3900 Kw), the peak firing pressure is in the range from 1660 to 1800 psig. Footnote: See Exhibit 46 at Documents 5 through 9.

JUDGE BRENNER: That is a Suffolk County exhibit?

MR. DYNNER: Suffolk County Diesel Exhibit 46.

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One must also remember that firing pressures may differ from cylinder to cylinder and engine to engine. TDI gives no specific authoritative peak firing pressure for the DSR-48. Rather, its manual for operation of the ETGs permits a variance in peak firing pressures of the cylinder in one engine. Footnote: TDI Instruction Manual at 8-3, Suffolk County Diesel Exhibit 9. This means that the minimum peak fire pressure read in any cylinder may not be exceeded in any other cylinder by more than 200 psi.

TDI has testified that the peak firing pressure of the EDGs at 3900 Kw is about 1800 psi. Footnote: Deposition of Gerald Edgar Trussell, May 7, 1984, at 128 to 29, Suffolk County Diesel Exhibit 10.

That concludes this limited testimony.

JUDGE BRENNER: Okay. Thank you.

And you will be providing copies of those exhibits to the Reporter today?

MR. DYNNER: Yes, sir.

JUDGE BRENNER: And I assume you want just the portions of the exhibits cited admitted into evidence? Will that be the procedure agreed upon by the parties?

MR. DYNNER: Yes, that's satisfactory.

MR. ELLIS: Yes, sir.

MR. PERLIS: That's fine.

JUDGE BRENNER: Let us know when you have the

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mechanics and are able to do it.

Are there any other preliminary matters?

(No response.)

JUDGE BRENNER: We have received no written papers this morning. I was hoping to see some.

When will we get the County's answer?

MR. DYNNER: Yes, Judge. As I mentioned yesterday, we are expecting that by Federal Express this morning it will be delivered to the County's Executive Office, and we have someone over there. At the same time, by the same Federal Express package, we will be getting the report that you requested concerning the remanded issues from the Appeal Board decision.

JUDGE BRENNER: Is that going to be a joint report?

MR. DYNNER: No, sir.

JUDGE BRENNER: If we could get it before the lunch break that would be helpful.

And the same goes for the reports from the other parties on the remanded issues, if they will be available.

And the Staff is going to give us the answer to the County's motion to reopen -- I mean to LILCO's motion to reopen and supplement by the time we adjourn today. Correct?

MR. PERLIS: That's correct. It is being flown

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up from Washington this morning, or this afternoon.

JUDGE BRENNER: All right. Thank you.

Is the other report, though, going to be delivered along with it, or in advance?

MR. PERLIS: Mr. Reis will be bringing up with him both reports.

MR. DYNNER: I should state for the Board's information that the County's response does not reflect the latest information concerning the pistons that we have discussed this morning.

JUDGE BRENNER: All right. Very good.

I think that the parties have accomplished a lot on the piston subject, as I said, and we do appreciate it.

We can now proceed with the questioning of the panel, and as we stated, the panel consists of witnesses for all the parties: Drs. Wachob and Rau for LILCO, Dr. Anderson for Suffolk County, and Dr. Bush for the Staff. That's correct, just those four people as we had indicated previously.

Whereupon,

HARRY FRANK WACHOB,

CHARLES A. RAU,

ROBERT N. ANDERSON,

and

SPENCER H. BUSH

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resumed the stand and, having been previously duly sworn, were examined and testified further as follows:

JUDGE BRENNER: We will begin with LILCO, and remember we told you you can direct the questions initially at any witness, and this goes for each questioner. Don't carry the subject along too far without giving the other witnesses an opportunity, an express opportunity to comment directly on the answer. That way we can avoid the witnesses having to guess when they need to jump in.

EXAMINATION

BY MR. ELLIS:

Q Good morning, gentlemen.

JUDGE BRENNER: They have all previously been sworn, as we know.

MR. ELLIS: Yes.

BY MR. ELLIS:

Q As Judge Brenner indicated, I will direct questions to one or more of you specifically perhaps, and then after an item has been pursued, I will give any member of the panel an opportunity to comment on what has been said. Don't hesitate, any of you, to indicate to me that you would like an opportunity to comment, and I will certainly afford you that opportunity.

I would like, though, to pursue some matters until such time as they are developed before the comments.

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I would like to begin this morning in the cam gallery area.

MR. ELLIS: And in order that we begin with the big picture, a common ground, so to speak, Judge Brenner, I would like to begin by directing some questions to Dr. Rau that are designed to put, literally and figuratively, a picture before us.

BY MR. ELLIS:

Q Dr. Rau, did you, at my request, prepare, either last night or this morning, a schematic or a diagram depicting what you observed in connection with the appearance of the cam gallery cracks observed in the original 103 block and in the 101 and the 102 block cam gallery areas?

A (Witness Rau) Yes, I did, Mr. Ellis.

Q All right.

And do you have those before you?

A Yes, I do.

MR. ELLIS: Judge Brenner, I would like to distribute these to the Board. The parties have them.

(Distributing documents.)

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MR. ELLIS: I would like to mark these for identification, if I may. It would be LILCO Block Exhibit B-61 and 62, and I would suggest that we mark the original 103 block cam gallery single page -- it is a single page exhibit entitled "Original 103 Block Cam Gallery." That one should be LILCO Block Exhibit 61.

The second one is also a single page entitled, "101, 102 Block Cam Gallery," and that we ask be marked Exhibit 62.

JUDGE BRENNER: All right. They will be LILCO Exhibit B-61 and LILCO Exhibit B-62 for identification.

(Whereupon, the documents previously referred to were marked as LILCO Exhibits B-61 and B-62 for identification.)

JUDGE BRENNER: Off the record.

(Discussion off the record.)

JUDGE BRENNER: On the record.

BY MR. ELLIS:

Q Dr. Rau, do you have before you what has been marked LILCO Exhibit B-61 and B-62?

A (Witness Rau) Yes, I do.

Q Were these prepared by you?

A Yes, sir.

Q And will you tell us what they depict, please,

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starting with LILCO Exhibit B-61?

A Yes. Let me indicate that perhaps for perspective you may wish to refer to Suffolk County Exhibit 77, which was also prepared by me by Failure Analysis. Suffolk Exhibit 77 was a cross-section or a profile view through the cam gallery saddle region and I simply refer to that to indicate that LILCO Exhibit B-61 and 62 is just a portion -- it's the same view and a portion of the cross-section through the cam saddle area, if you like, the upper left portion of Suffolk Exhibit 77.

What I am depicting in LILCO B-61 are the cracks in the cam saddle -- the cam gallery saddle region in three stages: at the top left I am depicting what the casting shrinkage cracks looked like immediately after the casting, if you like, after it has been broken out of the mold. In the second or middle sketch I have indicated the same region after the surface of the cam gallery had been gouged out or ground away, leaving however a portion of the casting shrinkage cracks. And on the third sketch at the bottom, I have indicated the appearance in profile after the repair weld had filled up the gouged out area.

I have also indicated at the bottom -- or third sketch at the bottom of LILCO B-61 the presence of an additional weld shrinkage crack located in the HAZ, that's an abbreviation for the heat-affected zone of the repair

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weld, an additional crack. It is in fact these two cracks, -- that is, the one I have labeled casting shrinkage cracks on which we have testified about the thick black oxide and the weld shrinkage cracks in the heat-affected zone from the repair weld, on which we have testified there is a very thin oxide -- and the differences between those two cracks which form the basis for many of the opinions which Failure Analysis Associates has offered in this hearing.

The second exhibit, LILCO B-62, is a schematic representation of the exact same profile or cross-section views through the cam gallery saddle region of Blocks 101 and 102, again depicting schematically smaller casting shrinkage cracks in the vicinity of the surface of the cam gallery immediately after casting. And the second view indicating, after grinding away prior to weld repairing, the removal of such indications. And then thirdly, the introduction of the repair weld and the presence of a weld shrinkage crack in the heat-affected zone of that repair weld.

These two exhibits together illustrate what I believe to be the differences in some of the key issues of the causation and the bases for the interpretation of the causation of the casting shrinkage cracks and the weld shrinkage cracks in the cam gallery area.

MR. ELLIS: Judge Brenner, so that the Board and

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the parties have some notion, I intend to return to give the parties -- the witnesses an opportunity to say more about Exhibit B-62. What I would like to do now is to pursue with Mr. Rau for one -- for a couple of more questions the oxide matter that he referred to as furnishing the basis for FaAA's opinions and then turn to the other members of the witness panel, if that is agreeable to the Board.

BY MR. ELLIS:

Q Dr. Rau, would you please tell the Board what oxides were observed by FaAA and what conclusions FaAA drew from that observations of oxides in both the casting shrinkage crack and the weld shrinkage crack in the heat affected zone?

A (Witness Rau) Yes, Mr. Ellis, I will.

On the shrinkage cracks which are depicted in LILCO B-61, I observed, along with Dr. Wachob, a relatively uniform thick black oxide. That oxide extended completely along the crack which, at the bottom of the three sketches in B-61, is labeled as the remaining casting shrinkage crack paren thick oxide paren. I observed an oxide which was between 2/10ths and 5/10ths of a mil, a thousandth of an inch thick and, as I say, relatively uniform along the entirety of the remaining casting shrinkage crack.

There are photomicrographs which have been marked which clearly illustrate that that thick black oxide is

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present continuously along that casting crack, that the oxide is relatively uniform in thickness both on the perlite which forms the basis of the cast iron or the matrix of the cast iron and also along the graphite flakes which are also part of the microstructure of the cast iron. In other words, we've got uniform thickness of oxide along the entirety of that crack as the microstructure goes from graphite flake to Widmanstaetten graphite to perlite, et cetera. It repeats itself.

There is no indication on that oxide of it being porous, of it being rust colored, no indication of pitting, no indication of rubbing, no indications of any beach marks either mechanically induced or due to differences in the thickness of the oxide layer, no indications whatsoever of anything other than a crack which was there from the time of fabrication and oxidized over a range of temperatures starting out relatively high and continuously on down to room temperature.

In contrast to this thick black oxide which is present on the remaining casting shrinkage crack there is another crack labeled as the weld shrinkage crack in the HAZ or the heat-affected zone parentheses very thin oxide.

This crack was clearly observed to have a very thin, almost negligible oxide, if you like. The metallographic cross-section, that is, the cross-sections

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through, in this view, when mounted in plastic, polished metallographically and examined in the microscope, clearly reveal the absence of any thick dark oxide.

It's clear to me through the comparison of these two cracks and the oxides on them that certain definitive conclusions can be reached based on engineering and scientific principles. Clearly the remaining casting shrinkage crack, as I have labeled it, was present long before or under conditions which are much more severe with regard to oxidation than was the crack which has been labeled weld shrinkage crack.

In other words, there's a thick dark oxide on one crack and there's a very thin or negligible oxide on the other, so clearly one of two things has happened: either the casting -- excuse me, the remaining casting shrinkage crack was there at high temperature and formed a thick oxide or, alternatively, it was there for a very much longer time than the crack which has been labeled weld shrinkage crack because the oxide is so much thicker.

The other point which I should make which is very important is that the weld shrinkage crack resides in the heat affected zone of the gray cast iron. It does not, at least over the majority of its area, reside directly at the interface, the immediate interface between the weld and the cast iron.

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Now this is exactly what we would expect in a nickel iron weld because in those areas where the surface was clean and there is no slag from the welding process itself you would expect a good bond between the perlite in the cast iron and the iron nickel. And in fact as the weld bead shrinks after the introduction of the repair weld and introduces tensile stresses in the vicinity of the repair weld, it is in fact the very brittle degenerate Widmanstaetten graphite structure of the original 103 which is the weak link. And it is in fact the heat-affected zone in the cast iron which contains this degenerate Widmanstaetten graphite which is going to crack long before the weld bead, the iron nickel -- which is quite tough -- and long before the joint between the iron nickel and the cast iron is going to crack, and that's exactly what happened.

So what we have are two cracks: that is, the remaining casting shrinkage crack and the weld shrinkage crack in the heat-affected zone, both of which are in gray cast iron, they are both of the same material. One has got a thick dark oxide, one has got almost no oxide. It's the same material, they are right adjacent to each other so clearly either one was there at high temperature when the other was not or one was there an awful lot longer than the other.

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For both those reasons, it's clear that neither of these cracks were introduced during operation but in fact they are both introduced during fabrication. The remaining casting shrinkage crack was originally formed during the solidification of the mold -- of the block, excuse me -- only a portion of it was ground out, gouged out, before the repair weld was made and the weld shrinkage crack in the heat-affected zone was formed during the shrinkage after the repair weld procedure.

There are many other points but I think those are the major ones.

JUDGE MORRIS: May I ask one quick question, Dr. Rau?

Do you distinguish between the casting shrinkage cracks and hot tears?

WITNESS RAU: There is a continuous spectrum, not very definitively, Judge Morris. There are certain distinctions which are sometimes made, but in point of fact they both result due to stresses introduced during the solidification process. And in the context of the discussion here I don't think we need to make a distinction between the two. It may only be a matter of deciding if there are any differences of opinion with regard to what temperature during the solidification process, the stresses introduced by shrinkage, may have produced the tears, and

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then one of us may call it a shrinkage crack and one of us may call it a hot tear, but for all intents and purposes it's the same thing.

JUDGE MORRIS: Thank you.

MR. ELLIS: Judge Brenner, I think this would be an appropriate time to ask Dr. Anderson if he cares to comment on Dr. Rau's remarks.

WITNESS ANDERSON: Yes, if I may.

I tried to jot down some notes and I haven't had a chance to study these two documents in detail, but I will give you what comes to mind.

First of all, I have one that says 101 and 102 Block Cam Gallery. These two sheets do show the two differences and I think they do it quite adequately and so we can talk from them.

My understanding is that the 101 and the 102 has not been sectioned like the 103, so that there really isn't an adequate way that we can determine whether this does represent 101 and 102.

For example --

JUDGE BRENNER: I wonder if I can make a suggestion? As Mr. Ellis said, we're going to come back to the details on 101 and 102 and you'll get that opportunity. I think it might be -- in fact, definitely it would be better now for you to focus your remarks on what Dr. Rau

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said about 103.

WITNESS ANDERSON: I am.

JUDGE BRENNER: Okay.

WITNESS ANDERSON: In this sheet 101 and 102 it says weld shrinkage crack, there is no way that I'm aware of that NDT techniques can tell a weld shrinkage crack from --

JUDGE BRENNER: You're not doing what I said. Focus on 103 and we're going to come back to 101 and 102.

WITNESS ANDERSON: Okay.

103 has been destructively cut, examinations have been made. Failure Analysis claims that the black -- and I put in quotes -- oxide layer is not porous. It very definitely is, because you can still smell the solvent from the cutting solution that was used. It has to be porous in order to carry that through.

Moreover, up until yesterday, I was pretty convinced it was oxide. Now I'm starting to have second doubts, for a very simple reason. I have a report on the microprobe showing oxygen.

Since Failure Analysis has takenthat sample, there has been some rusting of the surface, hydrous oxide. It's an artifact, it has nothing to do with what was originally there.

I asked for a map of where the probe readings were taken so that I would know where they had been sampled.

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And I understand that none is available, there is no record of where they were taken. And so it is possible that the probe readings were taken on the new hydrous oxide, which is an artifact.

So I'm not sure that there is proof at this time that it is an oxide.

With respect to its color being dark, there has been no test for carbon, and I think that should have been done.

I'm also worried now because of the development of the hydrous oxide artifact on the surface that possibly good x-ray work to determine what the original structure was could not be done. We may not be able to do that in the future.

The question of beach marks. I didn't see any. However, my understanding is that cast irons, under some conditions, do have them and, under other conditions, do not. I don't think that's an important point.

The uniformness of the surface from two-tenths to a half a mill, I think is consistent with either theory, that they are pre-existent or that they were growth. And I would have trouble resting a decision on that point.

I think the most important item that has not been discussed by Failure Analysis is decarburization, and I've explained it once; if you wish, I'll explain it again.

WRBwrb

If there is a high temperature oxidation of a crack of a metal, there has to be commensurate decarburization. Carbon is more subject to oxidation than the metal; therefore, there will be a loss, a discernible loss, in the microstructure. And that's a clear indication. And there has been no discussion of it nor have I seen it in photographs nor in the samples, decarburization which would be commensurate with a high temperature oxidation.

That takes care of the notes that I made.

MR. ELLIS: Judge Brenner, I would like to follow up on those now.

BY MR. ELLIS:

Q Dr. Rau,-- Let me take one point at a time.

Dr. Anderson, am I correct that you say that in your view the oxide coating is porous because of the solvent still -- the odor of the solvent was still present?

A (Witness Anderson) Yes. There is a distinct odor of the solvent which helped me identify what the cutting material was. And that is an indication of porosity of the surface coating.

Q Well, before I ask Dr. Rau to comment on that, or Dr. Wachob, do you have transcript page 25,654 in front of you, Dr. Anderson?

A No, I don't.

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MR. ELLIS: May I ask that counsel provide that, please?

Does the Board have that?

JUDGE BRENNER: Yes.

BY MR. ELLIS:

Q I'm looking at line 22, Dr. Anderson.

A (Witness Anderson) I don't have it.

MR. DYNNER: I'll give it to him in just a moment.

MR. ELLIS: I'm sorry.

(Document handed to Witness Anderson.)

BY MR. ELLIS:

Q Dr. Anderson, am I correct that between November 1st and today you have not done any -- not performed any non-destructive examinations on portions of the 103, the original 103 cracks during that period, have you?

A (Witness Anderson) Correct.

Q All right.

And am I also correct that the testimony beginning -- the question beginning at line 22 and carrying over to line 8 on 25,654, that you were asked -- and I quote:

"QUESTION: Did you see, did you observe whether or not the oxide that existed in the cross-sections of the cracks or surface indications in the cam gallery of the original EDG 103 was spongy or porous?

WRBwrb

"ANSWER: You're referring to the oxide alone, the oxide which is characterized on the surface of the cast iron?

"QUESTION: That's my question now.

"ANSWER: You know, it did not appear to be porous or spongy that I could detect. It did not smudge easily, it has a characteristic that made it harder than a disconnected graphite structure. I don't believe that it was porour."

That was your sworn testimony on the 1st of November, wasn't it?

A Yes. But since that time I've had a chance to think about it, and there are different porosities. I could not-- It is still true: I could not see observable porosity, in other words, spaces or some kind of latticework, I could not and did not. But in order for the solvent to have been absorbed, there had to be porosity.

Q Dr. Rau, did you want to comment on the porosity point?

A (Witness Rau) Yes, Mr. Ellis, I would. I have two points to make in that regard.

First of all, in examining the cam gallery regions there are, in fact, cracks there. Those cracks by capillary action will suck up solvents and retain them within the sharp cracks. And the graphite and the degenerate graphite,

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to the extent there are little pieces pulled out... Graphite is notorious for sucking up liquids of all types and retaining them. The fact that you smell the solvent bears no relationship to whether or not the oxide is porous.

The second point is, we have definitive metallographic examinations at high magnifications under a microscope which show that the oxide is not porous.

That's all I have to say.

Q Dr. Bush, you, I believe, testified previously that you considered the oxide not to be porous. Is that still your testimony?

A (Witness Bush) Yes.

Q All right. Let's turn to the second point.

The second point, Dr. Rau, that Dr. Anderson mentioned was hydrous oxide--

JUDGE BRENNER: Mr. Ellis, just a moment, please.

Dr. Bush, why doesn't Dr. Anderson's opinion that because he can smell the solvent he thinks the layer is porous change your mind?

WITNESS BUSH: No.

JUDGE BRENNER: Why not?

WITNESS BUSH: Well, there are many ways of picking up a highly volatile liquid. It does not necessarily have to penetrate the oxide. There are other surfaces there. And I think that to assume that the only

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mechanism would be through the oxide is only one of several approaches. Because when we do cutting we flood with solvent, and I would expect to see it on surfaces that have no oxide, as well. We know that this material in particular has cracks in other regions. So I would tend to go along with capillary action.

JUDGE BRENNER: Dr. Anderson, you're outvoted two-to-one on porosity. What do you think about it, having heard the other two comments?

WITNESS ANDERSON: Well, this was an open surface, so that only cracks were running off of that surface, and I feel that a solvent which is -- a volatile solvent is going to be lost in air. I saw it days, weeks after, I guess, it had been cut, and, therefore, if we had just had a dousing of it and letting it run into troughs it would have been lost.

It had to chemically absorb onto the porous surface that was there. It wasn't just a physical attachment, it's a chemical attachment.

JUDGE BRENNER: A summary in my own mind of what Drs. Rau and Bush just said would be, among other things, that evidence of smelling the solvent is very poor probative information on the subject of porosity of the layer as compared to examination of the microstructure which has been done.

WRBwrb

What do you say to that?

WITNESS ANDERSON: I don't think the microstructure can tell you. Remember how the microstructure was prepared: it was polished, and therefore if I have pores the grits will tend to fill them. You have a problem there. So you tend to somewhat change what you are polishing when you have hard things and soft things.

Moreover, if you do any etching that can change it also.

So, really, microstructure is not the ultimate end-all. Probably the end-all is BET examination, and there just wasn't oxide to do that.

JUDGE BRENNER: Dr. Rau, why was your microstructure examination any good to whatever pores there might have been? Would they have been filled in by the processing, as stated by Dr. Anderson just now?

WITNESS RAU: No, sir. We take great care in the metallographic preparation of the cast irons, as per the standard procedures utilized. We examine the oxide both in the as-polished, etched condition after various combinations of etch and polish. The appearance did not change.

Of course, there are certain circumstances where you can, in fact, cover up certain very small pieces of porosity. But that is not true of the oxide. The oxide is, in fact, hard. There's no way it could have been covered up

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by the polishing procedures which we utilized in this case.

JUDGE BRENNER: Dr. Bush, did you want to add something?

WITNESS BUSH: Yes. I would tend to disagree for one reason, and that is that--

JUDGE BRENNER: Tell me who you're disagreeing with.

WITNESS BUSH: In this instance with Dr. Anderson, in the context that we have a very soft ingredient here, namely, graphite. And certainly if one is worried about filling in porosity and so forth, then one has to assume the loss of a lot of graphite in the polishing operation, too, by the pulling out by the same mechanism. At least in the photo micrographs I examined, the graphite appeared to be retained, which, to me, is an indication of a careful metallographic process.

JUDGE BRENNER: Dr. Anderson, you looked like you wanted to add something.

WITNESS ANDERSON: Well, I had one more thing.

An oxide is essentially the type of hardness that we're using to polish the specimen. So they are about the same.

Yes, it's true that the graphite will tend to be pulled out from the matrix. We're also encapsulating this in plastic to keep that matrix from pulling out. And that

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could alter things, too

But I was not there during the polishing, and, therefore, while I know it is common to fill the porous ceramic materials, under some circumstances, with great care, you could avoid it. But I did not see that, and I do see the persistence of the odor, and I have no other explanation for it.

JUDGE BRENNER: Mr. Ellis.

MR. ELLIS: I would like to proceed now with Dr. Rau.

BY MR. ELLIS:

Q Dr. Rau, Dr. Anderson also said, I think, that the most important thing was decarburization. Would you comment on that, please?

A (Witness Rau) Mr. Ellis, I don't want to ask the questions, but I really don't understand exactly whether Dr. Anderson is referring to decarburization -- that is, loss of graphite -- or decarburization of the perlitic matrix, and I would like to ask for a clarification before I answer.

MR. ELLIS: Judge Brenner, it might be expecting too much of me to repeat that question.

JUDGE BRENNER: Dr. Anderson, could you enlighten us?

WITNESS ANDERSON: Well, I would say it would be

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the loss of any carbonaceous material. The carbon is in the structure in any form due to the oxygen potential on the surface and the elevated temperature.

BY MR. ELLIS:

Q Dr. Rau, does that respond to your question?

A (Witness Rau) Not completely.

I guess you're saying "both."

A (Witness Anderson) Yes.

A (Witness Rau) Well, I would disagree that it is most important. Surely, at elevated temperatures -- Dr. Wachob would like to comment, too -- you would expect oxidation of the graphite flake as well as oxidation of the perlite which consists of iron ferrite and cementite, which is a carbide.

The physical observations are that there is oxidation of both those components, both the graphite flake oxidized and the perlite in between the graphite flakes oxidized. The oxide that's developed is relatively uniform along the entirety of the casting shrinkage crack.

So I don't understand the relative import of it. It's a statement of what can happen, and, in fact, does happen.

But the oxidation at elevated temperatures -- and by "elevated" I mean, you know, during the cool-down at temperatures between, say, 1000 or 1100 degress F. and room

WRBwrb

temperature -- will produce oxidation of both those components, and did, in fact, produce oxidation of both of them.

Q Dr. Wachob, did you want to add anything?

A (Witness Wachob) We did spend some time evaluating the literature on decarburization of cast irons, and were unable to find a definitive reference that even addressed that issue.

There are instances in the literature which strongly suggest that carburization -- I mean oxidation of the graphite may occur much more so than would the oxidation of the carbon from the metal carbide, perlite, that we are talking about.

The temperature at which we are anticipating shrinkage cracks to occur is in the range, too, where you really don't lose much carbon due to either one of them.

So, with that, and with Dr. Rau's statements, I think that that's consistent with what we observed.

Q Dr. Bush, you might wish to comment on that. Would you begin with: am I correct it is still your opinion that the evidence is sufficient to warrant a conclusion that the coating, or the layer is an oxide?

A (Witness Bush) Yes. I would say the evidence would confirm that.

I would expect some degree of oxidation there;

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however, this tends to be predominantly a surface effect. And in my estimation, looking at the photo micrographs, we can neither confirm nor deny that perhaps there has been some oxidation of the graphite. I don't think, however, it is necessarily the overwhelming, or controlling mechanism.

In other words, I can perceive a loss of some graphite as the formation of an oxide is occurring, but concurrently say that doesn't affect my opinion one iota whether there is some oxidation of the graphite.

Q Dr. Rau, Dr. Anderson indicated that while he concurred that there were no beach marks, he thought they may or may not be present, and that that was not important.

Do you have a comment on that?

WRBeb

A (Witness Rau) Yes. As I stated I think previously, the absence of any indications of gradual progression during operation by any of the cam gallery cracks which we observed is completely consistent with it being a fabrication-induced crack.

And although it is true it is more difficult to see mechanically-induced beach marks in cast iron than it is in some metals, they can in fact be seen and I think in fact would in fact be seen by a combination of both surface coloration, oxide thickness and mechanical means if in fact there had been any extension of the casting defects during operation.

So in my opinion, the absence of any such beach marks is very significant with regard to the conclusion we have reached about the source and lack of motion of those casting-induced cracks during service.

Q Did you want to respond to that, Dr. Anderson?

A (Witness Anderson) Well, the "oxide layer" certainly could serve to mask the surface from beach marks, but it is just not necessary for cast irons to have beach marks that are discernible. I think that's my comment.

JUDGE BRENNER: Can I back up with Dr. Rau?

Dr. Rau, in discussing the absence of beach marks, are you limiting that observation by you only to what you call the casting shrinkage cracks, or do you also

WRBeb

include the weld shrinkage cracks, what you call the weld shrinkage cracks?

WITNESS RAU: I didn't mean to limit it, Judge Brenner, although because of the thickness of the oxide on the casting shrinkage crack, I think there is a higher probability of seeing some change in that thickness of oxide if in fact that crack had been progressing during operation than on the weld shrinkage cracks where there is negligible or very, very thin oxide.

So in that region you would be depending upon, almost exclusively, mechanical changes in the surface to produce a mark that is a beach mark which would demark or delineate the crack front during progression.

So I think it would be easier in the original casting but I didn't mean to exclude the fact that you could see it also in the weld shrinkage cracks if in fact there were any.

BY MR. ELLIS:

Q Dr. Anderson, you indicated that--

JUDGE BRENNER: Are you leaving the subject of beach marks?

MR. ELLIS: Yes, sir.

JUDGE BRENNER: Perhaps we should ask Dr. Bush.

MR. ELLIS: I'm sorry.

BY MR. ELLIS:

WRBeb

Q Dr. Bush?

A I have already expressed my opinion, but I would expect to see it. Obviously it is more difficult. One might have to resort to electron microscopy in some instances.

We should have a major exhibit, not a part of the official record, that would indicate it because Failure Analysis conducted either a four-point or three-point bend or compact tension to develop the DA/DN curves. These would be a pretty clear indication, and if one examined those crack surfaces, I think one could indicate.

Now admittedly they are not precisely the same but it would certainly resolve the fact as to whether you can or cannot develop beach marks. I personally feel you would develop them. This is a question in Dr. Wachob's regime, however.

Q Would you want to respond to that, Dr. Wachob? Comment on it? Or Dr. Rau?

A (Witness Rau) Well, we would agree with Dr. Bush with one clarification:

The fatigue crack propagation experiments which he ran on the original 103 material were not run under a range of different high-cycle and low-cycle fatigue intermixed, and if you like, were running constant cyclic load conditions. So you wouldn't expect to see as easily

WRBeb

or as pronounced beach marks on that kind of a test as you would in one which is, if you like, oxidizing and undergoing a range of different types of loading, different duty during service.

But otherwise I would concur.

Q Dr. Bush, am I correct that you agree with Dr. Rau that the absence of beach marks does support the FaAA view on the causes of cracks?

A (Witness Bush) I would tend to say so. You recognize, however, that the photomicrographs as such do not unequivocally establish the presence of absence of beach marks. I am more inclined to consider factors such as the thickness of the oxide film and the continuity of the oxide film under these circumstances, more so than I am the presence of absence of beach marks on the particular crack we examined.

MR. ELLIS: I am going to leave beach marks, if you want to pursue it.

JUDGE BRENNER: Dr. Rau, could you briefly tell me again what you looked at as evidence for the fact that you have established the negative, that is, the absence of beach marks? Whenever anybody is establishing a negative, the argument always is well, maybe you didn't look hard enough.

WITNESS RAU: I can assure you, Judge Brenner, we

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looked very hard.

The examinations which were made consisted of breaking open the cam gallery crack, examining visually with your eyeball to start with, and then proceeding up in increasing magnifications using a stereo-binocular microscope and a wide range of lighting conditions from direct to indirect in an attempt to reveal slight changes in elevation of the surface, slight differences in the appearance of the oxide, slight differences in coloration, all the factors which, under normal conditions, lead to a beach mark appearance.

None of those observations on up through the high-power fractography provided any indication whatsoever of any beach mark. And all I can suggest is that we looked very hard and there just was no such evidence, as contrasted to observations on other surfaces which we believed to be fatigue-induced like ligament cracks, for example, where in fact you can see evidence of marks or different positions the crack was located at different periods of time, indicating progression of the crack during operation.

JUDGE BRENNER: Dr. Bush mentioned electron microscopy, which I have difficulty pronouncing; something like that. Dr. Bush mentioned looking at it through an electron microscope. Did you do that?

WITNESS WACHOB: Scanning electron microscopy

WRBeb

was performed on the specimens to look at the fractographic features of the cam gallery, and no evidence was found.

WITNESS RAU: Is it clear, Judge Brenner, what that is?

JUDGE BRENNER: Do I know all the details of it? No. Do I need to know all the details of it?

WITNESS RAU: It is just a high-power microscope which works on electrons rather than light, but otherwise it is just looking at things with a higher microscope.

JUDGE BRENNER: You just summed up the full extent of my knowledge of the subject.

We have the luxury of being able to put together what Expert A says about something, so my basis would have been Dr. Bush's belief that that might show you something that a regular microscope examination might not, and that was sufficient for my purposes for now.

But thank you for the further explanation, Dr. Rau.

Mr. Ellis.

BY MR. ELLIS:

Q Dr. Anderson, in your listing of concerns or possible disagreements with Dr. Rau's explanation you indicated no test for carbon had been done. Am I correct? Is that one of the items you listed?

A (Witness Anderson) Yes. I have not seen any

WRBeb

test for carbon. It is generally not possible with the electron microscope or microprobe. It normally has to be done independently. I have not seen any information concerning carbon tests.

Q What is the significance of that, Dr. Anderson?

A Well, we have this dark black oxide on the surface of the crack. If indeed the oxide is not an oxide or only partially an oxide and there is carbon from the destruction of the iron matrix, then the presence of carbon would be significant.

And therefore, for completeness, for understanding, for substance in an opinion, it would be important to do a carbon analysis and characterize the surface with respect to carbon.

Q I'm not sure I understand why the presence of carbon would be significant. Could you elaborate?

A I thought I did. The carbon is a residue if you remove the iron matrix. Therefore, the presence of carbon would indicate that the iron matrix had been removed in some manner by some process such as graphitization or fretting or whatever.

Q I assume, Dr. Anderson, that you have done no -- that you have not performed any analysis to determine whether carbon is present.

A No, I don't physically possess the samples to do

WRBeb

that. Moreover, the presence of carbon would be contra-indicated in an oxide.

If it had been a high-temperature oxide formation, then the presence of carbon would not be expected mixed into the oxide layer, so that if you looked at this dark layer and you found carbon, that would certainly indicate that it was not a high-temperature oxide mechanism.

Q Dr. Rau or Dr. Wachob, would you comment on that, please, with respect to the carbon?

A (Witness Rau) Yes, Mr. Ellis.

In my opinion it was not necessary to examine for carbon. The metallography again, the examination of this oxide in cross-section in profile at high magnification clearly indicates that it is a continuous chemical compound, in my opinion and based upon the microprobe work, surely an oxide, but even if it had been some other compound, a sulfide, it's continuous. It surely is not a carbide. I don't think even Dr. Anderson is proposing that.

For that reason, it is really irrelevant whether or not there is some graphite in that oxide. There surely is graphite on the fracture surface, and if you did a scan, a chemical scan on the fracture surface, you would in fact see carbon because every time the graphite comes up to the surface you would see it.

WRBeb

So in our opinion it didn't seem to make any sense to go do an analysis which would detect the presence of something you already know is there. It is just irrelevant to the conclusions drawn.

Q Dr. Rau, Dr. Anderson mentioned in his previous answer that it might be a sign of graphitic corrosion. Do you agree with Dr. Anderson's opinion that the surface, the layer of surface material observed in the cam gallery cracks is attributable to graphitic corrosion?

A No, I definitely don't agree with that, for several reasons.

Graphitic corrosion is a name given to a very particular kind of corrosive attack of gray cast iron or cast iron and in particular, it is generally nothing more than an oxidation of cast iron, so there is nothing magical about graphitic corrosion.

However, when graphitic corrosion occurs or under the conditions it is normally named that, it is normally associated with many features which are not seen in the thick dark oxide which is on the cam gallery cracks. There is no pitting; there is no eating away of the perlite matrix leaving a network only of graphite flakes. And quite frankly, there is none of the reddish rust kind of oxide which is what you typically get when you get graphitic corrosion.

WRBeb

It is black, in part because of the graphite, but when you look at higher magnifications you see that it is primarily an Fe203, that is, the low-temperature, high-graded form of the oxide which looks rust colored. There is none of that kind of evidence.

This is a thick, dark, uniform oxide, completely inconsistent with those characteristics normally attributed to graphitic corrosion.

Dr. Wachob wants to add something.

A (Witness Wachob) In addition to that, we have two other pieces of evidence that would suggest that graphitic corrosion is not occurring. The first one is on the weld shrinkage crack where you had base metal exposed, the gray cast iron exposed to the same environment as what we are calling the shrinkage crack, one would anticipate seeing graphitic corrosion on that surface, and it is not there.

And it is extremely -- I would say impossible to have two locations that close in proximity and not have graphitic corrosion on one area as well as on the other, and demarcation is very strong as to where one oxide occurs and where the other oxide occurs.

Secondly, in other areas of the cam gallery such as the fuel pump bracket area or in the saddle of the bearing for the camshaft itself, there is no evidence of a

WRBeb

thick dark black oxide. Those regions were unpainted and therefore, should have had graphitic corrosion associated with them also if that process had occurred, and it is not observable.

Q Before giving Dr. Anderson a chance to respond to that, let me turn to Dr. Bush so that Dr. Anderson has all of it before him, because I think that you testified, Dr. Bush, that you too agreed that graphitic corrosion is not the cause of the layer of surface material observed in the cam gallery crack.

WRBbrb

In the weld area, would you state your basis for that as well?

A (Witness Bush) Well, my inferential process was very similar. In fact, I think I have a few written comments that I made in the last few days.

One of the obvious things is the fact that near the crack mouth -- that is, near the upper surface -- if one argues that the mechanism controls one, if anything, would expect it to be more extensive rather than less extensive; and I did raise the question, I believe, in the actual testimony about the fact that if this occurred, and if we were assuming the same environment, it should occur elsewhere, where we have unpainted surfaces.

I have to depend on hearsay evidence. I've asked questions of various people, and the answer I've received is they did not observe any such. And so that tends to, in my estimation, reduce the probability of selective graphitic corrosion in the crack areas vis-a-vis other areas.

Q Well, am I correct that it is your opinion that graphitic corrosion is not a cause of the layers?

A I do not believe this to be the cause. That's correct.

Q Dr. Rau, did you have something to add?

A (Witness Rau) I just had a point of clarification of what Dr. Wachob said. It wasn't clear to

WRBbrb

me that everybody would understand.

In addition to other regions of the cam gallery, I think -- I'm sure Dr. Wachob was talking about the weld shrinkage crack, which runs along the heat-affected zone of the repair weld. That crack, if graphitic corrosion were occurring, as I think Dr. Bush was implying also, would have to have been exposed to the same environment that the deeper portion -- that is, the original casting shrinkage crack -- was exposed to if graphitic corrosion were occurring.

And since it's the same gray cast iron, the absence of any thick dark oxide on that crack precludes the possibility of graphitic corrosion, because if the crack were not there at all then there would be no contact of the original casting shrinkage crack below the surface with the environment. And therefore you get no corrosion at all.

And if it were there, then it would have to have had the same thick dark oxide that the casting shrinkage crack below it had. And since neither one of those is met, it cannot have been graphitic corrosion vis-a-vis other areas.

Q Dr. Anderson, it's your turn now.

JUDGE BRENNER: Let's go to Dr. Bush for a moment, please, if we could.

Dr. Bush, what do you think of Dr. Anderson's suggestion that the layers should be tested for carbon?

WRBbrb

I'm asking you both as to the feasibility of doing that and -- in light of the presence of graphite, what would it show you?

WITNESS BUSH: I don't think it would show too much.

In the first place, I would anticipate -- assuming that there is oxide, and I think the microprobe evidence confirms this -- that if we have an oxidizing atmosphere it will either form carbon monoxide or carbon dioxide.

By like token, there's no reason to say that we can't, in the process of oxidation, shake loose or break loose particles of graphite, of which there are a very large number, and if these happen to be embedded in the oxide in the process of its formation, they really wouldn't tell you very much.

So I'm not exactly sure. I'm pretty sure -- in fact, I would be surprised, if a very careful analysis of it was done, if you didn't find some graphite, or some -- I shouldn't say graphite -- some carbon, as such.

JUDGE BRENNER: Would it be possible to test it in such a way as to distinguish artifacts of graphite -- if I'm using the right word -- from the possibility that there is carbon throughout the entire layer?

WITNESS BUSH: It should be. There are three

WRBbrb

things.

One, you could do a careful metallographic examination and, if indeed it's embedded, I think there's a possibility. Unfortunately, the color is the same and, so, one would have to be very selective. That's one possibility.

If one indeed believed that there were some complex compounds formed, and normally one wouldn't anticipate under this one, then you would probably have to resort to an x-ray diffraction to see if you had simple oxides or, by some mechanism that I can't, offhand, visualize, that you formed some complex carbides at the same time.

That would be, I guess, the court of last resort, because then you would say that, indeed, the carbon is becoming a physical portion or, more specifically, a chemical entity combined with some other materials. That would be the way that it could be done. Whether it should be done is another matter.

JUDGE BRENNER: Let me go back to Drs. Rau and Wachob.

On the point of testing for carbon, one of your points was that you would expect it to show carbon because of the presence of graphite in any event. Why don't you think you'd be able to distinguish that type of carbon

WRBbrb

presence from the presence of carbon as part of the layer itself, along the lines suggested by Dr. Bush?

WITNESS RAU: I didn't mean to imply you couldn't do so. But the normal method you would use -- to take a beam spot for example and interrogate it, you would interrogate a bunch of graphite flakes as well as the oxide. You could use certain microprobe techniques to delineate that if it were an important issue. I just -- as I said, I don't believe it was an important issue. That's why we didn't do it.

JUDGE BRENNER: Is it possible to still do it, given the condition of the samples?

WITNESS RAU: Yes.

JUDGE BRENNER: Is it difficult, in terms of time, money, or anything else you want to tell me about?

WITNESS RAU: Well, I don't know that it's terribly expensive to actually do the measurement. We're probably talking about a day or two on the electron microprobe. But the interpretation's not so easy.

And I should point out that, in addition to the carbon, which is present in the graphite, the perlite, which is in between the graphite flakes, also includes cementite, which also has carbon in it. So you're going to see -- I mean, it's going to light up like a Christmas tree when it comes to looking for carbon, and the interpretation's not

WRBbrb

going to be that easy. And I really don't understand what use would be made of it, independent of what the results showed. But yes, it could be done.

JUDGE BRENNER: I guess you wanted to return to Dr. Anderson.

MR. ELLIS: Yes, but may I just finish this one point?

BY MR. ELLIS:

Q I'm correct, am I not, Dr. Bush, that you don't recommend that this carbon test be done, do you?

A (Witness Bush) I do not recommend it.

Q Thank you.

Dr. Anderson, it was your turn a few moments ago.

A (Dr. Anderson) There are two questions: the carbon question and the crack question; and if I could address both those?

Q Yes. Go right ahead.

A Okay. First the carbon.

As I say, at high temperatures, if you're forming an oxide at high temperatures, the thermodynamics are such that the oxide should be free of carbon. And therefore, a simple Sims or Auje analysis of the surface would certainly tell you the presence of carbon. And then that would remove from all doubt -- and certainly from my questions -- whether

WRBbrb

that was a high temperature formation on the final surface or whether it was not.

Also, I believe that that oxide should be removed in some location so you could look at the base metal and, perhaps, further determine whether there are beach marks present there or not.

Now, the crack environment: there's a gross misunderstanding of corrosion here. The area of the crack has its own environment, and therefore to say the surface should corrode more and therefore we should look at other areas of the block and see their oxide layers is not what we're talking about.

We're talking about a crack-driven mechanism. And I think the best analogy I can, without giving you my fifty minute lecture on corrosion, is to remind you that an oxygen-deficient area is the area that goes into solution -- essentially, corrodes.

And a crack, of course, the bottom of the crack is the area of greatest oxygen deficiency. And the way to prove that is to put a rubber band around the pipe and let it corrode, and it will cut in half; it will corrode under the rubber band where the oxygen can't get to it, and you'll end up with two pipes.

The crack environment is different than the rest of the environment of the engine; and, therefore, to look at

WRBbrb

the block in the outside world and to say, "Well, we don't see any signs of oxidation there," or so forth, is just not true.

JUDGE BRENNER: Well, what about oxidation in the other cracks, though, Dr. Anderson, such as ligament cracks?

WITNESS ANDERSON: I would imagine that they have different etiologies, different formation, and different characteristics, and probably see different atmosphere -- whether it is as oily or less oily I don't know. But I don't think I could compare cracks in entirely different areas that are formed for different reasons.

JUDGE BRENNER: Have you completed your answer, Dr. Anderson?

WITNESS ANDERSON: I think so. I forgot what more I was going to say, if I'm not.

WITNESS RAU: Judge Brenner, may I add something to that point; or, if you'd rather go on --

JUDGE BRENNER: All right, go ahead, and then I'll go back to Dr. Anderson.

WITNESS RAU: I'd just like to point out that we have not a different crack; we have the weld shrinkage crack in the gray cast iron immediately adjacent to -- and, in fact, connected to -- the casting shrinkage crack. That particular crack is also a crack and would also experience

WRBbrb

comparable crack tip chemical environments as did the shrinkage crack; and, in fact, there is no thick dark oxide on that crack, which is immediately adjacent to the casting shrinkage crack. So although what Dr. Anderson said may be true, it's really irrelevant.

JUDGE BRENNER: I guess I'll give you an opportunity to respond to that, Dr. Anderson.

WITNESS ANDERSON: Thank you.

Since we're looking at oxygen potential, obviously the top of the crack or the early portions of the crack have different oxygen potential than deeper in the crack. And so it's a well-known phenomenon that, once a pit or a crack starts, the corrosion is most serious the furthest away from the surface.

Moreover, we have a bit of a problem. We have a discontinuity of materials up where the weldment is. The weld metal is a different material, and therefore you can't really comment on the dissimilar metal aspect up there that is occurring with what's occurring down at the bottom of the crack where the oxygen potential is the lowest.

JUDGE BRENNER: But even if there might be a difference in degree, as suggested by you, wouldn't there be evidence of graphitic corrosion in the portion of the heat-affected zone by the weld cracks, if your theory that graphitic corrosion occurred is correct?

WRBbrb

WITNESS ANDERSON: Yes. When I looked at the -- that's true, and when I looked at the specimen it appeared to me that there was a continuation of this black material, going up to the actual surface and around where it had separated from the weldment. I do believe that it was less abundant.

JUDGE BRENNER: How did you look at it?

WITNESS ANDERSON: Through the Failure Analysis binocular microscope.

WITNESS BUSH: May I comment on that, Judge Brenner?

I considered this aspect. There are a couple of questions I can't answer, but at least I can respond to some.

In the first place -- and this is reported in the testimony -- though I have not seen the metallography, it is my understanding that a martensitic layer was reported in the heat-affected zone. That immediately tells me that the temperatures at that particular region were probably in the vicinity of a thousand degrees Centigrade, or very close thereto.

That is adequate, even in a short time, to form a thin film, almost -- possibly an oxide film; this would depend on the type of welding process used. Now, whether that film would remain in place when the metal comes against

WRBbrb

it or whether you'd get a wash I can't tell. That's very much a function of the adherence of the film, et cetera. But, certainly, by a mechanism such as this one could understand how there could be a film up in this particular region in there, because you certainly have high enough temperatures.

One other aspect of it that is relevant is that, using a very simple model, and knowing that martensite is untempered -- which, again, I would have to take as hearsay since I haven't seen the metallography -- that would immediately tell me that they did no pre-heat.

And if I do a back-of-the-envelope calculation, I'm talking of strains in the vicinity of one-hundredth of an inch. A hundredth of an inch doesn't sound like very much, but as you convert it to stress using the modulus, you come up with a totally ridiculous number of about 250,000 pounds per square inch, which obviously never occurred. It does point out the fact that one could have extremely high residual stresses in this region and it would not be too surprising if it cracked. In fact, my gut feeling would be by the time they got around to painting the thing it was already cracked.

WRBeb

BY MR. ELLIS:

Q So that the record is clear, Dr. Bush, you were referring to the weld shrinkage crack. Is that correct?

A (Witness Bush) I'm talking of the weld shrinkage crack, and I'm talking of the hot region in there because as soon as you try to lay down the bead you get into this situation.

JUDGE BRENNER: I want to back up to you, Dr. Anderson, on the test for carbon.

Why don't you think the interpretation would be very difficult in trying to separate the possible presence of carbon as part of that layer itself from the presence of carbon elsewhere in that region, such as by graphite flakes in the layer, carbon in the cementite, et cetera?

WITNESS ANDERSON: Well, I think if you look further down from the surface you would find some differences in the type of carbon formed, but I'm not proposing that. I'm saying that if you essentially examine the immediate surface, the immediate unaltered surface, and that there is the presence of carbon in any form whatsoever, then the presence of that carbon could -- counterindicates a high-temperature mechanism to form the oxide.

Down at the base I think it would be more difficult to discriminate what it was doing. But at the final top surface, the presence of carbon would indicate

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that it wasn't a high-temperature oxide formation.

WITNESS WACHOB: Judge Brenner, may I add one point to that?

In many of the studies that I've been involved with with Eska and Auje, you will get carbon on the surface, independent of what the material is, just from it being out in the air and then you have this potential contamination for subsequent analyses below. So that itself is going to confuse the issue, let alone the other issues that we are trying to directly address.

WITNESS ANDERSON: That's a good point. He is absolutely correct that contamination can cause a problem and therefore, you probably would have to do a surface and then profile it down some depth, just to make sure that you have blanked out your surface.

WITNESS BUSH: If I may make one comment in that respect, I think there is an issue that has been inferred but maybe it hasn't come out explicitly.

It is quite possible and in fact, it is very probable that if one were to examine this oxide from the outer surface down to the metal surface that you will have a whole series of oxide forms in there that have formed because what you are depending on is diffusion through what is not an impenetrable film.

It has to go to the metal in there, and by like

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token, there can be diffusion back in some instances if you have volatile species.

So what we're really talking about is not a single layer that is homogeneous, we're talking of a whole series of compositions. And if we were magically able to reach in and analyze at every few microns, we probably would see a distinct gradient of compositional and crystallographic gradients across that.

JUDGE BRENNER: Dr. Bush, if some of the graphite oxidized, would it always have to have disappeared as a gas, or could there be some compounds of carbon present as a result of oxidation of the graphite -- I mean compounds still present in the layer?

WITNESS BUSH: I suppose there are conditions where one can form oxi carbides. I must confess this gets beyond my knowledge. I know they can be formed. I usually relate them to very specialized conditions for formation rather than conditions that are approaching, say, atmospheric pressures and so forth.

I attribute these usually to pressures and temperatures that are outside of the bounds we are talking about. I cannot say they won't form. That's an area I haven't looked at very much. I would kind of think that the probability wouldn't be that high.

Maybe Dr. Wachob is closer to that area than I

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am.

JUDGE BRENNER: We will let Dr. Wachob answer almost immediately, but let me state on reason I asked in case you want to add a comment, Dr. Bush, and then we will go to Dr. Wachob.

You mentioned CO and CO2 earlier.

WITNESS BUSH: That's correct.

JUDGE BRENNER: And my rudimentary chemistry is almost so rudimentary it is practically non-existent. But I was wondering if some carbonates could have formed?

WITNESS ANDERSON: No. At high temperatures only CO is stable. As you come down in temperatures, then CO2 becomes stable. And you have to get very low before you get a carbonate.

WITNESS BUSH: I would expect these in fact, the volatile species, if they formed to tend to back diffuse out through the layer and disappear rather than-- As I say, there could be special conditions where they would form, but I have difficulty visualizing what they would be as such.

JUDGE BRENNER: Dr. Wachob, you wanted to comment?

WITNESS WACHOB: I wanted to comment on the aspect that Dr. Bush had brought up with respect to variations in the oxide layer itself.

In several studies I have been involved with

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recently and in reviewing the literature, in this instance one would anticipate seeing a variety of oxides and in this oxide film that we're talking about, it is a very difficult problem to interpret.

The technique of using an x-ray beam may be very simple. However, the final interpretation of what the oxide is made up of is a problem.

Secondly, if the oxide begins to form at one temperature and as it cools down, you will automatically get the other oxides as you cool down. So the whole problem of trying to do a quantitative, very detailed analysis of this oxide to give you a definitive answer cannot be done.

WITNESS ANDERSON: As I sit here thinking and listening to my colleagues, I think maybe it is going to be difficult -- I think it is impossible for Failure Analysis to do it at this time.

Remember my earlier comment about I could smell the solvent? It's a hydrocarbon solvent, and it has gone into the matrix. And therefore, I think after doing that they would find carbon, and then I would say Aha, you found carbon, therefore it can't be high temperature. And then we have another-- It could have come from that source, too.

I think the specimen, that particular specimen, has been ruined.

I would be curious, though, to know-- It is my

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understanding the specimen has been examined in the electron microscope. The electron microscope has a tight vacuum in it. It's required.

I would be curious to know from Failure Analysis whether there were any problems pulling the vacuum on the specimen, which would mean that there was a considerable amount of solvent that had to outgas, and whether there was any discernible odor after they took it out from this high-vacuum environment.

JUDGE BRENNER: I'll tell you what. We are at the point of the morning break, in any event. This would be an opportunity, (a), for the FaAA witnesses to think about what you just said. Beyond that, you and Counsel for both parties can ask them during the break, and by obtaining information like that, you can decide, either through LILCO's Counsel or your own Counsel, how to probe further any points along those lines that you want to probe.

I don't have a direct connection in my own mind immediately, but I'm sure you've got one, and we'll continue to focus on it so we won't forget it. And you will get an opportunity, if everyone thinks it's important after you have all discussed it, to have that asked. That is one benefit of this procedure.

Let's take a break until eleven o'clock.

(Recess.)

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JUDGE BRENNER: Back on the record.

Now, one reason we're pursuing everything we can as it comes up with all the witnesses is the hope that we can cut down very dramatically on any necessary follow-up. I don't know if those hopes will be realized or not; but we'll see. It's our desire, as we go through it, as we stated and as I think we are effectuating, to give each witness his say.

Mr. Ellis, why don't you proceed?

MR. ELLIS: Judge Brenner, what I would like to do now is: with respect to Exhibit B-61, I want, now, for the benefit of the parties and the Board, to have the witnesses relate the appropriate photomicrographs to that exhibit.

BY MR. ELLIS:

Q Dr. Rau, do you have before you photomicrographs relating to Exhibit B-61?

(Pause.)

JUDGE BRENNER: Mr. Ellis, I'll let you do that if you want to. There was one more point mentioned by Dr. Anderson that I thought we would go back to the other witnesses on, and that was Dr. Anderson's view that the FaAA's report -- that they found oxygen present through that probe -- is in fact not probative because it's not clear just what portions were, in fact, sampled.

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MR. ELLIS: All right, sir. Before I get to the photographs, let us deal with that, then, right away.

BY MR. ELLIS:

Q Dr. Rau, I believe one of the comments made by Dr. Anderson was that the results of the probe were doubtful, in his mind, because of hydrous oxide that was found, I think he mentioned.

Do I have that correct, Dr. Anderson?

A (Dr. Anderson) I'm sorry. The results of --

Q Of the microprobe.

A (Dr. Anderson) Yes, yes. I'll clarify it, if I may.

There are artifact hydrous oxides that have formed since the time that it was cut out of the block, and they have nothing to do with the etiology of the crack. I have seen the probe analysis, but when asked on where it was taken on the surface, the information I got was "nobody knows," and therefore my worries are that the operator may have stumbled onto the newly formed hydrous oxides.

Q All right.

Dr. Rau, Dr. Wachob, can you respond to that and give the reasons why you believe the microprobe results are reliable?

A (Witness Rau) Let me simply indicate that my instructions to Dr. Wachob were to do sufficient sampling to

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reach a definitive conclusion with regard to whether or not there was an oxide there.

Dr. Anderson is completely correct: there were isolated areas of rust. The hydrous oxide he's talking about is rust. And the engineer who accompanied the samples and sat there during the examination was instructed to sample various areas to assure himself that, in fact, he was adequately sampling the oxide and not the rust spots. And, in fact, my understanding is there were multiple regions sampled and all of which produced virtually identical results.

I'll let Dr. Wachob comment about the details.

A (Witness Wachob) As Dr. Rau said, there were many regions on this fracture surface where the -- what we've defined as the shrinkage crack was examined with a microprobe. In all instances, the results were basically the same. Therefore, three areas were chosen and the results were presented to each of the parties. They are representative of the oxide environment, or oxide layer, that is present on that surface.

And, in that we're looking at a large number of them, possibly one of them will provide this ferrous oxide hydration that Dr. Anderson has talked about. But we have sampled many areas, and they were all consistently the same as the results provided to Dr. Anderson.

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Q Dr. Anderson, did you want to respond?

A (Dr. Anderson) I think my response is: thank you for explaining it.

MR. ELLIS: May we now go on to the photographs of --

JUDGE MORRIS: Mr. Ellis --

MR. ELLIS: Oh. I'm sorry.

JUDGE MORRIS: I always have trouble with qualitative adjectives.

What does "many" mean?

WITNESS WACHOB: The results were provided for three specific areas. And my understanding, discussing with the engineer that went over it, is that there were six or seven areas that were examined.

JUDGE MORRIS: Thank you.

BY MR. ELLIS:

Q Dr. Rau, I had asked you whether you had selected and had before you photomicrographs that corresponded to Exhibit B-61. Do you have those?

A (Witness Rau) Yes, Mr. Ellis. I do.

Q Can you identify those by the "DW" number?

MR. ELLIS: And then, Judge Brenner, what I'd like to do is to introduce those, and I'll provide copies for the Board and the parties. But I want to use those from the album.

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JUDGE BRENNER: All right. Do you have copies now?

MR. ELLIS: Yes, sir.

JUDGE BRENNER: Why don't you hand them out now, even though we're not yet identifying them, so we'll have them in front of us while he's talking.

MR. ELLIS: Very well.

(Counsel distributing documents.)

WITNESS RAU: Judge Brenner, just for clarity: the ones that Mr. Ellis is handing out are those which are not already in the record. There are already two metallographic micrographs which have been previously. I think, entered into evidence as County Exhibit S-4.

JUDGE BRENNER: We have them, yes.

MR. ELLIS: Judge Brenner, I have handed to the Board and the parties a single sheet which has two photographs on it, together with a handwritten caption, which I would like to have marked.

The photographs are entitled, at the top, "Tip of Casting Shrinkage Crack, Magnification 100X, DW No. 19484," and the bottom drawing is captioned "Magnification 500X, DW-29484;" and I'll furnish original photographs to the Board and to the Reporter for this purpose. I'd like to have this marked B-63.

JUDGE BRENNER: All right.

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(Whereupon, the photographs captioned "Tip of Casting Shrinkage Crack, Magnification 100X and 500X" were marked as Exhibit B-63, for identification.)

JUDGE BRENNER: When are you going to provide the originals, Mr. Ellis? Not today, I take it.

MR. ELLIS: No, sir. I can't do it today.

JUDGE BRENNER: All right.

Off the record.

(Discussion off the record.)

JUDGE BRENNER: Back on the record.

Mr. Ellis?

BY MR. ELLIS:

Q Dr. Rau, would you describe what is depicted in the photographs that make up LILCO Exhibit B-63 and how they relate to LILCO Exhibit B-61?

A (Witness Rau) Yes, Mr. Ellis, I will.

There are two photomicrographs; these are basically high-magnification pictures of the tip of what's been designated the casting shrinkage crack on LILCO Exhibit B-61. In other words, there's two microscope pictures, one a hundred times magnification, at the top, and one at five hundred times magnification, at the bottom. And these are taken, if you like, as a blow-up, right at the far left-most

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tip of the casting shrinkage crack. In actuality, you have to turn them both upside down, so that the tip points towards the left, to coincide completely with LILCO B-61. These two photomicrographs illustrate the thick dark oxide present on the casting shrinkage crack.

I also mentioned that there are two other exhibits, I believe, already in evidence as County S-4. These are analogous metallographic samples, also taken from the same cross-section, and in fact are representative of the location of the weld shrinkage crack in the heat-affected zone, which is almost identically where the arrow points at the bottom-most label -- that is, at the bottom of the three sketches on LILCO B-61 is a label, "Weld Shrinkage Crack in HAZ (very thin oxide)". The tip of that arrow points midway through the curved crack which runs in the heat-affected zone between the gray cast iron and the repair weld.

The micrograph at the top shows a 50-times magnification of that location -- I'm sorry; in S-4 they're actually set up left and right, so on the left-most photomicrograph in S-4 you have a 50-times magnification of that location. And then on the right-most photomicrograph you have 100-times magnification of the weld shrinkage crack in the heat-affected zone of the gray iron.

The comparison of S-4 with LILCO's B-63 indicates

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the complete absence of any substantial oxide on the weld shrinkage crack in the heat-affected zone adjacent to the repair weld, as contrasted to the thick dark oxide on the casting shrinkage crack, which is illustrated by the photomicrographs of B-63.

JUDGE BRENNER: Could somebody lend us the photographs from B-63 without depriving yourself of any needs this morning?

MR. ELLIS: May I approach the witnesses?

(Document handed to the Board.)

BY MR. ELLIS:

Q Dr. Rau, do you have B-63 in front of you, still, sir?

A (Witness Rau) Yes, I do.

Q Can you describe, looking at B-63 and at S-4, once more, please, the location of the oxide layer in B-63 and the absence of it in S-4?

A Yes, sir.

In S-4, the right-most -- that is, the 100-times magnification photomicrograph -- shows the crack progressing from lower left to upper right. The whitish area with the dots in the upper left hand corner is, in fact, the iron-nickel weld material which doesn't etch as easily as the cast iron.

Towards the lower right is the cast iron. And,

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in fact, if you take LILCO Exhibit B-61 and turn it 90 degrees counterclockwise so that the cracks are pointing downward you'll have it in the same orientation as County Exhibit S-4.

So, what we're looking at is the crack running along the heat-affected zone. If you look at the dark line running from lower left to upper right, that is in fact the weld shrinkage crack; and you notice that there is no oxide discernible on either side of that dark shrinkage crack.

As I mentioned previously, that particular photomicrograph is almost exactly located at the midpoint of the weld shrinkage crack, or almost exactly where the arrow from the label "Weld Shrinkage Crack in HAZ (very thin oxide)" on LILCO B-61 is located.

By contrast, if you look at Exhibit -- that's LILCO Exhibit B-63, we have a comparable 100-times magnification photomicrograph illustrating the tip, or the deepest portion, of the casting shrinkage crack in the cam gallery region; this is from cam saddle number seven of the original EDG-103 block. On this particular photomicrograph you can clearly see the presence of the thick oxide on both sides of the crack and along the entire extent of that crack.

It's perhaps easier to see at the higher magnification -- the bottom of the two photomicrographs on

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B-63 -- that in fact there is a thick oxide all along what was previously a graphite flake.

And then, in the region of perlite between the graphite flake -- in other words, if you move towards the left hand sid, perhaps one third of the way from the left hand side of the photograph toward the right, you find the end of the graphite flake, which is oxidized. You then find a region of perlite, which is also oxidized. And then, at the very far left, you start to see another -- what used to be a graphite flake, which is also got oxide on the matrix adjacent to it.

In any case, there's a thick dark oxide of uniform characteristics on this crack, which is completely absent from the crack illustrated -- on the same cam gallery -- by County Exhibit S-4.

Q Dr. Bush, you agree with those observations, don't you?

A (Witness Bush) Yes, by and large. One could always argue that you have lost some oxide, but I would think, considering the adherent nature of it and the fact that it's retained quite well down the crack, I would agree that the oxide layer near the weld is much less evident and thinner than it is deeper.

JUDGE BRENNER: I didn't understand, Dr. Bush, "One could always argue that you have lost some oxide

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somewhere."

WITNESS BUSH: Anytime you are trying to polish specimens like this, the polishing technique is very critical. However, since I retained it deep in, where would tend to think so, I would pull it out. I can infer that what I see is truly a structure, as contrasted to one that had been degraded by removal of oxide by the polishing. It is only a caveat, nothing more.

WITNESS RAU: Judge Brenner, can I just add one thing I forgot to mention? I may have implied it, but the two exhibits, B-63 and S-4, are both on the same plane of polish, done on the same day. They were prepared in the same metallurgical mount so, although Dr. Bush is correct, you can see differences, this is exactly the same plane of polish, all polished at the same time by the same guy in the same mount.

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JUDGE BRENNER: Do you have a 500 power shot of what you call the weld shrinkage area comparable to the pictures in S-4?

WITNESS RAU: Unfortunately, Judge Brenner, I do not, at least not that particular location.

BY MR. ELLIS:

Q Dr. Anderson, I wanted to give you an opportunity to comment on the observations, if you have one.

A (Witness Anderson) Thank you. I do.

My understanding is that the probe analysis was done of the open crack and flat surface and there was no probe done of the side. This is a very narrow crack that I see magnified at 500 X and therefore, to say it's an oxide, I believe it is based upon another analysis of the flat surface and not going across this. And therefore, in this particular plane, I cannot tell if it is a polishing artifact or not.

And my other comment is of the weld area, I would really need to see that at 500 X before I could make a comment that there was the absence or the presence of this layer.

Q Dr. Rau, Dr. Anderson says that the layer may be a polishing artifact. Do you agree?

A (Witness Rau) No.

Let me simply indicate that I have examined the

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regions in S-4 at higher magnification. I'm sorry I don't have a photograph illustrating that.

But in point of fact there is nc significant oxide, no thick oxide anything like the oxide which is present and illustrated by County Exhibit B-63 --

Q You mean LILCO B-63?

A I'm sorry, LILCO B-63.

Dr. Anderson is correct that the microprobe analysis we're referring to was done on another sample from the same cam gallery crack which was broken open and then examined on the fracture surface with a microprobe. But at worst they would be identical to this region but just-- In fact, it may even be this region after it was broken later; I can't recall.

Dr. Wachob informs me that it was the adjacent quarter-inch slice which was broken open and examined, so we're basically about an eighth of an inch to three-sixteenths of an inch away from where this particular cross-section was taken on the cam gallery saddle crack, saddle Number 7, the original 103, where the microprobe work was done.

Q Dr. Anderson, does that help you at all in connection with your observation that the layer could be a polishing artifact?

A (Witness Anderson) I can't say that it is or is

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not from that response. I think what I would feel more comfortable is if we had a collection of 500 X photomicrographs which we could compare in the upper region, middle region and lower region. I think that would be very useful.

Then if this was an artifact, I think we would get information at the 500 X level which would disclose that. But standing by itself, I just cannot comment.

JUDGE BRENNER: Drs. Rau and Wachob, do you have a 500 X photograph of that so-called upper region, what you call the weld shrinkage region, of a different area but of the same region? In other words an area different than that depicted in S-4?

WITNESS RAU: Judge Brenner, if we do I don't have it here.

BY MR. ELLIS:

Q Dr. Bush, am I correct that you do not consider the layer as a polishing artifact?

A (Witness Bush) I don't consider it an artifact.

I would tend to agree with Dr. Anderson, it would be nice to make a comparison at the higher magnification. There are three 500 X in here that represent progressive positions as you move what I would call down the crack, in other words towards the crack tip.

Unfortunately, the one that is missing is the one

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we are currently discussing.

JUDGE BRENNER: In other words, you have three that are below the weld portion?

WITNESS BUSH: (Nodding affirmatively.)

JUDGE BRENNER: Is that Yes?

WITNESS BUSH: I'm sorry, I have to remember this. The answer is Yes.

JUDGE BRENNER: Once in a while I see transcripts where it says "Witness nodding," but it doesn't say which way.

BY MR. ELLIS:

Q But while your testimony is, Dr. Bush, that it would be nice to have those, am I correct that it is still your opinion, based on what we do have, that it is an oxide layer?

A (Witness Bush) That's correct. The evidence I have gathered leads me to that conclusion. It is simply that more information would presumably further validate that assumption.

Q Dr. Anderson, I believe just before the break, Dr. Rau and Dr. Bush testified that you would expect a variety of oxides to form over temperatures in the cam gallery crack.

Do you agree that there would be a variety of oxides formed?

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A (Witness Anderson) I think that that is one possibility that should be examined for. If it was high temperature we would essentially -- in cooling down, we would essentially have a range of oxides that could occur. If you look at the iron-carbon-oxygen ternary diagram you could essentially draw a line from the pure iron to whatever final atmosphere we arrive at. And every phase on that line would be present.

I don't see that that's a problem, and I think that you could get a definitive answer without any question by doing an x-ray analysis of the surface to determine the presence of oxides and their relative abundance if that was necessary. But just the presence I think would pretty well put everything to rest.

Q Dr. Rau, would you comment on that, including whether you would expect to find a variety of oxides in the layer that is wustites and hematites and magnatites, and whether determining whether that is the case is significant to the conclusions that FaAA has drawn?

A (Witness Rau) Well, let me indicate that first of all I don't agree that it is a very easy task. It can be done. It is a very easy task to shine the x-ray beam onto the fracture surface and get back some signals.

From having attempted this on numerous previous occasions, there are multiple types of oxides. Three have

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been mentioned already. That is the high-temperature wustite, the intermediate-temperature magnatite, and the lower-temperature hematite, plus there are various hydrated forms of these oxides, all of which have different crystallographic structures when you look at them with x-ray.

Consequently if a crack forms at high temperature and then oxidizes over a range of temperatures from the high temperature down to room temperature, you will in fact generate a whole range, as Dr. Anderson has indicated, of different oxides. Depending upon the conditions which you use with your x-ray beam to interrogate those oxides, you may penetrate to various depths and sample different volume fractions of different oxides. And it will be very difficult to sort out what the relative fractions were at various locations in the oxide.

In any case, I believe there are multiple forms of oxide there. I believe that the thickness and coloration of the oxide are conclusive with regard to the temperatures at which they formed in the sense that they are in fact formed at moderately elevated temperatures and formed over the cooling from these temperatures down to room temperature.

I do not believe that an x-ray analysis which would detect the presence of the magnatite and may or may

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not detect the presence of the lower temperature oxides -- the hematites I think it would -- would be particularly meaningful. I wouldn't know what to do with it after in fact it had been measured.

Or the presence or absence of some of the wustite also I don't think would be meaningful because again the hot tear could have started above the temperature where the wustite forms, progressed down through the temperature range where the magnatite forms, continuing on to oxidize in the range where the hematite forms. And the ability to detect the presence of those three oxides would not be conclusive with regard to the conditions under which they formed.

I believe that their thicknesses are in fact conclusive in respect that it had to be an elevated temperature for the reasons we have already indicated.

Q Dr. Bush, would you concur with Dr. Rau that you would expect to find various oxides and that it is not necessary to test for those, and his remarks concerning the thickness of the layer as being the important feature?

A (Witness Bush) I expect there to be a variety of oxides, as I indicated. Whether one should test or not is another matter.

It would appear to me if you had exclusively a low-temperature oxide, one that formed essentially at room temperature, that an x-ray defraction pattern would not

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have the confusion of many types of oxides. Now whether one needs to do that or not, I don't know. I personally don't think that's the case.

Among other things, again based on reading the testimony and not on direct evidence in the context of observing testing, the level of adherence of the oxide, et cetera, to me tends to be more indicative of something that started to form at high temperatures and went down, as contrasted to exclusively a room-temperature oxide.

That's an inference again, nothing more.

Q And would you agree that the thickness of the layer is a function of the temperature at which it formed?

A I would not expect in normal circumstances to see an adherent, tight oxide of substantial thickness that formed exclusively at room temperature. There are some circumstances that can do it, but in the environment as I visualize it, I would not anticipate that to be the case.

JUDGE BRENNER: Why do you say "room temperature"? What about operational temperature of the diesels?

WITNESS BUSH: From the oxidation point of view, even though you get a doubling time or a doubling rate in 20 or 30 degrees, a spectrum up to 150 or 200 degrees Fahrenheit is relatively trivial compared to 600 or 800 degrees Fahrenheit.

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BY MR. ELLIS:

Q Dr. Anderson, I want to give you now an opportunity to comment.

A (Witness Anderson) Well, I think it is very clear that the test is meaningful because if it was a low temperature, the mechanism is different and therefore, you would not find high-temperature products there.

If it was a high-temperature product -- or a high-temperature mechanism that was operating, then you are going to go through several temperature phases and you may actually impose some low-temperature mechanism on top of it.

But I believe the x-ray would be definitive and would end all discussion of the matter.

Q Would you expect to see, Dr. Anderson, various oxides in this layer, both wustite and magnatite and the hematite as well?

A I believe I would have a mixed spectrum if it was a high-temperature mechanism. I believe it would be a simple spectrum if it was a low-temperature mechanism.

JUDGE BRENNER: Do you agree or disagree with that, Dr. Bush?

WITNESS BUSH: Yes, completely.

MR. ELLIS: I'm sorry, Judge Brenner.

WITNESS BUSH: I do agree I would expect a

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complex oxide if I started on high temperatures and went down.

If I had an exclusively low-temperature phenomenon, I would expect to see a very limited number of crystallographic forms of oxides.

JUDGE BRENNER: All right.

Dr. Rau?

BY MR. ELLIS:

Q Dr. Rau, do you agree with that?

A (Witness Rau) Not completely. Certainly if it were a low-temperature oxide you would not have the additional complications of the wustite and the magnatite, but you do have FE203, plus various hydrated forms of it, so it is not simply a single crystallographic structure.

Q Let me stop you right there.

Do you agree with that, Dr. Bush?

A (Witness Bush) I said simpler; I didn't say it would be a single one necessarily. So yes, that would be the case.

Q All right.

Go ahead, please, Dr. Rau.

A (Witness Rau) I think that probably answers your question, although I would simply add that if we did have purely a low-temperature oxide, it couldn't possibly be as thick as it is, number one, and it would have to have some

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rusty color to it, and it didn't have that either.

So there is strong direct evidence that it is not in fact exclusively a low-temperature oxide, and for that reason, I don't think it was necessary or meaningful to do an x-ray analysis to identify the presence of low-temperature oxide which I fully expect to be there, in part anyway, due to the cooling down from the formation temperature.

MR. ELLIS: Judge Brenner, I propose to leave that particular point, in the event you wanted or Judge Morris wanted to pursue it.

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JUDGE BRENNER: How long would it take to do the test?

WITNESS RAU: Judge Brenner, there's not a simple answer to that. As I say, the test could be done in a matter of a day or two, getting the X-ray spectra. But I would indicate to you the interpretation, if in fact it is not only low-temperature oxide, can take months, in fact, may never get resolved completely. I have been involved in situations where that's the case. But getting the X-ray spectra is only a matter of a day or so.

WITNESS ANDERSON: Can I add to that?

I am not aware of the Failure Analysis capability in X-ray work, I haven't seen their facility. I would say that if someplace that I was aware of, such as Crone Associates in Chicago that specialize in micro X-ray analysis and actually sell equipment and do analysis, I would assume that they could do it with interpretations certainly within some fraction of a week.

BY MR. ELL S:

Q Dr. Rau, is FaAA experienced in conducting such tests?

A (Witness Wachob) We have been involved in several instances where the oxide was a major factor in our cases and in each instance we were unable to definitively in a short period of time come up with that. I agree with

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Dr. Rau's statement that one of the ones we are working on presently it has been six to nine months in discerning the actual composition of the oxide.

Q My question was is FaAA experienced in doing that sort of test?

A Yes, we are experienced with how it's done.

Q And have you done it on a number of occasions?

A We have had laboratories do the experimental work and then we have done the analysis, yes.

JUDGE BRENNER: Dr. Rau, what if you weren't trying to discover exactly what the composition was but just trying to determine whether it was relatively simple in terms of a smaller number of oxides as compared to the larger number that it sounds like all of the witnesses would expect if there was a high-temperature mechanism at work? Would the interpretation also be difficult and take a long time?

WITNESS RAU: It is a continuous spectrum, Judge Brenner. I mean clearly it depends on how complicated the X-ray spectra comes out to be and the one which we are most recently familiar with and Dr. Wachob has made reference to, the spectra was very complicated. We didn't expect that. We set out what we thought would be a relatively straightforward examination and simply found out that it was not that simple.

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But yes, if you are only looking -- if it turns out to be a simple spectra you can analyze it quite directly. But oftentimes when you have an oxide formed over a range of temperatures or a range of operating conditions, it does not turn out to be that simple.

JUDGE BRENNER: What I am thinking of, I probably didn't ask the question very well, if it turned out to be a complicated spectrum, wouldn't that be probitive evidence of the fact that your hypothesis is correct that the oxide formed over a range of temperatures including very high temperatures down to lower temperatures?

WITNESS RAU: I would like to give you a simple yes, I think the answer is yes but again --

JUDGE BRENNER: You won't know until you see --

WITNESS RAU: I don't know until I see how complicated, and I think again because of the different hydrated forms of the oxides at low temperature you've got to be cautious arriving at a conclusion too quickly based on the results of the X-ray spectra.

BY MR. ELLIS:

Q With respect to LILCO Exhibit B-63 -- using that if you would, please, Dr. Rau -- Dr. Anderson, on page four and five of the rebuttal testimony states that:

"The characteristics of nickel iron weld material are such that they minimize shrinkage and

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therefore minimize the likelihood of tensile stress caused by post-cooling shrinkage."

Can you tell us whether you agree with that and, with respect to the exhibit, tell us what evidence you have for your opinion?

A (Witness Rau) Yes.

I guess I would have to hear the specific words again of the testimony, but let me paraphrase it and you can re-ask it if I don't get to the subject matter.

I do agree that the iron nickel is more closely matched in coefficient of thermal expansion than some other fillet rods might be, but I strongly disagree with any implication that the use of iron nickel would eliminate or even reduce significantly the magnitude of shrinkage stresses that are generated during a repair weld done without significant preheat.

LILCO Exhibit B-63 is not relevant to that particular issue but County Exhibit S-4 is in fact more relevant because that deals with the shrinkage crack associated with the repair weld. And if you look at County S-4, you see the presence of the crack in the cast iron adjacent to the repair weld, clearly this is an iron nickel weld rod, we have measured it to be that, and clearly it has cracked in the cast iron adjacent to that weld, in my opinion, as a result of the shrinkage stresses imposed when

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the repair weld was made.

In other words, the cast iron was not preheated substantially and, when the weld metal was puddled in there in liquid form, allowed to solidify, it shrinks and is basically prevented from shrinking by the cast iron around it and so tensile stresses are generated in and around the weld and those tensile stresses were sufficient to exceed the tensile strength of the degenerate Widmanstaetten graphite microstructure of the original 103 block in this location and that introduced these weld shrinkage cracks, if you like, associated with the repair weld.

I think that's it.

Q Dr. Bush, would you agree with that analysis of Dr. Rau?

A (Witness Bush) Yes, as I stated earlier in this instance, although the coefficients are quite comparable -- in fact you can almost overlay them over an extended range of temperature -- the fact that one object is basically at room temperature and sees only nominal heat and the other one is cooling down can lead to very, very high residual stresses under this circumstance.

Q Thank you.

Dr. Anderson, let me get two other points clarified and then I am going to come to you so that you can comment on this.

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Dr. Rau, was martensite present in the weld area and, if so, what is the significance with respect to tensile or shrinkage stresses?

A (Witness Rau) Yes, martensite was present in a very, very small heat-affected zone immediately adjacent to the weld metal. In a zone of less than five thousandths of an inch adjacent to the interface between the weld metal and the cast iron high, magnification metallography reveals the presence of martensite.

Now what that means with regard to preheat and shrinkage stresses is that the cast iron was not preheated up above the martensite start temperature. In other words, it's common practice to preheat the cast iron up to the order of 1000 degrees Fahrenheit prior to repair welding.

Clearly that was not done in this case because if it had been done then the entirety of the cast iron immediately adjacent to the weld would have cooled relatively slowly because the entirety would have been heated up above 1000, the weld would have been made and then everything would have more gradually cooled down.

The martensite, which is a hard, strong different phase of steel, forms as a result of very rapid cooling. And in my opinion, the presence of the martensite indicates that there was not significant preheat in the cast iron prior to the repair weld, in other words, the cast iron was

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nearly cold, and so that when the weld metal was melted in there the cold cast iron, if you like, sucked the heat out very quickly and resulted in a rapid cooling sufficient to generate the martensitic structure in this very narrow heat-affected zone. I think the very size of the zone as well as its presence are indicative of not very large, if any, preheat prior to the welding.

Q And Dr. Rau, yesterday Judge Brenner asked Dr. Anderson whether presence of pre- and postheat treatment increased the shrinkage stresses resulting from the weld.

What is your view on that?

A I think Judge Brenner was quite correct. One of the reasons you do a preheat is in fact to minimize the magnitude of residual stresses. And the absence of any preheat will maximize the shrinkage stresses because the shrinkage stresses are proportional to the difference in the amount of cooling the weld bead undergoes compared to the amount of cooling that the adjacent cast iron which is holding it or it is attached to undergoes.

And if you preheat them all to the same temperature -- because you would never preheat it to the melting point -- if you did you theoretically would get no residual stress in the weld bead. If you introduce no preheating at all, of course, you will get the maximum difference in the amount of shrinkage and therefore the

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maximum tensile residual stresses in the weld.

Q Dr. Anderson, would you care to comment on whether you agree or disagree with Dr. Rau's observations?

A (Witness Anderson) Well I am looking at the photographs which were mentioned which is, I guess, Suffolk County's Number 4 and there are several things that are obvious.

First of all, the weld was put in very cool and the heat -- I can say that because there is no gas porosity. Gases in metals dissolve to a greater extent at higher temperature and I don't see any there.

Secondly, the heat-affected zone must be very small. I agree that the heat-affected zone would be the martensitic region and the martensitic region is brittle. That is the brittle material that one would find that would have the characteristics of being able to crack mechanically.

I think it is rather difficult to determine whether it is a shrinkage phenomena which is in effect a mechanical one or whether it is an operational one which is also a mechanical one.

I do notice that if I set the weldment insert back in it does not fit, there is material missing between the cast iron and the weld metal. And there apparently has been removal of material by some mechanism which could be

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corrosion at low or high temperatures, we just don't know. This removal of material so that the crack does not match the opposite face could also be a polishing artifact but I have been assured that that never happens in their laboratory.

I would say I just really can't tell with confidence whether it is a shrinkage -- if I knew it was done at higher temperature, if I saw gas porosity and I knew it was higher temperature, if I saw more of a heat-affected zone which would indicate a higher temperature, then I would be persuaded that it is a shrinkage phenomena. But in the present condition, I just can't say.

JUDGE BRENNER: But in any event am I correct, Dr. Anderson, that you think that the cracking occurred in the brittle zone where the martensite is present?

WITNESS ANDERSON: Yes. There is general agreement, I believe, that the heat-affected zone is where the cracking would occur.

JUDGE BRENNER: If that's the case and if we assume for the sake of argument that that crack occurred due to operational stresses, then wouldn't that be attributed to the welding, including the technique, rather than any concern we should have for the blocks?

WITNESS ANDERSON: The welding has certainly sensitized that area to make it more likely to crack,

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however the heat-affected zone does not proceed significantly below the weld metal and therefore the cracks below the weldment do not represent cracks through a heat-affected zone or a martensitic region.

JUDGE BRENNER: Right. Those crack that you're talking about are the ones that Dr. Rau has called --

WITNESS ANDERSON: -- casting shrinkage cracks.

JUDGE BRENNER: Yes.

Right?

WITNESS ANDERSON: That's correct.

JUDGE BRENNER: Okay. But just staying with the weld cracks, what Dr. Rau has called weld shrinkage cracks, if I assume that they are operationally induced based on your testimony that you cannot rule that out, would it be correct that we should not worry about that because it is not predictive of what would happen to the portions of the blocks where there is no welding?

WITNESS ANDERSON: I think that the weld did sensitize the area. Therefore it made it more likely to crack in that region than it would be if there was no weld there at all.

Now I can't tell you how much sensitization or how much we have undermined the material but we certainly gave it the ideal path in which to fail along.

However my point is there is no sensitization

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below that area and had there been no weld we might very well still connect and crack down to the depth that we see here, I can't say.

JUDGE BRENNER: Well have there been any cracks found in the cam gallery areas of the 101 and 102 that were not welded?

WITNESS ANDERSON: I believe all of those were welded, I believe that the new 103 which has indications was not welded.

JUDGE BRENNER: I was going to ask about that separately.

WITNESS RAU: Judge Brenner, can I correct what I think is an error on the record?

I don't know whether it is Dr. Anderson's opinion or not but in point of fact the cracks which I have called the weld shrinkage cracks in the heat-affected zone are not in regions -- at least not primarily in regions where the martensite is present.

The martensite is present in a very narrow region immediately adjacent to the weld metal itself and, although some portions of the crack may occasionally extend into this region, the majority of the crack is in the degenerate Widmanstaetten graphite structure some substantial distance away from the martensitic structure. This martensitic structure is only over the first five thousandths of an inch

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adjacent to the weld metal. And as you can see from S-4 some of these cracks even at 100 times are very very much further away from that martensite. They are in the heat-affected zone but not in the zone which was heat-affected enough to produce the martensitic structure.

WITNESS ANDERSON: I appreciate that clarification because I could not see that, and if you say it's five thousandths, that's why I could not see it.

JUDGE BRENNER: If that's the case, Dr. Anderson, if those weld-associated cracks, if I can call them that, were operationally-induced, wouldn't you expect to find more of them in the -- well wouldn't you expect to find some of them in the 101 and 102 block in portions of the cam gallery where there were no weld repairs?

WITNESS ANDERSON: Yes.

JUDGE BRENNER: But none have been found.

WITNESS ANDERSON: I can't cite any that have been found.

MR. ELLIS: A couple of follow-ups, if I may, Judge Brenner.

BY MR. ELLIS:

Q Dr. Rau, even if the crack in the heat-affected -- in the weld, heat-affected area was affected by operation, would that tell you anything about the nature of propagation or lack thereof of the crack below the

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heat affected zone and below the weld repair?

A (Witness Rau) I don't understand your question, Mr. Ellis, I'm sorry.

Q Even if -- let me attack it this way:

Look at page four, if you would please, of Dr. Anderson's rebuttal testimony. In the middle of the page he says:

"If these premises are correct, then the weld material would adhere to the cast iron relatively uniformly and would break cleanly from that base metal if the moving force were tensile stress resulting from weld shrinkage. The fact that some cast iron was still adhering to the weld material that had separated from one side of the crack therefore makes it more likely that the separation was caused by operating stress and not weld shrinkage.

Do you agree with that, sir?

A No, I don't, Mr. Ellis.

Q Can you tell us why, please?

A Surely.

The presence of the crack in the cast iron which contains the degenerate Widmanstaetten structure, that same location would occur independent of whether or not the stresses induced were from operation or whether they were

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induced from shrinkage during the repair weld process. When the material is exposed to stress from any source it is going to crack along the weakest link in the chain exposed to that particular stress. And the fact that in some places the crack is a little further from the interface with the weld is strictly the crack seeking out those regions of highest concentration of degenerate Widmanstaetten graphite, if you like, and seeking out the path of least resistance and has no -- doesn't tell you anything about the source necessarily of the stresses.

JUDGE BRENNER: Dr. Anderson, in fairness to you, I think that's very close to what you said orally just a little while ago. Am I correct that you would agree with what Dr. Rau just said?

WITNESS ANDERSON: Yes, I do.

WITNESS RAU: Can I add one other point which I think again is a clarification?

I think I heard Dr. Anderson say that there is no evidence of porosity in this weld shrinkage crack and if there were that would be indicative of formation during casting or during fabrication.

I think if you look at S-4 you will in fact see that there is evidence of porosity: the rounded indications -- I don't have the orientation right, I think it's in the upper right of the photograph at the lowest magnification --

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show them. And I think if you go to the lower magnifications, a view at just one times magnification with your eye -- which has been provided to all parties -- of that same area, you will clearly see the presence of extensive porosity in and around the weld area. I could identify a particular photograph if that would be appropriate.

It is labeled as DG 103, Cam No. 7, 1612 -- excuse me, I612 and on the back it is designated by HFW4, 9/3/84. And it is again a low magnification view of the metallographic cross-section completely analogous to the sketches I produced on LILCO's B-61 except it is the actual photograph of the piece of metal. And it shows five or ten large pores located throughout the weld and along the weld interface.

WITNESS BUSH: Apparently Dr. Rau's interpretation of what Dr. Anderson said and mine are totally different. I thought he made the point that if there had been substantial preheat he would expect to see porosity and in the absence of porosity it might tend to be more towards the what I call a room temperature one. Now it looks like we are reversing ourselves 180 degrees. Perhaps I misunderstood but I thought for the record it might be better to clarify it at this time rather than try to do it a week from now.

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JUDGE BRENNER: Good idea. Especially since there will be no week from now.

Dr. Anderson, why don't you clarify what you point was on porosity in the weld area?

WITNESS ANDERSON: I think Dr. Bush was very perceptive, that was the thrust of the comment. There is no preheat and that's the characteristic that we see there.

JUDGE BRENNER: Dr. Rau, does that mean there was preheat since you found porosity?

WITNESS RAU: I don't think that's what Dr. Anderson just said, unless I misheard him. He said that there is porosity and that means no preheat.

Did I hear that correctly?

WITNESS ANDERSON: I'll say it again:

If a metal is heated up for some significant time at elevated temperatures it dissolves gases. If it cools down rapidly then there would be an expulsion of those gases. The heating up and the cooling down and the expulsion of gases would be such that the weldment is puddled in to the cold block, not preheated block, and I would see -- or expect to see this artifact on the surface as was pointed out.

(Pause.)

You're shaking your head. Should I try it again?

JUDGE BRENNER: I'm confused.

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WITNESS ANDERSON: Okay.

Do you want to --

WITNESS BUSH: It didn't sound like what I heard the first time, but the second time --

JUDGE BRENNER: Well let's stay with Dr. Anderson for now.

Tell you what. We're going to take a lunch break. And your task, should you choose to accept it, is the always difficult one of explaining it to me so that I understand it. Not an easy task.

MR. ELLIS: Judge Brenner, in response to the Board's request, I have those pleadings now.

JUDGE BRENNER: All right.

(Documents distributed.)

JUDGE BRENNER: Mr. Ellis, do you have an idea of how much time you have? I realize that's a highly unfair question given the way the procedure has evolved of your being interrupted all of the time and your being unable to gauge the answers also.

MR. ELLIS: Judge Brenner, I am doing my best to give all of the panel members --

JUDGE BRENNER: I'm not criticizing you at all.

MR. ELLIS: Right. And I hope in doing that I'm creating the kind of record that you had in mind, and I would think that two or three hours is the most that I

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would have.

JUDGE BRENNER: I expect to finish this panel today so that we could come back tomorrow morning and have the oral discussions we need to have on the various papers before us and the reports we asked for. We'll see.

Does the County have a lot of follow-up based on what we had so far or are my observations correct that this procedure would tend to minimize follow-up rather than maximize it?

MR. DYNNER: We are certainly going to have a number of questions and areas to be explored. It's really hard to say at this point how much but certainly there are a few areas which we feel have to be clarified.

JUDGE BRENNER: All right. Let's take the usual hour and a half lunch break and return at 1:35.

(Whereupon, at 12:05 p.m., the hearing in the above-entitled matter was recessed, to reconvene at 1:35 p.m., this same day.)

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AFTERNOON SESSION

(1:40 p.m.)

JUDGE BRENNER: All right.

Whereupon,

HARRY F. WACHOB,

CHARLES A. RAU,

ROBERT N. ANDERSON,

and

SPENCER H. BUSH

were recalled as witnesses and, having been previously duly sworn, testified further as follows.

JUDGE BRENNER: I apologize we are a few minutes late. We have been trying to absorb or at least read the papers we received this morning and were not able to get through all of them and we'll be receiving more.

As a preliminary matter there is at least one Staff document dated November 9th that we have never received because we have been up here I guess is the reason so certainly we should have received by now a document dated November 9 and hopefully it will be included in the package we get today, the Greenman affidavit is what it is. It was referenced in the County's answer and apparently involves housekeeping.

Preliminarily we are thinking of having an off the record discussion relative to the matters remanded by

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the Appeal Board next week in Bethesda, probably Tuesday. It would be a conference of parties. But we want to read the Staff's papers on the subject and we'll let you know more about that tomorrow morning.

MR. ELLIS: Can I tell Mr. Irwin that he need not be on the plane up here for tomorrow then?

JUDGE BRENNER: Yes, you can tell him that.

MR. ELLIS: Thank you.

JUDGE BRENNER: All right.

Mr. Dynner, you wanted to catch up and take care of the exhibits which support the stipulated material you put on the record this morning regarding firing pressures.

MR. DYNNER: Yes, sir. I have supplied to the parties and to the Court Reporter copies of Suffolk County Diesel Exhibits 9, 10 and 46 which were referenced in the footnotes to the testimony that was provided orally and stipulated this morning. I will briefly describe these exhibits.

Exhibit 9 is page 8-3 of the TransAmerica Delaval instruction manual for the DSR 48 diesel engine. It has a cover page to that effect so it is really a two-page document.

Exhibit 10 is an extract from the deposition of Gerald Edgar Trussell taken on May 7, 1984 and consists of a cover page together with pages 128 and 129.

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Exhibit 46 consists of documents numbers five through nine which were attachments to a letter of July 25, 1984 from counsel for the County to the American Bureau of Shipping and they are the documents which relate to the firing pressures.

I have pointed out to Mr. Ellis and I will point out to the Board to avoid confusion there that this document number five in that series of documents is a repeat of the extract of the deposition of Mr. Trussell. It has a few extra pages to it but it is in there because of the statement on pages 128 and 129.

(Whereupon, page 8-3 of the Delaval instruction manual for DSR 48 engine was marked as Suffolk County Exhibit 9 for identification.)

(Whereupon, pages 128 and 129 of 5/7/84 Trussell deposition were marked as Suffolk County Exhibit 10 for identification.)

(Whereupon, attachments five through nine to 7/25/84 letter to American Bureau of Shipping were marked as Suffolk County Exhibit 46 for identification.)

MR. DYNNER: I would move these exhibits into evidence.

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MR. ELLIS: Judge Brenner, I have no objection to the exhibits, I simply have not had the time and opportunity to check these against the exhibits that were originally offered. But I have no objection to the exhibits if they are the ones that were referred to in the testimony, I just got the package.

JUDGE BRENNER: All right. Well let's admit them into evidence by stipulation and subject to check. If we don't hear from you, Mr. Ellis, or anybody else we will assume that they match the initially furnished exhibits. And we are admitting them by stipulation solely for the purpose of their support for the stipulated facts which Mr. Dynner previously set forth on the record.

(Whereupon, the documents previously marked for identification as Suffolk County Exhibits 9, 10 and 46 were received in evidence.)

JUDGE BRENNER: Were you going to supply a written version of what you read into the record this morning? I'm not requiring it.

MR. DYNNER: Since it was so short I didn't plan to and the transcript would be available and it doesn't seem to me at this point a pressing matter, so we have not prepared a written report.

JUDGE BRENNER: All right.

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If there are no further matters --Mr. Perlis, did you have something?

MR. PERLIS: Judge, I have a report on the Staff's review on the pistons if you would like that now.

JUDGE BRENNER: All right.

MR. PERLIS: First of all, the Staff has reached preliminary conclusions as to the adequacy of the pistons and I will explain later what we need before we can reach a final conclusion.

The preliminary conclusion is that the pistons are adequate and the basis is as follows:

Adam Henriksen and Paul Louzecky performed a thorough visual examination of all of the pistons and related components for evidence of scuffing.

Mr. Henriksen's qualifications have already been, I believe, in evidence in this proceeding. Mr. Louzecky has a Masters in mechanical engineering and has 50 years' experience in diesel engines rated up to 15,000 horsepower.

Specifically Messrs. Henriksen and Louzecky looked at piston rings, cylinder liners and the pistons themselves. They found no evidence of scuffing. They did observe limited fettering around three rings -- on three of the rings but the limited fettering of the kind they saw was not unexpected and they considered it insignificant.

In terms of the non-destructive testing that was

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performed on the pistons, LILCO has performed liquid penetrant tests --

JUDGE BRENNER: Mr. Perlis, I wonder if I may ask one thing: Do you know if they looked at it in conjunction with the observers for the other parties, particularly the County?

MR. PERLIS: It is my understanding that they looked at it independently and they were not there at the same time as the County.

JUDGE BRENNER: All right. I'm sorry for the interruption. Go ahead.

MR. PERLIS: I'm told there was some overlap but that their examination was not done at the same time as the County's.

LILCO has performed liquid penetrant testing in the stud boss area of all eight piston skirts. Larry VanFleet, who is an employee of Pacific Northwest Labs -- and I will get to his qualifications in a minute -- he examined LILCO's liquid penetrant procedures and found those procedures to be consistent with the ASME Section 5. He verified that the examiners had the procedures with them and were following those procedures in the performance of the examinations. He also checked the credentials of the lead examiner and was satisfied with those credentials: I am told the lead examiner was certified as a Level 2 inspector.

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Mr. VanFleet has 10 years of experience in non-destructive testing. He is a Level 3 inspector for radiography, magnetic particle testing, dye penetrant testing and ultrasonic testing.

Mr. VanFleet witnessed the complete liquid penetrant testing on two of the piston skirts and found the manner in which the tests were performed to be satisfactory. He did see the other six piston skirts while the developer was still on the skirts, and Mr. Louzecky saw all eight pistons with the developer still on. Neither man observed anything of concern on any of the eight skirts.

For the Staff to reach a final conclusion we need to see LILCO inspection report on the eight pistons and specifically what the Staff needs to see is certification that the procedures that Mr. VanFleet witnessed performed on two of the piston examinations were in fact performed on all eight non-destructive examinations. And also we need a provision of LILCO's interpretation of any indications that may have been found.

We are told that we will be getting that report from LILCO some time today. We may be able to give a final report -- or our final conclusions to the Board tomorrow but it should be some short time period.

I would just conclude that again our preliminary conclusion is that we do find the pistons adequate.

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Assuming that LILCO's inspection data verifies these preliminary conclusions, the Staff then would be in a position to issue a final conclusion as to the adequacy of the pistons, and that we do intend at some point in the future to issue that opinion in writing. There will be some written report.

MR. ELLIS: Judge Brenner, the inspection report: I just assumed the Staff had it. I did give it, the one page, to Mr. Dynner. It's a one-page report, and I'm taking steps to have it delivered to the Staff this afternoon.

JUDGE BRENNER: That includes both points mentioned by Mr. Perlis as to what the Staff is looking for?

MR. ELLIS: I believe it does. I can't be certain about it, but I'm pretty sure it does.

JUDGE BRENNER: All right. Maybe, given that time frame, you can give us an oral update tomorrow morning, Mr. Perlis.

MR. PERLIS: We will try.

Again, because of the time difference, we need to talk to someone out at Richland, Washington. If we can get to them today, we'll try and give you a report tomorrow morning.

JUDGE BRENNER: Mr. Ellis is going to give it to you today.

All right, I think we can return to your

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questions of this panel now, Mr. Ellis.

EXAMINATION (Continued)

BY MR. ELLIS:

Q Dr. Anderson, have you made any analysis to reach a conclusion as to the nature of the stresses, residual stresses, beneath the repair weld?

A (Witness Anderson) No, I haven't.

Q I take it, then, you would not disagree with the testimony that Dr. Rau and, I believe, Dr. Bush gave that there will be balancing residual compressive stresses beneath the repair weld?

A I believe that they would be balancing, yes; otherwise there would be motion. I think it's the magnitude that is in question.

Q Do you have an opinion as to the magnitude of those stresses?

A No. I don't think that anybody does.

JUDGE BRENNER: Off the record.

(Discussion off the record.)

JUDGE BRENNER: Back on the record.

BY MR. ELLIS:

Q Am I correct, then, that, Dr. Anderson, that you do not have anything, one way or the other, whether the balancing residual compressive stresses beneath the repair weld would retard or prevent crack growth?

WRBwrb

A (Witness Anderson) I haven't seen any numbers that would allow me to make that decision; so it is correct, I don't have an opinion.

I think it should be examined. But there is no data.

Q Dr. Rau, have you examined the stresses, or made any analysis of the compressive stresses beneath the weld?

A (Witness Rau) Yes, Mr. Ellis, I have made an evaluation which included qualitative analysis of the magnitude and direction of the residual stresses, both in the shrinkage -- excuse me; in the repair weld and also in the cast iron beneath the repair weld.

As I testified previously, it's my opinion that there must be balancing compressive stresses beneath the repair weld. And, contrary to what Dr. Anderson has just said, the approximate magnitudes of those compressive residual stresses can be estimated from the approximate yield strength of the cast iron.

In other words, the weld material itself was stressed up to the point that exceeded the strength of the cast iron with the degenerate Widmanstaetten graphite properties. Because we know the size and we know the -- we have measured the strength of the old 103 block material with the degenerate Widmanstaetten graphite, we can bound, or estimate, if you like, the magnitude of the tensile

WRBwrb

residual stresses. And I think everybody agrees that the balancing compressive stresses must add up to the sum total of the tensile residual stresses in the weld times the size of the repair weld of concern.

So my evaluation has indicated that I would expect compressive residual tensile stresses perpendicular to the casting shrinkage crack. The magnitude of those stresses will be somewhat less than the tensile yield strength. And they would be distributed over a distance of the same order as the depth of the repair weld.

So if the repair weld is 3/8-inch deep, that would be in tension, and it would be a region of approximately 3/8-inch, perhaps a little bit larger than that, which would be in residual compressive stress balancing those tensile stresses beneath the repair weld.

MR. ELLIS: Judge Morris?

JUDGE MORRIS: Excuse me.

If you postulate the weld shrinking crack, would your answer change in any respect?

WITNESS RAU: If I postulate that the weld shrinkage crack cracks? Is that your question?

JUDGE MORRIS: That it exists.

WITNESS RAU: Yes, Judge Morris. My statement would not change. But when the weld shrinkage crack forms, there would be a simultaneous relaxation of the tensile

WRBwrb

residual while that crack formed adjacent to the weld, and a corresponding relaxation or diminution of the compressive residual stresses beneath the weld. So they would both tend to approach zero as that crack formed.

But whatever residual stress remains, if any, it would continue to be compressive beneath the weld, and any residual stress which remained in the weld would have to be small if it is cracked; obviously it would continue to be small, but tensile.

JUDGE MORRIS: Thank you

BY MR. ELLIS:

Q Dr. Anderson, does that explanation help you?

A (Witness Anderson) Well, I am still troubled by it. I can see where estimates could be made that are favorable to finding compressive forces.

What I was referring to of "no data" was that I'm not aware of any experiments that have been run to verify the analytic estimates.

Q Dr. Rau, do you consider it necessary to conduct experiments; and, if not, why not?

A (Witness Rau) No, Mr. Ellis, I did not, and I do not, consider it necessary to run experiments to verify that the residual stresses are compressive beneath the weld. They clearly must.

It's clear that the residual stresses from the

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casting process itself, which are responsible for the formation of the casting shrinkage crack, had been relaxed, or dropped off to the point that the casting crack stopped forming, extending, during the casting process.

So clearly the residual stresses associated with the casting are either negative or very low. Therefore, the superposition upon that of additional compressive stresses associated with the repair weld would only serve to reduce those already low residual stresses to a lower value still. And that, in conjunction with the live stresses associated with the throughbolt clamping, are sufficient to maintain that area in full compression perpendicular to the shrinkage crack indications under all operating conditions. Therefore, I don't believe it's necessary to measure how much more compressive they might be due to the repair welds.

Q Dr. Bush, am I correct, from your previous testimony, that you are in general agreement with Dr. Rau on the compressive stresses and the testimony he has given -- the compressive stresses below the weld?

A (Witness Bush) I believe the model I suggested is almost in virtual 100 percent agreement with that; obviously not the values. And certainly in the cracking I made the caveat that you would get relaxation.

There is a caveat that I would like to raise, if I could, because it relates directly to what we just heard.

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I would agree that the level of residual stresses, either compressive or tensile, after the weld is cracked would tend to be quite low. That's one.

The second item is that I believe that we should have overall compressive stresses to a degree.

The only caveat I have remaining is that you can have a compressive field near the bolt, but because of the geometry the maximum compressive field may be located well away from the surface, and you might have bending moments, either on the rear face or the front face.

I have no way of evaluating these, but I would anticipate that this would tend to reduce the compressive field. The only time this becomes a problem is if I have an extremely deep crack, which means that it might be moving into a positive field. I don't anticipate this is going to be a problem. That's the one concern I would have under this whole situation. This tends to apply more to the 103 block, which makes the issue somewhat academic, contrasted to the others.

Q Yes.

Dr. Bush, the physical evidence we have indicates that if these bending stresses exist, they exist deeper than any of the cracks that were found on the 103 block, the original 103 block.

A That would be my anticipation. I would expect

WRBbrb

that, but I must confess I haven't seen what I would call even a semi-rigorous analysis of this situation; and since there was no strain gaging in this area, I can't say unequivocally.

Q Dr. Rau, did you want to comment on that?

A (Witness Rau) I would just like to add that the extensive operation of the original 103 block with the degenerate Widmanstaetten graphite and with the casting shrinkage cracks for over 1200 operating hours, many of which at very high conditions, without any evidence of crack extension is a very clear and definitive evaluation that, in fact, what Dr. Bush has said is, in fact, true.

In other words, if, in fact, there are bending stresses, they obviously must be well below what the deepest of the cracks were in the original 103 because they persisted over this entirety of time; they did not extend at all.

Q Dr. Anderson, I'm going to leave this particular subject, but I don't want to leave before giving you a last opportunity, if you wish.

JUDGE BRENNER: I wonder if I could jump in; but let me wait and see if Dr. Anderson did want to add something.

WITNESS ANDERSON: I think I would just be repititious if I said something else. I think it goes back

WRBbrb

to whether the cracks below have actually progressed because of engine operation or whether they were there unchanged from the time of fabrication.

WITNESS BUSH: If I may, I would like to simply indicate that, perhaps later on in conjunction with questions from the Staff, I intended to indicate my approach to this, to such a problem in the sense of analyzing the probabilities as one goes along through an event tree.

The significant item would be the item that was raised last: namely, one has to look at all of the evidence there, which is -- even though I may concede that there might be positive bending stresses -- you have to look at not only the 101, the 102 and the 103 blocks; you have to look at every bit of evidence and see what the situation is. And this is what I have attempted to do. I will address it further, but I think it tends to supplement or amplify on the issue that Dr. Rau raised earlier.

JUDGE BRENNER: Dr. Bush, just so that I understand, in terms of strain gaging to determine whether there are bending moments there: if somebody felt it necessary to do that, that would have to be a strain gage on the water jacket side of the cam gallery?

WITNESS BUSH: That's correct.

JUDGE BRENNER: Is that feasible?

WITNESS BUSH: Yes.

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JUDGE BRENNER: Is it feasible, even after an engine is essentially assembled, to do that?

WITNESS BUSH: Well, you would have to relax the bolts to get the meaningful data on the thing; but the answer is yes. It's simply a matter of coating for protection on the thing.

In fact, I regret during our discussion as to the instrumentation that I was not -- I was too dense to consider that one, because I was thinking more of the cam gallery area. And I realize belatedly, after looking at the data, that we would have had a more significant test if there had been a few strain gages on the rear face, essentially opposite those particular cam galleries that have been instrumented. I consider myself remiss in this respect.

MR. ELLIS:

Q Dr. Rau, did you have a -- let me follow up with Dr. Bush.

Your comment, Dr. Bush, that you need to look at all of the evidence in order to reach a conclusion: did you have in mind there the significant number of hours of operation on 103 and the measures temps of those --

A (Witness Bush) I would go far beyond that. Let me take two minutes and indicate.

If I had a new diesel generator that had -- a

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completely new model, and I found cracks in the cam gallery and I were a consultant, I would have one answer: take them out and repair them.

Here we have three diesel generators, all of them with, essentially, repairs in every one of the locations. That leads me to one conclusion: that if I were to look at diesel generators manufactured for other purposes, I would say the probability of similar repairs there would be virtually one, because these are introduced during the fabrication or casting process.

This tells me that I have a much larger body of evidence, perhaps not admissible in the legal sense, but in the technical sense that I need to look at. And I would say that in this particular region plants that have had, perhaps, 20 to 30,000 operating hours would represent a very significant series of data points on that.

And one of the things that I would rely on -- I couldn't rely on that personally, but I could rely on people such as Mr. Hendricksen -- would be that if leaks had been observed in this area, what is known as the grapevine is very effective throughout almost all industry: I would have expected those to surface.

And so that is the type of information that I would collect together that gives me some degree of confidence. This tends to go along the line of Dr. Rau's

WRBbrb

comment that that -- when one looks at it: so it isn't just the analysis, it's the actual experience in units that are outside of the problem. So this is a consideration, from a technical point of view, that I would consider. That was the point I was making.

Q And all of that is what leads you to your conclusion that they are process cracks that have not propagated?

A That's correct.

When I look at a thing like this, I attempt to develop a mental event tree, go down that whole branch, consider the logic -- or the most logical branches, then rule out the least logical branches until I come to a conclusion. And that's the approach that I used here.

Q Dr. Rau, did you have a comment on that?

A (Witness Rau) Yes, Mr. Ellis.

I did in fact consider the possibility of bending in the cam gallery area. At the time, we had discussions about what the nature of the strain gage program might be, if in fact it were done.

It was my belief, based on the analyses completed, that it was not necessary to instrument the water jacket side of the cam gallery, although I did agree with Dr. Bush that it's a possibility of getting bending stresses. There were two reasons why I believe it was not

WRBbrb

necessary to go any further.

The first reason was that the analyses I had done, the hand stress analyses that I had done in the cam gallery area, indicated that there was, in fact, a bending component, but the bending component was such that the strains or stresses would be highest on the outside, where the weld repair is, not on the water jacket side, as a result of the through-bolt clamping.

So, although there might be bending, even with a deep crack it was my opinion that the bending would be such that it would even be more conservative, if in fact it existed.

(Pause.)

Q Are you done, Dr. Rau?

A No. I'm trying to think of my second point.

(Pause.)

I'm sorry. I've lost it now. It'll come back.

MR. ELLIS: I'm prepared to move on to another part, if I may, Judge Brenner.

JUDGE BRENNER: A point unrelated to whether or not there are -- whether or not the stresses in the cam gallery region are compressive?

MR. ELLIS: Yes, sir.

JUDGE BRENNER: I guess I have a few questions.

Dr. Bush, I want to ask you: let's assume there

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is a strain gage program performed on the 103 engine at 3300 kw that shows that the stresses there are always compressive.

WITNESS BUSH: We have to recall that when we put strain gages on -- conventional strain gages -- we're measuring surface conditions. We have to go to inference from other behavior to attempt to establish what I call the stress distribution through the section.

In other words, one might -- if one has bending moments at the surface, it would reduce the level of compressive stresses whereas, perhaps, three-quarters of an inch or an inch below the surface, as we have here -- particularly near the bolts -- the compressive stresses might be substantially higher than is noted here.

In the 103 block, I presume one could use conventional gages under those circumstances to establish -- but we have to realize that we would only be seeing the behavior near the surface, nothing more.

JUDGE BRENNER: All right.

Assuming only for now that the necessary analyses, including inferences as well as the surface measurements, are valid for the 103 block, and the conclusion is that the stresses are always compressive, would it be valid to apply that result to that same block running at 3900, starting up at 3900 kw? And I'll ask the

WRBbrb

same question in terms of operating at 3500 kw.

WITNESS BUSH: I can only give you a qualitative opinion.

I believe so. I would expect that the possibility exists, though I think I'm less concerned with this area than some other areas, that I would see a reduction in peak stresses, peak compressive stresses; and I can only say that my gut feeling -- and that's all it would be -- would be that the amount of change would not be significant. And that's about all I could say on that case.

JUDGE BRENNER: Is there an analysis that could be performed, short of running the same test at the different loads, in order to determine the applicability of the results at that lower load to operation at the higher loads?

WITNESS BUSH: I guess what I would want to see in this circumstance would be at least some strain gage data at higher loads, even for a very short time. One might do it just a matter of hours to indicate if, indeed, both -- assuming that we have the same bolt up stresses -- that both the range and the essential values were, say, within ten percent or something of that nature, which is what I would say would be within reasonable error under the circumstances, recognizing that this material does have somewhat superior properties.

WRBbrb

If one saw that, then I believe one could begin to make correct inferences to behavior at the other values. These values, to my knowledge, do not exist.

JUDGE BRENNER: I'll get to you in a minute, Dr. Rau.

I want to ask you a similar question, starting with the same baseline: that is, assuming for the sake of argument that the analysis for the 103 block at 3300 kw is valid for that block at that load, and that the result shows that the stresses in the cam gallery region are always compressive, could you take that result and apply it to the 101 and the 102 block, given the cracks and weld repairs that exist in those blocks, even at the same load? I'm not changing the load.

WITNESS BUSH: You're addressing the question to me?

JUDGE BRENNER: Yes.

WRBeb

WITNESS BUSH: I think inferentially, yes. You would have to take into account that you have a material that has lower mechanical properties. This is fairly straightforward.

You would have to make some assumptions about the crack tip behavior in there, but if the 103 measurements -- Say hypothetically you had some at 3300, 3500, and 3900, and they showed the same thing. Then I believe it would be possible to extrapolate from those data, including the assumption of a crack of a finite depth, particularly if you were able to wash out the problem of residual stresses from the welding and make some inferences with regard to the 101 and the 102 blocks.

JUDGE BRENNER: How could you, as you said, wash out the problem from the residual stresses?

WITNESS BUSH: If the welds have cracked, and most of them have, then to me at least that reduces the level to the point where the contribution to the residual stress begins to become insignificant.

JUDGE BRENNER: Dr. Rau, I think you wanted an opportunity to comment.

WITNESS RAU: Yes, I wanted to respond, Judge Brenner, to your question about whether a calculation could be made to infer what the stress levels in the cam gallery would be at, say, 3500 Kw or 3900 Kw from the

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measurements made at 3300 Kw.

The answer to that is definitely yes. The strain gage measurements themselves indicate that the primary cyclic stresses, the alternating stresses in the cam gallery area are the result of cylinder firing pressure. In other words it's the combination of the pressure expanding radially within the cylinder and, simultaneously, lifting up on the head which in turn lifts up on the through-bolts, or if you like, unloads the through-bolts to make them less compressive.

So we know from the measurements that the primary force that is causing the cyclic variation is in fact the pressurization and unpressurization as the cylinders fire.

So all that need be done to compute the effect of the variation or the cyclic stresses at 350 or 39 from the results at 33 is to scale by the cylinder firing pressure at those relative power levels.

And if in fact you do that you find that the stresses remain fully compressive perpendicular to the crack indications at all power levels up to 3900.

JUDGE BRENNER: As long as the strain gages are in place, wouldn't it be better to run the engine at those higher loads even for a brief period of time, just to make sure there isn't some mechanism occurring that you have not taken into account?

WRBeb

WITNESS RAU: I think it is fair to say that that would provide additional confidence, but in my opinion, examining the strain gage output, I don't believe there is any other mechanism that could cause a significant deviation from the kind of calculation I have indicated. And therefore, I don't personally think it is necessary, but surely it would be a verification of the opinion I have given.

JUDGE BRENNER: All right.

I may have cut you off. I don't know if you wanted to add anything on my other question which was applicability of the strain gage testing to the 101 and the 102 blocks.

WITNESS RAU: Yes, Judge Brenner.

I would agree with what Dr. Bush has said. The presence of deeper weld shrinkage cracks in 101 and 102 than were present in the replacement 103 will in fact modify the magnitude of the residual stresses that might be present. There are, of course, perhaps other differences in residual stresses due to the different casting procedures of the replacement 103.

As I think we have discussed over the last couple of hours, there is no question that the presence of the weld shrinkage cracks in 101 and 102 would only serve to reduce the magnitude of the residual stresses that are present.

WRBeb

The live stresses associated with engine operation which were measured by the strain gaging on the replacement 103 would still be representative of the live or operational stresses that are imposed upon 101 or 102. And the only impact of the larger weld shrinkage cracks would be on the residual stresses.

I don't mean to imply that the crack doesn't have some impact on the crack tip stress intensity factors with regard to fracture mechanics analysis, but that can be taken into account analytically. It doesn't really affect the validity of the strain gage measurements with regard to what the stresses would be, and for computing whether or not weld shrinkage cracks of any depth would or would not extend.

JUDGE BRENNER: Well, we have heard, in the context of a discussion of the piston contention, that the 103 engine has been at least somewhat disassembled after the latest test runs.

Are the strain gages still in place on the cam gallery area, if you know?

WITNESS WACHOB: The strain gages have been removed from the engine block.

JUDGE MORRIS: Were measurements made as a function of power level or only at the operating power?

WITNESS RAUN: Judge Morris, they were made as a function of power level. What was done was to-- As

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indicated I think in the preliminary results which at least were identified for identification, -- I don't have the exhibit number -- what was done is that the compressive stresses generated by tightening up the through-bolts were first measured as a function of percentage of the maximum torque specified.

Then the engine was run through a fast start, and the cyclic stresses in combination with the steady were measured with the strain gages during the quick start.

Then the engine was held at 3300, and the steady state operating stresses at that condition were measured.

And then the engine was stepped down to various power levels in the cyclic and steady stresses were recorded at each of those power levels.

JUDGE MORRIS: I guess that is LOLCO Exhibit B-60.

WITNESS RAU: Yes, sir. That's the preliminary results.

JUDGE BRENNER: For identification. It is not in evidence, as lawyers know.

Did FaAA make a recommendation not to run the engine at higher than 3300 Kw for purposes of the camshaft gallery region strain gage data?

WITNESS RAU: No, Judge Brenner, we did not make any recommendation, one way or the other.

WRBeb

As you may recall, the recommendation to even do that testing was made by the NRC Staff. It was our feeling, based on our calculations, that even that wasn't necessary. So we also did not specify any particular power level.

JUDGE BRENNER: Mr. Ellis, when you said you were leaving the subject, I'm not sure that that meant that you had no questions to ask of the other witnesses about Dr. Anderson's rebuttal testimony, Answer 3, which is at the top of page 2. And if you are going to get there at some point, I'll be quiet for now. But otherwise I have some questions.

MR. ELLIS: Judge Brenner, are you talking about--

JUDGE BRENNER: I'm talking about his testimony regarding the preferred way, in his view, to evaluate the amount of residual stress, including removing a piece.

MR. ELLIS: I thought we had covered that general subject, but let's cover it now. I was still going to be in the same general area of cam gallery, but let me cover this now. I thought I had.

JUDGE BRENNER: I'm talking about the top of page 2, Answer 3. I guess I missed it if you asked him about that.

Why don't you proceed?

BY MR. ELLIS:

WRBeb

Q Dr. Rau, I think you just testified that you did predict the existence of residual stresses in the areas using analytical methods short of actual testing. Am I correct?

A (Witness Rau) Yes.

Q On page 2, Dr. Anderson, you say:

"The preferred way to evaluate the amount of residual stress in a structure with any degree of scientific certainty is to undertake strain gage testing of the surface in its existing state and compare those results to strain gage readings of the same surface after a piece has been removed from the vicinity."

You say that's the preferred way. What other ways are there, other than that way, that are acceptable?

A (Witness Anderson) I answered the same question once before, but if you would like it again....

JUDGE BRENNER: Why don't you do it in summary form? I was more interested in getting Drs. Rau and Wachob and Bush, their opinion. Dr. Anderson did answer a question on it earlier.

But why don't you give it in summary form, so the other witnesses will hear it again?

WITNESS ANDERSON: There are plastic coatings that can show stresses. You can put a plastic coating over

WRBeb

it and then look at the stress patterns in the coatings.

BY MR. ELLIS:

Q Dr. Rau, can you comment on -- have an opinion as to whether it is necessary to perform the type of evaluations that Dr. Anderson refers to at the top of page 2 of his rebuttal testimony and if not, why not?

A (Witness Rau) Okay.

It definitely is not necessary to make those measurements in the way in which Dr. Anderson prefers. I would agree with Dr. Anderson, I also prefer to make residual stress measurements in generally the way in which he has described. It is in fact the most reliable way.

That is, you put strain gages on and then you gradually cut out the part and then you gradually grind away at one surface and keep measuring the change and the part's shape, and eventually you infer what the stresses must have been throughout the part.

But of course that method is completely destructive. You've destroyed the entire part in order to ascertain what the residual stresses were throughout the entire part. So although it may be preferred, it is not necessarily realistic. There are other ways to do it. The analytical ways which I've described are one.

There are ways that Dr. Anderson has described that are also used. There are ways which he has not

WRBeb

described which we also take advantage of.

There are x-ray techniques which can be used by some people to infer residual stresses. I personally find them quite unreliable and don't recommend them, but there are other people who don't agree with me completely.

There are ways to drill holes in the metal part after having placed the strain gages around the outside where the hole is being drilled. By inferring the amount of relaxation when you drill holes you can infer what the residual stresses must have been at the surface into which you drilled those holes.

I have used all these techniques on various occasions. They are all appropriate ways to get at the magnitude of the residual stresses. I don't think any one or the other is necessarily preferred because it depends upon a number of considerations. And I believe any number of those would be appropriate and have been appropriate for evaluating the cam gallery region of the EDGs.

Q Dr. Bush, are you in general agreement with Dr. Rau's statement?

A (Witness Bush) Yes, to a degree. There indeed are several techniques that can be used. Whether it is necessary in this case is another point.

I might indicate that the ASM Handbook in this particular area cites specific measurements of residual

WRBeb

stresses, very high residual stresses, and the removal of such stresses. They are using a somewhat different technique.

They have stresses and they relax them and then by difference determine them, which is a way that is still another method. You are not removing material in this instance. Obviously that means high-temperature strain gages. They are reporting stresses in the 20 to 25 Ksi range in the higher-strength materials.

But there are techniques. They can be removed -- or they can be measured, rather, not removed. I'm sorry.

In this instance where the predominant cause is I think the weldment, it is probably marginal to consider actually physically measuring, since the weld is cracked.

Q Dr. Bush, I am correct, am I not, that you don't now recommend that -- or do not recommend that the FaAA perform the sort of destructive testing that Dr. Anderson describes at the top of page 2 of his rebuttal testimony?

A That's correct.

JUDGE MORRIS: Recognizing perhaps the non-desirability of taking material from the existing blocks, could anything be learned from the old 103 block?

WITNESS RAU: Judge Morris, I don't think very much can be learned from the original 103 block for the following reason:

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The destructive examination, as well as the TSI depth gages, have confirmed the presence of deep casting shrinkage cracks in each of the cam galleries of the original 103. Due to the presence of those cracks, the residual stresses which were in part responsible for their formation have been relaxed extensively. And although it would be possible to put the strain gages on and cut up those cam galleries and to measure that portion of the residual stresses that may have been there originally which still remains, I believe, as Dr. Bush has indicated, that they would now be quite low and might even be lower than the sensitivity of your measurements.

But in any case, I don't believe they would be terribly relevant, or give us any additional information to reach a conclusion.

JUDGE MORRIS: Dr. Anderson, Dr. Bush, do you concur in what Dr. Rau just testified?

WITNESS BUSH: Generally, yes.

There is another factor and that is that I would be very unwilling to extrapolate from any measurements on the old 103 block to any of the other blocks with regard to residual measurements. There are too many differences, microstructure-wise, I think to justify it.

WITNESS ANDERSON: I concur. I don't think the old 103 would be representative and therefore, the data,

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through of some merit, would not have too much value.

JUDGE MORRIS: Thank you.

BY MR. ELLIS:

Q Dr. Anderson,--

MR. ELLIS: May I proceed, Judge Brenner?

JUDGE BRENNER: Go ahead.

BY MR. ELLIS:

Q Dr. Anderson, turning to another subject, do you have any evidence that graphitic corrosion has ever occurred in a Delvac 1240 lubricating oil environment less than .05 percent water?

A (Witness Anderson) No.

But I haven't examined that material, but it is not clear to me that the crack environment is as you express it. That may be the bulk environment but it may not be the crack environment.

Q Would you agree, Dr. Anderson, that such graphitic corrosion is unlikely in such a lubricating oil environment with that percentage of water?

A I haven't looked at that. I really cannot agree or disagree. I just don't make a statement there.

Q How about you, Dr. Rau?

A (Witness Rau) I don't believe graphitic corrosion would occur to any significant rate in lubricating oil with that percentage of water. There is absolutely no

WRBeb

evidence that I am aware of anywhere in the world that in fact graphitic corrosion has ever occurred in a high-quality diesel lubricating oil with these percentages of water.

My engineering judgment, just based on experience, is that you are not going to get extensive corrosion or oxidation of any kind with the high-quality oils which have the anti-oxidants and anti-acids. It is just not going to occur.

Q I used Delvac 1240. Are you familiar, Dr. Rau, with the oil that is now being used?

A I know what it is. I am generally familiar with it.

Q Does it have an anti-oxidant?

A Yes, sir.

Q Dr. Rau, would fretting corrosion cause the dark color on the oxide that you saw?

A No, sir. The dark color comes from the kind of oxide it is, basically the magnatite intermediate-temperature oxide.

Q Dr. Anderson, do you agree with that?

A (Witness Anderson) Generally fretting corrosion tends to have a more chocolate color to it. But in the presence of the graphite that would be available as a colorant, it could certainly explain that color.

Q Dr. Rau, do you agree with that statement?

WRBeb

A (Witness Rau) No, I don't agree with it as a general statement, Mr. Ellis. Perhaps under more specific hypothetical conditions, some graphite could make the oxidation product darker or blacker, if you like.

But under the conditions relevant to the cam gallery, that is, low temperature, relatively low temperature, it is my belief that any kind of oxidation, whether it be fretting corrosion or something else, which occurs at those temperatures is going to result in an Fe203 which -- low-temperature oxide or rust, if you like, in hydrated form. And it is going to have less colors, which may be tempered by the presence of graphite but it is not going to turn black as the oxide appears in the cam gallery cracks that I observed.

Q Dr. Anderson, is it your view that fretting corrosion can occur in an oil environment with one angstrom of movement?

A (Witness Anderson) The oil environment tends to ameliorate the problem. It also ameliorates it if one material is harder than the other.

I was surprised in my reading on the subject to find that such small movements are included within the fretting description. Most of the fretting that I have seen has been much larger movement and much more amenable to oils preventing the fretting.

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However, we have a crack environment which is unique and we certainly do have small motions possible. So I would say in the crack environment it is entirely possible.

Q Dr. Rau, do you have a comment on that?

A (Witness Rau) Yes, sir. I disagree very strongly with that statement.

The one angstrom sort of motions which Dr. Anderson has made reference to, I have only read in one place that it is possible under relative motions of that size. It generally requires much larger sliding motions than that, factors of 100 and 1,000 times larger than that.

Q Dr. Rau, let me just interrupt you and ask you for those of us uninitiated, how much is an angstrom?

A An angstrom is 10 to the minus 8 centimeters. It is 1/100 million centimeters, or 1/10 to the minus 10 meters. It's a small number.

Generally speaking, as I was saying, the magnitudes of slip and vibration that are required for fretting are very much larger than that.

In addition, in my experience at least, you require both slip and vibration to introduce any fretting corrosion, and there is absolutely no evidence that we have got any slip in the cam gallery area. There is no indication from my review of the stresses that I would

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expect any slip.

I can understand how there would be opening or, let's say, pulsating closure if you like, but I don't see any reason why it should be sliding back and forth, given my understanding of the loading there.

In addition to that, the presence of this high-quality lubricating oil would substantially reduce the amount of wear, friction and galling. And in fact cast iron has got a reputation for being particularly good at resisting galling, particularly when it's lubricated.

In my opinion, if you had a high-quality lubricating oil between cast iron surfaces it would require very substantial amounts of slip to get any fretting corrosion.

Q Dr. Bush, am I corre--- Are you in general agreement with Dr. Rau on fretting corrosion?

A (Witness Bush) Somewhat. I would like to add one other aspect of this.

By and large, the fact that they have the oil there I think has ameliatory effect.

There is one other aspect of fretting corrosion. I am not quite sure I agree with Dr. Rau that you must exclusively have slip. I am aware of cases where the surfaces tend to come together and the surfaces in essence bond to a degree, and pull apart. That's one aspect of it,

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though I agree generally it would be both slip and vibration.

There is another aspect and that is that in many instances of fretting corrosion -- it may not apply here because of the lubricating properties of the graphite per se -- is that generally in fretting corrosion, when you essentially pull material out, which is exactly what you're doing here, between the two surfaces, the particles then tend to oxide and then tend to embed.

So if you examine the surface, in many instances at least you will see evidence of it. In fact, they often state that this is fretting corrosion as contrasted to some other type of corrosion because you literally disturbed the two surfaces by the presence of the material, normally an oxide.

Q And am I correct that no such evidence existed here?

A Well, let's put it this way, that if those surfaces had been truly disturbed, I would have thought it would have tended to be evident at 500 X magnification. I can't rule it out exclusively, but it didn't seem to be there.

That's a question that perhaps Dr. Wachob could address better than I. But certainly I didn't see it, and normally one doesn't even have to etch under those

WRBeb

circumstances because you'll see it in the sense that what you have is what amounts to a very, very thin layer of cold work.

Q Before I go to Dr. Wachob, -- and then I'll come back to you, Dr. Anderson -- let me confirm, Dr. Bush:

Am I correct that it is still your view that fretting corrosion did not play a role in connection with the cracks in the cam gallery?

A I don't see any evidence that makes me believe that fretting corrosion was a factor.

Q Dr. Wachob?

A (Witness Wachob) Two other aspects of fretting corrosion which I think give us evidence to indicate that there really is none in this instance are, one, when you have fretting corrosion you are grinding the surfaces together and making little iron oxide particles and you are piling them up on the surface.

Some of them, as Dr. Bush has said, get embedded in the surface. Other ones, however, tend to build up and make a caked layer on the fretting surface. And in that instance it can easily take and disturb or remove that oxide afterwards if you were to break up the fracture surface.

Dr. Anderson and everybody else here I think strongly believes that the black oxide we're talking about is very tenacious on the surface. It is not very easily

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removable.

So those are two almost extreme -- two of the problems, so that therefore, in this case we don't have a loose oxide. We have a very tight, tenacious oxide.

Secondly, one of the other points that frequently occurs when you're looking at the fretting corrosion is that you do get pits on the surface and subsequently, those can lead to additional problems. And in no instance did we see on the fractographs to a thousand X any indication of pits or light oxide.

Q Dr. Anderson?

A (Witness Anderson) Yes.

I believe that we talked about very small dimensions on the slip which could not be resolved by the techniques that have been used to date. Moreover, the lubrication in the crack is uncertain. The buildup of these oxide products from fretting could very well be hammered into a coating equivalent to what we saw, a tenacious coating. It is just not clear what the crack dynamics are doing to it.

I think once again it goes back to doing a proper analysis of the surface of the crack by some x-ray technique to really put to rest the question.

Q Dr. Wachob, did you have a comment?

A (Witness Wachob) Yes, two aspects.

WRBeb

First, in looking at the metallography of this dark oxide on the surface, you get a very good indication that it has not been disturbed and particles embedded in that surface.

Secondly, when you look at the literature that indicates fretting corrosion, the amount of oxide or weight loss or whatever they are going to define the oxidation process as as a function of slip or motion, is, at least in some cases, very linear. So that as your motion becomes less and less, your slip becomes less and less. You get down to infinitesimal amounts of oxidation, and you are not talking about the large thick oxide that would be produced in this instance.

Q Dr. Anderson, let me give you the last opportunity. Do you disagree with that in any respect?

A (Witness Anderson) The last word? Well, I'm not sure that .2 of a mill is a large thick coating, and I think it boils down to a credible analysis of the coating material, a complete, credible analysis of the coating material.

WRBwrb

MR. ELLIS: Judge Brenner, I'm going to go on to another subject, unless...

JUDGE BRENNER: Go ahead.

BY MR. ELLIS:

Q Dr. Anderson and the panel generally, look at page 6 of the County's supplemental testimony, please, at the top.

Do you have that, Dr. Anderson?

A (Witness Anderson) Yes, I do. I'm reading it now.

Q There's a statement there-- Take all the time you need to read the material around it, if you think it's important.

The statement I'm focussing on is,

"If air did enter the cam gallery area it could do so only by diffusion in small amounts over a short period before the surface metal cools to the point where any hot tears present would not form oxides."

What is your basis for that?

A I must not be on the right page. What page, again?

Q Page 6 of the supplemental testimony.

A Thank you.

Q I'm sorry; I may have misspoken.

WRBwrb

JUDGE BRENNER: You said it right.

WITNESS ANDERSON: Okay. The basis for the paragraph is what you would like?

BY MR. ELLIS:

Q The basis for the statement relating to,

"If air did enter the cam gallery area, it could do so only by diffusion in small amounts over a short period before the surface metal cools to the point where any hot tears present would not form oxides."

A (Witness Anderson) Okay. I just explain, the first strong reducing conditions, the organics literally catch fire at the beginning, and there are a number of vent holes which are burning when we initially pour.

The block is a homogeneous block, there are no vents or ventilators built into it other than the vents I've described, at the top, to get rid of the gases.

When it cools so that the organics are no longer burning off, then air can truly backfill. However, if the temperature is increased, the partial pressure of the oxygen is decreased. Pv equals NRT. There's a relationship between the temperature and the--

JUDGE BRENNER: Dr. Anderson, I wonder if I could interrupt. It may help to focus it. But if you think you still need to say what you wanted to say, feel free to do so.

WRBwrb

The focus, I think, has been testimony by other witnesses that the cooling process of this large block is a lengthy one.

WITNESS ANDERSON: Yes.

JUDGE BRENNER: Now, if you could focus on that cooling period.

We have other testimony, as I think you should know, that there is plenty of time for air to get in there while the temperature is hot enough to form oxides as a part of that relatively lengthy cooling process. Maybe you can address that period of the process.

WITNESS ANDERSON: Yes, I can.

The surface-- The mold, at least the molds I saw, are not preheated and, therefore, there is a tendency for the surface to solidify very rapidly on pouring.

The surface then eventually will see the oxygen that enters into the mold, and the surface in general can oxidize a bit, although-- And there is something called a mill scale, as it were, which forms in the gross parts of the material.

So there is oxygen that can come it. But it's after the surface is formed.

BY MR. ELLIS:

Q Dr. Anderson, will you agree that air can enter the mold at temperatures between 500 and 1500 degrees

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Fahrenheit?

A (Witness Anderson) Can? Yes.

Q So that the reducing effect that you refer to in your testimony would not be present during those temperatures?

A The reducing effect is initially the first few hours.

Q At what temperatures?

A Well, we're going to be pouring somewhere, I presume, around 24 to 26 hundred F.

Q And so the reducing effect that you refer to in your testimony is at those temperatures?

A Well, that's the pour temperature. And there is loss between the ladle, and there is loss when it initially enters the mold. So it will be something down... 1800, or something like that.

Q But am I correct that you would agree that the reducing effect that you refer to in your testimony is not present in the 500 to 1500 range?

A That's fair enough.

Q Dr. Rau, do you have any-- Do you agree; and, if not, why not; with the statement that I read on page 6 of the supplemental testimony? And in your answer you might address your agreement or disagreement with other things that Dr. Anderson has said.

WRBwrb

A (Witness Rau) I disagree strongly with the statement, for the following reasons:

Although there is, or may be, a reducing environment in the very, very early stages of solidification as the large casting cools, it shrinks. And as it shrinks it gets smaller. And the metal, the cast iron, gets smaller a lot faster than does the sand mold. Therefore, over a period of time, gaps result after the cast iron has solidified between the metal in the block and the adjacent mold.

Typical foundry practice is to allow about 1/8-inch gap for every foot in length of the casting. So we might expect, over a 10-foot-long block casting, to expect ten times -- .125, or over an inch of gap. Or, if you like, at every surface over a foot you might expect to see an air gap of an eighth of an inch, or something like that. It's not quite that large: that's a rule of thumb. It turns out to be about a factor of a third to a half of that.

But in point of fact, every interface which was originally a boundary between the metal and the mold becomes an air gap. And during the cooling process, as you come down in temperature that gap gets larger and larger. And there are gates which feed the liquid metal throughout this very large casting. Every one of those gates also shrinks

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away from the mold and provides a continuous path between the oxygen in the air and every piece of metal throughout that surface of the mold. And there is just no question that there will be an air environment surrounding that mold over a very large fraction of its cooling and therefore certainly air will be present and able to oxidize any surfaces that are exposed, like a shrinkage crack, if you like, at temperatures where oxidation can very definitely occur and occur at quite rapid rates.

A (Witness Anderson) May I say one thing?

Q Yes, by all means.

A Okay.

When the pour is made and we have completed the metal addition we then backfill all of the areas which the metal has been filled in and we have essentially a very special sand, it's not ordinary sand, beach sand, but it is a very special sand which is used to cover the top of the mold. And therefore the shrinkage that does occur has to -- any air has to diffuse through this material that has been used to fill the top of the mold.

Q But in terms of whether -- well, strike that.

MR. ELLIS: Judge Brenner, I don't see any -- I have nothing further in this area. I'm going to move on.

JUDGE BRENNER: Do you want to get Dr. Bush's opinion or at least ask him --

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MR. ELLIS: Yes, let me do that.

BY MR. ELLIS:

Q Dr. Bush, in your opinion would air enter into the cam gallery area in temperatures after the reducing effect of, say, roughly 1800 is past?

A (Witness Bush) Yes, I am on record stating that given the time that we have that I would expect interdiffusion.

Q Let me cover one other point while we're on page six, if I may --

JUDGE BRENNER: Let me back up with Dr. Bush, if I could.

Dr. Bush, what about Dr. Anderson's point that the metal has already begun to solidify. At that point would that detract from the conclusion that the high temperature oxidation could still take place?

WITNESS BUSH: I have assumed in all of my models that it has indeed solidified. In other words, I considered the case where we have solidification at least of the surface and perhaps partway through, possibly complete solidification depending on the thickness. I assumed that you have a reducing phase during the burnoff and by this time we have probably dropped the temperature down below 1800 degrees. Now that is just a rough number.

I would anticipate at this stage that even if one

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piled sand on it it isn't a very effective barrier against air penetration over an extended period and I would anticipate it would move in, see the metal surface, indeed form mill scale and by that token if there are cracks there the air can enter the cracks and could form some oxide products, at least that's the way I have considered the model.

JUDGE BRENNER: Dr. Anderson, did you want to comment at all?

WITNESS ANDERSON: The material that is added at the end is to limit the entrance of oxygen. Most castings, when they come out do have what I have referred to as scale by the very nature of the fact that they are warm for some period of time. In the larger castings that I would imagine this block is made it certainly should be solidified on the surface and have dropped considerably in temperature.

JUDGE BRENNER: I should have focused you a little better. As I understood Dr. Bush he said all of his assumptions have been consistent with what you said would occur and yet he concludes that the oxidation could still occur if there is no prohibition against the air coming into contact with the surfaces so as to form the type of oxidation observed.

WITNESS ANDERSON: I'm sorry, you said the type cf oxidation observed? What are you referring to?

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JUDGE BRENNER: The thick layer which Dr. Bush and Dr. Rau, I think, says is an oxidation that occurred during the cooling process.

WITNESS ANDERSON: Well the reason that they try and minimize the exposure to air while it's hot is not to prevent the scale of oxides, the reason is to try and prevent the loss of carbon by decarburization in the material which can essentially soften and put a ferritic matrix on the outside. I continue to say that it is not clear there is an oxide present until the appropriate analysis has been done.

JUDGE BRENNER: Yes, I understand that. But the part of your testimony that we are focusing on is one reason -- your statement, as I read it, that one reason there cannot be an oxide is because there is a mechanism that would prevent the contact of air with the surface and Dr. Bush and Dr. Rau have both stated why they disagree with you.

WITNESS ANDERSON: The mechanism I am postulating is the fact they backfill and essentially cover it with sand afterwards and also the lack of decarburization and that's the underlying basis for what I was saying there, that the oxidation wouldn't occur.

BY MR. ELLIS:

Q Dr. Rau, did you have any further response to

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Dr. Anderson?

A (Witness Rau) I don't know if it's appropriate but it seems to me that the presence of the scale, which is in fact an oxide, is clear evidence that the oxygen has -- or the air has in fact penetrated the sand and gotten to the mold. So I don't understand the argument.

JUDGE BRENNER: What scale?

WITNESS ANDERSON: The scale on the surface.

WITNESS RAU: The mill scale on the surface, all over the surface of the casting which is subsequently sandblasted off.

JUDGE BRENNER: How do you know there was mill scale all over the surface.

WITNESS RAU: I think Dr. Anderson said there was.

JUDGE BRENNER: How does he know?

WITNESS ANDERSON: No, I haven't seen them do a block. I said it is common in castings. I haven't seen a block poured. I saw the sequence for a head --

JUDGE BRENNER: All right. Let me make sure I understand the facts.

Nobody has seen any mill scale on the blocks that we're discussing here, am I right?

WITNESS ANDERSON: Correct.

WITNESS RAU: Well I have seen some oxides on the

WRBagb

-- some portions of the block which were not painted, but I can't be sure they were there from the time of solidification.

JUDGE BRENNER: All right. The next point is some of you have seen large castings that have been covered with sand under the procedure described by Dr. Anderson which have, nevertheless, developed mill scale even in the presence of that covering with the sand process, is that right?

WITNESS RAU: Absolutely.

JUDGE BRENNER: Dr. Anderson?

WITNESS ANDERSON: The purpose of the sand was to eliminate that on the heads. I did not. I saw one shook out head and I did not see mill scale.

JUDGE BRENNER: I was asking about general knowledge beyond these particular blocks as to whether or not mill scale forms even with the process you described of putting sand, a special sand on top of the mold.

WITNESS ANDERSON: Yes, it can, yes.

JUDGE BRENNER: It can or it is common for it to occur?

WITNESS ANDERSON: I would almost go to say it's common. I think the sand is a makeshift -- even though it is special I think it is makeshift and at best modestly effective.

WRBagb

BY MR. ELLIS:

Q Dr. Anderson, you mentioned in one of your answers just a moment ago pistons. They're steel, not cast iron, aren't they?

A (Witness Anderson) I thought I said heads, I meant to say heads.

Q I'm sorry.

WRBbrb

Looking at page 6 of the supplemental testimony, Dr. Rau, let me cover one point that has been touched on.

There Dr. Anderson states that, in the paragraph beginning "Third" -- do you see that, sir?

A (Witness Rau) Yes.

Q Dr. Anderson states that a normal hot tear configuration would have a more uniformly V-shaped configuration and that, in the ordinary course of events, an oxide formed during the cooling process would have been removed in the upper area of the crack where the grinding took place, but the crack surface from which the weld had separated had a uniform layer of the dark substance from the top to the bottom of the crack.

Do you agree with the conclusions in that paragraph?

A Well, I don't understand, quite frankly, everything that is said there. I could try to take it piece by piece, but there is a lot of ambiguity in what is meant by --

Q Well, let's do that, then.

Let me begin by, perhaps, accepting a suggestion from a colleague.

Dr. Anderson, would you explain what you meant by that paragraph?

A (Dr. Anderson) The paragraph, the portion of the

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paragraph that talks about the variance in the layer being thicker at the mouth than at the base?

Q The paragraph that begins with "Third," on page 6 of the supplemental testimony.

A That begins with "Third"?

Q Yes.

A Okay.

As I have previously mentioned, the procedures are to grind out the crack to essentially solid substrate. Beyond the shape of the crack and how it seems to fit into a river indicates that there was some grinding that occurred. The welding was then laid into that area that had been ground out.

Now --

Q May I interrupt you just a moment, please, Dr. Anderson?

A Yes.

Q Did you assume in that paragraph that the entire fabrication crack had been ground out?

A I had no evidence to assume that. The only things that I could take into account is that the procedures that I was told that TDI used when I was there for a visit was that they ground out the crack.

And the other thing was that the standard procedures for weld repair, the recommended procedures in,

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essentially, the welding handbook is to grind out the entire crack.

Q Yes. I understand.

But the point I was getting at was: does your paragraph that begins with "Third", does that assume that the entire weld was ground out -- that is, that the entire crack was ground out? I'm sorry.

A I'm not sure I'm making that assumption. I'm saying that it was ground to the best of their ability to grind it. They may have thought that they had ground to a substrate and ended the crack, terminated the crack, or they could not tell. I'm not sure.

But I'm saying that any oxide formed in the upper portion initially, at the time this hot tear occurred, would have been removed, so that there would be a different material present in the upper portion after that repair had taken place.

Q Dr. Rau, does that clarify matters for you?

A (Witness Rau) Well, somewhat. I mean, surely when they grind out the cavity in the surface of the can gallery, whatever portion of the casting shrinkage crack was there is ground away. So I would agree it's different material. But it's nonexistent, so it's not a matter of grinding off any thick dark oxide. The whole crack and everything with it has been gouged out in preparation for

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the repair weld, as shown in LILCO Exhibit B-61 in the middle of the three sketches.

Q When Dr. Anderson says a normal hot tear configuration would have a more uniformly V-shaped configuration: do you agree with that?

A To the extent that a normal V-shaped configuration means a crack as I've illustrated in the upper sketch of LILCO B-61, I agree you get cracks. But I don't think that they're normally a uniform "V"; and, in fact, I think more often than not when you have shrinkage cracks you end up with multiple parallel cracks as a result of that shrinkage.

So I don't think I would agree that it's a uniform "V", but I would agree that there's cracks. And to the extent that he means that a crack is V-shaped, in a very qualitative sense I guess I'd agree with that.

MR. ELLIS: Unless another member of the panel has reason to comment, I'm going to go on to another subject.

JUDGE BRENNER: I wanted to ask Dr. Bush: Dr. Anderson is apparently stating in the part of his testimony that Mr. Ellis has been inquiring about just now that he does not believe that the cracks below the weld material were formed during the casting process because, if they had been, they would be more uniformly V-shaped in

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configuration.

Do you have an opinion on that one way or the other?

WITNESS BUSH: If there is a single crack, and considering the temperatures that form -- they will tend to open up more and they're more visible than other types of cracks; and in that respect I would tend to agree on there. That's about as far as I guess I would go under the circumstances.

JUDGE BRENNER: Well, are the shapes of the cracks in the old 103 block beneath the weld indicative of the fact that they did not propagate during the casting and cooling process?

WITNESS BUSH: No.

I would consider that what I can see in the old 103 block would be something that would be related to the casting and cooling process. This is unrelated to the portion where the weld is. This is simply going below the weld and examining it from the point down.

JUDGE BRENNER: Dr. Anderson, have I been interpreting your views correctly in my questions to Dr. Bush?

WITNESS ANDERSON: Yes. You've done very well.

But the point is that with a hot tear, what you're trying to do is really stretch the material some

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significant amount and, therefore, if it's a single crack you would tend to open it up more than I see here. If there are multiple cracks, then that's one way we can add all those cracks up and see that we are getting the necessary stretch.

JUDGE BRENNER: Aren't there multiple cracks?

WITNESS ANDERSON: Yes.

They run down the bottom. They branch, and do change.

JUDGE BRENNER: Let me make sure I understand you.

Since -- if there are multiple cracks, then why did you state in your written supplemental testimony that you would expect them to have a uniformly V-shaped configuration if they had formed during the casting and cooling process?

WITNESS ANDERSON: The reference material I looked at that showed hot tears shows that in multiple cracks there is considerable separation. There's a considerable amount of space and wedging and separating, which I best explained as V-shaped, in order to accomodate the shrinkage of the metal.

JUDGE BRENNER: Well, aren't the cracks that occurred more or less V-shaped? I'm not sure if your emphasis is on a general V-shape or on uniformity, or --

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WITNESS ANDERSON: No.

I looked at these and saw that, in principle, they are fairly tight. And when I compared them to photographs in the literature, I could not make the statement that they were tight. They seemed to be much more open, much more spacey.

JUDGE BRENNER: The layman's impression, on my part, would be that if you had fabrication cracks, that because of the stresses and the strength of the material present, propagated larger distances than those would -- might tend to be more open and therefore more V-shaped; but if you had a situation where the stresses, as compared to the strength of the material, were not as great, and therefore the cracks did not propagate as much, that such cracks would tend to be less of a uniform V-shape. But nevertheless, both types of cracks in both instances could still have occurred during, or as part of, a hot tear or shrinkage process during fabrication.

WITNESS ANDERSON: It would depend somewhat on the dimensions. If we have a large constrained dimension, a large circle where there has to be a tremendous amount of movement of the material, then I would expect very large, open cracks. If it's a smaller section, and that area really is not adjusting much by its shrinking and stresses in the hot tear, then that would be very small and enclosed.

WRBbrb

JUDGE BRENNER: Well, what do you think the situation would tend to be in terms of what we're discussing here -- that is, the cam gallery area?

WITNESS ANDERSON: Having hefted the section of it, and seeing that it's fairly massive, I would expect to see some change in dimension that would be significant. As Dr. Rau mentioned, there's a fairly large fraction of an inch per foot of material, and we're talking about many inches of material here; and so therefore we would expect to see the need to take into account that type of opening in the crack.

I may -- if I have a minute, I may have a picture of a normal crack which shows that.

(Pause.)

JUDGE BRENNER: We can let you come back to it, if you want --

WITNESS ANDERSON: Okay.

JUDGE BRENNER: -- and that'll give you a chance to consult with your counsel, in case he wants to do something with it also.

WITNESS ANDERSON: Good. Thank you.

JUDGE BRENNER: Dr. Bush, did you want to follow up here?

WITNESS BUSH: I would only comment that, in such instances, I'm virtually certain that these cracks are the

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type that are visual to the casual eye.

In other words, when you shake them out, shake out the sand and grit blast to get rid of any oxide that might be there, it isn't something that you have to do a penetrant test examination such as we've seen. You can look at them; they're pretty visible. And it's strongly indicative of that in the fact that every one of these had it in three blocks. In other words, I don't think anybody had to look very hard to establish that there were cracks in these particular regions.

And that's fairly typical of this, whether they are multi-cracks or whether they are a single crack that opens up, it's usually you don't have to look very hard to see them.

JUDGE BRENNER: Dr. Rau?

WITNESS RAU: If I may add something, I think there are two other things that are relevant to this conversation that haven't come up.

The first is you must recall we're dealing with, in the original 103, degenerate Widmanstaetten graphite structure. And it's, I think, clear that the degenerate Widmanstaetten graphite structure was present at the time that the shrinkage cracks formed, which means they're going to form at lower amounts of stretch or stress than they would in the typical gray cast iron.

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The other thing which should be kept in mind is that the coefficient of thermal expansion is such that if you calculate the amount of strain that would be generated by a fully constrained cam gallery -- in other words, the cam gallery region is held firm, and it's just allowed to shrink, from 1500 degrees Farenheit down to room temperature, you would calculate strains in the order of one percent.

So we're only talking about a crack opening, even at the surface of the cam gallery, if it were a single crack, of one percent over, say, the one inch. It's not going to become a big, gaping, open tear, based on those amounts of strains. And to the extent there are multiple tears parallel to each other, each one of those will have a portion of the total.

So it's not really a fair representation to think in terms of a big, wide, gaping-open shrinkage crack, because that's not what happens. There's not enough differential thermal expansion to make it gaping; it's just going to be a crack.

JUDGE BRENNER: Any last words on that subject, Drs. Anderson or Bush?

WITNESS ANDERSON: I think it has all been said.

MR. ELLIS: Would this be an appropriate time, Judge Brenner?

WRBbrb

JUDGE BRENNER: Yes. How much more do you have on cam gallery?

MR. ELLIS: I'm going to try to use the break to cut two sections, and then I will have two and a half areas and I'll be done with those. The remainder is not, of course, nearly as extensive as cam galleries.

JUDGE BRENNER: All right. Let's take a break until 3:30.

(Recess.)

AGBeb

JUDGE BRENNER: On the record.

Mr. Ellis, you may continue.

MR. PERLIS: Excuse me, Judge Brenner. One preliminary matter.

We have been in contact with Larry Van Fleet out in Richland, Washington. We read to him the report that we got from LILCO this afternoon. It is a standard form. His conclusion is now final that the pistons are acceptable. I can go into some detail if you'd like as to what he is basing his conclusion on.

JUDGE BRENNER: How about a little bit of detail, but not too much.

MR. PERLIS: Basically the LILCO report -- and he had a similar form in hand to follow the report as it was being read to him. As I say, it is a standard form. The acceptance criteria used are also a certain standard. They were well-defined and appropriate for the exams.

As I said earlier, he was familiar with the procedures and the inspection materials and had already found them acceptable. He had met the examiner and had already approved his qualifications and adjudged him to be proficient.

Basically te report just provides the written comments that he was looking for.

JUDGE BRENNER: There was a question of the

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interpretation of the indications that you mentioned earlier.

MR. PERLIS: That's correct. And LILCO, in the report, has concluded that there were no unacceptable indications.

JUDGE BRENNER: Maybe I misunderstood. I thought that you knew that all along, but that what the Staff person who needed to know didn't know was what the meant.

MR. PERLIS: If I said that earlier I misspoke. What the Staff was waiting for was for LILCO to certify their view of the results. The Staff had already reached their preliminary conclusions, and those turn out to be the same as what are now LILCO's verified results.

JUDGE BRENNER: All right.

Now are the LILCO results going to be written up in any form other than this apparent one page that the Staff has that we don't have?

MR. ELLIS: I don't know, Judge Brenner, not to my knowledge.

JUDGE BRENNER: All right.

Well, we have approved the settlement. We will that approval now.

The Staff's written report will be put together as soon as feasible I'm sure, and the Board and parties will receive that. And as I stated earlier, if there is any

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basis in those reports for any party to bring anything to our attention, they are free to do that. Otherwise, the matter is settled as of this time.

MR. PERLIS: Judge, the Staff had originally been planning to issue one report based on all the diesel inspections for all the different areas. We can issue a separate report for pistons if you desire. We were not planning on doing that.

JUDGE BRENNER: I think that would be better, so we don't have to wait for a possibly extended time frame, if some problem in somebody's mind crops up.

MR. PERLIS: That's fine.

JUDGE BRENNER: If there is a technical reason why things should be interrelated so that to do so would detract from the substance, I wouldn't ask you to do it.

MR. PERLIS: No, we can issue an earlier report just on the pistons.

JUDGE BRENNER: Fine.

Perhaps we can get that one page for our use at some point. It doesn't have to be right now, the LILCO report of the inspection that Mr. Perlis has been referring to.

All right, at this time you may continue, Mr. Ellis.

MR. ELLIS: Thank you, Judge Brenner.

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JUDGE BRENNER: Did you want to give us an estimate of what you have, assuming fewer interruptions by me?

MR. ELLIS: Yes, sir. The break was productive, and I certainly expect to be finished before the day is over, and perhaps in an hour.

BY MR. ELLIS:

Q Dr. Anderson, I think I am correct that you have already testified that the degenerate Widmanstaetten graphite is already present when the casting shrinkage cracks formed. Is that correct?

A (Witness Anderson) Yes.

Q Wouldn't that fact lead you to expect that the cracks in the 101 and the 102 blocks would be shallower than the cracks in the old 103 block?

A On the surface, if we accept the characterization that 101 and 102 are completely free from any degenerate stucture in all locations, if we accept that premise, then we would expect them to be shallower.

Q Dr. Rau, do you agree with that conclusion that you would expect the cracks to be shallower on 102 and 101 based on the microstructure?

A (Witness Rau) Yes, Mr. Ellis, based on the degenerate Widmanstaetten graphite, I expect, given the similarity of the molds and the castings and the extensive

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times required for the cooling, that the shrinkage stresses and strains would be comparable in the original 103, 101 and 102.

And due to the severely degraded mechanical properties, and tensile strength in particular, of the original EDG 103 block, I would expect the cam gallery casting shrinkage cracks to have been substantially shallower than those in the original 103.

Q Dr. Anderson, I will come back in a moment, so that you don't become apprehensive, to the assumptions that you made in giving your answer, but let me proceed now for just a moment.

Dr. Rau, is there in fact any empirical evidence to support that the -- the prediction that the 101 and 102 blocks would have shallower cracks?

A Yes, Mr. Ellis. There have been both surface inspection, that is LP, liquid penetrant inspections, and also a TSI depth gage measurement of the cam gallery regions in 101 compared to the original 103.

The surface indications indicate that in general the cam gallery indications in the vicinity of the surface of the repair weld in 101 and 102 are smaller in length and finer -- that is, less bleedout from the LP, indicating at least qualitatively shallower indications.

But perhaps more directly relevant is the TSI

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depth gage measurements which indicate substantially shallower indications in 101 than in the original EDG 103. In particular, the TSI depth gage quite accurately predicted the crack depths that were subsequently determined by destructive examination in the original 103.

And that same TSI depth gage indicated a maximum crack depth in 101 of .16 inch compared to those depths of -- the deepest cracks -- excuse me -- of .91 in the original 103. And the majority of the many different TSI depth gage measurements in 101 made were less than .1 inch deep.

So in general the depth of the indications in 101 were very significantly shallower than those in the original 103. And that's consistent with the difference in mechanical properties.

Q Is that evidence the basis for the drawing that you made that has been marked as -- is it LILCO Exhibit B-62?

A Yes, it is, Mr. Ellis.

Q I take it in addition to those measurements also-- Well, strike that.

Dr. Bush, would you also-- First of all, I take it you would also agree that the degenerate Widmanstaetten graphite was present at the time that the casting fabrication cracks were formed.

A (Witness Bush) Yes, I would expect it to be.

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Q And would that fact lead you to expect that, as a realistic matter, the cracks in the cam gallery of the original 103 block would be deeper than the cracks in the 101 and 102 blocks?

A Not necessarily, on that basis.

As I indicated earlier, there are other factors that play a role. Given that-- Using the same mold and you're pouring from the same degree of superheat and all other things being equal, I would say yes, I would expect the cracks to be deeper.

I have no way of proving that to be the case and therefore, in my estimation, it is not a closed subject.

Q Dr. Bush, would you agree that the evidence of the depth measurements, the TSI depth gage measurements does, however, support the prediction that Dr. Rau makes that the cracks would be shallower in the 101 and the 102?

A I suspect that to be the case, but unfortunately, I am not a firm believer in the reliability of this type of depth gage in all conditions.

I recognize that it has been calibrated against a real crack. I suspect that indeed we have substantially shallower cracks, but I have no way of proving it conclusively. In other words, we're depending on an instrument to keep telling us the same thing, and any degree of bridging, et cetera, might give us false results.

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Q Dr. Rau or Dr. Wachob, would you address, if you would, please, the point that Dr. Bush just raised concerning the reliability of the measurements. I realize you have already testified-- Let me put it this way:

In addition to the fact that the TSI depth gage measurements on the original 103 block were confirmed by the sectioning, what other bases do you rely on in support of your conclusion that the depth gage measurements on the 101 and the 102 are deserving of reliance?

A (Witness Rau) Well, what Dr. Bush has said is generally true in the sense that you can't be 100 percent sure that there might not be some bridging and the cracks might be deeper than that which is indicated by the TSI depth gage.

However, this depth gage measurement was not simply made on a single crack indication in the 103 cam gallery. I don't remember the exact number but as I recall, each of the nine different cam galleries were examined with the TSI depth gage and multiple indications on each cam gallery, to the extent there were multiple indications, were also interrogated.

And again, my recollection is that they were certainly in excess of 20 linear indications in the cam gallery of 101, each one of which was evaluated with the TSI depth gage.

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Although I would agree with Dr. Bush that in any one of those there might be some bridging and therefore not be completely accurate with regard to crack depth, I don't believe it is reasonable to expect that all 20 of them would suffer from the same sort of problem. And on average I believe, since they are all .16 or below, and in fact most of them are less than .10, I believe them all to be associated with the weld shrinkage as indicated on the bottom of the three sketches on LILCO Exhibit B-62.

I think I should add also that the comparison of the liquid penetrant inspection and the depth gage -- TSI depth gage measurements is informative because the liquid penetrant inspections show clearly that the cam gallery indications in 101 are in fact located at the edge of the repair weld.

Now clearly since there is an indication at the edge of the repair weld, the TSI depth gage is very likely to be reading the depth of that particular welding shrinkage crack which occurs on shrinkage of the repair weld after it is made.

I think when you put the two together, the significant number of depth gage measurements and the combination with the LP inspection results, I believe there is very strong evidence that the cam gallery indications are significantly shallower in 101 than they are in 103, the

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original 103 block.

Q Dr. Bush, would you then agree with Dr. Rau that while one can't be 100 percent sure, there is substantial evidence that the cracks are shallower?

A (Witness Bush) As I think I've indicated, I suspect they are but I just don't consider the evidence as totally conclusive. And therefore, I am simply taking a somewhat conservative viewpoint that we haven't unequivocally established that this is the case.

Certainly the evidence collectively tends to point to this, and the more measurements one makes, what I call the margin -- the less the margin for error under those circumstances. But as I say, I am not prepared to sign off because I have unfortunately been burned sufficiently badly on non-destructive examination measurements of other types that I do not accept anything without validation.

Q Dr. Anderson, do you wish to comment on this particular area?

A (Witness Anderson) Well, I think there's two points.

I do agree that they are probably not as deep. I think that there should be monitoring by the techniques that they've described to see whether there is a change over a period of time related to the running of 101 and 102.

My other comment is in looking at the drawing

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which represents 101 and 102, there is an indication on the lower picture that it is a weld shrinkage crack. There is no non-destructive procedures that could identify unequivocally a weld shrinkage crack that I'm aware of. In other words, the procedures can't identify that, whether it is or is not.

Q Dr. Rau, as I understand Dr. Anderson's remarks just now, he says that the weld shrinkage crack that you have labeled on LILCO Exhibit B-62, that there is no non-destructive examination that can verify that that's a weld shrinkage crack.

Can you comment on that?

A (Witness Rau) I would agree that if you had no knowledge of the location of the weld or even of its existence and simply had a non-destructive inspection indication of depth that you could not, based on that indication alone, conclude that it is a weld shrinkage crack.

And I guess I would have to agree that you can't be 100 percent sure right now that it's a weld shrinkage crack. But I would submit that the location, based on the liquid penetrant inspection at the surface which shows in fact that that crack indication is located right beyond the termination of the repair weld, is very strong circumstantial evidence that it is in fact a weld shrinkage

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crack.

If in fact there were casting shrinkage cracks in the area and they were ground out in preparation for weld repair, it would not be reasonable to grind adjacent to the indications and leave them intact. I mean the intent would be to grind out the indications, at least to center your grind on it. And again I believe the inference would be from the location of the crack as recorded by LP that it is in fact a weld shrinkage crack.

Q Do you have any comment on that, Dr. Anderson?

A (Witness Anderson) No.

JUDGE MORRIS: I have a couple of points, Dr. Rau.

Your middle sketch on B-62 seems to indicate that whatever defects were there originally were totally ground out. Was that your intent in the drawing?

WITNESS RAU: Yes, Judge Morris, it was.

JUDGE MORRIS: Is there evidence to show that that was in fact true?

WITNESS RAU: There is only indirect evidence, Judge Morris, the evidence being that the TSI depth gage indicates that the depths of most of the indications are less than .1 of an inch, .1 inch deep, and the deepest of any in 101 was .16.

Because the cam galleries of 101 and 102 haven't

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been cut up, I don't know precisely how deep the wells are, but I have examined how wide they are, and they are generally slightly smaller than the width of the welds on the original 103. They're, you know, half inch to three-quarters of an inch.

But because they are slightly smaller in width I would suspect they are slightly shallower in depth, which would put them in the quarter inch to three-eighths inch depth. Since that is considerably larger than .1 or .16, which is the TSI depth gage, the inference would be that the crack depths recorded are shorter than the depth of the weld.

And that's why I infer and have constructed this schematic to illustrate my belief that the shallower casting shrinkage cracks have been removed by the gouging and the subsequent weld repair and that what remains at the edge, or if you like, beyond the heat-affected zone of the repair weld is in fact the secondary weld shrinkage type of crack that was also observed in the original 103.

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WITNESS BUSH: May I supplement that, Judge Morris?

JUDGE MORRIS: Well I wanted to follow up but I will certainly give you an opportunity.

If there were defects there to begin with, wouldn't they have been casting shrinkage type cracks?

WITNESS RAU: Yes, sir.

JUDGE MORRIS: So what is the relationship of the weld shrinkage crack dimension to that original casting shrinkage crack dimension?

WITNESS RAU: None whatsoever, Judge Morris.

The weld shrinkage crack is the one which I believe has been measured by the TSI depth gage; and the original casting shrinkage cracks, which have been removed -- or, perhaps, not removed -- there's no direct relationship between them.

The only evidence we have is that in the original 103, when the original casting shrinkage crack was not removed by the grinding, that in fact the weld shrinkage crack came around and grew all the way around into contact with it, so that when the TSI depth gage was put on it, it measured the full depth down to the tip of the casting shrinkage crack.

That was not -- since the max depth here, again, is 0.1, or 0.16, and the welds appear to be deeper than

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that, it appears to me that they are completely ground out.

JUDGE MORRIS: Do you want to follow up on that, Dr. Bush?

WITNESS BUSH: Yes.

That's one reason I have taken a conservative viewpoint. If you take the middle diagram of B-62, and let's postulate that indeed they didn't grind everything out and there's something of -- maybe not of substantial depth, but of finite depth: if the weld shrinkage crack that is shown in the bottom diagram does not contact this hypothetical crack, we have a bridge there. And the depth gage will not detect that, and neither will the penetrant test.

I am not saying that this is the case. I'm simply stating that we have no evidence that tells us that it is not the case either. So it's kind of an open issue, one might say, under the circumstances; and, in fact, that is one of the reasons for putting crack opening displacement gages on in this area, because if indeed there was limited pop in there and there were such a crack, it should be readily detectible, I would think, by the COD.

JUDGE MORRIS: Any further comment, Dr. Rau?

WITNESS RAU: Yes. Let me just add one thing.

I would agree completely with Dr. Bush: that's a possibility. And I have indicated it's not my belief that

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that's the case.

But let me add one other thing: even if that were the case -- that is, if in fact there were some of the original casting shrinkage crack which had not been gouged out before the weld repair was placed in: if in fact the weld shrinkage crack which formed did not link up with it, then what we have is sound material in between the end of the original casting crack and whatever the weld shrinkage crack is.

And from the fracture mechanics point of view, that makes a significant difference with regard to structural integrity. And I think, either way, it's substantially less serious than the depth of the crack thta was present in the original 103.

More specifically, if there's sound material out toward the surface where the repair weld is, then the effective crack depth is not the entirety of the original casting shrinkage crack, but only half of it. I don't want to go into details, but in point of fact the effective crack driving force is reduced by a factor of two if there's sound material on the outside; and in either case it would be a substantially less -- or, let's say, a larger margin against any additional propagation than would be the case in 103 where it definitely was surface-connected.

JUDGE MORRIS: Can you tell me very briefly what

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a TSI gage does, and how it operates?

WITNESS RAU: It will be very brief.

It is basically an electrical measurement. My recollection is it's a constant -- it's either constant amperage or constant voltage; but in point of fact you put probes across the crack indication on the surface, and it imposes an electrical current which has to run around the full depth of the crack indication. And it adjusts either the voltage or the amperage to keep one of them constant, and measures the other. And by calibration with known indications, you get the path which the electrical current must traverse -- or the corresponding voltage -- and from that you infer the crack depth.

JUDGE MORRIS: Do you agree, Dr. Bush?

WITNESS BUSH: Yes.

JUDGE MORRIS: Given a crack of a depth of, say, 0.16 inches in this material, how accurately do you think a TSI gage could determine that crack?

WITNESS RAU: Well, you're looking at a non-expert on the TSI depth gage. I believe Dr. Johnson of our Staff has testified that he believes it would be accurate within, I think, ten percent. But I don't recall. He said something on the record, and I can't recall the number, and I'm really not in a position to be any more specific than that.

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JUDGE MORRIS: Does that sound reasonable, Dr. Bush?

WITNESS BUSH: When you say a statement like that, you have to say ten percent of what, because -- that's why I'm always reluctant: if it's ten percent of one inch, that's a tenth of an inch, which is what I would believe. If it's ten percent of a tenth of an inch, I wouldn't believe it.

JUDGE MORRIS: Well, I set the condition of 0.16 inches in depth, and I'll make it any width you like.

WITNESS BUSH: I wouldn't believe that. Ten percent, I think, might be optimistic for a short crack, yes.

JUDGE MORRIS: Well, what would be pessimistic?

WITNESS BUSH: I would expect the error, on a short crack, to be as much as the apparent depth of the crack. If I had a tenth-inch crack, I wouldn't be too surprised to see an error, you know -- in other words, two-tenths wouldn't surprise me one iota under those circumstances, because of the way the measurements are made.

But that's still not a very big -- you know, it sounds like a big absolute error, but in fact it isn't a very significant one.

WITNESS RAU: I'm going to have to take a little bit of exception to Dr. Bush's statement.

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I'm not an expert in the TSI depth gage, but I have done an awful lot of experimentation in fracture mechanics, and one of the techniques we use in fracture mechanics to measure the size of a crack is to put electrical potential measurement above and below the crack and measure the length of the crack by the change in the voltage.

I know that those measurements are accurate within 50 mils with crack sizes of the dimensions we're talking about here. Granted, that's not as complicated a geometry as running along a shrinkage crack, but I personally don't believe you'd be off by a quarter of an inch trying to measure a 0.16 crack with this technique. I mean, you might be off more than ten percent of 0.16, but I don't believe it would be 200 percent.

JUDGE MORRIS: That tells me all I want to know, unless --

(Laughter.)

WITNESS BUSH: I didn't say what he said I said; that's the only problem.

JUDGE MORRIS: Thank you.

JUDGE BRENNER: I've got a few questions on this same point, unless you wanted to follow up on the same point, Mr. Ellis.

MR. ELLIS: If I may for just a moment; it may not be the same thing, but it may help.

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BY MR. ELLIS:

Q Dr. Rau, even given this bridging effect that Dr. Bush said was possible, would you think that it is significant nonetheless that many measurements were taken which were all on the same order of magnitude?

MR. DYNNER: I am going to object. There's no evidence that there were many measurements all of the same magnitude. If he wants to make that "if," I have no objection.

JUDGE BRENNER: I don't remember whether we have testimony on it or not. Why don't you just ask him directly as a foundation question and then proceed to your next one?

MR. ELLIS: Yes, sir, I will.

BY MR. ELLIS:

Q Dr. Rau, there were measurements on the 101 cam gallery by depth gauge measurements that you have testified to. What were the results of those?

A (Witness Rau) I don't recall all of the numbers but as I have indicated each of the nine cam galleries in 101 were measured with the TSI depth gauge. There were multiple indications on many of those cam galleries and again my recollection there were more than 20 such indications, the depth of which was evaluated with a TSI depth gauge. The deepest indication was .16, my recollection is there were no other indications amongst

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the 20-plus that were in excess of .1 inch. Most of them were considerably below that.

Q My question then is given the range of measurements that you observed, does that give you any confidence with respect to the briding effect that Dr. Bush alluded to played a role in making the measurements reliable or unreliable?

A Yes, I tried to indicate that previously. The statistics are such that the more of those you do the more chance you have of finding one in which there is no bridging, if you like, or eliminating the one where there might be some bridging from biasing your depth measurements.

I think the total conclusion though which I have reached is based not only on the number of measurements, that's one of the factors but it is also based upon the known differences in the mechanical properties of the original 103 compared to 101.

My belief that the stresses generated by the casting process should be similar given the very long time and the same molds and, as far as anybody knows, the same casting conditions and my knowledge that the fracture strength and the corresponding fracture strain is very substantially lower in the original 103 leads me to believe that the cracks must be very substantially shorter.

And put all of those together and it is my

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opinion that in fact the large number of measurements are indicative that we do not have original casting cracks extending beneath the weld.

Q Well Dr. Rau, assume that the cam gallery cracks in 101 and 102 are deeper than you have depicted on LILCO Exhibit B-62 and in fact are a deep as the 103 original block cracks that were found. Does that affect FaAA's conclusions concerning the cam gallery cracks?

A Definitely no, those cracks are not going to propagate even if they are as long or as deep as the cracks in the original 103 and, as I testified earlier today, the extensive operating experience with the original 103, which surely has the markedly inferior mechanical properties resulting from the degenerate Widmanstaetten graphite, have clearly demonstrated that even with very deep cam gallery indications the cracks are not going to extend. And even the same depth crack, if it were to exist in 101, 102 or the replacement 103 would have even a larger margin against crack propagation.

MR. ELLIS: Judge Brenner, that's all I intended to follow up on.

JUDGE BRENNER: You have mentioned the 101 block. Remind me, what's the situation with respect to known cam gallery cracks on the 102 block, are they only on saddles 8 and 9? I just don't remember, I'm sorry.

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WITNESS RAU: My recollection is I have seen them on 8 and 9, perhaps on 1 also, I can't recall. But the inner cooler had not been removed at the time I looked at it in order to access the cam galleries in the middle of the engine and it is my understanding that there haven't been any depth gauge, TSI depth gauge measurements of 102 in those center cam gallery regions.

JUDGE BRENNER: On the 101 block -- I think this may have been in one of your earlier explanations this afternoon about ten minutes ago but, if so, I missed it -- do you believe that the measured, the TSI depth gauge measured weld shrinkage cracks are as deep as the weld itself in each case?

WITNESS RAU: No, Judge Brenner, I do not. The majority of those TSI depth gauge measurements are less than .1 and -- between 50 mils and 100 mils. And from the appearance, the external appearance of the welds I have estimated that the welds are probably more likely to be something like a quarter-inch which would be substantially deeper than the depth of the weld shrinkage cracks which have been indicated by the TSI depth gauge.

JUDGE BRENNER: On the original 103 block were the shrinkage cracks as deep as the weld itself in all cases?

WITNESS RAU: The weld shrinkage cracks, your

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Honor?

JUDGE BRENNER: Yes.

WITNESS RAU: Yes, your Honor, in all of the ones which we cut up the weld shrinkage cracks went all of the way around the perimeter of the repair weld and joined up, if you like, with the original casting shrinkage crack. And in fact the original TSI depth gauge measurements recorded the full extent indicating in fact that they had joined up.

JUDGE BRENNER: If the weld shrinkage cracks are -- what you call the weld shrinkage cracks are in fact that, why aren't those cracks down to the depth of the weld on the 101 block?

WITNESS RAU: Well your Honor, it's got to do, I believe, with the residual stress states that are generated during the repair weld process. If you look at the bottom of LILCO B-62 where I have indicated the repair weld in the area of the cam gallery, if there is no preheat in the repair weld process when that weld starts to shrink it will attempt to get smaller in all dimensions, that is, both vertically if you like and also horizontally it will attempt to get smaller. Now vertically it is held on because it is bonded to the cast iron both on the top and the left and the bottom. But obviously there is no metal on the right-hand side of that little weld bead.

So what that means is that you can support as

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it shrinks a tensile residual stress in the vertical direction, but you can't support a correspondingly large tensile stress in the horizontal direction because there is a free surface there and it just shrinks without being constrained or held and therefore it doesn't generate as high a tensile residual stress in the horizontal direction which would be necessary to continue to the extension of the repair weld shrinkage crack all of the way around or halfway around the weld bead. And I think that's why we see the weld shrinkage cracks at the bottom and they extend until they essentially run a distance where the stresses drop off to a level below the strength of the typical gray cast iron and then they stop.

JUDGE BRENNER: Well what was different between the 103 and 101 block to cause the difference in the, what you call the weld shrinkage cracks in one case going down to the depth of the weld and in the other case not doing that?

WITNESS RAU: The difference is the degenerate Widmanstaetten graphite which reduces the strength of that cast iron in that vicinity, the strength, by a factor of, you know, something like two, 100 percent lower and, even more important, the fracture strain by more like a factor of three.

So we're dealing -- the horizontal stresses in the original 103 would only have to be one-third as high to

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cause the crack to extend all of the way around to the casting shrinkage crack in that particular material, they would have to be three times as high -- the strains would have to be three times as large in order to produce that same crack in typical minimum properties Class 40 gray iron. I think that's the reason why it happened in the old 103 and has not happened in my opinion in 101 or 102, those two.

JUDGE BRENNER: Did LILCO or FaAA know there were what you call casting shrinkage cracks in the original 103 block before the destructive examination of that block?

WITNESS RAU: Yes, your Honor, as I testified weeks ago now when we attempted to verify after our original testimony that the grind-out mark was in fact the maximum extent of the cracks in the old 103 and couldn't find that documentation and therefore I established the cut-up procedure to establish definitively what the depth of those cracks were, the liquid penetrant inspections which were done at that time, before any cut-ups were made -- the paint was taken off and the liquid penetrant was done. That then identified the presence of the repair welds and the presence of these weld shrinkage cracks at the edge of those repair welds before -- well just immediately before we started with the cut-ups.

JUDGE BRENNER: All right. Thank you.

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Dr. Anderson and then Dr. Bush, do each of you in turn want to comment?

WITNESS ANDERSON: I only have one comment:

Without doing appropriate destructive sectioning there is no way of analyzing the weld on 101 and 102 with respect to preheat, postheat. We cant -- There is just no basis for saying it was done the same as 103. You would have to look at it as 103 was looked at.

JUDGE BRENNER: What do you think of Dr. Rau's view that even if there were what he believes to be casting shrinkage cracks below the weld repairs in the 101 block but not detected due to a bridging, if you will, of there being no connection between the shrinkage -- what he believes is a shrinkage crack, a weld shrinkage crack and what he believes is a casting shrinkage crack, that then that's not a problem because you have sound metal there which would counteract any further postulated propagation?

WITNESS ANDERSON: Sound metal with cracks on both ends of it, I don't think I would accept that. I think that after a period of time that sound metal with cracks running at both ends would probably join the cracks.

JUDGE BRENNER: Even though they have not done so yet with the operational history?

WITNESS ANDERSON: Apparently have not done so, at least have not opened up enough to be detected.

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JUDGE BRENNER: Dr. Bush, I would appreciate your comment on that last point and also anything else you want to add.

WITNESS BUSH: I think there is a finite possibility if we assume two cracks that are separated by sound metal if the sound metal isn't an extensive length that they may link up. One thing where it might occur is when you unbolt, if indeed you still have some residual stresses -- and I'm not saying how much they would have to be -- I could visualize that that would occur. I can't say that I am very concerned because I think the important item is not necessarily where we have that one but whether we consider that the crack, even that this longer crack, would continue to grow into the material; in other words, is there enough crack driving force. I don't believe there is personally.

WITNESS RAU: Let me just make one quick statement because I heard Dr. Bush make the statement here today, I didn't understand it then and I guess I still don't, with regard to the unbolting.

When the casting is made and the weld repairs are made there is no bolting, the bolting comes subsequently and would superimpose the compressive stresses on the cam gallery region. If you then subsequently unbolt it seems to me you are no worse off than you were before you started,

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and I can't see any driving force for anything to happen after unbolting that didn't happen before it was bolted.

WITNESS BUSH: I agree assuming that there is a sufficiently long crack that indeed you have relieved the residual stresses below a certain level. That's the moot point so far as I'm concerned, have we sufficiently relieved the residual stresses. I suspect we have but I can't prove it. I would consider the unbolted condition as being more conducive to crack growth than would be the bolted condition because of the superimposed compressive stresses and therefore I can't unequivocally say yes it has grown as far as it will and there will be no further movement. So I am left with the dilemma of not being able to sign off on the thing.

As I indicate the big concern, of course, is whether, even if you postulate this crack and it grows whether it will continue to grow through the material, that is really the critical item and no so much as to whether we break a ligament between two cracks. At least that's my personal opinion.

BY MR. ELLIS:

Q Dr. Anderson, I want to come back to the original question I asked you and as I recall you said, in agreeing with the statement that if the degenerate Widmanstaetten graphite was present to a greater extent in old diesel

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generator 103, my question is do you have any evidence that shows that the degenerate Widmanstaetten graphite was not present in greater amounts in diesel generator 103 than in 101 and 102?

A (Witness Anderson) Well I think 103 has been very thoroughly characterized. I think that a considerable amount of high quality work has gone into characterizing 103, old 103. I think that the amount of work in the 101 and 102 is very modest and I am not aware of any examples that have been taken out in the cam gallery area to compare the metallography at that location which is what we are really speaking of here. I believe that the amount of material in 101 and 102 has been taken from other areas and again it represents only a small fraction of what is actually there. So I don't believe it is completely characterized.

Q Let me follow that up, Dr. Anderson:

On page one of your rebuttal testimony you state there that "...FaAA evaluated less than 100 grams of material from each block and each block weighs 24,000 pounds."

How did you determine that less than 100 grams of material from each block was evaluated?

A Well that was an estimate based upon Dr. Wachob's description of the samples that he took. The 24,000

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pounds comes from the drawings on TDI, they mention the finish cast weight.

Q How did you estimate the 100 grams?

A Again as I said there was a description by Dr. Wachob on the pieces that he had taken, dimensional, if I recall, and I estimated what those dimensions would give me in volume and hence in weight.

Q Do you recall how many samples made up the 100 grams that you estimated?

A Per block?

Q Yes.

A Not precisely. I have a feeling it was two plus transfers but I don't recall precisely.

Q In your view, Dr. Anderson, is it appropriate to judge the validity of the sample by comparing the weight of the sample to the weight of the whole?

A I think in this it kind of characterizes the value of the sample. I would think that more important would be the locations and, as I said, I am not aware of a sample that has been cut out from the cam gallery region which we have been focusing on.

Q So that I am clear I take it that you agree that comparing 100 grams to 24,000 pounds is not the best way to evaluate the validity of a sample?

A I believe I said that that -- it was indicative

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of the amount of sample that has been looked at and then I went on to say that I think the location is important and therefore I would expect a sample would be taken from the cam gallery and that that sample would have much more value on what we're talking about, the cam gallery cracks.

Q Dr. Rau, Dr. Wachob, would you comment on Dr. Anderson's opinion with respect to the 100 grams out of 24,000 pounds and whether that is an appropriate measure and also with respect to the locations that they were taken, the samples were taken from?

A (Witness Rau) Well I don't think that the weight of the sample has any relevance at all with regard to whether it is metallographically representative of the totality of the part. If, for example, you look at one metallographically polished section, you are looking at one plane and theoretically there is no weight whatsoever to one plane, it is basically zero compared to the weight of the entire structure. It just basically has no meaning whatsoever.

Surely the distribution of specimens and whether they are representative of the structure is a meaningful evaluation of the sampling procedure. We have looked at four separate locations in each of the 101, 102, original 103, as well as many other areas in the original 103. We have looked at the same location in each of those three

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blocks, two separate locations on the block top around cylinders 4 and 5, another material sample in the crotch area in the same general vicinity and a number of replicas on the order of 10 per block down on the end block top near cylinder 1.

In the case of the original 103, in addition to sampling those four areas and the multiple replicas in the one area, we also cut up extensively the block top in-between cylinders 6 and 7 and also in-between cylinders 4 and 5. We have also cut up, as you know, the cam gallery regions in cam gallery saddles 6 and 7 of the original 103.

We have demonstrated through a comparison of those sample location as well as other locations in the block top, of the web, in-between -- excuse me, coming down from the block top towards the cam saddle, and also in the cam gallery/cam saddle regions that the microstructures were comparable, that is, that the degenerate Widmanstaetten graphite when present in the block top of the original 103 was present in all locations of the block top which we examined, was present in the web area, was present in the cam gallery regions and that the sample regions we had taken were in fact consistent and representative of the entirety of the casting.

I have also indicated that given the size of this casting and the slowness of the cooling that I expect very

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similar cooling rates throughout the casting, not identical but similar. And for that reason I don't find any reason to suspect substantially different microstructures in different locations and I therefore believe that the sampling locations we have performed are representative of the microstructures of the block.

Q Dr. Wachob?

A (Witnes Wachob) I would like to add one thing, the critical component of the decision on where to sample was based on the cooling rate and the thick section technology and the thick section portions of the block. The important component to looking for Widmanstaetten grahite is looking at those areas which are very thick and cool very slowly.

I believe Dr. Anderson was talking yesterday that it only occurs on quick or rapid cooling, the formation of this Widmanstaetten graphite, and that is incorrect. Widmanstaetten graphite occurs only in very large castings which cool very slowly. So that the choice of a very thick section in a block which cools very slowly is what you want to look at when you want to look for Widmanstaetten graphite.

The comment that he made about meteors and fast cooling rates and this and that and that's where you get Widmanstaetten graphite is really with respect to

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Widmanstaetten ferrite which happens to be a navicular needle-like microstructure that occurs in iron, and it is not the Widmanstaetten graphite that we're talking about here.

Q Dr. Bush, did you have any comment that you wanted to add?

JUDGE BRENNER: Could I make one suggestion?

We have had some questions about Dr. Anderson's answer number one in his rebuttal testimony.

Dr. Wachob, I think that one of the specific points Dr. Anderson makes in support of his view that there should have been greater sampling is his characterization of your testimony in his last sentence of his answer. And to paraphrase it he says you found -- in his view what you said was that you found characteristics similar to Widmanstaetten graphite in the 102 block samples and therefore in finding at least that one example you should have had more extensive sampling.

Can you respond to that?

WITNESS WACHOB: Again we were looking at regions that are associated with the thick sections of the block tops. Those areas that we did examine did not show any extensive Widmanstaetten graphite, there were only very isolated, very remote regions that were associated with anything and we had a problem of even deciding if that was

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truly Widmanstaetten graphite that existed there.

In the 103 block where we see Widmanstaetten graphite, we see it everywhere to very extensive amounts and I believe that the areas that we have looked at on the 102 block are quite representative of the microstructure that is there and 99.99 percent of the micrographs that we see show that it is a typical gray Class 40 type iron. Dr. Rau may want to also add I think to that statement.

JUDGE BRENNER: Let me stay with you for a minute.

When you say what you found at the most may have been isolated examples of Widmanstaetten graphite and you even had difficulty deciding whether to so characterize it, do you mean isolated in the sense that only some samples had extensive Widmanstaetten graphite or do you mean that within each sample the indications you are talking about were isolated, within each sample where it occurred?

WITNESS WACHOB: There were no instances where extensive Widmanstaetten graphite occurred, it was only on one or two type locations within all of the specimens that we saw it in one very specific instance, not even all thoughout that specimen just in one particular spot on the structure.

And if we went back and -- if you look at Exhibit, I believe it is, 38 of our testimony....

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(Pause.)

....the photographs here are shown at comparable magnifications. And what we are trying to bring across is the fact that in each one of those there is what is called the utectic cell boundary. This last portion of the material, once it begins to solidfy and grows outward in a sphere, all these spheres eventually intersect and the last solidification process takes place at this boundary.

If we look at 101 and 102, you can see a very faint white circular region associated with those photographs and it is almost in the center of the photograph. That happens to be the boundary that one would typically anticipate seeing.

If you compare those two photographs with the 103 photograph on the right, the dark, black, blotchy lumps that are scattered all around the boundary are what we are referring to as the extensive Widmanstaetten graphite.

In 102 there are, as we said, regions that we can characterize distinctly as Widmanstaetten graphite, it is only a possibility that that's what it is. And you don't see any in these photographs that are of any substantial occurrence in the boundaries that would show the significant degradation and mechanical properties that that 103 photograph on the right very clearly demonstrates.

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JUDGE BRENNER: Dr. Rau, you wanted to add?

WITNESS RAU: I just wanted to make sure the record was clear. I don't believe Dr. Anderson's characterization of what we said was in fact what was in the record. What was in the record is in fact what Dr. Wachob has said, is that there were certain features or characteristics in the microstructure which had the same features as Widmanstaetten graphite.

We were not able, and I think that's what we testified, to in fact confirm that even those features were degenerate Widmanstaetten graphite. As as Dr. Wachob has very I think emphatically indicated, that those features, whatever they might be, occupy such a small fraction of the entirety of the cell walls and certainly the entirety of the microstructure as to have no significant impact on the mechanical properties.

JUDGE BRENNER: Both of you have just paraphrased what Dr. Wachob said at 24,755, the page after the page cited in the rebuttal testimony. But I wanted to make sure we had focused views here.

Mr. Ellis.

BY MR. ELLIS:

Q Dr. Anderson, you do not have, do you, any evidence, factual evidence or analyses to support an opinion that the gray cast iron in the 101 and 102 blocks have

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anything other than the predicted ultimate tensile strengths for a gray cast iron Number 40?

A (Witness Anderson) Well, with the previous reservation of adequately characterizing the block, I have seen no information that has anything other than Schedule 40 iron.

Q To follow up, Dr. Rau, on one-- Strike that.

Dr. Rau, as you know, the Staff has recommended strain or wire gaging to monitor crack behavior in the 101 and the 102 blocks.

Is it your opinion that such wire gaging is necessary or indicated?

A (Witness Rau) Yes, Mr. Ellis, that is precisely my opinion.

Q That it is required or indicated?

A No, that it is not required. I'm sorry.

Q All right.

Would you tell the Board your basis for that, please?

A Yes, sir.

There are a number of bases for that. The first would be the stress analyses I have performed, and the indication from those stress analyses that there are compressive stresses perpendicular to the crack indications in the cam gallery area.

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It is my belief that those analyses, which have been confirmed by the actual strain gage measurements on the surface of the new 103, have shown conclusively that there are in fact compressive stresses superimposed by the through-bolts clamping up and those compressive stresses are maintained during the entirety of operation of the engine.

In addition, the TSI depth gage measurements and my analysis of the differences in mechanical properties and its impact on the probable depths of the cracks in the original 101 and 102 indicate to me that the indications -- the depth of indications is substantially shallower than those that were present in the original 103, and that this increases the margin against any crack extension rather substantially above that which has already been demonstrated by the extensive operation of the original 103.

Which brings me to my third point, that the extensive operation of the original 103 which contained the extensive degnerate Widmanstaetten graphite, which very, very substantially reduces the fatigue and fracture properties of the gray cast iron, -- and I think that has been demonstrated in our exhibits, I think B-40, -42 and -44 -- the more than 1200 hours of operation, many of which have been at power levels of 35 and 39 hundred, the absence of any crack extension, even in this degenerate Widmanstaetten structure, suggests to me enormous margins, larger margins

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of safety in 101 and 102 and the replacement 103.

And for all those reasons, I believe personally it is not necessary to monitor the indications in 101 and 102.

Q Dr. Anderson, I want to give you an opportunity to comment on that now. But also in your answer or in your comment, would you also answer the question whether you agree with Dr. Bush that with respect to the cam gallery cracks, the blocks would be suitable provided they are monitored using this wire gaging?

A (Witness Anderson) Do I understand the thrust of the question is that the wire gaging would be continuous and would be left on, or would there just be a small amount of testing and then they would be removed and no further analysis?

Q I will ask Dr. Bush that, but assume in your question that it is continuous until thie first refueling outage.

A I think it could be a valuable asset and I would encourage it.

I might add one more thing, that we're gaging, we may also want to continue with the crack depth measurements independent, just to get anothe_ independent look at their depth, and so perhaps a routine TSI gage analysis added to the instrumentation would be worthwhile.

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But I would be all in favor of it. I think that would essentially answer any concerns I have with that area.

Q The depth gage measurements that you mentioned to monitor, that would be at the first refueling outage to see if they've grown?

A Oh, yes. I wouldn't do that on a daily or a weekly basis. I would do it at some frequency, of an outage would be fine.

Q Dr. Bush, did you want an opportunity to comment?

A (Witness Bush) I believe I'm on the record rather completely on this issue. I don't see why I should use more time on it.

MR. ELLIS: Judge Brenner, I'm prepared to go on to circumferential cracks.

JUDGE BRENNER: Dr. Bush, forgive me. You certainly are on the record on this point.

Tell me again why you wouldn't want to apply the same gaging to the 103 block, at least on the saddles where there have been indications.

WITNESS BUSH: I wouldn't rule it out completely. When we made the decision originally it was there. But I really don't-- I feel these are very shallow cracks. I don't see what I'd call the driving force of residual stresses to the level that I would anticipate there because in the absence of welds, any cracking would tend to

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automatically relieve the residual stresses that might be due to cooling, which I wouldn't anticipate would be extremely high.

On that basis, if we gage 101 and 102, which admittedly have deeper cracks, they would certainly act as leaders, you might say, in the circumstance.

Also the fact that we have a higher strength material in 103 is a factor that I think I would take into consideration.

JUDGE BRENNER: Would you have the gage on all saddles of 102?

WITNESS BUSH: No. I would select saddles that are readily accessible on the basis that they should be representative. I would like to see it at both ends-- I don't mean the two outboard positions but at least in the area of accessibility, and that's about where I would tend to draw the line.

JUDGE BRENNER: The same for 101?

WITNESS BUSH: That's correct, sir.

MR. ELLIS: I am going to switch now, Judge Brenner.

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BY MR. ELLIS:

Q Dr. Anderson, I'm going to switch to the block top now. Dr. Anderson, did you examine some of the section of the block top in the original 103 in which circumferential cracks were detected by FaAA?

A (Dr. Anderson) Yes, I did.

Q Were the samples that you examined -- were the surfaces where the circumferential cracks were visible polished metallographically?

A No.

Q Would you agree, then, that when gray cast iron is sawed apart or ground, pieces of graphite flakes can be broken out of the cast iron surfaces?

A I would say it more emphatically than "can". I think they are. There were examples of it, yes.

Q Does metallographic polishing of cast iron require more care than the same operation on structural steel?

A Say again?

Q Does metallographic polishing -- I'm sorry; I'll speak up. Does metallographic polishing of cast iron require more care than the same operation on structural steel?

A Yes.

Q Why is that?

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A Well, you have a soft phase and a hard phase, so the composite can give you a lot of problems, any time you have a mixture of hardnesses.

Q Dr. Rau, do you agree with Dr. Anderson that gray cast iron, when it's sawed apart, pieces of graphite flakes can be broken out of the cast iron surfaces?

A (Witness Rau) Yes, they certainly can -- and, of course, in the degenerate Widmanstaetten structure of the original 103, the problem is even more severe.

Q When graphite flakes are broken out, Dr. Anderson, of the cast iron surface, are shallow irregular holes -- do shallow irregular holes in the surface remain?

A (Dr. Anderson) Well, they can be holes or they could be trenches, depending upon the crystallographic orientation of what's torn out. Generally, if they are penetrating the surface at an angle where there would only be a hole it is less likely they'll break out, so it's normally trenches -- more length than just a round hole.

Q Dr. Anderson, then is it your opinion that an accurate or detailed examination of a gray cast iron microstructure requires prior metallographic polishing?

A For completeness you would have to do a metallographic -- or polishing before doing your analysis, yes.

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Q Would you agree then that the observation that you have on page 12 of your supplemental testimony...

(Pause.)

Strike that, I'll start again.

Do you have page 12 before you now?

A Yes, I do.

Q Is it fair to say then that the microscopic examination that you refer to on page 12 of your supplemental testimony, given that it was not metallographically polished is not a reliable examination?

A I believe that it was factual. The cutting marks did leave a residue of graphite that had pulled out and I ignored those. I found another pattern on a different plane or a different organization which appeared to link up to the cam gallery cracks and it was my belief at the time I viewed it that these were associated cracks below the surface of the crack that touched the surface.

Q I'm sorry, may I have that answer read back? I thought I....

(Whereupon, the Reporter read from the record as requested.)

BY MR. ELLIS:

Q Dr. Anderson, isn't the sample that you are referring to on page 12 a sample from the liner landing ledge and not from the cam gallery?

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A (Witness Anderson) Yes, that is the liner landing ledge, yes.

Q It is not your testimony though, is it, that anything there linked up with the cam gallery, is it?

A Did I say cam gallery?

Then I'm sorry, the lateness of the hour has -- I meant the crack up above on the landing, I'm sorry.

Q All right. Thank you. I felt that's what you meant but I wanted to give you --

A I'm sorry.

Q That's quite all right. That's why I had it read back, I wasn't sure I heard it correctly either.

Dr. Rau, do you agree that the, as Dr. Anderson has testified, that the examination, microscopic examination of the specimen that was not metallographically polished gave him a factual view that was reliable?

A (Witness Rau) Well it is definitely not reliable in my opinion; I don't know whether it is factual or not. In point of fact, you cannot draw any definitive conclusions about cracking or Widmanstaetten graphite in the original 103 material without careful metallographic polishing.

I have examined that same cut surface that Dr. Anderson examined and find it full of tears and missing pieces of graphite throughout the cut surface and I am completely unable to separate those tears from any of the

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true features of the crack.

There have been, and we did do, three independent non-destructive inspection examinations of that region, that would be LP, liquid penetrant, MP, magnetic particle, and eddy current inspections, all of which clearly identified the same depth of the crack on those cut surfaces, and indicated no additional cracks nearby linking up or in any other way associated with the circumferential cracks. And I believe what Dr. Anderson thinks he saw are simply artifacts associated with the cut surface. You simply cannot draw those kinds of conclusions without metallographically polishing the surface.

Q Well Dr. Rau, then would you disagree or agree with the statement or the conclusion that Dr. Anderson reaches at the bottom of page two of the rebuttal testimony that the cracks he observed and described in the cited testimony "...had an organized appearance consistent with the orientation of the larger crack above them and were not random, as I would expect, artifact from Widmanstaetten graphite to be."

A (Witness Rau) I don't know what Dr. Anderson means by "organized appearance." As I have indicated, my examination of that area indicates tears and missing graphite and trenches in all directions in and around the

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vicinity of the circumferential crack and I am unable to draw any conclusion from visual examination and have to rely on the non-destructive inspection examinations to find the extent of the cracks.

Q The tears and other things that you have described, what are they the result of in your opinion?

A They are simply the roughness of the cutting operation. It introduces local heating during the cutting, it introduces mechanical tearing and abrasion which has the effect of breaking up the graphite flakes and degenerate Widmanstaetten structure and just leaving an interconnected layer along the surface of these tears and broken out pieces of graphite which just obscures the true depth of the crack. You have to go to the non-destructive inspection techniques to sample below the surface to get an accurate reading of whether it is a crack or whether it is just an artifact.

Q Dr. Anderson, did you want to comment?

A (Witness Anderson) I'm not sure, did I understand right that liquid penetrant had been done on that surface, the unpolished surface that I looked at?

Q Would you tell us, Dr. Rau, where the liquid penetrant was done?

A (Witness Rau) Yes. You want to know where?

Q Yes.

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A Well that was done on the -- well the liquid penetrant inspection that we did after the pieces were cut out, the one I'm referring to was done on the profile view, the cut surface, which runs not exactly but roughly perpendicular to the circumferential direction of the liner land, so it is actually looking at the liner land, counterbore and circumferential cracks in the original 103 you get kind of a profile view. It is on that surface that all three of those indications: eddy current, LP and MP were performed.

Q Dr. Anderson, given that magnetic particle, liquid penetrant and eddy current testing was done, would you agree that these are more accurate and reliable for detecting cracks than a visual examination of a specimen that has not been metallographically polished?

A (Witness Anderson) Yes, I would believe that an LP of the surface I looked at could be informative. I have some problems with that because we all agree -- I think we all agree for once -- there was a lot of tears and tatters on the surface and I'm not sure that an LP of the surface I saw can distinguish between those.

But if an LP was done and in the organized area that I found, if there was no signs of penetration I would be amazed and I would have to defer to that. I would really like to see the photographs. I haven't.

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And there was no LP signatures or residues during my inspection.

Q Well Dr. Anderson, I think -- I'll come back to that in a minute.

Do you agree that when a piece of cast iron is sectioned you have these tears and rough edges and that sort of thing on the surface, do you agree with that?

A Yes, no question.

Q And I take it you would also agree therefore that doing a liquid penetrant examination of that surface wouldn't be terribly informative because you have just roughed the surface up by tearing it apart?

A That was my question, that that surface really needs further preparation before you could get a definitive answer. But if, as I understand, it was done on the surface as I saw it and that the LP showed no cracks whatsoever, I would like to see those pictures. I would defer to the LP analysis.

Q Dr. Rau, would you comment on that -- Dr. Wachob?

A (Witness Wachob) The inspection, the LP inspection that we are discussing has been provided in an inspection report as well as photographs to the County and they were discussed at the deposition of Dr. Rau and myself and Mr. Taylor and they are consistent with the results that we have been discussing about the maximum depth of about

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3/8ths.

MR. ELLIS: Judge Brenner, would this be an appropriate time? I think I have very, very little left and I may use the time to reduce it. I would estimate that tomorrow morning I will have less than 30 minutes and perhaps even 15.

JUDGE BRENNER: Okay.

Are you going to ask Dr. Wachob why an LP examination of a roughed-up surface would be valid?

MR. ELLIS: Dr. Wachob, would you answer that question, please?

JUDGE BRENNER: And any assumptions -- I asked it in that way to save time. If any of the assumptions in the question are incorrect, feel free to correct them.

WITNESS WACHOB: The LP exam that was performed on that piece was done in a standard accepted procedural way and the depth of these other artifacts of cutting are so shallow that when you clean off the surface as you are required to do in that procedure that you clean out the penetrant. There really is no crack associated with it, it is only an artifact of the surface.

WITNESS RAU: Just in addition you have heard the term I think "bleed out." If you have a crack of some depth when you do a penetrant inspection after you put the developer on it, it bleeds out and as you wait it gets

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darker and darker and darker. There is a very substantial difference between the amount of bleed out in the indication of the actual circumferential crack and any indication at all from these tears and graphite trenches, if you like.

That is not to say that you can't fiddle around with the liquid penetrant procedures to indicate these kinds of artifacts but there is a very clear and precise difference between what they would appear like and the procedures under which they are visible because they are in fact very shallow.

BY MR. ELLIS:

Q Dr. Anderson, I might just close by asking whether, now that you have heard that, does that help to allay your concerns?

A (Witness Anderson) There has been some references mentioned, I will refer to those and see if I can find what I'm looking for.

JUDGE BRENNER: All right. I'm sure that once we adjourn for the day if you want to get any references as to what LILCO or its agents think you should look at they can help you instead of your having to go through everything.

All right. I assume it is clear to the parties but let me state that after we finish this panel we do want to have discussion on LILCO's motion to re-open and supplement the record and the answers thereto.

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(Pause.)

It's not clear.

MR. ELLIS: It was clear, I am just --

JUDGE BRENNER: Okay. Good.

MR. ELLIS: -- hoping I have a hotel room tonight.

JUDGE BRENNER: You can stay here in the aisle if you want to.

(Laughter.)

JUDGE BRENNER: All right.

Because when you talked about somebody else coming or not coming, that was with reference to the remanded matters.

MR. ELLIS: Yes, sir. No, I'm going to be here for that and I have told Mr. Irwin that it is next week in Bethesda on the remanded items.

He did indicate to me that he -- there has been some problem with the airline schedule and the attachments did not make it up here, but I hope that is not a real problem.

JUDGE BRENNER: It is not going to be. If they are extensive I think I would just as soon have them in my office.

MR. ELLIS: Yes, sir.

JUDGE BRENNER: But whatever transpires, we can

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adjust either way.

MR. ELLIS: They were delivered to your office today, Judge Brenner.

JUDGE BRENNER: Okay. That's fine.

All right. We'll adjourn until 9:00 tomorrow morning.

(Whereupon, at 5:00 p.m., the hearing in the above-entitled matter was recessed, to reconvene at 9:00 a.m., the following day.)

CERTIFICATE OF OFFICIAL REPORTER

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

NAME OF PROCEEDING:

LONG ISLAND LIGHTING COMPANY
(Shoreham Nuclear Power Station)

DOCKET NO.: 50-322-1 (OL)

PLACE: Hauppauge, New York

DATE: November 14, 1984

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission.

(Sigt) William R. Bloom Anne G. Bloom

(TYPED) William R. Bloom & Anne G. Bloom

Official Reporter

Reporter's Affiliation

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