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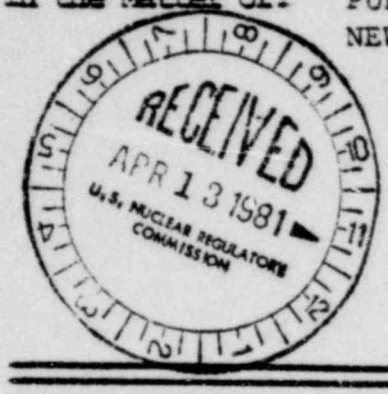
NUCLEAR REGULATORY COMMISSION

ATOMIC SAFETY & LICENSING APPEAL BOARD

In the Matter of: PUBLIC SERVICE COMPANY OF NEW HAMPSHIRE, et al. (Seabrook)



Docket Nos. 50-443
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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING APPEAL BOARD

In the matter of:	:	
PUBLIC SERVICE COMPANY OF NEW HAMPSHIRE, et al.	:	Docket Nos. 50-443 50-444
(Seabrook Station, Units 1 and 2)	:	

Temple Street
Nashua, New Hampshire

Monday, April 6, 1981

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

BEFORE:

Alan S. Rosenthal, Chairman
Atomic Safety and Licensing Board

Dr. John H. Buck

Dr. W. Reed Johnson

APPEARANCES:

On behalf of the Licensee, Public Service Company of New Hampshire, et al., (Seabrook):

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C O N T E N T S

P R O C E E D I N G S

CHAIRMAN ROSENTHAL: Please be seated. This is an evidentiary hearing being conducted by an Atomic Safety and Licensing Appeal Board of the Nuclear Regulatory Commission. The hearing is being conducted pursuant to an order of the Commission entered on September 25, 1980.

The order directed this Board to reopen the record in the licensing proceeding on the construction permit applications for the Seabrook Station, Units 1 and 2 for the purpose of taking additional evidence on certain issues relating to the seismicity of the Seabrook site. The Commissions's order identifies those issues. Their scope has been considered in orders of this Board.

This Board has also entered orders relating to the procedure which will be followed in this proceeding, with particular reference to the order of presentation of testimony on the issues which are being considered. I assume counsel for the respective parties are fully familiar with the terms of the relevant orders.

At this point, I call upon counsel to identify themselves formally for the record, and I'll start with counsel for the applicants, Public Service Company of New Hampshire, Et Al.

MR. DIGNAN: Mr. Chairman, members of the Board, my name is Thomas G. Dignan, Jr. I am a member of the law firm

of Ropes & Gray, 225 Franklin Street, Boston, Massachusetts. I appear for the Applicant today with my colleague Mr. Robert K. Gad, III.

CHAIRMAN ROSENTHAL: Thank you, Mr. Dignan. And now counsel for the Intervenor, New England Coalition on Nuclear Pollution.

MR. JORDAN: Members of the Board, I'm William S. Jordan, III, a member of the law firm of Lehman & Weiss in Washington, D.C., appearing for NCAP.

CHAIRMAN ROSENTHAL: Thank you, Mr. Jordan. And counsel for the Nuclear Regulatory Commission staff.

MR. LESSY: Mr. Chairman, Mr. Buck and Mr. Johnson, my name is Roy P. Lessy. I'm Deputy Assistant Chief hearing counsel of the NRC staff. I'm appearing on behalf of that party.

CHAIRMAN ROSENTHAL: Thank you, Mr. Lessy. All right, are there any preliminary matters which counsel wishes to raise with the Board? Hearing none, we'll proceed with the first witness on the initial issue being considered, and that is, as counsel are aware, the question of the intensity which should be assigned to the safe shutdown earthquake for Seabrook. Mr. Jordan.

MR. JORDAN: Your Honor, NESCP will call Dr. Michael Chinnery.

CHAIRMAN ROSENTHAL: All right. Dr. Chinnery, if you will come over to the witness stand here and just remain standing for one moment.

MICHAEL CHINNERY

Was sworn by Chairman Rosenthal.

CHAIRMAN ROSENTHAL: Thank you very much. Dr. Chinnery, you may be seated. Mr. Jordan, you may proceed.

BY MR. JORDAN: Thank you, your Honor.

Q Dr. Chinnery, would you give us your full name and address for the record?

A My name is Michael A. Chinnery, C-h-i-n-n-e-r-y. My residence is at 110 Gray Street, Arlington, Massachusetts.

Q And, Dr. Chinnery, have you previously filed in this proceeding a statement, a document entitled Statement of Dr. Michael A. Chinnery on remand to the Automic Safety and Licensing Appeal Board submitted by the New England Coalition on Nuclear Pollution?

A Yes, I have.

Q And just to be more specific, that is consisting of sixteen pages exclusive of references plus four exhibits?

A Mm-hmm; indeed.

Q And have you submitted a statement entitled Rebuttal testimony of Dr. Michael Chinnery on remand to the Automic

Safety and Licensing Appeal Board consisting of fifteen numbered pages, three figures and references?

A Yes, I have.

MR. JORDAN: I have copies of these for the reporter, three copies of each.

Q Dr. Chinnery: Do you adopt these now as your testimony?

A With the addition of three corrections.

Q Could you tell us those?

A In my direct testimony, these are all typographical points but in my direct testimony, Page 13, table 1, there's a mistake in number there, expert 4 the best estimate should be eight so it should be VIII; and on Page 14 of that direct testimony, third line from the bottom, there's a reference to the 1955 Cape Ann earthquake. That should be the 1755 Cape Ann earthquake.

In my rebuttal testimony on Page 2 there are two words which are mistyped. On Line 10, the word "unavailable" should be "unassailable". There is a difference here. And a small point at the fifth line up from the bottom where it says a professional judgment, it should be on professional judgment. Those are all the corrections.

MR. JORDAN: Thank you. The witness is available for cross-examination.

MR. DIGNAN: Could we have them put in evidence before we start cross-examination?

MR. JORDAN: I'm sorry. I would have these, this Dr. Chinnery's statement, that is the one entitled his statement of Dr. Chinnery marked for identification as Exhibit Intervenor's Exhibit 1; and then Dr. Chinnery's rebuttal testimony marked for identification as Intervenor's --

CHAIRMAN ROSENTHAL: Mark for identification or are you moving they be incorporated in the record?

MR. JORDAN: Well, given, your Honor, that they are not written in the question and answer format but written as in effect written evidence, I'm taking the approach in fact it's the same approach I gather that was used the last time around which is to have them be exhibits rather than incorporated impaginated with the records. You can't read the transcript through in a question and answer format. This is why I have chosen this approach.

CHAIRMAN ROSENTHAL: Well, I assume that you intend them to be included as part of the record and I also would suppose that your intention is that they be bound in the transcript of today's proceedings at this point?

MR. JORDAN: Well, I obviously -- we intend they will be part of the record. I should think the simpliest way is to have them bound into the transcript and I will make this motion if that's the appropriate one to do.

CHAIRMAN ROSENTHAL: All right. Mr. Dignan?

MR. DIGNAN: As I understand it, there's now a request

at this time the Board admit into evidence Exhibit 1 and 2 as previously identified.

CHAIRMAN ROSENTHAL: Just one moment.

(Panel conferring)

CHAIRMAN ROSENTHAL: We come clear on one thing, Mr. Jordan. We have Dr. Chinnery's prepared testimony.

MR. JORDAN: Yes, sir.

CHAIRMAN ROSENTHAL: To which there was appended certain documents listed as Exhibits 1, et cetera --

MR. JORDAN: Yes.

CHAIRMAN ROSENTHAL: -- is that correct? Is it your proposal that his testimony be introduced as an Exhibit? I don't follow this, so I would have thought you would have been offering his previously submitted prepared testimony as the direct testimony of this witness, have the exhibits attached to it.

MR. JORDAN: Well, your Honor, let me back up a step to -- I'm simply interested in having the simplest, clearest record for you to review. It seems to me that the simplest is to -- to request that Doctor Chinnery's direct and, indeed, his rebuttal testimony be bound into the record at this point as --

CHAIRMAN ROSENTHAL: As his testimony?

MR. JORDAN: As his testimony. And I consider the Exhibit appended to his testimony to be an integral part of it and would have them bound there as well.

CHAIRMAN ROSENTHAL: Well, I am going to treat this as a motion to accept as part of the record Doctor Chinnery's prepared testimony, together with the Exhibits that were submitted in conjunction with that testimony. All right. Now, so understood, Mr. Dignan?

MR. DIGNAN: Yes.

CHAIRMAN ROSENTHAL: And the rebuttal testimony as well.

MR. DIGNAN: Mr. Chairman, I have no objection to the admission into evidence of the prepared testimony and the prepared rebuttal testimony and the Exhibits other than Exhibit 1 to the prepared testimony. I do object to the admission of Exhibit 1. My grounds are very simple. They aren't -- they aren't technical, or legal or otherwise. The problem is that certain statements in what has been marked as Exhibit 1 to the Chinnery testimony are inconsistent with the statements in the

direct testimony itself. This is due, undoubtedly, because of the time periods which elapse between the writing of the first and the writing of the last. However, it seems to me that one cannot ask to have it admitted for the truth of the matters contained clearly in consistent statements. And, as I understand it, no limitation was put on that offer, and that the -- that the Exhibit 1, that article, is put in for the truth of the matters contained, and there are statements in there just inconsistent as a matter of fact with his statements in the direct testimony.

As an example, on the testimony on page 14, the Cape Ann earthquake is said to be -- and at modified mercalli intensity 7R8. In Exhibit 1 to the testimony, pages 91 to 96, you would see a reference to two MMI IX earthquakes, including the Cape Ann.

I'm not trying to hold anybody to this or trick them or anything, but it seems to me that we at least ought to have a statement as to which is in control, and they're inconsistent.

CHAIRMAN ROSENTHAL: Why can't you ask this Witness about these perceived inconsistencies on cross-examination?

MR. DIGNAN: I can. There's no question I can do that. I felt I had an obligation to point out the question on motion, and counsel's free to withdraw one of the Exhibits or leave them there. And I can assure you I will spend some time with the inconsistency.

CHAIRMAN ROSENTHAL: Mr. Jordan.

MR. JORDAN: I think the appropriate approach is to have it addressed on cross-examination.

CHAIRMAN ROSENTHAL: Mr. Lessy.

MR. LESSY: I -- I think the objections go to the weight, not the admissibility of Exhibit 1, and Mr. -- Mr. Dignan's objection to Exhibit 1 is well-taken; but I think in an administrative proceeding the objection goes to the weight of his -- of the -- of the inconsistency, not the admissibility.

CHAIRMAN ROSENTHAL: The objection is overruled, and Exhibit 1 will be accepted into evidence, subject, of course, to the right of counsel to cross-examine Doctor Chinnery with regard to any perceived inconsistencies between his prepared direct testimony and statements of fact that may be contained in Exhibit 1 or, for that matter, any other Exhibit. All right. Any further -- all right, it's admitted into evidence, and Doctor Chinnery has been made available for direct examination.

I'm assuming that the applicants will conduct cross-examination first. Is that correct?

MR. DIGNAN: I am prepared to go forward, yes, Mr. Chairman.

CHAIRMAN ROSENTHAL: All right.

MR. DIGNAN: Mr. Chairman, before we commence cross-examination, could we have a brief bench conference?

CHAIRMAN ROSENTHAL: Yes, you may.

(Bench conference.)

CHAIRMAN ROSENTHAL: Before we commence cross-examination of Doctor Chinnery, it should be made clear that as the toms will be used in this proceeding, Exhibits 1, 2, 3 and 4 refer to Exhibits that were appended to the prepared direct testimony of Doctor Chinnery, and that those Exhibits were submitted as an appendage to that testimony. They bore those Exhibit numbers. So that is what the references to Exhibits 1, 2, 3 and 4 mean. All right, that's clear. We will now proceed with the Applicants' cross-examiation of Doctor Chinnery.

CROSS EXAMINATION

BY MR. DIGNAN:

Q Doctor Chinnery, good morning.

A Good morning, Mr. Dignan.

Q Doctor Chinnery, as you know, my background is entirely legal and not technical, and if I fluff up as a result on a technical term, I hope you will free to correct me or ask me to reask the question so that we both know what we're talking about.

Doctor Chinnery, let me ask you this broad question. If there are any inconsistencies -- and I'm not suggesting that there are -- but if there be any inconsistencies of statements of fact or probability in your July-December 1973 article, which has been marked as Exhibit 1 to your direct testimony, and the direct testimony itself, which should I view as the controlling statement, the one in the testimony or the one in the article?

A The one in the testimony.

Q Doctor, would you be kind enough to turn to page 1 of your testimony, please.

A Okay.

Q In that -- that page, you say there are two methods which have been proposed for the estimation of the SSE, and then you set out the two methods called the deterministic method and the probabilistic method. My question is, who is proposing the probabilistic method other than yourself?

A I -- I would not really say that I had proposed the probabilistic method; however, there's no question that my method in some way is not the normal approach to the interpretation of the pending state. In my view, the NRC commissioners themselves have admitted that my approach is a valid approach of the pending state.

Q Do I understand you're saying that you understand the commissioners have proposed the probabilistic method?

MR. LESSY: Would you repeat the question, please?

Q Do I understand you to be testifying it's your understanding that the commissioners of the Nuclear Regulatory Commission have proposed the probabilistic method; is that what that is --

A No.

Q Then I would like you to tell me who has proposed it. Your statement in your testimony is two methods have been

proposed. If it isn't you and it isn't the NRC, who is it?

A All right, a probabilistic approach has been discussed by many people in the subject of seismic risk, and I'm not sure I can quote them all. I will attempt to quote some of them for you.

Q Before you quote them all, the statement is "has been proposed for the estimation of the SSE." And I believe we can agree that that means safety shutdown earthquake, a term coming from the Nuclear Regulatory Commission regulations. Who has proposed the probabilistic method for that?

A I see the point of your question. Certainly myself. I read Doctor Trifunac's testimony as indicating the same probable --

Q Which Trifunac testimony, the testimony in this proceeding?

A Yes, in this proceeding.

Q That has not been admitted into evidence yet?

A Right, that's not in evidence. I'm willing to assume responsibility for it.

("ii") We have a statement "here the historical record is taken only as a sample of the long term seismicity of the tectonic province."

Now, on what basis do you take a historical "sample"?

A This is perhaps a more technical term than it might seem. I use sample in the sense that it is used by statisticians. When you have a series of observations, occurrences, we typically call it a sample realizing that it's only a small part of a whole continuum so I can give you many examples. If you throw dice a few times, that clearly in a few samples out of a total population. The technical term that you would obtain if you threw the dice with extremely large number of times and the basic problem in statisticians to use a sample to discover the properties of the apparent population so I use the sample in that sense the historical record is limited. It is not an infinitely long record. I think our problem here would be very different if we had a record for some thousands or more years. We have a limited seismic record so I can consider that to be a sample and I use it in a technical sense of the word.

Q Whether or not used in a technical sense of the word in your judgment is the historical record in New England a good or a bad sample to work from?

A As you'll see later where it describes the nature of the data, I think there are some problems with it. I think it is all we have however so I believe it is something we have

to work with.

Q Okay. And, Doctor, I would like to direct your attention to the sentence that begins at the end of page 1 and carries on over to page 2, and I will read it for the record quote In this case, the concept of the 'maximum earthquake potential' used in the definition of the SSE has to be modified. End quotes.

Now, my question to you is: What do the words "in this case" refer to?

A I would like to give a slightly longer answer to that if I might, Mr. Chairman. I think this is a key point in the whole of my testimony, and if you will allow me just a couple of minutes to try to --

CHAIRMAN ROSENTHAL: Well, as long as it's responsive to the question.

THE WITNESS: It is, I think.

Q Dr. Chinnery, the question is very simple. The words "in this case" in the english language have an antecedent and the question to you is: What words ahead of those in your testimony constitute the antecedent?

A All right. The admission that there is a long-term seismicity of which the historical record is a better sample. This is a particular case I'm talking about.

MR. DIGNAN: Could I have the reporter read that back, please?

MR. LESSY: I was going to ask the same. Would the reporter read the answer back.

(Last answer read back by the Court Reporter.)

Q I'm afraid I must not be clear in my questions, Dr. Chinnery. I would like to focus in this case in the sentence I just quoted. And my question to you is: What does the words "in this case" refer back to in your testimony? Does it refer back to the problem of probabilistic method? Does it refer to the proceeding in which we're involved? What does it refer to?

A It refers to the preceding sentence and to the content of the preceding sentence.

Q The entire content? Okay.

A Yes.

Q Okay. Thank you. Now, you say that the concept of the maximum earthquake potential has to be modified and I guess my first question is: Modified from what?

A From the definition in Appendix A.

Q So I understand then that in order to discuss you theory, we must alter the definition of maximum earthquake potention as it is stated in Appendix A, is that correct?

MR. JORDAN: I object, your Honor. That appears to be calling for a legal conclusion that has already been reached by the Commission which is that you can consider Dr. Chinnery's hypothesis without altering Appendix A.

MR. DIGNAN: If he had done that, I wouldn't have a

problem but he says he is not going to change Appendix A.

CHAIRMAN ROSENTHAL: Well, let's make it clear. Do you regard your method as involving an alteration in the content on Appendix A?

THE WITNESS: Mr. Chairman, I think the definition of the safety shutdown earthquake in Appendix A is an extremely difficult one. It's a difficult one to use in a practical scientific way.

I think looking at the whole problem from my point of view, that one would naturally be led to a different phraseology for it. But that could well be included as an interpretation of the present statement. So I think it could be better phrased, more clearly phrased.

CHAIRMAN ROSENTHAL: You don't perceive a departure from the substance of Appendix A as you understand it?

THE WITNESS: I don't believe it's a change in substance.

CHAIRMAN ROSENTHAL: Okay.

THE WITNESS: Those others may --

MR. DIGNAN: May I proceed?

CHAIRMAN ROSENTHAL: Yes.

Q In light of the Chair's statement, would you, were you to contribute that in this case: "The earthquake potential" used in the definition of the SSE has to be modified?

A I said it could be better rephrased. I would prefer --

Q Doctor, before you proceed with that answer, would you

reread the rest of that paragraph? I am not trying to trap you here.

(Witness complies.)

A I understand.

Q Having reread that paragraph, do you wish to change either the answer you gave to the Chairman or do you wish to withdraw that statement?

A I would like to give a very short background to what I'm talking about here to put it in context.

THE WITNESS: May I do that, Mr. Chairman?

CHAIRMAN ROSENTHAL: Yes, you may.

A The whole key point in all the arguments that I'm making here is one, and it's a very simple one. I have not ever believed that it is valid to date the historical record which is of limited length several hundred years in the New England area and because everything to do with the seismic crust being entirely on that record and in particular, I, I do not in my professional judgment believe that the largest earthquake that can happen in the New England area or within the province that contains the Seabrook site is contained necessarily within that historical record.

If one wants to admit that there may be large earthquakes occurring at infrequent intervals in this area, they clearly, the concept of maximum earthquake potential as it is phrased in the regulations needs to be, if not actually modified in

language at least reunderstood because maximum earthquake potential I refer to earthquakes that are not contained within the historical record. Let me stop there.

Q But, Doctor, let's go on to Page 2. You say "-- having talked about modification and the SSE must be defined as that earthquake which will occur in the tectonic province containing the site with some fixed acceptable level of annual risk or probability. This acceptable level of risk is not defined in the NRC rules and regulations."

Have I read that correctly?

A Yes, you have.

Q And we can agree not only is it not defined but there is no attempt to define such an annual risk in the regulations, is there?

A True.

Q All right. Now keeping that in mind, I come back, aren't you proposing the alteration of the Appendix A or modification to use your word to include a concept that it does not even include?

A Yes.

Q Now, coming down on about halfway on Page 2, you have a statement "In New England the historical record of earthquake occurrence is approximately three hundred years long. The only catalog of seismic events in this area that has been published in the scientific literature is that by Smith (1962, 1966)."

Is Smith the only catalog?

A This is a good question. Mr. Dignan, I am not sure about that.

Dr. Chiburis has produced a catalog. I am not sure if it's been published in what I could call the regular scientific literature. Perhaps you can advise me on that.

Q I am only paid to ask questions, Doctor.

(Laughter.)

Q You have got to pay me an expert fee if you want me to testify.

A I would like to say what I mean by scientific literature, however. I think that scientific literature means something to a scientist that is the regular journals that are available to everyone in libraries and documents of that kind.

I know there have been lists of earthquakes produced as part of the proceedings here at Seabrook. To me that is not scientific literature but clearly we could get into an argument about that but it's not worth crying about.

Q Is there any catalog put out by the USGS that you are aware of?

A There certainly is a book entitled Earthquakes of United States. It does not I think pretend to be technically complete in this area; and the other catalogs have far more earthquake technically at the lower intensities.

Q All right. Doctor, now taking the Smith catalog which

I have, are you able to tell us how many of Smith's earthquakes that he rated at modified mecurial propensity five -- and off the record

(Discussion off the record.)

Q How many of Smith's are modified mecurian propensity five or better were on bedrock?

A I don't know the answer to that, Mr. Dignan.

Q Did you make any attempt to break earthquakes down in terms of what kind of foundation conditions were involved at their centers?

A No, I didn't. I think I have a reason for not doing so but I did not.

Q Well, obviously you want to give the reason even though I haven't asked you for it. Well, feel free. We'll get that out of the way and not for redirect. Go right ahead.

A There are many ways to determining the size of an earthquake. Many ways, far more than we would like to have, in fact.

The so-called intensity of an earthquake is one. It's not an easy -- I will characterize in the ideal circumstances at least it is something which is a sort of an average observation of damage over a wide variety of soil condition and sites. It is not hopefully a limited thing with some of these historical earthquakes this may be a problem.

What I am trying to say that in attempting to

characterize the earthquake sort; you are not attempting to characterize the damage or the intensity so you use the damage as an indicator of the size of the source.

So Ideally if you had enough observations one should not have to worry whether the intensities were measured on soil or on bedrock. One would have a number which in some such way which would characterize, which would relate to other measures of plot magnified.

Q So intensity so we can be clear to my untechnical mind is the measure of damages, observed damage in a --

A There is no uses of intensity.

Q Modified Mercalli intensity?

A True. There are two uses. One use is a point measurement of damage during an earthquake where we have a scale which clearly relates to numbers to amount of damage. There is another use of the word intensity which is to characterize the size of the earthquake and there is a settled, but there is a rather important difference between these two.

Q Well, subtle difference is simply that the intensity which is used to characterize the earthquake is the intensity for larger intensity observed in terms of observing the damage, isn't that right?

A No.

Q In many instances the -- can you give me one --

MR. JORDAN: Excuse me. Mr. Chairman, may be allow him

to answer the question?

CHAIRMAN ROSENTHAL: Let him finish his response.

A I think you'll find that if you look at the San Fernando earthquake there are figures of intensity of eleven or twelve; in certain places it was not an intensity of eleven or twelve earthquake; it was characterized by an average of all the observations so those peak valleys do not enter in and when the earthquake sites were characterized it was not characterized as an intensity twelve.

Q What intensity is given to the San Fernando earthquake, Doctor?

A I am not absolutely sure about that. I think it's about an eight but I could be wrong with that. Perhaps somebody else knows the answer to that. There is much less than the absolute peak valleys that were observed anyway.

Q And of course the San Fernando earthquake is a recorded earthquake, is that right?

A Certainly was; right.

Q Now, the historical earthquakes were not recorded, correct?

A Right.

Q Now, I am going to ask you to give me any example in the record, the historical record where there was a larger intensity observed in that than Smith assigned to it, in fact?

A I -- no, I don't. I did not examine all the individual

points that went out to make that out.

DR. JOHNSON: May I interrupt for just a minute, Mr. Dignan, to get a clarification here?

MR. DIGNAN: Sure.

DR. JOHNSON: Would it be more correct when you use the second or use -- or when you're applying the second use of the word "intensity" to call that epi-central intensity as opposed to simply intensity?

A Yes. It is frequently referred to as meaning epi-central intensity but as I say, it, it is not necessary. The maximum point in intensity is for a substantial area around the south as looks for essentially, the smooth valley of the intensity reading within that area.

DR. JOHNSON: Is it true that you can get within a very confined space a number of different indications of the intensity of the particular seismic event?

THE WITNESS: That is certainly true, yes. There are very many conclusions used: soils conditions, focusing on seismic waves and many other things which really confuse the issue and lead to quite a wide scatter in the observations.

DR. JOHNSON: All right. Thank you. May I ask that all of you when you refer to earthquakes particularly those in California, give the date as well as the location because in many cases there are several earthquakes in the San Fernando Valley, for instance, I think you were all referring to 1971?

THE WITNESS: I was, indeed, yes.

MR. DIGNAN: Yes, I was too, Doctor.

DR. JOHNSON: Thank you.

Q (By Mr. Dignan, continuing.) Let's explore a little bit what you are discussing with the Board, Doctor.

Are you telling me that there can be times when, because of the conditions of the earth measure a high plane of the foundation that you can get very high intensities when in fact the earthquake is epi-central intensity is much lower than that?

A When the size of the source is much less than a maximum, yes. I think there are many instances when you can get an intensity twelve from a very localized area, from a rather small earthquake but that you don't want to use twelve to characterize that earthquake source because that was clearly an analogous reading.

Q What would account for that occurring?

A There are many discussions of this in the literature and I'm not probably the best person to answer that question.

As I understand it, there are, as I say, the result is true of peculiar soil and rock conditions; I think a very large efficient is the radiation from the seismic source which can be focused and defocused in various parts of the field of observation.

Q Doctor, I would like to move down on Page 2 now.

You are referring to a, a network of instruments. Well, let me just read it: "Instrumental records again have a variable quality are available since the 1920's but only in the last few years has a proper seismic network been installed. This network has detected relatively few events since it was created and contributable to the assessment of the seismic risk area."

I take it this network refers to the proper one that is recently been installed, is that right?

A Yes.

Q Rather than the 1920's?

A I'm talking to the one that's been recently been installed.

Q All right. When was that installed?

A It was installed during about the mid-70's. There was stations in up-state New York somewhat earlier than that; even in the 1960's there were a group of observatories run by Western Observatory in Maine and New Hampshire for a brief period but funding the stations being installed operated by MIT, Western Observatory, University of Connecticut and Lamont numbering all together thirty New England, the up-state New York some seventy stations. These were not installed until I would think the completion date was about 1975 or something like that.

Somebody may want to correct me on that but it's of that order of magnitude.

Q Now -- now, you say this network has detected relatively few events since it was created. I assume the reason it has detected relatively few events is because there have been relatively few events?

A That is true.

Q It's not that there was a hardware problem with the system?

DOCTOR JOHNSON: Excuse me again, Mr. Dignan. I wonder if -- Doctor Chinnery, if you could tell us what the lower level of intensity of that network is?

THE WITNESS: Again, I -- I cannot be sure. I'm sure that the network can detect certainly below magnitude 2; I would suspect below magnitude 1; in other words, well down, much below -- if you're talking in terms of intensity, this takes it down to intensity 2 or 1.

DOCTOR JOHNSON: Are these the earthquakes that an individual may not be able to feel?

THE WITNESS: Yes.

DOCTOR JOHNSON: Human observers would not be able to detect?

THE WITNESS: Much smaller than felt earthquakes, yes.

DOCTOR JOHNSON: Excuse me.

MR. DIGNAN. Thank you.

Q On page 3, Doctor, would you be kind enough to look at the first full paragraph beginning "both of these questions,"

and review it for a minute and just tell me when you have?

A Yes.

Q In your judgment, Doctor, can geology be used to demonstrate past seismicity of an area?

A I think I have to ask you to rephrase that a little. In areas of frequent earthquakes, it has indeed proven possible in the last few years in California to start examining seismicity using geological evidence, yes. Whether this is possible in New England, I question.

Q I am told by the geologists that -- that they have a pretty good 10,000-year record in the geology of New England as to the seismicity. Would you dispute that statement?

A I don't feel I can, because I'm not a geologist. I -- I have not heard such a statement or seen the grounds for it. I would be surprised if it were true.

Q Now, on the bottom of the page, you claim the concept of a tectonic province is a legal one -- that's in Appendix A -- and has no clear scientific significance. Doctor, do you have handy a -- a -- the Appendix A definition of a tectonic province, or would you take my word for it?

A There's one right on top of that pile.

THE WITNESS: Would you get it for me, Bill? Thank you.

A Yes, I have it.

Q I believe the definition reads "a region of the North

American Continent characterized by a relative consistency of the geological structural features contained therein."

A Excactly.

Q To my untutored mind, I thought that was a geological concept rather than a legal one?

A I disagree, Mr. Dignan.

Q Okay.

A There is no scientific definition of a tectonic province that remotely resembles this. Now, this is phrased in a scientific way; but, to me as a scientist, the words "relative inconsistency" mean nothing. I cannot interpret those in my scientific way.

Geological structural features I understand, but what relative inconsistency means I do not.

Q You say as a scientist you cannot interpret words "a relative inconsistency of the geological structural features"?

A No. The earth is extremely homogeneous on a great many scales. It's inhomogeneous on very small scales, and intermediate scales and very large scales.

The suggestion is made here that on some scale, probably of the order of some hundredths of -- of kilometers in scale, one can define a province which is in some sense uniform. Now, that's the way I would take it to be; and if that's so, I can only ask where you can do this, and I would say that the various different interpretations of tectonic province

to be found in -- in discussions of -- of seismicity of the Eastern U.S. just indicate how difficult the scientists do find it to define a tectonic province in this sense.

Q Doctor, I'd like to pick up at the bottom of page 3 and over to page 4 your testimony. You say, "but it is not at all clear that large provinces can be defined within which the seismotectonic characteristics are in any sense uniform." Well, assuming that to be true, isn't it the geology that has to be uniform as far as Appendix A is concerned?

A You are -- you are right.

Q So is this another place --

MR. JORDAN: Your Honor, I gather the Witness was continuing his answer. Can he be allowed to continue?

CHAIRMAN ROSENTHAL: Well, he responded that Dignan was right, if that was an answer to the question.

Q So, Doctor, now I take it this is another place where you would have us alter, clarify, whatever word you wish to choose, the language in Appendix A?

A I do not consider that to be my own idea. All the discussions -- at least virtually all the discussions I have seen on tectonic provinces -- I think you will find Handy and Devine in a variety of well-known -- McGuire, well-known papers on this subject have used seismicity as part of their definition of tectonic provinces in the sense in which we're using it here. So they have indeed gone beyond only geological information to

include seismological information.

Q Now, would you try my question, which is, is this another place where you would have us alter the language of Appendix A?

MR. JORDAN: I object again, your Honor. I think that again we're getting to the issue of legal interpretation. The Commission has ruled that Doctor Chinnery's hypothesis is cognizable under Appendix A. Whether Doctor Chinnery agrees specifically with the way the Commission interprets Appendix A really isn't at all relevant.

CHAIRMAN ROSENTHAL: Mr. Dignan.

MR. DIGNAN: Two things. One, if we're going to knock out all legal conclusions in this testimony, we can start with page 1 and start knocking out testimony. There's one legal conclusion after another that my brother has referred to. I don't mind them, but I think I have a right to test them. Secondly, it is not at all clear to me that Doctor Chinnery's theory is in Appendix A.

What the Commission ruled was that they had heard the whole thing, that there had been some advances in the field, and the Board should have another shot at it, and the Applicants should have another shot at it and the Commission might be able to consider it. What I'm trying to take cognizance of is Doctor Chinnery's theory. You've simply got to read Appendix A. And until there's a rule written to rewrite Appendix A, this

material is irrelevant to the design of the Seabrook Nuclear Power Plant.

CHAIRMAN ROSENTHAL: I'll let the question stand.

MR. DIGNAN: Would you repeat the question to the Witness, please?

(Question read.)

A Mr. Dignan, I think altering the language of Appendix A would make it much clearer what the intent of the concept of a tectonic province is. I do not believe personally that you can define a tectonic province in A in a way which can be used to determine the safe shutdown of an earthquake without determining the seismicity of that province.

Q Now, Doctor, you -- you refer in here, in that statement, to large tectonic provinces being defined. Who says a tectonic province has to be large?

A Yes, that is a very wishy-washy term. I agree with you. I can only interpret -- what I mean by that, I mean something which is not a mile across. And one has to put it perhaps on the horizontal scale of perhaps a hundred kilometers to a thousand kilometers or more.

Q Well, Doctor, let me ask you this question. If we dealt in very small, tightly-defined tectonic provinces, isn't it a fact that a large part of your theory just could not be demonstrated if one confines themselves to small tectonic provinces? Doesn't your theory require large areas in order to

have the vents necessary to do the analysis?

A Mr. Dignan, you can apply Appendix A to extremely small areas.

MR. DIGNAN: Mr. Chairman, can I have an answer to my question?

CHAIRMAN ROSENTHAL: Did you understand Mr. Dignan's question?

THE WITNESS: Please repeat the question.

(Question read.)

A To give you a straight yes or no, I shall have to get you to define "small."

Q The Seabrook site.

A The width of the Seabrook site --

Q Yes.

A -- between the fence?

Q The bedrock at the Seabrook site.

A Okay, if one were to say that one had to base one's entire -- yes, one could not apply probabilistic methods, you're quite right. You couldn't apply any kind of method, because there have been no earthquakes within the boundaries of the Seabrook site.

Q And that is why the choice of area one utilizes when going through the type of exercise you do is critical to the result that you prepare to demonstrate, is it not?

A Yes, it is critical to any method.

Q And it is also true that it is critical that a proper temporal time period be selected?

A What do you mean by "proper," Mr. Dignan?

Q Well, I am suggesting that if one moves time periods of the studies you have made, one could get much different results.

A That would not be the case if we had adequate data. I think -- let me put it this way. I am concerned with earthquakes which happen infrequently, that occur in the average every few hundreds or few thousands of years.

Now, clearly if one tries to find something out about those earthquakes using data from ten years, one is not going to find any information out. So in that sense, I agree with what you say, but it's self-evident to me.

Q Well, Doctor, isn't it also true that if one took, for instance, the time periods you use in your testimony and lengthened them out to pick up some larger events, some fairly startling things will happen to the lines that you have drawn?

A Startling?

Q Yes. Is the word "startling" not clear?

A I would like to refer to my 1979 paper and answer that question.

Q Doctor, you should feel free to have anything in front of you that you want.

A No, I don't mean to -- I mean, I think my 1979 paper

attempted or addressed that exact question that you're asking; where it said was, supposing we had in several areas of the Eastern United States data from a rather limited recent period, would it in any way enable us to extrapolate backwards to predict that earthquakes may be possible in those areas of rather larger magnitude or epicentral intensities, which is what I was using there. And the conclusion of that paper was that, indeed, you could on a quite reasonable probablistic basis. So I think that paper, in fact, answers your question. I think you can do that. You can take short periods and extrapolate.

Q I must have missed it, but what I'm asking you is, is it not so that if you change the time period that you utilized in the '79 paper to pick up larger events in the areas that you worked with, some very startling things will happen to these lines you have drawn?

CHAIRMAN ROSENTHAL: By "startling," you mean the line will change, or it will bend or it will do something? Startling is a rather imprecise term.

Q Will the lines bend, to use the Chairman's term?

A Plotted in the way that Mr. Holt did in his testimony, which I think is what you're referring to --

Q Forget Mr. Holt. Let's stay with my question.

A No, because the question is how do you plot these things and what do you consider valid to plot. There is a definite question here as to what and how one should make this

kind of plot. If one has a 300-year interval of data, one should not plot any events on that gravity that have average return periods of any more than 300 years. One should preferably use them considerably less so that the points have some meaning.

CHAIRMAN ROSENTHAL: Doctor Chinnery, I thought Mr. Dignan's question was that if you change your time period -- now, you may believe that there is no reason to do so; that the time period you've selected is the appropriate one -- but his question was, if the time period was changed, the line that you drew that appears in your graph would be altered.

Was that the question?

MR. DIGNAN: Exactly.

CHAIRMAN ROSENTHAL: I don't think I understood your answer to the question.

THE WITNESS: Okay, let me explain. Let us suppose we have a 300-year record of earthquakes in an area. Now, if during that time you have ten earthquakes, then it is probably not too unreasonable to say that roughly that size of earthquake happens every 30 years. If you have five earthquakes, clearly one can start to worry about the statistical variation of things, but still one may convince oneself that with some level of error, can say these things occur roughly every 60 years. What I'm saying is that when you have one earthquake, one should not plot this, because it is not telling you a thing.

CHAIRMAN ROSENTHAL: I understand that.

DOCTOR BUCK: I think you're not -- you're trying to explain what you did, and you're not answering Mr. Dignan's question. The simple question was asked, if you use a different time period, would the lines change. Now, that's the question he has asked.

THE WITNESS: With the exception of those very large eqarthquakes, no.

DOCTOR BUCK: Let's leave the exceptions out. Use all the earthquakes and use the longer time period or wherever there's a different time period, would the slope of the line change or would the line change in character? No exceptions; just answer the question as it was stated.

THE WITNESS: But I was trying to -- you see, the question is how do you plot these large earthquakes; and if you plot them in the way -- in one way, it produces a different --

DOCTOR BUCK: Doctor Chinnery, we're not asking at the moment -- Mr. Dignan is not asking at the moment for an answer. He is asking you a purely hypothetical question. If you change the time periods, would your lines change? Now, that's yes or no.

THE WITNESS: All right, I'm going to say no.

Q The line would change not at all, Doctor?

A Right.

Q And you want to stand by that testimony?

A Yes. You have to follow that up to see exactly why I say no.

Q Well, do you wish to explain your no?

A Yes. You see, this is the crucial point in this, this thing is that I do not consider it valid to plot events that occur very infrequently so I would not plot the very large ones.

Q Well, Doctor, the last time you testified here, you plotted a data point VII and used, do you recall that?

A Yes.

Q Dr. Salo asked you what your conversation was in that data point. Do you recall that testimony?

A Going back some -- I don't know what I did say. I don't know, though.

Q Well, We'll get into it later.

A Okay.

Q I was just interested to find that now you don't plot infrequent events?

A No. There are enough intensity sevens within the circled reports but not enough intensity eights. If you plot the sevens that you fall on a separate line, so this is, this is satisfactory for me.

CHAIRMAN ROSENTHAL: Just so, Dr. Chinnery, I'm clear

in my own mind, you testified that even if you expanded the time period and plotted the earthquakes in that expanded period, it were not plotted in the period which you took, it would be no effect on the curve, line or the slope, is that -- I'm just trying to --

THE WITNESS: Mm-hmm.

CHAIRMAN ROSENTHAL: -- trying to determine whether that was the answer you gave to Mr. Dignan because if not then I'm -- I misunderstood your answer.

THE WITNESS: Okay. Let me repeat that for clarification.

If there is one earthquake, one large earthquake with any historical record and you plot it on the graph, that that kind of earthquake happens once every three hundred years because there was one within the last three hundred years, then you will change the shape of the graph.

Now, I do not consider that a valid thing to plot and this is why I would not plot it and therefore I would not change the slope of the graph.

Q Now, did you just tell me that you thought there were enough sevens to make up a data point upon which you are willing to rely or did I mishear you?

A That's the way I recollect it, yes.

Q Okay. Doctor, I want to show you -- I would show it to your counsel beforehand -- a page from the transcript of prior hearings in this matter.

You were testifying in response to questions of Dr. Salo at the Atomic Safety and Licensing Appeal Board.

A Mm-hmm.

Q I would like you to review Page 4056, line 15 over to 4057, line 1. And then I am going to ask you if you would like to reconsider your prior answer.

A (Witness reading.) I certainly was not willing to accept intensity seven then, was I? I agree with you.

Q What has changed and allows you to accept intensity sevens now?

A In order to answer that, I would like to see Exhibit 1 if I might. Could we have it? I would like to check the number of those earthquakes and if, if it is indeed just a very small handful I might well retract that statement.

Okay. A lot depends on which area we're talking about, we're focusing on what I have called the Boston/New Hampshire region there. I list in Exhibit 1 three earthquakes of which two were in 1949 together. There is a substantial question whether those two were a single event or two separate events.

I think probably it would be better to list them as one event. This is -- this is kind of uncertain. We have a total of three which could logically be reduced to two. This is kind of uncertain and I would retract my statement then, Mr. Dignan. I would say that sevens in that area are -- there is not enough. In other areas there are enough but not in the

Boston/New Hampshire zone.

Q Now, Page 4 you also point to figure one showing a map of the epi-centers of earthquakes listed in the Smith (1962, 1966) catalog.

Doctor, a preliminary question: Are you the draftsman of this map or was Smith?

A Smith.

Q Okay.

A I, I drafted the little lines on it but the points are Smith's and in fact there's an error in plotting the 1940 earthquakes, I think.

Q Okay. What years did this data, does this data cover in the Smith catalog? There is no date on the map.

A He, he went all the way back to 1530 or maybe in the sixteen hundreds. 1534 was the title. I believe the more recent catalog of Chiburis and so forth is much better in terms of some of these other earthquakes.

Q All right. Now, so I can be clear, you did not use all of the earthquakes in this map inside the dotted lines in your analysis, did you?

A No, I only went back to eighteen hundred.

CHAIRMAN ROSENTHAL: Mr. Dignan, we'll take a ten-minute recess at this point.

(Morning recess.)

CHAIRMAN ROSENTHAL: Mr. Dignan, you may resume.

MR. DIGNAN: Thank you, Mr. Chairman.

Q (By Mr. Dignan) Doctor Chinnery, I would like to take you to page 7 of your testimony, if I might, and just above the heading "Frequency Intensity Relationships," there's the statement: "In my view, the most reasonable and most conservative assumption is that the seismicity of the Boston-New Hampshire zone is a valid basis for estimating the risks at the Seabrook site." And I'd like you to concentrate on the words "most reasonable and most conservative." In your view, as you approach these matters, is it always the most conservative assumptions that are most reasonable?

A I have a little trouble with that word, Mr. Dignan. I never quite know what it means, and --

Q Well, you used it, Doctor.

A I did.

Q And I'm asking you to tell me what it means.

A That's what I need to do, is define the word as used there. I'm not sure that that word is necessary there. I'm willing to omit the term "most conservative" and stand on the term "most reasonable."

Q Well, is, in fact, this the most conservative assumption, whether or not it's the most reasonable?

A I don't believe so.

Q So you would like to withdraw from your testimony the

words "and most conservative"?

A If by conservative we mean the assumption which leads to the highest possible seismic risk at the site. And I certainly don't mean that. So if that's the way you interpret that word conservative, then let's strike it out -- I think it will be simpler -- and just leave most reasonable.

Q Doctor, I'm not interpreting it at all. I'm asking questions. Do you want the words in or out?

A I'll be quite happy to remove them from that particular context.

DOCTOR JOHNSON: May I interrupt, Mr. Dignan?

MR. DIGNAN: Yes.

DOCTOR JOHNSON: I'm confused, Doctor Chinnery. You said here that it was the most conservative; and are you changing your testimony that now there is another interpretation of the seismicity in this part of New England that would lead to a more conservative estimate of the seismic risk at Seabrook, and that is also reasonable? I think reasonable and conservative -- you -- you've paired them. I think that's a good thing to do, but is there not another reasonable but more conservative region that you would use?

THE WITNESS: Well, to give you one example, in -- and Doctor Trifinac's testimony, which has not yet been admitted, he does discuss a whole variety of seismicity models, some of them with a substantially increased risk over the one I have

considered. So he clearly feels that there are some others, whether they're correct or not; but they certainly lead to a higher risk and, therefore, you could in that sense say that they are more conservative.

DOCTOR JOHNSON: Well, I was asking about your statement right now. It's your opinion that I'm -- I'm trying to determine.

THE WITNESS: I know.

DOCTOR JOHNSON: And are you aware of a more conservative region to consider other than the Southern New Hampshire-Boston region which is reasonable for the Seabrook site?

THE WITNESS: I'm really saying that the word conservative means different things to different people; and, therefore, it probably raises more problems than it's worth.

DOCTOR BUCK: Well, what does it mean to you?

THE WITNESS: To me it means not too much different from reasonable; but if you indeed interpret it in terms of -- of the actual risk at the site, I think that's raising a question I did not intend to raise in that particular statement.

DOCTOR BUCK: Well, what does conservative mean to you? You say it's not much different from reasonable. Well, what does it mean?

THE WITNESS: I had in mind that you can take, for example, larger tectonic provinces. You could, for example,

take the whole of Southern New England, and you could include Maine, and do the same thing that I have done. Now, I do not think that that leads to a valid interpretation, because I believe technically in the northern parts of New England we have a very incomplete record of earthquakes.

If you were to throw that data in without any considerations, you would conclude the overall seismicity was less. I think by choosing the particular area I did, I achieved a -- a -- what is to me the best scientific compromise in trying to find an area where we have reasonable data, which does include the Seabrook zone. So I used it in the sense -- I think it is best expressed by the word most reasonable.

MR. LESSY: Excuse me, Mr. Chairman. May I just ask to clarify a question here, or shall we wait until the point that Mr. Dignan stops?

CHAIRMAN ROSENTHAL: If Mr. Dignan has no objection.

MR. DIGNAN: A round-table discussion is fine with me.

MR. LESSY: Okay. Doctor, the phrase most reasonable --

DOCTOR BUCK: Is your microphone on?

MR. LESSY: Can you har me now? I'll -- all right. Thank you.

Just -- sorry for the interruption. But the -- the phrase most reasonable and most conservative assumption, Doctor Chinnery, was -- was that your language, most conservative assumption, or was that suggested to you to be put

into the testimony at this point?

THE WITNESS: That was my language.

MR. LESSY: That was your language?

THE WITNESS: Absolutely.

MR. LESSY: And you meant it more or less synonymously with the word reasonable?

THE WITNESS: Yes. I was thinking of one particular set of traces, and now that I think about it, it's a very much wider range of traces, and I don't want to imply that what I have chosen here is the assumption that leads to the highest risk. I'm quite convinced that it's not so. So I would rather remove that word than give that impression.

MR. LESSY: Sorry to interrupt. That saved me about five questions.

DOCTOR JOHNSON: I think the Board is finished with its interruptions as well, Mr. Dignan.

Q (By Mr. Dignan) Now, in response to Doctor Johnson earlier and one earlier question to me, you talked about Doctor Trifunac's testimony that is going to be offered in this proceeding. I take it you reviewed that testimony in detail?

A I have read it, yes.

Q Did you read the conclusion expressed by Doctor Trifunac?

A Yes, I did.

Q Did you agree with it?

A I -- I shall have to find a copy of it. Here it is. One second, please.

Q It's on page 10 of his statement.

A Yes, I have it here. I agreed with everything he did up to page 9, Mr. Dignan. I don't think that I would agree with the rather forceful way -- no, actually, he says the word may be acceptable. In that case, yes, I can go along with his conclusions.

Q Do you go along with Chinnery's inclusion?

A With Trifunac's?

Q Excuse me, with Trifunac's conclusion.

A Yes, with emphasis on the word maybe, it would be acceptable.

Q Yes. So you think that the present design -- well, let me not characterize. You believe that the proposed SSE design spectra for the Seabrook site corresponding to 0.25 G peak acceleration may be acceptable?

A May, yes.

Q Doctor, are we engaged in this exercise so that we can remove the word may from Doctor Trifunac's conclusion? I mean, is it that you and Doctor Trifunac are just not yet satisfied with your work to date that indicated that it may be acceptable?

A I think that is one way of phrasing the problem that we have before us, yes, Mr. Dignan. Could I elaborate on that a

fraction?

Q We're doing fine. I'm thinking of saying no more questions right now.

A Could I elaborate on that for a second?

Q Certainly. Go ahead. You should address your questions to the Board, Doctor. They run the hearing.

A In my view, the net conclusion of Doctor Trifunac's testimony and mine is -- and this is as near as I can get to it -- that the risk that the safe shutdown earthquake or, in fact the design of ground acceleration will be exceeded during the lifetime of the -- well, not during the lifetime. The risk that it will be exceeded lies somewhere in the range of ten of a minus three or ten of a minus four per year. Now, I interpret that from my own calculations as to how the safe shutdown earthquake may be exceeded, and I interpret that from Doctor Trifunac's conclusion for the ground acceleration being exceeded. The question is is a risk of somewhere ten to the minus three and ten to the minus four an acceptable risk, and this is why I concentrated on the word maybe. That is not a scientific decision; that is a regulatory decision.

Q You all through?

A Finished. Thank you.

Q Now, on Page 8 of your testimony -- and I would like to direct your attention to the part that appears on linear frequency and data intensity. You say in the third line: "Clearly, the data are sparse. For the period 1800-1959 only six data points are obtained for intensities II to VII."

Now, there are six data points from II through VII. When you say the data is sparse, are you talking about the number of earthquakes rather than number of data points?

A Both.

Q Well, you are only going to have data from II through VII in any event, isn't that correct?

A Not in some other areas but in this particular area.

Q In this particular area?

A That's right.

Q Okay. Now, but you also thought the number of earthquakes were sparse?

A Yes. I think there is not that many.

Q Now, indeed, didn't you testify earlier that you now believe that the seven point, the Roman seven point is probably no good because there were so few earthquakes involved, right?

A Yes. I say that in the bottom of that paragraph.

Q Okay. Now, you then go on to say "The remaining four data points actually lie in a relatively good straight line but

the slip of this line (about 0.50) is, as we show see below, unnecessarily low, and would lead to my estimates for the rate of occurrence of large earthquakes."

Well, that may be so but why does that give you a reason to change the slope if your theory in fact is valid?

A Agreed. I am working on a conclusion which I reached in my 1979 paper on one that McGuire reached on his paper to 1978, I believe. That the best way of approaching seismic risk for those kinds of facilities is to assume a uniform slope throughout the Eastern United States. This is quoting the words I believe of Dr. McGuire. That was the conclusion of my 1979 paper.

Once I have a uniform slope, then I do not attempt to put a line through this particular data that fits that data set. I look at how that data set compares with the slope that I've determined as being applicable to the entire Eastern U.S.

Q Well, Doctor, you said "agreed" at the start of that answer.

Did you mean to, by saying agreed, agree with me that that simply the fact that the slope would give you a high estimate did not give you a scientific basis for changing the slope?

A I was not willing to use the value of point five which would have given it larger risk of large earthquakes because I don't believe that point five is in fact the right slope to use.

Q Well, not --

A If one were to use it, one could.

Q Yes. And of course if you put point five in there --

A Mm-hmm.

Q -- and rested your testimony on it --

A Mm-hmm.

Q -- your data wouldn't fit the eights of Cape Ann that are left out of the, as you neatly fit them in later in your testimony?

A The conclusions have to be reached. I take it you're talking about in terms of the conclusions in my '79 paper?

Q I am talking about your conclusions in your testimony that having gone through your exercise, you then say now if we pick up the Cape Ann earthquake or earthquakes that were left out, we'll see that their occurrence fits in nicely with our slope. But it wouldn't fit in so nicely if you had stayed with the slope which you apparently got to with it the point five slope because it would have occurred much sooner and that slope would have, excuse me, much later then that slope would have projected?

A No, it would fit better, Mr. Dignan. If you refer to my '79 paper, which is Exhibit -- I don't know, 2, I would imagine, I'm not sure -- is that right? Exhibit 2? The principal conclusions of that paper are shown in Table 5 on Page 769. And in that Table for the Boston/New Hampshire area, I have

listed my estimates for return periods of earthquakes of various sizes. And I've also listed the calculated probability that one event of each of those sizes will occur during the three hundred years historical record.

Now, If I put a point five slope through the data points shown on Page 9 on my testimony, what happens is that the turn period for an intensity VIII earthquake becomes less. They occur more frequently; predicted to occur more frequently.

Q That's right.

A So instead of them occurring every six thousand years, they will occur at some other period which is perhaps closer to two thousand years. I don't know but it will be roughly around then. Therefore, when I compute the probability that they will be one of intensity VIII in a three-hundred year record, I will have a higher probability.

Q I agree you will get a higher probability.

A Yes.

Q There is no question about that.

A It makes the data more consistent rather than less.

Q Now, it does not make it consistent with the larger events that you left out and that you neatly plug into the slope you have given us which is the not point five, to wit, a 1727 and the 1755 events.

CHAIRMAN ROSENTHAL: Is that a question or a statement?

Q Questionmark at the end. Isn't that so?

A I -- let me ask you to rephrase, to say that again. I didn't quite follow your argement.

Q Take a look at your figure which follows Page 14 of your direct.

A Okay.

Q Now, by using the .57 slopes, that line goes neatly through the center of the 1755 earthquake, does it not?

A I have done there what I said before. We had a break that one shouldn't do and I have plotted that earthquake as if it occurred once every three hundred years or once every two hundred years to be exact. That's why it's left as an open circle but perhaps that is not explained fully in the text. The question of how you --

Q The question is very simple. Am I right in understanding the line goes right through essentially the center of that rectangle?

A Yes.

Q If it's a .57?

A Yes.

Q If it's a point five it comes through a very limited outer edge of that rectangle, does it not?

A Yes, that's true.

Q If it is no where near as good?

A I disagree because I don't believe we know the intensity of those earthquakes that accurately, Mr. Dignan. That's why

it's put as --

Q All right, Doctor. You want to raise or lower those intensities in your judgment?

DR. JOHNSON: Excuse me. I'm having a problem. You keep referring --

MR. DIGNAN: Epi-central intensity, Doctor.

DR. JOHNSON: Yes. But there is on one, the 1955 -- excuse me, the 1755 Cape Ann earthquake plotted you keep talking as if another Cape Ann earthquake is included on that plot but it's not.

MR. DIGNAN: Oh, I am aware of that, Doctor, and I intend to follow that down.

DR. JOHNSON: All right. But what you're using a plural in both questions and answers and we're referring only to a -- I mean we're referring to a single earthquake. That's my problem.

MR. DIGNAN: All right, Doctor. Mr. Smith studied another or his study showed it was in 1727?

A Yes, he did, I'm sure.

Q How did you account for that?

A I did not attempt to.

Q Well, if it's an VIII and 1755 is an VIII, it was a return period between the two of them for thirty years.

A I have not attempted to judge whether those values are correct for either earthquakes, Mr. Dignan. I know Dr. Holt has done that and I don't think he's reached that conclusion

and I have not reassessed all of the evidence to a --

Q Well, if you can't tell me that you're satisfied that the Smith, I think we both agree he called them IXs.

A Yes, I think he did.

Q How good is your data in your six point? How do I know that those were actually sixes in that point, Doctor?

A I cannot argue with you. I think that these data have been looked over by Western Geophysical, and I think that we have a new set of data now and I don't believe the conclusions you reached from the new data set are substantially different from the ones you reached from the old data set, and this is shown in my rebuttal testimony.

Q What my simple question is, is this, Doctor: If you will not accept Smith's IX --

A Mm-hmm.

Q -- you wouldn't accept the VIIIs, why is it that you accept the IVs, the Vs and the VIs?

A I did not say that I would not accept them. What I said, Mr. Dignan, is that I had not reexamined the data myself to check out whether those were in fact valid numbers.

Q Did you re-examine the data on IV, V and VI to determine whether they were valid?

A No, I did not. I am not in the business of going through that kind of thing. However, I feel I could use the catalog that had been compiled very carefully by Western Geophysical

and shown in my figure three and my rebuttal testimony is the data that Dr. Holt came up with, I think agrees extremely well.

Q Well, let's stay with the direct testimony which is, as I understood it, like the '79 paper was based on the Smith data?

A Yes, it was.

Q I appreciate the compliments to Mr. Holt's but --

A Yes.

Q -- but the fact of the matter is I would like to stay with your testimony.

Now, did I just hear you tell me you really have no basis for telling me that any of these data points which are derived from Smith's data are in fact valid because you have not investigated to check it out?

A I have not investigated any individual earthquakes intensity value in that catalog.

Q All right.

A You're quite right, Mr. Dignan.

Q And that may be valid science, I don't know, but then if you did that, why didn't you accept Smith's IXs and VIIIs and plot them in that way for consistency sake if for nothing else?

A Because I chose to plot off the eighteen hundred and these are earthquakes that happened in the seventeen hundreds.

Q Right.

A That's why they don't appear on the plots.

Q And of course these earthquakes do get back to something we discussed earlier ones. One does plot them as Mr. Holt has done, it does, to use a phrase that maybe I shouldn't have started using, startling things to the line, doesn't it?

A Only if you plot them with a recurrence times equal to the length of time between those earthquakes and the present which is not the correct way of doing it.

Q Well, I could also plot them I suppose thirty years apart. That's the difference in the two, 1727 and 1755 or twenty eight years to be exact, is it?

A Mm-hmm.

Q That's what is known as imperical occurrence time, I would think?

A You cannot use the data to establish the recurrence time after intensity VIII earthquake. I don't think you can use the Smith catalog and I don't hink you can, the Chiburis catalog either. There are not enough events to establish that.

If you cannot establish that, that is not a valid point on any of those graphs.

Q Well, if I can't use the data to establish the recurrence time of an VIII, what are we doing here, Doctor, because isn't that what you did use this data to establish a recurrence?

A I did not use the intensity VIII data to do that.

Q Used on the IV, V and VI data?

A Yes.

Q Which is as far as you're concerned no better or worse than the intensity VIII data?

A There's a lot more of it, and scientifically more one would expect that averaging effect of taking a lot more events will give you something worthwhile so when, when you have a lot of earthquakes, I think the number of intensity Vs, for example, within that period is fourteen; still not large but I think large enough to divide that period from 1800 to 1959 by fourteen. It starts to be meaningful. And the return period you get certainly is not exact but it is not going to be drastically in error and this is probably true even should some of those Vs be IVs and some of the IVs be Vs.

In other words, you have enough earthquakes there is an averaging process which tends to take the count of at least some of the error in the catalog.

Q Now, Doctor, why is that you changed from putting the best fit lines through IV, V, VI and VII as you did in the prior proceeding in this case?

A On the basis of the 1979 paper, I decided that I subscribed to the proposition that there is not a significant variation in "B" value in the Eastern United States, not one I could find convincing to me. Therefore, I looked for the "B" value which would fit or be consistent with the data from

a whole variety in the Eastern U.S. and then I used that in the particular area I was concerned with, the Boston/New Hampshire area.

Q So you started out, then, if I hear you correctly, assuming that there had to be a consistent "B" value involved here? This is not something that you did arrive from from an analysis or calculation?

A Science does not quite work that way. You start out looking at what you have when your data lines up and I refer you to Figure -- the two figures on Page 766 of my 1979 paper, on the basis of that information I concluded the most reasonable assumption would be a uniform "B" value in these areas.

And as I say, the similiar conclusion was reached on quite a different basis by Dr. McGuire in his paper.

Q What if that assumption was wrong? What does that do to your theory?

A Well, one obvious thing it might do if I were not to go to the Boston/New Hampshire area with the value of .57 in mind, I might feel obligated to put a point five slope through, as we have commented,and that would lead to increased risk estimates using the same method. That would be the effect.

Q And increased risk method, that I think you'll agree with me, even you would conclude were, were not likely?

A Well, I, I simply feel that one is searching for a reasonable solution to the problems, and the most reasonable to

me is if fact A slopes uniformly that it's .57 rather than an apparent point five, I think the difference between those two numbers is really not that large. You could end up with somewhat different risks for an intensity VIII earthquake or larger but I think that within the, the kind of uncertainty that we're forced to deal with as to the problem.

Q Now, now, Doctor, in doing your work for this testimony and in, also in your 1979 paper, which is Exhibit 2, you deliberately chose the period 1800 to 1959 in order to exclude Cape Ann 1755 events, didn't you?

A That was certainly a measure range where I think the other reason is that I really find it hard to believe the catalog is, is very reliable before 1800 but that's a very arbitrary cutoff point. I agree.

Q Well, the reason I -- I put it so strongly to you, Doctor, is that in your paper on pages 761 to 762 you state, "since we wish to exclude the 1755 Cape Ann earthquake from the data set, events have been accumulated in both the Southern New England region and the Boston-New Hampshire zone for the period 1800 to 1959."

A Yes.

Q Now, I understood, then, from that that that was the reason you picked this time, to get the Cape Ann earthquake out of the analysis?

A Yes, that -- that certainly is the principal reason.

Q Now, what would happen to the line had the Cape Ann earthquake stayed in the analysis or the data point of VIII,, or VII or whichever way you wanted to include it?

A We are coming back to the same argument we had before. I would not have considered it valid to plot the Cape Ann earthquake on.

CHAIRMAN ROSENTHAL: The question, Doctor Chinnery, is not whether you thought it valid to plot the Cape Ann earthquake or not. I think you've made it clear that you did not think it was valid. The question is what would have been the consequences in terms of the line had the Cape Ann earthquake been plotted.

A Had it been plotted at a return period of about 200 years, then it would have raised the bottom end of the graph. It would have led to a smaller slope. And any subsequent

calculations of seismic risks would have led to larger probabilities of large earthquakes.

Q Now, so we can understand each other on what raising the bottom of the line means, if Cape Ann went in -- let's assume you put it in as an VIII. It would create a new data point at VIII that doesn't exist; and, in addition, it would raise the data point on VII, would it not, because each of those points plot all earthquakes of that epicentral intensity and any larger ones; isn't that right?

A Yes, it would, but by an extraordinarily small amount. This is a logarithmic scale, and it would hardly show.

Q This is what you and Doctors Buck and Johnson do so often, you draw it on a logarithmic scale because it's always a straight line, and that's why lawyers don't belong in this case.

But, in any event, what I'm getting at, the inclusion of that event would not simply create a new VIII point or just raise a VII; it would have an effect on every point in the line. And I could agree with you that it's smaller and smaller as you get up there. Is that right?

A Yes.

Q So when you say the line would curve up, it would simply mean that the last data point would curve up; it would start into a curve that, I guess, some mathematicians call a quadratic?

A It wouldn't quite be a quadratic. It would be an "S"

shaped curve.

Q Doctor, I'd like you to turn to page 10 of your testimony.

A Mm-hmm.

Q I would like to direct your attention to the particular statement, "The vast majority of seismologists have accepted the linearity of frequency-magnitude data as a working hypothesis." Now, first of all, I want to ask you a preliminary question. Did you mean frequency-magnitude there or frequency-intensity?

A I meant frequency-magnitude.

Q Okay, fine. Doctor, what is your definition of the phrase "working hypothesis"?

A You're asking me to define the scientific method, I think, which is a little difficult to do. When in -- in any endeavor of science, you start out with some kind of graph, with the data points on it, and they seem to form a straight line, it's very reasonable to start out and say okay, they look roughly straight. Let's work on the basis that we have a straight line.

Now, the method of science takes that as a starting point and uses it to make predictions to design experiments, to collect more data, and, as time goes on, using it in that way more data comes in; that data may or may not substantiate the original working hypothesis. The more data you get, the more confident one becomes that it is more than a working hypothesis. And

perhaps you might get enough data where you actually call it a natural law. So a working hypothesis is the first stage in the development of a natural law.

Q Okay. Now, you have said here today that a vast majority of seismologists have accepted the linearity of frequency-magnitude data as a working hypothesis. Now, I would just like to get an idea of the universe from which this vast majority comes. How many seismologists are you talking about there? Approximately. I'm not asking you to name them or anything. Are we talking ten men; are we talking 50 or what?

A Well, there are some -- perhaps a thousand seismologists in this country and perhaps several thousand in the world. When I say "vast majority," it happens to be all the ones I know, which certainly is not the total number.

CHAIRMAN ROSENTHAL: How many of those thousands in this country do you know?

THE WITNESS: A fair number. I suppose something of the --

MR. DIGNAN: Mayor Curley counted majorities in Boston that way for years, too.

THE WITNESS: Of course, out of all those seismologists, there aren't that many that have addressed this. I would say the order of a hundred. That's the number that I would pull out.

Q Now confronted with your vast majority statement, Doctor, I tried to read some of these technical papers,

probably without success; but I want to ask you this question: This vast majority that has accepted it as a working hypothesis, is it not so that what they have accepted it as a working hypothesis for is to classify different seismic regions rather than accepting it as a working hypothesis in an exercise to predict maximum earthquakes?

A Yes, mostly seismologists are not working in terms of earthquakes or seismic risks; they're working in other fields of seismicity trying to understand the generation of cone, and it's in that context that they will use it.

Q Now, other than yourself, sir, which has accepted it for the purpose which you are using it?

A I'm sorry, I didn't understand. Was that the end of the question?

Q Yes.

A Other than myself?

Q Yes, for the purposes for which you are using it as opposed to some other purpose.

A You're taking a sentence which refers to frequency-magnitude data, which is not in the context -- we're talking later in terms of frequency-intensity. So are you referring to frequency-magnitude still?

Q I am.

A In the context in which I used it there, that is a general statement that applies to across seismology. I'm not

sure what you're getting at, Mr. Dignan.

That paragraph there is discussing the frequency-magnitude problem, which is a very different one perhaps than what we're discussing in this particular proceeding.

Q Now -- I will take that answer. Now, down below you said, "Of what scientific literature there is, the vast bulk assumes that frequency and intensity relationships are linear." Then you say, "See, for example, references quoted in Chinnery 1979." Do you see that statement?

A Yes, I do.

Q Well, to start with, I couldn't find any quotes in Chinnery, 1979. Do you mean to refer to all the references signed in the article as opposed to quoted?

A Yes.

Q I assumed that. And I read --

A I'm sorry.

Q No, that's all right. I read them. And again I'm going to ask you, isn't it true that what they were doing was looking at frequency-intensity just as before we talked about frequency-magnitude and the -- and the acceptance of the linear theory was in the context of classifying seismic regions as opposed to trying to predict maximum earthquakes?

A I don't think the question of linearity of frequency-intensity or magnitude data has anything to do with the question of predicting maximum earthquakes, Mr. Dignan. This is a

question of whether you can take seismic data from a region and categorize it by a linear relationship.

Q Well, Doctor, I understood that the exercise you had engaged in -- and please correct me if I'm wrong -- was that you were coming to a conclusion which you asked this Board, and ultimately the Commission and courts, to accept, which is that there is a likelihood of 10^{-3}, 10^{-4} that a certain maximum earthquake will occur at Seabrook and the result will be a nuclear disaster. Now, if -- if I missed the boat, and you are not here to try to tell us that we picked the wrong earthquake, please enlighten me and I'll go home. But, that's what I thought you had done here.

A That is not why my argument is, Mr. Dignan, no. I don't think that just because the safe shutdown earthquake is exceeded or that the design acceleration is exceeded that the plant will fall down. That is a whole different question. It's an engineering question. It has nothing to do with my particular calculation.

Q Forget it. But, you are here to predict that the safety shutdown earthquake should be something higher than the one now, am I right or am I wrong?

A No. What I'm here to say is that there is a risk that the safe shutdown earthquake may be exceeded and that risk is, as near as I can make out, in the range 10^{-3} to 10^{-4}. So what that translates to, in my layman's language, is you are

saying there could be an earthquake greater than VIII, intensity VIII, once every thousand to ten thousand years?

A Yes, I'm saying that we do not know enough to rule that out.

Q All right. So that, to my untutored mind, constitutes a prediction of the maximum earthquake potential, to use a phrase right out of the regulations; is that right?

A It's saying that the -- if you really want to know the largest earthquake that could ever occur in New England, I -- clearly the historical record is not enough, and we have to go to other ground on which to establish that.

Q And to do that, you used this frequency-intensity relationship --

A Yes.

Q -- and said -- and assumed it was linear?

A Yes.

Q And my question to you now, sir, is very simple. This vast bulk of scientific literature that assumes the frequency-intensity relationship is linear, is it not true that that literature is driving at the solution to a different problem than you are and, I suggest, where the assumption is not so critical as it is to your theory and your endeavor?

A I think many of those studies -- in fact, you're quite right -- are not aimed specifically at nuclear power plant siting safety, but that does not mean to say that they

are not equally valid, and the best knowledge we can lay our hands on.

Q My point is that they aren't even aimed at predicting a maximum earthquake, are they?

A Establishing the linearity of a frequency intensity curve does no more -- does not help you in establishing the maximum earthquake, Mr. Dignan. I have to separate those two things. They're different to me.

Q Yeah, but the -- the linearity of that curve is an assumption which underlies the results you give us here today, is that not true?

A There are two -- more than two, but these are two crucial things. First of all, that the slope is linear, that the line to relationship is linear; and that, secondly, there's a whole discussion as about how far that slope continues.

Q My point is, then, your assumption that this curve is linear is critical to the results which you reach and give us here today, is it not? I believe you already said so. I'm not trying to trap you here.

A Yes, I think it is.

Q All right. And that being the case, Doctor, is it not true that where those other authors were using that assumption, the assumption of linearity of that curve was not critical to the results they were seeking?

A I think if I were to quote the results of the Tera

study, for example, that there the people doing that study were well-aware that it was being used for safety purposes.

Q The Tera study, Doctor, was not done when you wrote your '79 paper, and those are the references you're citing here and that you're relying on?

A Yes, that's true.

Q Okay. And my question to you is, my review of those references indicated that they were not designed at reaching conclusions in such a way on a given subject that that assumption was as critical as it is to your conclusions being reached on your subject here today; and isn't that so?

A I would have to admit that's true, Mr. Dignan.

J-1

Q Doctor, would you go to Page 12 of your testimony?

A Mm-hmm.

Q And I would like you to concentrate down towards the bottom of the page. You say, you're talking about the Tera Study --

A Yes, mm-hmm.

Q -- and you say "These are listed in Table 1 and illustrate," then you say, "There is little point in averaging opinions such as these." Why? Because you follow it up with, "notice, however, that five of the ten experts admit the possibility that the upper bound to earthquake size may be X or greater in this region."

If it's invalid to average, what validity of statement of yours of that follows the region of average?

A I think, I think the thing that the Board has to consider is not what my opinion is or what Dr. Holt's opinion is. It's what the range of scientific opinion on this particular topic is. It happens to be one that's been particularly subjected. We have very little clear scientific evidence which can uniquely tell us what the largest earthquake that can occur in any area is so we're down to opinion.

The best one can do is look at a variety of experts and look at the range of opinion which is involved. Now, what I quoted there was that five out of the ten experts clearly were unable to convince themselves that an earthquake larger

than X or larger was completely ruled out in this area. That's the way I rule it.

Now, to me that is a very valid piece of information for the Board in making its decision.

Q But an average of best estimates is not?

A No. I didn't think an average of estimate is not at all.

Q But you consider the fact that five --

A These are opinions. Mr. Dignan, it's like averaging the democrats and republicans. I don't think you can do that.

Q You're likely to get an independent.

The five of the ten experts you think is a more valid thing for the Board to focus on than any average, is that right? Is that what you want to leave it as?

A I think if five out of the ten experts could not rule out the possibility of an intensity X or larger, that is a fact that should be used in the regulatory process. However, it should be used, I think, the -- it would be of use to the Board.

Q What should the Board do with the fact that assuming your version is right that the VI, VI to VII and the magnitude scale is in the range of VIII intensity, that one, two, three, four, five, six of these experts, according to your table, have a best estimate of an VIII. Is that a valid piece of information that they should consider?

A I think certainly they should but I think the other one is more reasonable because the question before us is not to get in -- my view, the question is not to take an average; an opinion is to find out what we know about this problem. And what this table indicates is that we have a real problem trying to define the epigone intensity. Now, that's the way I interpret that.

Q Incidentally, Doctor, the information that you produce on Page 13 came from the Tera Report --

A Yes.

Q -- Page II-19, am I right?

A Good question.

Q If your counsel can, can he equip you with a --

A It's in my box there.

(Documents handed to the witness by Attorney Jordan.)

A Yes, indeed.

Q All right. Now Page II-19, the data on that is in answer to question 2-2?

A Yuh.

Q And that dealt with the unconstrained time period, did it not?

A Mm-hmm.

Q Did you bother to take a look at what they, the highest estimates were if the question was confined to the next hundred fifty and next thousand years?

A I looked over them. There are some comments of mine somewhere in the back of this thing that explain my attitude. I could not answer those questions.

Q Well, just let me ask you this inasmuch as the Tera Report is going to come in evidence in this thing and inasmuch as I think you will agree that Seabrook is going to only be around something less than a hundred fifty years. Do you think it would be valid to look at a, look at the answers that came up on that question in making this determination?

A I, I object to -- not to you but object to the way in which that question was phrased to the group of experts. This is my trouble here.

Q The question is a very simple one, Doctor. We've discussed the philosophy you have as to what the Board should look at. Now I am asking you a very simple question: Is it valid for the Board to look at the best estimate of the highest estimate, excuse me, not the best estimate, of the same experts as they look at it for a hundred fifty years? And just so it's clear, Doctor, by my calculations, one, two, three, four, five out of the eight who answered said VIII or less than VIII. You know, obviously, I'm not bringing this up to hurt my case but I'm just asking you why isn't that a valid thing for the Board to look at if you say this other information is valid for them to look at?

A The trouble is that the question 2-2 and 2-3 are really

inconsistent with one another. To me, the correct question to look at the answer is question 2-2 because I think the experts should have had a great deal of trouble answering 2-3.

Q Well, obviously you did?

A Yes.

Q But some did not?

A But pages B-1 and B-2 have the reasons for this.

Q But some equally qualified individuals did not have --

A You're quite right, yes.

Q So all I am asking you, Doctor, is, isn't it a valid exercise for the Board to take, to consider these hundred fifty years figure from Tera just to be weighed along with these high estimates and unconstrained times?

A Yes, I cannot disagree with that. I disagree, however, still very strongly with the form of that question.

Q I gather nobody was entirely happy with the Tera Report?

A Now, that particular one is a particularly difficult one. You can, you know, interpret the -- you see it would be what's the size of the eathquake, whether it has a return period of a hundred fifty years, and I'm sure that's what several of the experts in fact did. That's not the way it's phrased, however, and if they did interpret it that way, they clearly did not do so grammatically as they always say.

In view of the difficulty and interpreting the question, I think the question 2-2 is much better because it's a much

clearer, well defined question.

Q Well, let's -- that's an opinion you're entitled to hold, I guess.

A Mm-hmm.

Q Now, in that -- in the question 2-3, which appears, the answers appear on page II-23 --

A Mm-hmm.

Q -- and the column of interest, with the exception of one expert, is the column that with a row that begins number 8, right, Cape Ann?

A Yes.

Q I just want to be sure I haven't incorrectly interpreted this table. It's my understanding that in that row the top figure is the answer to the question B, a hundred fifty years --

A Mm-hmm.

Q -- and the bottom figure then there is a top and bottom, is the answer to a thousand years?

A Yes.

Q Where there's just one statement in the middle of the block that indicates that the expert did not discern a difference between a hundred fifty and ten thousand years?

A Yes.

Q And I also understand that expert number 8 never accepted zone number 8 in that his proper answers to this question were found in the column or the row headed forty seven

which was the seismic zone he elected to deal with?

A Yes.

Q Is that correct?

A Yes.

Q Now, is it fair to say that the highest number in there are yours?

A I did not answer that question.

Q Oh, you were, you were the expert, Expert 5 --

A Yes.

Q -- who declined in answering --

A Yes.

Q -- to that question? Okay. And do you know what -- well, did you ever give an opinion of any kind on a shorter return period than unconstrained time?

A No, because I did not assume -- I could not answer those questions within the way they were phrased.

Q Now, on the unconstrained time in Cape Ann, you said XII was the right number, did you not?

A Yes.

Q Okay. If that be so, why is it that you are not here trying to persuade us to build this plant to a XII?

A I think, although I have the feeling and all I think of my friends, including this group of experts have a feeling that a XII may occur so extremely rarely that in fact it is not a valid thing to worry about in New England.

The trouble is that even though we have a feeling like that, we have no scientific evidence so the question is what to do about that.

When somebody asks me a direct question what is the largest earthquake that could ever occur in this zone, I have to answer it -- I cannot see convincing evidence to rule out the very infrequent occurrence of an intensity XII.

Q And so --

A So this is why I answered the question XII.

Q And, Doctor, isn't it a fact that assuming that every theory that you have laid out here is so and you follow it --

A Mm-hmm.

Q -- religiously, your answer to that question would have to be a XII no matter what area of the world I asked you about?

A I have not found an area that, yes, that I could say anything different. Yes.

Q So what you, your ultimate conclusion really is that every nuclear power plant should be designed to a XII?

A No. Only in the case that you use the orthodox or what I originally describe as the determinance approach to Appendix A which is, would be true but you are quite right if you simply take the larger earthquake that can occur in the tectonic province containing this site and you follow, strictly speaking, the wording of the Appendix A and you place that

earthquake at the site, you take no account of the fact that it is introduced, rare event then you're right. You have to build all the nuclear power plants for XII.

I am arguing very strongly that that is not the correct approach to Appendix A. Now, when you introduce the idea of probability, when you introduce the idea that this is a very rare event with a risk which is nonzero but still well below any perceptible level,then you do not end up with a XII as a design criterion.

Q Well, Doctor, we all agreed that none of us know about a XII in New England in the last three hundred years, right?

A True.

Q What's the guarantee that the time isn't coming tomorrow if your theory is right?

A There's a probability involved. There's no guarantee. The probability for intensity XII I have not continued to graph on but it is very, very low.

Q When we talk about reasonable or conservative assumptions, doesn't it look, from your theory, that every nuclear power plant could be approached in this country to resist a XII?

A No, because probabilities are --

Q All right.

A -- involved. Just as one will make sure that the risk of an aircraft hitting the plant is less than a certain amount. One can never make it exactly zero. And it's the same thing in

terms of the safe shutdown earthquake. So one has to pick a level at which one says the risk here is low enough; it's acceptable.

Q What do you deem low enough risk that's acceptable?

A I would go along with a number I have heard mentioned in a variety of different contexts which is a total risk of actual failure of the plant of 10^{-7} per year.

Q And what earthquake would we have to design Seabrook against to 10^{-7}?

MR. JORDAN: I object. I don't believe there is any basis for Dr. Chinnery being qualified to make that assessment which goes far beyond the hypothesis and evaluation of what earthquake probabilities actually are.

CHAIRMAN ROSENTHAL: I think that's probably right. However, he did answer without objection the question as to what he regarded as the acceptable level of risk and I think that having answered that question it was fair enough for Mr. Dignan then to ask him for what I take it is a mathematical computation as to what the intensity level at the plant would have to be designed to against, in order to whether the level -- is that the question you asked?

MR. DIGNAN: That's what I did.

CHAIRMAN ROSENTHAL: So I think, you know, having answer that first question, the second question is a perfectly appropriate one. This objection may have come one question too

late.

MR. JORDAN: Well, with all due respect, your Honor, it seems to me that the question of what the probability of failure is after the earthquake occurs is far more than a mathematical calculation of some sort.

What Dr. Chinnery gives up in effect is shall we say 10^{-3} of an earthquake in a year. The question then is given the earthquakes an enormous technical question of what the probabilities have all along the line.

CHAIRMAN ROSENTHAL: He --

DR. BUCK: He's already answered that in the intensity XII is unacceptable so something must have happened to the reactor in the meantime. He's already given that as saying that an intensity XII is unacceptable.

MR. LESSY: Mr. Chairman, if I just might venture in this discussion at this point, I was looking at Page 15 of Dr. Chinnery's rebuttal. The level of testimony, the last two sentences, in which he says and I quote, "In this case, the annual risk at the Seabrook site might well be sufficiently small over the lifetime of the plant structure that it may be disregarded, and a smaller SSE may be chosen. In particular, we can estimate that the probability of occurrence of an intensity IX event is roughly 10^{-3} per year in the province containing the site. A consideration of overall risk (perhaps 10^{-7} a year, as used by Farrar) and substantial safety factors

may then lead to a choice of SSE smaller than X. Clearly, the probabilistic approach provides a much more rational approach to the estimation of seismic risk."

I think Dr. Chinnery in his rebuttal testimony ventured in the area that Mr. Dignan is now inquiring into.

CHAIRMAN ROSENTHAL: Well, in any event, I'll allow that question to stand.

Do you recall it, Dr. Chinnery?

THE WITNESS: Yes, I do. I recall I answered that in my Interrogatories. Are they part of the record or not?

CHAIRMAN ROSENTHAL: No.

MR. LESSY: They will be.

A Let me just then briefly say what is in those. What I consider one has to do is to multiply two probabilities together. One, the probability that the design ground motion and I should have to put it this way: My testimony does not go all the way towards that but the design ground motion will be exceeded and, secondly, the probability that if that design ground motion exceeded the plant fall down so the safety question becomes inevitably involved and to me the correct thing to do is to multiply these two probabilities together so if you have a safety factor of ten thousand, in other words, if an acceleration of the .25 goes, exceeded but there is only a one chance in ten thousand that the plant will fall down, then multiplying that by the probability that that exceeded

incidence will happen at all of 10^{-3}, for example, one to a total probability of plant failure 10^{-7}, under those particular set of numbers, I will conclude that the plant was safely designed.

Now, that is taking 10^{-3} and 10^{-4}. I have not myself -- I'm not aware of any actual estimate of the safety factors involved so I cannot in fact do that calculation for the Court.

Q Excuse me.

MR. LESSY: Might I have that answer repeated? There was noise outside.

CHAIRMAN ROSENTHAL: Will the reporter repeat the answer to the last question?

A The question of risk is composed of two parts, One part is the probability that the design acceleration will be exceeded and one part is the probability that if the ground motion is exceeded, substantial danger will occur to the plant.

I think both of these are small numbers. They need to be multiplied together to get the overall risk of the plant failure.

Take an example where perhaps the possibility of exceeding the design ground motion was 10^{-3} per year and an example where the safety factors involved are ten thousand and in other words giving a 10^{-4} risk of failure, I will multiply those two together to get 10^{-7} and I will conclude that the plant is safely designed.

Now, it's really pushing it in terms of my particular area of expertise to go this far but that seems logical to me as a scientist.

MR. LESSY: Thank you.

MR. DIGNAN: Am I up, or is Mr. Lessy up?

MR. LESSY: You may continue.

MR. DIGNAN: Thank you.

CHAIRMAN ROSENTHAL: Very generous of you, Mr. Lessy. This is Mr. Dignan's cross-examination, not yours.

MR. LESSY: I really had asked the reporter to repeat the question.

CHAIRMAN ROSENTHAL: Well, I understand that, but I think that Doctor Chinnery has restated his answer, so it should not be necessary for the reporter to return.

Q (By Mr. Dignan) What's your understanding of what those two numbers today would give us? Would it be an VIII? Is it designed for an VIII?

A I do not know the safety factors, Mr. Dignan. I have never come across that number anywhere.

Q Doctor, would you turn to page 14 of your testimony, please.

A Mm-hmm.

Q There you state near the top of the page, "We have selected a 'tectonic province' containing the site, which extends from Southern New Hampshire to Northeastern Massachusetts." You put the words "tectonic province" in quotes in your testimony.

A Yes.

Q Why?

A A very specific reason. What -- what I said originally in there, I have concluded on the basis of the studies I mentioned earlier, that it is reasonable to assume a uniform slope value in a whole variety and, hopefully, all of the Eastern U.S.

Now, if this is so, then I can take a large tectonic region, and if I plot its seismicity, I'll get a particular graph with a certain slope. I can take half that region, I can take a portion of it and plot the seismicity there, and I'll get the same slope.

The very useful thing about the particular assumption or conclusion, whichever you like, that the B value is constant is that it's a much less of a reliance on one's tectonic province being a definitive one. It means I can take an area and say, well, at least this area looks pretty much like an area that I can deal with. It may be part of a larger tectonic province defined in the sense in which its mentioned in Appendix A, which, as I said, I have trouble with. So I'm not using the word quite in the same sense as it is -- as it is in Appendix A. It may be a part of a larger tectonic province or it may, in fact, be a tectonic province itself. I'm indicating that I was not trying to make that judgment of whether this particular area corresponded exactly to the definition in Appendix A .

Q Well, you -- you didn't -- not only didn't exactly use

it as in Appendix A; you didn't use anything like Appendix A?

A I used geological information hearings.

Q We agreed that in the -- in the definition in Appendix A, which I understand you don't like, it is defined as "characterized by a relative consistency of geological structural features"; is that correct?

A Yes.

Q And you have no basis for saying that this province you picked is in fact characterized by a relative consistency of geological structural features?

A I did not attempt to address that question in my testimony.

Q So I'm correct, that you have no basis for saying that this province you have picked is characterized by a relative consistency of geological structural features?

A I think that's a separate question. I think there are -- some arguments could be made for that, but they are not contained in my testimony.

Q I don't care whether they're contained in your testimony or not. My question is, am I not correct in believing that you have no basis for saying this "tectonic province" which you have chosen as characterized by a relative consistency of geological structural features?

A All right, to answer that question in the sense that you can pick any area and say it is relatively consistent, I

think you can say this one is.

Q Doctor, didn't you tell me you weren't a geologist?

A Yes. I'm a geophysicist.

Q And I'm going to ask you one more time, isn't it true you have no basis for any statement as to the relative consistency, no matter how broadly you want to read that term into the geology of the term of this region, do you? You've never studied --

A I have had many occasions to review the geology of this area, Mr. Dignan. I'm not an expert in the sense that I'm not a geologist. I have reivewed the geology many times.

Q So do you feel that this region is characterized by a relative consistency of the geological structural features?

A It's all a question of what one means by relative consistency. I could se a -- an interpretation of a geology which is consistent with that definition, but I'm not sure that that's the best one or the only one.

Q In any event, you never did address the question, right?

A I did not attempt to address that question. As I understood it, the whole question of tectronic province was not in issue here.

Q Doctor, I'd like to take you to page 16.

A Mm-hmm.

Q Doctor, are you aware of any geological evidence at all

of a modified mercalli intensity IX epicentral intensity, I'm talking about -- IX or X occurring in the last 10,000 years in New England?

A No, I'm not. I argue that there needn't necessarily be any.

Q Doctor, I'd like to take you to your 1979 paper.

A Mm-hmm.

Q Which is Exhibit 2 of your testimony, and particular would you turn to page 757?

A Yup.

Q You make two assumptions there. The first assumption is, "all subregions within a given region have a linear frequency-intensity relation of the form log Ni=ai-bI or where Ni is the cumulative number of events in the ith subregion with intensities greater than or equal to I, and ai is a parameter describing the level of seismic activity of the ith subregion."

A No, the following sentence, if you'll read the following sentence, Mr. Dignan. It says -- it isolates that as a sub -- as I have done it, you are quite correct. I could have written about i --

Q Yeah, that's all right. I'm not arguing, but am I correct that's one of the assumptions that you make?

A There are two assumptions: one, that it's linear, and, one, that the slope is constant, yes.

Q And in addition, you make an -- an assumption that the

maximum possible intensity in each subregion, if one exists which is lower than the nominal maximum of XII, is larger than the largest event recorded within that subregion during that period of the earthquake record; and that is another assumption that is made, is that right?

A Yes.

Q Now, as I understand it, you've assumed linear -- a linear relationship on intensity and frequency. You've assumed a common slope, and you assume that the historical record is lower than the maximum, and you start with those three assumptions; is that correct?

A Yes.

Q To my untrained -- at least unscientifically trained mind, you assume you were correct when you started? Am I wrong?

A Well, the point is that this paper was not to explore the maximum earthquake question. What it was trying to do was to separate that question out from the question of the linearity and the uniformity of slope, and that's what this paper addresses. It does not address the maximum earthquake question at all.

Q But am -- am I not right that those three assumptions are just that, they are assumptions?

A I think I -- let me give you a little background as to why I wrote that in. I wrote it in because of a reviewer.

CHAIRMAN ROSENTHAL: I think before you do that you might respond to Mr. Dignan's question. He asked you whether these are assumptions and nothing more. Now, they either are assumptions, or if they're something more than that, of course, you can go ahead and indicate what they are beyond assumptions; but I think he's entitled, Doctor Chinnery, to a response to the question, which was posed to you.

THE WITNESS: Yes.

A In terms of certain conlcusions in the paper, they are indeed assumptions. In terms of other conclusions in the paper, they are not necessary assumptions. And I can explain that further if you wish.

Q That's the second time you said "necessary assumptions." The first time you left out the word necessary. Do you mean to distinguish between the fact that some of the conclusions in the paper did not depend upon the accuracy of those assumptions, in fact --

A Yes.

Q -- while others did?

A Yes.

Q Is it fair to say that the conclusions, to the extent they are expressed in this paper, which is a relevance to the matter at hand, do depend on the accuracy of those assumptions?

A They do, but in a subtle way. They do not depend on it in a very obvious way.

The conclusion, for example, that all the data which is shown in figure 8 and 9 are consistent with the same slope is not in any way dependent on any of these assumptions - at least it isn't at first glance.

When you look at the problem in a little more detail, one becomes concerned about which particular areas I chose to plot in the first place. I did not in this paper do something which one could do, which was to take 50 or a hundred different possible choices of area and plot each one of them and see if each one was consistent.

Now, the reason for the discussion of these assumptions at the beginning is in the way in which they bear on the choice of the area. This is true for the Mississippi Valley. For example, there's many ways in which one could choose a subset from that data to explore in this particular paper; but if the slope does turn out to be uniform and -- and, agreed, there is something of a vicious circle here -- but if it does then, in fact, that choice of area is as good as any other one.

Q Doctor, could we turn to page 769.

A Yes.

Q That is -- and you've referred to it before. This is Table 5, where you predict probabilities of large events in four regions of the Northern United States, including the Boston-New Hampshire zone, so called. With respect to that,

you say at the top of the page, "We cannot pretend that these numbers are very accurate. In fact, because of the subjectivity that has to be used in obtaining the linear relations [equations (1) to (5)], there is no way to make a realistic assessment of errors. We therefore view the numbers on Table 5 as being a qualatative indication of risk, rather than quantitative." I'm about to do something I was taught never to do in law school. Doctor, feel free. Would you tell me what that means?

A Well, first of all, please understand this paper was not written with these particular proceedings in mind, and I would never had said such a thing had I considered that these proceedings would have happened.

Yes, I'll tell you what it means. I've quoted in the table numbers like 537. It to me is clearly nonsense. One cannot determine that to the accuracy of one year. Nevertheless, whenever you do this kind of a calculation, one does come up witb a number like 537. One has the option then of rounding it to 550 and writing that down or, if you don't believe that, rounding it to 600 and putting that down. I chose instead to put the numbers down as they emerged directly from calculations as in the equations described in the paper, and that I -- what I -- basically what I am indicating here is that they're qualitative; and now to me what that means is, when I'm talking of Boston-New Hampshire, I say the return time for an intensity X is 5,623. I'm indicating -- and most scientists will

understand what I'm getting at here -- there's, okay, it's about 5,000 years. It may be three; it may be seven. These -- these differences. It may be four; it may be six. It's an order of magnitude which is rather important.

And, in fact, when you start to -- and to risk the differences between these numbers are not that important. But, I think it tells you it's more likely 5,000 than it is 500, it's more likely 5,000 than it is 50,000. So it's an indicator of -- and that's what I meant as qualatative rather than quantitative.

Q Well, where is the subjectivity being employed in this analysis?

A Okay, that I can easily come to. Let me direct your attention, for example, to Figure 4, which shows the Mississippi Valley data. I think the data are extremely consistent with a linear relationship; but, nevertheless, if ten people were to come along with a ruler and put a linear relationship through those points, there will be some scattering. I have clearly made the subjective choice that intensities III and IV data are incomplete over this period, and t-is is something that is very hard to prove or disprove. And it's something I think is scientifically reasonable, but I, obviously, cannot prove it.

The remaining data are very consistent with a straight line. It is possible to -- to fix that straight line using a standard mathematical technique like least square method. I'm not sure that that is any more valid than putting a line through,

much as I did, which was a ruler and pencil, which is essentially using the human being to do the least squares method.

So there's an element of subjectivity in however one does it. There are other means of attempting to fit lines to this type of data which use different assumptions again. The choice of method is subjective in itself.

Q Was there a certain amount of subjectivity in selecting the time period to use, 1840 to 1969?

A Very little, because in each case I was attempting to start, as you mentioned earlier, after the large earthquakes. So in the case of Mississippi Valley I did not want to start it at 1800. There are enough earthquakes there; I could start later; and I picked 1840 as happening to be a convenient time.

I think you will find that the catalog that Natalie produced -- yeah, the catalog that Natalie produced went from the period 1833 to 1972, Figure 3.

And so, I couldn't have gone any earlier than 1833, anyway, using that particular catalog. I happened to start it at 1840 to -- that was a very subjective choice, and I don't think it makes any substantial difference at all.

Q It was a subjective choice?

A That was a subjective choice.

Q Doctor, would you come to page 771.

A Mm-hmm.

Q There you say that the Cape Ann earthquake -- I assume

by that you're referring to the 1755 earthquake?

A Yes.

Q You say Charleston and Cape Ann earthquakes.

A Yes.

Q Are we referring to one Cape Ann earthquake or more than one?

A One.

Q Okay, that's what I thought. Is that the 1755 event?

A Yes.

Q You say they are both consistent with more recent data from small events.

A Mm-hmm.

Q Okay. Now, first of all, did you ever try to plug in the 1727 Newbury event and see what that got you along with the 1755 event? There was a 1744 event; and, incidentally, these show up on your Exhibit 1 on page 96. Exhibit 2 there was a 1744 event on Cape Ann that Smith characterized as an intensity VIII, is there not?

A You could easily be right.

Q What I'm taking this off is, if you look at your Exhibit 1, the paper, your earlier paper --

A Right.

Q -- you have the larger quakes in Southern New England listed, and you have them with Smith's original intensities.

A Okay, yes, I see the page.

Q And Smith has a -- what I'm getting at is this: Smith has a 1638, intensity VIII; he's got a 1727, intensity IX; 1744, intensity VIII; he's got the 1755 event in at a IX?

A Yes.

Q And then he says 1791 event, but I guess that would not be in the Boston-New Hampshire zone as you define it; is that right?

A Yes, that's true.

Q But there are four events that at least are VIII or larger in this zone, and my question is, did you ever attempt to not just note the 1755 event but put all four of those into your chart and see what that did? Because I suggest you would now have three events of VIII or larger in 300 years, not just one, which would, it seems to me, not be such a good fit to your line as the 1755 alone is.

A True. My computation said that the return period of an intensity VIII or greater was about 371 years.

So what you're asking is if that happened to be true, what is the possibility that you might get four of them in a period of --

Q No, Doctor, that's not my question. My question is very simply this: You make the point in your paper and in your testimony that the 1755 event, when plotted onto your data, fits very nicely; and I'm saying that if instead you had picked up all of the VIII's that Smith had called, to wit, the 1755,

the 1744 and the 1727 and plotted them into your chart it wouldn't have been on that line or anywhere near it?

A Let's see. It would have been above the line by about a quarter of an inch, you're quite right.

Q Mm-hmm. And a quarter of an inch is a long way on lined paper, isn't it, Doctor?

A Yes, it is. I don't personally ascribe to the 1638 -- I, I had at the time summed out, introduced the 1727 earthquake. I did not mention in that particular study because I rather suspected there may have been some other publication which listed those as less and it's hard to recall. It was done a long time ago.

MR. DIGNAN: Mr. Chairman, at this point I have concluded my questions on the direct testimony. Is it the Board's pleasure that I move straight on to the rebuttal testimony or was the Board thinking of some other sequence of events? I'm indifferent. I am prepared to start in the rebuttal but I thought I would seek the Board's pleasure.

CHAIRMAN ROSENTHAL: Well, before seeking your pleasure or announcing your pleasure, if we have one, what's the feeling of the other parties?

MR. LESSY: Have Mr. Dignan continue into the rebuttal testimony so that each party will have a distinct set of cross-examination of Dr. Chinnery.

MR. JORDAN: We agree.

CHAIRMAN ROSENTHAL: All right. Mr. Dignan, you may proceed.

MR. DIGNAN: Thank you, Mr. Chairman.

Q Doctor, I would like to go to Page 3 of the rebuttal --

A Mm-hmm.

Q You have a statement in there "In fact, a good scientist will never quote an observation without also quoting his best estimate of the error associated with his piece of data".

Did I read that statement correctly?

A Yes, I think so.

Q Where, in your direct testimony do I find your best estimates of error?

A Perhaps I'm not that good a scientist. Let me see if I can find it.

DR. BUCK: Mr. Dignan, would you mind giving me the rebuttal testimony again?

MR. DIGNAN: Yes, I'm sorry, Dr. Buck. Page 2.

DR. BUCK: Page 2.

MR. DIGNAN: The beginning of the first full paragraph or the second sentence.

DR. BUCK: Thank you. Okay. Thank you very much.

THE WITNESS: It was intended to be error but you may not read it that way. Page 11 of my direct testimony refers to the determination of the slope.

Q Yes.

A And it refers to the slopes typically lying in the range opening advised 4.6 and the slope that I've been using throughout is .57. My 1979 paper, I think there I refer to that as having a potential error of at least .03. I hope I put that in. I

certainly intended to.

Q Can you give us any estimates of your, of your error with respect to the error inherent in the data points that you used, if there be any error inherent?

A No, I did not, because I had no way of measuring that error.

Q In fact you note it's just not possible to put a confidence level on those data points, is there, and you so testified so previously?

A Yes, I have testified to that before.

Q Now, you, on Page 3, dwelling at some length on Occam's Razor. And that's I guess a favorite not only of scientist but of lawyers, too.

And you say that an equation with Occam's Razor says you should not look for a more complex reason for things in life when there is a simpler explanation.

Let me ask you this question, Doctor: Why would it not be the simpliest explanation to accept the theory of an upper bound in a seismic region and call it on that basis rather than going through the exercise you go through? Isn't that a much simplier explanation of the facts as we know them?

A What I have said in, later in this which I think is my opinion on this is that any interpretation of the data which in some sense says that's -- well, let me phrase it this way: If you place a linear relationship between the data and the

historical record and it fits moderately well, that's supposing that is true, if what you mean by an upper bound assumption is one that cuts that off immediately beyond the data that we have, in other words it says nothing larger than occurred in the historical record will ever occur, that to me is a very complicated law. It's a law which is straight for a while then suddently changes and drops down vertically.

Now, to me that's a very complicated law so perhaps this is why we have trouble communicating on this. It's not the simpliest one by any means to me.

Q Well, it depends upon what one calls the historical record, doesn't it? What if one calls the historical record the historical record in writing, Smith, and what he looked at --

A Mm-hmm.

Q -- plus the geological history of the area, or is that not a valid basis for putting an upper bound on intensity?

A That question is addressed later in here. The question of whether geological evidence can contribute to this whole question is a very difficult question and one we need to take up here.

My opinion is that it cannot, the geological evidence is not sufficient to enable you to establish an upper bound.

Q Doctor, you know, a lot of this paper read to me and I, I want you to understand what I'm saying is dealing with a philosophy of science as well as dealing with the immediate

problem, and I would like to ask you a few questions along that.

As I understand it, you are a believer that we have very little data on which to make projections in New England by virtue of the fact that we have a short historical record in the three-hundred, four-hundred year record, whatever you want to call it --

A Mm-hmm.

Q -- and that the lack of data comes from the fact that in that time period these large events haven't occurred.

A Mm-hmm.

Q And you call that a lack of data, am I correct?

A Yes.

Q And you say from there we've got to look for another explanation, is that right? We just can't use this historical record because large events haven't occurred?

A We should not rely entirely on the historical record to tell us all that could happen, yes.

Q Now, this is -- this question is going to sound facetious but I do not mean it facetiously.

A Mm-hmm.

Q And I would like a direct answer to it.

If the theory is that because we've never had a large earthquake in New England you have to start doing calculations to get a large earthquake to design against. On that basis, why shouldn't I start making every nuclear plant in Nebraska

against a tidal wave because we have had no experience in tidal waves in Nebraska. I ask you now -- I am asking you facetiously, I know, but I think you can see the problem I have with the philosophy and the logic that underlies this theory.

A Well, I in turn cannot accept that we have never had a statement. If you phrase your question saying that during the last three hundred years we have not had, then I would go along with what you say. This is equivalent to saying that just because in the last ten years there have not been a hurricane in Boston there never will be and I disagree.

Q Okay. Doctor, then you wanted to make that correction that I repeat: Shouldn't the Nuclear Regulatory Commission start designing these plants in Nebraska against a tidal wave because as you say, all I can say is that in the last three hundred years there's been no tidal waves in Nebraska? That's all I can say.

A I think --

CHAIRMAN ROSENTHAL: So long as you say there isn't.

MR. DIGNAN: Maybe five hundred.

CHAIRMAN ROSENTHAL: I don't know. You may be aware of an ocean in the proximity of Nebraska that my map doesn't reflect. I think there's perhaps another element there, isn't there?

MR. DIGNAN: I'm assuming we're going to get to that.

CHAIRMAN ROSENTHAL: I'm surprised at the answer I got.

THE WITNESS: I, I, I can't relate to your question.

Q Do you know what a gray flag is, Dr. Chinnery?

A What's a gray flag?

Q A gray flag, I think, as a very good lawyer just threw you one. One might would get on it is the reason we don't design against it because we know there's no oceans in the proximity of --

A Of course.

Q -- Nebraska?

A Yes, of course.

Q And that's the real reason it's not because we haven't had any tidal waves. It's because --

A Yes, exactly.

Q -- we know that. Why do you reject the concept that geologists have, have an ability and no it cannot come to within the same bounds of certitude by examining the geology of an area?

A Because there's not necessarily true that earthquakes that may give rise for damages will leave enough geological damage to even see these things.

Q Did you review the papers -- I think it was by a fellow named Fuller -- concerning the geology around the New Madrid area?

A I have not but I have, I have heard of that work and I know it's quite clear around the New Madrid area. There are

evidences of earlier evidence. I am willing to accept that one.

Q Big ones?

A That's right.

Q There is big ones.

A And the kind of evidence is kind you still expect to see lying around in ten thousand years formation of a large mess.

In New Madrid they occur relatively frequently and they occur in an area which is extremely -- in the river valley and the valley of the Mississippi River in Missouri and, and the cliffs and so forth are composed of extremely soft materials there and will show a great deal of slumping and what you see is in fact the slumping in those areas. You do not see any faulting or anything remotely resembling faults in New Madrid or in Mississippi anywhere.

What you do see are --

Q Well, you don't see anything, you don't see anything like faulting or anything close to it in terms of a fault in New England, do you, either?

A No, you don't. So in either case one concludes earthquakes are deep. Now, in New Madrid they occur much more often in that area which make the evidence much more available.

Q Wait a minute, Doctor, let's not start that outline. Isn't it true that in New Madrid bedrock is much deeper under a great deal more over --

A Yes.

Q -- than New England?

A Yes, exactly.

Q Is that right?

A Yes.

Q The situation is similar but we have the bedrock relatively close to the surface?

A Right.

Q And as I understood you to admit to me earlier you know of now geographical evidence of a major event above an "L" in the last ten thousand years in New England.

A No. Mr. Dignan. I must go further than that.

There are many faults in New England. I do not know of any evidence that any of those faults have moved.

Q Exactly.

A In recent geological time.

Q Right. How do you get --

A So clearly if we have earthquakes they are not occurring on those faults, they are occurring at depth.

Q All right. And if they are occurring at depth, the depth, that distinquishes them, for example, from the situation in California, does it not?

A Yes, it does; very much so.

Q And is this another possible factual or scientific or whatever you want, reason why we aren't seeing these high

intensities here in New England because of the epi-center or the focus to the epi-center on the surface of the earth?

A No. At least I don't think so.

There are several parts to the answer to that question. Let me get them straightened out.

The question of what depth they are at is hard to say. The evidence both in the record of these proceedings and elsewhere suggest that they occur perhaps ten, perhaps twenty kilometers deep in the earth's crust. The question then of what kind of intensity those earthquakes will produce at the surface in spite of the fact that they produce no ground breakage is a whole different question and there one can only look for the kind of intensities as a function of time.

So if there's an infrequent large earthquake at depth in New England, one's going to have an infrequent motion of the surface in response to that and the question of what geological results or evidence that that might lead to is one that nobody to my knowledge has adequately discussed.

My point is that in many parts of New England there will be no easily available geological evidence because the overburden is so thin as you just said. So I'm not surprised and I don't think you can rule out the occurrence of relatively large earthquakes at that -- New England, giving rise to quite substantial ground motions on an infrequent basis.

Q Have you finished your answer?

A Yes.

Q Thank you. Would you turn to Page 6, please?

A Witness complied.

Q In that, on that page, you criticize Mr. Holt --

A Yes.

Q -- because of his use of the events in the Mississippi Valley has three separate events. And you point out in your footnote too, "It is very questionable whether that set of events should be regarded as three events or as a single occurrence."

Didn't you --

A Yes.

Q -- treat two Ossipee earthquakes in New Hampshire as two events in your calculations?

A Yes, I did.

Q And what --

A But I did not use those intensity data.

Q And you did not treat those as two separate events?

A I did not make decisions as to whether they were or not. I included them as two separate events. Yes. You are quite right.

Q And that's what I mean.

A And intensity 7.5 are above the curve.

Q And what I am getting at is you did with those two events which were very definitely close in space and time, were they not, Doctor?

A Yes, they were.

Q And yet you criticize Dr. Holt for doing the same thing with the three events down in Mississippi. And I guess my question is why, if he is doing exactly what you did?

A Because he is taking those three events, dividing them into a hundred seventy years and concluding that they are occurring on the average every fifty six years.

Q Well, I am not sure that's what he was doing but is that what happened?

A That's what I have to read off of his graph.

Q Isn't that what you did with the two events in Ossipee, just that you added a few more within?

A I, I did it in the -- from, since from 1800 on so it didn't look so bad.

Q Oh, I see. All right.

Now, you state that Dr. Holt appears to prefer a non-linear relationship -- now, this is Page 7 -- you say, but his arguments are not convincing and then you go on.

What is your basis for saying that Mr. Holt has preferred any kind of relationship based on your data? Do you know anywhere where Mr. Holt has adopted your theory?

A Oh, no.

Q All right. Now --

A But he does -- I was referring to a specific diagram in his testimony where he had put a curve relationship through

a set of data.

Q What he did was he expanded the temporal period, brought in the big events and showed that once you took the biggest event in this, if you took them in, the curve got badly skewed from a linear curve?

A No. I wasn't talking about that. I think it was his figure 7. I wonder if we could have Dr. Holt's testimony. I think it was his figure 7 that I was referrring to not the earlier one. Did I get that number correctly? Yes. His figure 7, where he's taking my data and suggested that there's another interpretation the quadratic model, Page 17 of Dr. Holt's testimony.

Q Well, what Mr. Holt did was take your theory, take your method, expand the time period, plug in all the events for the extended time and gave evidence that, to use my word here, some, is that right, like changes occurred in the lines once you plugged in all the events, isn't that what he did?

A Which particular piece of data are you referring to? Are you referring to figure 7 or not? I think figure 7 supports my contention extremely well.

Q Well, Let's get down to what you're contending what figure 7 is.

A Oh, well, my contention is stated in my rebuttal testimony is that that's one of the better linear relationships that I have seen for this particular area. It's using a "data

base, it has a slope of .55 which is extremely close to the range I'm talking about.

The only point that's very much away from that is the intensity three point which I'm not surprised that that one is away. So I think that's quite a good fit. I see no justification in that data for attempting to propose a guadratic model.

CHAIRMAN ROSENTHAL: Mr. Dignan, with your permission, we might take a luncheon recess at this point.

MR. DIGNAN: Whatever the Court says.

CHAIRMAN ROSENTHAL: And we'll resume at precisely quarter of two.

(Luncheon recess.)

CHAIRMAN ROSENTHAL: Mr. Dignan.

MR. DIGNAN: Mr. Chairman, I have no more questions of the Witness.

CHAIRMAN ROSENTHAL: All right. Mr. Lessy.

MR. LESSY: Thank you, Mr. Chairman.

CROSS EXAMIANTION

BY MR. LESSY:

Q Doctor Chinnery, do you have a copy of the interrogatory responses that you wrote in response to the Applicant's interrogatories?

A Yes, I do.

Q In response to Applicants' interrogatory No. 17, you state in part, "Clearly the choice of acceptable levels of risk (10^{-7}) in the above example needs careful consideration by the NRC. In my view, if safety factors in plant design are established to be high, a formulation such as given above is not likely to increase, and may actually decrease the structural requirements needed to account for seismic risk. Certainly, it is possible to approach this definition on a sound scientific and engineering basis." Asl in response to Applicants' interrogatory 21, you state in part, "The safety factors built into the structre are an essential part of the assessment of seismic risk." And my question, sir, is, would you please explain in more detail your comments as to how safety factors and plant design may decrease the structural requirements needed to account for seismic risks?

A Yes. I think, without divesting your question at all, I can refer to the end of my rebuttal testimony, which I think in the summary at the end of that that is just at the very point that you're asking about.

The basis of my opinion is that I think it's very, very difficult using what seismological and geological information we have to thoroughly establish what the largest earthquake that could happen in this area really is; and, therefore, if one is forced to design the safe shutdown earthquake as being the largest earthquake which will ever happen, then one is forced on any rational basis to choose a rather large size of earthquake. Whereas, if you can start to consider the probability of an earthquake and the likelihood that it will do actual damage to the structure, one can define a risk, which I mention here is 10^{-7}. Whatever that number is, I think there's a level of risk which is certainly acceptable to me and would be acceptable as being comparable to many other kinds of risks.

And if safety factors are so that if the design acceleration were exceeded, for example, that the chance of any actual failure happening was very low, then that's what I mean by safety factors -- and I'm not sure if I'm using exactly the right engineering terms -- then the -- the amount of risk that need to be put into the earthquake itself becomes rather small.

Q It is -- is it your opinion that a decision such as seismic design must by necessity embody engineering judgments and

herein understanding the manner of which the seismic input is to be utilized?

A Personally, yes, it is.

Q Now I'm going to show you a document which is a letter written by you dated October 23rd, 1980, addressed to Ellyn R. Weiss and signed by you.

A I have a copy here.

Q This document was attached to a pleading entitled NECNP request that Doctor Chinnery be called as a Board witness and memorandum on related matters which was filed in this proceeding on October 31, 1980.

MR. LESSY: Does the Board or anyone else need a copy of that pleading?

CHAIRMAN ROSENTHAL: I don't have one with me.

DOCTOR BUCK: I have it here. This is the NECNP request.

MR. LESSY: Please disregard the handwritten notations on the front of this pleading.

Q But, Doctor Chinnery, do you recognize the letter attached to this pleading?

A Yes, I do.

Q Is that a letter that you sent to Ms. Weiss?

A Yes, it is.

MR. LESSY: I'd like this, the pleading, or the letter addressed to the pleading be identified as Staff Exhibit

No. 1.

CHAIRMAN ROSENTHAL: All right. So identified.

Q Now, Doctor Chinnery, the first paragraph of your letter provides, "I have now had a chance to read a number of documents that you have recently sent to me. These include a copy of part of the decision of the Appeal Board (date unknown), the transcript of the May 29, 1980, presentations to the NRC commissioners, and the NECNP memorandum to the Appeal Board dated October 17, 1980." With respect to that sentence which I read from, my first question is, to the best of your recollection, what documents other than the ones you listed in that sentence did Ms. Weiss send to you?

A Well, at that point in time when I prepared that letter, which was last October, I think that was all. Subsequently there have been a variety of other documents, testimony and so forth which I have recieved.

Q All right. Now, you state that in the last two sentences of the second paragraph of the first page, "Further, I do not support the aims and objectives of that organization. I support the construction of nuclear power plant facilities, which I feel are essential for the maintenance of our present technological society." Now, with respect to those two sentences, Doctor Chinnery, what were you referring to in that -- in the first sentence when you talked about the aims and objectives of the NECNP?

A I am not trying to eliminate the use of nuclear power as a means of electrical power generation.

Q Did you understand that that was NECNP's objective?

A No, but I have a feeling that it's one of them.

Q Was there any document upon which you based that feeling which was provided to you?

A No. I have received the newsletter of the NECNP. I got placed on their mailing list, so I occasionally get an idea of their activities from that.

CHAIRMAN ROSENTHAL: Mr. Lessy, would you give me some idea of what the relevance of all this is to Doctor Chinnery's testimony as an expert witness on the seismological issue?

MR. LESSY: Possible -- at this point, on those two sentences, possible questions of bias.

CHAIRMAN ROSENTHAL: Bias against whom?

MR. LESSY: Bias by NECNP as regards Doctor Chinnery and the use of his testimony.

CHAIRMAN ROSENTHAL: Well, I'll let you proceed a little further with this, but I would have to tell you offhand I have great difficulty in -- in seeing the relevance of this.

MR. LESSY: Well, Mr. Chairman, I thank you for the opportunity. If -- if a party presents an expert witness with documents, I think the other parties would have an opportunity to find out what those documents are to the extent that they might bear on the testimony of that witness. But, I am almost done

with this particular line of questioning.

CHAIRMAN ROSENTHAL: All right.

Q (By Mr. Lessy) Under -- under the first sentence of the second page of that letter, Doctor Chinnery, you state, "The matter was taken up again in the Appeal Board hearing (date unknown), to which I was not invited. At that time, my testimony was judged to be 'technically deficient and inconsistent with Appendix A.'" Now, with respect to that sentence, has your counsel explained to you that the previous Appeal Board proceedings consisted of oral argument on legal issues as opposed to the de facto presentation of expert opinions?

A No, he didn't. All I saw was the transcript, the printed transcript of the -- at least the printed summary of the Appeal Board findings. That's where I extracted that from.

MR. LESSY: I included in my question with respect to that letter. I'd like to move it into evidence as Staff Exhibit No. 1.

CHAIRMAN ROSENTHAL: Any objection? Hearing none, the letter will be admitted.

MR. DIGNAN. Yes. Could I respectfully ask the Board to ask of counsel whether it is being offered for the truth of the matters contained or what the purpose of offering it is?

CHAIRMAN ROSENTHAL: Well, Mr. Lessy, what --

MR. LESSY: It is being -- not that the truth of the

matters contained therein, no; as being a communication between Doctor Chinnery and his counsel in this proceeding concerning the background of this proceeding and his objective vis-a-vis NECNP.

MR. DIGNAN: Again addressing the Board, as I understand the offer, it is confined to proving that it does prove the relationship between Doctor Chinnery and NECNP; and if that is the restriction of the offer, I have no objection.

CHAIRMAN ROSENTHAL: Is that the limited purpose for which it's being offered into evidence, Mr. Lessy?

MR. LESSY: Yes, that's the purpose of the letter.

CHAIRMAN ROSENTHAL: So understood, I gather Mr. Dignan has no objections. Mr. Jordan?

MR. JORDAN: I have no objection.

CHAIRMAN ROSENTHAL: All right, the letter dated October 23, 1980, from Doctor Chinnery to Ms. Weiss will be accepted into evidence as Staff Exhibit 1 for the limited purpose indicated.

Q All right. Doctor Chinnery, on page 2 of your rebuttal testimony, you state, "an awareness of the inherent uncertainty in a result or a conclusion need not indicate a lack of technical ability, but often represents a deeper understanding of the scientific problems involved." My question is, shouldn't you apply this awareness of uncertainty to the essential input parameters in your probabilistic calculations such as maximum

magnitude, B values and the linear extension of frequency-intensity relationships?

A Yes, you should.

Q I -- any method should have the associated undertainties spelled out as clearly as possible, and then the judgment would be which method can legitimately claim to have the smallest uncertainty.

Q Since the results of probabilistic calculations are couched in terms such as chance of occurring or probability of exceedance, isn't it incumbent upon practitioners of these methodologies to account for associated uncertainties to decision makers?

A I think there's a great difference between an uncertainty and a probabilistic assessment. I -- I don't -- it's a problem that we have a great deal, how to characterize estimates made using statistics or using probability studies. I don't personally feel that there is a direct equation there between the two. An uncertainty exists in any method. It exists in the -- what I call deterministic method just as much.

As Doctor Jackson mentioned in his staff testimony, there is an inherent error in attempting to apply the normal methods of determining the safe shutdown earthquake, making the assumption that the largest earthquake that will occur in an area did in fact occur in historical times. The question is, is that a large error or a small error.

I -- I cannot go along with the notion that probabilistic methods are in some sense more inaccurate than other methods. I think they have one greater advantage: they allow you to in some sense begin to categorize the error; whereas, the more orthodox method to the safe shutdown gives you no estimates of what the errors are.

Q Now, prefacing upon the probabilistic method which is at issue here, do you feel that you have adequately conveyed any uncertainties in your methodology to the decision makers or to the parties in this proceeding?

A I don't pretend to be perfect in this respect, Mr. Lessy. Have I covered every possible base? I shudder to think I have.

I've attempted to make what appear to be reasonable assumptions, and I think that time may tell that some of those are either more reasonable or less reasonable than I think they are. With our present state of knowledge, my view is they are reasonable assumptions, and they lead to reasonable conclusions. That is not to say they will always be correct. I cannot estimate how they may change in the future, but I see equally as difficult the problem of -- of -- of estimating errors in any kind of approach to the determination of the safe shutdown earthquake.

Q Do you believe that there are any uncertainties in your methodology other than in your reasonableness of its assumption?

A I find that hard to answer. There are -- there are so many aspects to the methodology. It's been discussed in terms of at least four different assumptions involved, and clearly each one of them can be discussed in terms of errors.

In addition, there are errors in the computation of ground motion from the determination of the safe shutdown earthquake. I think in many ways Doctor Trifunac's method takes a much more adequate look at the -- at the error problem which is involved in the overall process of achieving the actual ground motion.

Q And why is that?

A He puts in a much wider range, for example, of seismicity than I do.

As I say, it's much easier really to discuss these point by point. I find it very hard to take an overall look at the whole thing and say the final answer that comes up. I've said this morning, and I'll still say, that in my estimation, that the answer comes out within about an order of magnitude for the risk involved. So I say between 10^{-3} and 10^{-4} is as near as I can estimate it, but this is not an easy thing to do, either. However, I think that's very useful information and can be treated as such within even a rather wide range like that.

Q Let's look at it specifically. How have you accounted for uncertainty in the linear relationship, in the proposed linear relationship?

A Well, there are two questions there; first is the linear, and secondly, what is the slope of the linear if it is. I have not consciously tried to allow for the fact it may not be linear, simply because I think the preponderance of evidence is that it is linear.

I think there's a good deal of question as to what the slope is. I have tended to try to err on the side that produces the lowest risk. So I think if you look at the variety of slopes which have been determined by other people, the vast majority of them are, in fact, smaller than the number I've been using, and these would lead to large estimates of risks. So I feel in that case it's not so much that I've taken account of the entire range of the data that's available, but I've picked a number which is on the more reasonable end of things and so on.

In terms of the size of the largest earthquake, I think this is one of the key problems here, and I'm not quite sure how to handle that problem. As I have said in here, I don't see that we can say for sure what the largest earthquake in an area like New England could be. I think we can start to make an estimate for how frequently those very large earthquakes may occur, and I agree they may not be the best estimates, but I think that's the very best thing we can do at the present time.

Q You are aware, of course, that nonlinear frequency-intensity relationships have been proposed by seismologists;

isn't that true?

A Yes, I make the statement somewhere that I think each of those studies are subject to some suspicion. And I could elaborate on that if you wish.

Q Isn't it true that the main reason that other seismologists have proposed -- one of the main reasons that other seismologists have proposed nonlinear or frequency-intensity relationships is because of the relatively poor fit of the linear relationships as to the data at higher intensities or magnitudes?

A I would rephrase it. I'm sorry. I have a frog in there today.

The reason is because the whole idea the magnitude scales saturate has only become known in the last few years. Until that time it was not known that each of our different ways of Mercalli Magnitude cut off by themselves at a level which varies with the particular definition you're using. So if you're using what is normally called the Richter magnitude, for example, it never gets larger than about 8.5, however large the earthquake is. If you're using body wave magnitude and B, which has been quoted at times in this hearing it never gets larger than B7. I have discussed this in several papers and several papers by California Tech have discussed this. I think the phenomenon is well-understood, and I think it's hardly necessary for me to go into the technicalities of it, but the

fact is that all the scales do saturate. The net result is that this leads to an, apparently curvature at the high end of the frequency-magnitude curves. I tend to want to leave it there. There's more that I can say; in particular, the paper of mine which deals with this very point in Science Magazine in 1975; and if -- if it's a point you want to follow up some more, we could submit that.

Q Well, is -- is the linear relationship which you propose between frequency of occurrence, is that an occurrence or is that an imperical relationship?

A I view it as an imperical relationship.

Q Now, you agree, do you not, that certain highly qualified earthquake engineers and seismologists, including Professors Cornell, Toksoz and Van Mark at your own institution, MIT, have proposed or considered relationships other than linear relationships, have they not?

A Yes. I need to go into that, though, to answer the question.

Q Excuse me?

A I need to say something a little more, I think, to answer that question.

Q I'll ask you a couple in addition to that.

A All right.

Q Now, could you explain in summary form and state your opinion with respect to the validity of these other proposed

nonlinear imperical relationships that have also come out of MIT?

A Okay, to take those particular examples, Cornell and Merz have -- have published a paper entitled -- I've got it here somewhere -- entitled 'Seismic-Risks Analysis Based on the Quadratic Frequency Law." Now, when you look at that paper, they are using the data that was collected, as you said, by Shlien and Toksoz in 1970 -- now, this is a paper entitled "Frequency Magnitude Statistics of Earthquake Occurrences." It was written in March 1970 in "Earthquake Notes." It describes a perfectly valid observation that when you plot the frequency-magnitude statistics of worldwide earthquakes using the standard earthquake catalogs, that you find a curvature at the upper end. These earthquake catalogs are using body weight magnitude and B. There's absolutely no doubt that this is true. The question is, why is it there?

1970 was before this whole question of magnitude saturation became understood. It was not understood until two papers that came out, one of my own and one of Kanamori and Anderson in 1975, and subsequently I wrote a paper in 1978, which explored the whole question of this curvature in measurements of MB and body weight magnitude. In my view -- and I have yet to find anyone that disagrees drastically with this. I think we have at least a rough idea of what the effective magnitude saturation is.

In terms of large earthquakes, which we usually measure by their surface wave magnitude or Richter magnitude, there's a saturation that begins about magnitude 7 and slowly falls off until, as I say, it's about magnitude 8 1/2; nothing more happens.

In terms of body wave magnitude, which is measured typically at shorter periods -- I'm sorry this is technical. To explain each one of these points probably would take longer than is necessary here, but it's a different definition of magnitude. This one saturates at about -- starts at MB6 and finishes at about MB7. And you can explain the curvature in all the Shlieu and Toksoz statements perfectly adequately using that concept of magnitude saturation. And this takes the whole point away from the paper by Merz and Cornell. So to me that's the answer to your question. It's not a valid -- doesn't rest on -- on sound principles anymore.

Q How about van Mark, Professor van Mark?

A I know him and I'm not aware of the work you're talking about.

Q Now, on Page 10 of your rebuttal testimony --

A Mm-hmm.

Q -- you state with reference to New England seismicity, that the only events that have been linked to a geologic structure in this area are the 1940 events in New Hampshire which occurred near the Ossipee Mountain Ring Dyke complex and possibly smaller events in the Connecticut River Valley which follows a major structural boundary".

I take it that in making this statement that you agree in essence that those events which you listed are listed to, link to geologic structure, is that correct?

A I think it's possible. I, I -- there's also a question of random occurrence too. There is no doubt that the 1940 events did occur rather close to the Ossipee Mountain structure; whether the Ossipee Mountain structure caused them,I think is another question which, which requires another level of geological intuition or knowledge. But certainly they did occur very close to it so it's a logical connection there but whether it's a scientific one or not, I don't know.

Q Do you agree that the Ossipee complex is at least eighty kilometers from the Seabrook site plant?

A Yes; oh, yes. Whatever the distance is.

Q Perhaps a hundred kilomters from the Seabrook --

A Yes.

Q How about intensity occurred near Ossipee?

A In 1940 there were two as I understand it.

Q Now, according to Table 4 of your 1979 article --

A Yes.

Q -- you state that there were a total of three intensity VII events in the so-called Boston/New Hampshire zone during the relevent perior according to the article from 1800 to 1959, is that correct?

A That's what I state there, yes; mm-hmm.

Q Now, if we assume that two of these events are geologically controlled to some extent by local structure at Ossipee, that leaves one event of intensity VII and according to Chinnery and Roger, that would be, the event would be the October 5th, 1817 earthquake? Is that correct?

A Mm-hmm.

Q Now, is it still your view that the 1817 earthquake was an intensity VII earthquake?

A I, I will not stand on these intensity values, Mr. Lessy. I did not determine in my use from the catalog. I think now there are better catalogs around and clearly if I were doing this study now I would use those instead, so I -- I cannot speak for them that was in Smith's catalog that way.

Q Well, are you aware of the fact that the Chiburis catalog

classified the 1817 earthquake as intensity VI?

A I was not but I'm not surprised.

Q Are you aware of the fact that USGS, United States Geological Survey most recently published on seismicity of Massachusetts entitled Miscellaneous Field Studies Map MI856-1980 has also reclassified this earthquake as epi-central intensity VI?

A No. I was not aware of that.

Q Are you aware that Street and LaCroix, 1977, using total felt areas in area of intensity, IV estimating the magnitude of that earthquake to be only 4.3 MBLG?

A I did see that work, yes. I didn't recall the number but I did see it. I'm willing to stipulate.

Q By what correspondence or correlation between 4.3MBLG and modified McCally intensity?

A I'm not sure of the answer to that. I attempted in that paper, Exhibit 1, Page 94, to give an approximate 4.3 correspondence to intensity V according to my estimation.

Q I'll accept that. Now --

DR. JOHNSON: Mr. Lessy, may I interrupt you?

MR. LESSY: Please do.

DR. JOHNSON: There's a certain level of knowledge with regard to these scales and magnitudes. It stops at MBLG.

Would you please ask the witness to define the MBLG scale and contrast it to, say, the M subject B scale or subject

K Richter Scale?

MR. LESSY: That's a fine question.

Q Sir, would you respond to Dr. Johnson's question?

A Yes. Dr. Johnson, I'll try to explain that. It's very confusing. It's confusing to seismologists, too, believe me.

We have many ways of measuring earthquakes. We have several types of instrumentation. Most seismic instruments or stations contain two kinds of instruments -- one with a filter that responds to large, long period signals about thirty or forth seconds in length, one which responds to signals about one hertz; and the reason we do that is these are regions of the spectrum where the noise is rather low so there is a noise band in between them so we, you frequently from these two instruments we get two different kinds of measurements of the amplitude of the waves coming through.

Typically, these stations are used to determine earthquakes in a global scale and the MS which is a Richter magnitude essentially, and the MB, which is called the body wave magnitude, are both normally applied to earthquakes large enough to be detected at stations throughout the world and then we take the measurement at each of these stations and average them to come up with a magnitude.

MBLG arose in a different way. Many earthquakes are not large enough that you can in fact detect them all over the world. You can only, if they are rather small, you may only

detect them at rather short distances, perhaps out to a thousand kilometers.

Now, there's always a problem in trying to match up your scales when you do this. We have a certain set of directions if you like which give a consistent scale for events at large distances, global distances, but when things are rather close, there is a difficulty.

Now, this has been worked out and I believe the MG -- MBLG scale was worked out by Nutley whose's at the University of Saint Louis and what he did was he looked at other ways of measuring the amplitude of the signals, the characteristics of the signals at these shorter distances and then he devised a scale which seemed to merge into the teleseismic scale, into the long distance scale as the event became larger. So this was a way of using close-in measurements to get something roughly equivalent to the measurements that we were getting for a larger event at larger distances.

DR. JOHNSON: Would it then be comparible to the M sub "L" scale using the Wood-Anderson seismograph?

THE WITNESS: M sub "L" really becomes really even smaller than MBLG. It's different in the way it's actually measured. It's a very Californian scale. You seldom use it outside of California.

In California it was devised quite early on to give you the full technical description. What you did in ML is you take

the maximum amplitude signal and the first thirty five seconds I think it is. Most of the other measurements have magnitude, take the size of the onset of the signal. This has been a curious difference because the thing you measure using ML often is not the same kind of wave. You tend to measure your later signal quite frequently. You're often measuring the amplitude of the surface waves, for example, from small events in California.

DR. JOHNSON: Is not ML a certain measurement as well as?

THE WITNESS: Yes, it is, but that's a large distance surface.

DR. JOHNSON: MBLG a surface or body wave measurement and over if it is either one as, at what frequency are MBLG measurements set?

THE WITNESS: Made a roughly one or two or three hertz, usually slightly higher than one higher frequency than one hertz. It is usually measured on the LG phase. That's why it's called that simply because that quite easily detectible.

Now, LG -- I am getting terribly technical and I apologize for this -- it's a higher mode surface wave. It is a type of surface wave but it's not a normal type surface wave. It's one that you happen to be able to detect quite nicely at short distances and I'm sorry it -- it's a subject that the more you get into it the more technical you, it gets, the differences between these scales.

DR. JOHNSON: Okay. Thank you, sir. Go ahead, Mr. Lessy.

MR. LESSY: Okay. Thank you, Dr. Johnson.

Q And we're talking about the 1817 Ossipee earthquakes, the 1870 Massachusetts earthquake and we've just gone over three experts re-evaluation of that earthquake.

Do you maintain that in light of those re-evaluations that the events should be classified still as modified Mercalli intensity VII?

A I don't know as I've heard enough evidence to decide for myself. I'm willing to accept whoever has studied that earthquake if, if you have a paper there that quotes it as being intensity VI, I'm willing to accept that. I don't think it's, it's a particularly significant point. I think you'll notice on my plots of Boston/New Hampshire seismicity that the intensity VII point is too high for my line.

And if there were only perhaps one intensity VII event in that same period, it would fit much better on my straight line. So -- but I do comment somewhere that there seem to be too many intensity VIIs and I think you might list, demolish one, perhaps two of those which just make the point fit my line very well.

Q Let's pursue that a little bit.

You said therefore of the three events associated with highest intensity, which is VII, that you have considered for a Boston/New Hampshire region, one event is believed by three sources to be smaller than intensity VII and the other two

you have suggested may have a link with local geological structure which is VII onto a hundred kilometers from Seabrook, is that correct?

MR. DIGNAN: I object. I think that mischaracterizes describing -- Dr. Chinnery suggested that.

CHAIRMAN ROSENTHAL: Dr. Chinnery can correct any mischaracterization of his testimony.

THE WITNESS: I think what I said is that they occurred near. That's my wording "near" the Ossipee Mountain Ring Dyke Complex. I did not put a causal connection between the two.

MR. DIGNAN: All right. Could we get a few prior answers from the witness. I know I am a little out of order here because I certainly recollect that prior testimony precisely the way -- if it's going to be revoke, I think it should be read back to the witness, and let him --

DR. BUCK: Would you, Mr. Dignan, use your microphone, please?

FROM THE FLOOR: Would you repeat the question please?

CHAIRMAN ROSENTHAL: Excuse me.

FROM THE FLOOR: I'm sorry but I came here and isn't this a public hearing?

CHAIRMAN ROSENTHAL: It is a public hearing, Madam.

FROM THE FLOOR: If I can't hear, it's not public.

CHAIRMAN ROSENTHAL: Well, we do the best we can. This is off the record.

(Discussion off the record.)

CHAIRMAN ROSENTHAL: All right, Mr. Dignan. You wish to have certain questions and answers read?

MR. DIGNAN: If the reporter could find the question and answer which I believe Mr. Lessy referred to and he can refer to it better than I can which he asked about the causal connection of the Ossipee, so-called Ossipee situation with a specific structure -- I think that should be read back to the witness in fairness to the witness and in fairness to all of us if Mr. Lessy is going forward.

MR. LESSY: I will note before his counsel objected, the witness said yes to my question but the question that the, first question that I asked in this particular line was according to my notes I take it that in making the statement that you in essence agreed these events, two Ossipee events are linked to geologic structure and it was the answer to that question that I think perhaps would be helpful to be read back.

CHAIRMAN ROSENTHAL: Could you read it back?

MR. DIGNAN: Off the record.

(Discussion off the record.)

(The following portion of the record was read as follows:

"Question: How about van Mark, Professor van Mark?

Answer: I know him and I'm not aware of the work you're

talking about.

Question: Now, on Page 10 of your rebuttal testimony --

Answer: Mm-hmm.

Question: -- you state with reference to New England seismicity, that the only events that have been linked to a geologic structure in this area are the 1940 events in New Hampshire which occurred near the Ossipee Mountain Ring Dyke complex and possibly smaller events in the Connecticut River Valley which follows a major structural boundary.

I take it that in making this statement that you agree in essence that those events which you listed are listed to, link to geologic structure, is that correct?

Answer: I think it's possible. I, I-- there's also a question of random occurrence too. There is no doubt that the 1940 events did occur rather close to the Ossipee Mountain structure; whether the Ossipee Mountain structure caused them, I think is another question which, which requires another level of geological intuition or knowledge. But certainly they did occur very close to it so it's a logical connection there but whether it's a scientific one or not, I don't know.")

MR. LESSY: Fine. Let me repeat the question that the Doctor answered and the counsel objected to.

Q And the question is, therefore, of the three events associated with highest intensity which is VII that you've considered for the Boston/New Hampshire region, one event is believed by three different sources to be smaller than intensity VII and the other two, you have suggested may have a

link with local geological structure far from Seabrook. Isn't that correct?

A Mr. Lessy, I did not suggest there was a link. My wording is exactly as was repeated. There is, that have been linked. These earthquakes have been linked to that geological structure. I have not personally made that linkage. It may exist. I do not deny it may exist but I am not establishing that as a statement of my own. So --

Q You recall the answer to my question?

A Well, so of those two events that, that indeed may be one event and it is certainly, was close to the Ossipee Mountain complex. I will agree to that, yes.

Q And the other events, intensity of VII was believed by three other sources to be smaller than intensity VII, isn't that correct?

A That could be essentially true, I agree. I agree with you.

Q In any event, with the date you've used have some impact upon the estimated return period of larger earthquakes in the Boston/New Hampshire region that could affect Seabrook?

A I, I argued at some length this morning, Mr. Lessy, and I still think it's a valid point that once you get to the point of taking a time and a given area and you only have one or two earthquakes of a given size, during that time interval you probably are better off not to plot the thing.

I, I expounded at some length, at length this morning and I say again I don't think it makes a great deal of difference.

It is for those intensities that you have a respectible number of earthquakes. Those are the ones that, ones to use to establish seismocity. I don't think taking that intensity VII point off the graph will do anything to it because it wouldn't move the other points substantially at all.

Q Isn't there something factually and theorhetically questionable with the methodology that leaves the time same result for high intensity of earthquakes regardless of whether there were three events or no events of the largest intensity examined during the period in question?

A I don't think so. Earthquakes -- all the studies that have been carried out suggest that earthquakes of a given size occur remarkably randomly. Their occurrence can be described -- well, the technical term is using a "poisson" probably distribution. But they occur randomly. It's rather like pulling a handle on one of those, one armed bandits in Las Vegas. Given this randomness of the thing, anything which occurs that frequently within a given time. If there are only one or two occurrences, it's clear to me that you can pick another period equally as long, another one hundred seventy years, one hundred sixty years, somewhere else during that, say the future. We haven't done it yet. You may find one another time. You may find two or three another time. You may find none.

You're working with statistics of very rare events when you're looking at one or two within a time period you're concerned with; and so these are what I call statistical fluctuations are going to get you sometimes. Sometimes you'll win; sometimes you'll loose.

Q I would suggest that you haven't won here because looking at very rare events is one thing but here you would eliminate all intensity VII events and yet you say that that would have no effect on your linear, on your extrapolation?

A It's -- there's something I didn't understand,Mr Lessy. Why remove all of them? You said one was -- I think your proposition was related directly to the Ossipee Mountain complex but were subsequently moved because of that.

Q Well, two events are, I suggest with geologically controlled by local structures in Ossipee and you indicated that that was a possibility. One event had been downgraded by three independent studies --

A Mm-hmm.

Q -- and there are only three.

A Mm-hmm.

Q I mean according to Table 4 of Chinnery 1979 you state that there were a total of three intensity VII events in this so-called Boston/New Hampshire zone.

Isn't that correct?

A That is certainly true. If one of those, supposing

we can eliminate two of them and I think you may have a point -- supposing we eliminate one, because it was over estimated and we eliminate enother because it was part of a pair, that these two earthquakes in 1940 were really one, now the one that remains was near Ossipee but it was also within the area that we're talking about so it should be plotted.

Now what I'm saying is if you change that point from three to one within that period, it's a question whether you should use the data point at all if there's only one; but if you do plot it, in fact it will come very close to the line I'm drawing here so I'm not quite sure what point you're trying to get at.

Q Well, assume that the one that you have remaining under you last answer is geologically controlled by local structures at Ossipee, okay, and you've told me that in essence that the elimination of all the intensity VII events from the data based upon relationship to the structure or downgrading by independent subsequent studies yields no change in your conclusions as to the return of higher intensity earthquakes in the Boston/New Hampshire region on that could affect Seabrook?

A I'm sorry. I've got more and more confused.

I have a statement somewhere which perhaps I need to bring up at that point where I say that of course all earthquakes are related to geological structure. I am not arguing that point. Obviously that's true.

The thing that we do not know are what kind of geological structure is related to which earthquakes.

Now, I want to make that clear. First of all, all earthquakes in any place are related to geological structures. I mean unfortunately they're related to some complex way we do not understand or we understand not very well especially in Eastern U.S. So finding one near Ossipee there may be a causal connection but there is no reason to remove it from the catalog because of that, all of them have a geological correlation.

Q Now, I'm asking you to assume that it was geologically controlled, the Ossipee earthquake, okay? Would that change your opinions at all regarding the return periods of larger earthquakes in the Boston/New Hampshire region and would they affect Seabrook?

A No, it wouldn't, because earthquakes all occur, occur all across this area. It would only change my notion if we could somehow demonstrate that large earthquakes only occur near certain identifiable structures. And to my, my opinion no one has been able to demonstrate that yet. Once we do that, then the situation will be different. We do not know what other kinds of structure to look for to identify. Ossipee Mountain may be one. There may be related to earthquakes although all we have there is this one pair of earthquakes that we don't know how effective it is in producing earthquakes if it is effective at all.

What about all the other similar kinds of institutions which have no earthquakes associated with them which exist in a line. The whole White Mountains are there. And most of those have no earthquakes associated with them. Why? There's no clear correlation between these things. I don't think you can take one earthquake or one pair of earthquakes and build a whole house full of theory on that and say that's therefore the only place you're going to get earthquakes or large earthquakes.

Q Isn't that what you've done?

A No, I'm -- my approach is to say we don't know the answer to this so basically you have to allow the earthquakes to occur from anywhere within this zone.

Q All right. Now, look at Page 11 of your rebuttal. You state --, "As far as we know," -- the area that is New England -- "is not subject to active tectonism and we must therefore conclude that earthquakes somehow are a result of overall compressive stresses acting on ancient tectonic structures. Such a theory does not help us to estimate the largest earthquake that could occur in New England."

Doesn't the observations you made regarding New England not being in an area of active tectonism inherently lead to the observation that the return periods for large earthquakes must be very long?

A I can't interpret that question in a useful way. I don't know what "very long" is. I don't see it why it necessarily

has to. I think the question of whether the area around New Madrid is subject to active tectonism is something that you're getting a grade of dispassion from. I have not heard anybody use that term in relation to New Madrid.

New Madrid has a lengthy history of large earthquakes so tht absence of active tectonism is very hard to make a very, conclusions from directly.

Q All right. Now, you indicated on the very next page, page 12, that, "There is sound geologic basis for saying that New England is in some way an unusual midplate region." Do you have that?

A Yes, I have it.

Q Did you make a comparative study of the geologic tectonics and seismistic regions 1 through 5 as compared to New England? This is referring to Figure 1, the global map that you rererenced in your testimony. That's Figure 1 after page 15.

A Oh, I see. No, I did not. I think -- well, one quick comment. Is that a midplate region? It happens to be up in Northern Canada, and it, to me, anyway, looks to be in a very similar kind of geological province to New England, at least in general overall character.

Q Did you ever study it?

A I have not studied it myself.

Q Now I'm going to ask you a hypothetical question, and my experience in hypothetical questions is that -- this is off the record.

(Discussion off the record.)

Q Now, the question is: Let us assume --

DOCTOR JOHNSON: Mr. Lessy -- this is Mr. Johnson up here -- you handed this out to us. You want to give us a minute to read it.

MR. LESSY: Certainly.

CHAIRMAN ROSENTHAL: Doctor Johnson's had his minute.

UNIDENTIFIED SPEAKER: I didn't get one.

CHAIRMAN ROSENTHAL: Madam, I'm afraid you're under some misapprehension as to the nature of public attendance at these hearings. It is not the obligation of counsel to provide copies of documents such as this to spectators.

UNIDENTIFIED SPEAKER: Well, aren't the questions supposed to be oral at a hearing?

CHAIRMAN ROSENTHAL: Questions need not be oral anymore, Madam, than the testimony is all oral. There's prepared testimony that was introduced into evidence, you will recall, at the outset this morning, which was not -- it was not in an oral form. It's perfectly appropriate for counsel to provide, as he has just done, the Witness the question in writing. All right.

Q (By Mr. Lessy) Now, the question which I'll read into the record is, Doctor Chinnery, let us assume that there is a linear relationship between intensity and frequency of occurrence: log Nc=A-Bi where Nc=the number of earthquakes of intensity I or greater. Also assume that Im=MMI XII or some lesser mutually agreed upon maximum intensity earthquake. If one were to make a plot of the probability of an earthquake of a given intensity (Io) occurring, what would happen to the shape of the curve at Io=Im?

A This is -- this is a curious question. I -- I think it's a trick question, but --

UNIDENTIFIED SPEAKER: That's why I wanted to know what it was.

CHAIRMAN ROSENTHAL: Madam, I would like to ask you once again, please do not interrupt the proceeding. Your entitlement to participate -- your entitlement, excuse me, to attend these hearings is the same entitlement you would have to attend a judicial proceeding, and I can assure you that if you continually interrupt it, a proceeding before most judges, you would have been requested to leave. Now, I'm asking you once again -- I hope for the last time -- to please refrain from commenting. It is not one of your entitlements, and what you are simply doing to the prejudice of everyone here is delaying the proceeding.

Q Now, the question, Doctor Chinnery, is if one were to make a plot of the probability of an earthquake of a given intensity occurring, what would happen to the shape of the curve at Io=Im?

A I think to be sure that I answer the question correctly I must give two answers. There are two kinds of plots that one can lay. One is what we can an incremental plot, in which the frequency of occurrence at each intensity value is plotted as a function of intensity, and the other plot is what we call a cumulative intensity plot, in which case at

each intensity value we plot the number of events at that intensity and greater, and the answer comes out different, depending on which one of these one uses.

Q Incremental?

A Yes, in the case of incremental, one has a straight line and a sudden drop off at the bottom due to the fact that it never becomes larger than the maximum intensity which one can see.

On a cumulative plot, that would be bent. The curve would be bent. But, in an incremental plot, it would be a straight line and then it would stop.

Q So your testimony is that there would not be a spike or a sharp relative increase in the probability function at Io=Im using an incremental plot?

A No, there would not.

DOCTOR JOHNSON: Mr. Lessy, I'm confused. I thought that this particular hypothetical question which you've handed out and subsequently asked orally referred precisely to a cumulative plot, because that's the way you have defined ends of C, and I don't understand what the discussion of an incremental plot is if -- as it relates to your hypothetical.

MR. LESSY: Did I say incremental?

DOCTOR JOHNSON: The Witness said there were two types of plots, and then you went on to say incremental.

MR. LESSY: It should have been cumulative.

DOCTOR JOHNSON: And I was confused at that point.

MR. LESSY: It should have been cumulative. But, the Witness answered, as I understood, using a cumulative plot that there would not be the spike or sharp relative increase in the probability function at Io=Im.

Q Is that correct?

A I don't think you quite got it right yet, Mr. Lessy, quite. I know what you're trying to get at, you see, and the only question is how to explain it to the Board. The only question is if you propose on a cumulative plot that what you have is a straight line with a straight line at the end, and then that implies something strange about the incremental plot. That's what you're saying.

Q Yes, I'm sorry.

A And next on the incremental plot there is a spike. So what I'm saying is correct, absolutely correct; but on the incremental plot, you will normally have a straight line and the thing finishing at the upper bound; and on the cumulative plot, they're the ones we're all dealing with, you expect to see some curvature at the end as you approach the upper bound. Does that answer your question?

Q Yes.

A I think that's what you're talking about.

Q Yes. Now, wouldn't that be a material factor in determining the return periods of earthquakes near the upper

cutoff, where you do have this spike or sharp relative increase?

A What -- what -- what it would mean is, when you looked at cumulative plots, you're going to look for this line to start bending over at the bottom, and you would expect to see an indication of an upper bound before you actually reached it. You would expect to see the straight line begin to curl over.

Q Now, I'd like you to answer my question. Wouldn't that be a material factor in determining return period of earthquakes near the upper cutoff?

A It turns out that the amount of this curvature on the cumulative plot is really quite small.

Q So your answer is no?

A It would make insignificant effect on the calculation. I think other sources of error are much larger than that particular one.

DOCTOR JOHNSON: May I ask a clarifying question?

MR. LESSY: Yes.

DOCTOR JOHNSON: I realize that this is a hypothetical upper bound magnitude or intensity that we're talking about, but would that upper bound be a universal upper bound everywhere, or would that upper bound depend on the particular seismic region or tectonic region that you were looking at the date -- if there were -- I think you've admitted at some point there is an upper bound, even without the use of this hypothetical or invoking this hypothetical. I think your testimony indicates somewhere there

is. And my question to you is, is that upper bound intensity a universal intensity, or is it one that will vary from location to location?

THE WITNESS: Again, we have a hard time answering that question. If we take all the earthquakes in the whole world, we can see relatively clearly that -- I can quote you some papers, but let me summarize the argument. The answer's in them.

It turns out that something only a little larger than the Great Chilean earthquake of 1970, which you may remember -- it was a very large one anyway -- and something a little larger than that probably is about as large as we can have.

Now, this was extremely large, of course, and it was so much larger than anything that we're talking about here. But, I mean, it's an extraordinarily large one. Everyone has a feeling that in areas of lower seismicity one ought to get smaller earthquakes. I think many of us are a little baffled that you do, in fact, get earthquakes as large as the number digiter in places like New Madrid despite the fact that we're beginning to find out about the New Madrid area, and there is structure there, and there's structure all over the place, and we don't know why that structure is unusual.

So we looked around the world, and, surely if these regions where the -- there are not that many earthquakes, we are still surprised at the size that some of these earthquakes can be. But, still we have a -- a feeling -- and it's not one

that we can prove or document very well at all -- but we do have a feeling that earthquakes surely as large as the Chilean earthquake are not likely in areas away from the boundaries of tectonic plates. It's very hard to pin this down into a quantitative statement.

DOCTOR JOHNSON: Well, then, if you were drawing this plot that Mr. Lessy asked you to draw, the cumulative plot for the area which includes Chile -- and I realize that this has faults in it -- and some other region, the plots would curve downward towards, essentially, zero probability at different locations or different values of -- of intensity; is that correct?

THE WITNESS: Well, I'd prefer to use the word "magnitude" there. Once you start talking these kind of earthquakes, intensity scale becomes a little meaningless. Something much smaller than the Chilean earthquake still produces an intensity 12; but if you deal in terms of magnitude -- even in terms of magnitude, you still have trouble because the scale itself is not very good with these large earthquakes.

DOCTOR JOHNSON: Well, I have a problem with your answer there, sir, because this whole hypothetical is based on intensity, and I was sort of thinking -- I realize that the Chilean earthquake was the largest magnitude. But, I'm -- what I'm really getting at is the maximum intensity that you might expect measured in terms of modified mercalli intensity. Would that be a function of the region in which you were making the

plot?

THE WITNESS: The same remark goes: that most of us feel that it will vary with region. But I am still unaware of any region of the world where we've clearly demonstrated what that upper bound is, anytime. This is one of our troubles.

So even though we have this feeling that it does change with region, in some regions it's going to be lower than others. It's awfully hard to say how much lower.

DOCTOR JOHNSON: Okay, thank you, sir. Excuse me, Mr. Lessy.

Q (By Mr. Lessy) Have you done any studies or calculations to estimate the effect of such a curvature on the linear frequency intensity relationship?

A You know, I did at one time, but it was many years ago, and I can't remember the details of it.

Q Well, if you can't remember the details of it, how can you say it wouldn't have an effect?

A What I said is that it will have an effect but it will be very small. Again, I have to defer to Mr. Dignan's comment about logarithmic plots. They -- when the number of events in each point goes down so much, the effect of removing one of those points or chopping off -- removing all the data beyond a certain intensity level has very little effect, two or three points away from that upper bound. Theoretically it's there; and if you draw the picture theoretically with a fine thin

pencil, you can see the difference. In practice, of course, it becomes virtually invisible. So I think for the -- as I remember it, it's the point next of the upper bound itself which will show the largest motion; and even then it's not a very large one. And given the stature of many of these, it would be very hard to identify unambiguously.

Q Well, suppose the upper cutoff was a Modified Mercalli intensity 11, wouldn't this spike, if you will, which we were discussing, wouldn't the assumption of fewer linearity have an effect upon the estimated return effect for an intensity 11 earthquake?

A For an intensity 11, it would; but for an intensity 10, it would not have a great, great effect. So, of course, fo intensity 11, it would reduce it down to zero. For an intensity 10, there would be a slight change in the point. I do not think it would be very large at all. And for an intensity 9 it would be, I think, very hard to observe at all.

Q At the higher intensities, the curve isn't linear, is it? I mean, you accept that at this point?

A At the very high intensities, yes. All sorts of things break down once you get up either to 11, which is close to 12, which is a self-imposed upper bound of the intensity scale, or when you get close to a true upper bound, whatever that number is.

Q Well, 10's close to 11, isn't that true, for intensity?

A If 11 was the upper bound, 10 would lie in a nonlinear region, yes, I would agree with you; but I don't think it would be very far off the straight line, is what I was saying.

Q Now, on page 6, note 1 of your rebuttal testimony --

A Yes.

Q -- you state that with regard to data for Zagreb found in Makjanic 1980, that, "if the data points from intensities I, II and III are omitted, the remaining points fit a linear relationship (slope about 0.52) very well." Now, Doctor Chinnery, on what scientific basis did you determine to include data from a Modified Mercalli epicentral intensity IV through VIII and exclude data from intensities I through III?

A An intensity I earthquake is not felt. An intensity II earthquake is normally not felt, either. Intensity III, I think, is borderline. And somebody please correct me if I've got that wrong, but I think I've got that very close. This means that they are instruments. There have not been instruments for that long in the Zagreb region. I do not -- I do not remember -- if you have a copy of the paper, I would like to see it. Do you have a copy there? I forget the period over which that particular data was collected.

To get a -- a -- a catolog of earthquakes which is complete down to intensity I or II I think is really quite difficult in these days of instrumentation, except in areas of very high population and very high density of instruments.

Now, I'll guarantee that they have not had that high of density of instruments in the Zagreb region.

Q Is there any in the Makjanic article?

CHAIRMAN ROSENTHAL: Mr. Lessy, I think the reporter did not get your question.

MR. LESSY: I'm going to reask it. Doctor Chinnery requested a copy of that article.

THE WITNESS: Yes, I -- I -- do you have the floor? Do I have the floor?

Q I guess I'll ask the question.

A Okay.

Q The reporter didn't get it. Is there any information in the Makjanic article that addresses the completeness of the different intensity data during the given time interval?

A I think -- what to me it indicates is that they quote the data for a 100-year period from 1869 to 1968, and they purport to have it complete down to intensity I. I challenge any seismologist here to think that that is entirely reasonable at all. There's no way that it can be completed at intensity I back to 1869 in the wilds of the Zagreb Mountains. I -- I'm astonished even the intensity IV is complete, quite frankly.

Q Is that study focused on Yugoslavia or Iran?

A Do I have the country wrong? Zagreb; I thought it was in Iran, but I might be wrong about that. My geography is not that good.

Q Well, you have included data for intensities IV through VIII and excluded data for intensities I through III, haven't you?

A That's true.

Q Have you conducted studies to indicate what the intensity of your proposed linear relationship is to excluding data at the lower intensities?

A No, I have not.

Q All right.

A I -- sorry. I shouldn't do that, should I?

Q Now, on page 4 of your rebuttal, you state that at least your approach -- "At least my approach offers the method for computing the risk involved." It is obvious to the point, is it not, that your methodology does not constitute a complete methodology for determining the seismic design of nuclear power plants; isn't that right?

A That is true.

Q Now, on that same page you state, "A law which is linear over the length of the historical record and then changes precipitously is a very complex one, and hard to justify theoretically." Assuming for the sake of argument that such linear relationship would exist, and assuming it only for that purpose, doesn't available geologic information provide insight into the understanding of the probable seismicity of an area prior to development of an historical record?

A Well, I've said that in my testimony -- and I'll say it again -- there's no doubt in my mind that geological evidence should be used wherever and whenever it can be used. I am not trying to argue against that. I do, however, argue that in the Eastern half of the United States we have a great problem finding any geologic evidence that pertains to historical earthquake records or the earthquake record in the east anywhere, with the one exception of the New Madrid area, where just recently, and very recently, we started to look and find some of these things. I don't know of any other place in the Eastern U.S. where this has been found.

Q Are you aware of methods that estimate magnitude for earthquakes from geologic observations?

A Only in plate boundary regions, California and type regions, and some in Utah. I have not seen anything in the Eastern United States.

Q Do you accept the use of such methods for estimating earthquake magnitude?

A Yes, I think -- well, they have the great advantage of this, that you have a much longer record. I think there's still some questions as to how good they are in actually estimating magnitudes, but there's no doubt that they give a good indication at least of the magnitudes.

P-1

Q Well, I think you've stated on Pages 11 through 12 of your rebuttal testimony that in order to arrive at the size of the largest earthquake that could occur in New England in your rebuttal testimony you proceeded with the following steps which I'll summarize to save time.

First, you estimate the size of maximum fault area that would not lead to surface rupture; then you utilize a Liu and Kanamori 1980 study of five mid-plate earthquakes which had similar fault rupture system areas as a model to arrive at moments of stress drops; and thirdly and finally, you then utilized Fitch and others, 1981, plot to arrive at what size earthquakes might be expected in New England.

Is that the general summary?

A Yes.

Q I'm going to --

(Documents handed to the witness by Attorney Lessy.)

Q Now, I've handed you Table 3, which is part of Liu and Kanamori article. Have I not?

A Yes, you have.

Q Now, are not events 13, 16 and 17 in Table 3 -- oh, excuse me. For the benefit of the people who don't have that, would you summarize Table 3 just as to what it's contents are?

A It's entitled Listing of MB and MS for Mid-plate Earthquakes and it lists some four earthquakes giving locations and their magnitude values in each of these different units.

Q Now, Are not events 13, 16 and 17 in Table 3 of the five events studied in the Liu and Kanamori article -- events 13, 16 and 17 --

A Well, this is the Liu and Kanamori article.

Q Yes.

A Now, they -- what was your question again? I'm sorry. Please repeat.

Q Events 13, 16 and 17 --

A Yes.

Q -- were studied were three of the five events studied in that article? Isn't that correct?

A Yes, they were each indicated by a double asterisk and it says at the bottom these events were studied in this paper. True.

Q Are you familiar with that paper?

A Yes. The Liu and Kanamori one yes.

Q What are the surface wave magnitudes listed for the three events 13, 16 and 17?

A 6.2, 6.4 and 6. Oh, that was MB. And terms of surface wave magnitude, 5.9, 6.3 and 5.5.

Q Now, events number 16 in that Table, isn't that the same as event 3 of table 2 of that article?

A Event 16 does appear to be the same as event 3 in Table one, yes.

Q Table 2?

A Oh, table 2? Yes.

Q Now, event 16, isn't this earthquake not the largest of the five studied by Liu and Kanamori in terms of both wave magnitude moment and stress drops?

A Yes, it's the largest in terms of each of those quanities. Yes.

Q Now, what approximate intensity would a magnitude of 5.5 to 6.3 earthquake correspond to in terms of modified Mercalli intensity?

A I think this is, this point we'll have some slight disagreement.

I think a, an MB 6.4 event in fact is a rather large one and that goes along with the rather high estimate at that moment. I, I would estimate an intensity X.

Q Now, in your rebuttal testimony, Dr. Chinnery, -

A Yes.

Q -- don't you make to correlation of a magnitude approximate to 7.5 earthquake as corresponding to a modified McCally intensity X earthquake?

A Yes, I do.

Q Well, certainly, then, a magnitude of 5.5 to 6.3 can't always be an intensity X earthquake?

A Yes. Let me see if I understand your question. Now, you're saying actually it's listed -- that particular one is listed were to have an MS of 6.3. This is the point you're trying

to get at?

Q There are three earthquakes. Those events 13, 16 and 17 --

A Yuh.

Q -- have a magnitude 5.5 to 6.3.

A MS?

Q Yes.

A True.

Q Now, don't they correspond to approximately a modified Mercalli intensity VIII?

A This is -- I don't think that I would even trust my own judgment to do that particular calculation.

Q Now, in your rebuttal testimony Page 12, eight lines from the bottom which is a discussion by the way of this article beginning on the bottom of Page 11 --

A Mm-hmm.

Q -- you say that magnitude range seven points to 7.5 corresponds roughly to a maximum intercentral intensity of X. Certainly you must have some basis for that statement.

A Yes, you're right.

Q So my question is: A magnitude of 5.5 to 6.3 represented by numbers 13, 16 and 17 in Table 3, that corresponds to a modified Mercalli intensity VIII earthquake, doesn't it? Using your own --

A I think the question of what, what this paper is trying to establish. Let us just try to separate out --

Q Now, sir, I would like you to answer my question.

A I don't believe the magnitudes that are quoted in each of these papers -- I don't want to rely on them to the decimal point. And most of these problems are there because one relies on them.

You see the MS quoted in the Liu and Kanamori article are substantially lower than the MS that would be inferred from other work at Kanamori and Anderson themselves have done.

Q Did you in your 1973 article discuss converting MS to modified Mercalli intensity?

A Not MS. No. I don't think I did. Did I?

CHAIRMAN ROSENTHAL: Mr. Lessy, part of the interpretation -- it's getting close to the time for the afternoon recess. Are you able to give me some ballpark estimate as to how much additional cross-examination you'll have of this witness?

MR. LESSY: Well, we're coming down the stretch, Mr. Chairman.

CHAIRMAN ROSENTHAL: Well, would you like to be -- some stretches are longer than others.

MR. LESSY: Maybe perhaps if the Board would like, we could take a ten-minute recess and --

CHAIRMAN ROSENTHAL: Why don't we take the recess now and --

MR. LESSY: I just would like to finish this one point, if we could.

CHAIRMAN ROSENTHAL: All right.

THE WITNESS: Should I answer your question?

Q Yes.

A The question is in the '73 paper did I relate to the surface wave magnitude?

Q Mm-hmm.

A What I say, clearly says the surface magnitude.

Q Can you use that for figures, table 3 of Liu and Kanamori?

A I don't believe so, no.

Q Is your face wave --

A I don't know what I would give, mind you.

Q What did you base your statement on in page 12 of your rebuttal testimony that magnitude in the range of MS VII to 7.5? What corresponds to maximum at the central intensity X?

A That was a loose statement, Mr. Lessy. Magnitude VII earthquake occurring in the middle of the crust is a large earthquake. It is nontrivial. The MS seven value came from the stated sources which originally came from Kanamori and Anderson and was a way to change seismic moment which was determined in the Liu and Kanamori article into a magnitude value that I could use.

What you were pointing out is absolutely right, that is, they have magnitude values already in that paper which I obviously didn't go, I went through too fast to see.

If you plot these on the global plots, my figure 2 in my rebuttal testimony, it shows how moment and magnitude and stress, all three, are related together and this is the point I wanted to bring out. The higher the stress drop, the higher the magnitude that results from a given seismic moment.

MR. LESSY: Mr. Chairman, this would be a good place to take a break.

CHAIRMAN ROSENTHAL: All right. We'll take a ten-minute recess.

(Afternoon recess.)

Q Doctor Chinnery, on page 12 of your rebuttal, you made a correlation using the magnitude scale Ms. But a range of magnitude from 7 to 7.5 corresponds to a maximum epicentral intensity of X. Now, I realize, as do you, that there are other measurements of magnitude, but I want to use the one you used; and using the magnitude Ms scale of 5.5 to 6.3, I'd like you to convert that to Modified Mercalli intensity for me as you did on page 12 of your testimony.

A I have trouble with that question, Mr. Lessy. I can't give you a -- a definitive answer, and I'd like to quickly explain why. My reasoning --

Q Why don't you give me your best estimate, then. If you can't give me a definite answer, give me your best estimate of correlating Ms in a range of 5.5 to 6.3 to Modified Mercalli intensity. If you can't give me an estimate, then that's an answer.

A Rather than guess, I would say that this has to be worked out properly and done -- and I cannot do that right here.

Q Did you work it out for page 12 of your testimony?

A I'll tell you. I picked the number 7, and I thought of the San Fernando earthquake, which had an Ms of about 7; and I know that there very high accelerations and very high intensities were measured. Clearly, it was an intensity X earthquake, the San Fernando earthquake, the 1971 San Fernando earthquake, and I made the correlation that way. You may well

question whether this was a good way of doing it, but this is the way I did it.

Q Well, I'll ask the question just like this. Isn't it true that in arriving at surface wave magnitudes for the five earthquakes that you ignored direct measurement of magnitude for these earthquakes that were readily available in the article by Liu and Kanamori which you were discussing?

A I did not see it, you're absolutely correct. That was not normally the way I did things. I did not use the numbers quoted in the paper, no.

MR. LESSY: Mr. Chairman, may I have the reporter read back that answer. Doctor Chinnery stopped, started and his voice dropped, and I didn't hear the entire answer.

(Answer read.)

Q And your testimony is that you cannot make, as you did on page 12, a correlation between the magnitude Ms7 to 7.5 to intensity Modified Mercalli epicentral intensity X as you did on page 12 of your testimony; you cannot make the correlation between Ms 5.5 to 6.3 to the Modified Mercalli scale for me today?

A Clearly, it would be less. I mean, it -- it would be less by one intensity unit at least.

The reason that I hesitate to want to quote a nine as corresponding to those particular magnitude values is that I'm not sure that even a ten is a reasonable representation of a

magnitude 7 to 7.5 earthquake, which in many instances will lead you to something higher than 10. So I -- when I mentioned 10 there, I didn't say it in the sentence, but what I intended to say was at least 10.

Q All right. Are you familiar with the 1966 Parkfield earthquake in California?

A Yes, I am.

Q Do you know what the magnitude Ms surface magnitude for that earthquake was?

A No, I do not. I remember the Mb. I think it was 6.6. Is that correct? I do not remember what the Ms was.

Q Wasn't the Ms 6.0, approximately 6.0?

A That could easily have been. I do not remember.

Q Do you have any documents with you today that would serve you in looking up that 1966 Parkfield earthquake?

A I'm afraid I don't.

Q Do you know what the Modified Mercalli intensity for the Ms 1966 Parkfield earthquake was?

A No, I don't. I'd be interested to know.

Q Are you familiar with a publication "Earthquake History of the United States," revised edition through 1970 by the U.S. Department of Commerce, National Oceanic and Atmospheric Administration?

A Yes, I am.

Q Have you used that?

A Yes, at times. It's a little out of date, particularly in New England.

Q Now, is the 1976 Parkfield earthquake listed in that publication?

A Yes, it is.

Q And that was Ms approximately 6.0?

A Well, I'll -- I'll accept your stipulation on that because I have nothing to say contrary to it.

Q What is, according to that publication, the Modified Mercalli intensity of that 6.0 earthquake?

A An intensity 7.

Q All right. Now, with that information in hand, can you make the correlation which I've been asking for you to make, the approximate Ms intensity between a magnitude 5.5 to 6.3 earthquake, converting that to Modified Mercalli intensity?

A No, I can't. The whole thing that we have demonstrated in the Liu-Kanamori paper is that they are a very much higher stress drop. The Parkfield earthquake has a stress drop of 25 bars. It's in California where stress drops are low. The Liu and Kanamori paper is suggesting that earthquakes in midplate regions have much higher stress drops. This leads to a larger seismic moment. And it will need a calculation to demonstrate what effect this will have on intensity; but, in my view, that will certainly increase the intensity from the same size of earthquake by a substantial amount, and I cannot say how much

that will be.

Q Do you believe it will be unreasonable to conclude that surface wave magnitude of 7 to 7 1/2, that that corresponds to Modified Mercalli intensity 10, that surface wave magnitude 5.5 to 6.6 would correspond to Modified Mercalli intensity 7 or 8?

A It's possible, but I -- as I say, I am not able to come up with that number. I would think it may be higher, but I don't have the information at hand that I can really pin it down.

Q If that were so, in other words, if the correspondence with the Modified Mercalli intensity 7 to 8, wouldn't it be true, then, using direct measurements of surface wave magnitude, that the maximum earthquake in New England under your analysis in your rebuttal testimony would be of surface wave magnitude intensity 7 or 8 rather than magnitude 7, intensity 10 earthquake?

A Mr. Lessy, I cannot accept that this is the correct way to go about things. If one starts to talk in terms of a magnitude -- or, let's say a seismic moment earthquake -- and to me, the thing that came out of that Liu and Kanamori study was two things: was midplate seismic moments in a certain range between 10^{25th} and 10^{26th} dime centimeters. Now, these are substantial earthquakes, whatever the magnitudes which are actually put on them. And secondly, they found some evidence

that these things have rather high stress drops. Only one of them was as high as 1,000 bars, but all of their data for those five earthquakes seemed high, substantially higher than normal, in the several hundred bar range. These -- the combination of these two things, of a fairly large seismic movement and a high stress drop, is going to lead to a very substantial earthquake.

What is needed in here is a calculation of what such an earthquake would do if it were at a depth of, say, 10 kilometers under New England; what it would do to the surface in terms of ground motion; and that would obviate all these other questions about how you go from magnitude to intensity and all the other kind of things. There's a calculation which is missing.

Q On page 12 of your testimony, there is a missing calculation there, too, because you merely make the jump from magnitude 7.5 to maximum epicentral intensity 10 without a calculation. Why did you calculate it rather than taking it out of the table in the article, where there were actual measurements of surface wave magnitude?

A I'd like to blame this whole thing on the people at the California Institute of Technology rather than me. What we have here is a situation where some very eminent seismologists have written two papers which don't agree with one another, and I'm sort of caught betwixt and between and I'm using the data of Kanamori and Anderson to interpret an observation of stress

drop and seismic moment and converting it to a magnitude; and you point out, quite correctly, that the magnitude when I do it that way comes out larger than the ones they have listed in their paper. I very much wish we had one of those authors here so we could ask him the answer to the question.

I think perhaps all this indicates is that we have trouble doing these kind of conversions. And, nevertheless, I still submit that an earthquake of that movement is a very substantial earthquake. It is a nontrivial one.

And I cannot justify the number 10 exactly. I agree with you. It may be 9; it may be 11. One of the troubles in this kind of thing is to -- is to do the right calculation so that one can come up with the correct answer.

MR. DIGNAN: Has the Witness finished his answer?

THE WITNESS: Yes.

MR. DIGNAN: Thank you. Mr. Chairman, at this time I want to move that the testimony on page 12 beginning with the word "in order to convert" on through page 13 down, ending "of at least X" before the heading Roman numeral IV, on the grounds that the Witness has now clearly indicated that he has absolutely no basis for the conclusions expressed therein at all.

MR. LESSY: I support that, Mr. Chairman. I was about to do it. The important point is that, on the basis of this calculation, the Witness states on -- on page 13 that, "in my

professional judgment, a magnitude 7(Ms) earthquake may well occur rarely in the Boston-New Hampshire zone, at a depth that may be as little as 5 to 10 kilometers;" and skip a sentence, he says, "As near as I can estimate, a magnitude 7 earthquake at a depth of 10 kilometers would lead to a surface intensity of at least X." I have asked repeatedly for that kind of estimation here, and the Witness has told me he can't provide it.

CHAIRMAN ROSENTHAL: Mr. Jordan.

MR. JORDAN: First, may I understand the motion is to strike through on page 13 through up to Roman numeral IV; is that accurate?

MR. DIGNAN: That's correct, Mr. Jordan.

MR. JORDAN: Well, Mr. Chairman, I admit to being somewhat over my head on the -- the science of this, but it seems to me that it's quite clear that Doctor Chinnery has gone on at some length as to the basis for his conclusions here. The fact that he hasn't used figures from a table in the Liu and Kanamori study -- I think he has just explained that he came up with his 7-7.5 magnitude in, I gather, another way. And he has, I think, laid out rather clearly the uncertainties; and part of his message here is the uncertainties. I fail to see that there's no basis for his testimony.

MR. DIGNAN: I should also make clear, Mr. Chairman, for the record, the motion is to strike the pages I indicated of the rebuttal testimony. I don't think I stated that in my

motion, so the record is clear.

DOCTOR JOHNSON: Could you restate, Mr. Dignan, the particular testimony which you asked to have stricken?

MR. DIGNAN: Yes, Doctor Johnson; it would commence on page 12 of the rebuttal, beginning with the first full paragraph on that page, beginning, "In order to convert."

DOCTOR JOHNSON: Okay. And then over to Roman numeral IV?

MR. DIGNAN: And then page 13 ending with the line "of at least X" just above IV.

DOCTOR JOHNSON: Thank you.

THE CHAIRMAN: Gentlemen, the Board will refer ruling on the motion to strike at this time. The Board or members thereof may wish when the turn at the Board comes to cross-examine, may use, if I may use that term, Dr. Chinnery to ask questions themselves relating to some of this testimony. For that, among other reasons, the Board is not prepared to grant your motion, Mr. Dignan, at this point. I might also say that as to my mind again, recognizing that this is a proceeding not before a jury but before a Board, two members of which are versed, to at least some extent in the intricacies of the area of exploration, then the ultimate conclusion may be that it's a matter of how much weight should be attached by the Board to it. But in any event for the time being the Board will hold the motion in abeyance.

All right. Mr. Lessy.

Q (By Mr. Lessy, continuing.) Dr. Chinnery, on Page 12 of your rebuttal --

MR. DIGNAN: Excuse me, Mr. Chairman. With respect to an inquiry of my colleague brought to my attention, I'm assuming by the ruling that I need not renew the motion to have it ruled on at the close of Board examination?

CHAIRMAN ROSENTHAL: You may assume that, yes.

MR. DIGNAN: All right.

CHAIRMAN ROSENTHAL: Your motion is actually -- your motion has been deferred but the motion is --

MR. DIGNAN: I thank you.

CHAIRMAN ROSENTHAL: -- quite alive.

MR. DIGNAN: Thank you.

MR. LESSY: One second, Mr. Chairman.

(Short pause.)

Q (By Mr. Lessy, continuing.) All right. As long as Page 12 is still in, then, I have a couple other questions.

Upon Page 12 of your rebuttal, Dr. Chinnery, your rebuttal testimony in which you discuss stress drops, have you undertaken an analysis of calculated stress drops for the earthquake in New England?

A No, I have not. I don't know that anybody has. I have calculated stress drops for other earthquakes.

DR. BUCK: Excuse me. I didn't hear that last answer.

THE WITNESS: I have calculated stress drops for other earthquakes but I have not done it for earthquakes that are in New England.

Q All right.Aren't you assuming in your rebuttal that the five earthquakes that you discussed stress drops have a hundred to a thousand bars and that New England will have earthquakes with similar stress drops?

A [illegible] first part of your question, I am not assuming that. I am quoting that from Liu and Kanamori.

Q Mm-hmm.

A What I am saying is that these are mid-plate regions.

New England is a mid-plate region; therefore, if we are to say that such things do not occur in New England, we have to say why.

Q And you're assuming that the, the five earthquakes that we -- Liu and Kanamori used, that if those earthquakes occurred in New England that they will have similar stress drops?

A Many of us felt for a long time that we ought to find highest stress drops in areas of older rocks, such as the Eastern U.S., where earthquakes probably break much harder and tougher material than they have to break in California.

Evidence has been slow to come along that such things do exist.

This paper happened to come across my desk just as I was sitting down to try to do this kind of thing, December 1980 paper. It does show some evidence for it. It's very preliminary and it shows five earthquakes. It's hard to say how much one can conclude from that. I think it goes a long way with seismological intuition.

However, that such earthquakes should have such higher stress drops I would be surprised if they didn't but it's hardly someting that one could judge a case on.

Here's a little piece of evidence that suggests stress drops are higher in this type of region so I would suggest to you that earthquakes in New England have substantially higher stress drops than California given what we know today.

Q All right. Are you familiar with Street and Turcotte's

direct study of thirty two actual earthquakes published in 1977?

A Yes, I have it here somewhere.

Q Why don't you get it, please.

A Okay. I have it.

Q Is it your belief as to what the stress drops would be in New England in your last answer inconsistent with Street and Turcotte's direct study of thirty two actual earthquakes in North America including New England, which arise at estimates of fifty bars or less? Take a look at figure 4 of that article on Page 605.

(Witness complied.)

A These were computed using the Bloom seismic model.

A substantially more sophisticated calculation was carried out by Liu and Kanamori.

I, I, I can't immediately point out to you exactly the reason for the differences in these numbers. I think all this indicates that we have a lot of understanding to do in terms of characterizing the seismic source.

DR. BUCK: Had you studied this paper before Dr. Chinnery?

THE WITNESS: I found it, saw it a couple days ago and made a copy of it.

DR. BUCK: How old a paper is it?

THE WITNESS: I haven't had a chance to study it.

Q How old is the paper?

A It's 1977.

MR. LESSY: Mr. Chairman,the staff has no further questions at this time.

CHAIRMAN ROSENTHAL: Mr. Jordan, you might prefer to defer your redirect examination till after the Board has conducted its questioning. On the other hand, if you would like to conduct it at this point, you may do so. What's your preference?

MR. JORDAN: Mr. Chairman, I am inclined to defer to the Board and do mine after you finish.

CHAIRMAN ROSENTHAL: All right. Dr. Chinnery, before I turn you over to the tender mercies of my technically trained colleagues, I just have one or two questions and you'll have to bear with my ignorance.

THE WITNESS: Yes.

CHAIRMAN ROSENTHAL: But turning to your 1977 paper, which was Exhibit 2 to your testimony, I was struck by the fact that in selecting your data points for southeast United States, central Mississippi Valley and south, southern New England, you took different time periods at the various intensity levels and it was not just on the upper levels.

For example, as I recall it, on the intensity III, you began with 1930 in the southeast United States, in 1900 in the Mississippi Valley, in 1928 in southern New England. Then for intensity IV, it was respectively 1900, 1870, 1900; intensity V, it was respectively 1900, 1870, 1860; and for intensity VI, 1900, 1840, 1800; and the same thing was true

in intensity VII.

Now, I would appreciate an explanation as to why, for each of these intensity levels you had a different starting period.

I'm sure there is a, there's a simple explanation but, again, we'll have to bear with my ignorance on this area.

THE WITNESS: Certainly, Mr. Chairman.

First of all, there is a, an assumption here which is has caused us a little bit of problem. The assumption is that things happen uniformly throughout all of time. This is -- we call it stationary assumption. It's worked but let's assume that's true that earthquake occurrences is a steady uniform process in time. If that's true, then what we want to plot is the rate at which these various intensities occurred, the average interval between.

Now, obviously, if you are dealing with small earthquakes, you don't want to go back to 1800. The fact is on our reporting of small earthquakes intensity III, let's say, from 1800 is nonexistent. Those things would barely have been felt and certainly not recorded. So what one wants to do is go as near to the present as one can in order to establish the rate of recurrence of intensity III events.

Now, what I chose in, let's see, in the central Mississippi Valley, for example, for intensity III, I said 1900 to 1969.

Now, this is pulled out of thin air I admit, but what I have there are a hundred events, and a hundred events in seventy years gives me a pretty good handle on the rate at which those particular events are occurring.

CHAIRMAN ROSENTHAL: Now, why don't you have -- go back to 1900 for southeast United States on intensity III? Your back to 1930. Now, I'm -- I can understand your point that in a low intensity given the lack of any instrumentation in those early days that --

THE WITNESS: Yes.

CHAIRMAN ROSENTHAL: -- that the data would just be too unreliable.

But what I don't understand is why here we take a particular level and it's III. You go back to 1900 for Mississippi Valley and yet to 1930 for southeast United States and 1928 for southern New England. I mean, why doesn't there have to be a consistent starting point whether it were 1900 or 1928 or 1930 or whatever?

THE WITNESS: I, I would like to say there was something very subtle and important about the way I did it. I chose fairly much at random intervals which seemed reasonable to me in terms of, first of all maximizing the completeness of the catalog and, secondly, getting enough events to establish the rate of occurrence with things and I frankly did not even compare the different regions I was looking at.

I'm not sure it's an important point.

The question is that each individual case I have got long enough to get a measure of the occurrence rate.

CHAIRMAN ROSENTHAL: Well, I suppose the question is whether, had you used a uniform starting point for intensity III, and a uniform one for intensity VI, again, it might be different for III or VI but it would have been consistent for all of those areas, whether they result in terms of your uniform slope was point .57 would have still obtained because of they understand it, again, you can certainly correct me if I'm wrong, because this is not an area where I've had any training or experience at all.

The conclusion thatyou reach in this article is that the frequency intensity data from these three regions are quite parallel to one another and consistent with a slope of .57. That's your conclusion?

THE WITNESS: Yes.

CHAIRMAN ROSENTHAL: And then you've got this data here and plotting this data you produced these slopes, one for each region.

THE WITNESS: Yes.

CHAIRMAN ROSENTHAL: And at least to my untutored eye it raises the question as to whether if you'd used the same periods for each intensity level and each region again there might be a difference in intensity levels but at least for three regions you had the same information for the same intensity

that same basically, that same line would have evolved for each of the three areas.

THE WITNESS: (Nodded head.)

CHAIRMAN ROSENTHAL: Now --

THE WITNESS: I understand your point. One reason I didn't even bother to try to make them similar is that most of the intensity values, I couldn't. What I was doing was trying to use the data after the last large earthquake so, for example, then, based on United States all I could use was data from 1900 on; and you'll see that intensity V, for example, in southeastern United States appears in 1900 to 1969.

CHAIRMAN ROSENTHAL: Why was that that youcould only --

THE WITNESS: The large earthquake there was in 1886 and I wanted to go to about 1900 to get away from the worst of the aftershocks of that event and pick up then the seismicity of that particular area.

Now, the central Mississippi Valley, the large earthquakes there happen in 1811, 1812 so I can go back further and there my intensity file goes back to 1870.

One is trying to draw a compromise of time. I think the question of which particular time periods is taken is not nearly as important as were the time periods taken long enough to include enough events to get a reasonably good estimate of the occurrence rate and it should not matter which time period one takes.

Now, I have not gone through and redone the graphs using different time periods to see what the differences would be and I think in a sense this is what you're asking. I have not done that and I wouldn't guarantee they will not be minor changes. I think the changes in the graphs will be very small. I will be surprised if they were large.

It's the kind of thing that one does not normally do. One doesn't go through all the different adoptions on these things.

CHAIRMAN ROSENTHAL: But you believe that for whatever period was selected that there were enough events in those various categories to make the results which your conclusions which you reach meaningfull?

THE WITNESS: Yes. I, I certainly -- where there is at least ten events in the time period, I feel quite comfortable with it but personally there are some cases five. Those points would worry me a little. Where there is substantially less than five, I personally discount those data points and I have not tried to use them much in the fittings.

This is the kind of subjective element which is very hard to get away from in looking at this kind of data, which data points does one take to be reasonable and which does not?

But your basic question, I cannot answer it. I haven't tried too many various combinations to see what happens if you do.

CHAIRMAN ROSENTHAL: Well, if someone else were to embark upon this same study, he or she might have selected different time periods?

THE WITNESS: Yes.

CHAIRMAN ROSENTHAL: You say this is a selective, a subjective selection by you and if I may use the term it was since arbitrary --

THE WITNESS: Y es.

CHAIRMAN ROSENTHAL: -- And you felt this provided you with enough data and that you had your reasons for taking it back in one case not further back than 1900?

THE WITNESS: Yes.

CHAIRMAN ROSENTHAL: This is the southeast United States and Mississippi Valley, you are, again, you are back as early as 1840 and in southern New England as early as 1800, and someone else may have reached the conclusion as to what periods --

THE WITNESS: That's right. And if I made -- there is also the question which regions to study. One could work in infinite number of combinations of different spacial regions to study.

You may well ask in exactly the same way why did I pick those particular areas to plot.

CHAIRMAN ROSENTHAL: That was my next question.

THE WITNESS: And it's equally valid and it's equally pertinent.

CHAIRMAN ROSENTHAL: Mm-hmm.

THE WITNESS: They were chosen simply because people have already published catalogs for those particular reasons. So perhaps I was being -- to give you an example, I've got a computer version of the whole data set and I have put it on a computer so I can put out this kind of graph for any particular area on any time interval that I want.

But frankly that doesn't help me. Now, I can do it for any area and I don't know which one to do it for. There are limitless combinations of sizes and shapes, various -- time intervals over which one might do it.

CHAIRMAN ROSENTHAL: Now, you could have selected California?

THE WITNESS: That particular date is set for the eastern United States, the one I have.

CHAIRMAN ROSENTHAL: Well, but for -- I mean your basic theasis, as I understand it, is, it's universal in application, isn't it? Doesn't it depend upon region?

The WITNESS: I don't want to make a big point about that. I did mention that several values in the issue is that seem to match and I said that even in the Western as I quoted one answer where it seemed that western U.S. areas were somewhat similar.

You'll notice that paper which is referred to by the NERC staff in Sacramento, a similar slope.

I have a feeling that this slope is in fact more widespread than just the eastern United States but it's more of a feeling and I cannot pretend I've been to enough places to really justify that.

CHAIRMAN ROSENTHAL: So at this point your confidence then is in terms of the east coast and I guess as far west as the Mississippi Valley?

THE WITNESS: Yes, that's right.

CHAIRMAN ROSENTHAL: The eastern half of the --

THE WITNESS: I --

CHAIRMAN ROSENTHAL: -- United States?

THE WITNESS: Yes. Is the way you would -- there's a suggestion it might be more than that.

CHAIRMAN ROSENTHAL: And you are persuaded that this is true in this half of the United States at least irrespective of the particular geological conditions that exist in this specific area?

THE WITNESS: Well, this does not deal with geological conditions. If the data all consistent with a uniform slope, then we are forced to, instead, to ask the question: How can we possibly get a uniform slope and we have some variation in geological conditions? IN other words, you have to twist the question backward. They seem to indicate imperically a relatively uniform slope and that's an interesting question and one that we do, have not achieved in that equal explanation for the

moment. It would appear that this kind of graph, this indication indicating something which is some crust or property which does not vary much from one place to another but what that property is, I don't know. Maybe the scale of the inhomogeneities in the earth's crust, for instance, so it might not be something that has a clear cut correlation with surace measurement of geology although it obviously in the long run has to be somehow related to those measurements.

CHAIRMAN ROSENTHAL: Before I give you Doctor Buck I've got one question that I think is probably designed more to satisfy my curiosity than to resolve any of the issues that are presented in this case. But, in Table 2 of your 1973 paper, you have these references to Rhode Island earthquakes going back to 1568, and four of them indeed in the later half of the 16th Century. Now, were these Indians that recorded the intensity 7 level for those earthquakes? As I recall it, this was before there were any -- any permanent settlements of -- of our English ancestors in this country, and I'm just sort of surpirsed to find them in there at all.

THE WITNESS: Yes, exactly. They were in the catalog issued by Smith. I believe there are a number of similar earthquakes in the catalog, which have subsequently been put together by Doctor Gerdes. They are clearly the result of Indian tales, and I -- I just can't make out how reliable they are. I would really like to hear Doctor Holt's opinion on that sometime.

CHAIRMAN ROSENTHAL: Well, I realize that you don't use them in your calculations.

THE WITNESS: No.

CHAIRMAN ROSENTHAL: Doctor Buck.

DOCTOR BUCK: Well, Doctor Chinnery, I have a lot of questions, and I know that Doctor Johnson also has a lot; and if his are as disorganized as mine in coming in an off-the-cuff basis, why you better expect us to jump around.

THE WITNESS: Fine.

DOCTOR BUCK: And I'm going to ask you a few, and then turn it over to Doctor Johnson to ask some more questions of his own, and then you can some back to me, okay.

Let me ask, first of all, there seems to be a great deal of unknown territory as far as seismology is concerned in New England. What possible sources are there for earthquakes in New England, do you know?

THE WITNESS: I don't think we know the causative mechanism of earthquakes in New England.

DOCTOR BUCK: What possible sources could there be? Have you any guesses as to what --

THE WITNESS: Yes, I have some guesses.

DOCTOR BUCK: Could you let me have them?

THE WITNESS: I think the earth crust is extremely complicated. There are many junctions and boundaries of various straights and shapes in an area that's been subjected to the mountain building. This is an area where the continent collided back some 500 million years ago. The remnants of that are so extremely complex structures throughout New England of very complicated shapes.

Now, the thing that we do know about New England is that it's under -- undergoing stress. It's being squeezed. We don't know why.

DOCTOR BUCK: Well -- all right, go ahead. I'll get

to that.

THE WITNESS: There are measurements of stress in the ground which we can make -- they have been made -- and it appears that the area is being squeezed.

Now, when you take a very mixed-up and heterogeneous material like this and you squeeze it, you get things which we call stress concentrations. These have been alluded to in several studies as possibly being related to some of the larger earthquakes in New England. For example, the Ossipee Mountain earthquake has been suggested, with a result of a stress concentration around that Ossipee Mountain structure. It's -- it's believable but very hard to prove.

What we don't -- what we do know is that there's a lot of other earthquakes in New England, and I think of moderate size, going all the way down to small size, and we have very little idea about many of these. But, intuitively, they are due to a very similar mechanism. What we don't know, though, is the scale. We don't know quite what's going on. We don't know if there are little kinks down there in the earth's crust, which can accumulate stress over a long period and then suddenly give in a large bank. It seems entirely possible that they can, and this is presumably what the 1555 earthquake around here was due to. Presumably the Charleston earthquake was due to this kind of mechanism. It's a very general concept. It's not anything that you can readily use.

So when you ask about sources, I think there could be a whole lot of geological contacts, changes in properties, in homogenetities which are concentrating stress, and over a period of time building up the stress in a very local area, to the point where it will break the ground. So I think that earthquakes in New England are a local process. This is my personal belief, and it is a belief, because it's very hard to get firm evidence on this.

DOCTOR BUCK: All right. Do you have any studies such as focal point analysis on any of the recent earthquakes?

THE WITNESS: There are; I have not done any of them.

DOCTOR BUCK: Well, do you know whether there is a uniformity about the focal plane, the uniformity of depth in the earthquakes, for example, and uniformity of direction of faulting?

THE WITNESS: We have some depth of earthquakes in New England. I'm not sure how good they are. Actually, we have trouble measuring depth of earthquakes anywhere unless they're right at the surface. Those depths, they were quoted in the 1975 Applicant testimony as being something of the order of 10, or 20 or 30 kilometers. In other words, they were quoted as being in the crust and somewhat below the surface. I think that's probably a reasonable --

DOCTOR BUCK: 10,000 is much the same as they are in California, isn't it?

THE WITNESS: Ten -- no, in California they reach the surface. And it's clear that we have not had any earthquakes break the surface here.

DOCTOR BUCK: The focal center given very often is 10,000.

THE WITNESS: That's true, 10 kilometers, that's right.

DOCTOR BUCK: That's what you're talking about here, is the depth of the earthquake?

THE WITNESS: That's right, yes.

DOCTOR BUCK: Okay, go ahead.

THE WITNESS: In terms of fault plane mechanisms, I have seen these things. My impression -- and I'm sure someone will correct me if they believe otherwise. My impression is that there is some consistency with the direction of the overall impression with the area, but otherwise not a lot of overall consistency in this area.

DOCTOR BUCK: Well, is the general area of New England -- let's go from -- from, oh, say Boston on, northeast from there so on; is there a general picture of -- of past earthquakes, past faulting, for example?

THE WITNESS: There have been many geological studies of faulting. There is, to my knowledge no evidence that any of those faults have moved since the -- well, in the last 100 million years.

DOCTOR BUCK: All right. My question is, then, is

there any indication that the fault plain solutions are used -- that people are getting now on present earthquakes would indicate a similar direction of faulting?

THE WITNESS: I have not seen any such study. I don't think so.

There have been some at New York state where there does seem to be a correlation with some well-known faults, but I have not seen such a thing in New England. I may have missed it. There's very little data, I can assure you about this.

DOCTOR BUCK: There are ring dikes I think both north and south of Seabrook, are there not? Differing in age, but there are ring dikes of some type or intrusions, at least?

THE WITNESS: Intrusions form a large belt that run from, well, Northern Vermont or even up further. They go all the way up to Canada down vaguely through the Cape Ann area.

DOCTOR BUCK: All right. Now, is there any reason to believe that an earthquake occurring in New England as a result of compression forces, shall we say, will have any connection on the occurrence of another earthquake 25 kilometers away, for example?

THE WITNESS: I've never heard anyone address this problem. My own answer or feeling is that I would be very surprised. I think the fault -- I mentioned the dimensions of the break of these earthquakes in this part of the world have to be small, otherwise we would be seeing breaks we could identify.

They have to have small source dimensions.

DOCTOR BUCK: All right. Let's just divert just for a moment. As I recall the California studies on earthquakes and the calculations that are made out there on possible recurrence rate of earthquakes are all based on measurements on a single fault; for example, they may take it on part of the San Andreas, or they may take it on the Santa Cruz or something like that and work out a recurrence rate of various types or sizes of earthquakes on an individual fault. Now, that to me seems a lot more reasonable that recurrence rate would be effective measurement of something happening on an earthquake when things are, in a sense, connected together. Now, on what basis -- as you say, there doesn't appear to be any connection between earthquakes in New England, one on the other, being of a small size. Why should recurrence rate mean anything?

THE WITNESS: I wish I knew the answer to that. I think there's a couple of comments of interest. I received the bulletin, for example, of the Southern California network of earthquakes in Southern California. Granted, that California has a whole master of faults. Nevertheless, the majority is that they determined are not relying on faults. I think in overa-l --

DOCTOR BUCK: But they, I believe, are taking on the direction of the focal plane, the analysis of the earthquakes in connections connected to the fault.

THE WITNESS: Connected in the form of the stress

test.

DOCTOR BUCK: Not on the fault itself?

THE WITNESS: Once you get off the fault, in other words, once you get away from the earthquakes large enough to cause a significant flip of the two sides of the fault, I'm not sure that you're dealing with a rather similar situation as in New England, even in California.

True, your fault mechanism may be lining up because your stress pattern is unifrom throughout California, but I think many of those earthquakes are, in fact, being triggered. They are a result of similar kind of inhomogenetities, local homogenetities that you find here. This may be one reason that we find such similar kind of earthquakes.

DOCTOR BUCK: Perhaps they are, but there seems to be a lot more reason to connect a group of faults and look at them as something happening on a fault, since you know that they are related to a particular structure; and certainly, there is a different rate, as I recall. Some of the things that I've learned about California seismology, there's a different rate of occurrence on transverse faults than there are on the Andreas.

THE WITNESS: Yes.

DOCTOR BUCK: Now, in New England I see no such connecting item, and I wondered just what's the basis for even assuming a recurrence, other than pure chance.

THE WITNESS: I don't think I'm assuming. I think when one plots something like this the question is, does one get anything that looks reasonable at all; and I think referring to my 1979 paper that you do in fact get things which are a strong indication that there's some underlying process -- I don't know what it is -- that gives a uniformity, that large ones are in some sense connected to small ones. I think the connection may be a little tenuous. It may be, as I say, a scale property of the inhomogenetities in the earth crust, but I think that connection is a -- is there.

DOCTOR BUCK: Well, I'm bothered by the fact, a physical connection or any real physical law that I can imagine that would produce a linear thing other than by pure chance, and how far that chance goes, to what level of earthquake, I don't know.

THE WITNESS: Yes. I think the experience around the world has been that anywhere you go, whether it's a plate boundary region or a nonplate boundary region, that you try to plot a frequency-magnitude, or a frequency-intensity or frequency whatever you have there; you get a remarkably straight line. And this is an imperical observation which is quite difficult to explain. Nobody has come up with a proper explanation of it yet, but that is not to say it's not valid.

DOCTOR BUCK: Okay. Let me just ask a couple more, and I'll turn it over to Doctor Johnson.

You say you have measured stress fields in New England.

Has it been -- has there been measurements made both north and west of Seabrook and south and east of Seabrook? Shall we say the Boston area and New Hampshire area? Has there been separate measurements made of those two?

THE WITNESS: I wonder if I brought a paper that would -- just one minute.

DOCTOR BUCK: Well, look, I'll tell you what we can do. We can go on, and if you wouldn't mind looking that up tonight --

THE WITNESS: I will see what I can find.

DOCTOR BUCK: -- and you can bring it with you tomorrow.

THE WITNESS: There are just a few. I know the name of the man.

DOCTOR BUCK: How are they made?

THE WITNESS: By drilling and -- doing over coring and drilling.

DOCTOR BUCK: And they all came out of a high-compression basis?

THE WITNESS: Yes, there was some misalignment, but, generally speaking, the directions of compression came out very roughly east-west. There's a -- there's a -- generally speaking, a slight change in direction as you move through from the Northeast to the Southeast of the United States, but there is some consistency there.

DOCTOR BUCK: At what depth were these made?

THE WITNESS: Drill-hole depth, which is about a thousand feet or so, 2,000 feet.

DOCTOR BUCK: Well, what does that have to do with the stresses that one finds at 10,000 kilometers?

THE WITNESS: 10 kilometers. A very good question. We cannot drill that deep and make this computation.

DOCTOR BUCK: So you have no measurements down at the point of --

THE WITNESS: No measurements, no.

DOCTOR BUCK: No measurements at that point, none of any kind?

THE WITNESS: No measurements at all, none of any kind.

DOCTOR BUCK: Well, did Kanamori do the test that we were just talking about a little while ago. I don't have that paper. The staff just gave it to you, and they were concerned with -- concerned with these midplate earthquakes?

THE WITNESS: Yes.

DOCTOR BUCK: Did they measure stress -- how did they measure stress?

THE WITNESS: They measured them by fitting a model of a seismic source to the observed wave forms that were cleaned.

DOCTOR BUCK: And how did they measure volumes on those things? Did they have any -- is this a constant stress all across or --

THE WITNESS: Well, the area of the fracture which

they come out with is measured by the --

DOCTOR BUCK: But they did it by seismic moment?

THE WITNESS: Well, you basically look at the spectrum of the signal, and the signal spectrum has a break in it; and from the position of that break, using some models, we can make an estimate for the size of the source area which fractured.

DOCTOR BUCK: How many regions do they model? In what regions did they model?

THE WITNESS: Regions, you do this earthquake by earthquake. So you get the signals that you get from an earthquake, and then you fit them with a model to the source.

DOCTOR BUCK: All right. Did they do this for the New Madrid area, for example?

THE WITNESS: No. They did this for five events that were shown in my testimony, and they are scattered around the world; many in odd places, I agree with you. So I can say this is good evidence. It's suggestive evidence. There was one in Alaska; one in Northern Canada, across to Greenland; one in the mid Atlantic -- two in the mid Atlantic, in fact, but away from the region; one in Australia, which is perhaps a reasonable area; and the interesting thing was that each of them seemed to indicate a somewhat higher than normal stress.

DOCTOR BUCK: And supposing one had a much higher than normal stresss, and you got a series of earthquakes that were in the range of IV or V, VI, would this not mean that even

those earthquakes were occurring in a very small volume?

THE WITNESS: Yes, that sounds reasonable.

DOCTOR BUCK: All right. In that case, would they not indicate a higher epicentral intensity than normal?

THE WITNESS: Yes, I -- I would have expected that.

DOCTOR BUCK: Well, do we have that sort of a record in New England?

THE WITNESS: I think we have remarkably few measurements of intensity from earthquakes in New England. There are, obviously, some in very recent years, and I haven't kept up with the complete record. The NRC should be able to answer that.

DOCTOR BUCK: Reed, do you want to go ahead for a while, and I'll pick up after you?

DOCTOR JOHNSON: I'll add, starting with the last answer, I don't think I understood it. I thought Doctor Buck asked if there were any high stress drops associated with the earthquakes in New England, New England being a midplate area.

THE WITNESS: Mm-hmm.

DOCTOR JOHNSON: Would not the Ms intensities be large as a result of those stress drops? And I thought we had a fairly decent record of the intensities of the New England earthquakes.

THE WITNESS: Yes, but I understood the question to mean that for a given magnitude of earthquakes, would not the

intensities be higher than normal, and that was what I said yes to. Now, they do not have many earthquakes in New England where we have both magnitude and intensity. This is the trouble. We have historical ones where we have only intensity.

DOCTOR JOHNSON: Okay, forgetting magnitude. I'll have to be more careful. If you have an earthquake with a high stress drop at a modest focal depth, would you not expect a high intensity measurement epicentral? Is not a high stress drop in an earthquake an example or a cause for a high magnitude -- I'm sorry, a high intensity of shaking on the surface in the epicentral region?

THE WITNESS: Once again, I have to rephrase it as before. Supposing you have a magnitude IV earthquake, then in California it may give one intensity. Here, having a higher stress drop, it may give a higher one. But, obviously, if you had a different size of earthquake to start with, you get a different size of intensity. So you can't really talk about the intensity without talking about the underlying size of the earthquake that caused it.

DOCTOR JOHNSON: Well, would you say that intensity on a Modified Mercalli scale could be related generally to peak acceleration measured in an earthquake?

THE WITNESS: Well, I really shouldn't answer that question, but let me say my one observation on the subject. I have seen a number of these compilations of plots of

acceleration against intensity, and I have been constantly amazed at the enormous scatters in those diagrams. You can get --

DOCTOR BUCK: Enormous what?

THE WITNESS: Scatters. You can get almost any accleration you want from almost any intensity you want with some slight limitation.

DOCTOR BUCK: Please, may I ask this question here? Do the spectra themselves, the picture, even though the spectra is scattered -- I'm talking about the actual spectra of the earthquake itself as it's perceived. Does that look tremendously different in New England than an earthquake of equivalent size in California, Madrid?

THE WITNESS: I have not seen enough recordings of earthquakes in New England, and some of the other people here probably know the answer to that question better than I do.

DOCTOR JOHNSON: When I'm speaking of intense -- shaking as measured by peak ground accleration, I am referring to a near field measurement.

THE WITNESS: Yes.

DOCTOR JOHNSON: Are you aware of the paper by Hanks and Johnson in which they display the peak ground acceleration as in the near field for a large number of earthquakes in different magnitudes can reach a general conclusion that the peak ground acceleration is not strongly dependent upon the magnitude of the earthquake?

THE WITNESS: I think I saw that paper. I did not remember that conclusion. I have, however, seen attempts to correlate intensity and they really come out with the same thing although they often don't phrase it that way.

The fact is it's very hard to see clear-cut relationship of acceleration to intensity in just the same way as magnitude in the Hanks and Johnson study.

DOCTOR JOHNSON: Well, if I have a large volume source of fault which ruptures over a long length and a, let's say, five kilometers' width --

THE WITNESS: Yes.

DOCTOR JOHNSON: -- that would be a large magnitude earthquake.

If I have this same event in which the length, this dimension of a rupture fault is small, so I have two

understanding of the earthquake would indicate to me that if we were dealing with stress drops of that magnitude that we would be witnessing large values of intensity in the epicentral region. And my question to you is: Are the observations which exist consistent with a set of earthquakes which have very large stress drops?

THE WITNESS: I don't think being on the surface of the earth that we're ever necessarily in what you call the near field of these earthquakes. The vast majority of earthquakes in New England are quite small. Most of the ones that we had an instrumental record for are quite small. The size of the break is going to be rather small, perhaps of the order of a kilometer, and there we're hitting perhaps 10 or 12 kilometers above that thing so we're not in the near field in the way that you would express it.

DOCTOR JOHNSON: Well, what then is the point that you were making with regard to large values of stress drops?

THE WITNESS: Okay. The whole question is: Is there any way that we can try to put some limit on the kind of earthquake that might occur in New England?

Now, New England is not on plate margins where we understand the process is going on. It is logical at least to say, "Let's look at all midplate regions and see what's happening elsewhere." At least that may give us a beginning of the thing.

Now, we haven't been recording very long. The paper

certainly one is going to have a, or by virtue of different observations of different places even regardless of the complexities in the ground outside the fault zone.

The radiation pattern is going to be extremely complicated because of this.

DOCTOR JOHNSON: Well, what I'm trying to get at is if the stress drops are associated with the rupturing fault is large, like a thousand bars, would I not expect a very large measured intensity in the near field region?

THE WITNESS: You're leaving out one variable. This is why I have to hesitate and I'm never quite sure how to answer.

You are saying that regardless of the size of the thing if it has a big stress drop you also get a big intensity?

I can't help thinking there have been some limitations to that. The inclination is to say that if you're close enough to it, you may be right but let's face it, one of these earthquakes of intensity I or II may occupy a distance measured in meters so you've got to be very, very close to get in the near field.

So if it becomes very complex to answer your question.

DOCTOR JOHNSON: Well, we -- you're -- the implication of your testimony, particularly the rebuttal testimony that we were dealing with a while ago is that the stress drops or intraplate earthquakes would be large in the order of several hundred to a thousand bars; and my limited intuitive

earthquakes -- a large-magnitude earthquake, a small-magnitude earthquake -- but they both have the same width of faulting and I made a measurement in the near field of both of those, would I not likely get roughly the same intensity?

THE WITNESS: Okay. Let me just be sure I know what you're asking. Can I rephrase it and say that the same faults area --

DOCTOR JOHNSON: No. These two have different fault areas. They have the same fault width. One has a long length, the other has a short length and the measurement, however, is made close to the region of the faulting in both events.

THE WITNESS: Oh, I see what you're getting at. Yes.

My view is that when you get to that kind of level of detail of a problem the seismic problem, the only way to tackle that is by some of these very complicated sources of calculation.

In other words, it tends to depend very strongly on the fine details of that process.

If you recall, you're close enough and close enough means within a few kilometers the way you phrase the question.

DOCTOR JOHNSON: Within 10 kilometers.

THE WITNESS: When you get into the kilometers of a fault like that, you're very susceptible to minor fluctuations.

Now, we know faults are not bald, blank slabs. They have many and expertise complications, all kinds, and those are the things which kind of govern what goes on. And almost

by Liu and Kanamori represent only in the last ten years. Now, in that ten-year period, they came up with f ve fairly well-recorded earthquakes and these rather low seismicity. Also low seismicity when you get into the interior of a plate. Five earthquakes, where they had enough information where they could really try to get at some of the properties of the seismic source which is what they did and came out with those numbers.

DOCTOR JOHNSON: Well, would you call the New Hampshire/Boston region a low seismicity region or were low seismicity a characteristic which allowed the measurement to be made?

THE WITNESS: No. I call it low seismicity because all the whole Northeastern United States is low seismicity; all areas in the middle of the tectonic plates are low seismicity compared to those on the edges which is, where most of the earthquakes occur.

DOCTOR JOHNSON: But are some regions lower than others?

THE WITNESS: Well, that's obvious they are, you know. We look around and see variations within that low level. Yes. But --

DOCTOR JOHNSON: You mentioned the locale of those five earthquakes, and you said that the value of these volume or the area of rupturing was implied from records?

THE WITNESS: Yes.

DOCTOR JOHNSON: All of those -- well, the ones you mention to me and you pointed this out in your testimony were

in rather peculiar places. I find this a little hard to understand why they were well-recorded if they were in no -- they were in Alsaska. And as I understand the major Alaskan earthquake of 1965, was, there was not even a record of that upper Canada, in the middle of the Atlantic Ocean. Why were these events well-recorded? I don't understand why there were good records for those at least those three.

THE WITNESS: These all were MB-6 events and now MB-6 events were recorded all around the world. Now, I think what we're saying instead there just aren't many MB-6 events in this type of geological region in the middle of the plates.

DOCTOR JOHNSON: And we only have a ten-year record of them?

THE WITNESS: We have very short records in which to do this thing. I think all you're saying is that we have very, very -- seeing this record in time and that it happens that these were the ones that occurred during that time period. That is not meant to say they weren't occurring elsewhere and that over the next ten years we may get another half dozen somehwere else.

DOCTOR JOHNSON: I think I understand that sort of thing.

Would you like to explain a little to me what appears in your figure, one of the rebuttal testimony? There's a plot of, lower part of Figure 1 --

THE WITNESS: Yes.

DOCTOR JOHNSON: I -- his log is the longer of the area of the rupture?

THE WITNESS: This is exactly right. So in No. 2 on the vertical axis means 10^2 or a hundred square kilometers.

DOCTOR JOHNSON: Yes, I got that.

And the longer rhythm of a seismic moment as I understand seismic moment, I know how it's defined as a product of a slip --

THE WITNESS: That's right.

DOCTOR JOHNSON: -- an area --

THE WITNESS: Yup.

DOCTOR JOHNSON: -- and the rigidity?

THE WITNESS: (Nodded head.)

DOCTOR JOHNSON: Is that measurement made prior to an event or subsequent to an event, an earthquake, I mean? I mean do you -- what I'm asking you -- is this seismic moment something that you know before an event and you can estimate therefore?

THE WITNESS: No.

DOCTOR JOHNSON: Estimate a --

THE WITNESS: It's an observed quantity.

DOCTOR JOHNSON: Observe the quantity?

THE WITNESS: Would you get it from the low frequency of the end of the seismic spectrum --

DOCTOR JOHNSON: All right.

THE WITNESS: -- so you take the spectrum and you produce it. You look at it until it becomes flat, the low end of the spectrum, and the level of that gives you a measure which you can convert into a measure of seismic moment.

DOCTOR JOHNSON: Then the parameter of stress drop that appears, would you relate that stress drop to the quantities which appear in the seismic moment?

THE WITNESS: Okay. The lines on that particular picture labeled 10, 60, 100 bars and 1,000, were all taken from an earlier paper of Kanamori and Anderson which they published in 1975 and I have it here in case you would like to see it in which they -- I will have to give you a title. Just one minute.

(Short pause,)

THE WITNESS: Title of the paper is Theoretical Basis of Some Imperical Relations i Seismology by Kanamori and Anderson Bulletin of Seismological Society of America, Volume 65 at 1975, page 1073.

And they go through a series of basic theories about that seismic source and they show how stress drop source area and seismic moment are all related.

They are geometrically related. These quantities, these are all geometry.

They found in 1975 rather to their astonishment that there was a singular consistency of earthquakes, that regardless

of where they happened they all seemed to have about the same stress drop and the numbers they quoted there are between 10 and a hundred bars.

Now, that was some early work. It was '75 and a lot of things have gone on since then.

The more we have looked at things, the more we've found that range of stress drop is growing larger and this recent paper by Kanamori -- Liu and Kanamori, is simply a reflection of that. They are starting to find now some earthquakes with some rather large stress drops.

DOCTOR BUCK: Doctor Chinnery, before you go on, I am missing a point here and I think you are, too, Reed, when you have stress drop. How do you do it? You must have some idea of what stress is before the earthquake in order to measure stress drop.

THE WITNESS: Well, you do not measure stress drop.

Let me see. These are -- how can I easily convince you of this? Seismic moment is a geometrical quantity. That is frequently written down as the product of the surface area of the fault and its displacement at the elastic rigidity of the material that's within and it's a geometrical quantity.

What I'm saying is that stress drop also turns out to be a geometric quantity with a different combination of these same parameters and the net result is you have three things which you can convert any one into the other. I'm sorry, any two in

the other.

DOCTOR BUCK: You will come out with a dime centimeter situation which is really work that's been done in moving the two sides of the fault and in order to do that you've got to have some stress measurement somewhere or know what they were?

THE WITNESS: The stress drop is the change during --

DOCTOR BUCK: It's the change?

THE WITNESS: Yes. And that doesn't depend on what the original stress was, at least to a first approximation.

DOCTOR BUCK: But you have got to have a difference of something.

THE WITNESS: A difference, yes.

DOCTOR BUCK: The problem is how do you get it?

THE WITNESS: Let's think of it this way: you have a given fault area, for example. The more that the slip is on that surface, the more stress that's going to be relieved.

DOCTOR BUCK: Well, that gives me about how much friction you had to begin with, I guess. But --

THE WITNESS: The amount of stress difference between the final stage and the beginning stage would depend on the moment on that little piece of fault.

You see, if you can accept that then you can see why once you have determined the amount of slip you can compute the stress drop. So what I'm saying is this: you observe the seismic moment. You observe the fault dimensions and you can compute the

stress drop and that is what's done in this diagram.

DOCTOR BUCK: What you're telling me is you compute your stress drops from the measurement of the amount of mass that's been moved so many centimeters?

THE WITNESS: Essentially.

DOCTOR BUCK: Or meters or whatever else?

THE WITNESS: Essentially you could say it's the calculating energy change, if you wish. These things must all be related.

CHAIRMAN ROSENTHAL: I think at this point we'll adjourn for the evening. We'll resume at nine o'clock in the morning. The Board still has questions remaining before redirect examination takes place.

As far as the schedule for tomorrow's concerned, I think at least up to the time of adjournment it will remain as it is, as it was today; I expect adjournment -- we'll just see how we stand by mid afternoon.

I am, as I indicated to some of you earlier in the day, quite anxious to finish the intensity issue no later than Wednesday in light of the fact that we have Doctor Trifumac on Thursday morning.

So with that, the proceeding stands adjourned until nine o'clock in the morning.

This is to certify that the attached proceedings before the

ATOMIC SAFETY AND LICENSING APPEAL BOARD

in the matter of: Public Service Company of New Hampshire, et al.

Date of Proceeding: [illegible]

Docket Number: 50-443 + 50-444

Place of Proceeding: Nashua, New Hampshire

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

[illegible]

Official Reporter (Typed)

[illegible]

Official Reporter (Signature)