CENTRAL FILES

NUCLEAR REGULATORY COMMISSION

IN THE MATTER OF:

PACIFIC GAS & ELECTRIC COMPANY

(Diablo Canyon Units 1 and 2)

Docket Nos. 50-275
50-323

Place - Avila Beach, California

Date - 16 December 1978

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UNITED STATES OF AMERICA

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(Diablo Canyon Units 1 and 2)

Cavalier Room,
San Luis Bay Inn,
Avila Beach, California.

Saturday, December 16, 1978.

The hearing in the above-entitled matter was reconvened, pursuant to adjournment, at 8:30 a.m.

BEFORE:

ELIZABETH BOWERS, Esq., Chairman,
Atomic Safety and Licensing Board.

DR. WILLIAM E. MARTIN, Member.

GLENN O. BRIGHT, Member.

APPEARANCES:

BRUCE NORTON, Esq., 3216 No. Third Street,
Phoenix, Arizona 85012.

MALCOLM H. FURBUSH, Esq. and PHILIP CRANE, Esq.,
Legal Department, Pacific Gas & Electric
Company, 77 Beale Street, San Francisco,
California.

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On behalf of the Joint Intervenors:

DAVID S. FLEISCHAKER, Esq., Suite 602 1025 15th Street, N.W., Washington, D. C.

STEPHEN KRISTOVICH, Esq., Center for Law in the Public Interest, 10203 Santa Monica Boulevard, Los Angeles, California 90067.

On behalf of the Regulatory Staff:

JAMES R. TOURTELLOTTE, Esq., MARC STAENBERG, Esq. and EDWARD KETCHEN, Esq., Office of the Executive Legal Director, U. S. Nuclear Regulatory Commission, Washington, D. C. 20555.

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PROCEEDINGS

MRS. BOWERS: Let me check and see if the parties are ready to commence.

Mr. Norton?

MR. NORTON: Yes.

MRS. BOWERS: Mr. Fleischaker?

MR. FLEISCHAKER: Yes.

MRS. BOWERS: And Staff?

MR. TOURTELLOTTE: Yes.

Whereupon,

STEWART SMITH,

BRUCE BOLT,

GERALD FRAZIER,

and

DOUGLAS H. HAMILTON

resumed the stand as witnesses on behalf of the Applicant, and, having been previously duly sworn, were examined and testified further as follows:

CROSS-EXAMINATION (Resumed)

BY MR. FLEISCHAKER:

Q Yesterday at the end of the cross-examination we were discussing the conclusion that an instrumental peak acceleration measured in the free-field of 1.15g was conservative and is a conclusion drawn in your testimony.

MRS. BOWERS: Can you give us the page number?

mpb2

MR. FLEISCHAKER: At page 29.

BY MR. FLEISCHAKER:

Q The conclusion which is designated by number five:

"A peak ground acceleration of 1.15g at Diablo Canyon for the maximum earthquake on the Hosgri is a very conservative estimate."

Now 1.15g is used there. Is that peak ground acceleration as measured in the free field?

A (Witness Smith) Yes.

Q In your opening statement, Dr. Smith, you noted that we could approach the determination of the instrumental peak acceleration taken in the free field by either a statistical approach or a modeling approach, is that correct?

A Yes.

Q Okay.

And in our discussion yesterday we got off on USGS Circular 672, and I have a series of questions for Dr. Bolt regarding his testimony on the USGS conclusions.

Dr. Bolt, at page 5846 of the transcript of yesterday you stated that you disagreed with the procedures utilized by USGS to derive the figures in Table 2, I believe.

A (Witness Bolt) Yes.

Q What is your opinion regarding the scatter of

mpb3

peak instrumental accelerations that have been recorded for the magnitude range 5.5 to 6.5?

A There is considerable scatter.

Q And do you have an opinion as to -- well, how many measurements do we have for instrumental peak accelerations in the range of 6.5 to 8?

MR. NORTON: Excuse me.

Peak instrumental accelerations in the range of 6.5 to 8? I didn't realize that there were any peak accelerations over 6.5 or anywhere near it.

MR. FLEISCHAKER: I'll withdraw the question.

BY MR. FLEISCHAKER:

Q How many measurements do we have of peak accelerations, instrumental peak accelerations of earthquakes in the magnitude range 6.5 to 8?

A (Witness Bolt) We have very few. This particular circular tabulates them in the appendix, something of the order of, when this was done, of two, I think, and maybe three now.

Q What are those three?

A Well, I'm thinking about the eartaquakes that are fairly near to the source. One would be the 1952 Kern County earthquake which at the time the circular was written was allocated a magnitude of 7.7. But work at Cal Tech, and my own rereading of the Wood-Anderson records has now adjusted

mpb5

that magnitude, revised that magnitude really to 7.2 on the Richter Scale.

We have a record from the Tabataz earthquake in Iran that I mentioned yesterday, its MS magnitude was 7.7. And we have a record now from the Soviet Union near the town of Gazli, G-a-z-l-i, which has a surface wave magnitude of I believe about 7.2. That was not available at the time of the writing of the circular.

Q How about Pacoima?

A Well, Pacoima was available. It's 6.5 magnitude.

Q So that gives us a total of four data points for 6.5 and above.

A Yes.

Q Do you have an opinion of what we might expect to see in terms of the scatter of data for instrumental peak accelerations for the magnitudes 6.5 to 8?

A Yes. I think that we found the scatter on both sides of about acceleration .7g. There will be some found which are less than that and some greater. And some of the scatter will involve the topography on which the instrument is sitting. Some will involve the surficial conditions, soil conditions. Some will involve the mechanism of the earthquake.

So there are a number of reasons why there will be scatter about this central value.

mpb5

Q Well, I'd like to limit our questions to the instrumental peak accelerations recorded.

A I was speaking to that.

Q The figure that you gave, .7g, does that represent the instrumental peak recording?

A It would be, yes.

Yesterday I spoke about the peak acceleration becoming asymptotic to some value between .6g and .8g. So in that light I'm speculating that there will be some average level to the scatter around about .7g.

Q Let me see if I understand.

This figure, .7g that you're talking about, does this represent your estimate as to the instrumental peak acceleration -- excuse me, as to the expected mean instrumental peak acceleration as measured in the free field?

A That's correct. And I'm addressing the range of magnitudes that you specified.

Q And to be clear, it does not represent, then, your estimate as to the peak acceleration at the source.

A That's correct.

Q It does not represent.

A It does not.

Q Okay.

What is your estimate as to the range of scatter? Let's be specific now. For magnitude 7,

mpb6

magnitude 7.5, and magnitude 8, what would you expect the one sigma, the values at plus and minus one sigma to be?

A I would suppose about .15g.

Now to get a one sigma value -- you're being quite specific, of course, in terms of a statistical parameter, and that carries with it a certain probability of exceedance, and so on. To estimate a standard deviation one must have quite a number of observations so that that cannot really be done at the present time.

Q I understand that. We only have four data points. And I'm drawing on your experience to -- Do you have an estimate --

MR. NORTON: We'll object. He says it can't be done. And speculation is not what we're here for.

MRS. BOWERS: Do you want to respond to the objection, Mr. Fleischaker?

MR. FLEISCHAKER: Yes.

It seems to me it's clear from the record that Dr. Bolt has given me a mean value, and it seems to me that if one can give a mean and can speculate as to a mean that one ought to be able to speculate as to a standard deviation.

So I would like to reput the question.

MR. NORTON: Mrs. Bowers, that may be. But the witness has said it can't be done. And he said it would be speculation. There aren't enough data points.

mpb7

If Mr. Fleischaker wants to object to his own earlier questions, I guess he can do that. I didn't choose to object to them. I am objecting to this one.

MR. FLEISCHAKER: Well, I understand that it can't be done given the data base. The data base is only four points. That's our problem.

But what I'm asking this witness is given his knowledge as to accelerations and all his knowledge in seismology generally, if he has an opinion as to what he would expect to see the mean -- what values he would expect to see as the mean plus and minus one standard deviation for these magnitudes 7, 7.5, and 8.

MRS. BOWERS: Does the Staff have a position on this?

MR. TOURTELLOTTE: No.

(The Board conferring.)

DR. MARTIN: I think we need some clarification regarding the possibility of calculating the mean and the standard deviation.

By sigma, if you have a set of numbers you can calculate the arithmetic mean of that group of numbers, and with the same numbers you can calculate the standard deviation. It's simply a matter of applying some formulas.

Would you give us some clarification of the intent of your statement?

mpb8

WITNESS BOLT: The intent of my statement was that -- and I would point out that I've already answered the question, what do you think or how do you speculate the standard deviation would turn out. I said .15g. So that's already on the record, you know.

But then I went on to say that so far as getting a stable estimate of the standard deviation concerning the scatter, I would expect, and I'm sure you would, that one would need something over six data points. If you put just three data points into the formula you'll get a number, but most people would think it's not a very stable value. It is likely to change drastically even with the next point that comes along.

That was the thrust of my comment.

DR. MARTIN: All right.

So you're saying you could calculate the standard deviation, but it wouldn't be reliable.

WITNESS BOLT: That's correct.

DR. MARTIN: Thank you.

MR. FLEISCHAKER: Can I --

MRS. BOWERS: Well, we still have this objection pending.

The objection is sustained.

MR. FLEISCHAKER: I thought he gave an answer.

MrS. BOWERS: He gave his opinion and then he

mpb9

explained why he could not do it reliably.

DR. MARTIN: He couldn't rely on the results, so he couldn't do it.

MRS. BOWERS: It would have no meaning.

BY MR. FLEISCHAKER:

Q By a trend of mean from the current data set, can you draw some conclusions with respect to the trend of the mean?

A (Witness Bolt) The trend against what?

The trend means that we have to correlate the mean against something.

Q The mean of peak accelerations against magnitudes. Peak accelerations on one bar and magnitude on the other, 6.5 to 8.

A Well, my own view of that is -- this is based mainly on the theoretical model that I mentioned yesterday in the testimony -- is that there may be a slight increase in the mean peak or maximum acceleration as the magnitude increases. But it would not be nearly the straight line type of extrapolation that the people involved in the USGS circular adopted at that particular time.

Q Utilizing -- Do you have an opinion as to -- for the magnitudes 7, 7.5, and 8, do you have an opinion as to the value of trend of mean plus one standard deviation in instrumental peak accelerations as measured in the field?

mpb10

MR. NORTON: Excuse me.

Dr. Bolt, the question is Do you have an opinion. Could you answer that yes or no, please.

WITNESS BOLT: I'm not sure that I do have an opinion on that. It's a bit complicated in the way you put it. I'm not able to follow exactly.

You said the trend of the mean and the standard deviation, is that correct?

BY MR. FLEISCHAKER:

Q No. The trend of the mean plus and minus one standard deviation.

A (Witness Bolt) Well, I've already indicated my opinion as to the trend of the mean.

My opinion as to the trend of the standard deviation would be that there would be no trend of the standard deviation because that goes to the scatter about the mean. I see no reason why that scatter would be different depending on the magnitude range of the earthquakes.

Q In the discussion between Counsel, the panel, and the Board and yourself, I lost the significance of your reply that the standard deviation would be .15.

Is it your testimony that that estimate is not reliable, is that the thrust of it?

A Yes. It's a speculative value based on calculating standard deviations for the peak acceleration about the

mpbl1

mean for low magnitude earthquakes, and consequently there is an assumption that some sort of scatter will be seen when one gets up to bigger earthquakes. And you have to be aware of that assumption.

I can't prove that in any way. Nobody can test it because we don't have the data. But I offer it for what it's worth to your question.

Q That value, .15, is that to be applied at each of the magnitude ranges uniformly?

A Yes.

Q A uniform description of the scatter?

A Yes.

Q Dr. Bolt, yesterday I was asking you about the -- whether there was any statistical significance to the values that were set in Table 2 --

A Yes.

Q -- of the USGS Circular 672.

A Yes.

Q And you replied that you weren't aware that they went into any statistical discussion at page 5862.

"Certainly not," is your response.

"I'm not aware that they went into any statistical discussion, certainly not in the paper. As you know, calculations are based on statistical formulas or distributions."

mpb12

A Yes.

They do give graphs with the points available to them at that time. These show a considerable scatter. But I don't see on any of the graphs any indications of measures of dispersion. There were arrows and that sort of thing indicating uncertainties in each of the points. That is a kind of statistical, elementary statistical treatment of the raw data.

Q Okay.

Well, I'd like to draw your attention now to page 5847, and your observation that the .8g measured recently at an earthquake in Iran, the City of Tabataz, is a flat contradiction to the extrapolation carried out in the USGS circular.

Do you have that in front of you?

A Thank you, yes.

Q And asked the question: How do we know it's a flat contradiction to the extrapolation carried out?

A Well, the extrapolation as specified in Table 2 would give a value for an MS magnitude of 7.7 of somewhere between 1.15 and 1.20g. That's the value that they recommended to be used, at least for this purpose in the pipeline design, and this circular is now getting into discussions of nuclear power reactor design. And yet a 7.8 magnitude earthquake in Iran gave a peak acceleration of .8g, which is a

mpb13

considerable way from that prediction of theirs, and to me that's a contradiction. That's against their prediction.

Q But we've agreed, haven't we, that we don't know, that the 1.15 might represent a mean or a trend of mean, estimate of the trend of mean.

A I'm not sure that you and I ever agreed on that point.

Q Okay.

Can you state that the .8g doesn't fall within...

A Well, I can state that if there is a standard deviation of .15g, then the .8 would not fall not only within one standard deviation, it wouldn't fall within two standard deviations.

Q That helped me formulate my question. Thank you.

Do we know what the standard deviation -- whether or not the USGS contemplated a standard deviation for the range -- for the peak accelerations in the range of magnitude 7.5?

A I can state that I can find no referenced standard deviation in connection with that point in this paper.

Q Then how do we know that the .8 is inconsistent with the value identified in the table?

A. It's inconsistent to me, and that's all I claim, based on my own analysis of the data.

Q Okay.

mpb14

By the way, Dr. Bolt, is it your testimony that the one standard deviation for magnitude equal to six is .15g, the standard deviation for peak acceleration, the value falls within the .15g?

Was my question clear?

A No. I'm sorry, would you mind repeating that?

Q What is the mean value for peak accelerations, instrumental peak accelerations measured in the free field for magnitude six earthquakes?

MR. NORTON: Excuse me.

Is this in reference to Circular 672, Dr. Bolt's opinion, or the Hanks and Johnson opinion, or what?

MR. FLEISCHAKER: Dr. Bolt's opinion.

WITNESS BOLT: I've never calculated that. I have calculated a mean for what I call earthquakes with low intensity and earthquakes with high intensity. But I have tried always to group earthquakes in different ways, depending on their fault mechanisms and other characteristics that might go to them being strong earthquakes and generating rather high frequency waves, rather than classifying things in purely magnitude terms, which I think I indicated yesterday I find not terribly valid.

Magnitude is just one number to do with an earthquake. And I don't believe that it's the number to rely on completely when one is trying to discuss peak ground

mpb15

accelerations.

BY MR. FLEISCHAKER:

Q So you haven't done it?

A (Witness Bolt) No, I don't believe I've ever tried to do just that exercise that you've put to me.

Q Dr. Smith, have you ever done it? "It" meaning derive a mean for the instrumentally determined peak accelerations in the free field for the magnitude six -- in the range magnitude 6?

A (Witness Smith) Range magnitude six to what?

Q Magnitude six to peak accelerations.

A Well, in the tetiimony I give a mean value for earthquakes in -- above magnitude 5.5.

Q My question, however, is limited to magnitude 6.

MR. NORTON: Excuse me, Mrs. Bowers.

The testimony is clear from all of these witnesses that there isn't any difference once you get above magnitude 5.5. So it doesn't make any difference whether you're asking 6 or 6.5, as I've understood all this last half-day's testimony.

I think that's what Dr. Smith is trying to tell Mr. Fleischaker right now.

MRS. BOWERS: Dr. Smith, is that correct, that it doesn't make any difference if you get above 5.5?

WITNESS SMITH: That was my testimony, and that

mpb16

is my opinion. But I don't think that was specifically the response that was being sought here. Perhaps that concept is being challenged, I'm not sure.

BY MR. FLEISCHAKER:

Q I think there's a difference between the magnitude at the source -- excuse me, the peak acceleration at the source, which is where there may be no difference, and I believe that that's the thrust of the testimony -- and the peak accelerations that have been measured, instrumentally determined in the free field, which is what I'm asking about.

Let's clarify.

Your reference to peak accelerations, there being no difference--no difference with respect to what?

A (Witness Smith) No difference with respect to magnitude.

Q For what?

A For the peak instrumental free field ground motion measured at close-in distances near the source.

Now we're not talking about things hundreds of miles away because there there's a very pronounced effect. Larger earthquakes produce larger ground motion at large distances.

When one is dealing with distances of what we call the near-field, within five to ten kilometers, say, in

mpb17

that range, there does not appear to be any magnitude dependence on the peak instrumental free field ground motion. Now this doesn't mean ground motion measured on top of a flagpole or anyplace else, it means what we would infer to be in a flat undisturbed region without a large structure there to perturb or to disturb the ground motion in some way.

Q Then you're excluding possible topographic effects?

A Yes.

Q And are you excluding possible amplification by directivity or focusing?

A No.

Q You're not?

A No.

Heat directivity and focusing are part of the physical process of the source that give us different ground motion and at different positions, azimuths and different positions from the fault.

Q Okay.

Dr. Bolt, the range of figures that you gave me for -- let me direct your attention to the testimony. I think it's page 5877, lines -- okay. Let me first backup to 5876, at line 13.

I asked the question:

"What procedures would you use to derive

mpb18

the expected peak accelerations in the free field for magnitudes of the earthquake range 6.5 to 8?"

In answer to that question on the next page on lines 9 through 15 you stated as follows:

"So on that model I think that there will be a fairly strong limit to the peak acceleration on the average that the physics of the situation indicates that the strong shaking at the site near to a source in the frequency range of ten hertz to one hertz will not be very different on the average for magnitude 6.5 than it will be for 8.25 earthquake."

As used there, what do you mean by "strong shaking"?

A (Witness Bolt) Well, I'm speaking about accelerations of the ground above amplitudes of about .1g.

Q So as used in that sentence we can substitute "instrumental peak accelerations" for "strong shaking"?

A No.

Where strong shaking is concerned -- yes, I agree with you. I think it would be appropriate there to substitute for "strong shaking" in that particular sentence "peak acceleration".

Q Instrumental peak accelerations as determined in the free field?

mpb13

A Yes. That's correct.

Q Well, would the peak accelerations at the site near to the source in a frequency range greater than ten hertz be very different on average for a magnitude 6.5 earthquake than it would be for 8.25 earthquake?

A Now the answer to that question is going to depend to a large extent on the kind of geological structure and surficial rock and soil conditions that one is speaking about, because the higher the frequency the waves, the greater the attenuation. So that if one is talking about 20 hertz waves, 20 cycles per second, that is, if they can travel through some hard rock like granite, there is not going to be very much attenuated.

So there could be some rather high amplitudes in those high frequency waves. But if they're passing through deep alluvium then they may not be very high. They would be damped out rather quickly, even in a matter of three or four kilometers.

We have very little guidance there except laboratory work on passage of rocks through -- passage of waves of high frequencies through rock in the laboratory conditions, and also theories.

Q Well, how does the attenuation of high frequency waves through rock of different properties relate to what we would expect to see on average as the instrumentally peak

mpb20

acceleration in the magnitude range 6.5 to 8.25?

A You are now reverting to our earlier discussion this morning, to the answers I gave at that time?

Q No, I asked the question in the frequency range beyond ten hertz --

A Yes.

Q -- would we expect to see on average not very different peak accelerations for magnitude 6.5 earthquakes than for 8.25? That's the question.

A My answer to that is that I would expect to see on average somewhat higher values for peak acceleration.

Q For?

A For these waves which you say have frequencies greater than ten hertz. But it would not be a function of magnitude. It would not be a function of magnitude. I guess it's to do with the local breaking of the rock on the fault, and as I pointed out yesterday, that that local behavior doesn't depend on the fact that the fault ruptures or for another 100 kilometers or not.

It's to do with the strength of the rock locally.

Q All right.

At the bottom of page 5877, in response to the question, you gave me a range of .6 to .8g.

A Which page was that?

Q 5877 to 5878.

mpb21

A Oh, it's at the top of -78, yes.

Q Now let me be very clear:

Does that range represent your estimate of the expected instrumental peak accelerations in the free field for the magnitude range 6.5 to 8?

A Yes.

MRS. BOWERS: Mr. Fleischaker, I apologize for interrupting your train of thought.

But looking at the question you asked, it seems to me you've just asked the same thing: expected peak accelerations in the free field?

MR. FLEISCHAKER: Well, I asked what procedures he would use, and that's right. I wanted to make sure that we were meeting head-on.

BY MR. FLEISCHAKER:

Q Now does that range of .6 to .8 represent the range of expected values?

A (Witness Bolt) Yes.

Q Now, let me go back to the question I initially asked.

A I'd like to just add to that -- and I think you'll clear it up -- but I would like to say that I'm speaking here about the amaximum accelerations. That's the peak. By "peak" I mean the maximum.

Q On the time history?

mpb22

A That's correct, yes.

Q Right.

That's what I wanted to know.

A Not the mean acceleration.

Q Right.

Nor the effective acceleration?

A No, but the maximum acceleration.

Q Right.

Let me go back, then, to the question, because I think I got an answer to what you would expect to see, but let me ask you:

What procedure would you utilize, or do you utilize to derive that value?

A Well, my own procedure is to plot the observed horizontal peak accelerations, maximum peak accelerations against distance, and to then start to extrapolate in towards zero, let's say zero distance from the source of the fault.

We have quite a lot of such observations. They are at some distance away from the source. We have very few within five kilometers, as I mentioned.

One has to make a choice then as to how to make the extrapolation towards zero. I think there is some indication in the data points as we have them that the curve that one would use for the extrapolation does turn down or flatten off as one reaches the fault, and I would be guided

mpb23

by the general theoretical considerations, mechanical considerations that I've spoken about a couple of times already to deal with the limiting nature of faulting. And so I would use a curve which is a quadratic of some kind which would tend to curve down, to flatten off.

And the result, then, would give me the answer I desired.

Q So it's an extrapolation from observed data?

A Observed data, plus the theoretical considerations that I'm holding to. In the USGS circular, they followed a different theory and somewhat I think circular argument because they start with the assumption of a 1.25g for large magnitude earthquake, and then extrapolate between that -- interpolate between that and the Pacoima value. And that, it seems to me, is forcing the data to go through a point which is itself being extrapolated in some way which is not clear from the data of smaller magnitude earthquakes.

You see, to get the 1.25g, since we have no observations for a magnitude 8.25, they have had to do some extrapolation anyway somehow.

They then lay down that that will be a point which will guide their interpolation back to the observed values. So it seems to me there is a certain amount of circular reasoning there.

Q Do you argue with the use of the Pacoima Dam

mpb24

data points?

A No. I would want to certainly include that. It's been very much studied. I would want to make allowance for the topography at the site. And if I was correlating, I would prefer to try to separate out the effect of the thrusting there. That was a thrust earthquake which in my view is likely to have larger peak accelerations in the near field because thrust faulting involves compressional forces which are squeezing the sides of the rupture together. And I would suppose that there would be a reason to allow the extrapolation curve to fall somewhat below that, the actual observed value.

As a matter of fact, the USGS people also in their work reduced the 1.15g which was actually the 1.25g -- different people measure it slightly differently. down to .9g for a number of reasons which they set out here in the paper.

Q You mean when they correlated it through a magnitude 6.5, which is what San Fernando was measured later, they reduced it to .9?

A Yes.

Q So in a sense in terms of their extrapolations they took into account the anomalous topographical effects of the -- but related to the location of the accelerometer in that case?

mpb25

A I think that their reason was different. They felt that the frequency of that peak acceleration measured from Pacoima was higher than might be of importance for engineering purposes. And so they put the records through a filter, which had the effect of removing the higher frequencies somewhat.

And that reduced that spike, that high frequency spike down to that .9g. I think that goes to their feeling of the use to which this work was going to be put.

Q Well, the -- wouldn't that high frequency contribution to the measured acceleration at Pacoima be in large part due to the topography underneath the accelerometer?

A It could well be, yes.

But I don't think that was the explicit reason why they reduced it. In other words, they were not making that assumption.

Q Would you expect -- what was the topography underneath the accelerometer?

A It was rather a sharp reach. The accelerometer was at the top of the reach.

Q It was granite?

A It was granite, yes.

Q Would you expect such a topography to contribute significantly to the -- or make a significant contribution to the high frequency component of the peak acceleration?

A Yes.

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MRS. BOWERS: Mr. Fleischaker, before you leave this Geological Survey Circular 672, I'm surprised the Anchorage earthquake is not listed with these. Was it smaller than --

WITNESS BOLT: No, Mrs. Bowers. Unfortunately we had no strong motion instruments in Alaska at the time. The history of the placing of strong motion instruments in the United States doesn't bear too much of a close look. There was just no money available for a long time, despite great pressure from the engineers, including Dr. Blume sitting here in this room, to get these instruments out.

We tend to put these instruments in after the big earthquake has occurred, so we have some measurements of aftershocks from Alaska but not the main shock.

MRS. BOWERS: Well, I was there shortly after it occurred and I saw the extent of the damage, and I thought you would consider it a great earthquake.

WITNESS BOLT: It was a great earthquake indeed, but there were no strong motion records obtained for that earthquake that would go toward settling many of the arguments we have, had we had them.

BY MR. FLEISCHAKER:

Q Dr. Bolt, we used the term this morning a couple of times, "near field," and I don't think we have yet a definition of that term.

eb2

Could you please define for us the term "near field"?

A (Witness Bolt) I think for the purposes of the discussion that's been going on here, one would define the "near field" as being within a wavelength or two of the source of the wave. That is to say if we're talking about one-second waves and the velocity of those waves is a few kilometers per second, say five kilometers per second, then this would give us a distance from the source of roughly ten kilometers.

There are other definitions that theoreticians use. I think they are not too helpful in this general context. -- With my apologies to Dr. Frazier.

(Laughter.)

Q It might become useful--

Dr. Frazier, do you have another definition?

A (Witness Frazier) I wouldn't use the word as Dr. Bolt has used it, and other colleagues including theoretical colleagues of mine. I wouldn't use the word in the sense they're using it.

Many people define the word slightly differently, "near field," slightly differently than the way Dr. Bolt-- Alternate definitions of the word are any distance within the minimum fault dimension. The fault has a length and a width characteristic. It's irregular, but you take some

eb3

width dimension and that region within the width dimension of the causative rupture is often defined to be near field.

It seems to me for the purposes of the discussion here and the reference to the Hanks and Johnson paper yesterday that something of the order of ten kilometers or closer would be defined as near field. I would be a little hesitant, though, to use the word.

MR. NORTON: Excuse me, Mrs. Bowers. It seems to me that if we're going to spend a lot of time on defining near field, I don't think there is any real dispute but what Diablo Canyon would be within the near field if a rupture occurred on the Hosgri Fault immediately adjacent to Diablo Canyon. There has never been any dispute about that by any of our experts or anyone else that I'm aware of, so I don't see much sense in pursuing this an awful lot.

MRS. BOWERS: Well, the record will show the witnesses indicated by nodding their head "Yes."

MR. FLEISCHAKER:

Q Dr. Frazier, do we find that the wave propagation in the near field is different from that which we might observe in the far field?

A (Witness Frazier) Let me make a supposition on your question. The supposition is that you are talking about wave propagation, not earthquake phenomenology. And the answer to wave propagation, being a theoretical question,

eb4

is Yes.

Q What's the difference?

A Dr. Bolt's definition of the near field dealt with wavelengths, and when waves propagate in elastic media, the mathematical solutions we get for these kinds of problems have two terms in them. And one term is what we jargonize or what we call "near field," and these terms behave a little differently.

The main difference in how they behave as opposed to far field is that the near field terms tend to decrease with distance more rapidly than far field terms do.

Q What is the difference in the earthquake-- Is there a difference in the earthquake phenomenology in the near field and in the far field?

A I think "near field" with regard to earthquakes has to do with when your observer is setting -- is stationed very close to the causative rupture, and if you're talking about one hertz, one cycle per second signal, then that would be mathematically in the near field also.

If you're talking about 10 hertz, 20 hertz, that's not in the near field; it's borderline, not being in near field.

Q Dr. Smith, when I began this morning I began by identifying the two ways that we might seek to determine the peak instrumental acceleration in the free field to be

eb5

expected at the site. One way was the statistical approach; the other way was a modeling approach. You mentioned both in your opening statement.

A (Witness Smith) Yes.

Q What is the modeling approach?

A By "modeling approach I meant one would attempt to take into account as much information about the specific source at hand as possible. For example, if one were considering a site that was close to a thrust fault, perhaps on the upward block of a thrust fault, then one would look for generally higher accelerations than on other kinds of faults, or if one were designing a structure for the top of a mountain near such a feature, then one would take that into account.

You try to take as much of the real physics into account as you can.

I think almost everyone in the field would agree that great progress has been made in this in recent years, but also there remain residual uncertainties and so it is also appropriate to take the view that perhaps some of the physics is unknown to us and therefore we should look at the statistics of the data.

And when we do that we obtain a great deal of confidence by the general consistency of the two approaches.

As was pointed out earlier this morning, there

eb6

are not many recordings of large earthquakes close in, and so it is for this reason that we have to take as wide a possible view as possible in interpreting this data to account for all possibilities.

Q Let me see if I can identify some of these factors. When you mention-- If we are going to model we would want to look at the type of faulting, I suppose, that the physical thing you're looking at there is the range of expected stress drop?

A That's one of the parameters, yes.

To illustrate how this might work, let's take the Pacoima Dam record. If we're going to use the statistical approach then we can't put any of our knowledge of physics into the problem. We simply say that, well, at Pacoima there is an effect we can see, the instrument is on top of the ridge. In other earthquakes there may be effects we can't see and therefore, to be strictly legitimate in a statistical approach, we put in 1.25 or 1.2g for the Pacoima record in the statistical hopper. Okay?

In the modeling kind of approach we would look at Pacoima and say what would be the ground motion if this had been a flat, undisturbed region near this particular fault? And many people worked on that problem and I think the consensus in the technical field is very clear that the motion would have been about perhaps .8g above the

eb7

San FErnando earthquake which is the source of the Pacoima record.

Q Isn't it also the case, though, that with respect to Pacoima, the highest instrumental peak acceleration is 1.15? Correct?

A About 1.2.

Q Isn't it also correct that the modeling that's been done would indicate that there was actually a higher peak acceleration south of the instrument because of directivity?

A I don't believe so.

Q Yesterday we discussed the work of a man named Heaton. Are you familiar with his work?

A I'm generally familiar with it, yes, the modeling. Dr. Frazier points out that Heaton was modeling primarily velocities, I believe, rather than accelerations.

This gets back to that really subtle point that I'm not sure you understood yesterday, that the peak acceleration and the peak velocity on the Pacoima record occurred at different times.

Q I understood that.

A Okay, fine.

Q Dr. Frazier, are you familiar with the Heaton work?

A (Witness Frazier) Yes, as I testified yesterday.

eb8

Q Do you recall whether he reached a conclusion that the peak accelerations associated with the rupturing along the fault at San Fernando could have resulted in a higher acceleration south of the Pacoima Dam accelerometer due to focusing effects?

MR. NORTON: Excuse me. Did that question say "could have" or "did have"?

MR. FLEISCHAKER: Could have.

WITNESS FRAZIER: The material we're talking about is a graduate student at Cal Tech's Ph. D. thesis which was turned in about two months ago, and in his thesis he did not model accelerations. His model isnot suited for -- or his procedure was not geared, was not intended for explaining accelerations.

He may have extrapolated such a conclusion. I'm not familiar with him making that conclusion.

1b

BY MR. FLEISCHAKER:

Q Dr. Smith, let me get back and see if I can identify the kinds of physical phenomena that we would seek to model in trying to determine the instrumental peak accelerations.

One that you've identified is the range of expected stress drops.

A (Witness Smith) Yes.

Q A second would be the effect of topography?

eb9

A In a general way, yes. I think there are instances where topography might be extremely important.

Q Would that relate primarily to attenuation and scattering effects?

A Well, it could be-- It's a very complex process but you could I think compare it with the amplification effects of a structure. Certainly the motion recorded at the top of a tall building will be very much different than at the base. And some of those same general effects would be operative in a region of severe topography.

Q So that topography we would separate out from attenuation?

A Yes, in a general way.

Q What other phenomena might we seek to model?

A Oh, let's see. We have stress drop, topography, --

Q Attenuation.

A The type of faulting I think would be very important.

Q Doesn't that relate to stress drop?

A Not entirely. We are not-- Let's see, the term modeling as used in this context is perhaps a little misleading. It would be more characterizing than -- because we are not proposing to make a specific, analytic model because we don't know how to include all these things.

eb10

But in a general kind of way, if you're going to design something for the top of a ridge like Pacoima Dam you would want to take the topography into account in some way, you'd want to take into account the style of faulting, whether it's a thrust fault or a strike-slip fault.

Q What would that relate to?

A THe level of motion. I think the evidence is clear that motion associated -- close in and associated with thrust faults is larger than with other types of faults. That's partially a result of the stress drop and there may be other, more complex features as well.

Q Would that go into our model in some sort of estimation of stress drop?

A That would only be part of the picture.

Q So sense of motion is one of the things.

How about the location of a fault with respect to the site?

A Yes. I think the geometrical relationship of the fault to the site would be important as well.

Q How would we estimate how the fault is going to break?

A I don't think we would attempt to do that. I'm not proposing a specific detailed model whereby one does that type of calculation. You would have to assume all possible types of rupture might be possible.

eb11

Q Well, is that an important parameter in trying to determine whether there is going to be focusing?

A Yes.

Q Has this modeling been done in the case of Diablo?

A No.

MR. FLEISCHAKER: Could we have our morning break?

MRS. BOWERS: Yes.

Let's take a ten-minute break.

(REcess.)

b 3

MRS. BOWERS: May we proceed?

MR. FLEISCHAKER: Yes.

BY MR. FLEISCHAKER:

Q Dr. Smith, I wanted to see if I could-- I'd like to ask you some questions about your testimony yesterday relating to Hanks and Johnson, which is Joint Intervenors' Exhibit 47.

A (Witness Smith) Yes.

Q At page 5896 I'm asking the question which I really never got out. The question I was going to ask related to whether Hanks and Johnson in their article concluded that we can expect to see higher -- we can expect to see more often the maximum peak acceleration with a magnitude 8 event than with a magnitude 5.5 event.

eb12

A Yes.

Q Let me ask you, do you have an opinion on that?

A Yes, I agree with that.

Q With what? I'm not sure.

A That you would-- It's more probable that you would have a recording of a high peak acceleration for a large earthquake than for a small one.

Q Is that because a large earthquake can be seen -- can be modeled as a number of discrete events?

A In part, yes.

Q And each discrete event could have the maximum credible peak acceleration. And so the larger the event the more chances you have to reach the maximum --

MR. NORTON: Again I'm going to object because that question is misleading and vague. Let me explain my objection.

If you're talking near field discrete events, then I think by definition that's obvious and that's where my confusion is. In other words, when you have a description of near field it's a distance-- As I understand it from all the testimony, near field can't be 400 kilometers away or it is no longer near field. And that's why your question I think is misleading.

MRS. BOWERS: Do you want to respond to the objection?

MR. FLEISCHAKER: I'll withdraw the question and note that I think the objection mischaracterizes the testimony in describing near field as a distance.

WITNESS SMITH: If it isn't distance what is it?

MR. FLEISCHAKER: Let me move on.

BY MR. FLEISCHAKER:

Q What is your reason for concluding that there is a higher probability that we will see the maximum peak as we move from magnitude 6.5 to magnitude 8?

MR. TOURTELLOTTE: That question has already been asked and answered.

MRS. BOWERS: This morning, Mr. Tourtellotte?

MR. TOURTELLOTTE: Yes.

MRS. BOWERS: So we don't have the transcript to check.

MR. TOURTELLOTTE: I guess I'd have to-- I think that was asked and answered to Dr. Bolt. Maybe this witness has the right to answer, though.

MR. FLEISCHAKER: My reply is that this question has not been answered, has not been asked today to Dr. Smith.

MRS. BOWERS: Do you recall the question, Dr. Smith?

WITNESS SMITH: Yes, I do.

As I indicated yesterday, it is partially a sampling problem. The ground motion effects of earthquakes

eb14

of any size is a highly variable function, changing greatly from one position to the next around the earthquake source. The size of the region that is subjected to high accelerations is simply larger for a great earthquake than it is for a small one.

If you had a 300-kilometer rupture along the San Andreas Fault, you would subject a great deal larger section of California to large accelerations than, for example, a small earthquake of magnitude 4-1/2 or 5. Therefore, given the distribution of structures and strong motion instruments and so forth, you would simply expect to find a larger number of data points in your sample representing large peak accelerations.

BY MR. FLEISCHAKER:

Q By "sampling problem" do you mean that it relates the probability of our seeing these high accelerations relates to the placement of accelerometers?

A (Witness Smith) In part, yes.

Q Does it relate also to the physical processes that are happening along the fault as it ruptures?

A Yes.

Q What are those physical processes?

A In my opinion focusing is one of them. Local inhomogeneities or barriers on the fault zone, the roughness of the fault zone would be another parameter that would

ek15

affect this. There are a large number of parameters that control the complex ground motion associated with an earthquake.

Q So there is a larger probability of getting the maximum acceleration from a large earthquake than a smaller earthquake, notwithstanding our ability to record it -- I mean not withstanding the existence of a machine?

A Yes, I think that's true also.

Q Dr. Smith, do you have an opinion as to the upper bound for the maximum instrumentally-determined peak acceleration in the range of magnitude 6.5 to 8?

A Not really.

Q A unique expert.

Do you have an opinion as to the expected peak acceleration, instrumental peak acceleration determined in the free field in the magnitude range 6.5 to 8?

A Yes. I think it's about .5, the expected value.

Q Do you have an opinion of the expected value for the maximum peak acceleration for magnitude 7.5 earthquakes?

A The expected value of the peak motion from magnitude 7.5 earthquake. So the assumption is that there is a class of earthquakes all of magnitude 7.5, and one wonders what is the expected value of the accelerations?

Q Right, an instrumentally determined peak in the

eb16

near field.

A In distance?

Q Yes.

A Yes. I believe that value is what I just quoted about .5.

I generally agree with Hanks that the processes which control the peak motion in the near field, within 10 kilometers of a fault, are essentially independent of magnitude. There may be some slight statistical variations in this, but considering the data that we now have, I would have to say that there is essentially no change in the expected values of peak motion as a function of magnitude.

Q How do you arrive at that conclusion?

A The conclusion that there is no magnitude dependence on peak motion?

Q No, the conclusion that the expected peak acceleration for a magnitude 7.5 earthquake is .5g in the near field?

A I took all of Hanks' and Johnson's data above magnitude 5.5 and I added to it all the most recent data, including earthquakes as recent as several months ago, and find that without any corrections for local effects such as the amplification that produced the Pacoima record, that any modification of the data whatsoever, the mean value I believe that I quoted in my testimony is somewhat less than .5.

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Q How many earthquakes did you have, how many data points did you have for earthquakes magnitude 6.5 -- Strike that question.

Could you give me the number of data points that you had for each of these magnitudes, 5.5, 6, 6.5, 7, 7.5 and 8?

A I can't give you that information right here. The bulk of the data is repored in -- will appear in Table One of the Hanks and Johnson paper that you mentioned and also some additional data -- a data point from a table prepared by Ambraseys which is listed among your references.

It would seem to me they're of the order of -- my recollection is it's of the order of 20 to 25 data points.

You see, each earthquake may produce several data points because several records may be read from -- several components of motion will be measured in any one site.

Q How many data points are there in addition to the ones listed in Table One that went into your computation?

A I would guess there might be an additional six or eight or thereabouts. That would include the Gazli records and the Naghan, Iran records and Tabaz records. I believe those are the principal important earthquakes that have occurred since the time that Hanks' and Johnson's table appeared.

Q Tabaz, Gazli and what was the other?

agb2

While he's looking for that, I would like to mark as Joint Intervenors' Exhibit 48, Table One from Hanks' and Johnson's article and offer that into evidence.

MRS. BOWERS: Well now we have the Hanks' and Johnson article, the entire article marked as Joint Intervenors' 47.

MR. FLEISCHAKER: Well I was being strict but we can introduce the whole thing. I was only going to introduce this table because this table was utilized by this witness in his testimony, in preparing his testimony.

DR. MARTIN: He said only in part, however.

MR. FLEISCHAKER: Only part of Table One?

DR. MARTIN: No, he said he had additional data.

MR. FLEISCHAKER: Right, and I was going to ask if he could provide for the record the additional data which we could have marked as an exhibit and introduce.

MRS. BOWERS: Well I just wanted to remind you that the whole article had been identified.

MR. NORTON: We have no objection to the whole article being introduced into evidence.

MR. FLEISCHAKER: Okay then I move Joint Intervenors' Exhibit Number 47 into evidence at this time.

MRS. BOWERS: Joint Intervenors have moved that their Exhibit Number 47 be accepted in evidence and Applicant has said they have no objections.

agb3

Does the Staff have any objection?

MR. TOURTELLOTTE: No.

MRS. BOWERS: Well Joint Intervenors Exhibit Number 47 is admitted in evidence.

(Whereupon, the document previously marked as Joint Intervenors' Exhibit 47 was received in evidence.)

MR. FLEISCHAKER: Now, let me ask counsel, can we obtain a copy of the additional data points that have been used by Dr. Smith and have those introduced into evidence?

MR. NORTON: I don't know if they exist in a form that is in the form of an exhibit. They are calculations and data and I don't know that the calculations are in the form of an exhibit. And I'll check with him during the break to see what form they're in before I answer the question.

MR. FLEISCHAKER: I'm not looking for calculations. Rather what I'm looking for is the data points which are earthquakes, designation of the time, place, magnitude.

MR. NORTON: Well I think data points are very difficult to put into evidence but I think you could get testimony as to what those data points are.

BY MR. FLEISCHAKER:

agb4

Q Dr. Smith, do you have before you a document that will permit you to identify those earthquakes that were utilized in your calculations, other than those that appear in Table One of the Hanks and Johnson article?

A (Witness Smith) Yes. I believe that I have all of them, it's possible I might not have a reference to one or more of them.

Q Could you read those into the record?

A An additional earthquake on April 6, 1977 in Iran; the three data points from the Gazli earthquake in the Soviet Union.

Q I need a magnitude, I think, for each of these data points.

MR. NORTON: Mrs. Bowers, the magnitude for those earthquakes has been stated at least five times by these witnesses in the past two days. The record is full of them.

MR. FLEISCHAKER: Well I think it's appropriate that since I asked this witness the question of how did he arrive at his estimate as to the maximum peak acceleration, and he indicated that he took a number of earthquakes and identified this table and sone other table that I should be able to obtain at this time, a listing of those earthquakes and the magnitude of those events.

DR. MARTIN: Do you have such a table?

WITNESS SMITH: I'm afraid I'm not going to be able to provide a detailed listing of this. But my reading of the direct testimony of the Intervenors' is that they're using the same kind of references I am and I don't see that it's necessary to try to reconstruct data points in this fashion. There's no question as to what earthquakes have occurred where, nobody disagrees about the magnitudes or the accelerations or any of these issues.

MRS. BOWERS: Do you happen to have the references to when this information has appeared before in the record?

MR. NORTON: No, we can do it, it's just a time thing. I can remember Dr. Bolt rattling off those earthquakes and their magnitudes. I guess he could do it again. You know, I can give you the page references in the transcript I just know that they're there.

DR. MARTIN: Excuse me, I thought the question was directed to the mean and standard deviation given in the testimony. And what was being sought was the actual data set used in calculating that.

WITNESS SMITH: Perhaps if I had a moment, I might be able to dig that out from my notes here.

MR. FLEISCHAKER: I don't care how we do it, I just want to do it in a way that's reliable and Dr. Smith feels comfortable with, it doesn't matter.

(Pause.)

agb5

WITNESS SMITH: I guess it's simpler than I thought. I have an annotation of the table here.

It consists of data on Table One of Hanks plus the magnitude 5.5 earthquake in Naghan, Iran, which was M_L, I believe, 5.5, and the Karakyr, Soviet Union record from the Gazli earthquake of May 17, 1976 with a magnitude of -- M_S of 7.2.

And in this particular instance, only those data points from Hanks' table which corresponds to earthquakes greater than or equal to magnitude 5.5 were used. That's the basis of the statement in the direct testimony as to the mean and the standard deviation of the near-field strong motion data set.

Since that time, there is circulating informally in the scientific community some fairly reliable information about the most recent earthquake in Iran which produced accelerations of 0.7 and 0.8g. And if I were to repeat this table today, I would include those. I'm referring to the Tabaz record which is not yet published, but is circulating informally which was the magnitude 7.7 earthquake on November 8, 1978. And if an earthquake were to occur this afternoon, I would also include that in the data set.

Excuse me, there's an error in that date that I just quoted. We could refer for the Tabaz earthquake -- September 16, 1978.

agb7

Q Now let's see, turning to Page 28 of your testimony, at Line Five you say:

"The average of all peak accelerations from earthquakes above magnitude 5.5 recorded in the near field is now 0.49g with a standard deviation of 0.40g."

That statement is the one that you've just given us here?

A Yes, I see those data points right here in front of me.

Q Okay. Now how did you determine that average?

A I just added them up and divided by the number.

Q So it's an arithmatic mean?

A That's an arithmatic average, yes.

Q And the standard deviation, how do you determine that?

A The standard statistical method.

Q Okay. So that would be 0.49 -- the 0.4g is the sigma?

A Yes.

Q Okay.

Dr. Bolt, I understand that you have recently published an article on accelerations in the American Geophysical Bulletin called EOS.

A (Witness Bolt) That was a general article on

agb8

earthquake hazards in EOS.

Q Do you have a copy of that with you?

A No, I'm afraid I don't. I could send you a copy.

(Laughter.)

Q I hate to ask questions about it not having seen it but only having heard about it.

MR. FLEISCHAKER: I think that's all the questions I have. Thank you.

MRS. BOWERS: Do you want a short break before you proceed, Mr. Tourtellotte?

MR. TOURTELLOTTE: No.

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BY MR. TOURTELLOTTE:

Q Dr. Bolt, we've had a great many opinions expressed in your testimony, and both written and orally in the past few days about seismology, and I think it helps us to understand seismology a little better if we can describe the nature of it in terms of its exactitude. That is, is indeed seismology an exact science, or is it a science that relies heavily upon interpretation of data?

A (Witness Bolt) Some aspects of seismology are very exact. Seismology is used in the exploration business to find oil, for example, and I'm told that it's the best technique that people have, to use the seismic waves and the interpretations of them.

Other aspects of it, because one is not at liberty to make the recordings one's self, that is to say, one depends on the natural occurrence of earthquakes of various mechanisms and sizes, has to be by its nature interpretive, but I wouldn't say that the science could be characterized in its entirety as being interpretive or not exact.

Q Would you say it's a combination of art and science, the art being the interpretation?

4

A I would say a combination of observational work and theory. I don't much like the notion of art and science, the dichotomy which has been used.

eb2

Q Observation and theory, scientific theory?

A Yes.

Q Can you give us a brief statement about the history of seismology as we know it today? When did it start? How is it developed?

A People have long been interested in earthquakes and in the last century, when the development of the theory of waves was being worked out, people started to realize that the earthquake's shaking could be interpreted in terms of various kinds of wave motion, P-waves, S-waves, and so on.

In the last century people, engineers and physicists, would go out and characterize damage and draw isoseismals and attempt to say something about focal depth and so on. However, it wasn't until the beginning of this century that seismographs came into common use, were developed. And as in all sciences, when the instrumental side advances, so does the theoretical, so that at last one could have records of seismic shaking rather than people's account of them.

And so rapidly from about 1910 through to the Second World War, there was a great increase in knowledge of wave forms, what occurred on seismograms at different distances, and how to locate earthquakes, and the development of the fault plane mechanism systems that I mentioned earlier in testimony.

eb3

After the Second World War with the coming of the high-speed computers, theoretical modeling, more complicated than realistic earth models, advanced rapidly. And during the same time there were a greater number of strong motion-instruments placed out inthe field. And so in the last decade or two we've started to get good observations of earthquake shaking near to earthquake sources.

And so in the last ten years there has been considerable advance in seismology of a much more physical and quantitative kind to do with the types of waves and their interactions near the source.

So that's a very brief account of the development.

Q So the kind of analyses that we are talking about today really have been developed in the past 20 years?

A That's correct.

Q As I sat here the past couple of days, one of the things that seemed to strike me, and I'm not sure whether it's an accurate representation or not, but I'd like for you to comment, it seems to me that seismology reaches -- or rather, seismologists reach conclusions and develop confidence levels on multiple rather than singular analysis.

Is that a fair representation of methods of arriving at conclusions?

If the question isn't clear, let me know.

eb4

A I would like you to say a little more about how this struck you so that I can go to the point.

Q Well, specifically what I'm talking about is a singular analysis would be like a two plus two analysis equals something whereas multiple analysis would be that you're not quite certain of how to go about a singular analysis so you take a number of apprcaches and develop a confidence level by reason of a sort of a commonality of answer.

Would the practice of seismology be more related to that sort of multiple approach rather than the singular approach?

A That's absolutely correct, and that leads to its great interest to many people. It's not quite the same as some parts of biology or some parts of physics where one can do a good deal of the observational work under strict control in the laboratories. But that doesn't mean of course that by extending the methodology and by comparing the lines of evidence, one can't reach inferences which turn out to be borne out very closely by future tests.

Q I would like to ask Dr. Smith a question about a line of questions that was directed toward him yesterday.

When we were talking about your 1975 methodology and how it had been developed and how you had not used it in this particular instance, is my understanding correct that in the case of making the present analysis, you used a sort

of multiple approach and discarded your 1975 method as one of the approaches that you might have used because that method was too conservative in your view?

A (Witness Smith) That's true, but that's not the entire picture because the context of the 1975 and 1976 papers was to attempt to characterize the capability of different classes of faults without having any specific seismic history of those faults. So that was the framework within which that was done.

And you must recognize that it has been a struggle over recent years for the seismologist to try to convince the geologists that there are other things of more importance than the mapped fault length. And I think the principal conclusions of my 1976 paper were that the slip is of overriding importance, and I would stand by that conclusion.

And I would also agree with your characterization of the way I used it.

Q Dr. Bolt, as we have entered these discussions the past couple of days, I noticed there has been some mention of geology and there has been some mention of engineering.

Is there an interface between seismology and geology?

A (Witness Bolt) There's an overlap.

Q An overlap. You don't choose to use the word "interface"? That's quite all right.

eb6

A I wouldn't use "interface" if that meant in some people's mind a barrier. I don't think there is an interface in that sense.

There's an overlap in interests. The seismologist must be aware of, in really great detail, the mechanism of faulting. The geologists' work in measurements of faults and no great detail about faults. So there's considerable overlap.

4

Q So will you agree then that while geologists have certainly a part of their practice which is pure geology, and seismologists have a part of their practice which is pure seismology, that there is an area where there are mixed questions of geology and seismology?

A I couldn't agree more.

Q I ask you the same question with regard to engineering, structural engineering in particular. Is there an overlap there and if so, what is it?

A There is certainly an overlap. Engineers have been interested in certain questions to do with ground motions and because seismologists have not sometimes provided the necessary answers, some engineers have gone into that field. I would say to some extent they start working as seismologists.

Seismologists generally don't get involved in pure engineering concerns such as the response of structures,

eb7

but there is a commonality which is to do with mechanics, with the study of Newtonian mechanics and all its cimplications: accelerations, velocities, displacements, forces, stresses, strains. These are subjects which are part and parcel of the training of seismologists and part and parcel of the training of most engineers.

So I think there is a very strong overlap.

Q In trying to develop a logical story here, we start out with geology, and then the seismology story, on to the structural engineering. You would say that the seismologists are caught in between?

A I would say they're the keystone.

(Laughter.)

Q I invite your attention now to the issue on tsunami. I believe that you stated that for a tsunami, the movement is mostly dip-slip.

A Correct.

Q And that that kind of movement usually has to be fairly substantial in order to generate tsunami.

A Yes. Studies have been published, mainly by Japanese seismologists, correlating the amount of runup in tsunamis or the amplitude of the actual waterwave as recorded on a tide gauge with the magnitude of the earthquake that caused it and the inferred displacement.

Tsunami heights can range from a few inches when

eb3

they are discernable to many feet, of course, and there is a correlation, a strong correlation found between the actual vertical displacement of the ocean floor and the height of the seawave when it crashes on the coast.

Q Have the type of movement necessary to create the tsunami for the 1927 event been seen on the Hosgri?

A Not in my judgment, unless the kind of displacement that was given in evidence by the geologists took place all at once instantaneously rather than spread out over a number of events over 17,000 years.

Q Is that likely?

A To my mind it's quite unlikely.

Q Do you recall what the size of the tsunami was for the 1927 event?

A Well, I've read the description that Byerly put together in his paper, and I would suppose it was a small to moderate tsunami.

1e

1E agbl

Q Has that type of movement occurred on the Lompoc in the degree necessary to generate a tsunami?

A I would have to defer to, perhaps, Dr. Smith on the Lompoc fault, I'm not a specialist on that, or to Doug Hamilton.

Q Very well.

A (Witness Hamilton) Responding first on the question on the Lompoc structure, we do see a substantial vertical kind of deformation of the seafloor associated with that structure. And part of that, as I believe Mr. Willingham testified, seems to be related to some kind of very recent folding, where we can't really clearly distinguish folding from faulting. Some of it is clearly associated with faulting that has offset the sea floor.

I would like also to go back to something Dr. Bolt just said in which he remarked on the -- on the amount of offset reported on the Hosgri Fault during fairly recent geologic time, and point out that in the area where the tsunami was reported, chiefly in the vicinity of Point Arguello and points near there, that we don't see any seafloor displacement on the Hosgri Fault.

The displacement there is a buried displacement that lies beneath a smooth seafloor and the youngest post-Wisconsinian section.

Q Is the inference to be drawn from that, then,

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even if the movement had occurred all at once, it isn't in the proper position to generate the tsunami for the 1927 event?

A Yes, the movement has to be very much pre-1927.

Q Well I'm not asking you about when the movement was, I'm saying that what I'm inferring from your earlier statement about the relative location of the offset and the relative location of the tsunami were in different places. So that obviously the dip-slip that had occurred wherever it occurred was not in the proper geographical location to generate that tsunami.

A Yes, that's true.

When we speak of possible offset of the seafloor which could have created that tsunami, the only place that we identify such offset is up at the south end of Estero Bay, a long distance away from where the tsunami was observed.

Q Is this additional evidence, then, that the '27 event is more likely to have occurred on the Lompoc rather than on the Hosgri Fault?

A In my opinion, that is certainly the case.

Q Dr. Bolt, I want to talk briefly about seismic moment.

Can you define the term, "seismic moment" for me? Particularly I'm interested in a description of the word,

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"moment" as a term in physics.

A (Witness Bolt) Well the term does come from physics. If we're talking about the movement of bodies in straight lines, it's only necessary for us to discuss forces.

But if we want to describe the rotation of bodies, then we have to think about lever arms, forces being applied over a certain lever arm as on a balance, and this is where physicists have introduced the idea of moment.

If a body such as a book on the table is pushed by one's finger, you apply force to one edge of the book, it will not only start to slide, but it will start to rotate. If we push with the same force at some other point on the book, then it will move and rotate slightly differently.

So this goes to the notion that it matters where the force is being applied. The moment is defined as the product of the force and the distance between the point of application of the force and the center of rotation of the system in mechanics.

So far as faults are concerned, we carry that over and think of the forces which are built up by slowly straining the rocks as being distributed over the surface of the fault. And all these forces are applying as tractions over the fault surface.

agb4

And it matters just how they're distributed. That is to say, it matters just what the distance between this force or traction and the center of the area that's going to slip is.

So the moment of an earthquake source or the moment of a fault is, as in mechanics, the product of the forces or tractions and the areal distribution of these forces or traction.

Q How is seismic moment used to explain earthquake size?

A It turns out that, if one has a great earthquake, like 1906 on the San Andreas Fault, that meant the slip of over 400 kilometers of the San Andreas that, when one multiplies all the forces by their areal distributions, one gets a very large moment.

The moment is not likely to catch on as readily as the Richter magnitude in the newspapers, because moments are given in figures like 10^{26} dynes/cm. and I don't see the newspapers saying that this earthquake was 10^{23} dynes/cm. moment. But that's the kind of units that you get.

If you have a small earthquake in which the rupture takes place over just a few kilometers, then the moment turns out to be very much less, maybe 10^{16}, something like that, 10 raised to the 16th power in these units, dynes/cm.

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So so far as the seismologist is concerned, it is a mechanical measure of something which physicists know about, which magnitude really isn't, magnitude doesn't have any units, it's a kind of an arbitrary scale.

Q Is seismic moment the most reliable way of measuring earthquake size?

A I think that in principle it is. In practice, it's rather difficult to calculate moment in many cases. So there is still work to be done on that. I'm not quite as optimistic about it as some people.

Q From your description of moment and your description of how it's used to determine earthquake size, I take it then that we're looking for a measurement to calculate moment, a measurement of force which is actually down within the earth's crust somewhere?

A That's correct.

Q And isn't that pretty difficult to measure?

A That's difficult to measure. Of course, the moment is the product of these forces with their distribution and consequently the force itself doesn't arise specifically it avoids that issue.

I would like to comment on your use of the word, "size." This is a key point in so many of the misunderstandings that go on.

Even though an earthquake, like the 1906

agb6

San Francisco earthquake is a great earthquake in the sense that a great area of Northern California was affected by it along this rupture of over 400 kilometers and the moment is very high, it doesn't mean that we should carry that over to the actual size of the ground shaking in the near field near to the fault that ruptured. So that we have to be careful in scaling the moment to what happens to Farmer James' barn, which was only a kilometer away from the San Andreas Fault and which actually didn't fall down at all, it wasn't damaged at all in the 1906 earthquake.

Many structures built along the fault in 1906 were not much damaged, not even glass broken in the windows of the ranch houses. So that, to the people who lived there, the idea that it had this great moment and great size was not of any consequence so far as the local effect was concerned.

I just wanted to make that distinction.

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Q In seismic moment measured at the time of the event or is it measured by aftershock?

A It's measured from the event itself, using either geologic or field measurements of the amount of displacement that took place in the earthquake, or it can be measured through a series of mathematical steps by the analysis of seismograms of the waves that came out from the fault, usually at distant stations where the instruments were not overdriven by the shaking.

The aftershocks only enter the picture insofar as they give an indication of the area of the dislocation which is sometimes not observable on the surface.

For example, in the 1964 Alaska earthquake where you had an underthrusting of the sea floor underneath the continent along the trench, a lot of the fault was of course unobservable, it was under the ocean, and so to get an idea of how much of the fault was involved, -- it's a reasonable argument -- they looked to see where all the aftershocks were, of which there were many, many hundreds, to see these aftershocks extended over a distance of hundreds of kilometers, and therefore, the extent of faulting was hundreds of kilometers.

That's where aftershocks enter the picture.

Q And how long have we been capable of measuring seismic moment?

A Well, I think we've been capable of doing it ever since geologists went out and looked at -- took measurements of fault slip, fault rupture, fault rupture length and so on, and we've been capable instrumentally of doing it since the first instruments went into operation in the beginning of the century.

However, the concept didn't arise until the last couple of decades and so it just wasn't done.

People have gone back to some of the historical earthquakes and calculated moments for them.

Q I guess my question was not well stated. I wanted to know how long have we been measuring seismic moment?

A Oh. I'm not sure when the first paper on the subject was written, but I would suppose 15 years.

Q So it's a fairly recent development in seismology?

A Yes.

Q Dr. Smith, yesterday you talked about certain assumptions in your 1975 paper, and stated that they were conservative. But you didn't tell us why they were conservative, at least to the best of my recollection.

Can you specifically enumerate why you believe the assumptions are conservative?

A (Witness Smith) Well, one of the assumptions mentioned was attributing all observed geologic slip to

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earthquakes. And as I think I explained then, it may well be that a significant fraction of what the geological record shows as slip on a fault may occur as creep or other slow processes which we would not characterize as an earthquake.

The other cricial assumption had to do with --

Q Excuse me. Could you go one step further and tell why that is conservative?

A Yes.

That's conservative because if part of the slip is attributed to creep, then one has a smaller residual amount that has to be explained by earthquakes, and this would therefore call for a lower rate of activity or smaller earthquakes.

Q Now number two.

A Number two had to do with the assumption that the distribution of earthquakes would go up to the maximum and that that maximum earthquake would have a rate of occurrence of just once per 20,000 years.

In actual fact I believe that fault zones are characterized by a maximum magnitude and that during a time interval of 20,000 years I believe that that maximum magnitude would be achieved several or many times rather than just once. And this would have the effect, in the calculations I was describing, of greatly reducing the estimate of the maximum magnitude.

eb4

The more the number of times that the maximum is achieved in the fault zone to produce a given amount of slip, the smaller that maximum earthquake need be.

Q I invite your attention to page 19 of your testimony, line 16 through 19, where it states:

"A rough calculation shows that one magnitude 6.5 earthquake every 500 years along a 200 kilometer fault will lead to a net slip of about 1.5 meters over the past 17,000 years."

How does that --

MRS. BOWERS: Mr. Tourtellotte, you said 500 years. Isn't it 700 years?

MR. TOURTELLOTTE: I'm sorry, I thought it said 500. Yes, it is 700.

BY MR. TOURTELLOTTE:

Q How does this statement relate to the conservatism that you just explained?

A (Witness Smith) Well, they are different types of calculations. It is not directly relatable.

Put another way, this particular earthquake would have occurred 24 times during the 17,000 year period.

Q Let me ask you this:

Doesn't this demonstrate what you were telling me a few moments ago, that in fact over the period 17,000 years that a maximum earthquake will occur several times

eb5

in order to achieve a given distance of slip?

A Well, I don't really think I am able to demonstrate that with the data at hand. I can only demonstrate that this record of seismicity is consistent with the geologic slip, the main conclusion of that being that the seismic history of the last 50 years out here does seem representative of what's been going on for the last 20,000 years.

So I cannot firmly conclude from this line of evidence that maximum earthquakes must occur many times but it does seem consistent with this data.

Q And in fact that's what you were telling me about the conservatism of using one earthquake, one maximum magnitude earthquake over a long period of time --

A Yes.

Q -- as opposed to using a reduced maximum several times over the same period of time?

A Yes.

Q Okay.

Your third assumption?

A The relationship between moment and magnitude?

Q Yes.

A I can only say-- This was in the context of changes that have occurred since 1975 or 1976, is that there's a greater understanding of the theoretical framework of seismic moment, at least in my mind. And so I might use

slightly different constants in the equation, and I would be dealing strictly with M_S surface wave magnitude now rather than mixing magnitudes.

This means, for example, that I would use the most recently calculated magnitude of the Kern County earthquake which is now -- excuse me. I'm mixing magnitudes.

I would say there would be an attempt to estimate some surface wave magnitudes for earthquakes which, in my data base, were only given as local magnitudes. I can't point to a direct measurable amount of conservatism that might arise from this recalculation.

Q So what you're saying then is basically the conservatism of your assumptions rest primarily on the two assumptions which you mentioned first today?

A Yes.

Q On page 12 of your testimony and also in your oral testimony yesterday you stated that local magnitude is saturated. What do you mean by the term "saturated"?

A Well, this relates back to the description of earthquake size, and I'm glad to have an opportunity to add some things to Dr. Bolt's description of earthquake size.

Magnitude of any type is just one measure of an earthquake. It's like saying you could describe a house by what color it was. There are clearly other things that you need to describe about the house.

eb7

Seismic moment is a measure of the long period energy of an earthquake. Except for really large structures, bridges and other features that have very long periods, it's of no real engineering importance.

We come down the scale of frequency basically from seismic moment to surface wave magnitude to body wave magnitude to local magnitude. And each of these measures is done in a different frequency band.

Now as I stated in the testimony, I believe that the local magnitude, M_L, which is measured generally above several cycles per second, is the most appropriate characterization of the size of an earthquake for engineering purposes.

When I spoke of saturation what I meant was that if you choose to use M_L to characterize the size of earthquakes, of a variety of types of earthquakes, you will find soon that you're not getting any values much above M_L equal to 7. The reason for this, as the earthquake size or total energy release increases, the high frequency motion in the distance range 20 to 400 kilometers where local magnitude is measured doesn't really appear to change much.

This is the same effect that Dr. Bolt alluded to when he said his house didn't care whether the San Andreas Fault ruptured a hundred miles further north or not, it was only the part of the rupture that was nearby that was

eb8

important.

So we find in the data set that we are not measuring any values of local magnitude much above 7.0.

So this is what I meant by the scale saturates. It doesn't mean the earthquakes aren't larger, it just means that this particular measure of the earthquake, which is a high frequency measure, seems to have a limiting value of about 7.

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Q What does this imply with respect to the energy spectrum of the earthquake source?

A This implies that the energy spectrum of the source, rather than increasing uniformly across all frequencies as the earthquake total energy increases, rather what happens is the spectrum would rotate, being about the same at high frequency but relatively increasing at low frequency, so that large earthquakes are relatively very rich in low frequency energy. So that's primarily what happens.

Q What's the significance of that in terms of design considerations?

A I think the significance is that --

MR. FLEISCHAKER: I object.

MRS. BOWERS: What's the basis for your objection?

MR. FLEISCHAKER: The basis for the objection is that the question is ambiguous, it's overly broad.

MR. TOURTELLOTTE: If it's ambiguous, I would think the witness would be able to tell me it's ambiguous. And I don't really think that ambiguity is a proper basis for any objection.

If he objects he could object to the form of the question, he could object to the form of the question if he think this witness is not competent to answer and he could object on that basis. He could object for irrelevancy or immateriality but not because it's ambiguous, that's between

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me and the witness.

MRS BOWERS: Mr. Norton, I should give you some equal time.

MR. NORTON: Well I would hope Mr. Tourtellotte's argument doesn't preclude me from objecting on the basis of ambiguity, because I think it is a valid objection, although I think in this case it is ill-taken.

MRS. BOWERS: Mr. Tourtellotte, the Board didn't think it was ambiguous, but are you questioning him in an area of engineering that would be beyond his expertise?

MR. TOURTELLOTTE: Well I rather imagine if it's beyond his expertise, he'll tell me that too.

BY MR. TOURTELLOTTE:

Q Let me ask you, Dr. Smith, is the answer to that question beyond your expertise?

A (Witness Smith) Clearly it's not.

MRS. BOWERS: Well the objection is overruled.

WITNESS SMITH: The significance of this effect is that the ground motion which a seismologist specifies for use by engineers should not be scaled with the peak value as a function of magnitude, that the spectrum of ground motion -- it's shape, that is, it's distribution of energy with frequency is a strong function of earthquake size.

In a manner of speaking, I guess that's a direct

agb3

contradiction to the current NRC practice of scaling spectra, so I'm taking this opportunity to criticize that procedure.

Q I'm glad I asked the question.

(Laughter.)

Is that really a complete answer, though?

A Will you restate the question? Perhaps I misinterpreted what you were asking, I'm sorry.

Q I guess the question was, what's the significance of saturation, the concept of saturation on design?

A Oh, I'm sorry, I answered the wrong question previously. Indeed, Mr. Fleischaker was right, the question was ambiguous.

(Laughter.)

MR. FLEISCHAKER: And I knew what I was getting.

I'd like to move to strike the answer as non-responsive.

MR. NORTON: Excuse me, the only person that moves to strike an answer as non-responsive is the person who asks the question, no one else can make such a motion.

MRS. BOWERS: Well we have a situation here, though, where --.

MR. TOURTELLOTTE: Actually, Mrs. Bowers, I don't believe that I stated that question exactly as I did originally and I guess I would like to hear it back so that Dr. Smith can hear it. I know I did not restate it

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exactly as I stated it before.

MRS. BOWERS: I think you're correct on that.

Mr. Bloom, could you find it, please?

(Whereupon, the Reporter read from the record as requested.)

MRS. BOWERS: We don't think that's ambiguous.

MR. TOURTELLOTTE: I was going to ask Dr. Smith:

BY MR. TOURTELLOTTE:

Q You feel like you've answered that question?

A (Witness Smith) Yes. I feel like I answered that question.

Q And that question is not ambiguous?

A No.

Q Thank goodness.

(Laughter.)

MR. TOURTELLOTTE: Mr. Fleischaker had something else he would like to say.

MR. FLEISCHAKER: I would like to object --

MR. NORTON: To save time, Mrs. Bowers, if Mr. Fleischaker is now going to come up with another reason to object to the question, it's too late. The question was objected to on the basis he objected to it. The Board overruledtthe objection. The question has been answered. You cannot now go back and think of another reason to object to it and then move to strike it. It's not proper procedure.

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He's stuck with the objection he made.

MRS. BOWERS: We felt the only possible basis to sustain an objection was that the witness might think it was beyond his area. And, of course, he testified that was not true.

MR. FLEISCHAKER: Okay. I'll withdraw any further objections.

BY MR. TOURTELLOTTE:

Q Dr. Smith, I'm not really certain --

MRS. BOWERS: Mr. Tourtellotte, when you attempted to repeat that question you came out with a different question, so where are we on that matter?

MR. TOURTELLOTTE: I'll just withdraw that question.

BY MR. TOURTELLOTTE:

Q I'm not exactly certain whether this has anything to do with seismic moment or not, but as long as I've got you, I thought I'd ask you a question about a sentence on Page Three of your testimony. It starts at the end of Line 12, and it says:

"For example, the characterization of the Nacimiento Fault as being capable of an event similar to the 1952 Tehachapi earthquake should be relaxed in light of the present-day understanding."

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And my question to you is why?

A (Witness Smith) Primarily because of the fact that the 1952 earthquake was in the Transverse Range Province and had a substantial component of thrust motion.

And the Nacimiento Fault I would characterize now as being more in the kind of tectonic regime for central California that was described by Jahns earlier.

Q So what you're saying, then, is the modern-day understanding of mechanisms is different than when that characterization was originally made?

A Yes.

Q Okay.

Dr. Bolt, we've talked a great deal about seismic moment, and there's been some mention of magnitude as also measuring earthquake size, is that correct?

A (Witness Bolt) That's correct.

Q Is magnitude a more simplistic method of measuring earthquake size than seismic moment?

A Both are one-number estimates of the size of an earthquake. And so, in that sense, they're both very simple and cannot possibly give a full account of what actually happens in an earthquake.

The advantage of moment theoretically is that it's related, as I pointed out earlier, to some physical theory, whereas magnitude is not directly related to some

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physical parameter; it has to be done so empirically.

Q I'm asking my question about magnitude vis-a-vis seismic moment in terms of the capability of measuring magnitude or seismic moment in a very rapid fashion. Is there a difference?

A There is a difference. It is more satisfactory to an observatory to measure magnitude from available instruments than to try to measure moment.

Q Why?

A Because the magnitude is measured by simply getting a rule and measuring the amplitude of the greatest motion in the particular wave train that is being considered for that magnitude, whereas moment calculations involve calculation of spectra. Now, some observatories actually publish moments. But so far as I'm able to determine, they first calculate the magnitude and then put that into a formula and read off the moment.

Q So that moment is more difficult to calculate?

A That's correct.

Q Did we have the means, in 1927, to calculate the seismic moment of that event?

A Seismic moment was not dreamt of at that time. I think that, mathematically, someone like Professor Byerly could have done the calculations. It would have taken him quite a long time to calculate the spectra on a hand calculator

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rather than the high-speed computers that are used today. So that the means were there in a certain sense, but the concept was not.

Q Could we today calculate the seismic moment of that event on the basis of the information that we have?

A I believe Dr. Smith did that.

Q I'd like to invite your attention now to teleseismic information. There were some questions asked about teleseismic information yesterday, and I'm not sure if you gave a specific listing of the limitations that are imposed upon the accuracy of teleseismic measurements.

But if not, could you -- is there some way to swifly enumerate what those limitations are?

A One of the limitations is the distribution of stations. Since 75 percent of the globe is covered by water, it is very unlikely that one will have a uniform distribution of stations around the source, around the epicenter.

This is a particularly acute problem in California for reasons that were discussed. The stations are mainly in the Eastern United States, Canada and Europe.

A second great problem is the application of a standard travel timetable, because unless some special study is made, it's difficult to know just how well the standard travel timetable will apply to the area of interest.

As I pointed out earlier, we know that applications

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of the usual tables to central Californian earthquakes can give large systematic offsets of tens of kilometers. And that matter has been dealt with by the National Earthquake Information Center, for example, adopting the solutions obtained by the local observatories, using their local networks rather than relying on the routine methods of location using overseas stations.

I think they are the two major difficulties. Of course, if we go back to another time, there were difficulties in timekeeping. The clocks at the various observatories were pendulum clocks, and it was very common for there to be errors of timing of up to 10 seconds. That is no longer a problem today with with the use of crystal clocks and radio signals.

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Q What kind of margin of error might one expect in the timing devices used worldwide in 1927?

A I would not be surprised -- and I know from experience in working with solutions for earthquakes at that time -- to find certain stations being off by up to ten seconds from Greenwich mean time.

Q Well what's the impact of that kind of error in the timing device?

A That means when one does the adjustment of all the times by some least squares procedure, or graphical procedure, one must expect to find considerable residuals or deviations from zero down the whole list of stations, and you can't be sure whether the deviation is due to a mislocation of the epicenter; that is to say that the epicenter should be pushed a little bit to the north, a little bit to the east; or whether it's just a clock correction. There's no way now to know that.

Q Is there any way to estimate how far off one could be in locating an epicenter where a 10-second mistake was involved?

A Yes. One device that I used, and I think others would follow it, would be to look at a group of stations which are in the same area of the world. And if they are showing large errors -- 5 seconds, something like that, or up to 10 seconds -- together at each one of these stations,

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if five of them show a residual of 5 seconds, say, then one would suppose that this is not a clock error and that the solution is wrong.

Now in the Gawthrop solution which he published for 1926 where he lists the residuals, he's come to a certain conclusion about the location. But when you look down his list of residuals you'll find that there are clumps of stations where there are residuals of up to 5 seconds systematically for a group of stations. So this indicates if there was an attempt made not to give them zero weight but to obtain a solution in which they were reduced to zero residual, that you would get a substantially different epicenter. And my experience would be that it would be of the order of, oh, 50 to 80 kilometers difference, a different way from the original one: that kind of uncertainty.

Q Let me get one thing straight. You mentioned Gawthrop's paper and you said 1926.

A 1927; I'm sorry.

Q Is that 50 to 80 kilometers related to my question about the 10 seconds, or is that how far off Gawthrop might be?

A I think it's related to both things. I think that's a representative example--That's why I brought it in here--of the kind of error that one could get in the circumstances you proposed to me.

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Q Gawthrop indicates that he used International Seismological Center and the Bureau Central Internationale Seismologique bulletins from Berkeley and Pasadena. Are you familiar with those?

A I am.

Q Is there anything about those particular sources that might bear upon Mr. Gawthrop's interpretation of the location of the quake?

A No. I think that that's quite appropriate. Those are the main cata logues. And anyone starting to rework this location would start from that data.

Q Then the data itself is not in question, but how he uses the data?

A That is correct.

Q How he interprets it.

A That is correct.

At the risk of sounding pretentious on the matter I would like to say I think that this should be in evidence: that Mr. Gawthrop in attempting to get the solution, the revision of that earthquake, was pretty new to the game; that my own experience in locating earthquakes goes back very many years, and, as a matter of fact, the program that I wrote in 1960 to locate teleseisms has been adopted by the International Seismological Center and is used to locate all teleseisms around the world. So that in my judgment when I

wb4

look at the residuals in his table that he has published, he has not achieved what I would take to be any improvement in the revision. But there are clumps of residuals there that he just cannot explain except by giving zero weight to them. And my solution would not be satisfied with that.

Q I'm not really certain whether you stated this in our exchange here or not. But is there any way to reconstruct with any precision what the degrees of errors were on the various timing devices in 1927?

A Yes. One could do it for a specific station by looking at the residuals for that particular station for earthquake solutions over a period of days.

Let's suppose the station at Paris was reporting its arrival times, they were being used to locate earthquakes during a week of the year, and there might be, say, ten earthquakes in that week. If in every one of those solutions the Paris station continued to show residuals of minus-10 seconds then one could conclude that during that week the clock at Paris was in error by that amount to a very high degree of probability.

So there were those kinds of ways of judging what was going on at the stations at the time.

Q On the other hand, if the clock just happened to be off that day you wouldn't have any way of knowing?

A You would not have any way.

wb5

Q Do you know of anybody who has undertaken this sort of a task relative to the 1927 event?

A I don't believe it has been done.

MR. NORTON: Excuse me, Mr. Tourtellotte; do you have any idea how much longer you're going to be on cross?

MR. TOURTELLOTTE: I'd guess about thirty minutes, or so.

MR. NORTON: The problem we have, as I understand-- or that Dr. Bolt has, is that he has to be north this afternoon and there's a plane standing by. And there is a storm building up.

I just wondered if there were some way maybe we could go a little bit later into the noon hour so as to finish, so he can get out of here. Because if you look out the door, there's definitely a storm moving in. And I know the Board will have some questions also.

MRS. BOWERS: We do have questions.

MR. NORTON: Yes. I was just wondering if it would be possible to work a little bit later into the noon hour today. I don't know that we can finish: that's what I'm trying to get a feel for. There's no sense in working over the noon hour if we can't finish before the noon break.

MRS. BOWERS: Well, of course, we can delay the luncheon break. But I think we should have a five-minute break right now.

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MR. NORTON: Okay.

MRS. BOWERS: Five minutes.

(Recess)

MRS. BOWERS: Are you ready, Mr. Tourtellotte?

MR. TOURTELLOTTE: Yes. But I think I should correct something I said before the break. I indicated that my questions would take about thirty minutes. Actually they'll take about four. It's the answers that are going to take a little longer.

BY MR. TOURTELLOTTE:

Q Dr. Bolt, just for the record, the Gawthrop paper that I was referring to in my questions to you earlier was one by William Gawthrop entitled "Seismicity of the Central California Coastal Region," and published as Open File Report 75-134, 1975 by the United States Department of Interior, Geological Survey.

Is that the same one you were referring to?

A (Witness Bolt) Yes.

Q Is it your understanding that report was published by Gawthrop as a part of his Ph.D. disseration? Or do you know why he published it?

A The only reason I seem to remember was that he was employed by the U.S. Geological Survey one summer and was given the job to do. But I don't think it was a Ph.D. thesis at that time.

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Q He was a graduate student at the time?

A He was an undergraduate, actually.

Q An undergraduate student.

A Yes.

Q In one of the earlier remarks on the record by Dr. Smith, he said that this method was one of the most unreliable ways to locate old events. Do you agree with that?

A If there are other methods possible, one could make the comparison, and I would agree in that case. Sometimes, of course, there are no near stations and it's the only method. There's no way of making a relative comparison.

But if one has near stations and the ability of reading S minus P intervals, then I would certainly agree with that.

Q How about specifically applied to the case at hand? Do you think it is the least reliable method of measuring the '27 earthquake in this case?

A I do.

Q Now I want to invite your attention to Circular 672, Table Two, which Mr. Fleischaker asked several questions about earlier.

A Yes.

Q I'm not certain that I understood the significance of the figures used in that table, and I wanted to see if you could shed some light on that.

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Are these seismic considerations for design, or are they strict requirements for design?

In other words, are these figures in Table Two supposed to guid people in coming up with designs or are they values which are sort of rigid and demanding, or is there some other characterization of them?

A I think in all fairness to the authors, they felt that they were representing as best they could, according to their methods at the time, values which would provide scaling parameters for the engineers in this case.

However, I think they're working in the context there where the U.S. Geological Survey had some responsibility to provide the basic numbers. And so that's why they refer, for example, in the abstract to the design of the pipeline system must accomodate the effects of earthquakes.

This report characterizes ground motions for the specified earthquakes in terms of peak levels of ground acceleration. In other words, they were in Table Two giving numbers which they believed, I think, would be used by the engineers in the design of the pipeline. It's only later that this circular has been used in a more general way.

2d ebl.

Q I invite your attention to Note 2. It says:

"The values in this table are for a single horizontal component of motion at a distance of a few (three to five) kilometers of the causative fault; are for sites at which ground motion is not strongly altered by extreme contrasts in the elastic properties within the local geographic section --"

A Geologic section.

Q Yes.

A You said "geographic."

Q I'm sorry.

"-- geologic section or by the presence of structures; and contain no factors relating to the nature or importance of the structure being designed."

I guess what I want to ask you about that is it says, to cut out some of the middle part, that the values in this table are for sites at which ground motion is not -- I'm sorry. Well, let me ask you this question:

Does that note then indicate that the presence of structures has some bearing upon the factors that are used here?

A I believe that is what they thought, yes.

Q And that also the nature or importance of the

eb2

structure could have a bearing on the values that would be used in design?

A Yes.

Q Consequently I think you're saying it was primarily devised for a pipeline but when you're applying it to a building such as a nuclear power plant thatthese values may not necessarily hold true, and that's recognized in the table?

A I'm saying that emphatically, yes.

MR. FLEISCHAKER: I object to the question because it's ambiguous. It may hold true for what? May hold true as measurements of ground motion parameters? May hold true as a zero period limit for a design response spectra? What are you talking about? It's ambiguous.

MRS. BOWERS: Do you want to respond to that, Mr. Tourtellotte?

MR. TOURTELLOTTE: No, I don't want to respond.

MRS. BOWERS: Well, the Board understood the question and we thought the answer was responsive. We don't think it was ambiguous, so the objection is overruled.

BY MR. TOURTELLOTTE:

Q Mention was also made yesterday of the Hanks and Johnson paper. Do you have a copy of that paper?

A (Witness Bolt) Yes.

Q Can you compare the Hanks and Johnson paper with

eb3

Table 2 of Circular 672 and tell me, are the central ideas contained in each of those consistent, inconsistent, or mutually exclusive?

A I think in the USGS Circular 672, there was no testing of the notion of magnitude dependence or independence whereas in the Hanks and Johnson paper, they are endeavoring to establish whether there is a dependence of peak acceleration on magnitude.

The approaches were quite different and I think starting from different points of scientific testing.

Q Which do you think is the more reasonable approach?

A Well, I think the Hanks and Johnson procedure is the more scientific in examining the behavior of observed motions. That's not a criticism of the approach that the Geological Circular 672 was aimed at. They had a specific job to do, and they did it according to the best ways they could work out.

Q Is it your understanding that they simply assumed a relationship between magnitude and peak acceleration in 672?

A I think that's right. It wasn't an explicit relation but it was an implicit one.

Q Do you believe that the Hanks and Johnson paper is a more realistic approach?

ob4

A I think that in their paper, the Hanks and Johnson paper, there is less hypothetical assumption which is guiding them. They are trying to start from a tabula rasa and say if we plot this data, what does it tell us, and then in that sense it is a more scientific approach to the data.

Q I was asking whethere it was more realistic, or scientific perhaps. Does that also mean it is more realistic, in your opinion?

A Well, not necessarily. It was realistic for the authors of Circular 672 to attempt what they did at the time.

It was realistic for Hanks and Johnson to attempt to do what they're doing, too.

I really wouldn't go to the realism of it.

Q Putting it another way, which do you think has the better application to the construction of the Diablo plant?

A I would say Hanks and Johnson.

Q Dr. Bolt, you indicated yesterday in response to a question by Mr. Fleischaker that you lecture on response spectra.

A That's correct.

Q Would you briefly describe the nature of your lectures, what they cover? I don't want all the lectures, understanding that, at least hopefully, we can't get through that in 30 minutes, but some brief description.

eb5

A That part of the lectures that deals with spectra?

Q Yes, with response spectra.

A I try to explain to the students what "spectra" means. One starts with Fourier spectra, which is the plotting of the amplitude orthe energy of each frequency component in the wave train against the frequency.

I then show some actual spectra, Fourier spectra, frequency spectra that have been calculated from strong ground motion records, and make a comparison of these spectra.

I then discuss what happens when one approaches it from the point of view of a particle which has a certain mass or an object that has a certain mass which is being driven by the ground motion, by the earthquake shaking. When that particular mass is being damped and is attached to a spring which has a certain elastic constant, then that particle or object will vibrate somewhat differently from the way the ground vibrates. And the particle will have a frequency spectrum also.

And I compare that spectrum with the original one that we started with.

Q I notice that you mentioned frequency waves. Just again for the record and to help people who are not really familiar with how these things work, I would like to ask you a few questions which are designed to help us conceptualize these waves.

eb6

First of all, we're talking about three different kinds of waves basically. Is that correct?

A P-waves, S-waves and surface waves.

Q All right.

Now a P-wave, if we were to conceptualize P-wave individually, would that be roughly like reaching out and grabbing someone by the necktie and pushing them forward and pulling them back and pushing them forward and pulling them back, that same kind of forward push-pull motion?

A That's absolutely right, so long as it is done fairly gently.

(Laughter.)

Q And on the S-wave, to conceptualize that, if one were looking down on an S-wave from up above, it would be traveling along sort of like a snake travels. Wouldn't that be --

A Like a sidewinder, a sidewinder snake.

Q And it would look-- If it were frozen in place it would look like a series of S's connected together?

A That's correct.

Q And then the surface wave is sort of like a wave that we see out in the ocean?

A The Rayleigh wave.

Q The Rayleigh wave.

A That's correct.

eb7

Q I noticed as I was coming in here today that there's a breakwater that extends a great distance out into the water, and that the ocean waves were striking that breakwater but thatthe ocean wave did not break over all at once. Consequently it was not striking the breakwater all at once, but the full impact of the wave was striking it at different points at different times as the wave came in.

Now is that roughly how the waves arrive at a structure, that is, not in a single moment or not in a single instant but at varying points of time with varying parts of the various waves?

A A very close analogy, yes.

Q And that would be true, actually of the P-wave, S-wave or surface wave?

I think probably the analogy might indicate that we were only talking about surface waves, but actually the P-waves or some of the P-waves would be pushing while others are pulling, while others are somewhere in between?

A That's correct.

End 2d
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Q And the S-wave, if you think of the configuration of the S-wave, there would be different parts of different S-waves hitting the structure at different times.

A That's correct.

Q And the surface wave is as we described pretty much the ocean wave hitting the sea wall.

A Yes.

Q Does the length of rupture have anything to do with response -- let me withdraw that question.

Is the response spectra related to the length of the rupture, or is it magnitude-dependent?

A I think it's related to both things.

Q Which is the more controlling feature in the near-field?

A Well, of course the site in the near-field is going to respond mainly in the high frequencies to the waves coming from that part of the fault which is adjacent to the site. And our testimony has been that this is limited because there's only a limited amount of fault which is adjacent to the site.

But if one has a larger magnitude earthquake or a longer fault rupture length, then the propagation will extend away from the site and it will soon pass out of the near-field. But waves will still come back. And these waves will tend to be longer and longer period because the

mpb2

higher frequency ones will be damped out.

This means that the spectrum will not vary much at the high frequencies, but for the larger fault rupture lengths, the larger magnitudes, the amount of energy coming in at the long periods will be somewhat greater the longer the fault rupture length.

Q I had another question here. I want to make sure I don't ask the same one.

Did you just define near-field motion in terms of strong ground motion or not?

A No, I didn't.

Q Could you do that?

A Define the near-field in terms of strong ground motion.

I defined near-field earlier in terms of the distance away from the source, and that's the only way I can define it.

Q When you talked about the length of rupture, how much rupture are we talking about in terms of meters or kilometers in the near-field?

A Well, I defined the near-field as being that area within a couple of wave lengths of the source. So for one sink of the wave we figured out it would be the order offive to ten kilometers. So that as the fault rupture extended out beyond that circle it would pass out of the

mpb3

near-field. So the total length of rupture would be 10 to 20 kilometers.

Q The length of rupture for the immediate near-field would be five to ten?

A That's correct.

Q Okay.

And the greater response of a structure would be felt within the near-field?

A At the high frequencies.

Q The high frequencies.

A If you had a high-rise building 10 to 20 stories high, it would be responding to the long period waves coming from the far-field.

Q At low frequencies.

A At low frequencies, because they do respond to low frequencies; or long reach would not be affected by the near-field motion, it would be affected by the far-field motion which generates these long period -- continual long period waves returning from the fault.

Q But if you have a fault, then, that is 80 kilometers in length, in order to have the kind of rupture that we're talking about and maximize the near-field response, you have to have that rupture along that five to ten kilometers which is directly away from the structure, isn't that correct?

A In my view, yes.

mpb4

Q And if the fault were 115 kilometers long, or 200 kilometers long, it would still have to be directly in the same place on the fault.

A To get the maximum, yes, the maximum ground motion.

Q Isn't that fairly improbable that an earthquake of five to ten mikilometers would occur at that particular given point on a fault of 80 kilometers in length?

A Yes. That goes to another factor in the degree of conservatism that's being fed into the whole design process, that you have to multiply the probability not only of the peak acceleration that you're using, but also the probability that the particular earthquake source happens to be in just that place where the maximum will occur. And that, of course, is subject to some probability distribution also.

Q And if you had a rupture, for instance, that occurred outside the band to the extent that it was outside the band, say half of it was in and half of it was out, then half of that -- only half of the earthquake would be felt as near-field and the other half would be something less?

A Yes.

Q And it would also be true, then, that if -- the longer you are to assume the fault to be, the less likely that you are to have a near-field event.

A No, that's not true. I think the opposite is

mpb5

true.

We've gone along together up to that, but I'm not quite sure how we....

Q What's the relationship between peak acceleration and response spectrum?

A Peak acceleration is used commonly to scale the response spectra at the high frequency end of the spectra.

Q How does the relationship between peak acceleration and response spectrum differ in the far-field versus the near-field?

MR. NORTON: Excuse me.

We are talking peak instrumental acceleration when we say acceleration, for the record?

MR. TOURTELLOTTE: Sure.

WITNESS BOLT: Would you repeat the question, please?

BY MR. TOURTELLOTTE:

Q How does the relationship between peak instrumental acceleration and response spectrum differ in the far-field versus the near-field?

A (Witness Bolt) I'm not sure that there's been any fine distinction made in a lot of practice that I've seen in this way.

I think that if one is dealing with an important structure in the near-field then the engineers would be

mpb6

looking pretty closely at the kind of ground motion that might occur, and it might be more appropriate to scale slightly differently than just a straightforward application of the peak instrumental response because other matters enter into it.

But I can't really give you a general answer to the question. I don't think it's been often thought of in that way.

Q Does anybody else on the panel have any views on that?

A (Witness Frazier) Is the question how does the shape of the response spectra vary between the near-field and the far-field?

Q. Yes.

A Dr. Smith alluded to that a little bit this morning, except I think he might not have been specific in regard to response spectrum.

Basically we have a lot of recordings at distances greater than 30 kilometers, and there have been a lot of response spectra calculated for those strong motion recordings, and they have been averaged and they have been plotted against each other and we have a lot of information about the shape of that response -- about what that response spectrum looks like.

My statement that I'm about to proceed with is a combination both of theory and of observations. When one

comes closer to the source of the rupture the shape of the spectrum changes to some degree. The low frequencies do not increase as rapidly as the high frequencies do.

Or I could word that conversely:

The shape of the spectrum is altered in such a way that the high frequencies climb very rapidly as one comes into the near-field and the low frequencies stay more comparable to the far-field values.

Q Dr. Bolt, in your consideration of response spectrum and in the type of research that you do, have you had any occasion to be associated with the term "effective acceleration"?

A (Witness Bolt) Yes.

I was working on a working group set up by the Applied Technology Council some two years ago, whose task it was to consider ground motions that might occur from earthquakes across the whole United States, and to specify by means of maps levels of ground shaking that might be expected. And our group, after much thought, decided that we would work in terms of an effective peak acceleration and an effective peak velocity on grounds that although there may be from time to time high frequency peaks of acceleration or peaks of velocity which were higher than the bulk of the observations, they should not govern the levels of acceleration and velocity used for hazard zoning, risk mapping, codes,

mpb8

building codes, and so on; but that an effective peak acceleration should be used, which meant for us an acceleration and a frequency band somewhat less than eight hertz, and which had all the topography taken out and any special soil effects and so on.

And that was adopted and these maps now are published, county by county, across the whole United States. And they're coming, I think, into general use.

Q What was the name of your group?

A The Applied Technology Council.

Q And who sponsors that group?

A Well, it's I think an independent group of its own. But it has support from structural engineers associations, National Bureau of Standards I think supported this particular project, the National Science Foundation.

Q And the acronym is ATC?

A ATC.

Q And what you published was a code, is that right?

A Well, what my working group did was to publish some maps. We show contours of effective peak acceleration, effective peak velocity for the whole country. And these maps would be the basis for the engineering codes for the application of certain factors in the design of structures.

Q Do you know if there was a code that was

mpb9

actually adopted?

A I think the reason for the ATC-3, it's called, work was to develop a document which local and state instrumentalities and federal instrumentalities would adopt. It's a matter of others adopting this document.

Q How many people were in that group, in the whole group?

A My own working group?

Q Now just your working group, the entire ATC group. Do you know?

A It would be in excess of 50.

Q In excess of 50.

A Leading engineers from the whole country.

Q And how were they selected?

A I was not involved in the selection process. They seem to be very distinguished people to me.

(Laughter.)

Q Would you characterize them as outstanding experts in their field?

A I would.

MR. FLEISCHAKER: I'm going to have an objection to this line of questioning if it goes any further because it seems to me that there has been a failure to demonstrate the particular relevance of this line of questioning to the issue before the Board.

mpb10

MR. NORTON: Mrs. Bowers, I would join in that for the simple reason that we made the same argument that this is not a quantity case. This is the quality of the witnesses who appear and their opinions, and the association of others unnamed and so on really has no place in the record I don't think.

MRS. BOWERS: Do you want to respond, Mr. Tourtellotte?

MR. TOURTELLOTTE: Well, I only have one more question.

But it seems to me that we're talking about a concept which is already in the testimony which we know is going to be applied in the analysis of the design of this plant. And it is a concept which was developed by a group, and it would be nice to know that it wasn't developed by a group of boyscouts meeting around a campfire somewhere.

I think that how that concept was developed has every bit of relevancy because it goes to either substantiate or to discredit the ultimate conclusions that might be drawn by the Board about the weight to be given to the concept itself.

And as far as the -- I don't know what really the objection is since it seems to be an objection to my asking -- maybe asking further questions. And I don't think that that's appropriate, since the question hasn't been asked

mpb11

yet. And consequently I --

MR. NORTON: You want your next question struck?

(Laughter.)

MR. TOURTELLOTTE: Consequently I don't really know that there is any way to rule against me.

(Laughter.)

MR. NORTON: Well, Mrs. Bowers, it was the same complaint I had yesterday, this problem about unnamed, unpresent people. Gee, do they support this sort of thing? You know, I think this case should be heard on the merits of the people that are here.

Dr. Newmark is going to be here, Dr. Blume is going to be here, Dr. Seed's going to be here. There's going to be a lot of people here who have used that method. And I think they could make a very convincing presentation to this Board that it's a proper method.

And the fact that we're going to go out and take a poll or something just doesn't, I don't think, have any bearing on the record. And I don't want to see it done on either side.

MR. FLEISCHAKER: I join in that. I think that the decision has to be made on the basis of the evidence in the record and the witnesses opinions getting into the record.

My objection has a little different focus, and it is this:

That regardless of the merits of this concept as applied to any other structure, hospital, dam, whatever, the issue before this Board is whether the application of this concept effective acceleration and the reduction in the amount proposed in the reanalysis is appropriate for Diablo Canyon.

So I don't think that it's relevant.

(The Board conferring.)

MRS. BOWERS: Are we correct that you don't intend to pursue this line of questioning any further, Mr. Tourtellotte?

MR. TOURTELLOTTE: I have one more question.

MRS. BOWERS: In the same area?

MR. TOURTELLOTTE: In reference to the ATC, yes, and effective acceleration.

MR. NORTON: Well, we have no objection to hearing the question. We may object to the question after we hear it, but until we hear it I don't know how we can object to it.

MRS. BOWERS: Well, we consider what we've heard so far essentially a kind of an historical discussion of his group, and since the same subject matter will be discussed by other witnesses, we do think it's relevant to the proceeding. But we didn't want you to get into identifying each and every member of the organization and their discipline and

mpb13

this sort of thing.

So the objection is overruled. But, of course, we'll give it the weight that we've just described.

BY MR. TOURTELLOTTE:

Q Is Dr. Nathan Newmark the foremost expert in effective acceleration in the ATC group, in your opinion?

(Laughter.)

MR. NORTON: Maybe number two.

(Laughter.)

MR. FLEISCHAKER: I have no objection to that question.

MR. NORTON: Mrs. Bowers, I really do. I don't have any idea what the answer is going to be, whether he's number one, number three, or number two. But I don't see how it's really relevant to be asking one expert whether one of fifty is -- how do you rate? I mean, did they take a poll?

To me there's just no foundation for that kind of a question.

MR. TOURTELLOTTE: I'll rephrase the question.

MRS. BOWERS: Fine.

BY MR. TOURTELLOTTE:

Q Dr. Bolt, did you tell me that you thought that Nathan Newmark was the foremost expert on effective acceleration in this group?

A (Witness Bolt) I don't think those are my exact

mpb14 words, Mr. Tourtellotte.

My recollection was --

MR. NORTON: Excuse me.

I think he's answered the question.

MRS. BOWERS: He's entitled to explain his answer.

WITNESS BOLT: I did say that Dr. Newmark was very much involved in the setting up of the ATC-3 work, and was interested in all aspects of it, and was certainly interested in the idea of effective peak acceleration, and I think he felt that it was very much justified. And I do think he's a very influential and capable engineer in this country.

MR. TOURTELLOTTE: No more questions.

MRS. BOWERS: Mr. Norton?

MR. NORTON: We'll pass to the Board.

MRS. BOWERS: Mr. Fleischaker?

MR. FLEISCHAKER: I just have one line of questioning.

RECROSS-EXAMINATION

BY MR. FLEISCHAKER:

Q Mr. Tourtellotte, Dr. Bolt, asked you some questions about some solutions for the location of the 1927 earthquake derived by Mr. Gawthrop.

A. (Witness Bolt) Yes.

mpb15

Q Now your reference to his work -- or your opinions about his work are based upon the 1975 publication.

A That's what I was referring to here this morning.

Q Are you aware of any additional work that Mr. Gawthrop has done in locating that event?

A Yes.

Q What is that?

A I've seen a preprint of a paper of his that's going to come out in the Bulletin of the Seismological Society of America. In reading that it doesn't change my view.

Q Okay.

EXAMINATION BY THE BOARD

BY DR. MARTIN:

Q Coming last, or near to last, my only source of questions, except for one, I'd have to steal from other members of the Board.

I have a few questions I believe for Dr. Smith.

What's meant by the capability of a fault?

A (Witness Smith) It has two usages. There is, I believe, a fairly precisely defined usage in the NRC licensing procedure which has to do with age of most recent movement, and that kind of thing. In a more general sense the capability of a fault has to do with the physical possibility of plausibility of generating earthquakes in the future.

So in general we speak of the capability of the

mpb16

fault as some way of describing what kinds of earthquakes and ground motion it's going to provide in the future.

Q All right.

Is this something that can be determined by a geologist or a geologist and seism logist working together, or a seismologist working alone?

A The intermediate of those: the geologist and seismologist working together.

There are several criteria that are applied. Some of them are geological and some of them are seismological.

As an example, if motion can be -- movement of the fault can be verified based on geological data alone, that is sufficient to categorize that fault as capable. On the other hand, if for one reason or another the fault is inaccessible for geologic investigations -- for example, if it's a very deep fault and cannot be drilled or trenched -- it is possible to classify it as a capable fault based on some earthquakes that it may have generated in the past.

Q Is that the basic criterion, the earthquakes it has generated in the past?

A Yes -- well, no, that's one of the ingredients. That also would be sufficient.

Q Well, would you give me the whole recipe? I would like the list of ingredients.

A Well, the list of ingredients is basically --

mpb17

my recollection is three:

Either repeated motion as determined from geologic evidence, repeated motion over a very long period of time of the order of hundreds of thousands of years; or single episodes of motion over a period of tens of thousands of years; or the occurrence of significant earthquakes on the fault during historic times.

There is no question, and in my submittals on this project dating back to 1967 I believe before the term "capable" actually got a precise definition in NRC usage there was no question but what essentially all of the faults in this part of California would be classified as capable. So that that's not a --

Q All right.

Does capability have anything to do with the magnitude of an earthquake that might be expected on a given fault?

A No.

Q Just whether or not it's capable of accounting for an earthquake?

A Yes.

Q All right. Thank you.

My other question, or possibly questions, has to do with the mean in standard deviation given on page 28 of your testimony. And I have part of the answer. That's

mpb18

the source of the measurements that were used to calculate this mean of the standard deviation.

My understanding is that some of those measurements were taken from Table 1 of a paper identified as Joint Intervenors' Exhibit 47. That's Hanks and Johnson.

A Yes, sir.

Q Can you indicate for me which numbers from that table were included in your sample?

A Yes.

All earthquakes above magnitude 5.5 from that table. So that would essentially be the lower one-third of the table.

Q Did you include Ferndale, which is exactly 5.5?

A Yes, it does include that.

Q All right.

So there are 12 numbers there that you used, is that correct?

A Yes.

Q And then in your evidence on page 27 you make additional mention of one of those earthquakes listed in that table, the Pacoima. And then there are two others.

I'm not sure how to pronounce it, Naghan, Iran.

A Yes.

Those are the additional data points that were brought out in the earlier discussion that were added to the

table.

Q All right.

Then mention was made, according to my notes, of two others. One was a 7.2 earthquake in Russia. The Gazli was mentioned.

Is this the station name?

A I'm misunderstanding. The Gazli region -- the Town of Gazli was where the earthquake occurred. Karakyr was the specific locality of the station.

So that is the Gazli.

Q Oh, those are the same.

A Yes, in the sense, you see, that Pacoima is the location of the station --

Q I have it down as two different earthquakes because I heard -- you were talking about a 7.2 magnitude and I noticed on page 27 it cites it as .6.

A Again, illustrating the differences in different types of magnitude scales, I believe the larger one referred to 7.2 is the MS or the surface wave magnitude.

Q I see.

So that's only one point.

And then there was one mentioned from Tabataz on September the 16th.

A Yes. That's not in the data sets. However a simple calculation would show that adding that to the data

mpb20

set increases the mean value to .155, which does not change my conclusion.

Q Okay.

So the data set consists for the .49 mean consists of 12 values -- 14 values.

A 14 values, right.

Q All right.

Did you calculate the mean for the 12 you got from the table?

A I don't have that in front of me, no. It would necessarily be somewhat lower because I had added the most recent very large values. I'm sure that in the time since Hanks and Johnson's paper has been published there have been other earthquakes which produced smaller accelerations.

So I can't make an unbiased kind of statistical sample out of this. It's used by way of illustration of selecting our largest earthquakes.

Q So the whole universe of acceleration measurements in the near-field consists of 14 or 15 and perhaps not more than 20 measurements.

A That's correct, of earthquakes above magnitude 5.5.

Q I see.

Now do you have any difficulty drawing statistical inferences from this particular mean and standard?

mpb21

A Well, if you're asking am I comfortable or satisfied with this data set, the answer would be no, I would like more data. But I think that it's sufficient to illustrate the principles, primarily the principle that the peak ground motion close in to an earthquake doesn't scale in some simple way with the magnitude, however the magnitude is defined, but rather is a function of other physical properties as is outlined by Hanks and Johnson.

Q All right.

I'll put it another way:

Would you be comfortable or uncomfortable using this estimated mean of the standard deviation to calculate the probability of observing an acceleration in excess of one gravity in the near-field?

A Well, one would not use a table of earthquakes in a direct way to do the calculate that you just described. In fact, what you are describing is called seismic risk analysis, and is a subject of lengthy submittals which will be discussed by the next panel.

So the answer is no, I would not use a table like this to calculate the probability of exceeding a certain level of ground motion.

Q Good, because my next question is:

Are you aware of any such thing as a negative acceleration?

mpb22

A Oh, yes. Every other half-cycle of a record is a negative acceleration.

Q Would this apply to this table? In other words, if you make statistical inferences in the normal way, from these values you come to the conclusion that about ten per-cent of such measurements would be less than zero.

A All right.

You notice I have not specified what I believe the probability of distribution of this data set to be.

Q In that case I wondered why you bothered to calculate the standard deviation.

A I think it's a legitimate measure of the dis-persion of the data without specifying specifically the kind of distribution that's represented. But in common practice it tells if there's a great deal of scatter in the data or not.

Q All right. I just wanted to make certain you weren't relying upon that statistic for your conclusion.

A Clearly.

Q Do you attach any significance to the fact that the recent data that has been added to this rather circum-scribed list of measurements have all tended to raise the mean? I mean if you just took this table you would have a ratio of one to twelve in excess of 1g; and now the last two measure-ments have changed that to 3 in 15 or 1 in 5.

A Well, I think there's a very good explanation

mpb23

for that.

The initial installations of strong motion instruments were in large cities and in important structures. It's only been in recent years that there has been some real focus on trying to put instruments on faults where one might get near-field motion.

And I would point out that the largest acceleration in the entire table, 1.3g, was a vertical motion from the Gazli earthquake. That instrument was installed in that spot as a direct result of a very large earthquake that had occurred several months earlier. It was a very large foreshock, about magnitude 7, I believe.

So earthquake engineers and seismologists went to the area and put instruments right where the fault was. So if one is after a uniform sample of earthquake statistics you would have to recognize that faults are in fact the focus of current investigations of ground motion. And so I don't find it surprising at all, in fact, I think it will continue to happen in the future, that we'll have more near-field motion measurements.

Q Okay.

I think I should backup just a moment. I said I wanted to make certain that you hadn't relied on the statistics in arriving at your conclusions. And may I be certain that you have not?

mpb24

A Indeed.

However, I would point out that if all of the world's measurements of ground motion were 2g, even though I didn't specify the statistical distribution to it, it certainly would give me cause to wonder in reaching my conclusion that the 1.15g specified here is conservative.

So it's one of a large number of factors that enter in. Though I didn't use the statistical distribution, again I take some credit for the fact that the dispersion is not any worse than it is. I think to do a proper probabilistic approach one must put into the problem other important things, like the probabilities of where the earthquakes will occur on the fault and how far away they are, and that kind of thing has been done by many other people in the form of the probabilistic seismic risk analysis, but I did not do that.

Q Thank you.

I believe that leads into a question that Dr. Bright mentioned to me earlier.

(Laughter.)

He doesn't think so.

MR. BRIGHT: I'm not at all sure.

BY MR. BRIGHT:

Q Principally what I'm concerned with, we've been listening to all of the post-graduate work here, and I think mine would be characterized by Seismology I, something like

mpb25

that.

I have a great deal of trouble in trying to decide what do you mean when you say "epicenter". I can look at the map and I see these neat little circles and Xs and whatever. But then I learned that you have a quake and it extends upward to one-half the length of the fault, and whatever.

What is an epicenter?

A (Witness Smith) The epicenter is the point of the first rupture in the earthquake. If a rupture occurs over hundreds of kilometers, the instant the rupture begins it starts to send out seismic waves. Those are the ones that are timed at various seismograph stations. And the solution is based on that.

So the epicenter is typically the point of first rupture or projection on the surface, the point of first rupture on the earthquake. So if you had a long fault the epicenter might be at one end, or it might be in the middle, depending upon where the first break occurred.

Q Does this have anything to do with energy release? I mean, is this point the strongest energy release?

A Not necessarily.

Q Not necessarily.

Well, that brings up another little thing.

Say you had a weakness in a fault that is in a

mpb26

particular zone that would not take as much stress without going into shear as the formations on either side of it. Now if this one gave way, would this have a tendency to make the zones that probably are under higher stress unload?

A Yes. What you're describing is part of the dynamics of the rupture process. One would imagine that the initial rupture would in fact take place at either the weakest point or the point where the stresses first exceed the strength in the material;as soon as there's an adjustment the release of stress is there.

All of the parts of the fault zone are subjected to different stresses. That information that something has happened to one part of the fault that arrives at other parts of the fault by means of seismic waves is in fact the seismic waves that take care of this adjustment of stresses along the fault.

So indeed, every part of the fault that ruptures does change the stress field all along the fault and causes a progressive rupture in some cases or causes a sporatic multiple kind of rupture in others.

Q Well, pursuing that just a little bit farther, if you have a fault which dies out on each end and you can define it and all of that, is there any greater probability of a slippage or an earthquake, I guess we could call it, starting at any given point along this fault compared to another point on the fault?

A Considering the current state of knowledge, I would say probably not.

Q Dr. Frazier?

A (Witness Frazier) There's just one slight case in which we do seem to know something. Big earthquakes tend to occur on the deepest parts of the fault surface, but as far as looking at a map, I don't think so.

b 6

Q So the fault slippage-- The earthquake,-- pardon me -- this is more of a running crack than it would be a brittle fracture? I mean it doesn't happen all at once, it starts and then propagates? Is that correct?

A (Witness Smith) Yes. The brittle fracture does the same thing. If you break a piece of glass and take high-speed motion pictures you'll see that the cract initiates at a point and runs along at some speed.

But indeed, large earthquakes are currently viewed as multiple ruptures along a fault. I think there is quite a bit of evidence now to indicate that the rupture process on the fault zone is a very complex one.

eb2

A (Witness Bolt) If I may draw an analogy which has been helpful to me, if one thinks of a block of ice struck hard at a certain place as I remember the old icemen used to do, one sees a fault passing through the ice to break the block in half, and the dislocation is the front of that break as it moves from one side of the block of ice to the other.

So that's the source of the earthquake. The passage of that break is relieving stress which is readjusting in the ice block and sending out waves which, if you had sensors on the surface of the ice block, you would detect as an ice block quake.

That's the model.

Q I have gotten the distinct idea that the magnitude doesn't seem to have anything to do with the total energy release, or at least it's an extremely complex function.

A (Witness Smith) The latter statement is true. Energy release increases with the size, the physical dimensions of the source, so the total energy involved in a large earthquake is very much more than in a small earthquake. That energy is distributed over a broader frequency band and over a larger portion of the earth's surface.

What we've been stressing is at the high frequency end of the spectrum, close in to the earthquake volume, to the earthquake source, there doesn't seem to be any.

eb8

significant dependence on magnitude.

Q Well, I guess that's what I was wondering.

If you had what you might characterize as a short but still rather powerful shock which would show up on an accelerometer or whatever which is not very far away, then the amplitude of the squiggles would be quite high but the total energy release would not necessarily be anything compared to a 200-mile running crack which has less -- produces less amplitude on your squiggle?

A Yes. That's well-illustrated in the Hanks and Johnson paper where very tiny earthquakes have produced quite large acceleration.

The other element of this which has not entered much into the discussion has to do with the duration of time during which strong ground-shaking takes place and that's extremely important. in some instances, particularly in damage to soils, and other kinds of situations where repeated action produces cumulative fatigue-type effects.

So the duration of shaking is a very strong function of earthquake magnitude, as one might imagine, since the ruptures are longer and the period of time during which elastic energy can be radiated is much longer.

Q Well, let's see. Would it be fair to say then that the magnitude can be related to a particular area in terms of how bad things are, but the total damage has to do

ab9

with the total energy release and not necessarily the magnitude?

A No, that's not necessarily true.

Dr. Bolt can add something that may clarify this, but I would again point out that in my view, the most important effect of an increase in magnitude is the duration of shaking and the most significant effects this has are on soil type failures, liquefaction of soils, and that kind of thing.

If you're talking about total energy integrating over the whole duration of shaking, then in that sense of damage to soils, that would be true, but in elastic response of structures, if the structure is in the elastic range, then the damage is essentially independent of the duration of shaking so it is only the peak motion, so that's virtually independent of the total energy involved.

Bruce, do you want to further clarify that?

Perhaps not.

Q I think that sort of told me what I wanted, I think.

One thing I was wondering about on this business of short and long faults and the magnitudes one could expect, I had asked the previous panel whether the energy release was a function of the materials that were involved, and they assured me that it was, among other things, of course. All of these things we must qualify by "among other things."

eb10

Is there a basic limit on the amount of stress that a rock formation will stand, the amount of energy that it will store up before it will dissipate it one way or the other? Everything has a limit. I was just wondering if --

A (Witness Bolt) I think that's the key to much of what we've been saying, going back to the basic physics of it, that around the hypocenter of the source, this earthquake, the rocks have stored up a certain amount of elastic spring, like the spring of a clock, but of course they can only store up a certain amount because after a certain stage, they will just flow. They will flow like plastic, so that limits the amount of energy that is there.

That's why one cannot go up and up and up in the earthquake acceleration or the earthquake velocity or earthquake energy. It is strictly limited by the strength of the rocks. -- Thank Goodness.

Q A part of the discussion this morning had to do with the business of the actual energy release resulting in the shaking and this sort of thing is not really a point sort, it's a line source, I assume, or maybe even an extended plane source. But on that basis there were comments made such that well, here you had something that was right next to the fault and nothing happened to it, but yet somewhere else bad things occurred.

Can you designate an optimum location -- I mean

eb11

optimized to where the damage would be greatest? One automatically assumes whenever you're looking at this kind of situation that okay, the closer the fault, the worse the situation, but it doesn't sound to me as if that is really what we're looking at here.

A (Witness Smith) Aside from having a structure built astride a fault that would be torn asunder such as many structures in Daly City will be some day, there really isn't, I don't think, any optimally bad location. It would depend upon the frequency response of the structure, whether it was sensitive to duration or peak values or what periods and so forth.

Q Well, then you would have to take into consideration the various kinds of quakes that could happen along the fault, and this sort of thing I presume.

A To determine the severity of the ground motion, yes.

I would guess, in my opinion, now probably the most hazardous areas might be on the upper sliding block of a thrust fault.

Q Okay. Well, I only have one other specific question of Dr. Smith.

There was quite a bit of discussion of your 1975 contribution to the FSAR, I believe it was. It had reference to the maximum earthquake potential. And as I heard the

eb12

question the Staff had asked, it was additional discussion of maximum earthquake potential.

Did I mishear the question?

The question proposed by the Staff, not this Staff, the NRC Staff whenever they were trying to goad you into greater efforts, or efforts. And I was just wondering, was this the only thing that was ever submitted on maximum earthquake potential inthis case?

A (Witness Smith) No. This was the only submittal and discussion of the distinction in earthquake potential between different classes of faults. Going back to the initial submissions in 1967, we had postulated a magnitude 6-3/4 earthquake anywhere in this region, including directly beneath the site.

This reflected the state of knowledge at that particular point in history. I don't believe we have ever departed from that viewpoint. A great deal of geologic discussion took place and the concepts of different classes of faults arose, and so this question was addressed to that point.

To my recollection there is nothing in between the original specification of the earthquake for this region, which I believe is in the direct testimony.

Q So this was in a way an upgrading of what had gone on or --

A No, I viewed it as a clarification. The

eb13

geologists had introduced new information and it was an attempt to quantify what the different types of faults might be capable of in terms of their earthquake potential.

Q Well, I was just rather curious. I know the NRC asks an awful lot of questions sometimes and when you submit a calculation, an analysis, or whatever, which you characterize as being conservative everywhere, and grossly conservative under certain conditions, I just wondered -- it kind of made me wonder why that was really done. Just to be on the safe side? Is that it?

A Well, I was unable I guess to devise a better technique at that particular point in history.

I would point out that to my knowledge, no one had proposed any quantitative methods to get to the earthquake potential from the geological information other than the very simplistic approach of fault length-magnitude correlations which I firmly believe are nearly irrelevant to the problem. So I viewed it as one step in trying to bring some new information to the problem, and if I had been able to refine it more, I certainly would have done so.

Q Well, I think that does it.

MR. BRIGHT: Thank you, gentlemen.

MRS. BOWERS: Approximately eight or ten years ago the Atomic Safety and Licensing Board heard evidence for the construction permit for Diablo Canyon, Units 1 and 2.

eb 14

Now this was prior to the discovery of Hosgri. And we've been listening-- I should put this in a personal -- I've been listening to the testimony from Mr. Hamilton and Mr. Willingham on sparker runs and of course we saw reproductions of high-frequency -- I don't know what you call them. And we've been hearing more about seismology from this panel of witnesses, and we've had a lot of information given to us about Hosgri.

I think Mr. Willingham said that he felt that the sparker run boats had traveled something like 37,000 miles just on Hosgri.

We've heard about near fields and far fields and the whole thing.

BY MRS. BOWERS:

Q How can this Board have confidence that there is not a Hosgri 2 beyond the present Hosgri?

A (Witness Smith) I think this is basically the same question that we heard eight to ten years ago, and I distinctly recall discussions in those days that whatever approach was taken, one would have to assume that the earth would not reveal all of its secrets at one time, and that new information certainly was likely to come out.

So I think that there probably are additional faults offshore that have been perhaps not noticed. The real question is can any of this new information be of such significance that it could affect the conclusions for this

this plant.

A (Witness Bolt) I'd just point out to you, Mrs. Bowers, that when you're weighing this question it seems to me, as I did, that one has to ask one's self, does it make any difference?

Q That's what I'm asking.

A And that's what we've been trying to say, that in the near field the evidence is that, without going through it all again, that there are some limits on these motions and that having reached those limits, because we're here in California essentially and we're not in North Dakota, have we reached those limits in specifications?

And I think the evidence is that we have. So the presence of additional factors would not essentially affect any Board decision. That's the way I look at the question.

Q Thank you. Now the record shows your position on this.

Mr. Hamilton?

A (Witness Hamilton) Could I add one further comment, just as it relates to this question that you raised of might there be other Hosgri-like faults that we have not yet discovered?

I think it is useful to point out that the off-shore surveys that have been done in the years since the

construction permits were discussed have really not been restricted to the Hosgri Fault at all. Certainly the most detailed information that we have gathered has related to that fault, but other, more regional surveys have also been run which have given us a vastly improved general understanding of the structure and the location of faults at distances offshore, at least as far as the distance onshore to the San Andreas Fault.

So we don't really have an unexplored region left to us now. We have gotten-- Actually since those years much more detailed onshore mapping has been done and similarly, there is a good understanding of the regional structure offshore that does characterize the places where faults are and where faults are not.

So I think it is fair to say that we have a pretty good understanding of the general structure that precludes the existence of any fault that could be as large as the Hosgri or as influential in the local design.

A (Witness Smith) I didn't get a chance to fully finish my response which I wanted to get in the record.

I think the situation is similar to what it was in the construction permit days in that estimates for the purposes of the design of nuclear power plants have to be conservative enough that you have confidence that there isn't going to be any new data that is going to change your

conclusions. We'd all be uncomfortable with that in geology and seismology if that were true.

I think that was true in 1967. I offer as evidence that the procedures used and the data available were very much different in 1967, that the conclusions reached were sufficiently conservative that in my mind the discovery of the Hosgri did not take us beyond the limits of what had been considered in 1967.

I would point out that the geologic maps in those days had a lot of red lines on them for faults and out here the maps showed blue water, but no seismologist would ever believe that the fault stopped at the shoreline. There had to be fault activity offshore.

So implicit in the work that Dr. Benioff and I did in those days was the assumption that there certainly must be faults out there, that the number and length and distribution must look something like it is on land.

I felt comfortable with the conservative estimates that were done in those days, and I firmly believe that the Hosgri provided us no information to take us beyond the envelope of the limits of what was proposed in those days.

I think we have refined the data and the procedures today.

MRS. BOWERS: We have no further questions.

Mr. Norton?

eb18

MR. NORTON: We have no possible redirect after all these questions.

MRS. BOWERS: Are you suggesting the witnesses be dismissed?

MR. NORTON: I thought perhaps the Board's questions may have raised questions in the minds of the STaff and the Intervenor. I was waiting for that.

MRS. BOWERS: That's right. I should have checked with the other parties.

Mr. Fleischaker?

MR. FLEISCHAKER: No further questions.

MRS. BOWERS: Mr. Tourtellotte?

MR. TOURTELLOTTE: I guess I have no other questions.

With regard to the last exchange I had with Mr. Norton and Mr. Fleischaker, I would only like to say that the Staff generally views the matter of whether they do their job or not as to who gets mad at them. If only the Intervenor is mad, then maybe they're not doing the right job with the Applicant and if only the Applicant is mad, maybe they're not doing the job with the Intervenor.

And when both of them get after me, I kind of feel like I'm doing it right.

(Laughter.)

MR. NORTON: Or wrong.

eb19

(Laughter.)

MR. TOURTELLOTTE: No, I was describing how I feel, not how you feel, Bruce.

MR. NORTON: We would ask that these witnesses be dismissed at this time.

MRS. BOWERS: Any objection?

MR. FLEISCHAKER: No objection.

MRS. BOWERS: Mr. Tourtellotte?

MR. TOURTELLOTTE: No objection.

MRS. BOWERS: The witnesses are dismissed. And thank you.

(Witness panel excused.)

MRS. BOWERS: We will plan to reconvene at 2:30.

(Whereupon, at 1:30 p.m., the hearing in the above-entitled matter was recessed to reconvene at 2:30 p.m. the same day.)

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AFTERNOON SESSION

(2:30 p.m.)

MRS. BOWERS: We'd like to proceed.

MR. NORTON: Mrs. Bowers, at this time we're calling our next panel, which consists of the author of the written testimony, Dr. John Blume. It's testimony that's in Volume Two of the submitted testimony, the first 50 pages of text and then there are a number of figures attached thereto.

On the panel with Dr. Blume are Dr. C. Allin Cornell, Dr. H. Bolton Seed and a carryover from the prior panel, Dr. Gerald Frazier.

Before giving a summary of the testimony, I think it might be appropriate to go over the witnesses' qualifications with them and probably have them sworn.

MRS. BOWERS: All right. The record will show that Dr. Frazier has been previously sworn.

Will the rest of you please stand and be sworn?

Whereupon,

GERALD FRAZIER

resumed the stand as a witness on behalf of the Applicant, and having been previously duly sworn, was examined and tesified further as follows;

and

Whereupon,

agb2

C. ALLIN CORNELL,

R. BOLTON SEED,

and

JOHN BLUME

were called as witnesses on behalf of the Applicant, and, having been first duly sworn, were examined and testified as follows:

DIRECT EXAMINATION

BY MR. NORTON:

Q Dr. Seed, your professional qualifications are set forth at Pages 79 through 81 of the volume previously filed called, "Witness Qualifications."

Do you have a set of those in front of you?

A (Witness Seed) Yes, I do.

Q And is that a true and correct copy of your professional qualifications?

A Yes.

Q Dr. Seed, could you very briefly explain to the Board, summarizing this -- how your experience and professional qualifications lead you here today?

A Yes.

I hold several degrees in civil engineering. One from -- a Bachelor's Degree from London University, a Doctor's Degree from London University and a Master's Degree from Harvard University.

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I've been on the staff of the University of California at Berkeley since 1950, and have served as Department Chairman in Civil Engineering for a period of seven years, between 1965 and 1971.

I've been involved with the design of nuclear power plants on behalf of many organizations and been consultant to numerous organizations, currently including the Executive Office of the President of the United States, the U.S. Nuclear Regulatory Commission, the Atomic Energy Organization of Iran, the West German Nuclear Regulatory Authorities, the U.S. Army Corps of Engineers, the Bureau of Reclamation and a number of power companies in this and other countries including Venezuela, Argentina, Brazil, Philippines, Switzerland and so on.

MR. NORTON: At this time we would ask that Dr. Seed's professional qualifications be placed in the record as though read.

MRS. BOWERS: The entire group of qualifications have been admitted into evidence, so Dr. Seed's professional qualifications will be inserted in the transcript as if read.

(The document follows:)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	Docket Nos. 50-275
	)	50-323
PACIFIC GAS AND ELECTRIC COMPANY	)	
	)	Applicants Ex. No. 7
(Diablo Canyon Nuclear Power Plant, Units No. 1 and 2)	)	December 1978

PROFESSIONAL QUALIFICATIONS
OF WITNESSES FOR
PACIFIC GAS AND ELECTRIC COMPANY

Name: Dr. H. Bolton Seed

Title or Position: Professor of Civil Engineering, Geotechnical Engineering, Department of Civil Engineering, University of California, Berkeley

Degrees: B.S. Kings College, London University 1944; S.M. Harvard University 1947; Ph.D Kings College, London University 1948

Professional Experience: Dr. H. Bolton Seed is a member of the faculty of the Department of Civil Engineering, University of California, Berkeley, since 1950 and has been engaged in research and instruction in soil mechanics, seismic ground motion, soil liquefaction under seismic excitation, soil-structure interaction analyses for seismic response, seismic design of large civil engineering structures, etc.

Dr. Seed was Chairman of the Department of Civil Engineering, U.C. Berkeley, 1965-1971.

Foundation Engineer, Thomas Worcester Inc., Consulting Engineers, Boston, 1949-50.

Since 1953, Consultant on soil mechanics problems and seismic design problems to:

U.S. Army Corp of Engineers
U.S. Bureau of Reclamation
U.S. Geological Survey
U.S. Nuclear Regulatory Commission
U.S. Navy
U.S. Veterans Administration
National Aeronautucs and Space Administration
Oakridge National Laboratory
State of California Department of Water Resources
San Francisco Bay Conservation and Development Commission
Bechtel Corporation, Consulting Engineers
Kaiser Engineers, Consulting Engineers
Stone and Webster, Consulting Engineers
Asphalt Institute
Shell Development Company
J. H. Simons Company, Consulting Civil Engineers
John A. Blume and Associates, Consulting Structural Engineers

H. J. Sexton and Associates, Consulting Structural Engineers
Agbabian-Jacobsen and Associates
R. E. Davis, Consulting Civil Engineer
Woodward-Clyde Consultants, Consulting Soil Engineers
Dames and Moore, Consulting Soil Engineers
Shannon and Wilson, Consulting Soil Engineers
Law Engineering Co., Consulting Soil Engineers
Abbot A. Hanks, Consulting Soil Engineers
Compania Shell de Venezuela
etc.

Consultant during past year on seismic design problems to:

World Bank
U. S. Nuclear Regulatory Commission
Atomic Energy Organization of Iran
U. S. Army Corp of Engineers
State Rivers & Water Supply Commission, Victoria, Australia
State of California Department of Water Resources
State of California, Division of Highways
State of California, Division of Mines & Geology
Bechtel Corporation
Woodward-Clyde Consultants
Los Angeles Department of Water and Power

Harza Engineering Company, Chicago
Tippets-Abbot-McCarthy-Stratton, New York
Ministry of Planning, Nicaragua
East Bay Municipal Utility District
Motor Columbus, Switzerland
Tehran-Berkeley/Pandam, Iran
United Engineers and Constructors
Metropolitan Water District of Los Angeles
Fugro, Long Beach
Pacific Gas and Electric Company, San Francisco
Westinghouse-Hanford Company
Department of Interior - Panel to Investigate Failure of Teton Dam

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agbl

BY MR. NORTON:

Q Dr. Cornell, turning to you now for a moment, your professional qualifications are set forth in the previously filed witness qualifications book at Pages 18 through 20.

Do you have a copy of those in front of you?

A (Witness Cornell) I'm sorry, I don't have them in front of me, I looked at them recently, however. I was given them but I don't have it with me. The mistake is my own.

(Document handed to witness panel.)

Yes, thank you, I have them now.

Q Is that a true and correct copy of your professional qualifications?

A Yes, it is.

Q Dr. Cornell, could you very briefly give us a quick thumbnail sketch of your professional qualifications which lead you to be here today?

A I have received degrees in architecture and civil engineering from Stanford University, the Ph.D. degree in 1964 in the area of structural engineering with secondary qualifications in the probability and statistics.

My professional experience has included being on the faculty of Stanford and later M.I.T., where I am now a full Professor of Civil Engineering.

agb2

My interest has been in the development of probabilistic methods for use in the design and construction -- setting design criteria for various types of structures.

I've also served as a consultant to a variety of private industry organizations, utility companies and the U.S. Government including the Nuclear Regulatory Commission in this same area as applied to nuclear power plants. But my interest has, in fact, extended over other types of structures as well.

Q Dr. Cornell, Dr. Blume's testimony covers a wide variety of subject matters basically involving the point where we have peak ground acceleration and it involves the seismic risk analysis, probabilistic analyses of peak ground motions, et cetera, all the way through response spectra, structural response and so on.

It's my understanding that you have reviewed and contributed to those portions of the testimony dealing with -- well, with certain portions as opposed to all of the portions.

Could you briefly tell the Board which portions you have contributed and can adopt as your own?

A I have been involved in a review capacity for PG&E on primarily the seismic risk or seismic hazard analysis and the associated submittals to NRC. That is my primary involvement.

agb3

Q All right.

And Dr. Seed, the same question. Except with you it's my understanding that you have been not involved with what Dr. Cornell just described that his involvement was with, and in addition you've not been involved with the damping aspect of the testimony, but you are prepared to adopt as your own the remaining testimony, is that correct?

A (Witness Seed) Well I have reviewed Dr. Blume's report and I accept it in principal and I totally agree with his main conclusions regarding the Intervenors' Contentions Three and Five.

As you say, I feel that my field of competence does not allow me to talk in detail about structural damping capacities or about probabilistic risk analyses.

Q All right.

MR. NORTON: At this time, we would ask that Dr. Cornell's professional qualifications be placed in the transcript as though read.

MRS. BOWERS: They will be placed in the transcript as though read.

(The document follows:)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	Docket Nos. 50-275
	)	50-323
PACIFIC GAS AND ELECTRIC COMPANY	)	
	)	Applicants Ex. No. 7
(Diablo Canyon Nuclear Power Plant, Units No. 1 and 2)	)	December 1978

PROFESSIONAL QUALIFICATIONS
OF WITNESSES FOR
PACIFIC GAS AND ELECTRIC COMPANY

Name: Dr. C. Allin Cornell

Title or Position: Consultant and Prof. of Civil Engineering M.I.T., Cambridge

Degrees: B.A. Architecture, 1960, Stanford University.
M.S. Civil Engineering, 1961, Stanford University.
Ph. D. Civil Engineering, 1964, Stanford University.

Professional Experience: Research, teaching and consulting in earthquake engineering with special emphasis on probabilistic approaches to seismic hazard definitions. Dr. Cornell has acted as consultant on seismic design criteria and risk analysis for several nuclear power plants in the U. S., on air-craft crash risk analysis for nuclear power plants, on seismic risk analysis and ground motion for major dam projects, on wind-loading design

specifications for high buildings, on probabilistic fire safety analysis; etc. The consulting services were rendered to U. S. government agencies, to utilities, to engineers/architects companies, and to special engineering consultant firms.

The book by J. R. Benjamin and C. A. Cornell "Probability, Statistics and Decision for Civil Engineers", McGraw-Hill Book Co., New York 1970, can be regarded as the standard text book in this field.

2Fcont'd
agbl

BY MR. NORTON:

Q Finally Dr. Blume, we turn to you. I understand you're going to give us a summary of your written testimony today. But before doing that, I would like to have you review your professional qualifications which are placed in front of you there and ask you if they are a true and correct copy of the same.

A (Witness Blume) They are correct.

Q All right.

Now Dr. Blume, I also understand you have some typographical corrections to make to your testimony, is that correct?

A Yes. Some minor ones.

Q All right. Could you do that at this time?

A On Page 8, Line 12, the word, "phrases," should be changed to "phases." In other words, delete the letter "r" in that word.

On Page 12, Line Two, the word, "zone " has been used here in the context of the width, it has nothing to do with the length and I think the best way to clarify that now might be to delete the word, "zone."

On the same Page 12, Line 25, I would like to add the word, "effective" after the word, "peak."

On Page 17, Line 26, I think the English got a little mixed up. It can be adjusted by deleting the three

agb2

words, "a host of," and substituting the single word, "many," M-a-n-y.

On Page 19, Line 1, after the underlined word, "response," I would like to insert: "(spectral)".

On Page 27, Line 24, there's a word missing. It should read: "This is another conservatism." So we add another word, "is," after this.

On Page 30, Line 24, there are two words interchanged in sequence, the last two words. It should read: "pliant under the hypothetical."

On Page 44, Line 8, after the word, "most," I would add the word, "seismic."

That's all the corrections I have.

Q Dr. Blume, at this time, would you give us a brief resume of your professional qualifications and experience that leads you here today?

A Yes.

I have three degrees from Stanford University, the last being a Ph.D. in 1967, so I guess I'm the young graduate of the group here. I was a dropout for 30 years.

(Laughter.)

I'm a licensed civil and structural engineer in California, however, I consider my specialties to include the fields of structural dynamics, in which I have done pioneering work, and the field of earthquake engineering

agb3

and probability and risk.

I was co-designer, constructor and user of the world's first vibrating machine for dynamic research on large buildings, bridges, ground and dams and I also participated in the original recording of the strong motion of earthquakes and the various tests on structures of different types.

I've had the honor of having many awards for my contributions in structural dynamics and earthquake engineering, including three separate times the Leon S. Moisseiff Award of the American Society of Civil Engineers; Ernest E. Howard Aware, 1969; honorary membership in -- No, pardon me, I was elected an honorary member of the New York Academy of Sciences; I'm an honorary member in the American Society of Civil Engineers; in 1969 I was elected I was elected to the National Academy of Engineering and I hold various other distinctions that I won't bother listing here today.

I'm currently serving as President of the Earthquake Engineering Research Institute which, although it is a national organization, has members from 27 countries, over 800 members total engaged in every field of the earthquake engineering problem: in research, teaching, industry and in government.

I've served on a great many public committees and panels in federal, state, local, and I'm on several right now that I won't bother listing. They're in the written

agb4

testimony.

I've been involved with the nuclear-fueled power plants ever since their beginning in the sense of earthquake engineering. In fact, I recall doing, personally doing, a dynamic analysis and setting up the criteria for the first plant in Japan way back in 1960.

Our firm has been engaged in this type of work continuously ever since the start of this type of operation with nuclear plants. For several years, we also served as advisors to the Nuclear Regulatory Commission, formerly called the AEC.

I think that's probably enough for now.

Q Dr. Blume, is the John Blume Earthquake Center of Stanford University named after you?

A Yes.

end 2F

WRB/mpbl

Q That didn't have anything to do with your Ph.D. in 1967, did it?

A No connection.

Q Dr. Blume, how long have you been involved in the Diablo Canyon project?

A I guess from the very beginning. I was a consultant to PG&E in the early stages at the same time the trenches were being dug out at the site. So I have been not continuously, but it seems like in the last couple of years or so it's been almost continuously involved with this plant.

MR. NORTON: At this time, Mrs. Bowers, we'd like to have Dr. Blume's personal qualifications -- professional qualifications placed in the transcript as though read.

MRS. BOWERS: The document you've identified will be placed in the transcript as if read.

(The professional qualifications of Dr. Blume follow:)

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of PACIFIC GAS AND ELECTRIC COMPANY (Diablo Canyon Nuclear Power Plant, Units No. 1 and 2)	Docket Nos. 50-275 50-323 Applicants Ex. No. 7 December 1978

PROFESSIONAL QUALIFICATIONS
OF WITNESSES FOR
PACIFIC GAS AND ELECTRIC COMPANY

Name: John A. Blume

Title or Position: President, URS/Blume 1971-present

Degrees: Stanford University, Ph.D. Structural/Earthquake Engineering, 1967

Stanford University, Engineer, Structural Engineering, 1935

Stanford University, B.A. Civil Engineering, 1933

Profession: Licensed civil engineer and licensed structural engineer in California.

Professional Experience: From 1933 to 1935 Dr. Blume worked for the U.S. Coast and Geodetic Survey as a research engineer in its California Seismological Program during which period he codesigned, constructed and used the world's first vibrator for dynamic research on large buildings, bridges, dams and the ground.

He also participated in the initial recording of the strong motion of structures of various types induced by wind, pull tests, forced vibration, explosives, and from earthquakes. In 1935 and 1936 he was field engineer on the construction of the superstructure of the San Francisco-Oakland Bay Bridge, mainly conducting the measurement and control of stresses during construction. In the period 1936-1940 he was an engineer with the Standard Oil Company of California, in structural and earthquake design and as field engineer on several large refinery plants. From 1940 to 1945 he was Engineer-in-Charge-of-Design for H. J. Brunnier, Structural Engineer, on emergency (war Program) work on various Army and Navy projects including gun batteries, mine casements, depots, harbors, terminals, wharves and docks. He started his own practice in 1945; the firm was incorporated in 1957, and in 1971 it merged with URS Corporation, a professional services organization, of which he is currently a director.

Dr. Blume has worked in and been the recipient of various national awards for contributions in structural dynamics and earthquake engineering. These include the Leon S. Moisseiff Award of the American Society of Civil Engineers three times,

in 1953, 1961 and 1969; the ASCE Ernest E. Howard award, 1962; Honorary Membership in ASCE; Honorary Life Membership in the New York Academy of Sciences; election in 1969 to the National Academy of Engineering; Honorary Membership in the Earthquake Engineering Research Institute (1 of 4 in 30 years); and Honorary Member, Structural Engineers Association of Northern California. He has served as president of four major engineering societies in California and is currently president of the national Earthquake Engineering Research Institute which has over 800 members engaged in all aspects of earthquake engineering, in research, teaching, industry and government.

Currently Dr. Blume is chairman of the San Francisco Seismic Investigation and Hazard Survey Advisory Committee, a member of the state Department of Water Resources Special Consulting Board for the Oroville Earthquake (regarding Oroville Dam), member of the state Department of Water Resources Special Consulting Board for the Safety of Auburn Dam, and a member of the Consulting Board for Earthquake Analysis, Department of Water Resources, State of California. He is a member of the National Science Foundation Science Applications Task Force, and recently served as a member of the

National Science Foundation Advisory Group on Earthquake Prediction and Hazard Mitigation, and of the National Science Foundation Research Applications Policy Advisory Committee. A few years ago he served as a chairman of a National Academy of Engineering ad hoc committee on all natural hazards.

Dr. Blume has conducted considerable personal research and has written or co-authored over 100 papers, comprehensive discussions, books or chapters of books, plus many hundreds of technical reports. Nearly all of these writings have been in earthquake engineering, in structural dynamics, or risk analysis related to earthquakes.

Dr. Blume's firm's experience with the earthquake aspects of nuclear-fueled power plants goes back essentially to their beginning. He clearly recalls developing the dynamic procedures for analysis and design of a pilot nuclear plant at Tokai, Japan, in 1960. Subsequently his firm developed seismic design criteria for and conducted dynamic analyses of many nuclear plants in the United States and in Japan, Spain, Switzerland, India and Pakistan.

Dr. Blume's firm has also served for several years as consultants to the Atomic Energy Commission

(now the Nuclear Regulatory Commission) on the review of many other nuclear plants for earthquake resistance relative to plant licensing. He has done pioneering as well as extended work in dynamic models of all types, computer programs, response spectra, special time histories of motion, software, etc., since the advent of computer analysis of nuclear plants for seismic resistance. He continues to do extensive research in the field of earthquake engineering and structural dynamics.

mpb1

BY MR. NORTON:

Q Dr. Blume, it's my understanding that you have a number of photographs that are not attached as figures, but are photographs which are illustrative of actual photographs of buildings and stuff that are illustrative and discussed in your testimony, is that correct?

A (Witness Blume) Yes.

MR. NORTON: If we can take just a moment we'll have them marked so we don't have to interrupt the summary presentation to do it at that time.

I've given everyone -- the Board three copies, the Court Reporter three copies and Counsel one copy. And if you take off the top piece of paper you will find as a first photograph this one I'm holding up.

(Indicating.)

And I think if we all go through and mark them one at a time in the order they are -- we didn't want to mark them out of order.

The first one will be -- Mrs. Bowers, I believe our next exhibit number is 8. Unfortunately the young lady with the exhibit list went to lunch.

Yes, this will be Exhibit number 9. I'm sorry, our last one was 8. So the first photograph would of course be Applicant's Exhibit number 9, which is a photograph of a map showing the Bay area, the San Francisco Bay area.

mpb2

(Whereupon, the document referred to was marked as Applicant's Exhibit 9 for identification.)

MR. NORTON: The second one would be Applicant's Exhibit number 10, which is a photograph showing the Fairmont Hotel after the 1906 earthquake.

(Whereupon, the document referred to was marked as Applicant's Exhibit 10 for identification.)

MR. NORTON: The next one would be Applicant's Exhibit 11, which will be the Dewey Monument in Union Square, and the St. Frances Hotel after the 1906 earthquake.

(Whereupon, the document referred to was marked as Applicant's Exhibit 11 for identification.)

MR. NORTON: The next one is Exhibit 12, which is a photograph showing the St. Frances Hotel today, and the Dewey Monument today.

(Whereupon, the document referred to was marked as Applicant's Exhibit 12 for identification.)

mpb3

MR. NORTON: The next one, Applicant's Exhibit 13, is a picture of the Claus Spreckels Building immediately after the 1906 earthquake.

(Whereupon, the document referred to was marked as Applicant's Exhibit 13 for identification.)

MR. NORTON: The next one, Applicant's Exhibit 14, is the old Palace Hotel and the Grand Hotel immediately following the 1906 earthquake.

(Whereupon, the document referred to was marked as Applicant's Exhibit 14 for identification.)

MR. NORTON: The next one, Applicant's Exhibit 15, showing the ruins of the Palace Hotel after the fire in the 1906 earthquake.

(Whereupon, the document referred to was marked as Applicant's Exhibit 15 for identification.)

MR. NORTON: The next one is another view of the old Palace Hotel and the Monadnock Building, and that would be Applicant's Exhibit 16.

mpb4

(Whereupon, the document referred to was marked as Applicant's Exhibit 16 for identification.)

MR. NORTON: The next one is Applicant's Exhibit 17, and it appears to be a picture that will have to be described by Dr. Blume as to which building it is. It looks like the courthouse, the post-office, the federal building, the court of appeals, post-office.

WITNESS BLUME: Yes, that's the post-office and court building.

MR. NORTON: All right.

That's a modern-day photograph, obviously, judging by the vehicles in the picture.

(Whereupon, the document referred to was marked as Applicant's Exhibit 17 for identification.)

MR. NORTON: Likewise the next one, which is Applicant's Exhibit 18, and it shows a modern picture of San Francisco on Market Street, and that might be more fully described by Dr. Blume during his presentation.

(Whereupon, the document referred to was marked as Applicant's Exhibit 18 for identification.)

mpb5

MR. NORTON: The next one is the Rialto Building, Applicant Exhibit number 19.

(Whereupon, the document referred to was marked as Applicant's Exhibit 19 for identification.)

MR. NORTON: The next one is the Banco De Roma. Currently I'm not sure of the name of the building, but it's in this picture clearly identified as the Banco De Roma Building. That will be Applicant's Exhibit 20. It likewise is a modern picture.

(Whereupon, the document referred to was marked as Applicant's Exhibit 20 for identification.)

MR. NORTON: The next one shows in the upper-left portion one of the buildings shown says Bank of America, and that again is a post-1906 picture which Dr. Blume will be discussing, Applicant's Exhibit 21.

(Whereupon, the document referred to was marked as Applicant's Exhibit 21 for identification.)

MR. NORTON: Applicant's Exhibit 22 is a modern picture of the Flood Building, or at least post-1906.

mpb6

(Whereupon, the document referred to was marked as Applicant's Exhibit 22 for identification.)

MR. NORTON: The next one, Applicant's Exhibit 23, is a modern picture, or a post-1906 picture of the Emporium.

(Whereupon, the document referred to was marked as Applicant's Exhibit 23 for identification.)

MR. NORTON: The next one is the Hibernia Bank Building, Applicant's Exhibit 24.

(Whereupon, the document referred to was marked as Applicant's Exhibit 24 for identification.)

MR. NORTON: The next one, Applicant's Exhibit 25, is the Mint Building.

(Whereupon, the document referred to was marked as Applicant's Exhibit 25 for identification.)

MR. NORTON: The next one, obviously is the Golden Gate Bridge, Applicant's Exhibit 26.

mpb7

(Whereupon, the document referred to was marked as Applicant's Exhibit 26 for identification.)

MR. NORTON: The next one is Fort Point, the next two. Applicant Exhibit 27 and 28 are Fort Point.

(Whereupon, the documents referred to were marked as Applicant's Exhibits 27 and 28 for identification.)

MR. NORTON: Finally, the last one is obviously a modern day picture, an aerial view of most of downtown San Francisco, Applicant's Exhibit 29.

(Whereupon, the document referred to was marked as Applicant's Exhibit 29 for identification.)

BY MR. NORTON:

Q I hope you'll be able to refer to them now as Applicant's exhibits during your presentation.

A (Witness Blume) Yes.

Q Without further ado we'd like to ask Dr. Blume to give a summary of his testimony at this time.

2g

3a flws

A Our testimony today is boing to be about the criteria used to evaluate the Diablo Canyon plant from the Hosgri exposure; that is, given certain earthquake magnitudes and locations, what shaking would this cause, and should be done about it.

We are not going to get involved in structural analysis and the details of actual analysis, but the intermediate step between the ground motion where you've been for some time and getting to the surface, and the interrelationship between the ground surface and the structures.

Now I had a long list of terms I was going to define: I think it's in my written testimony. I've heard many of them discussed and I intend to skip them unless the Board wants me to cover some of them. I'll note the ones I'm skipping, and if you'd like me to give our definition I'd be pleased to do so.

I imagine you're well familiar now with acceleration, velocity and displacement. I've heard the term time-history used interchangeably with records of strong ground motion, so I won't get into that one.

The words "instrumental acceleration" have been used. I think I would like to point out what my definition is. I'm referring to "instrumental acceleration" as the peak absolute value of motion in terms of acceleration units that would be measured by an instrument in the free field.

wb2

And by "absolute value" I mean it doesn't matter it's plus or minus, whichever is the greatest numerical value.

This has been referred to for this plant as 1.15g, or gravity units.

The term "effective acceleration" has been used a little bit also. It's coming to come up over and over again, I'm afraid. In the terms with which we will use it, effective acceleration is referring to that acceleration that the designer uses to construct or to anchor his response spectrum. I will define "response spectrum" in more detail later. In other words, it is that acceleration which is considered significant and is used to develop the response spectrum for which the plant is designed.

The number .75g has been used in that regard and has been used in the reanalysis of the plant.

I don't think "natural period" has been discussed, though it may have been. If you consider an oscillating body that moves from some extreme point on, say, the left side and swings over to the far right side and then all the way back to the starting point again on the left side, that complete oscillation is the period. The time to do that oscillation is the period usually given in seconds. And the term will be used over and over again.

Now everything structural has a natural period of vibration, or maybemany natural periods of vibration. We

wb3

like to think of a pendulum as being the most simple type. But buildings and instruments and plants and piping and all these things have various natural periods of vibration.

The reciprocal of that, or, in other words, that number divided into 1 is called the frequency or the natural frequency. And it's usually given in terms either of cycles per second or, in more recent years, in terms of hertz, which stands for the same thing.

Damping has not been covered very much. I will get into it in more detail later. But for a preliminary definition let us consider the fact that energy cannot be lost, it can transfer form from one form to another but it is never lost.

Now if you start a body vibrating it has a lot of kinetic energy as it's moving through the greatest oscillations at the fastest speed. And the body would keep vibrating forever if there were no damping. In other words, damping is a form of energy loss or transfer. In the vibrating body in the context that we're speaking today that leads to the gradual decay of the oscillations to the point where the motion completely stops. It's a form of energy transfer, and there are various forms.

Stress and strain are often talked about. Let us take a brief sample of a bar in tension anchored at one end and somebody pulls on the other end. This creates tension

wb4

in hat bar.

The stress is the total force used to pull that bar divided by its cross-sectional area. In English units it's usually given in pounds per square inch.

Strain on the other hand refers to the amount of deformation of that bar in terms of the unit amount of strain per unit length.

So we use these words interchangeably when actually we shouldn't.

Now according to our classic law called Hook's Law, stress and strain are proportional. This is only true in the elastic range up to the point of yield. And yield is defined as the point where it no longer is a proportional situation.

In the design of a plant such as Diablo Canyon under extreme earthquake motion design often extends almost to the yield point sometimes: it may go to the point or slightly beyond. When one goes beyond the yield point there is not failure if the material is ductile. And we then enter a whole new world of structural and dynamic significance called the inelastic range. And in this inelastic range we have a tremendous amount of potential energy absorption that I'm sure will come up later in the case.

In other words, just because a stress reaches its so-called yield point, or the end of the point where it

wb5

proportional to strain, does not mean failure, except for a brittle material like glass.

Now I want to get into the response spectra. I know it's been mentioned two or three times. But it's probably one of the most important things to be brought up in the case. And I'm going to put a slide on now that may help me to explain the response spectra.

Q Excuse me, Dr. Blume, please be sure to refer to these now as they appear in your testimony, which figure it is, so the record will be clear.

A All right.

(Slide)

This is Figure A from the written testimony. And I've oversimplified this. I've found over the years that the concept of a response spectrum is rather difficult even for many engineers.

Let us take here a time-history of ground motion. This is just drawn at random: it's not a real time-history. And we're going to take a series of what I call "lollipops." These represent single mass, single degree of freedom vibrating systems, each one having the same damping characteristics.

Now let's take the one on the far left. In the computer we will put as input first of all the complete time-history and the ground motion, and then the characteristics

wb6

of this single oscillator, its natural period of vibration and its damping. And then we will subject that oscillator to the effects of the entire time-history of motion and come up with a maximum result over that entire period. And that will create just one point right below it here on the diagram.

This will be repeated for each and every one of these, although in the computer we put them very, very close together. Each time this is done we find a point on the lower diagram which simply represents the maximum response that would be obtained by that particular oscillator being subjected to that particular ground motion.

Then if we merely connect all of these up with a dashed line we have a response spectrum for that given damping value and for that earthquake.

Now I didn't mention the zero period. You see the lower scale is period and over here we would have the zero period. That point right there is simply the same as the effective acceleration that's been talked about.

Now I'll got to the next slide.

(Slide)

This is Figure B from the written testimony. It's merely an example; it has nothing to do with Diablo Canyon plant. But we have here an example of 1, 2, 3, 4, 5 response diagrams for a particular earthquake. This happens to be the

wb7

El Centro Earthquake of 1940 in the north-south -- southeast direction.

The Greek letter lambda merely represents the damping ratio. The upper curve has no damping at all. The next has 2 percent of critical; 5 percent; 10 percent, and 20 percent. So right away I have to define what I mean by "critical damping."

Damping ratios are usually given as either a percentage or as a fraction of critical. And critical is that damping at which a system would simply not oscillate. If you disturbed it or pulled it over it would merely go back to its starting position and not oscillate at all. That's called critical damping.

So even though damping is a very complex system, we refer to it in this term which really represents a viscous damping proportional to velocity. It's not correct, but it's a very convenient method and it is universally applied in the nuclear and other fields.

Now as you saw on the second diagram, actual response spectra are rather jagged: they have peaks and valleys, sharp peaks and valleys. And we go through a process called smoothing.

Now there are various ways you can smooth a spectral diagram. One is to simply average the peaks and valleys and go through a process where you come up with a

wb8

curve that's equivalent to roughly what you have. A more conservative procedure that is too often done is to take only the peaks and connect the peaks and draw the line. Another method is to take several earthquakes, instead of one, perhaps eight or ten or more, and to run them for the same oscillator and then for very narrow period bands to run statistical analyses and determine average and standard deviation points as required.

By various means we smooth spectra. Now the reason for smoothing is very simple. If we didn't, you can see that the designer, in trying to apply that to an actual case, would have a very,very difficult time. He also might be tempted to get into the valleys and avoid the peaks. So this is avoided by the smoothing procedure.

There are standard spectra. Regulatory Guide 1.60, for example, provides recommended standard spectra. We were one of the two firms that worked in the development of that guide. I think it's an excellent document for typical average sites. But it does not apply to Diablo Canyon for several reasons.

One is, this is a rocky site. Another is the Hosgri controlling earthquake is so close to the plant. Nevertheless the comparisons have been made by ACRS and others.

Spectra can be scaled. This is another procedure.

wb9

One can decide what zero period or effective acceleration he wants to work with, and then scale other records from other earthquakes up or down depending on which way he wants to go. And you can put a bunch of these together and average them out. There are all sorts of ways of arriving at spectra.

A procedure that is often used in design is to modify a time-history in such manner that it will produce a given smoothed response spectrum. This can be done. It's perfectly legitimate. If it is not done, and if it is desired to use a time-history in analysis, you would then be finding the peaks and valleys again that we do not want. So pulses are added or subtracted at strategic points in an actual record or in an artificial record in order that a time-history can be derived which, in turn, will almost exactly match any given response spectrum.

I'd like to discuss briefly now the history of this project insofar as response spectra and design specifications are concerned.

In our opinion -- and I think it is borne out by ample facts -- there have been three designs, or three analyses of this plant, not just two.

The first I call the original pre-Hosgri design. Then as soon as PG&E and its consultants learned about the existence of the Hosgri offshore and the fact that that had

wb10

to be considered in addition to the prior earthquake, there was an initial Hosgri criterion developed by PG&E, the applicants, using 6.5 as the magnitude. And I will show you how that works a little later one.

We are still of the opinon that that was an adequate design criterion, 6.5 maximum magnitude for that fault.

End 3A

In the last couple or three years we've been working to what we now call the current Hosgri criteria based upon 1.15g instrumental acceleration, 0.75g effective acceleration, as recommended by NRC, and using a 7.5 magnitude earthquake as recommended by USGS.

In developing the criteria for the original plants we considered four earthquakes as prescribed by Drs. Benioff and Smith: the San Andreas fault, 48 miles away with a magnitude 8.5, the Nacimiento, 20 miles away with a maximum magnitude of 7.25, the Santa Ynez, 50 miles away, that is extended out into the ocean so that the Santa Ynez would have been 50 miles away, a magnitude 7.5; and for the first time ever a local earthquake nonassociated with any known fault considered 12 miles away to its focus in any direction, including straight down underneath the plants and having a magnitude of 6.75.

I think this is very, very important in realizing that we use this earthquake in helping to explain why the plant can today meet the present criteria.

MRS. BOWERS: Pardom me for interrupting.

You didn't give this a name. Is this hypothetical, a hypothetical earthquake?

WITNESS BLUME: This is entirely a hypothetical earthquake. The only name we have for it is Earthquake D. We call A, San Andreas, B, Nacmiento, C, Santa Ynez, and

mpb2

D was the one that was just floating around.

For these various earthquakes we used actual earthquake records as models. We used the 1952 Taft record north 69 degrees west for earthquake B. And for earthquake D we used the 1957 San Francisco record taken at Golden Gate Park on sandstone. And these were normalized after much discussion and many meetings back in Washington with AEC then, they were finally normalized to .40g for the D earthquake at the zero period and .30 for the B earthquake.

May we have the next slide, please?

(Slide.)

This is Figure 1 from the written testimony. And I want to point out first of all, and very carefully, that this is for the operating basis earthquake, not the safe shutdown or the double design earthquake.

You'll see the very sharp peak that I'm pointing to; the highest peak of the diagram is for earthquake D, the one without any known fault. And it comes up to a very very sharp peak and it finally comes in at zero period to .20g.

The other curve, the lower curve, is for earthquake B, which is the Nacimiento situation, and that was normalized to .15g, which you can see, at the zero period again.

Now the designers were forced to take whichever

mpb3

one of these governed any particular situation, and I was very very unpopular at the time for having come up with this thing. They thought it was crazy, I think. But we had to do it in view of the report of this seismologist. There was no way out of it.

We had to consider two separate earthquakes as governing that plant.

Now double these values, exactly twice, were used for the shutdown conditions. So that would bring this up to .40g, which would be the shutdown acceleration. But this high hump due to the local earthquake D has been extremely beneficial in the plant meeting the present criteria.

(Slide.)

This next slide is from page 11 of the written testimony. It's just a portion of the page, and it shows the damping values that were allowed in those days for the original design. Again I'm repeating this old material because it is very very instructive in explaining why we have so much value in the plant today.

The damping values allowed then were very very low as compared to what they are today. For example, today this is seven percent and these are up to two or three, and depending upon conditions.

BY MR. NORTON:

Q Excuse me, Dr. Blume.

mpb4

When you say "these" with your pointer, it doesn't mean much in the written record.

A (Witness Blume) That's correct.

Well, five percent for the concrete structures is now seven percent for both concrete and bolted steel structures, whereas in those days you can see on this slide that two percent was used for bolted steel structures.

The damping variations for piping are even much greater, which will come out later, I think, in the testimony.

Now when the Hosgri was discovered Dr. Smith and Dr. Jahns, Mr. Hamilton, many others worked on the problem feverishly. And it was decided by them -- and I concurred, feeling I had some background of 35 years in the earthquake field -- that 6.5 maximum was all that could support. So the second criterion was developed based upon 6.5 maximum magnitude, a normal surface distance of six kilometers, and a depth to the focus of only five kilometers, which is quite shallow.

These result in a hypocentral distance which is the slant distance of the plant to the focus as low as eight kilometers.

We also arrived at an effective acceleration of .50g.

I'm still of the opinion that that was a reasonable value for the given conditions.

mpb5

Now, to model the conditions, then, I used another approach all together which I felt would be more appropriate for a close-in situation where we have a plant within a few miles, two or three miles of a potential earthquake of that size, 6.5 magnitude. So after a study of all the records available in the world at the time, I finally used eight record components which had magnitudes ranging from 5.3 to 6.6. They were normalized to .5g. By that I mean they were scaled to .5g; as I mentioned a little while ago, we just scale up and down. And this led to a composit array of eight spectra on one diagram. And the diagram was divided into narrow period bands.

Each band had the data points analyzed statistically to arrive at a mean, a median, and standard deviation. And we proceeded to develop response spectra along those lines.

I think Table 14 will help out in that regard.

(Slide.)

I'm now showing page 14 from the testimony, a part of the page. And this indicates the eight earthquake records that we used to develop the model for the conditions at the site.

You'll notice in the third column from the right side that the epicentral distances were in the order of three to eight, eight, seven, and three, which were very close models to what we're talking about, namely five or six

mpb6

kilometers.

Another common factor was that each one of these records was taken on rock, which also made it a pretty good model.

I'll now show the results.

(Slide.)

This is Figure 2 from the testimony. The upper curve labeled E-Hosgri, five percent, represents this new situation. And, by the way, we gave the designation E to this newly discovered Hosgri fault when we first worked on it.

We previously had B and D, and then we gave this the designation E.

The upper curve shows five percent damping, and the curve just below that shows seven percent damping. And we've done this because in the interim between the time of the initial design and the present day everyone agreed that five to seven percent was a logical change in allowable damping values.

So, for comparison purposes in the old design of B and D, which are the two dashed curves or the other curves shown -- they're not all dashed on this figure -- we can see that there are places in the response of the plant namely from a period of about .1 to .2 or more where earthquake D, the original design criteria was greater than we had with

mpb7

the brand new earthquake E for the Hosgri.

Now this did not obtain throughout the entire length, but it did in the very very important area of .1 to .2 where there is not only many important structures, but many important equipment, periods, and frequencies.

(Slide.)

I will now show page 15, part of page 15 from the testimony, which shows the new damping values which were allowed in Regulatory Guide 1.61, and were in general used throughout the country for earthquake design of nuclear power plants. You'll see on the bottom line that reinforced concrete is now seven percent of critical damping. Coming up we find bolted steel structures also seven percent; welded steel structures four percent of critical; small diameter piping, two percent; and equipment and large diameter piping, three percent.

Now these values, which as I said were acceptable, and we will demonstrate that later, are much greater than the original values.

Now, what does this mean?

Damping is a very important parameter. The greater the damping that you use in analysis, the lower the response of the structure, the lower the response diagram. Therefore you would use less material, and you would have a lighter structure if you started out with heavy damping. But

mpb8

we did just the opposite. We started out with very light damping. Years later we found out with new knowledge we could go to heavier damping. So we had excess material provided in the structure on today's standards.

So between the two, between the change in damping and the fact that we had that hump from earthquake D, we had two very very important factors that make it feasible with some modifications to design -- to have a plant that will stand not only what we consider a reasonable earthquake like 6.5M, but a very very conservative earthquake, 7.5M.

I won't spend any time talking about the current criteria except to show a couple of graphs later on.

I would like to say that the current criteria are two-fold. One set was made by Dr. Newmark for NRC. Another set was made by myself and my office for the Applicant. There were a great many meetings at which these things were discussed.

These sets were made completely independently. Some compromises were made in the course of some of the meetings. But in general it wound up that both sets were prescribed and the Applicant was forced to analyze his structure for both the Blume criteria and the Newmark criteria, and this really makes four designs now that we're talking about.

I think the next slide will be useful.

(Slide.)

3B

3C agbl

This is Figure D from the written testimony, and it gives us a comparison of the various curves. Now, going back in time, we first of all had the original DDE at five percent damping, which is shown by the dashed and dotted line with the hump in it.

Then we have the second go-around with the E. Hosgri for seven percent damping, and these can all be compared due to the change in allowable damping values. And we see the condition that I showed on a prior slide, where they're very close together in general.

Then at the top we see the Newmark and the Blume curves that we're presently working with.

The Newmark curve is the one with the dashed line with the straight top, the flat top, and for seven percent damping.

And the Blume curve was derived by using those same eight earthquakes, rock earthquakes that I talked about only simply scaled up from five to 7.75. It was a scaling process again. So we then have to account for all this difference.

Well, fortunately, as I mentioned before very briefly, a great many of the very, very important parts of this plant are so strong and rigid that they fall in the high frequency or the low period range, and there weren't really too many things to be worked out although there have

agb2

been very extensive alterations.

I think now I would like to turn off the slides for a while.

Contention Three states that 0.75g is not appropriate for the safe shutdown earthquake. I concur. I do not think it is appropriate because I think it is way too conservative. I've outlined what it is, it's 7.5m, 0.75g effective and 1.15g instrumental.

I believe that Contention Three is perhaps misleading in its very wording, because it refers to the maximum laboratory acceleration as 0.75g whereas, in fact, it is 1.15g.

I'd like to mention briefly that peak acceleration is not the only criterion. In fact, it is a very weak criterion for plant resistance, spectral response is much more meaningful and there are many other factors, such as redundancy and ductility and reserve capacity and other things that are even more important in many cases.

It has been mentioned that a response spectrum does not bring in the element of duration or time of shaking. This is true because the only thing plotted on these diagrams is the maximum response. But I wish to point out that shaking in the elastic range under Hook's Law, again, the duration is unimportant, unless you're going up to thousands or millions of scycles, it doesn't make any difference.

agb3

Duration is only important when you have a failing structure and then the longer it shakes, the more it fails. But when you're in the elastic range, it is not important.

There are various ways of going from instrumental to effective acceleration and I'm sure we'll talk about many of these during this hearing. One simple way is to count the number of peaks such as has been done in Circular 672.

I think Dr. Newmark started out with 1.15g and estimated that about seven peaks would be ineffective, and so he came down to 0.75g at about the seventh peak, sixth or seventh peak.

We have done a very comprehensive study that is reported in DLL-30, one of our exhibits that are on file, in which we deliberately took records, 18 to 20 records and clipped various amounts of the peaks off and then reran the response spectral diagrams through the computer.

In general, we found that clipping peaks or augmenting peaks, either way you go, produces a very, very weak response or change in the response spectrum. And what a structure really feels is the response spectrum, not the peak acceleration.

In general, we found we could clip as much as 30 percent off of all of these records and average only about a five percent change in the response spectrum.

In other words, random spikes -- and I define a

agb4

spike as a very narrow peak on an accelerogram, have very short time duration and, therefore, contain very little energy even though they are high acceleration--are not really meaningful as far as structural response is concerned.

Now, to justify going from an instrumental acceleration to an effective acceleration, there are various ways. I like to think of four basic situations. One is observation, very careful observation of what has really happened in the real world and what has not happened in actual earthquakes.

Another is theory and analysis. And I think that clipping study that I just mentioned is a good example of that. Another is testing and experimentation. And, finally, engineering judgment, something we can't seem to do without. And those four are not mutually exclusive, necessarily.

I would now like to go back to the slides, because my first example of observation, of looking at what has happened and has not happened, is going to be the Great San Francisco earthquake of 1906, which was an 8.3 magnitude earthquake very, very close to San Francisco.

(Slide.)

This is Exhibit Number Nine, and it's simply a map of the San Francisco Bay area. This red triangle represents the heart of downtown San Francisco in the olden days in 1906 and the other red triangle represents the

agb5

present Golden Gate Bridge, but we'll be concerned with Fort Point, which was right there tat the same location.

Now this line on the left is the San Andreas Fault, one of the greatest known faults in the whole world. The San Francisco earthquake, rated 8.3 in those days and I think it's still about there for surface wave magnitude, is supposed to have had it's greatest rupture up here in Marin County, but actually it ruptured for over 200 miles as I recall.

And if we did the same thing that we do for designing any plant, we have to take the nearest distance to a moving fault or a rupture plane, so we're now going to look at what happens to the buildings in San Francisco.

I've made a personal study for many years of the 52 largest buildings in San Francisco at that time. Now I wasn't there at that time, I want that to be clear, abundantly clear.

The next slide, please?

(Slide.)

I have all sorts of records and things.

Here we have the Fairmont Hotel, the same one that's there today, after it had been burned out, after the fire, after the earthquake. You'll see all of these other buildings in the surrounding lots are completely burned out and the debris hauled away. But they rebuilt the floors and

agb6

so on of the old Fairmont in the original shell, the original walls.

That building, as for every building that I'm going to show you briefly today, was not designed for any earthquake value at all. Most of them accidentally had some value due to either good engineering judgment or to designing for wind.

But none of them today, according to calculations would pass even an ordinary building code, let alone a nuclear code like Diablo Canyon which is perhaps 20 times greater than an ordinary code, 10 to 20 times.

Next slide, please.

(Slide.)

This next slide is -- shows the Dewey monument still standing in Union Square. But the main thing I wanted to show in this old photograph was the Saint Francis Hotel.

This part was under construction on the right prior to the earthquake and this part was the existing hotel.

Next slide, please.

(Slide.)

You'll see the same buildings there today. Here's the part that was under construction, it's still there, and then here's the part that was already there and the Dewey monument is still there. This is the new edition of the Saint Francis Hotel in the background and this is a modern

agb7

photograph.

Next slide, please.

3C2 wbl

(Slide)

You see here the tallest building in SanFrancisco burning after the earthquake. These are flames coming out the windows. This is a 19-storey building called the Claus Spreckels Building. It's at Third and Market Street. It had some diagonal steel members, some of which failed. But the building itself stood up very well.

Next one, please.

(Slide)

We have here a view of the Palace Hotel on fire together with, I believe it is the Grand Hotel across the street also on fire. You'll notice that these buildings are standing. They're not ruins at all until the fire burned them out.

Next, please.

(Slide)

A view of the Palace Hotel after it was completely burned out. It, too, had wooden floors. The walls were merely brick. But the damage that you see in the foreground is due to the wrecking, the deliberate wrecking -- by my father, incidentally: you'ld see his name on this job sign if you could read it. --deliberately wrecking this building and getting ready to pull down the old Palace Hotel and build a new one.

Next, please.

wb2

(Slide)

This is the Monadnock Building in fairly good shapes. It had some cracks, but it was not destroyed. And this is the Palace Hotel which, as I said before, was completely burned out.

Next slide, please.

(Slide)

A modern day photo of the Post Office at Seventh and Mission. This building is on soft ground. It had very, very severe shaking, and the estimated damage was less than 10 percent of the cost of the building.

Next, please.

(Slide)

A view of Market Street, a modern view. This building that I'm pointing to, the tallest one in the picture, is the old Claus Spreckels Building with a new architectural face. About 15 or 20 years they simply put a new architectural face on it, and that building is still in use today.

Next, please.

(Slide)

The Rialto Building at Montgomery and Mission Streets. You'll notice this brick face building which has a steel frame is practically all windows and glass. You would call this a building that wouldn't have a chance of

wb3

passing any code at all in existence today. The damage was about 15 or 20 percent.

Next slide, please.

(Slide)

This I believe is the Cole Building on Montgomery Street, a big massive building with bearing walls. This picture was taken a few years ago. It had some damage, but it was put back in service.

Of the fifty-two total buildings that I studied -- and they were all of them all the big ones -- only seven failed to go back in service. And, of those seven, four were burned out completely by fire, one had terrible construction, and the other two were a combination of fire and earthquake failures.

Next slide, please.

(Slide)

A view of Market Street during the BART construction, the underground railway. I point this out to show the Flood Building on the left and the Emporium Building on the right, both of which went through the earthquake as I will show following.

Next slide, please.

(Slide)

There's the Flood Building today. We found the damage to be quite nominal. Or, rather, we didn't find it; we

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found it in the records. But what we have done is to make calculations of the value of this building according to modern methods, and we would figure for about 2 percent of gravity. And it stood that earthquake.

Next slide, please.

(Slide)

The Emporium, still there in business. This big building was also damaged slightly but put back into service.

Next slide.

(Slide)

The Hibernia Bank, another type of building. The dome had some damage and the walls were cracked a little. But there it is.

Next slide.

(Slide)

The old Mint Building at Fifth and Mission Streets. Today they've taken off the parapet wall to make it safer for pedestrians on the street. But the building went through the earthquake.

Next one, please.

(Slide)

I'm not showing this to show the Golden Gate Bridge but to show you Fort Point under the Golden Gate Bridge. That's the brick building that you can see on the

wb5

lower left side of this picture.

Next slide.

(Slide)

A view of the Fort Point inside, showing one of the old cannons and these walls which are heavy brick walls, of course, but not nearly as heavy as the concrete walls at Diablo Canyon. And this structure was practically undamaged. As I recall, only one wall had a crack in it.

Next slide, please.

(Slide)

Another view of Fort Point.

Fort Point, if you remember the map I showed, is only four or five miles from the moving fault of an 8.3 earthquake.

Slide, please.

(Slide)

This is just a general view of San Francisco pointing out two things. One is the fact that nested amongst all these modern buildings you have the old ones that went through 1906.

The second point is that of all the buildings you see here the design coefficient of base shear is roughly in the order of 4, 5, 6 percent and, in a few cases, may go to 8 percent. And we're talking 75 percent for Diablo Canyon for a smaller fault and a smaller earthquake than this city

wb6

was subjected toand will get again some day.

The lights, please.

MR. TOURTELLOTTE: Mrs. Bowers, for the record, because I'm not certain that in every case the slides were identified, I think it would be important to have it established that they were discussed in the order in which they were numbered.

Is that correct?

MR. NORTON: Yes. They were shown and discussed in the same order in which we numbered them earlier.

At this time we would move that those photographs be moved into evidence as Applicant's exhibits as marked.

MRS. BOWERS: Mr. Kristovich?

MR. KRISTOVICH: No objection.

MRS. BOWERS: Mr. Tourtellotte?

MR. TOURTELLOTTE: No objection.

MRS. BOWERS: Well the slides that have been identified by exhibit numbers will be admitted into evidence.

MR. NORTON: For the record, that's Applicant's Exhibits 9 through 29.

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(Whereupon the documents referred to, heretofore marked for identification as Applicant's Exhibits 9 through 29, were received in evidence.)

3d

WITNESS BLUME: The second major earthquake that we used as a model, and this is discussed at great length in some of the documents on file, was the Nicaragua earthquake where the Esso oil refinery complext at Minagua in 1972 was shaken very, very severely by an earthquake that killed 10,000 people.

It so happened that a record was made right at the oil refinery plant so we know exactly what the ground motion was by a reliable instrument. It was in the order of .39 and .34g horizontal and .33 vertical. The plant kept operating. They had to shut it down artificially in order to get -- to make an inspection for safety purposes.

The plant design, unlike the San Francisco buildings that I just showed, varied depending upon when each structure was erected, but in general, we estimated, after very close study, that the average value of the plant structures wouldn't be over about 10 percent of gravity. And yet the damage was so minor that as I said, the plant kept operating.

There were a few stretched anchor bolts and a few cracks in concrete foundations, but it was amazing that nothing went out. This is explained in detail in some of our reports.

Another one explained in great detail is the Huachipato steel plant in Chili which, in 1960, was subjected

eb2

to a 7.5 magnitude earthquake on what we think is an extension of a fault very similar to what we're talking about only in the sense that it was probably very close offshore.

I haven't the time today to go into the details of this plant, but I will summarize by saying that it shut down for six days after undergoing this tremendous earthquake. The cost of repair was .4 of one percent of the original cost. It consisted of many high, narrow vessels, chimneys and stacks and ovens and all kinds of things which we have analyzed in great detail.

And backing into the value-- We can often do this. When there aren't actual records taken of ground motion we have used structures as instruments and computed what the ground motion might have been by knowing how the structures reacted.

So again we find that we have a case where the motion was apparently fairly severe and the damage was not at all catastrophic. There were many errors made in their design. They neglected buckling characteristics and they neglected dynamics of structures but nevertheless the plant only was down for six days.

I think on the same subject we'll show the caretaker's house now.

(Slide.)

We're looking at Figure 3 from the written

eb3

testimony, and I think everybody has to take a look at this because you've all heard how hard Pacoima Dam shook, 1.2g measured motion. Well, this is the caretaker's house at the mouth of Pacoima Canyon, very close to the dam where the motion was measured, in fact it says here "within one-half mile of the dam and within one mile of the VA Hospital" which was thoroughly destroyed and killed over 50 people.

And yet we have here an old-fashioned brick chimney which was undamaged and neither was the structure damaged.

So there are these things that I think prove, and we can find many more of them, that damage is not always what it's cracked up to be.

(Laughter.)

Modern buildings are designed, as I mentioned, roughly to .05 to .10 base shear coefficients and even though in this short summary I can't go into the differences between base shear coefficients and ground acceleration effective or instrumental -- I would hope to later -- the point is that for simple buildings you can make a fair comparison to sway.

If you go to the regulated structures such as schools and hospitals in California which are considered a threat due to what happened to them -- 1933 school buildings were damaged and then in 1971, the hospitals were

eb4

damaged, so they have enacted regulations which approximately double the values I gave to about .10 to .20.

Now if we make the approximation that these are the same, these base shear coefficients are roughly the same as acceleration for a simple structure, we find that even the most modern structures, the most vulnerable, the most important are designed to a fraction of what we're talking about at Diablo Canyon, and they are closer to larger faults, potentially more active faults with more strain, and they are surrounded with dense population like San Francisco and Los Angeles.

There are all sorts of unrecognized safety margins in this picture. All I can do today is to list a few.

The test data that I made on the strength of materials are always done in a manner --

(Slide.)

This is Figure 4 from the written testimony. It is merely taken as an example. Pay no attention to these equations or what they represent.

All I can say is this is a test, a laboratory test type procedure of certain values of concrete. It could be steel; it could be wood; it could be anything else. The principle is the same.

What is done is to get test values shown by these little dots, and then they draw a line that either comes

eb5

underneath all of them or perhaps comes under 95 percent of them, and they say All right, that's our value.

Then they proceed to take that lower line value and apply safety factors to it.

So there's a safety margin to start with, the way that is done.

(Slide.)

Another unrecognized safety margin lies in the fact that we have to consider both horizontal components as being equal and apply them in the analysis of a structure.

(Slide.)

This is Figure 5 we're now looking at from the testimony, and in this figure we see the ratio of the average horizontal peak ground acceleration -- pardon me. It should be the ratio of the maximum to the average horizontal peak ground acceleration as a function of distance from the source.

I obtained this information from literally thousands and thousands of records taken-- No, pardon me. This is real earthquake. I thought I had underground nuclear. Hundreds of records taken all over California and western Nevada over this time period, '54 to 1970, and averaging out we get these points. And we find that the maximum is always greater than the average peak ground acceleration, and this increases with distance.

But if we take a very short distance such as 10

eb6

kilometers, which we would have for Diablo Canyon, roughly, you'll see that we have at least 13 percent, meaning that when we are forced to take the peak horizontal values and use it in design in both directions, we are building in a conservatism that nature doesn't have.

(Slide.)

Another conservatism is shown in this Figure 6 from the testimony.

I've assumed here a steady-state response of an oscillating system. By "steady state" I mean that there's a mechanical vibration being applied to it, a harmonic forcing function, so that it builds up what we call resonance or the maximum possible motion which could represent the perfect tuning of the shaking of the natural period at the ratio one, and we would get maximum.

This assumes, however, that two things are constant. The one is the shaking frequency and the other is the natural frequency of the object being shaken. Well, neither case is true with the real earthquake. The earthquake motion jumps around. It's not a constant period. And the period of the structure varies slightly, even in the elastic range.

We know this from various observations that have been made, that concrete is not strictly linear even at very low stresses, and there are period changes.

eb7

So the minute you slip off from perfect resonance either way off the peak you will find enormous reductions in response.

WRB/mpb1

I think in order to save time I'm going to simply sum up my statements on the various unrecognized safety factors and say that there are many of them. I've listed 22 in a paper that is in some of the filed documents, unrecognized safety margins and safety factors that can multiply out when they occur together, as they often do, to provide actual strength of a modern engineered structure that is far times greater than given credit for, maybe several times greater.

I have sort of made it a personal crusade to get some of these things corrected. I've written two or three papers on it. But it will take a long time.

Now I'm not criticizing the NRC at all; I'm criticizing the whole profession for applying extreme disturbing forces, such as earthquakes and wind and so on, and not being realistic about the values they have to resist these things.

(Slide.)

The plot we're looking at, Figure 7 from the written testimony, is another subject all together and I'll touch upon it briefly. Since this is a summary, we have made three independent probabilistic studies of peak ground motion occurring at the site under the assumption that there will be a 7.5 maximum earthquake, and under various other assumptions, the procedures shown here for procedure one was

mpb2

from one of our reports. I think that was Report DLL-11, if I remember correctly. Procedure two was divided into two elements. This is from LL-45, D-LL-45.

In the report LL-45 we took two time periods. One was 20 million years, and the other was 10,000 years. I obtained data from Mr. Hamilton that gave the slip rates on all of the faults in the region over those time periods, and we proceeded to utilize that in a very complex probabilistic study that cannot be described today, but can be later if need be. And we came up with the answers you see here.

Then there was another procedure followed -- pardon me, that was in Report 41D -- LL-41, those two time periods.

And then in a third procedure, number three, we had the plate tectonic boundary assumption which was mentioned by Dr. Smith, I believe, where he obtained data for the entire State of California. We assumed it belonged to the tectonic plate and then proceeded to distribute it properly to the Diablo Canyon site.

To sum up, we find that for 1.15g, which would be right about here, we can come down through here, and you get the different answers. And they amount to return periods ranging from roughly 10^5 to 10^6. In other words, 100,000 to maybe a million years average return period, depending on which assumptions were made in the procedure.

mpb3

Now independently Ang and Newmark did another report for NRC, and they came up with results in the order of -- which straddled ours, if I recall, and I don't remember their exact numbers, but I do know that they had two extremes which straddled one of our reports, and we were quite pleased with that.

Summing up, we feel that the average return period in very round numbers for the 1.15g at the site providing and assuming that 7.5M could occur, which is one of the prime assumptions in this, is about 100,000 years.

Now even if that happens -- and there is a big "if" there -- this is not failure at all. All that means is that you just get up to the design level and start to test your design hypotheses and your design materials. Beyond that point there is this great inelastic world that I mentioned, and this value of all of these unrecognized safety factors.

So what I'm really saying is that based upon these various studies on an average of once every 100,000 years if you could support a 7.5M you might begin to test the structure.

I'll now simply show you some of the design values that are used.

(Slide.)

Here we have the case where tau is equal to zero, and you'll hear more about tau. That has to do with

mpb4

soil-structure interaction and the reduction in the high frequency range for the size of the foundations. But in this case there is no reduction. And these are the spectra for various damping used for the various small structures coming into .75, the effective acceleration.

Next slide, please?

(Slide.)

The same thing with tau equals zero for the Newmark curves.

Next slide, please?

(Slide.)

This is part of page 43 from the written testimony which compares some of the tau factors that were used. We have here the so-called Blums criteria and the so-called Newmark criteria. The peak ground accelerations after reductions for tau for the containment became .67 in our case and .60 for the Newmark case. For the auxiliary building it became .63 against .55. For the turbine building, .54 against .50. And for other buildings which you just saw they are both .75.

In other words, even with this tau reduction procedure the Newmark method is slightly different than our method, again done completely independently.

Next slide, please?

(Slide.)

mpb5

This shows figure number 10 from the testimony brought into -- no, this is the vertical spectra. Figure 10 shows the vertical spectra which is simply two-thirds of the horizontal, which has been pretty much of a standard practice throughout the country.

Next slide, please?

(Slide.)

That's the Newmark set for two-thirds for the vertical.

The next one?

(Slide.)

Here we have a tau situation. This is our curve for the containment and the intake structures, which happen to have the same tau value. The tau value is based upon the length or size of the foundation and the shear value, the shear velocity of the material at the site, which was assumed at 3,750 feet per second for this purpose.

What this amounts to, as you can see, in the high frequency range these start to bend down and come in about .67 as compared to coming in at .75 where a tau is not used.

Tau is an expression which simply refers to the time it takes for a wave to pass the length of the foundation.

Next slide?

(Slide.)

mpb6

That's the Newmark set for tau equals .04. It comes in at .60 here.

Next one?

(Slide.)

Tau is .052 used for the auxiliary building. It comes in to .63 in our case.

In our case there's no effect beyond about .4 second. Out here there's no change whatsoever, but less than .4 second. They do peel in a little lower and come down to this value.

By the way, this is Figure 14 I'm talking about.

Next one?

(Slide.)

I don't see the figure number, but it's for the auxiliary building, the Newmark set. It comes in to .55.

I don't think we'll have to go through the rest of those in view of the time element. But the point is that for every type of structure we have complete sets of design spectra. We have damping values that were agreed to. And a complete analysis has been conducted for both the Newmark and the Blume criteria for all the structures.

Another panel, the one to follow, will get into the details of how the analyses were done and what the specific results were. In many cases there had to be remedial measures, some of them very costly, in order to bring the structures up

mpb7

to the standard shown.

I would like to, in view of the time -- is my time about up?

Q Yes.

A Shall I summarize here?

Q I think, Dr. Blume, we're going to have some more direct when we get this panel back on. In other words, I will have some questions for you. And if you want to briefly summarize now, fine. But we can also do it when this panel comes back, in which case you will have had an opportunity to read the transcript and perhaps could give us a five minute capsule at that time to get everybody back aboard, because that will be a few days down the road. And it might be a good idea to do it that way.

A Well, in view of the hour, maybe that's the best way to go.

Q All right.

MR. NORTON: Mrs. Bowers, at this time we'd like to ask that this panel be released until Wednesday morning at 8:30.

It's my understanding that Dr. Silver and Dr. Graham will be here Monday morning. Dr. Cornell is from MIT, of course, and he has some business to attend to. And rather than have them on a floating schedule where maybe they go on Tuesday afternoon and maybe not, we would rather specifically

mpb8

bring them back at 8:30 Wednesday. And I suspect it will take most of two days with Dr. Silver and Dr. Graham in any event, and it could even take longer, I suppose. But I don't really see any real need for these people to wait around for the next three days.

MRS. BOWERS: Mr. Kristovich, any objection?

MR. KRISTOVICH: No objection.

MRS. BOWERS: Mr. Tourtellotte?

MR. TOURTELLOTTE: No.

MRS. BOWERS: Well, then, we'll recess this panel of witnesses until 8:30 Wednesday morning.

MR. NORTON: If the Silver-Graham cross is done. If not....

MRS. BOWERS: Fine.

(The panel temporarily excused.)

MRS. BOWERS: Let me check with the parties:

Is there any reason for us to -- any unfinished matters?

MR. NORTON: Yes, there is one thing.

We might as well put Dr. Blume's testimony in today's transcript as though read, if there is no objection, rather than put it in at the beginning of next week's testimony.

MR. KRISTOVICH: No objection.

MR. TOURTELLOTTE: No objection.

TESTIMONY OF
JOHN A. BLUME
ON BEHALF OF
PACIFIC GAS AND ELECTRIC COMPANY
DECEMBER 4, 1978
DOCKET NOS. 50-275, 50-323

My name is John A. Blume. My qualifications are set forth in Exhibit 7.

My testimony is about the criteria used to evaluate the Diablo Canyon plant for the postulated Hosgri 7.5M earthquake and how those criteria including the response spectra were developed.

Basic Terms And Definitions

Before proceeding with my testimony on specific points, it may be desirable to discuss some of the basic terms that will be used repeatedly in these proceedings. Although many of these may be familiar to all interested parties, there should not be differences in definitions or interpretations that could lead to misunderstanding.

When the ground moves resulting from an earthquake, there is acceleration, velocity, and displacement, as for the movement of anything else such as an automoble. An accelerating automoble is increasing its velocity (miles per hour) and also moving a distance (miles) which in dynamics we call displacement. Earthquake motion, unlike the automobile motion, reverses back and forth many times during the time duration of a strong earthquake. Thus the acceleration,

velocity, and displacement not only vary with time but have opposite directions. Records are made of ground motion using instruments and recording systems which delineate the actual motion of the ground and how it varies with time. These records are called time histories. Acceleration is often measured. The maximum or peak acceleration, whether moving in one direction or the other, during the entire record of strong motion is called the absolute peak instrumental acceleration, or often simply instrumental acceleration.

It has become more or less traditional for earth scientists and many engineers to consider the peak acceleration of an earthquake at a given location. There is often confusion, however, between the peak acceleration as measured by the instrument, "instrumental acceleration" and the acceleration value that might be used in developing the criteria for the analysis or design of a plant which hereinafter is designated as "effective acceleration." The peak instrumental acceleration, which usually represents an extremely short duration spike or pulse on a time history, need not be used directly for design purposes. The reasons for this are many and will be discussed subsequently.

The effective acceleration used as the basis for the evaluation of the Diablo Canyon plant for the hypothetical 7.5M earthquake on the Hosgri fault is 0.75g. However, the

peak instrumental acceleration from which that value was derived is 1.15g. This is an important point.

Records of strong motion or time histories of motion may have duration of from a few seconds for small events, or for large events at great distances, to as much as a minute or so for nearby great events. These records usually require some minor corrections for instrumental characteristics and other matters after which they are carefully digitized and processed.

The term natural period or natural period of vibration will be frequently used. The natural period is the time required for an oscillating body to move from any given point away from and back again to that same starting point. A pendulum for example may swing from the highest point on the left side to the highest point on the right side and back again to the highest point on the left side. The time required to do this is its natural period, usually given in seconds. Structures, equipment, piping systems, etc., have natural periods of vibration including not only a fundamental or basic mode but various other modes. These periods are considered constant in the elastic state for all small amplitudes of motion. The natural frequency of vibration is simply the reciprocal of the period, and is given in cycles per second, now termed hertz.

Damping is related to the energy change during vibration and it varies for different materials and structures.

Energy is never lost but it changes form. The kinetic energy of motion of a vibrating body or system is reduced by energy converted to heat through friction and the internal stressing of materials, and by other means. The rate or degree of this loss of kinetic energy is called damping. If there were no damping at all an oscillating system would never stop. If the system were critically damped it would not oscillate and, upon being displaced, it would simply return to its static position. Although damping is a very complex subject and has many forms, in earthquake analyses viscous damping is generally assumed and it is given as a ratio to or percentage of critical damping, which in turn is that damping value which could just prevent oscillation.

As materials are loaded they deform. For example, a steel bar anchored at one end and subjected to an applied pull, or tension, at the other end,, lengthens. The applied tension or force creates stress or force per unit area of cross section of the bar. The lengthening or deformation creates strain, or deformation per unit of length.

The elastic state of stress is that in which the strain or deformation is or may be considered as directly proportional to the stress or the loading.

The inelastic state or the ductile range is that range of stress or loading beyond the elastic state or beyond the yield point, wherein strain or deformation increases more rapidly than stress or loading. The properties

in the inelastic state may range from brittle to extremely ductile depending upon the materials and how they are used.

The response spectrum is an extremely important concept in the analysis and design of nuclear power plants for earthquake motion and will be referred to repeatedly. If a complete time history of motion is used as the disturbance input, it is possible to calculate the maximum response of a simple one-degree-of-freedom elastic, damped oscillator when subjected to the entire time history of motion. Such a simple oscillator might be represented by a single rigid mass on a vertical stick having stiffness but no weight, or a "lollipop" shape. The results of such a calculation would produce only one point for a response spectrum curve and that point would be for the natural period of vibration of this particular oscillator with its particular damping ratio. If a whole series of oscillators of the same damping are subjected one at a time to the same ground motion record, and if each oscillator has a different natural period, there would be a whole series of points for a plot of spectral acceleration versus period such as shown in Figure A. Connecting these points would provide a "response spectrum" for the particular ground motion record and for the particular damping of the oscillator. If the same procedure were repeated using oscillators with other damping values, a whole family of spectral curves would be obtained for the particular strong motion record. Figure B represents a set

of such spectral curves for the 1940 Imperial Valley earthquake recorded at El Centro, California. Of course these extensive calculations are done in computers.

Most acceleration response spectra made from an earthquake record are rather jagged with many peaks and valleys. It is customary to obtain smooth curves for use in analysis and design in order to avoid the problems associated with these peaks and valleys and to avoid sensitivity in response caused by minor variations in natural period. There are various ways this "smoothing" can be done. One simple way is to draw the smooth curve through the jagged one either by averaging the peaks and valleys or, as is more often done, to almost envelope the peaks. A better way is to not rely upon one ground motion time history but to use several appropriate records representing as near as possible the conditions under consideration. This results in a whole series of response spectra for each damping value which series can then be treated statistically by various methods to obtain an average curve for all the records used as well as other curves representing any statistical deviation from the average that may be desired. This procedure has the advantage of not only providing a broader base of information but of providing probabilistic distributions at any period value or statistical confidence level of interest.

Response spectra can also be constructed artifically, or they can be obtained from standards like NRC Regulatory

Guide 1.60, or from ratios of spectral values to either ground acceleration, velocity or displacement, depending upon the period or frequency under consideration. A most convenient procedure is to consider the dynamic amplification factor, DAF, as the ratio of the spectral response at any given period, damping, and statistical confidence level to the effective acceleration. It so happens that effective acceleration used to construct spectral curves is the same as spectral response at any damping value at zero period or infinite frequency. Effective acceleration is therefore sometimes referred to as zero period acceleration or anchor point acceleration. Using the DAF factor for any desired confidence level one can readily adjust spectral curves to any specified effective acceleration. This is sometimes referred to as scaling the acceleration value.

Response spectra may be in units of acceleration, velocity, or displacement, each of which may be plotted against period or frequency and on linear or log scales. In addition, a useful device is a 4-way log paper on which one can read spectral acceleration, velocity, and displacement plotted against period or frequency on one diagram. An example is Figure C.

It is often convenient in analysis to use a time history instead of a response spectrum. However, as discussed previously, time histories produce spectra with peaks and valleys. To overcome this problem a time history is selected

to best represent the conditions of the problem and it is then artifically altered, usually with additions of pulses of proper sizes and at strategic locations in the time domain to cause the spectrum made from the <u>modified time history</u> to closely match the prescribed spectral diagram. This work has to be carefully done and, of course, with computer aid.

<u>An Overview Of Diablo Canyon Earthquake Criteria</u>

It seems desirable to provide in somewhat more detail than has been covered thus far, the earthquake design criteria for the Diablo Canyon plant. They may be derived into three phrases or sets as follows:

1. The original (pre-Hosgri) criteria
2. The initial Hosgri criteria
3. The current criteria employed for the Hosgri reanalysis

The first set is self-explanatory. It was developed before the Hosgri fault was found and was used as the basis of the construction permit and much of the actual construction.

The second set was developed by PGandE and its consultants immediately after the Hosgri became known to them. It was replaced before adoption, however, by the current set of criteria. I am still of the opinion that the second set is adequate for the conditions as they are now known and would provide reasonable assurances that the plant

can be operated without undue risk to the health and safety of the public.

The third set, the current Hosgri criteria, is based upon 7.5M on the Hosgri with 1.15g peak instrumental acceleration as proposed to NRC by the U.S. Geological Survey, and with 0.75g effective acceleration with some high frequency reductions for the soil-structure interaction effects on large foundations.

<u>Original Criteria For The Diablo Canyon Plant</u>

Various studies were made to obtain data for the establishment of the original Diablo Canyon seismic design criteria. Dr. Richard Jahns and Mr. E.C. Marliave conducted extensive geologic studies, and Drs. Hugo Benioff and Stewart Smith conducted detailed seismological studies. From this intensive work four basic earthquake faults or earthquake sources were determined as follows:

A. The San Andreas fault; 48 miles away, maximum M = 8.5

B. The Nacimiento fault; 20 miles away, maximum M = 7.25

C. The Santa Ynez Fault extended; 50 miles away, maximum M = 7.5

D. Local earthquake, unassociated with any known fault, with the focus 12 miles away in any direction including down; maximum M = 6.75

At the time there was no knowledge of the underwater Hosgri fault.

Given the above, it was determined by our firm, URS/Blume Engineers, that of the four postulated earthquake conditions two of them, namely conditions B and D, controlled the design criteria. Condition D -- having a hypothetical earthquake not associated with any known fault and with a hypocenter taken as only 20 km (12 miles) from the plant in any direction including straight down -- was unique and this 6.75 magnitude earthquake so close to the plant definitely controlled the criteria for the high frequency range of the response spectrum. I decided to use two earthquake records as the basis for the shape of our design spectra. The Taft 1952 earthquake, N69°W record was used as a model for earthquake B, and the 1957 Golden Gate Park S80°E record taken on rock was used as the basis for earthquake D. The Taft record was for M = 7.7 recorded about 42 km away from the epicenter and the S.F. Golden Gate Park record was for M = 5.3, about 8 km from the epicenter.

The zero period acceleration or the effective acceleration for these earthquakes was taken as 0.15g and 0.20g for earthquakes B and D, respectively. Thus, two response spectra were developed for each of the damping values under consideration and the operating design criterion was to use whichever of these two curves governed. This first two-earthquake spectrum was necessary in view of the earth scientists' report on the non-fault-associated earthquake D. In many respects the hypothetical earthquake D

anticipated a Hosgri-like earthquake. Figure 1 shows these operating basis design curves for 2% damping based on earthquakes B and D. Two times these accelerations were used for the safe shutdown condition, then termed the "double design earthquake," DDE.

The acceptable damping ratios used at the time were in most cases much smaller than those found to be proper today by NRC criteria and common usage. Low damping values lead to high computed response and more material in design. The plant built to those criteria has far greater strength to resist earthquake demands than such design criteria would indicate. The damping ratios used in the original design were as follows, shown as percent of critical damping:

Vital Piping	0.5% except the
Primary loop, which was	1.0%
Welded steel structures	1.0%
Bolted steel structures	2.0%
Concrete structures	5.0%

It is to be noted that the peak effective accelerations for the original plant were 0.20g for the Operating Basis Earthquake and 0.40g for the Double Design Earthquake or the DDE. These values spring from the unassociated earthquake D.

The Initial Hosgri Criteria

When the Hosgri fault zone was discovered offshore from the plant, the same team of earth scientists (with the exception of Dr. Benioff and Mr. Marliave who were then deceased) plus Earth Science Associates studied the Hosgri information in great detail, and the conclusion was reached that the maximum magnitude the Hosgri could support based upon all the evidence, was in the range of 6.25 to 6.5. The normal distance to the Hosgri fault was about 6 km. The depth to the hypocenter was conservatively taken as 5 km. Thus, the minimum hypocentral distance was about 8 km. This was at the time designated as earthquake "E."

In view of these conditions and the rock site, I considered all the available larger magnitude, close-in strong motion records then recorded on rock, as a logical basis for modeling the Diablo Canyon-Hosgri situation. Ten such records were considered including the Koyna Dam records. Subsequently, however, it was learned that the Koyna Dam records had been altered in some manner and they were rejected for that reason. Using the 8 remaining records as outlined below, response spectra were made for each from the normalized time histories. These results were scaled to 0.50g which, after much study and analysis in accordance with modern attentuation procedures, was considered by PGandE and its consultants to be a conservative peak acceleration to be

used for the given magnitude, distance, and site conditions so as to provide criteria for reasonable assurance of safety.

There are today many procedures and equations used for developing site acceleration for a given earthquake magnitude location. As part of the intensive work done in the study of the Diablo Canyon plant, the Blume "SAM" procedures were updated with all available earthquake data for California and Western Nevada, and also in view of all other new information available regarding the relative motion on rock and soil. The updated procedures were: SAM IV for magnitudes of 6.5 or less, and SAM V for magnitudes greater than 6-1/2. This SAM procedure has been described in a paper given at the Sixth World Conference on Earthquake Engineering in January 1977. The use of the procedure is described in report D-LL 11, and the entire World Conference paper is included as Appendix D-11B of that report. It is my opinion that this is the most appropriate method available today for conditions such as at the Diablo Canyon site.

The magnitudes for the 8 earthquake records used were the greatest recorded to date on rock stations and range from 5.3 to 6.6. The conditions are quite representative of the 6.5M earthquake close to a rock site such as being considered for the Diablo Canyon site. The Pacoima Dam record was used without modification for the response of the rock ridge and the adjacent dam. The following table provides data about the 8 records used.

Earthquake	M	Depth (km)	Recorded at	Epicentral distance (km)	Component	Peak Acceleration (g)
Helena 1935	6	5	Helena	3 to 8	EW	0.16
Helena 1935	6	5	Helena	3 to 8	NS	0.13
Daly City 1957	5.3	9	Golden Gate Park	8	N80W	0.13
Daly City 1957	5.3	9	Golden Gate Park	8	N10E	0.11
Parkfield 1966	5.6	7	Temblor 2	7	S25W	0.33
Parkfield 1966	5.6	7	Temblor 2	7	N65W	0.28
San Fernando 1971	6.6	13	Pacoima Dam	3	S14W	1.17
San Fernando 1971	6.6	13	Pacoima Dam	3	N76W	1.08

The 8 response spectra were divided into period bands and each period band treated statistically in order to obtain its mean value and its standard deviation. As a final step, smooth curves were drawn through the points representing the various period bands for the peak effective acceleration of 0.5g and for a confidence level on the curve shape of about 80 to 90%. The resulting curve for earthquake "E" is shown on Figure 2 together with the prior DDE curve governed by earthquakes B and D. The 5% curves are drawn for comparison purposes only.

However, damping values had been re-evaluated in the period between the original design and the discovery of the Hosgri. While 5% was actually used for structures in design, 7% was later considered proper for the DDE or SSE

(1975) and was in fact in NRC Regulatory Guide 1.61. If the curve E in Figure 2 is lowered to the 7% value (shown by the dashed line), the 7% curve E exceeds the B-D curve only where there is no important structure or system and/or where more strength was provided than required by B and D.

The new damping values in Regulatory Guide 1.61 to be used with earthquake "E" for the safe shutdown condition were:

Equipment and large diameter pipe	3% of critical
Small diameter pipe (≤ 12 in.)	2% of critical
Welded steel structures	4% of critical
Bolted steel structures	7% of critical
Reinforced concrete structures	7% of critical

It was found that the plant qualified for these E criteria without any physical modifications. However, more stringent earthquake criteria were then suggested by the NRC staff upon advice from the U.S. Geological Survey (USGS).

The Earthquake Criteria Employed For The Hosgri Reanalysis

The USGS recommended that the NRC postulate a Magnitude 7.5 earthquake on the Hosgri fault and consider the ground motion for near-site events, as set forth in USGS Circular 672 for derivation of an effective engineering acceleration. Dr. Newmark, a consultant to NRC, proposed that the peak instrumental acceleration for a 7.5M earthquake

of 1.15g in USGS Circular 672 be assigned an effective acceleration for the purpose of developing response spectra of 0.75g with certain reductions for large foundations. Although I remained, and do remain, of the opinion that the initial Hosgri criteria of 6.5M and 0.50g effective ground acceleration were more than adequate for the Hosgri exposure, PGandE agreed to reevaluate the plant on the basis of a hypothetical 0.75g peak effective acceleration together with the spectral modifications in the high frequency range for the averaging or filtering effects of the large, massive foundations on the high frequency ground waves. These high frequency adjustments were termed Tau factors, and will be discussed in more detail subsequently.

Comparison Of Design Criteria

The following table shows data proposed and utilized in the design and in the re-evaluation of the Diablo Canyon plant for the Hosgri conditions:

	Original design criteria	Initial Hosgri criteria	Current Hosgri criteria
Governing fault or earthquake	Eq. "D"	Hosgri, "E"	Hosgri
M_{max}	6-3/4	6-1/2	7-1/2
Min. hypocentral distance, km	20	8	8
Peak instrumental acceleration	---	---	1.15g
Peak effective acceleration	0.40g	0.50g	0.75g*
Structure damping ratio	5%	7%	7%
Peak S_a for above ratio	1.5g	1.3g	1.8g*

Note: The spectral shapes are different for the above criteria and therefore proportionate values cannot be used; see response spectra.

* Reduced in some cases at high frequencies for Tau factor.

Figure D shows spectral curves as follows: the original 5% damped DDE curve, the 7% damped "E" curve, and the current 7% damped curves (Blume and Newmark) for the M = 7-1/2 earthquake with no "tau" reduction factor. There are other current curves which will be covered subsequently along with tau factors which provide allowances for the mitigating effect of large foundations.

CONTENTION 3

Contention 3 is that the 0.75g acceleration for the safe shutdown earthquake is not an appropriate value. I concur --it is too conservative! I make this statement on three counts: (1) the 7.5 magnitude is conservative in my opinion; (2) the 0.75g is conservative even for the 7.5M; and (3) there are a host of other conservatisms or

unrecognized safety margins in the analysis procedures that are overlooked in the current design and review process; i.e., there are safety factors piled one upon the other leading to probabilities of failure that approach the vanishing point. Normal building codes require design forces equivalent to only a fraction of 0.75g for even greater earthquakes and for very dense populations. The earthquake disasters and great loss of life around the world are from buildings that wouldn't stand 0.075g; in fact, most have no code requirements or engineering design whatsoever.

It is not to be overlooked that the peak instrumental acceleration associated with the 7-1/2 magnitude is not 0.75g but 1.15g. Thus, the contention itself may be misleading -- the maximum vibratory acceleration assigned to the Diablo Canyon site is 1.15g, not the 0.75g stated.

It seems desirable to note at this point that peak acceleration per se is not the sole criterion for analysis or design of this or any other plant. There are many other parameters that are equally or more important as, for example, spectral response acceleration, damping, allowable stresses, ductility, etc. In fact, it is possible to omit peak ground acceleration and go directly to spectral response. However, the inclusion of peak acceleration has become a traditional approach and one that expecially appeals to earth scientists and others who record ground motion with instruments. I

emphasize that the structure "feels" response acceleration and not peak ground acceleration.

There are various procedures in getting from a given instrumental acceleration to an effective acceleration. The number of cycles of peak motion are sometimes considered and several of the highest "spikes" on the record are discounted as having no structural significance. Observation and judgment must enter this process because the measured data are sparse in some areas. We have made studies which show that extensive clipping of peaks from the time history records has only a minor effect on peaks of response spectra which are the real indicators of structural performance. (Report D-LL30.) Likewise, time history peaks can be augmented with similar results. If an acceleration peak or spike has very short duration, the energy involved is small. This can be visualized in view of the fact that the time integral of acceleration is velocity, and the kinetic energy of motion is velocity dependent. Random spikes that lack periodicity and have short duration are apparently not effective in dynamic amplification nor therefore in structural response.

Instrumental Versus Effective Acceleration

It gradually became clear as reliable strong motion records were obtained that peak instrumental ground acceleration, even for moderate earthquakes, was considerably greater than the base shear coefficient values of buildings

that had survived even much stronger earthquakes. The difference was so great that it could not be reconciled with typical safety factors or the elastic dynamics of the problem. The definition of this problem and its extension to response spectra, together with the first attempt to reconcile recorded motions with building performance, was by Blume (1958). It was shown that (a) earthquakes were stronger than they had been given credit for, but (b) most buildings were also much stronger than conventional analyses would indicate. A procedure was proposed to reconcile the kinetic energy of the earthquake demand with the stored energy and work capacity of real, complex buildings. (Blume 1958a, 1960, 1961.)

It is essential to clarify at this point that "effective" acceleration is not the same as the base shear design coefficient except for a completely rigid mass, which is rare if indeed one ever exists except as a theoretical model. Most structures have many degrees of freedom, or modes of vibration, and they, and the ground under them, have some compliance. The result is that peak ground acceleration, instrumental or effective, should not be used directly in design. Effective ground acceleration can be used to construct response spectra or to proportion time histories of motion for use in analysis. However, for general purposes of discussion only, peak ground acceleration can be compared herein to base shear coefficients of

assumed rigid structures. Real structures have base shears that depend upon the characteristics of the structure as well as the ground motion.

There are many reasons, some well known today and some not yet generally recognized, why effective acceleration would be less than instrumental peaks. (Report, D-LL 26.) Those considerations which apply to the Diablo Canyon plant will be discussed. The reasons can generally be explained by one or more of four approaches, although the data for these may in some cases be sparse. The approaches, which are not mutually independent, are:

- Observation of what has happened and what has not happened
- Theory and analysis
- Testing and experiments
- Engineering judgment

For examples of observation, the damage, or lack of same, from three major close-in earthquakes will be considered. These three cases have in common the fact that under the rules and procedures being followed for the Diablo Canyon Plant, none would qualify; in fact, they wouldn't come anywhere near qualifying and would be expected to be total losses.

The first case is the great San Francisco earthquake of 1906 ($M \cong 8.25$) with the moving San Andreas fault about 10 miles from downtown San Francisco. If used with

the current (Hosgri) procedures to design Diablo Canyon for a location in San Francisco, the peak "Instrumental" acceleration would no doubt exceed the 1.15g assigned to the Hosgri and the "effective" acceleration would no doubt exceed 0.75g.

However, of the 52 major buildings in downtown San Francisco (none specifically designed for earthquakes forces), all but 7 were repaired and put back into service. Most are still in use today. Of those that did not go back into service, 4 were destroyed by fire and at least one was very poorly constructed. The tallest building, of 19 stories, is still in service today. A few of the surviving buildings still in use include the Central Tower, the Fairmont Hotel, the old part of the St. Francis Hotel, the Post Office Building at Seventh and Mission, the Ferry Building, the Monadnock Building, the Emporium, and the Flood Building. The old Palace Hotel had rather minor earthquake damage but its floors were completely burned out subsequently. Fort Point, only a few miles from the moving fault, had only minor damage. None of these buildings would be able to stand, on paper, by conventional analyses more than 5% or 10% of Diablo Canyon's "effective" acceleration of 0.75g and the resulting spectral response accelerations.

The second case is the ESSO refinery complex at Managua, Nicaragua, which was subjected to the 1972 earthquake that killed some 10,000 persons. The magnitude was 6.25 but

the moving fault was only about 3 miles away from the refinery and the focus only about 2 miles deep. The accelerations were recorded on a modern instrument right at the ESSO refinery -- the peaks were 0.39g EW, 0.34g NS, and 0.33g vertical. The plant structures and vessels had various design levels ranging up to a maximum of 0.20 base shear coefficient and averaging about 0.10 to 0.13 under old Uniform Building Code criteria. For some of the more rigid structures these coefficients could roughly be compared to "effective" acceleration. For other structures, comparisons should be made to the greater spectral response accelerations, properly adjusted. There were all sorts of vertical vessels, pumps, heat exchanges, pipes, buildings, tanks, foundations and instruments.

The ESSO plant had only minor damage. It was shut down for inspection and then started up again in less than 24 hours. For more details, see report D-LL 35.

The third case is the Huachipato Steel Plant, near Concepcion, Chile, which was subjected to a 7.5M earthquake on May 21, 1960 that caused about 0.4% damage but no collapses. The plant was shut down for 6 days and was then back on normal operations. See report D-LL 35 for details.

The epicenter was about 80 km south of the plant but the fault extension is only about 15 km from the plant. Because a larger earthquake occurred to the south of the May 21 epicenter on the following day, and because the steel

plant is to the north of the first epicenter, there is reason to believe the plant on May 21 was right opposite the moving fault. The plant was apparently subjected to motion not much different than Diablo Canyon would have if the Hosgri could in fact rupture opposite the plant from a 7.5M earthquake.

The plant design was on a static rather than dynamic basis for coefficients estimated to have been in the range of 0.10 to 0.30. However, not only important dynamic phenomena but buckling phenomena were not fully considered in the design and much of the damage is attributable to those factors. A rather generous equivalent design coefficient would be 0.12 at 1-1/3 times normal stresses. There is no record of the instrumental peak acceleration. However, an extensive study of the plant by me (Blume, 1963) led to the development of the most likely spectral response acceleration diagram (Report D-LL 35). The probable spectral acceleration value at the period and damping of the most critical structures is 1.2g and the probable effective acceleration was 1/2 to 1/3 of this.

There are many cases of weak structures surviving earthquake motion. The caretaker's house at Pacoima Dam, Figure 3, is a classic example. Obviously, peak acceleration is not the sole criterion for damage.

The major new structures in San Francisco, Oakland, Los Angeles and in other earthquake regions are generally

designed for base shear coefficients in the range of 0.05 to 0.10. Special structures like schools and new hospitals have about double that value, all at 1/3 increase in allowal stresses. Allowing for the stress increases to yield values and for the differences between accelerations and base shear coefficients, modern buildings would have effective acceleration values based on conventional methods in the range of 0.15g to 0.25g. Yet, these cities are subject to more instrumental acceleration than Diablo Canyon because they are closer to major faults and have alluvium materials in most locations rather than rock.

Observations clearly indicate that design by current procedures to any level approaching 1.15g or even 0.75g is not indicated even for the most critical engineere facility.

Unrecognized Safety Margins

Reference needs to be made to unrecognized safety factors or safety margins. Such matters are very important in reconciling recorded instrumental ground motion with damage, or lack of same, and in reaching engineering opinic Many of these unrecognized items are unrecognized in the sense that they are only just beginning to be understood, and many in the sense that they are not allowed under curre design procedures or standards including NRC standards for this plant. It is with the latter context that this secti of testimony is basically concerned. The subject is relev

to the matter of 0.75g effective acceleration or 0.75g tau-reduced acceleration because when the plant is qualified at 0.75g it is in fact qualified at a greater value because these safety margins are ignored. I shall only list some of the most pertinent unrecognized "bonus" values in the system. This is not intended to be any reflection on the NRC review process or reviewers, but on the state-of-the-art and traditional practices. These same practices should not be extended from say a 0.075g design practice to a 0.75g design practice for equal or less earthquake exposures.

(a) In establishing design values for materials, the conventional practice is to make tests, to plot test values on a graph, then to draw a line or curve that represents the lowest values of these test points, and finally to establish safety factors based on that line or curve. Figure 4 is an example, taken at random, for some concrete tests the exact nature of which is immaterial to this discussion. The point is that the equation to be used is based on a line that sub-envelopes all test point; i.e., the real average value is greater than recognized, say in the range of 15 to 30 percent (Report D-LL 18c).

(b) Material strength is specified in such a manner that very few test values for the material supplied can fall below that value without rejection of the while lot. Thus the suppliers provide extra margin to avoid this severe penalty. The average value of steel and concrete

greatly exceeds the specified value. Real test values have been used for much of the re-evaluation for the Hosgri. However, conrete increases strength with age in a nonlinear manner. The concrete at Diablo Canyon is now 6 to 10 years old instead of the 28-day or 90-day ages at which the concrete tests were made. This age gain has not been allowed in the re-evaluation, and yet it is there in the plant concrete; the gain could be in the range of 20 to 60 percent.

(c) When the <u>horizontal components</u> of ground motion are used in analysis it is customary to assume that both components are equal to the peak prescribed ground acceleration and are thus also equal to each other. The facts are that in measurements of actual ground motion the minor component orthogonal to the major component is invariably less than the major component, generally much less. In other words, they are <u>not</u> equal. Figure 5 is the ratio of the maximum to the average peak acceleration, normalized to M = 6 for plotting convenience, plotted against hypocentral distance for all recorded California and Nevada earthquakes in the period 1954-1970. At short distances such as 10km (Diablo Canyon is 8km normal slant distance to the Hosgri) the ratio R is 1.13. This is equivalent to the small components being only 77% of the large component M. Yet in analysis s is taken as 100% of M! This another conservatism, and it could provide excess strength in the order of 10 to 30 percent.

(d) Analysis procedures assume constant natural periods or frequencies of vibration for structural and mechanical systems. In reality, there are small variations in period even at non-damaging stress levels. This is due to the nature of materials, especially concrete, and to other factors. These small variations are quite effective in preventing resonance and in decreasing dynamic amplification. To demonstrate this principle, Figure 6 is a plot of part of a resonance curve for a 7% damped oscillator responding to a steady state harmonic forcing function. At perfect tuning, the ratio of the forcing frequency and the natural frequency is 1.0 and the response is maximum or 100%. However, if the natural frequency varies only slightly, say 5%, the response is about 80%, or 20% less. Thus the assumption of constant natural periods is conservative and could lead to overdesign in the order of 10 to 30 percent.

(e) Floor response spectra for upper levels have been used in the re-evaluation. This, in the first place, is generally considered conservative compared to coupled system analysis. Moreover, the floor response spectra have been computed for constant periods. Nevertheless, the spectral peaks have been widened to allow for possible differences in natural periods from those computed, but without any reduction of the peak response value. Thus there is greater area under the response curve and thus move energy introduced into the disturbance than would be expected

from the earthquake. The amount can be estimated only on a case by case basis but can be considerable.

(f) Smooth response spectra are used in analysis, whereas for any one earthquake (or even for several earthquakes) the actual spectra are jagged with peaks and valleys. Because the smooth curves tend to envelope the peaks, this introduces another conservatism or safety margin into the system. The earthquake peak response may fall where a valley should be, in fact, this is quite likely. This conservatism may lead to 10 to 20 percent overdesign in many cases.

(g) Ductility and work potential (which absorbs energy in the inelastic range) have not been allowed with the Newmark spectra, i.e., the response must be completely elastic. There is thus a great reserve capacity in the inelastic range to absorb energy with even a very slight damage which has thus not been tapped. This is very conservative. Every tall building in a major earthquake has to enter the inelastic range to survive, even under the most modern building code requirements! And yet the Diablo Canyon structures are to remain in the elastic range under much more severe earthquake criteria. There could be reserve capacity for this item estimated at 30 to 100 percent.

(h) Seismic stress in most members and elements is only a part of the total stress picture. For example, a pipe has internal pressure, a concrete wall supports loads

from above. The only exception is bracing designed solely for lateral forces of wind or earthquake. It has been shown in report D-LL21 that members or elements designed for other than seismic stress alone have much more reserve strength for seismic loading than they are given credit for. This may amount to several hundred percent of unrecognized seismic value. The reason for this involves the allowable stresses under each type of loading and the fact that more material is provided than would be needed for seismic purposes only. This item can vary from no extra value (for braces) to several hundred percent.

In view of the above conservatisms in analysis procedures, as well as others, it is clear that there are unrecognized safety margins (if properly considered on a joint probabilistic basis using mean values and deviations from the mean values) such that when the plant is qualified at 0.75g effective acceleration, its most likely capacity is greater than 0.75g by as much as several hundred percent.

D-LL Reports

Reference has been made to certain reports with the designation D-LL preceding the report number as, for example, D-LL18, D-LL26, etc. Other references will be made subsequently. A word of explanation may be helpful.

During the intensive study of the plant the under hypothetical 7.5M and 1.15g/0.75g criteria, special investigations were made and, in some cases, extensive research

conducted on specific aspects of the problem. There were so many of these studies that a designation system was used. The D refers to Appendix "D" of Amendments 50 and 53 of the Hosgri Report, and the LL to the special series of studies and reports by URS/Blume Engineers.

These various reports are available for reference to more complete details than can be provided in this testimony. The reports have been available to the NRC staff and the ACRS subcommittee as well as others. They are part of the public record.

Some of the D-LL reports which are most pertinent to this testimony will be briefly described. These reports pertain to observation, theory and analysis, and testing and experiments, all of which provide input to professional engineering judgment. An asterisk in the margin indicates that some of the results of the report have been, or will be discussed in this testimony.

D-LL5 Blume; "On the Adjustment of Response Spectra," concludes that because of the conventional method of floor response peak widening and of constructing response spectra spectral diagrams used in analyses and design overestimate the actual earthquake input to a structure or system at various periods.

*D-LL6 Blume; "Material Strength," reports on the strength of steel and concrete and shows how the strength of concrete changes with age.

*D-LL9 Blume & Kabir; "Data on Damping Ratios."

*D-LL11	Blume; "Probabilities of Peak Site Acceleration from Assumed Magnitudes up to and including 7.5 of all Local Fault Zones."
*D-LL-8A	Blume; "On the Major Component of Horizontal Ground Motion Versus the Other Component."
*D-LL18B	Blume; "Effect of Natural Period Variations."
*D-LL18C	Blume; "On the Transition from Test Data to Desing Equations."
*D-LL21	Blume; "Seismic Stress Versus Total Stress."
*D-LL26	Blume; Instrumental Versus Effective Acceleration."
*D-LL28	Blume; "The 100-Year Earthquake."
*D-LL30	Blume; "The Effect of Arbitrary Variations in Peak Ground Acceleration on Spectral Response."
*D-LL35	Blume; "Performance of Industrial and Power Facilities in Major Earthquakes."
D-LL36	Blume, Somerville and Czarnecki; "A Comparison of Observed and Estimated Peak Ground Accelerations and their Probabilities." This shows that records taken in San Luis Obispo (of small earthquakes) have peak ground accelerations quite compatible with the corresponding magnitude estimates from report D-LL11 and from another independent procedure.
D-LL37	Blume and Kiremidjian; "Recurrence Relationships by Fault Units." This report concludes that the recurrence rates used in D-LL11 are conservative as compared to those obtained in another, independent effort.
*D-LL39	Blume; "On the Attenuation of Ground Motion by Large Foundations."

*D-LL41 Blume; "Probabilities of Peak Site Accelerations Based on the Geologic Record of Fault Dislocation."

*D-LL42 Blume; "The Effect of Variations in Peak Ground Velocity on Diablo Canyon Structures and Equipment."

D-LL43 Blume; "Discussion of Attenuation Equations," regarding SAM4 and other attenuation equations.

*D-LL45 Blume; "Plate-Boundary and Diffused Areal Probabilistic Considerations."

D-LL46 Blume and Kiremidjian; "Data Sets and Their Treatment in Obtaining Attenuation Relationships." A comprehensive discussion of the data and its treatment in relating magnitude, distance, acceleration and probabilities.

D-LL47 Blume and Kiremidjian; "Near Field Data Effects and Further Treatment of Attenuation Relationships." An extension of D-LL46.

*D-LL49C Blume; "Damping Versus Strain in Reinforced Concrete Shear Walls."

Consideration Of Velocity And Displacement

Report D-LL42 shows that peak spectral acceleration is the basic control in analysis and response of essentially all the plant structures and important equipment and piping. The reason for this is the great rigidity and resulting short natural periods of these items. This in turn means that the peak velocity of the ground (and of the spectral curves) and peak displacement could be assumed to be greatly increased over the criteria values with little or no effect on the plant. Thus there need be no concern over long period motion with greater amplitudes than assumed.

Probabilistic Aspects Of Peak Ground Acceleration

During the course of the reevaluation of the plant for hypothetical 7.5M Hosgri earthquake, the question of probability of occurrence of average return period for the peak accelerations was discussed at various hearings. URS/Blume Engineers conducted three independent and intensive probabilistic studies of the peak ground acceleration which have been reported in Appendix D, Amendment 50 or 53, in particular, reports D-LL 11, D-LL 41, and D-LL 45. These provide data that is quite useful as an aid to judgment. All reports are based on the assumption that the Hosgri could produce earthquakes up to and including 7.5 Magnitude.

In D-LL 11, an earthquake magnitude recurrence curve was drawn based on data obtained by Dr. S. W. Smith for the period of 1930 to 1975. The area considered for these data consisted of 54,000 square kilometers surrounding the site of the plant. In our probabilistic study, and with the concurrence of Dr. Smith, we divided this total areal activity equally to four fault zones, namely the Hosgri at 6 km normal distance from the plant site, the Nacimiento at 25 km, the Rinconada-Ozena at 33 km, and the Santa Lucia Bank at 50 km. The activity rates in this particular analyses were determined by regression analysis of recorded data.

The faults were very carefully modeled as a series of small discretized segments, and the conservative assumption was made that all of the energy of each earthquake would be

assigned to the fault rupture segment closest to the plant. Magnitudes of from 4 to 7.5 inclusive were considered in increments of 1/8M. The attenuation from each source to the site was obtained by the SAM attentuation equations (Appendix D-11B of report D-LL 11) and the sum of all the possible combinations of events was obtained to provide the probability of exceedance of any ground acceleration at the site.

As an independent approach to the same problem and in order to compare results, the history revealed by the geologic record of fault slip over long periods of time was used as the sole source of earthquake activity for report D-LL 41. Fault slip data were provided by D. Hamilton. There were some other differences from the prior report in that a larger area was considered, 13 faults including the San Andreas were considered instead of the prior four local ones, and various other parameters were studied. The work was done for two alternative time periods going back from the present 10,000 years in one case and 20,000,000 years in the other case.

A third study was conducted for which the activity rate during a 45-year period through 1976 over a 3° wide strip extending diagonally for most of the length of California was obtained by Dr. Smith and provided to URS/Blume for the study. In report D-LL 45, the assumption was made that all of the activity in the strip was related to the

tectonic plate boundary, which activity was simply prorated (subsequently) to various lengths of the strip that might affect the Diablo Canyon site. For a given total activity that would affect the site in at least a small degree, various combinations of activity distributions were made to the faults and to the diffused areas. Ten faults were considered.

Figure 7 compares results for all three studies. In this figure "Procedure 1" refers to Report D-LL," Procedure 2" to D-LL41, and "Procedure 3" to report D-LL 45. A convenient summary of the average return periods in years for 1.15g or greater peak instrumental acceleration is given in Table I. All of these studies assume M = 7-1/2 can and will occur on the Hosgri.

TABLE I - SUMMARY OF AVERAGE RETURN PERIODS FOR 1.15g INSTRUMENTAL ACCELERATION

Report Reference	Data Time Span, Years	Method or Basis	Average Return Period for 1.15g or greater, Years
D-LL 11	45	Regression	54,000
D-LL 41	10,000	Fault Slip	74,000
D-LL 41	20×10^6	Fault Slip	29,000
D-LL 45	45	Plate boundary; Diffused	132,000
D-LL 45	45	Plate boundary; to 10 faults	661,000

It is my opinion that the study of fault slip over the last 10,000 years is the most reasonable basis, and it

agrees quite well with the recorded events in recent decades. This would give 74,000 years as the average return period for 1.15g acceleration at the site for the given assumption of 7-1/2 M maximum.

Ang and Newmark (1977) also did a Diablo Canyon probabilistic study by other procedures and obtained for 1.15g instrumental acceleration average return periods of from 67,000 to 83,000 years with various models. These results straddle the 74,000 years noted above and provide an excellent independent check.

In view of all the conservatisms and assumptions, it is concluded in simple round numbers that 1.15g instrumental acceleration (if 7-1/2 M is considered possible on the Hosgri) has an average return period of about 100,000 years. This by no means indicates plant distress -- only the first plateau of the evaluation criteria beyond which there are the many unrecognized safety factors.

The effective acceleration, 0.75g, is that associated with 1.15g instrumental acceleration. Therefore, it has the same average return period -- roughly 100,000 years. In other words, even if the Hosgri could produce M = 7-1/2, the 1.15g instrumental and the 0.75g effective accelerations have an exceedingly remote probability of occurrence.

In overall conclusion about peak ground acceleration, based on analysis, judgment, and the review of work by all the other consultants, I consider the 0.75g effective

acceleration, which corresponds to the assumed 1.15g instrumental acceleration, to be very conservative for the review of the Diablo Canyon plant for the Hosgri exposure. In fact, assuming 1.15g instrumental acceleration at the site, I would consider 0.60g effective as more than adequate for this nuclear plant in view of all the conditions and the many unrecognized safety margins.

CONTENTION 5

My conclusions regarding Contention 5 are:

1. The seismic loading conditions used for the Hosgri evaluation are, in my opinion, extremely severe and, in fact, have vanishingly small probabilities of occurrence, even over tens of thousands of years assuming capacity of the Hosgri to produce 7.5 magnitude. The spectral values have even less probability of occurrence than the 0.75g effective acceleration because the spectral shapes per se are well above the mean shape values and thus compound the margin in the 0.75g.

2. The allowable stresses, the allowable damping values, and the almost total elimination of ductility in combination with the severe loading are also very conservative.

3. The statements under Contention 3 are also pertinent in large degree to 5, and reinforce the overall conclusion that the analysis criteria for the Hosgri are extremely conservative in all respects. Others will

demonstrate that these criteria have been met in analysis and/or by testing or strengthening measures.

Basic Response Spectra

The first major step after the determination of 0.75g as the effective acceleration was the development of response spectra for various damping values, as well as the damping values, per se. It will be assumed for this particular section that the damping values would conform to NRC Regulatory Guide 1.61. The justification for our acceptance of these values and the conformance to them will be presented subsequently. Development of the response spectra was undertaken independently by Dr. Newmark and by me, the former for NRC and the latter for PGandE. It was decided to use both sets of spectra in analysis; whichever would be the most conservative for any structure or element would govern. This in itself was a unique, conservative procedure.

There are many ways to develop a response spectrum. One is to match its shape (not its amplitudes which are scaled) to the shape from a particular, appropriate earthquake, and then to smooth the curve to avoid sharp peaks and valleys. This was done by me for the original plant except that two earthquakes were used, one to model earthquake D close in, and the other to model earthquake B at greater distance. This too was a unique procedure. Another method is to use various empirical ratios of acceleration, velocity and displacement. Still another method is to follow a recog-

nized or a standard shape such as in NRC Regulatory Guide 1.60.

After much study for the Hosgri condition, closeby and on a rock site, I used 8 closeby, rocky-site records for the largest earthquakes recorded under such conditions. These records were described previously. More records would have been used if good records were available. It was felt that this presented an excellent model for at least the important higher frequency part of the spectral diagrams. The spectra for the 8 records naturally varied within each period band of interest, but the results for small period bands were treated statistically to obtain median, mean and standard deviations for each band. The results were plotted and smoothed; however, because of using 8 records statistically, little smoothing was required. All data points were scaled to 0.75g zero-period acceleration. Allowances were then made in the middle and longer period range for the fact that therein peak velocity and peak displacement tend to increase relative to peak acceleration with increases in magnitude. The hypothetical 7.5M was greater than the magnitudes of the recorded events so appropriate increases were made.

My final spectral diagrams for the 7.5M Hosgri evaluation are shown in Figure 8 for the case where tau = 0 (no reduction for foundation size) and for elastic conditions (no ductility allowance). Note that all curves approach 0.75g at zero period. These curves were to be used for

miscellaneous small structures. Figure 9 shows the Newmark curves for the corresponding conditions.

The damping values were to be as NRC Regulatory Guide 1.61 which allows 7% damping for bolted steel or reinforced concrete structures under extreme (shutdown) conditions.

My curves were made in contemplation of an allowable ductility excursion up to 1.3, or a deformation 30% greater than the yield point deformation, if and where needed to meet the spectral values. The effect of this is to slightly reduce the requirements because it (properly) allows for the work done not otherwise recognized. Normal major buildings undergo ductility excursions of several hundred percent in major earthquakes. However, the Newmark spectra were to be met with no ductility excursion. Thus the two sets of curves should not be compared directly. It was agreed after several meetings between the NRC staff and PGandE that my proposed ductility reduction be allowed only to the point of not falling below the corresponding Newmark curve value with no ductility, and further that the most conservative of the Blume and Newmark results would govern each analysis made. Thus two analyses had to be made. The Newmark criteria were generally the most conservative. The vertical spectra have 2/3 of the horizontal spectral accelerations and are shown in Figures 10 and 11.

Spectra Adjusted For Large Foundations (Tau)

As previously noted, we considered 0.75g as a very high zero period or effective acceleration for the given conditions and, especially, in view of the many unrecognized safety margins in the analysis and review system. When the tau factor reductions were proposed for large foundations they seemed to constitute a step in the right direction and one that would lead to more reasonable values. It was recognized, of course, that the tau procedure is a simplification of a very complex wave motion-structure action problem. It can be looked upon as an "engineering equivalent" such as is traditionally used for various loadings and conditions as, for example, wind forces, rail and truck loadings on bridges, live loads on building floors, current forces on wharves and docks, etc. There is ample evidence of the excellent performance of large building foundations in earthquakes. The tau factor is a manifestation of this.

No one who has ever been to sea or has been around boats and ships in rough weather would deny that large ships do not "feel" the waves, seas, and chop as do small boats. There are 6 degrees of freedom, three translational and three rotational, all of which can be felt in the small boat, and only a few (generally) can be felt in a large ship. The amplitude of motion is less in the large ship, especially in pitch, yaw and roll. The large ship "irons out many waves," "averages," or "filters" them. The analogy

to Diablo Canyon and other large structures is a good one except, of course, that the soil or rock has much different properties than water. The effect is there, regardless of the soil and rock properties and regardless of the refinements in analysis.

The larger the foundation and the shorter the traveling wave length, the more effective is the so-called tau reduction. Therefore, it is more effective at high frequencies or short periods than elsewhere. In fact, the reduction varies from a few percent at zero period to nothing at about 0.4 or 0.5 seconds period in my spectra and to slightly longer periods in the Newmark spectra. These spectra and the reduction apply only to horizontal translation. The vertical spectra have no tau reductions.

The values of tau determined by Newmark and by me varied slightly due to different approaches, and so did the peak ground accelerations (PGA) or zero-period accelerations associated with the tau-factor for each structure. Table 2 shows the values used.

TABLE II - TAU AND PGA VALUES

	Blume Criteria		Newmark Criteria	
	Tau	PGA	Tau	PGA
Containment and Instake Structures	0.04	0.67g	0.04	0.60g
Auxiliary Building	0.052	0.63g	0.052	0.55g
Turbine Building	0.08	0.54g	0.067	0.50g
All Other	0	0.75g	0	0.75g

My curves for the containment and intake structures are shown in Figure 12 and the Newmark in Figure 13; for the auxiliary building in Figures 14 and 15 respectively; and for the turbine building in Figures 16 and 17, respectively.

Torsion or twisting of a structure about its vertical axis occurs when the center of mass and the center of rigidity do not coincide. This has long been recognized and provided for in most building codes.

In the last decade or so, the building codes have also required an "accidental" torsion to be considered by the introduction of an assumed eccentricity or artificial distance between the center of mass and the center of rigidity at each level. As a coauthor of these requirements, I know that the accidental torsion provided for in building codes had as one of its basic purposes the increase of the polar moment of inertia of rigidity or torsional stiffness for "core" type building with all most of the lateral resistance at a central core, and, as the other purpose, to provide at least some torsional value when the building inevitably reaches the inelastic stage in seismic response and loses its structural symmetry. In the case of Diablo Canyon, there is large initial polar moment of inertia of rigidity with the heavy exterior walls, and the structure will probably never go into the inelastic range. Thus for Diablo Canyon, and for similar plants, "accidental" torsion is not required for the same reasons as for ordinary buildings.

It is obvious, however, that foundations that are long compared to the ground wave lengths not only tend to average or "iron out" those waves that cause translation but may also be affected by certain types and directions of waves so as to induce some torsional response. In other words, even a symmetric structure can have its foundation so affected by waves that are not symmetric along the length or depth of the foundation. Another simple explanation of this phenomenon is that the ground motion is not applied at a point, as the codes imply, but at all underground surfaces with changes in amplitude, angles of incidence and of azimuth as functions of time. Building codes do not recognize this.

Torsion has been provided for in the Diablo Canyon Hosgri review by assuming eccentricities of mass and rigidity where none in fact exist. In view of the fact that these structures do not need this as do ordinary buildings, it constitutes a real adjustment and bonus for wave-induced torsion. It is an "engineering equivalent" procedure, as is the tau-factor.

Another factor to be considered in torsion is that the foundations of each structure are not only large but adjacent to the other foundations; in fact, the overall plant, with Units 1 and 2, constitutes a very large contiguous area which cannot be compared to isolated structures free of any neighbors.

Attenuation of high frequency motion by large, deep, rigid foundation structures is real and should be considered. Torsion effects are also real, although not always significant, and they exist whether or not "tau" reductions are taken for translational motion. In view of all factors and the many unrecognized values or margins, it is my opinion that the tau reductions and the torsional criteria applied are both proper and adequate for the current state of the art.

<u>Damping</u>

There has been much discussion in the ACRS meetings about damping values. The values in NRC Regulatory Guide 1.61 which have been in use for several years for many plants, were questioned. As a result of this, new data were obtained and studied and old data were reviewed. Reports on damping values, D-LL 9 and D-LL 49C, were prepared by URS/ Blume Engineers.

Two facts regarding this complex subject are particularly important. One is that elements with friction between parts, such as bolted steel joints or concrete with minor cracks, have considerably greater damping at the same strain levels than where friction is not possible, as for example in welded joints or in uncracked concrete. The second point is that damping increases with strain or deformation. These two factors are not necessarily mutually exclusive. Another important consideration is that a

structure not only receives energy from the moving ground but returns some of it to the ground; this is often termed radiation damping.

Another point that is often misunderstood is that it is not necessary to develop high strain levels throughout an entire structure to develop high damping levels. Local high strain levels can be quite effective in absorbing the kinetic energy of motion, as shown by tests.

Various tests and measurements of damping have been shown in report D-LL 9 and in report D-LL 49C. Table III shows damping results, from nine test series, for two levels of strain -- at micro levels and at or about the yield point. At the yield levels, all test results are at or above 7% of critical damping; 7% is the value used at about yield level in the Diablo Canyon structure analyses.

TABLE III - SUMMARY OF DAMPING VALUES

	At micro levels of stress and strain	In the yield range of stress and strain
CVTR Reactor	6 to 9%	----
EGCR Reactor	1.5% to 5%	----
22 Concrete Buildings	mean 5.6%	----
	1.25 x mean 7.0%	----
Bridge Piers	3.4% to 16.6%	----
	(average 8%)	----
Models of Bridge Piers	----	7%
Models of Coupled Shear Walls	----	10% at 1.1 x yield
Models of Coupled Shear Walls	----	8% at 0.9 x yield
Models of Shear Walls	2% to 4%	7% to 10%
Scales Building Models	2% to 3%	up to 9%

The tests of the shear wall models (Figures 18 and 19) are particularly interesting for two reasons -- they are of reinforced concrete shear walls, as is much of the Diablo Canyon structures; and the base, and also the support of the base, of the wall test specimens was such as to essentially eliminate all radiation damping to the soil. The latter point is significant for those who contend that radiation damping is present in much testing of damping. (Of course, the Diablo Canyon structures, as compared to these models, will have the benefits of any radiation damping even though it is not credited.)

Figure 20 shows the wall test results. The line "average curve" was drawn by the test authors, and the 7% line was drawn by us for comparison purposes. The 7% damping value occurs at a strain level in the reinforcing steel of about 0.16%, well below the yield value.

Figure 21 is a different plot of the same data. This shows that 7% damping is achieved at about 75% of yield stress in the bars and that at yield the average test value was 9%, all without radiation energy loss to the soil.

Similar results are available for bolted steel buildings and frames.

It is my opinion that 7% of critical damping is conservative for Diablo Canyon structures subjected to the hypothetical 7-1/2M Hosgri earthquake. The value could be 8 to 10% at such extreme loading.

Application Of Current Criteria
For Hosgri Reanalysis And Safety

The response spectra and the damping values were applied to each structure as appropriate to obtain the moments, shears, axial forces and stresses at various points in the structures. This was done by others and the results provided in terms of the stresses obtained as compared to the stresses allowable under NRC regulations. In some cases "overstresses" were found and physical alterations have been or are being made to the structures involved so as to meet all the criteria.

In addition, "floor response spectra" were developed to represent the amplified motion at some upper level, or floor, where piping or equipment is attached or anchored. This procedure will also be described by others.

It is my opinion and testimony that these criteria, starting with the hypothetical 7.5M Hosgri earthquake and working down through 1.15g peak instrumental acceleration, 0.75g effective acceleration with the tau factor adjustments, and with the damping specified, are very conservative in view of all the conditions, and that when these criteria are met there is much more than a reasonable degree of engineering certainty that the plant can be operated without undue risk to the health and safety of the public from or induced by earthquake motion.

REFERENCES

Most references in this testimony have been to the D-LL reports which have been tabulated within the preceding testimony. These reports in turn generally provide additional references to pertinent sources of material.

The following were also noted:

Ang, A. H-S., and N. M. Newmark, "A Probabilistic Seismic Safety Assessment of the Diablo Canyon Nuclear Power Plant," November, 1977.

Blume, John A., "Structural Dynamics in Earthquake Resistant Design," Journal of the Division, ASCE, July 1958 and discussions ending September 1959; also published in Transactions, ASCE, 125:1088-1139, 1960.

Blume, John A., "A Reserve Energy Technique for the Earthquake Design and Rating of Structures in the Inelastic Range," Proceedings, Second World Conference on Earthquake Engineering, Tokyo, 1960.

Blume, John A., N. M. Newmark and Leo H. Corning, "Design of Multistory Reinforced Concrete Buildings for Earthquake Motions," Portland Cement Association, Skokie, Illinois, 1961.

Blume, John A., "A Structural-Dynamic Analysis of Steel Plant Structures Subjected to the May 1960 Chilean Earthquakes," Bulletin of the Seismological Society of America, 53:439-480, February 1963.

Page, Robert A., David M. Boore, William B. Joyner, and Henry W. Colter, "Ground Motion Values for Use in the Seismic Design of the Trans-Alaska Pipeline System," U.S. Geological Survey Circular 672, 1972.

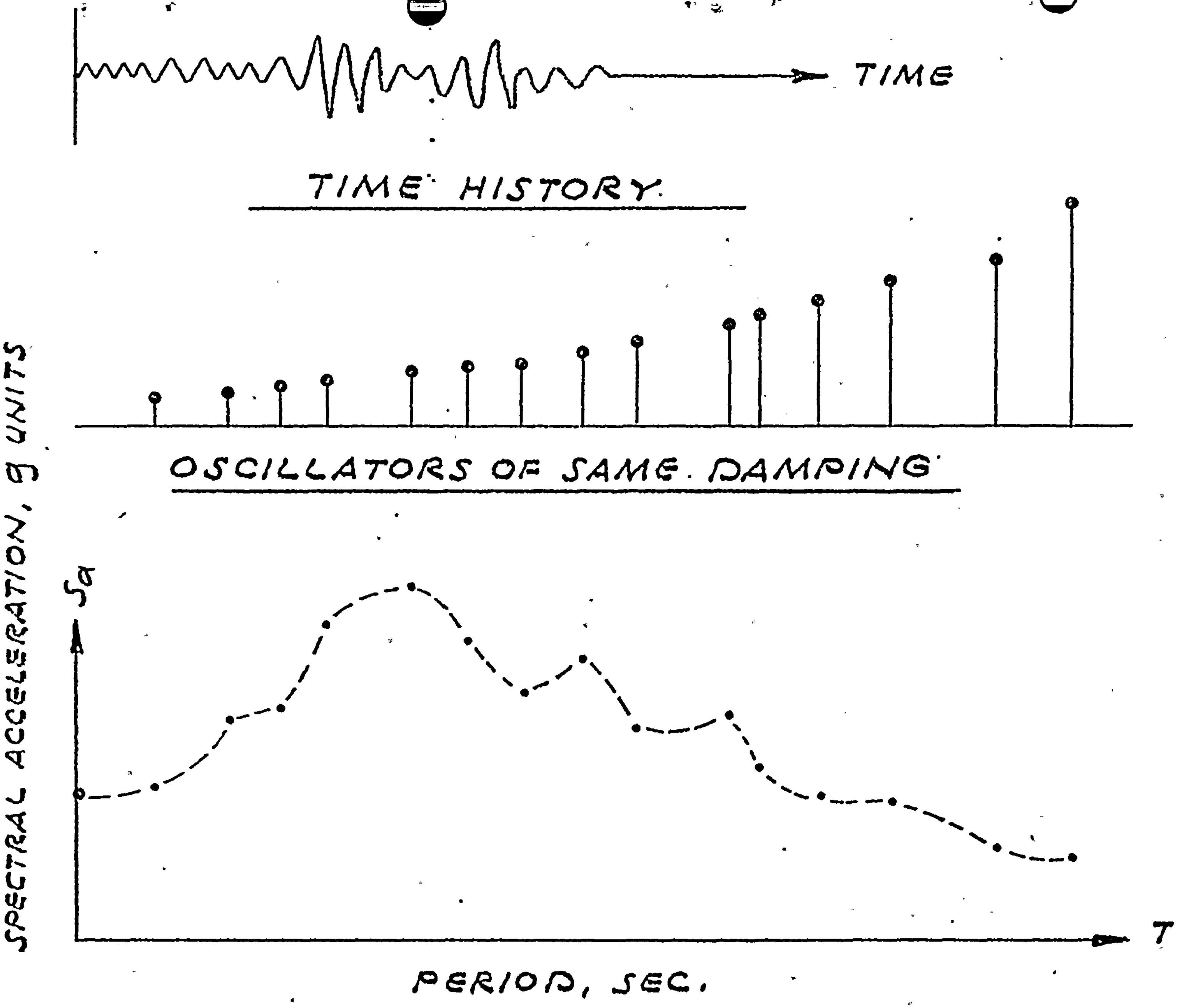


FIG. A - CONSTRUCTION OF A RESPONSE SPECTRUM

RESPONSE SPECTRA

DAMPING = 0, 2, 5, 10 & 20 %

1940 EL CENTRO EARTHQUAKE, SOOE

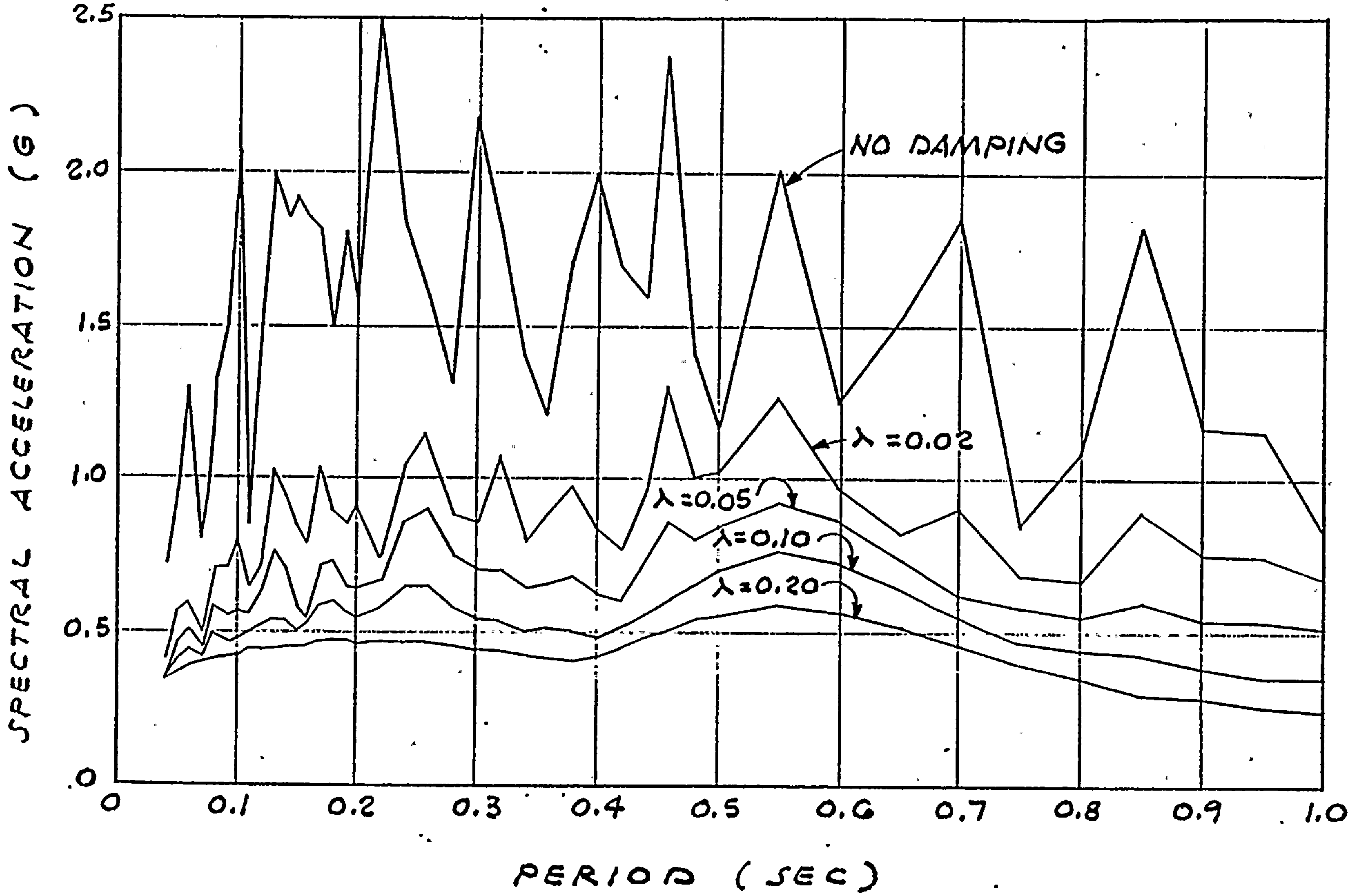
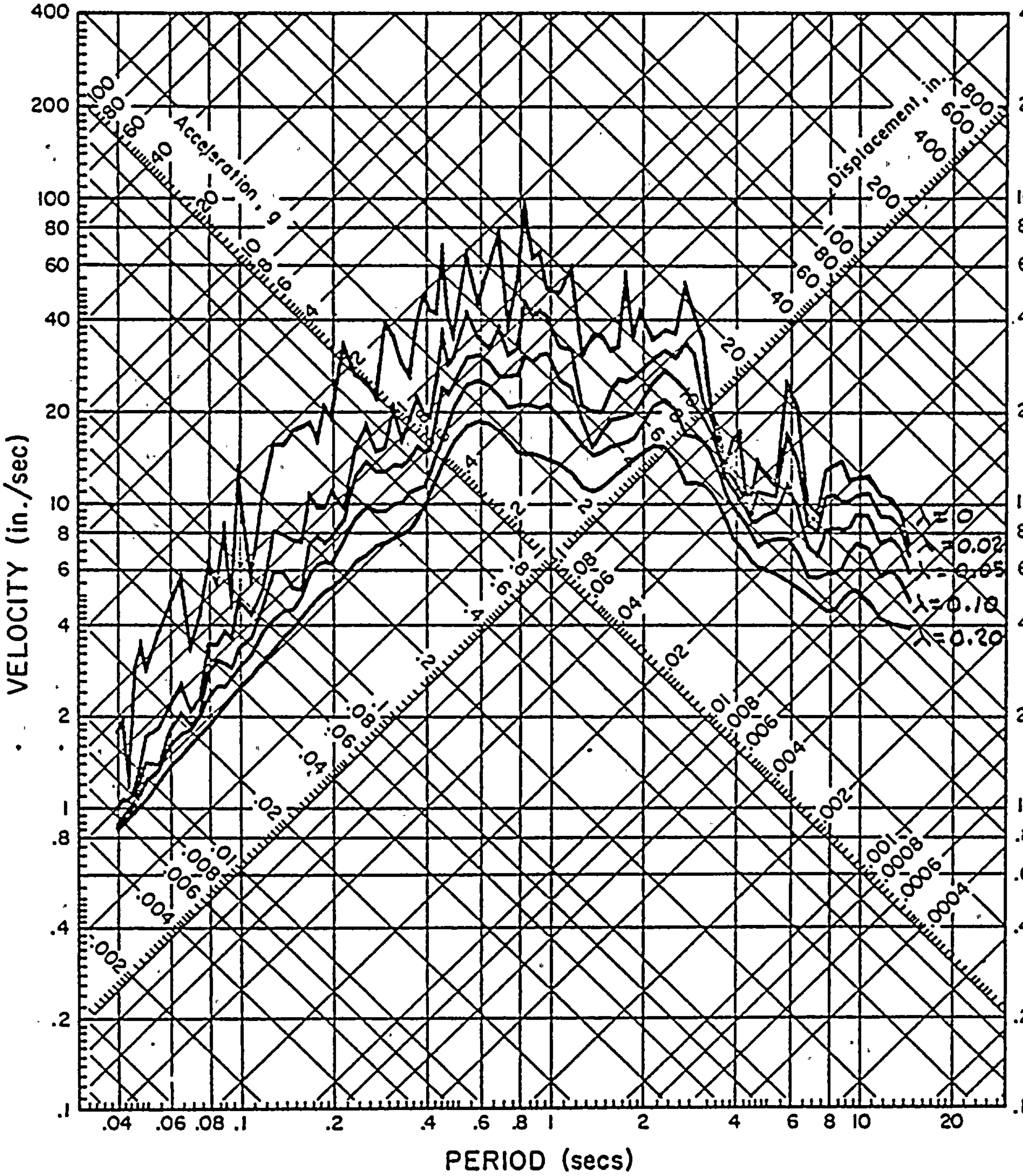


FIGURE B

BLUME



4-WAY LOG PLOT OF RESPONSE SPECTRA
1940 EL CENTRO EARTHQUAKE, SOOE — FIGURE C

BLUME

DIABLO CANYON UNITS 1 & 2
DESIGN SPECTRA

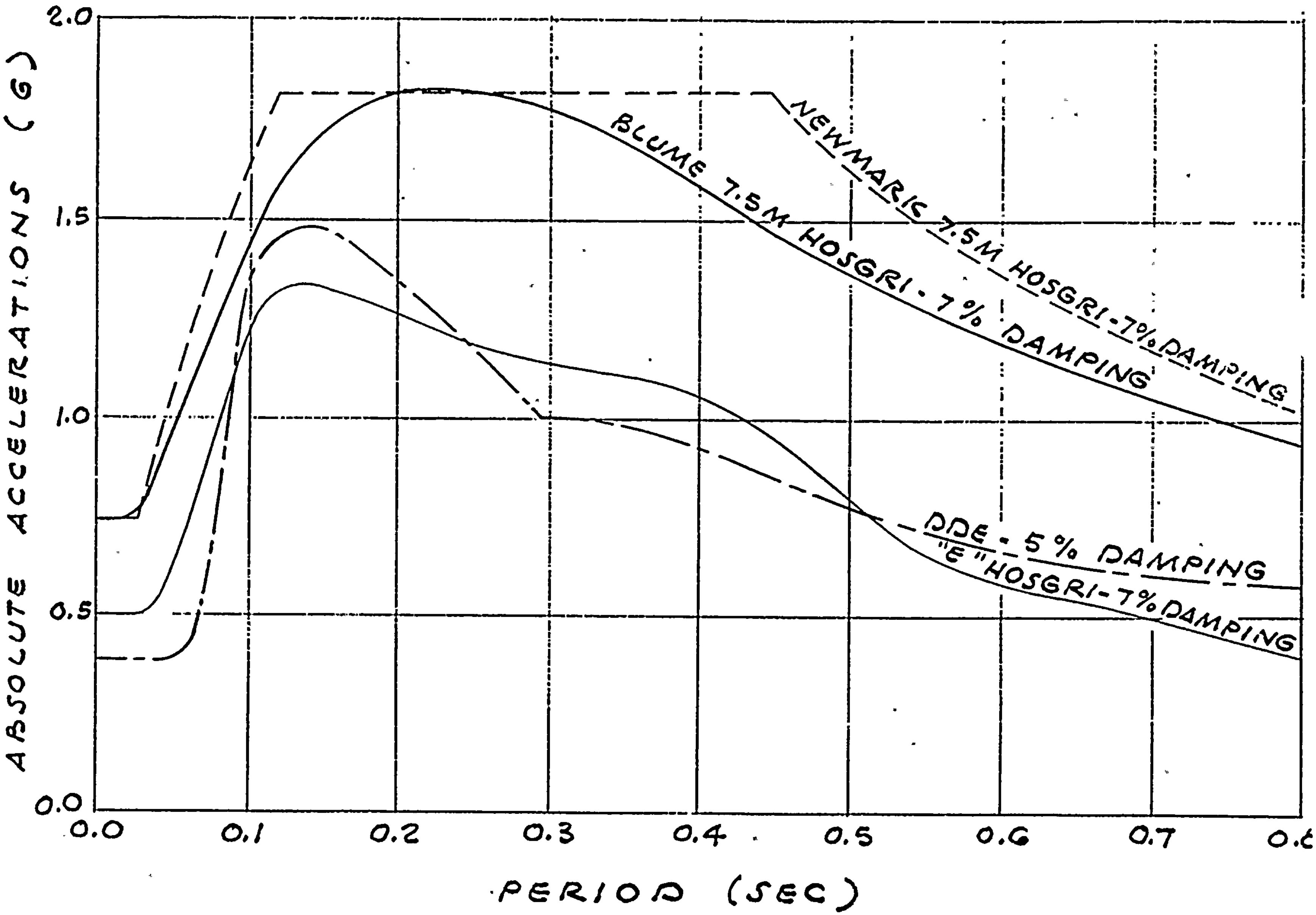


FIGURE D

BLUME

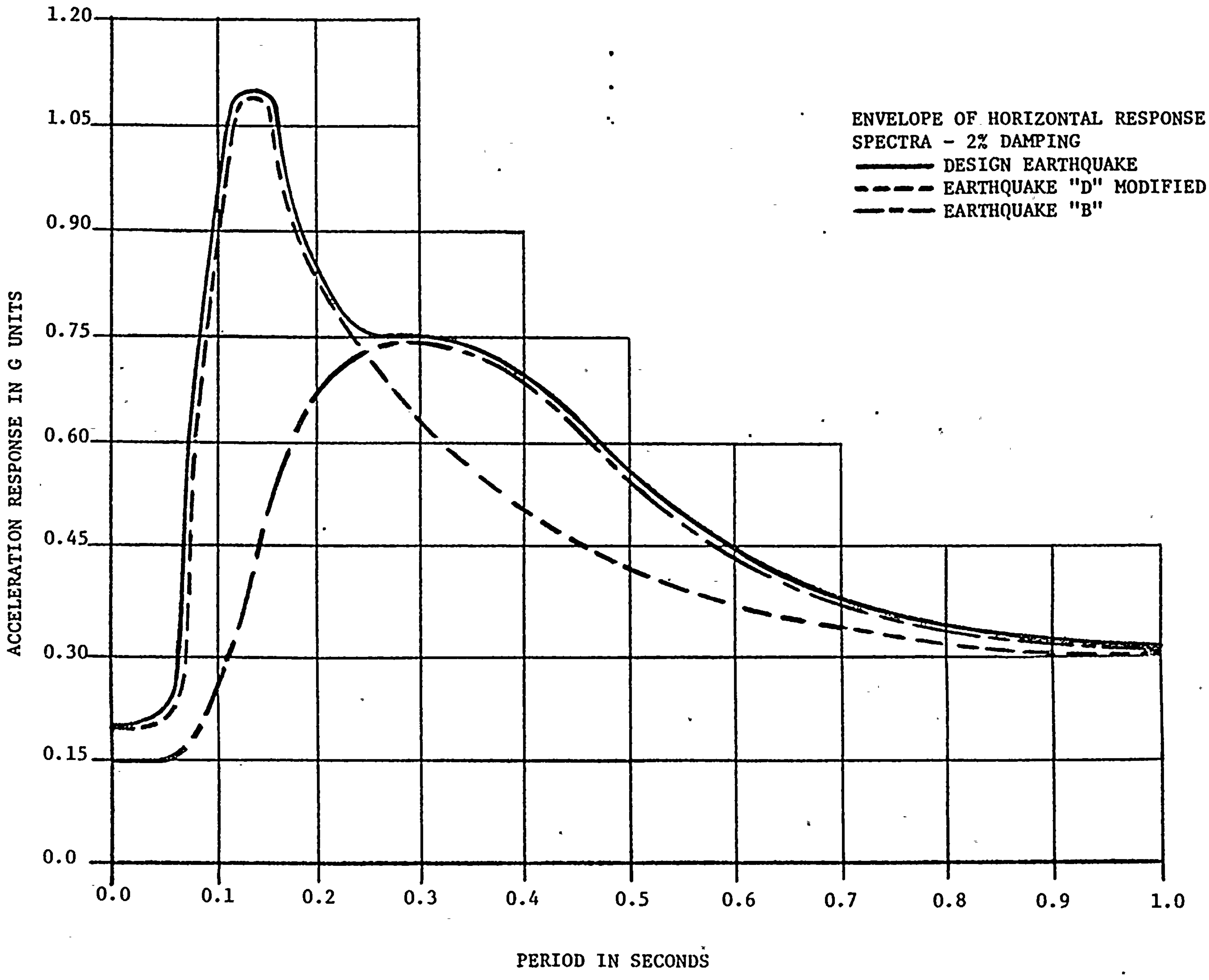


FIGURE 1

BLUME

DIABLO CANYON UNITS 1 & 2
COMPARISON OF RESPONSE SPECTRA
EARTHQUAKES B, D AND E

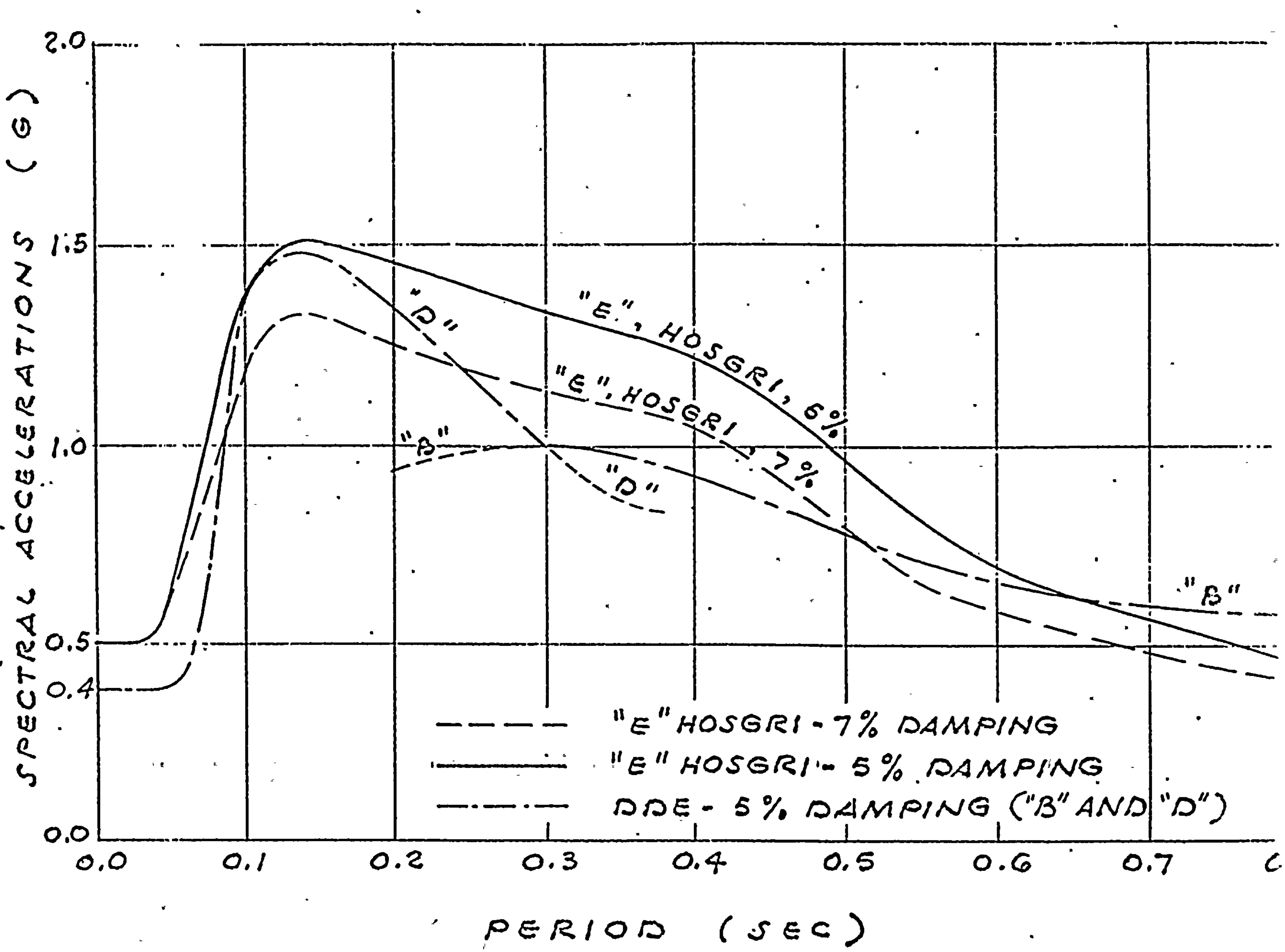


FIGURE 2

BLUME

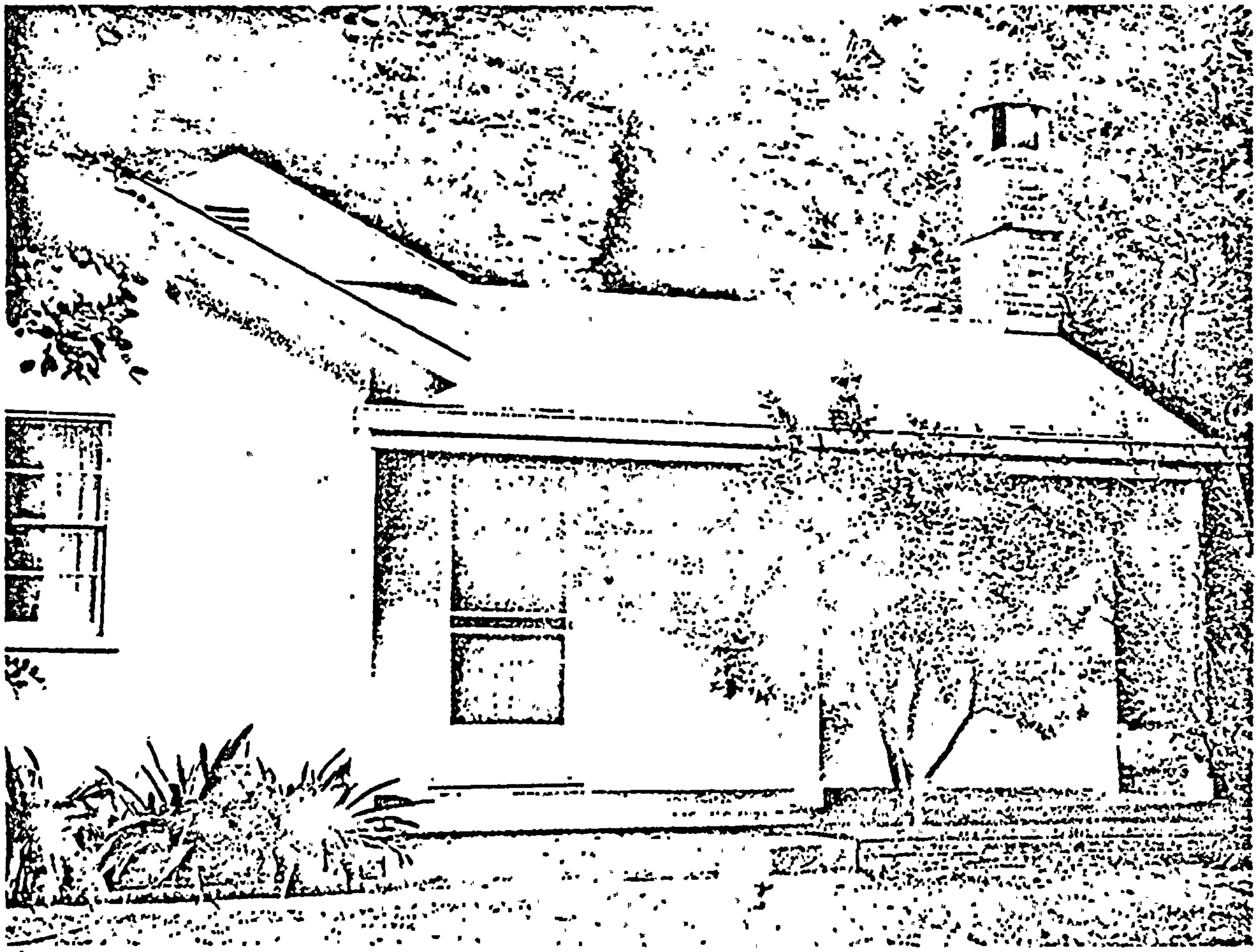


Figure 3 - The Pacoima Dam caretaker's house at the mouth of Pacoima Canyon. This older structure, with a brick chimney, was not damaged by the shaking even though it was within one-half mile of Pacoima Dam and within one mile of the Veterans Administration Hospital. The degree to which the ground shaking here might have differed from that at these other sites is not known.

From: "Engineering Features of the San Fernando Earthquake," P. Jennings, Ed., California Institute of Technology report EERL 71-02, June 1971.

(Fig. 18C-2 in D-LL18C)

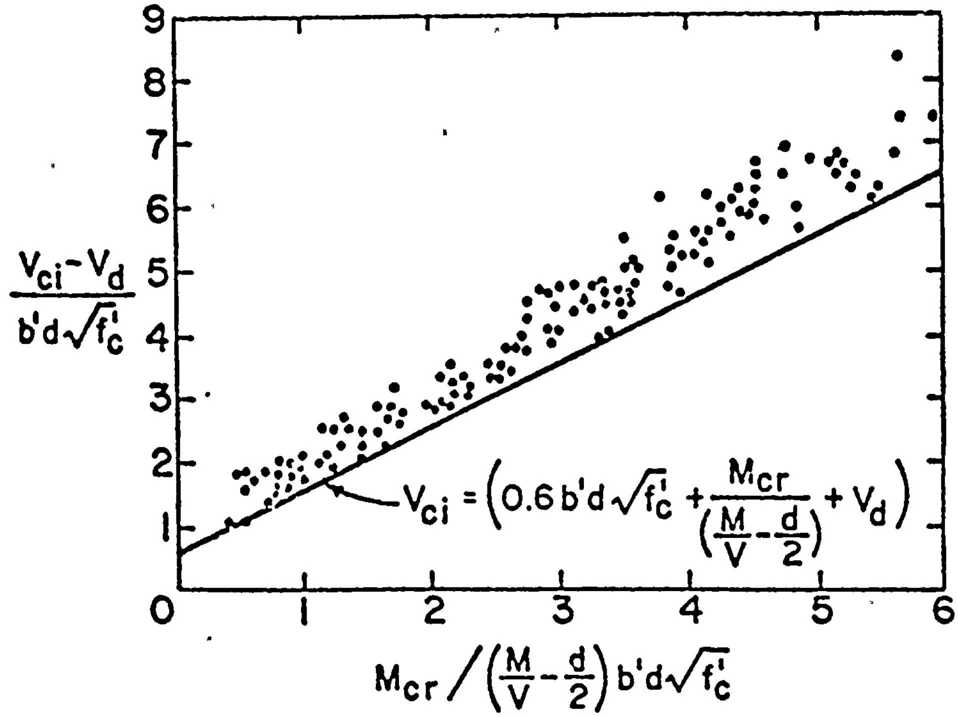
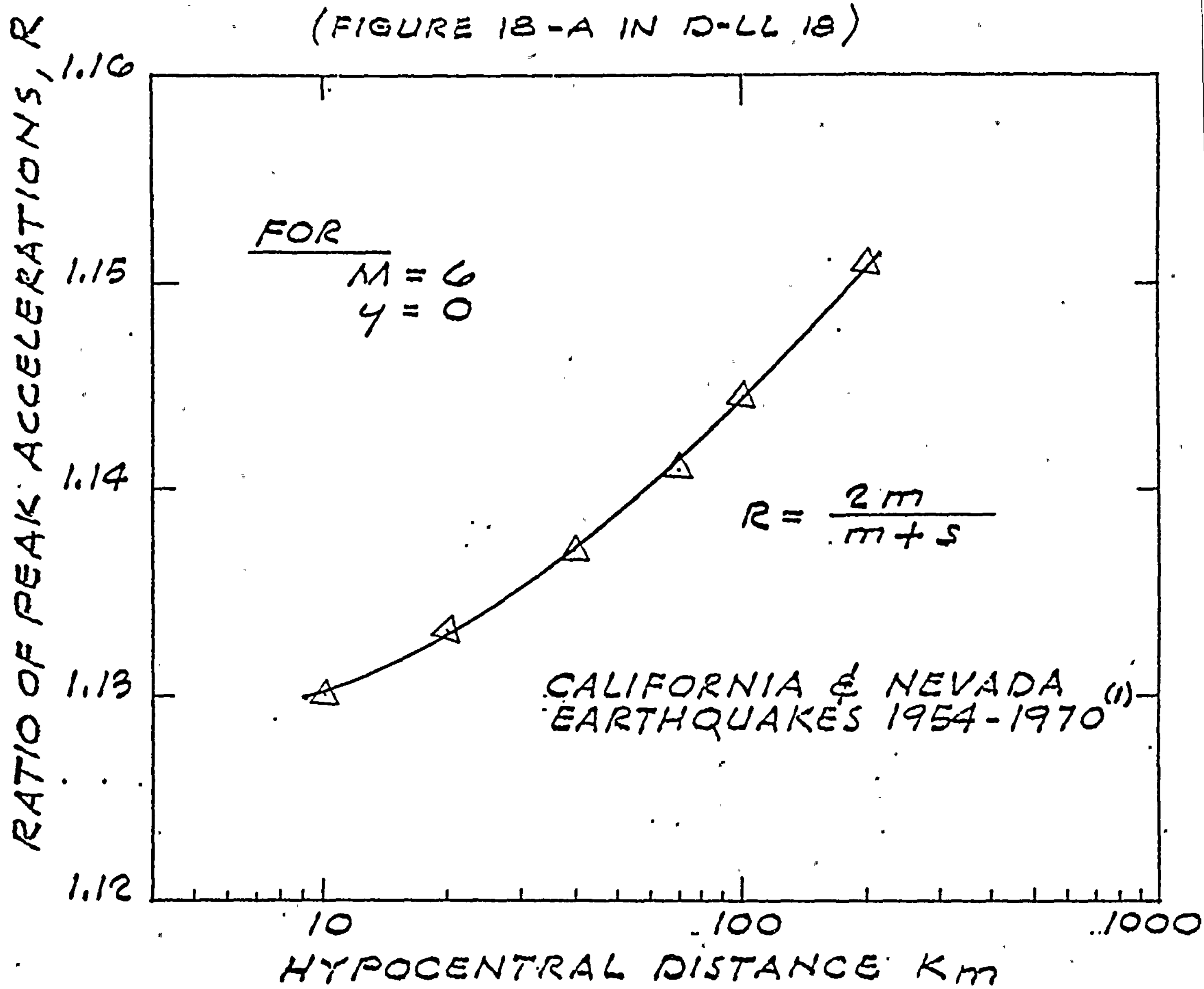


Fig. 4: Diagonal cracking in those regions of beams previously cracked in flexure

FIGURE 4

BLUME

(FIGURE 18-A IN D-LL 18)



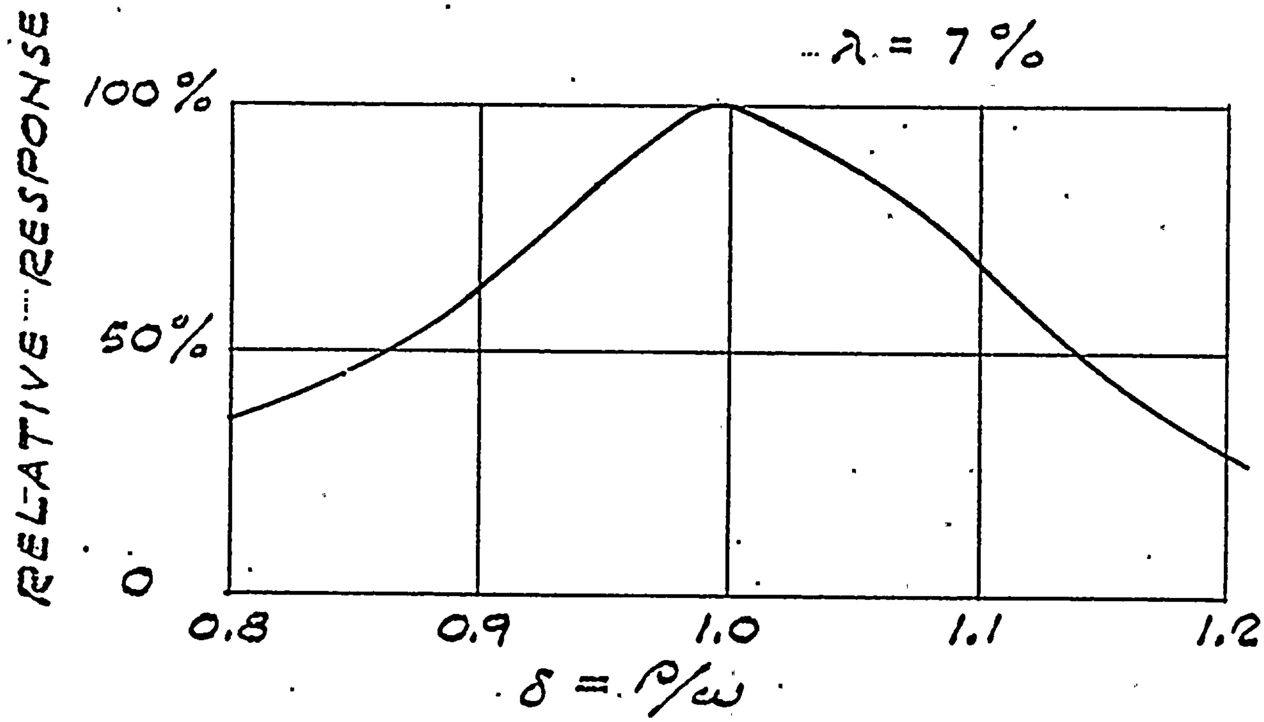
RATIO OF MAXIMUM TO AVERAGE HORIZONTAL PEAK GROUND ACCELERATION

DIABLO CANYON NUCLEAR POWER PLANT

FIGURE 5

BLUME

(FIGURE 18-B IN D-LL18)



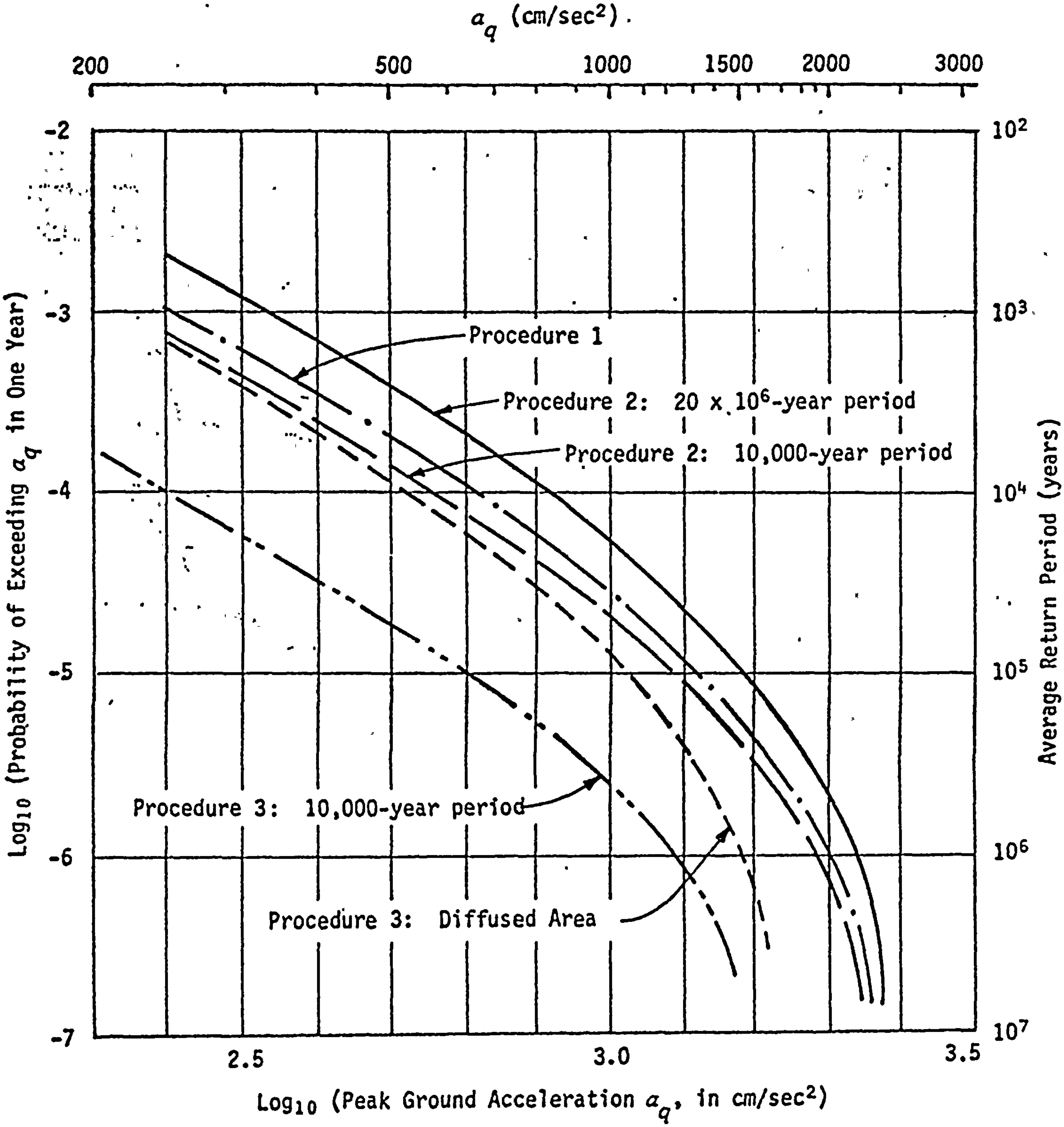
RELATIVE RESPONSE OF A 7% DAMPED OSCILLATOR IN THE STEADY STATE UNDER A HARMONIC FORCING FUNCTION.

DIABLO CANYON
NUCLEAR POWER PLANT

FIGURE 6

BLUME

[illegible]



a_q (cm/sec²)
200
500
1000
1500
2000
3000
-2
-3
-4
-5
-6
-7
10²
10³
10⁴
10⁵
10⁶
10⁷
Procedure 1
Procedure 2: 20 x 10⁶-year period
Procedure 2: 10,000-year period
Procedure 3: 10,000-year period
Procedure 3: Diffused Area
2.5
3.0
3.5
Log₁₀ (Probability of Exceeding a_q in One Year)
Average Return Period (years)
Log₁₀ (Peak Ground Acceleration a_q, in cm/sec²)

FIGURE 7

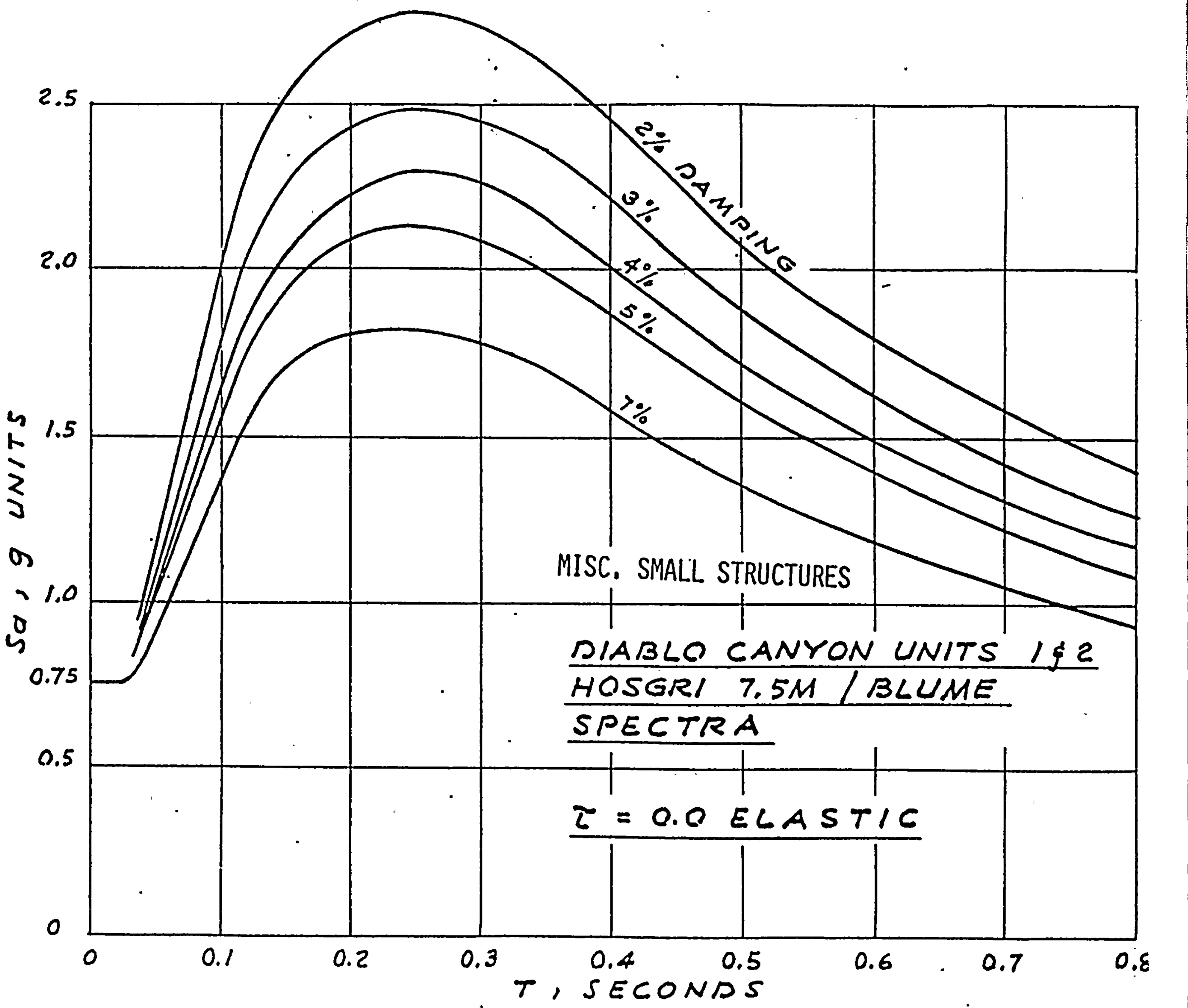


FIGURE 8

BLUME

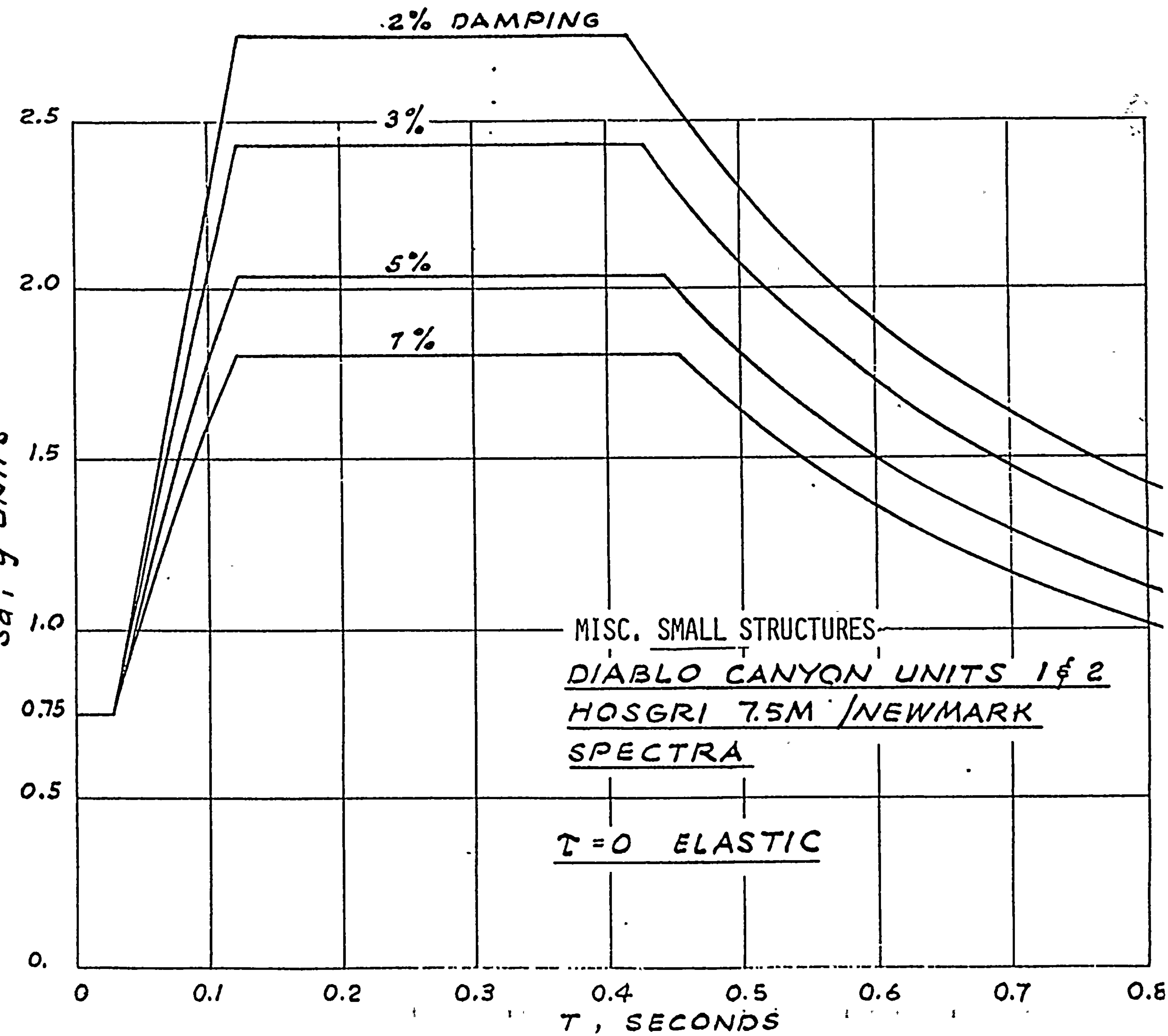


FIGURE 9

BLUME

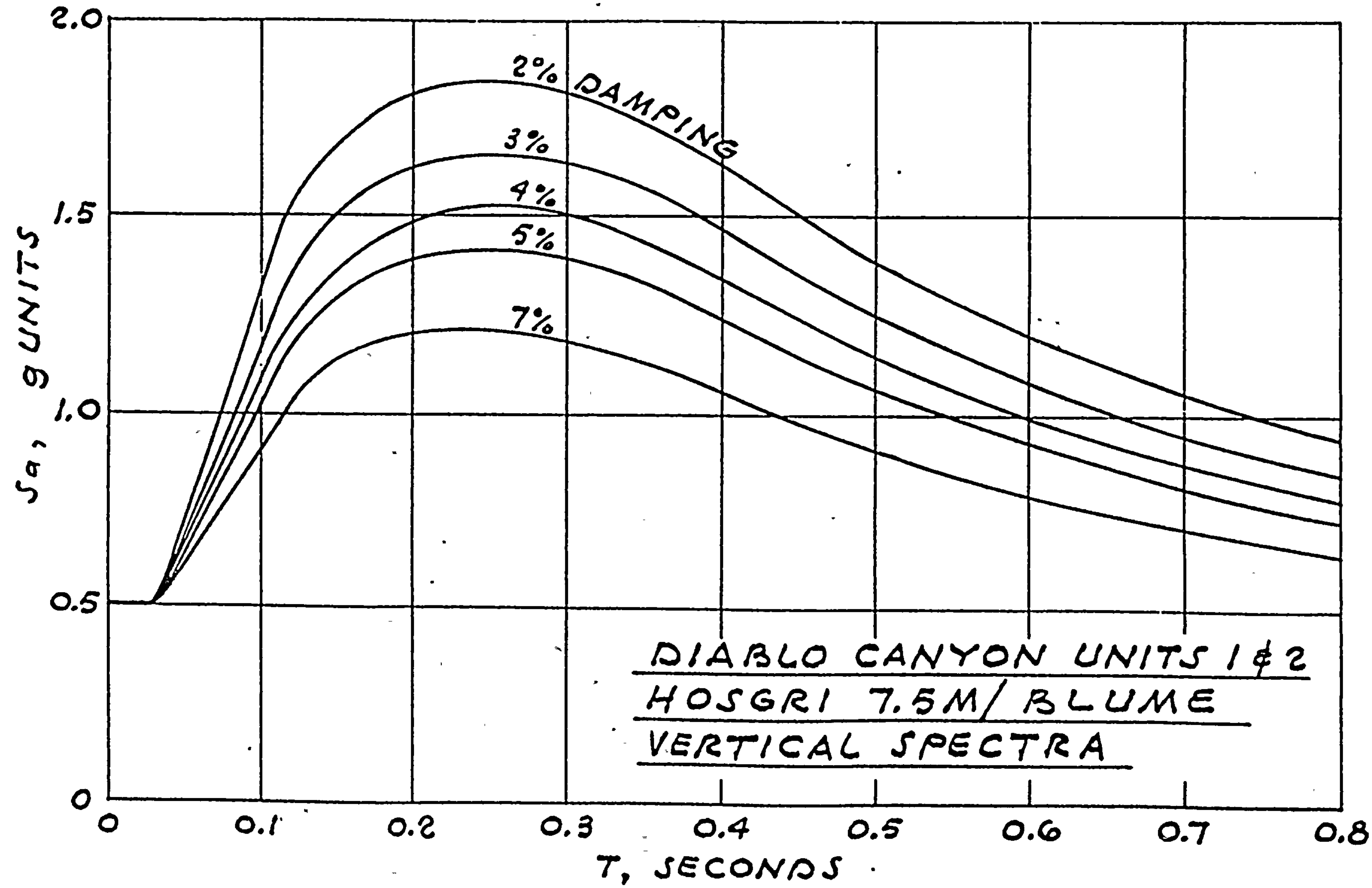


FIGURE 10

BLUME

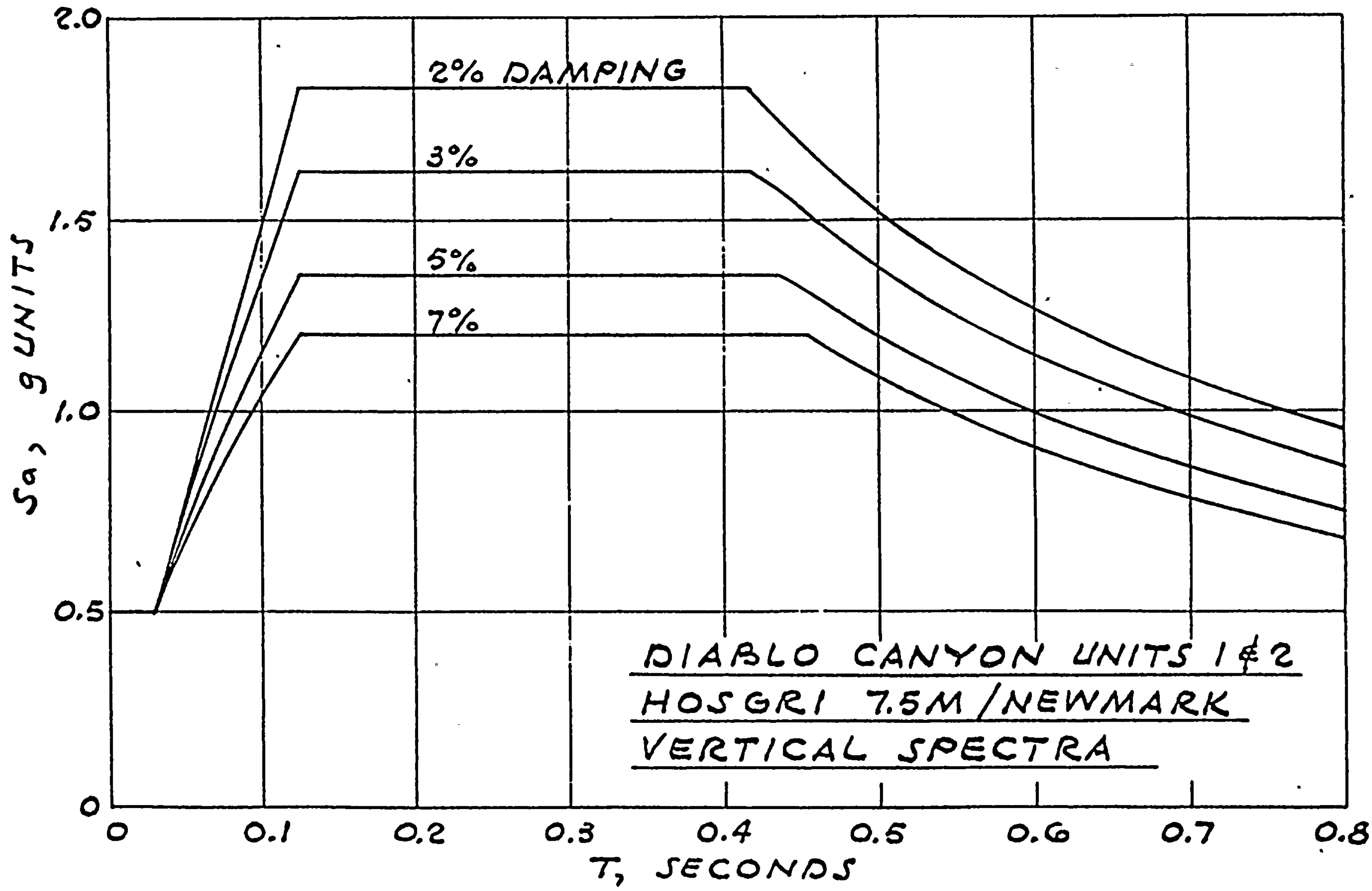


FIGURE 11

BLUME

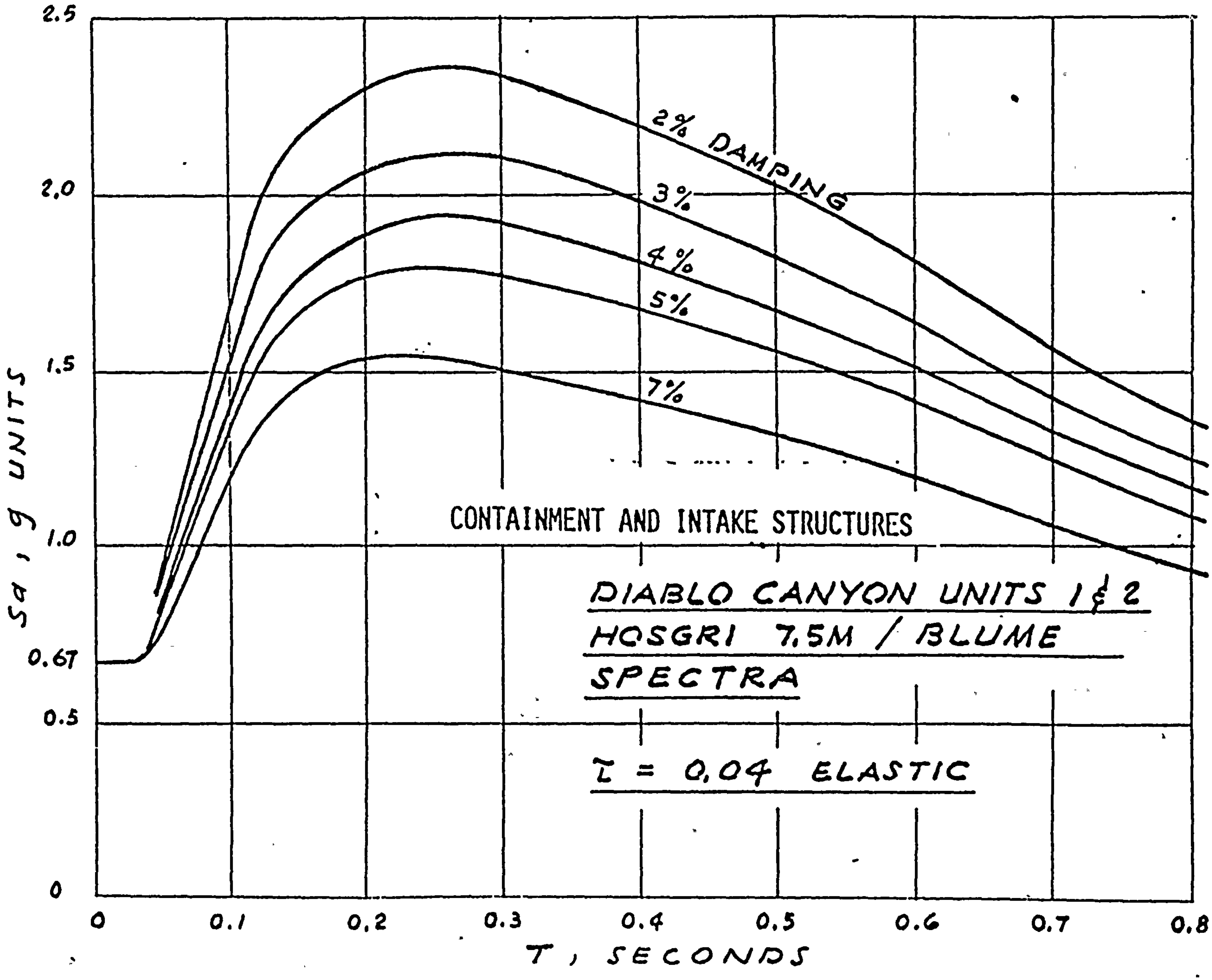


FIGURE 12

BLUME

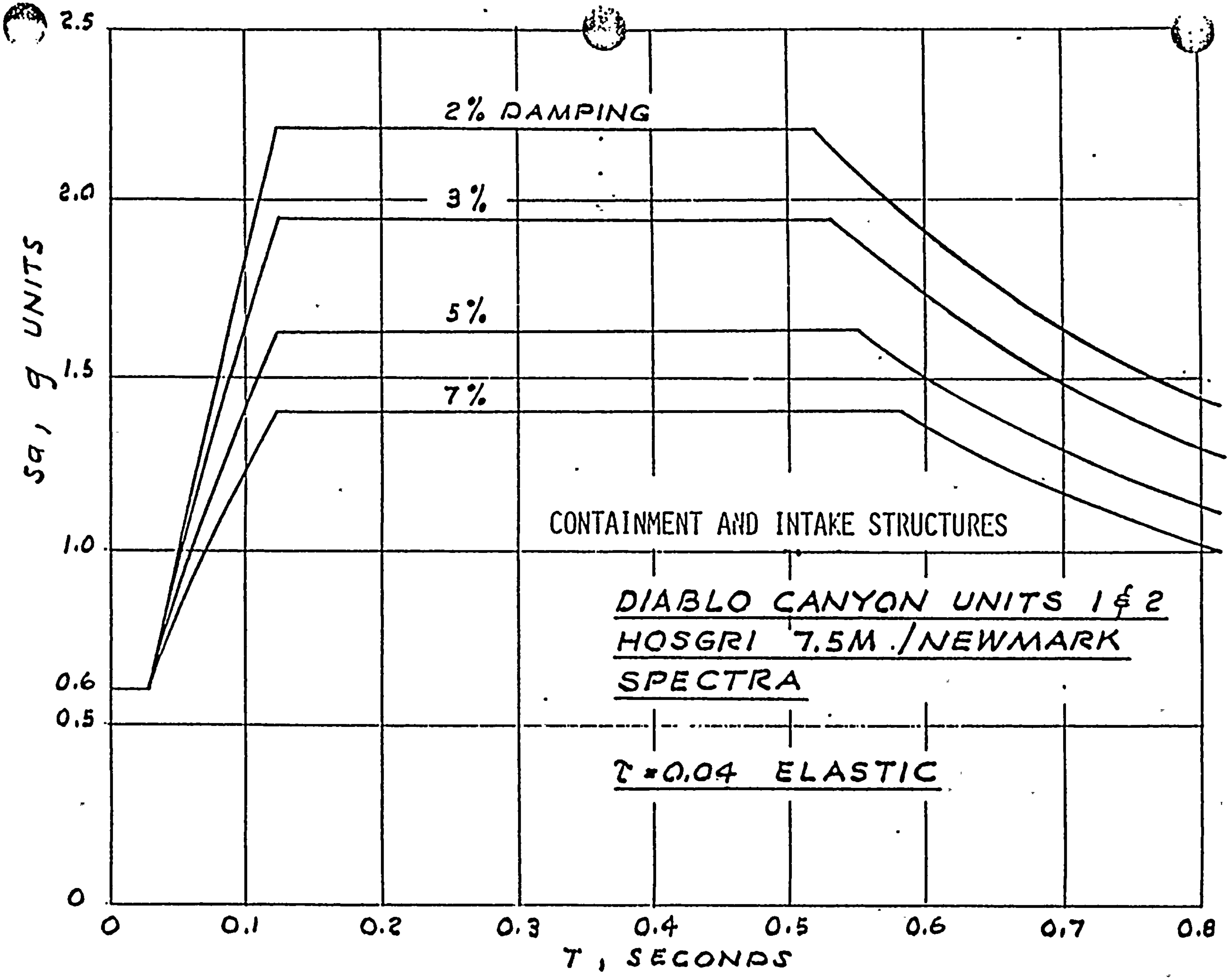


FIGURE 13

BLUME

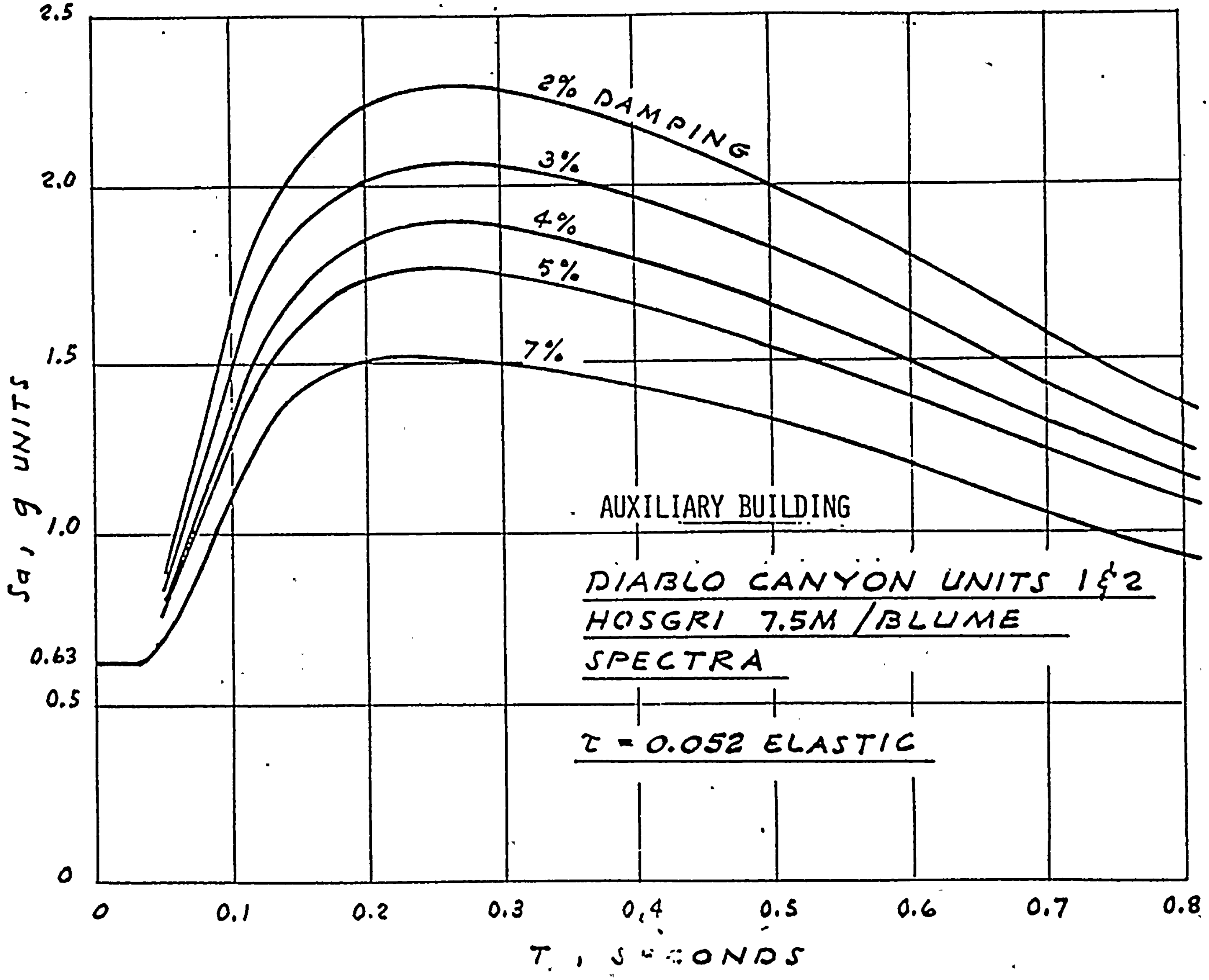


FIGURE 14

BLUME

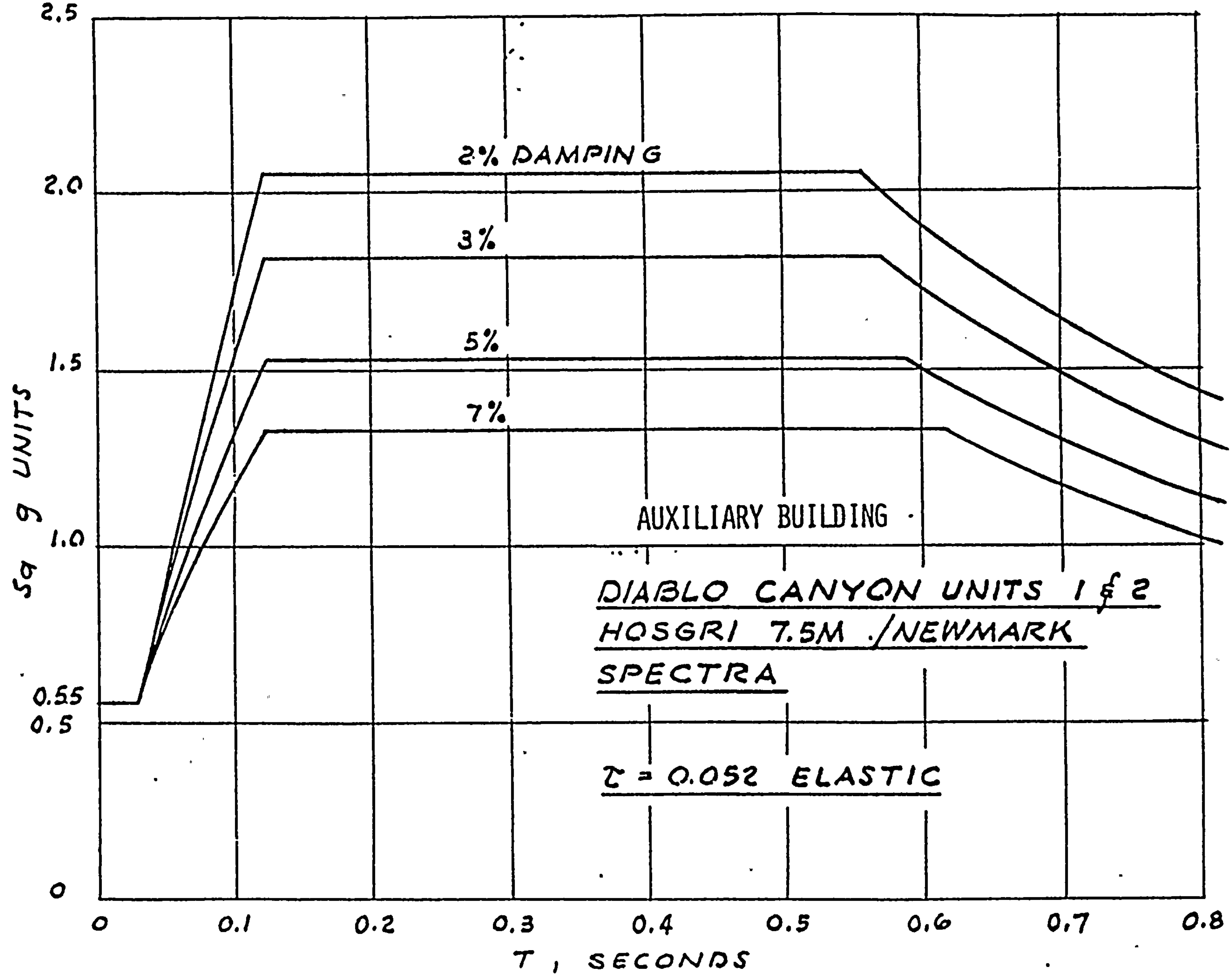


FIGURE 15

BLUME

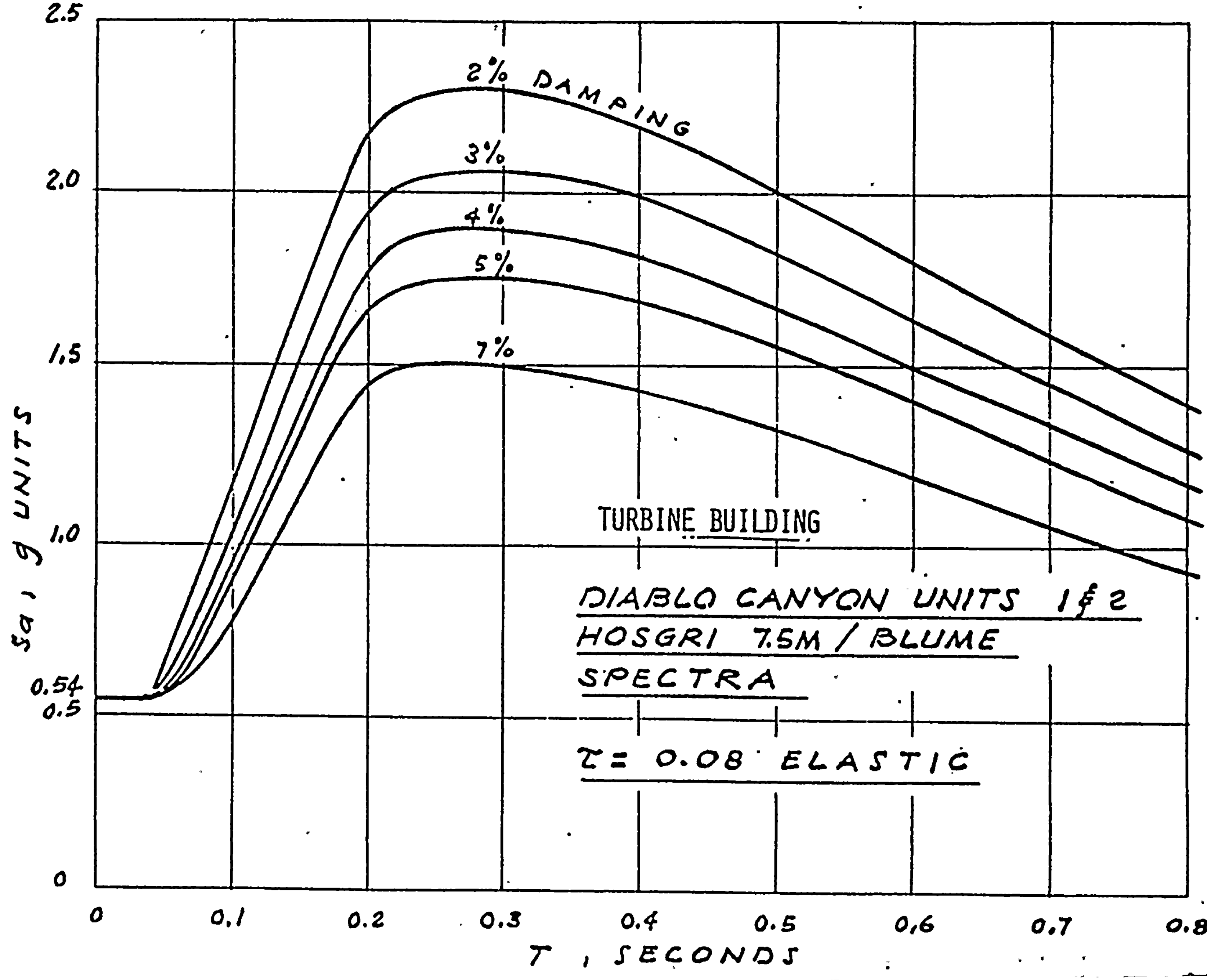


FIGURE 16

BLUME

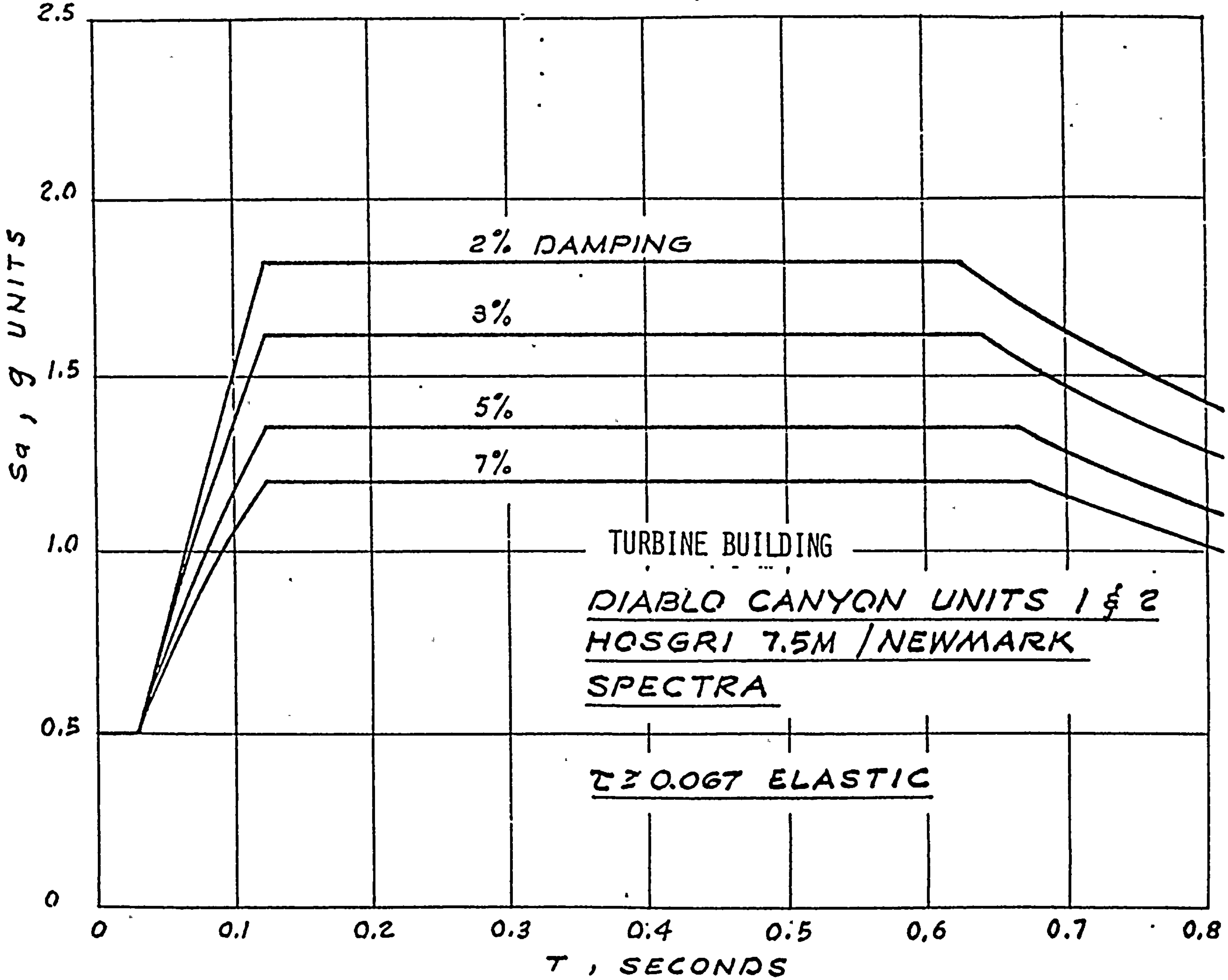
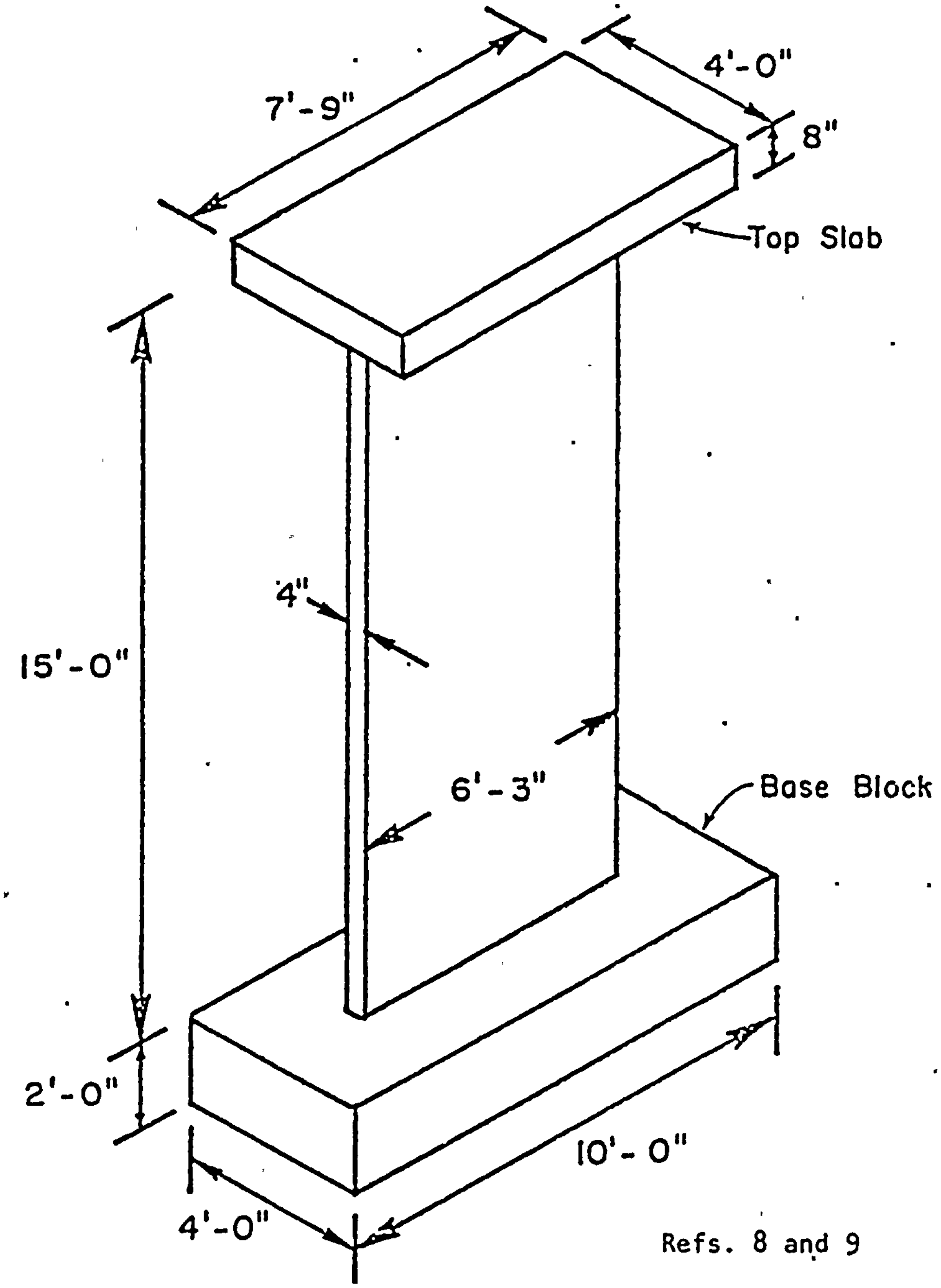


FIGURE 17

BLUME



Refs. 8 and 9

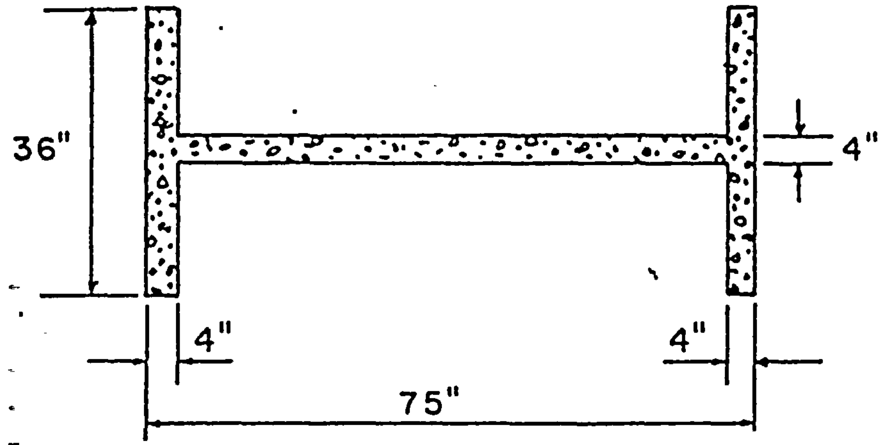
Nominal Dimensions of Test Specimen
With Rectangular Cross Section

(Figure 9-H in D-LL9)

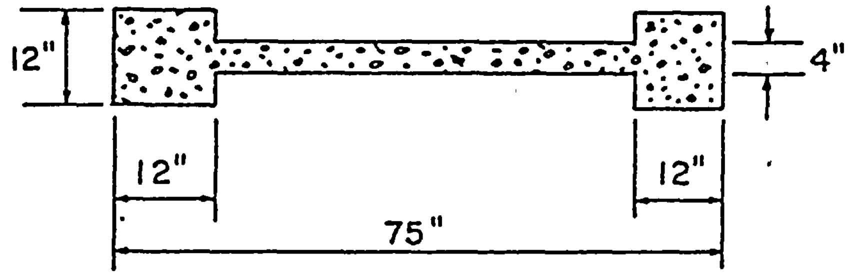
DIABLO CANYON
NUCLEAR POWER PLANT

FIGURE 18

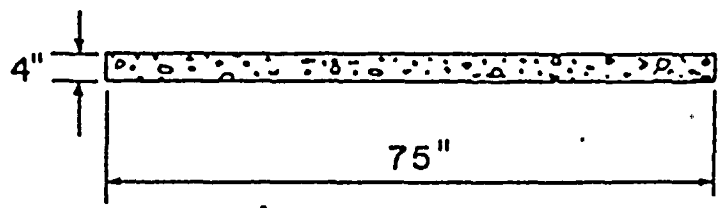
BLUME



(a) Flanged Section



(b) Barbell Section



(c) Rectangular Section

Nominal Cross-Sectional Dimensions
of Test Specimens

(Figure 9-I in D-LL9)

DIABLO CANYON
NUCLEAR POWER PLANT

FIGURE 19

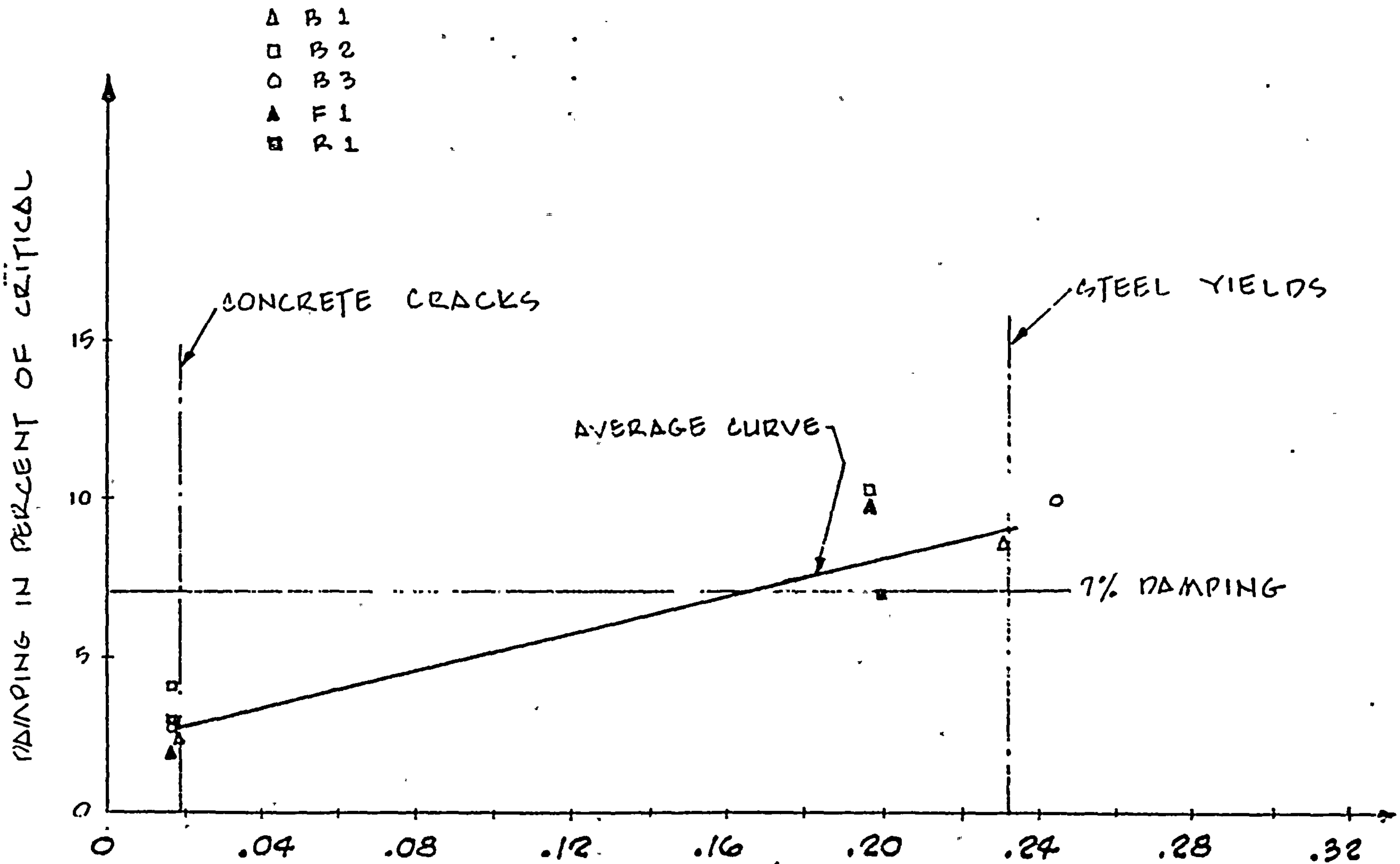
BLUME

DIABLO CANYON
NUCLEAR POWER PLANT

FIGURE 20.

BLUME

(FIGURE 9-J IN D-LL9)



DAMPING VS. STRAIN LEVEL IN THE PCA SPECIMENS

[illegible]

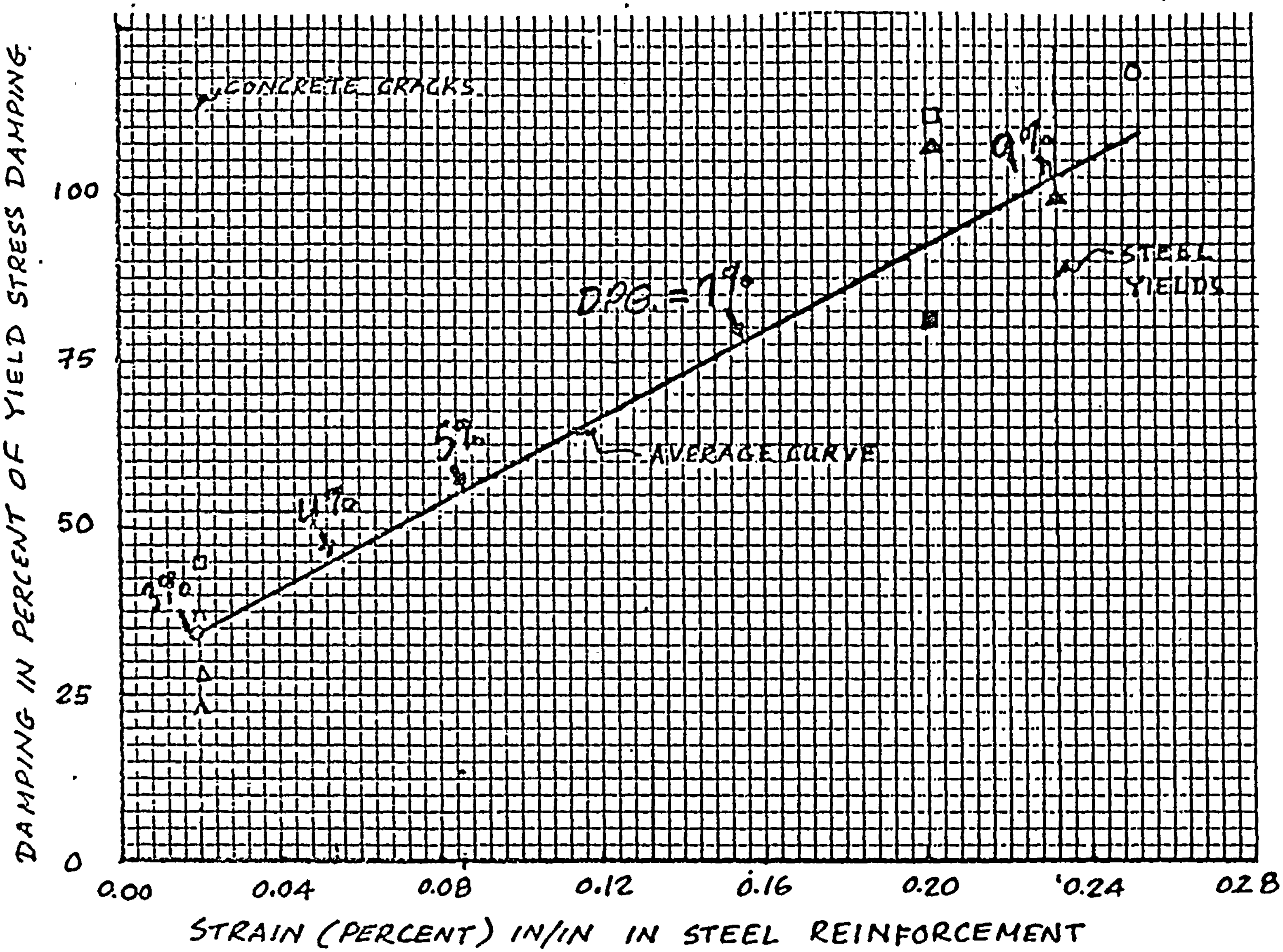


FIGURE 21

(FIGURE 49-I IN D-LL 49)

DAMPING VS STRAIN LEVEL IN THE PCA SPECIMENS

BLUME

mpb1

MRS. BOWERS: Is there any other matter that we need to consider before we recess today?

(No response.)

MRS. BOWERS: Well, we'll recess, then, until 8:30 Monday morning.

(Whereupon, at 4:15 p.m., the hearing in the above-entitled matter was recessed, to reconvene at 8:30 a.m., December 18, 1978.)