April 12, 1990

Docket Nos. 50-275
and 50-323

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Mr. J. D. Shiffer, Vice President
Nuclear Power Generation
c/o Nuclear Power Generation, Licensing
Pacific Gas and Electric Company
77 Beale Street, Room 1451
San Francisco, California 94106

Dear Mr. Shiffer:

SUBJECT: TRANSMITTAL OF DOCUMENTS AND REQUEST FOR ADDITIONAL INFORMATION RELATING TO NRC STAFF REVIEW OF DIABLO CANYON LONG TERM SEISMIC PROGRAM (LTSP) (TAC NOS. 55305 AND 68049)

Enclosed are two consultant reports that relate to the NRC staff review of the Diablo Canyon Seismic Reevaluation Program. The enclosed material is as follows:

1. Letter dated March 1, 1990 from Keiiti Aki of USC, to Jean Savy of LLNL; subject: Letter report on reviewing the PG&E response to questions 1 through 19 (the questions were issued by the NRC by letter dated .

2. Letter dated March 12, 1990 from Ralph J. Archuleta of UCSB, to Jean Savy of LLNL; subject: Letter report on reviewing the PG&E response to questions 6, 8, 15, 16, 17, 18, and 19.

In order to maintain our review schedule, we request that you address the comments contained in the enclosures at the ground motion meeting to be held on April 30 and May 1, 1990. If you have any questions regarding this request, please contact me.

Sincerely,

original signed by

Harry Rood, Senior Project Manager
Project Directorate V
Division of Reactor Projects - III,
IV, V and Special Projects
Office of Nuclear Reactor Regulation

Enclosures: as stated

cc w/encl: See next page

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UNITED STATES
NUCLEAR REGULATORY COMMISSION
WASHINGTON, D. C. 20555

April 12, 1990

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Nuclear Power Generation
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cc w/encl: See next page

Mr. J. D. Shiffer
Pacific Gas and Electric Company

Diablo Canyon

cc:
Richard F. Locke, Esq.
Pacific Gas & Electric Company
Post Office Box 7442
San Francisco, California 94120

Ms. Sandra A. Silver
660 Granite Creek Road
Santa Cruz, California 95065

Mr. Peter H. Kaufman
Deputy Attorney General
State of California
110 West A Street, Suite 700
San Diego, California 92101

Managing Editor
The County Telegram Tribune
1321 Johnson Avenue
P. O. Box 112
San Luis Obispo, California 93406

Ms. Nancy Culver
192 Luneta Street
San Luis Obispo, California 93401

Regional Administrator, Region V
U.S. Nuclear Regulatory Commission
1450 Maria Lane, Suite 210
Walnut Creek, California 94596

Mr. John Hickman
Senior Health Physicist
Environmental Radioactive Mgmt. Unit
Environmental Management Branch
State Department of Health Services
714 P Street, Room 616
Sacramento, California 95814

NRC Resident Inspector
Diablo Canyon Nuclear Power Plant
c/o U.S. Nuclear Regulatory Commission
P. O. Box 369
Avila Beach, California 93424

Bruce Norton, Esq.
c/o Richard F. Locke, Esq.
Pacific Gas and Electric Company
Post Office Box 7442
San Francisco, California 94120

Dr. R. B. Ferguson
Sierra Club - Santa Lucia Chapter
Rocky Canyon Star Route
Creston, California 93432

Chairman
San Luis Obispo County Board of Supervisors
Room 270
County Government Center
San Luis Obispo, California 93408

Michael M. Strumwasser, Esq.
Special Assistant Attorney General
State of California
Department of Justice
3580 Wilshire Boulevard, Room 800
Los Angeles, California 90010

cc:

Dr. Keiiti Aki
Department of Geological Sciences
University Park
University of Southern California
Los Angeles, California 90089-0741

Dr. Ralph J. Archuleta
Department of Geological Sciences
University of California Santa Barbara
Santa Barbara, California 93106

Dr. Robert D. Brown, Jr.
U.S. Geological Survey
Mail Stop 977
345 Middlefield Road
Menlo Park, California 94025

Dr. David B. Slemmons
Center for Neotectonic Studies
MacKay School of Mines
University of Nevada-Reno
Reno, Nevada 89557-0047

Dr. Robert Fitzpatrick
Building 130
Brookhaven National Laboratory
Upton, New York 11973

Dr. C. J. Costantino
Building 129
Brookhaven National Laboratory
Upton, New York 11973

Dr. Michael Bohn
Sandia Lab. - Organization 6412
Post Office Box 5800
Albuquerque, New Mexico 87185

Dr. Steven M. Day
Department of Geological Science
San Diego State University
San Diego, California 92182

Dr. George Gazetas
Dept. of Civil Engineering
212 Ketter Hall
SUNY-Buffalo
Buffalo, New York 14260

Dr. Jean Savy
Mail Code L-196
Lawrence Livermore National
Laboratory
P. O. Box 808
Livermore, California 94550

Dr. Anestis S. Veletsos
5211 Paisley Avenue
Houston, Texas 77096

Dr. Ken Campbell
U.S. Geological Survey
P.O. Box 25046, Mail Stop 966
Denver Federal Center
Denver, Colorado 80225

Dr. M. K. Ravindra
EQE
3150 Bristol Street, Suite 350
Costa Mesa, California 92626

Dr. J. Johnson
EQE
595 Market Street - 18th Floor
San Francisco, California 94105

DEPARTMENT OF GEOLOGICAL SCIENCES
TELEPHONE: (213) 743-2717

March 1, 1990

Dr. Jean Savy
MS L-196
LLNL
P. O. Box 808
Livermore, CA 94550

Dear Jean:

This is my letter report on reviewing the P.G. & E. response to questions 1 through 19.

Let me follow the list of issues I raised in my letter to you dated March 9, 1989. The first one was the issue of the selection rule, especially, the effect of including the Parkfield and Morgan Hill earthquake data. This issue was addressed in three questions, namely, 2, 13, and 14. The P.G. & E response to these questions reveal that the effect of the selection rule is small but significant. The effect is greater for frequencies lower than 5 Hz, and the expanded data base gives the result very similar to the site specific spectrum, and we can no longer say that the latter envelops the former. The peak value of the 84th-percentile spectra indeed exceeds that of the site specific spectrum by about 15% if soil site records were excluded from the data base. This confirms the diluting effect of soil site records, although the reduced sample size may also be responsible at least in part as claimed by P.G. & E.

My second issue was actually a non-issue, because I agreed with P.G. & E. about the surprising magnitude dependence of data dispersion. The response to Question 6 gives further convincing argument that the observed magnitude dependence is not due to the artifact of data collection and processing. The explanation offered by Steve Day and accepted by P. G. & E. that the reduction of dispersion for large earthquakes may be due to the fact that the contributions from many segments of a fault plane tend to average and smooth the effect from each segment is also acceptable to me.

The third issue I raised in my March 9 letter was with regard to the use of the word "empirical source function". This is confusing and misleading because it does not represent the source effect of the target earthquake, but is simply the empirical Green's function corrected for the propagation effect in a very crude manner. Furthermore, the word "empirical source function" wrongly suggests that it may not include the effect of the recording site. I repeat this objection because P.G. & E. still use the word "empirical source function" in the responses to Questions 4 and 7.

A more fundamental issue is about the physical model of faults used for the numerical simulation. In the response to Question 18, P.G. & E. introduces three stress drops, namely, global static stress drop, global rupture duration stress drop, and local stress drop. I can follow the definition and description of the first two stress drops given by P.G. & E., but cannot accept those of "local stress drop".

First, global rupture duration stress drop as defined by Somerville and others (1987) is very close to the so called "Brune's stress drop" if the corner frequency is replaced by the reciprocal of the rupture duration. The physical significance of this stress drop as used in strong motion simulation is unclear. For example, Boore and Atkinson (1987) stated "this parameter is known by several names; we prefer to refer to it simply as the stress parameter and thereby not attach any physical significance in terms of fault models".

On the other hand, the local stress drop defined for the specific barrier model of Papageorgiou and Aki (1983) referred in the P.G. & E. response to Question 18 has a clear physical meaning. Their model is composed of circular cracks of the same size filling a rectangular fault plane, and the local stress drop is the static stress drop occurring for each circular crack.

Since the local stress drop is assumed to be the same for all cracks in the specific barrier model, it will be different from the global rupture duration stress drop, unless one crack occupies the whole fault plane.

The final model of P. G. & E. for the Hosgri fault (as i interpreted in my March 9 letter) is a fault plane filled with 4x22 subevents, each occupying 3x4 km^2 area, with the slip velocity of 50 cm/s and local stress drop of 50 bar. This is very similar to the specific barrier model with local stress drop of 50 bar except for the introduction of asperities by allowing a variation of stress drop from a subevent to another.

I am very much confused now because earlier I understood that the stress drop now called "global rupture duration stress drop" by P. G. & E was the local stress drop of subevent similar to the circular crack of the specific barrier model. Now, by definition given in the response to Question 18, it is defined as the Brune stress drop with the corner frequency replaced by the reciprocal of duration.

I wonder if this serious inconsistency is something existing only in my mind because of my misunderstanding of the physical model of P.G. & E., or is due to the nebulous nature of their physical model as reflected in the statement of Boore and Atkins mentioned earlier. In any case, since the model of P.G. & E. should have a clear physical meaning, it must avoid the use of a parameter regarded as something to which any physical significance in terms of fault models should not be attached.

In response to Question 18, the local stress drop estimated by Papageorgiou and Aki (1983) is quoted as 200 to 370 bars. This estimate was made without the consideration of local site effect on the strong motion accelerograms. Recently, Aki and Papageorgiou (Proc. of 9th World Conf. Earthq. Eng., Aug. 2-9, 1988, Tokyo-Kyoto, Japan, Vol. VIII, P. 163-169) revised the stress drop using the frequency dependent site effect estimated by Phillips and Aki (1986). The revised value now ranges from about 100 to 200 bars.

The fifth issue raised in my March 9 letter is satisfactorily answered by responses to Questions 9 and 11. The reliable frequency range of the numerical simulation is above 2 Hz.

The sixth issue is the step-by-step explanation of errors in the numerical simulation, and was addressed in detail by P.G. & E. in the response to Question 7. This response gives a clear documentation of the quantitative measure of the goodness of fit for strong motion simulation used for estimating the uncertainty of simulated ground motion. The

separation of uncertainty into three parts, namely, modeling, random and parametric, helps to clarify the sources of uncertainty in strong motion simulation.

It is clear that the important sources of uncertainties are (1) smallness of sample size presently available for validation, and (2) some arbitrariness in the choice of parameters for future earthquakes. I wonder if it is possible to include the Loma Prieta earthquake data for further validation.

Finally, there are several questions raised with regard to the topographic effect on strong ground motion. In none of the responses to these questions, the P. G. & E. paid attention to the anomalous effect expected for the SV incidence at the critical angle as discussed by Kawase and Aki (BSSA, 80, No. 1, 1990) and Nava et al. (Seismological Research Letters, 60, No. 6, 1989). The anomalous effect described by Kawase and Aki for the Whittier Narrows earthquake indicates that the Diablo Canyon site topography effect due to SV waves from the sources in the ocean side may be "deamplification" rather than "amplification". This is relevant to Question 12.

The anomalous effect at the critical incidence is also relevant in the interpretation of the Nahanni records at sites 1 and 2 in terms of a source coupled site effect.

Sincerely yours,

Keiiti Aki

KA:st

UNIVERSITY OF CALIFORNIA, SANTA BARBARA

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF GEOLOGICAL SCIENCES

SANTA BARBARA, CALIFORNIA 93106
FAX (805) 961-2314

March 12, 1990

Dr. Jean Savy
Mail Stop L-196
Lawrence Livermore Laboratory
P. O. Box 808
Livermore, CA 94550

Dear Jean,

Having read PG&E's responses to questions 6, 8, 9, 15, 17, 18 and 19 there are a few comments and some questions of my own. Before addressing each of these responses, there is a fundamental dilemma presented by the PG&E methodology for numerical simulations. Namely, if the radiation coefficient that modulates the seismic radiation is homogenized (made practically isotropic), what then is the difference between a thrust faulting mechanism and a strike-slip faulting mechanism? As far as I can determine, there isn't any. My reasoning goes as follows. If slip occurs on a fault plane prescribed by its strike and dip, the angle which determines the style of faulting is the rake. Because the dip and strike angles are fixed by the geometry, the only angle left to be homogenized is the rake. Thus homogenization of the radiation is equivalent to randomizing the rake which in turn is equivalent to randomizing the sense of slip, i.e., randomizing the style of faulting. If this is true, all differences between thrust, strike-slip and oblique faulting are due to geometry, not style of faulting. This is particularly bothersome given that the numerical simulations are validated against data, e.g. Question 8, comparing styles of faulting and Question 17 comparing empirical data with numerical simulations.

Question 6.

This analysis seems complete. Physically it makes sense with what Steve Day once said. Namely, one might expect less dispersion with the larger magnitude events because the larger the fault plane the more opportunity there is for different rays to interfere with each other. Again one has to look at what is being considered, peak acceleration. This is a single number. A magnitude 6.5 earthquake might generated 10-15 seconds of strong acceleration. Ten seconds sampled at 100 or 200 samples/sec leaves one with 1000 to 2000 data points from which to pick one number. However, a magnitude 5 earthquake might generate only 3-5 seconds of strong motion. Now, one picks a single value out of 300-500 samples.

As mentioned in the response, "no differentiation was made between soil- and rock-site data in the analysis..." The figures Q6-3 would not support such differentiation even if the assumed regression function allowed for it. Those figures are telling. There is one datum within 10 km for a M 6.8 - 7.4 earthquake showing peak acceleration about 0.45 g; all other data (3 of them) are larger than 0.6 g.

Question 8.

If I understand this correctly, this answer says that peak acceleration and average response spectral level (3 - 8.5 Hz), scale in the same way for different styles of faulting. As noted above, what is the meaning of strike-slip or thrust if the radiation coefficient is homogenized? Even if that were not the case, why would one expect the response spectral level to scale in the same way as the peak acceleration unless the peak acceleration was primarily due to the 3-8.5 Hz radiation. I don't consider the difference between 16% and 19% to be significant given the analysis shown in Question 6.

Question 9.

In PG&E's response to this question there are statements made that may or may not be true. The purpose of this comparison was to validate if possible the truth of such statements. In particular, on Page 2, third paragraph: "However, at the frequencies of importance to the plant site, strong ground motions are characterized by incoherent source and wave propagation phenomena, and these theoretical seismograms methods do not provide adequate representations of the high-frequency ground motions." "these theoretical seismogram methods" refers to frequency-wavenumber. I take exception to this statement. These methods have been used by a number of authors to model strong motion records. The upper limit on frequency is more a matter of computer time than appropriateness. PG&E has certainly not shown any waveform comparisons to indicate that the generalized ray method works. The comparisons are always in the form of a PGA or response spectrum. The PGA comparison is almost useless. Again one generates 2000-3000 acceleration points. The maximum value is selected and compared with the maximum number from some recorded accelerogram that also contains 2000-3000 points. Given that the stress drops are approximately correct as they will be since one doesn't find 10 m of slip on areas of 100 m, and allowing for conservation of energy, i.e., 1/R attenuation which is supported by the data, why shouldn't one of the 2000-3000 numbers agree with one datum? The PGA from the numerical simulations doesn't even have to be within one second of the time for the PGA in the recorded accelerogram. There is no attempt at waveform modeling. Likewise, response spectrum comparisons ignores the phase information in the accelerogram. This statement by PG&E is based on other authors' attempts to model waveform data. To say that these other methods are inferior contradicts the statement made by PG&E on Page 6, third paragraph: "At frequencies above 2 Hz, there is generally good agreement between the response spectra generated using the two methods, and there is excellent agreement in peak acceleration."

Comparing the computed accelerograms using the two methods, Figures Q9-1, Q9-2, it seems clear that the generalized ray method is giving a limited view of the ground response. To say that the multiples are coherent, and thus irrelevant, presupposes the answer. One has to wonder what an accelerogram would look like if the full Green's functions (frequency-wavenumber) were used and not a simple set of generalized rays. Although the comparison might be more exact between the tangential components for reasons given in the response, what do the radial and vertical components look like for the frequency wavenumber computations? I would like to see those plots at the next meeting.

The averaged response spectrum is presumably the average response of the 22 traces shown in either Figure Q9-1 or Figure Q9-2. Which Figure, i.e., which depth? It's clear that the frequency-wavenumber method produces a significant peak in the response spectrum in precisely the 3-8.5 Hz range. Is this important to the engineering analysis that follows?

Question 15.

The numerical simulations were not used to assist in determining the relations for magnitude and distance. Why? I thought a primary concern was the lack of data within 10-20 km. Thus the numerical simulations were meant to fill in that data gap. The curve in Figure Q15-1 is

constrained by only seven data points. The curves are extrapolations from distances greater than 20 km (See Figure Q6-3). The regression analysis used by PG&E has a pseudo distance factor that dominates at the 4.5 km epicentral distance of the site. The pseudo distance is the factor 0.616 exp (0.524 M) which is equal to 27 km for a M7.2 earthquake and 19 km for a 6.5. This pseudo distance is added to the epicentral distance (4.5 km) and used for the regression analysis. In short, the distance of 4.5 km makes a 25% difference for a 6.5 and about a 20% difference for a 7.2..I have always been confused why the peak acceleration should have a distance dependence like $1/R^2$. (1/R from geometrical spreading and 1/R from intrinsic attenuation?)

I recall PG&E (Sadigh) once showing that the footwall accelerations were no different from the hanging wall. That is contradicted in the statements concerning the style of faulting. How can the average radiation coefficient for a thrust event be larger than a strike-slip event. For example, consider a vertical fault. In one case the style is strike-slip, in the other reverse. Would two such faults produce equal amplitudes within one fault depth?

Question 17.

The style of faulting is the one number consistently less than 1.0 in converting from thrust event recordings to strike-slip for the site. Again the issue of style of faulting indicates that thrust events produce higher accelerations. Yet, in the response to Question 18 PG&E argues that stress drops are not higher for thrust compared to strike-slip.

Question 18.

The average stress drop of 50 bars and the local stress drop of 500 bars seems to encompass a wide range of possibilities. The numerical simulations may be overestimating the strike-slip ground motion if a real difference exists between thrust and strike-slip faulting.

Question 19.

The issue of directivity is controversial. UC Berkeley will get a different M_L from Caltech and seismologists attribute this to directivity. M_L though is based on 1.0 Hz, a fairly high frequency. Differences for the 1922, 1934 and 1966 Parkfield earthquakes are noted for stations south and north of the epicentral region. Is this directivity? I question whether average horizontal peak accelerations will ever show directivity. Why average the results from four test cases? Why not show each one separately and let us see what the results look like? I do like the plots in the figures for this question; I would have preferred to see each of the four runs separately plotted in this manner.

Sincerely,

Ralph

Ralph J. Archuleta