Enclosure

Lawrence Livermore National Laboratory

NUCLEAR SYSTEMS SAFETY PROGRAM

January 13, 1986
EG-86-009

Robert L. Rothman, Geophysicist
Engineering Branch
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U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Bob:

Please find enclosed the commentsof the LLNL members of the advisory panel regarding the PG&E long term seismic program.

I must say that I was surprised to discover the status of this project for PG&E and I am questioning the fact that, in light of the complexity of the problem, the requirements put forth for the assessment of the strong motion could be fulfilled in the limited time remaining. This apparently also was an important concern of the three advisory panel members (APM).

In addition to the comments of the APMs I noted the following:

A member of the PG&E team described the important effort planned for the soil structure interaction part of the study, mentioning that several instruments have been located in the existing structures and in the free field. Since some records have already been obtained from these instruments, I think that PG&E should plan to use them to derive purely data based transfer functions, rather than using numerical modeling, every time it is possible. If they really can calculate the transfer functions from the data for a few locations, this would provide the possibility of validation of their numerical models. It might have been a misunderstanding on my part, but it did not seem that PG&E had plans to perform this validation.

Sincerely,

Jean B. Savy
Engineer, Geosciences Group

JBS:sc

DEPARTMENT OF GEOLOGICAL SCIENCES
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December 16, 1985

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Dear Jean:

This is in response to your request for my comments on the Diablo canyon nuclear power plant--seismic reevaluation program meeting with LLL advisory board and PG&E which was held in San Francisco on December 12, 1985.

I was very much disappointed by the PG&E presentation on their numerical ground modeling program, because they did not come out with a stronger program remedying weakness I noticed in their Long Term Seismic Program Plan (Jan. 1985). Furthermore, they appeared to have gone backward and still undecided about what to do in the future. I feel that the weakness in the program plan might have caused uneasiness about the numerical modeling approach among the experts of PG&E.

The major weakness of the modeling approach is our ignorance about model parameters. We are told in the meeting that the frequency range of engineering interest with regard to the Diablo canyon plant is 2 to 20 HZ. Since greater details about the earth structure and fault rupture process must be known for deterministically simulating ground motions at higher frequencies, we must resort to stochastic modeling for frequencies higher than a certain critical frequency.

I feel strongly that there is a need for stochastic modeling for a significant part of the above 2 to 20 HZ range. Neither the January 1985 program plan nor the presentation at the December 12 meeting paid enough attention to this important problem. They must clearly identify the frequency range to which the deterministic approach can be applied in view of available knowledge on the earth structure and rupture process, and propose how to go about estimating model parameters in the stochastic modeling of rupture process and wave propagation.

A hybrid approach of deterministic and stochastic modeling may also help to define the coherent and incoherent components of ground motion, and clarify the question of the so-called tau effect.

Keiiti Aki

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ADVISORY REPORT ON MEETING OF 12 DECEMBER 1985

The following is my report, as a member of the Nuclear Regulatory Commission advisory panel, on the 12 December 1985 meeting between the N.R.C. staff and Pacific Gas and Electric Company. The advisory panel was convened for the purpose of reviewing theoretical (numerical) ground motion estimation to be performed under P.G. & E.'s Long Term Seismic Program (LTSP). I will first summarize what I consider the main accomplishments of the meeting of relevance to theoretical ground motion estimation, and then offer my own comments on P.G. & E.'s presentation and the ensuing discussion at the meeting.

Summary

Two issues relevant to theoretical ground motion estimation for the Diablo Canyon site were aired at the meeting. First, P.G. & E. described the requirements of their structural analysis and soil-structure interaction analysis programs for input from the ground motion analysis tasks of the LTSP. These requirements are very specific, and they should be primary considerations in evaluating the relative weight to be given empirical versus theoretical ground motion estimation techniques under the LTSP. The requirements cited by P.G. & E. are:

1. Frequency content 2 to 20 Hz.
2. Realistic time history information, including duration, amplitude decay, and apparent dispersion
3. Realistic partitioning of ground motion between vertical and horizontal components, including reasonable representation of time lags between maximum amplitudes on the two components
4. Wave type and phase velocity information as a function of time
5. Realistic representation of spatial coherency of motion

It was emphasized that some input from the ground motion tasks would be required within the next year or less. Thus, the scope and weighting (theoretical versus empirical) of the ground motion estimation work should be designed to maximize information of the above type, with minimal uncertainties, and with the best prospects for substantial results within 6 months to a year.

The second accomplishment of the meeting was that P.G. & E. communicated to the N.R.C. and the panel its intent to modify the theoretical modeling task (Chapter 6) of the LTSP Plan of January 1985, but was not prepared to propose specific changes at this time. My impression is that a substantial deemphasis of the theoretical modeling task is being considered. For example, doubt was expressed about the feasibility of meeting the above structural analysis goals in a timely manner using theoretical earthquake ground motion methods, on the grounds that the theoretical methods are still in the research stage. At least 3 different speakers expressed this view by characterizing the original P.G. & E. theoretical ground motion task as "an NSF project".

Comments

My comments will focus on the appropriateness of using theoretical modeling to achieve P.G. & E.'s engineering objectives as outlined above. As noted by Leon Reiter at the meeting, theoretical modeling has been used previously as an adjunct to empirical procedures, providing independent response spectral estimates for the San Onofre Nuclear Generating Station site. That work, by Del Mar Technical Associates, represented a major advance in the state of the art in earthquake ground motion simulation. No comparable technical advancement would be required to perform a similar study now for the Diablo Canyon site. The primary challenge would be to incorporate up-to-date observational and theoretical understanding of the earthquake source into the proven numerical procedures. The effort might be compared in scope to the effort required to incorporate recently acquired strong motion data into existing empirical procedures. My opinion is that, with perhaps a 3 to 6 month effort, credible theoretical predictions could be obtained for the low-frequency ground motion components at the Diablo Canyon site. By low frequency, I mean frequency components up to several Hertz (i.e., roughly those frequencies controlling peak velocity). In this

Steve Day

regime, theoretical models have the potential to provide much more complete information about site-specific ground motion than does the small near-field strong motion data base. In particular, there is no other convincing way to quantify uncertainties introduced by the possibility of localized asperity rupture near the site or by rupture directivity toward the site.

Above a few Hertz, a sound physical basis is lacking for characterizing the earthquake source. A theoretical treatment adequate for high frequency response spectrum estimation will certainly have a large stochastic component. The approach proposed in the LTSP, Chapter 6, was to parameterize this component in a physically plausible manner and then fit the free parameters to a portion of the existing strong motion data base. This seems to me to be an excellent approach, which is conceptually superior to the purely empirical approach of fitting simple curves to the strong motion data. I would not expect this theoretical approach to supercede conventional methods in less than 2 years or so, however. Thus, I agree with P.G. & E. that substantial research is required before the Chapter 6 approach yields a reliable methodology for establishing absolute design spectral levels at high frequency.

On the other hand, I don't see how the above structural analysis requirements can be met without a major research effort of some kind. Standard empirical procedures cannot provide the time history information and spatial characterization specified by P.G. & E. For example, structural analysis requirement 4 above calls for wave type and phase velocity (angle of incidence) characterization of the earthquake ground motion. This requirement can be addressed either in an elaborate experimental research program (i.e., by array recording of appropriately positioned artificial sources), or through a theoretical modeling program. My opinion is that even preliminary theoretical models will provide a far more reliable characterization of wave type and angle of incidence distribution from a large earthquake than are likely to be obtained experimentally during the life of the LTSP. At best, a few carefully designed experiments could be used to validate the theoretical method; however, the experimental approach is severely handicapped, in that artificial sources cannot emulate the geometry of a large earthquake. Similar considerations apply to requirements 2 and 3.

In summary, I believe that a theoretical modeling program along the lines proposed in Chapter 6 of the LTSP would be 1) a valuable adjunct to empirical methods for establishing design spectral values, and 2) the preferred tool for characterizing the time histories and spatial characteristics of ground motion for input to soil-structure interaction analysis. The stated objectives of Chapter 6 are feasible on a 2 to 3 year time scale, and it is realistic to expect intermediate results of engineering importance in less than 1 year. A few field experiments would be useful to augment and validate the theoretical method, but experimental results alone cannot be expected to realistically characterize either the time histories or spatial characteristics of site specific ground motion a few kilometers away from a magnitude 7+ earthquake.

Steve Day.

Comments on Meeting between the NRC and PG&E,
Dec. 12, 1985
San Francisco, California

Ralph J. Archuleta

Specific Comments:

(1) *New Instrumentation:*

The concept of putting more instruments within the local area is excellent. However, the instrumentation plan itself needs more consideration in light of the site specific questions germane to the plant. As the plan is presented the data obtained will, in all liklihood, raise more questions than answers. True, the additional vertical seismometers will provide better constraints on the hypocentral locations of earthquakes in the area. The current design used for telemetering the seismogram to a recording device is notorious for its clipping of the signal. As a consequence, the most important information related to ground motion, the S-waves, is totally absent. While it is true that these instruments are designed primarily for recording small earthquakes, the usefulness of the recorded S-wave motion as potential empirical Green's functions cannot be overlooked. Nor can one overlook the utility of using S-waves to constrain the S-wave velocity structure and the attenuation structure. Although it may not be possible for all of the stations to be three-component, certainly 2/3 to 3/4 should be, particularly those closest to the plant itself. This instrumentation should planned carefully such that when the velocity transducers are physically incapable of correctly recording the motion, i.e., the pendulums are actually hitting the stops in the transducer, that the accelermoters will be triggering. There should be at least 20 percent overlap of the range of ground motion covered by both the velocity transducers and accelermoters.

Considering Mr. White's presentation of engineering needs, it was surprising that there was no mention of installing a small operature array near the site. Mr. White specified such information as: directions of arrival of seismic waves, spatial coherence as a function of frequency, wavenumber/frequency spectrum of waves. This type of information is precisely the type provided by data obtained on small aperture arrays. The proposed instrumentation, including the current location of the accelerometers near the site, is inadequate to provide this information. The areal network would be well complemented by this array. In addition it would provide data germane to the engineering needs and be site specific.

(2) *Seismic Plans:*

Mr. Brady enumerated four primary areas that pertain to site specific hazard mitigation for the plant: (1) empirical ground motion models, (2) incorporation of

recent data into the existing data bases, (3) wave propagation and site effects, and (4) numerical methods. Although PG&E might have specific approaches in mind as to how one performs the tasks under each of these topics, Mr. Brady was vague as to what these approaches would be. What was clear was the emphasis on empirical models over numerical methods. Items (1), (2) and (3) all dealt with gathering data or incorporating data into models. Naturally, data are the principal ingredients for any evaluation of the seismic hazard. However, simply using data to reduce the variance on a regression curve of peak acceleration versus distance or magnitude or rock type or fault plane solutions or whatever independent variable one wants to choose is unlikely to produce a solid answer as to the risk posed by the nearby faults. As was pointed out by one person at the meeting the engineering needs require time histories while the empirical approach provides a single parameter estimate of the ground motion. This incongruence might be remedied by the final, and least emphasized, approach--numerical methods.

There are many questions regarding the cause of ground motion that cannot be answered by simple linear regressions. In fact, data from the Morgan Hill earthquake or Mexico would not be accurately predicted from such regression curves. Adding more data to these types of analysis will probably not make them a better predictor. Understanding the cause of the large accelerations is the basis for making predictions as to what might occur. The most fruitful approach is to undertake numerical modeling of these well recorded earthquakes in order to understand the mechanism that produced the recorded ground motion. Naturally the very high frequencies in the 10-20 Hz range will not be predictable in a point by point time sequence. However, the lower frequencies ($f \leq 1.0$ Hz) may be well predicted with some insight as to how seismic radiation in the critical frequencies, 1.0-10.0 Hz might be generated and propagated. In fact, the extension of the faulting model that adequately predicts ground motion at the lower frequencies to a model that reproduces a correct measure of the ground motion at higher frequencies is a key element in the numerical approach. There are many other questions such as the effect of oblique slip, sub-horizontal faults, rupture velocity, inhomogeneous slip distributions, etc. that can only be answered by using numerical modeling methods.

There was practically no discussion as to how PG&E intends on integrating data from small earthquakes with either their empirical approach, which relies almost entirely on strong motion records, or their numerical methods. The concept of using small earthquakes as Green's functions is well established. The implementation of this approach toward simulation of large earthquakes has had limited success. Importantly it may be the only way to characterize the high frequency radiation that is consistent with the ground motion description needed for engineering purposes.

General Comments:

One of the more striking aspects of the meeting was the mismatch between the engineering needs with respect to ground motion and the seismic plans to provide ground motion estimates. There was very little in common. This should be remedied.

Although PG&E was quite vague about their plans for assessing the seismic hazard at the site, it was clear that their emphasis was on empirical methods, which I interpret as regression analyses. The motivation behind this long term seismic plan is that ten years after 1978 there would be more data and more sophisticated approaches for assessing the seismic hazard at the Diablo Canyon power plant. Regression analysis hardly represents state-of-the-art analysis. True, there are more data so there are more points to be plotted. This type of analysis reduces ground motion to a single parameter. It is equivalent to describing an earthquake by simply quoting a magnitude. In the near-source region such a description of an earthquake is totally inadequate. It comes back to my main point that the seismic plans do not provide the information needed by the engineers.