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Dear Chet:

I participated in the 3-day meeting August 8-10, 1989, held in Rockville/White Flint, concerning the Diablo Canyon Nuclear Power Plant. The meeting was attended by NRC staff, PG&E representatives, and consultants. A partial roster is enclosed. Leon Reiter was the main spokesman and questioner on behalf of the staff. The meeting was informative and useful. Some of the highlights follow, - not in chronological order.

On the second day, the characterization of the Hosgri (as to fault type, slip type, and dip) was discussed again, at length, with bits of new data and new reasoning. All geologists present, including Bob Brown of the USGS, and all geophysicists who gave an opinion, now finally agreed that the Hosgri is most probably a steep strike-slip fault, perhaps with some obliqueness in slip (i.e., a reverse component). The NRC staff appeared to be satisfied and relieved. This rather tardy consensus alone made the meeting a success, because the fault dip and predicted focal mechanism are prime parameters in the logic tree that leads from source to ground motion. The arguments supporting strike-slip included the familiar ones, but were bolstered by re-processed seismic reflection profiles, recognition of flaws in Jim Crouch's thrust fault interpretation, seismicity (a cloud of hypocenters roughly terminates near a hypothetical steeply dipping Hosgri fault), recent focal mechanisms of small earthquakes (including a strike-slip mechanism beneath the trace of the Hosgri), and calculations of horizontal:vertical slip ratio based on the San Simeon slip rate vs. vertical offset of Cenozoic strata in seismic profiles. Thrust faults certainly are present, and some adjoin the Hosgri, which itself may lean a bit. PG&E favors a dip of 70°-90°, which seems reasonable. I enclose a rough diagram which I sent you in the hey-day of the thrust hypothesis. Alternative possible dips and behaviors of the Hosgri fault are retained in branches of the logic tree, but are not very heavily weighted.

Parts of the logic tree were displayed and discussed at length. Leon asked how the weights were chosen for alternative possible parameters for the Hosgri fault. It seems that they were produced subjectively by a group including Clarence Allen, Bruce Bolt, Lloyd Cluff, Kevin Coppersmith, Woody Savage, Cole McClure, Bill Lettis, Tim Hall, Doug Hamilton, and perhaps one or two others. The combined experience, judgment, and responsible reputation of most of these people is first-class. The group met for extended sessions, we were told, reviewed data, expressed opinions, argued, and arrived at numerical weights they could all live with.

Kevin Coppersmith talked about segmentation. The Hosgri fault has been divided into 6 segments, using Knuepfer's empirical world-wide data. The end-points of segments were picked after statistical treatment, described by Bob Youngs (?). Statistics purportedly show which features/changes

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110

along faults actually correlate with differences in behavior of various parts. For example, double bends in the fault trace, fault junctions, large step-overs, differences in vertical slip rate, and other items are deemed to be statistically significant. A distinction is made between fault segments and rupture segments, as rupture in 1 event can involve 2 or more fault segments. Statistical analysis of empirical data has been more successful in identifying the kinds of end-points of segments that may be ignored (transected) by propagating rupture than in identifying points that will stop a rupture. This kind of study is worthwhile, but not yet very useful.

Coppersmith reported on the use of moment magnitude (M_w) rather than surface wave magnitude (M_s) in characterizing the results of faulting. As I understand it, a given amount of slip gives the same magnitude (M_w) regardless of the mode of faulting (strike-slip, reverse, or oblique). However, the type of source mechanism for a given amount of slip might affect the ground motion, if not the magnitude.

Burt Slemmons presented new plots and curves relating dimensions of surface faulting to EQ magnitude, based on world-wide data on all kinds of faults. He divided the faults into two groups, those in contractional domains, and those in extensional domains. In one study, he used average displacement along the fault, rather than maximum displacement. These procedures produced only small differences from earlier plots of displacement vs. magnitude.

During the course of the meeting, it became evident that virtually all attendees were satisfied with the M_s 7.2 magnitude chosen by PG&E for the maximum credible earthquake from the Hosgri fault.

Two small but bothersome geologic structures received much attention because of their proximity to the Diablo plant: the San Luis Bay fault and the Olson fault (?). These do not worry me because: they do not display the juxtaposition of unrelated rock units that would signify large displacement; although seen locally at the ground surface, they cannot be traced, and are therefore probably minor; their low slip rates indicate that tens of thousands of years would be required before a 1 m slip could occur. These faults could not produce an M 6 earthquake, much less an M 7.

In the end-of-meeting caucus of the NRC staff and consultants, the usual demands for more work by PG&E were toned down a bit. Most of the staff suggested things that are still needed, but they seemed to agree that field work is reaching the point of diminishing returns. Some amplifications and clarifications of the written record were requested, inluding full explanation of the manner in which key decisions and judgments were made. It was proposed that a small group of USGS geologists/geophysicists should spend time with PG&E counterparts in going over certain critically located seismic profiles and other types of observations (mostly offshore) that have not yielded useful information hitherto, or that have yielded confusing results. I urged the staff to include George Thompson.

The presentations and discussions were excellent. The staff asked some important questions. The geologic/seismic investigation is drawing to a close, and probably should not be prolonged much farther.

With all good wishes,

Ben

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Benjamin M. Page