RELATED CORRESPONDENCE

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of)
) Docket Nos. 50-361 OL
SOUTHERN CALIFORNIA EDISON COMPANY,) 50-362 OL
ET AL.)
)
(San Onofre Nuclear Generating Station)
Units 2 and 3))

WRITTEN TESTIMONY OF JAMES N. BRUNE

I. INTRODUCTION

My name is James N. Brune. I am Professor of Geophysics at the University of California at San Diego. My educational background includes a Bachelor of Science degree in Geological Engineering from the University of Nevada, and a Ph.D. in Seismology from Columbia University. In recent years I have carried out a number of studies relating to seismicity and tectonics of southern California and northwest Mexico, and to earthquake source mechanism and strong ground motion, particularly in southern California and Mexico. I am currently a principal investigator on contracts and grants funded by the United States Geological Survey and the National Science Foundation which pertain to earthquake hazard in southern California and northwest Mexico (USGS), strong ground motion in northwest Mexico (NSF), earthquake mechanism and strong motion along the San Jacinto fault (USGS) and special studies of strong motion generated by the Imperial Valley 1979 earthquake (NSF).

my biography and list of publications is attached hereto and incorporated herewith.

II. SUMMARY AND CONCLUSIONS

(1) The state of our scientific knowledge is too limited to allow us to predict with confidence the maximum magnitude and consequent maximum ground accelerations to which the San Onofre Nuclear Power Plant may be exposed during its lifetime. The existing SER and FSAR reports do not counter these limitations. New and untested methods are introduced without adequate justification. Undefined and unscientific terms are used in conclusions. Assumptions are left unjustified and important data are not adequately considered. As a consequence of the above limitations, the adequacy of the design basis earthquake has not been established by scientific evidence.

(2) A proposed design earthquake of magnitude $M_s = 7$ is not the maximum which could reasonably occur on the OZD. Based on the length of the OZD, our present understanding of the mechanics of earthquakes, the statistical distribution of earthquakes with magnitude, and the tectonics and historic seismicity of the OZD-Southern California-Northern Baja California region, it is quite possible that an earthquake of magnitude greater than 7.5 could occur along the OZD.

(3) The slip-rate methodology introduced by the applicant is not valid for estimating the maximum magnitude earthquake on the OZD. It has not been verified by evidence, submitted to adequate critical review, nor established from principles of fault mechanics. The data cited in support of

it can be explained by known statistical properties of occurrence of earthquakes, and the fact that only a short history of seismic observations is available.

(4) Methods of estimating maximum magnitude based on some assumed fraction of rupture along a given fault zone have not been verified by evidence, submitted to adequate critical review, nor established from principles of fault mechanics. Choice of smaller fractions of rupture length represent probabilistically more likely events and not maximum events in the sense of true mechanical limitations.

(5) Because the data base of recordings of ground accelerations of large earthquakes ($M \sim 7$ and greater) at close distances (< 10 km) is very limited, and because our physical understanding of earthquake source mechanism and generation of strong ground motion is very limited, we cannot confidently establish expected values of ground motion for San Onofre from a major earthquake on the OZD. Different investigators have obtained significantly different results from correlations of peak ground acceleration with magnitude and distance, using essentially the same limited data base. Present evidence indicates that peak horizontal and vertical accelerations could exceed the SONGS design earthquake values. The probabilities of such exceedances are not known, but such exceedances have occurred several times in recent earthquakes.

(6) Numerical modelling procedures have not yet

been developed and utilized sufficiently to be relied upon in predicting ground motion, and have not at present been able to constrain expected values of ground acceleration significantly beyond the unreliable constraints arising from correlation of peak acceleration with magnitude and distance based on assumed curve shapes.

(7) The Imperial Valley earthquake of October, 1979 (IV-79), $M_s \sim 6.5$, had a relatively low intensity compared to other earthquakes such as the Imperial Valley 1940 earthquake and the El Alamo 1956 earthquake. Nevertheless, it recorded peak accelerations and response spectra which exceeded the Design Base Earthquake for SONGS. Given the larger magnitude for the design earthquake at SONGS, the lower attenuation in the source region of the OZD, and the relatively low intensity and moment of IV-79, we may expect that exceedance of design values of acceleration and response spectra could occur at SONGS for an M = 7 earthquake on the OZD.

(8) The concept of "magnitude saturation" of observed values of peak ground accelerations with magnitude, previously hypothesized by Hanks and Johnson to apply between M = 5.5 and 6.5, is contradicted in the most recent studies of Hanks and McGuire, and the validity of its applicability for larger magnitudes is in doubt and not supported by data. Therefore, we may expect a general increase in average peak accelerations with magnitude above

M = 6.5, but the slope of the curve is not known at present.

(9) The ground motion which might be caused by high stress drop earthquakes such as occur in Northern Baja California and in certain regions of Southern California have not been adequately considered in the FSAR and SER. The 1956 El Alamo earthquake (M_s = 6.8) generated a much greater area of intensity VI than the IV-79 earthquake, and this suggests that the near source ground accelerations were also considerably greater. Present understanding suggests that such high stress drop events, if they occurred on the OZD could produce ground accelerations considerably higher than the design ground acceleration.

III. GENERAL STATUS OF ESTIMATING EARTHQUAKE HAZARD

The state of our scientific knowledge concerning geology, tectonics, faulting mechanism, and generation of strong ground motion is too limited to allow us to predict with confidence the maximum magnitude and consequent maximum ground accelerations to which a critical facility such as the San Onofre Power Plant may be exposed during its lifetime. The historic record of seismicity, our techniques of geologic investigation (especially in areas covered by water) and our understanding of earthquake mechanism, are all too limited to establish the earthquake potential of zones of deformation such as the Offshore Zone of Deformation (OZD) and the Cristianitos Zone of Deformation (CZD) which may pose a seismic hazard to the San Onofre Power Plant site. Furthermore, once the magnitude for a design basis earthquake is decided upon, the data base for, and understanding of, the generation of strong ground motion, and in particular our knowledge of appropriate critical parameters such as fault dimensions, stress drop, rupture propagation, seismic wave attenuation, and seismic inhomogeneities, are too limited to predict with confidence the probable ground accelerations to be expected for large earthquakes (M_s near 7 and greater) very near faulting (distance less than 10 km).

Our lack of knowledge and the need for further research has been clearly indicated in the report of the Panel on

Earthquake Problems Relating to the Siting of Critical Facilities of the National Academy of Sciences:

> We do not now have the optimal information base that is required to site all critical facilities to protect the citizens of the United States from the hazards posed by earthquakes--surface faulting, strong shaking, ground failure and tsunami. As a consequence, many facilities are 'overdesigned,' undoubtedly, others are 'underdesigned' to resist seismic effects.

Without the further research recommended by the panel, we cannot in general say which is which, and specifically in the case of San Onofre Nuclear Power Plant, whether or not it is underdesigned. As pointed out in their report:

> Major gaps exist in our knowledge of seismic phenomena, and nowhere is this better illustrated than in attempts to specify the locations, frequencies and maximum sizes of future earthquakes that might affect critical facilities--the questions of 'where,' 'how often' and 'how big.' Seldom can all three of these questions be answered with anywhere near the confidence we desire.

In commenting on the specification of maximum earthquakes, the Panel comments:

> Such events have been called the 'maximum credible earthquake,' 'maximum expectable earthquake,' or, with regard to special facilities, 'safe shutdown earthquake' or simply the 'design earthquake.' None of these terms has been precisely defined in a usable way, and what is 'credible' or 'expectable' to one person may not be to another.

Furthermore, the Panel says:

> More often than not, in present practice, the concept of maximum credible or maximum expectable earthquake has some intrinsic elements of both scientific probability and acceptable risk.

In the opinion of the Panel, and I agree, this is not a satisfactory long term state of affairs,

> efforts must be made to separate the evaluation of scientific likelihood of a potentially disastrous event from the assignment of the risk that society is willing to accept for a particular critical facility.

These last sentences are particularly important in the present case because at critical points of conclusion both the NRC Staff and its consultants, and the Applicant and its consultants, use the term "conservative" or some similar term, without defining it, and also use the terms quoted earlier by the NAS Panel as terms not "precisely defined in a usable way." This leads to uncertainty about the meaning of statements and in particular, uncertainty as to the level of risk assumed for society when a particular statement is being made.

Concerning the characterization of ground motions, the Panel comments:

> ...the statistical base of ground motion data is extremely limited.... At present, these estimates [of ground motion] are subject to considerable uncertainties, reflecting the limited historical data base and lack of detailed, quantitative knowledge of the influence of physical factors on ground motions. Data are particularly limited for

> near field and large magnitude earthquakes; unfortunately, such events pose the greatest hazards to structures.

The above quotes from the National Academy Panel represent a generally accepted evaluation of our lack of knowledge concerning earthquake hazard. Neither the Applicant and its consultants, nor the NRC Staff and its consultants have presented evidence that these conclusions are inappropriate for considering the seismic hazard at San Onofre (SONGS), and I believe that they must be taken as a base from which to judge any claims made by the Applicants or Staff to have scientifically established the conservatism and lack of risk associated with the Design Base Earthquake for SONGS. A general description of the evidence presented can be characterized as a compilation of evidence and arguments which support the theory that ground motion corresponding to the design base earthquake (DBE) are not likely to be exceeded, (at some <u>unspecified</u> level of probability), while evidence and arguments to the contrary are inadequately presented. There is no statement that the DBE cannot be exceeded. The scientific evidence is mixed with non-scientific and undefined terms in such a way as to make the conclusions difficult to evaluate and of little use in arriving at a true scientific understanding of the seismic hazard. New and untested methods are presented with inadequate scientific justification. Much evidence which seems to me to be almost irrelevant is presented in great detail whereas critically important simple and true statements

such as "we do not know" or "we do not understand" are omitted, the result being that the truth is clouded and a misleading impression of understanding is given. It is clear that, with the burden of proof to show that the design basis earthquake ground motion could not be exceeded, the burden could not be scientifically held, given our present lack of knowledge.

IV. ESTIMATING THE MAXIMUM MAGNITUDE EARTHQUAKE ON THE OZD

The NRC concluded in its Safety Evaluation Report (SER):

> The present evidence indicates an extensive, linear zone of deformation at least 240 kilometers long extending from the Santa Monica mountains to at least Baja California. We and our consultants consider this zone of deformation to be potentially active and capable of an earthquake whose magnitude could be commensurate with the length of the zone.

I assume that "an earthquake whose magnitude is commensurate with the length of the zone" means that the magnitude corresponds to an earthquake with rupture length corresponding to the length of the zone, since choice of any lesser rupture length would be less than the maximum length of rupture possible on the zone, and an arbitrary reduction in conservatism for which no basis is stated. Using the equation of Slemmons' (1977) Table 13 relation for strike slip faults (Curve E), a fault length of 240 km corresponds to a magnitude of 7.87. If we add one standard deviation, a magnitude of 8.6 is obtained. If we assume a shorter length of 200 km for the OZD, as suggested by Slemmons (SER), we obtain values of 7.76 (mean) and 8.45 (mean plus one standard deviation). If we assume a magnitude corresponding to one half of the 200 km, we obtain values of 7.35 and 8.05 respectively.

A 1967 report to Secretary of Interior Stewart Udall regarding the Bolsa Island Nuclear Power Plant states, in the section entitled "Seismological Considerations" that

> In specifying the maximum earthquake for which public safety must be assured, a highly conservative approach has been adopted for two principle reasons:
>
> (1) The consequences of some types of serious failure in a nuclear facility must be guarded against even if the likelihood is very remote; and
>
> (2) the historic record of earthquake occurrence is so short that it cannot encompass the entire spectrum of possible events.
>
> In view of the mandatory conservatism, we suggest that the maximum earthquake for which public safety must be assumed should be a magnitude 8 shock on the Newport-Inglewood Fault, or on one of the parallel offshore faults (Palos Verdes, San Pedro Faults).

Similarly, other studies have suggested an M = 7.5 and M = 7.25 as the OZD design magnitude (USGS Open File Report, 81-115, (1980); Woodward-Clyde Consultants LNG Report, (1978)).

There is no physical reason why an earthquake rupture could not proceed along the whole length of the OZD. No evidence is presented by the Applicant and its consultants or the NRC Staff and its consultants which suggests that rupture along the full length of the OZD is not possible. However, this is clearly less likely than rupture along some fraction of the total zone, and thus a less conservative assumption would be that only a fraction of the fault would rupture, as proposed by the Applicant and its consultants and by the NRC Staff and its consultants. The choice of a smaller fraction (and consequently choice of a smaller magnitude) is a probabilistic choice with some greater level of risk implied.

Slip Rate Method for Estimating Maximum Magnitude

The use of the geologic slip rate method proposed by WCC is not valid for estimating the maximum earthquake that could occur on the OZD, since there is no known reason why a fault zone of given length with a low slip rate cannot have as large an earthquake as a fault zone of the same length with a fast slip rate. Of course the probability of observing such an event in any given time period (e.g., the relatively short historic time period over which magnitudes have been estimated) is lower the lower the slip rate. Therefore, use of such a method could be considered a probabilistic method for determining the maximum probable event in a given time period, and not a deterministic method for estimating the maximum magnitude which might occur at any time.

Figure 361.38-4 of the Woodward-Clyde Consultants Response to NRC Questions shows a "Line Bounding Extremes of Bracketed Ranges of Data (MEL)." This line is taken by the Applicant to represent the bounding curve which gives an M = 7.0 earthquake for the OZD, taken to have the same slip rate as NIZD, .5 mm/yr (no justification is given for assuming the bounding curve is a straight line rather than a curve). The slope of the bounding curve is controlled by only two points at slip rates below 1 mm/yr, and thus is quite uncertain. As indicated by Slemmons in the SER (p. E-7)

> The data base for these figures is based on a very short historic record of earthquake activity; future earthquake and new data are

likely to extend th limits to
some indeterminate higher value.

This can be appreciated by consideration of one point on the graph, namely, the point for the 1933 Long Beach earthquake. If the magnitude for that event had been a little over one unit higher, $M_s = 7.5$, the slope of the bounding curve would, of course, have indicated $M_s > 7.5$ as a maximum magnitude for a slip rate of 0.5 mm/yr and for the OZD. Thus, the slip rate method begs the question since it assumes a priori that the 1933 Long Beach earthquake is the controlling earthquake for a slip rate of .5 mm/yr. There is no justification for this because the historic record is too short. The 1956 El Alamo earthquake, discussed later, had a magnitude of 6.8, yet the slip rate average over the last several million years is less than for the NIZD. It can be noted that if we had had only two data points from the data above a slip rate of .5 mm/yr, we might have inferred a bounding curve with the opposite slope, i.e., maximum magnitude <u>increasing</u> with decreasing slip rate, a result which might be expected from a rock mechanics point of view, since it is observed in the laboratory that rock strength along faults increases with time between successive failures (Scholz, personal communication, 1981).

Anderson (personal communication,1981) and Luco (personal communication, 1981) have given simple interpretations of bounding curves, such as the MEL presented by the Applicant, in terms of a relationship between maximum magnitude, slip rate and recurrence time for the largest event. In such an

interpretation, the bounding curve could correspond to a recurrence time for the maximum earthquake of 2000 or 5000 years. Thus, there are two possible interpretations of such a bounding curve: either there is some, at present unknown, mechanism operating such that no earthquake falls beyond the curve (as implied by the applicant), or more likely, the curve represents the result of limited sampling of seismicity for which the recurrence time for the maximum earthquake is of the order of 2000 to 5000 years, a reasonable situation for fault zones such as the OZD.

Evidence from Historic Seismicity

From the point of view of the historic seismicity of the OZD, an earthquake with magnitude greater than 7.5 is credible. Since an earthquake of magnitude 6.3 has already occurred on the northern section of the OZD (Long Beach, 1933) and a magnitude 6.8 earthquake has occurred on the San Miguel fault system to the southeast (El Alamo, 1956), and since it is very unlikely that the largest or near-largest event in the region would occur in such a short time sample as our historic record, given the relatively low slip rates, it is credible that a magnitude unit higher could occur at any time.

The San Miguel Earthquakes and Maximum Magnitude Based on the Slip Rate Method

Because of the proximity and the geologic and tectonic relationship of the San Miguel earthquakes to the OZD, they should have been given important weight in testing the slip rate method. The slip rate on the NIZD as given in Woodward-Clyde Consultants Appendix B is about 3.5 km in the last seven million years, giving an average slip rate of .5 mm/yr (Figure B-7). On the other hand, the slip on the San Miguel fault zone in the last seven million years is only approximately 250 meters (Gastil, personal communication, 1981; Harvey, 1980) giving a slip rate of about .04 mm/yr. According to the bounding curve given in the FSAR, Figure 361.38-4, this corresponds to a maximum magnitude of less than 5.5, totally in disagreement with the magnitudes of actual earthquakes (M up to 6.8). This further supports the conclusion that the bounding curve in 361.38-4 is a result of a sampling limitation, not a physical limitation on the magnitudes of earthquakes. If we take the value of .04 mm/yr (averaged over the last seven million years) for the 1956 earthquake, we obtain a bounding curve which gives a magnitude of about 7.5 for the OZD (.5 mm/yr).

The calculated slip rate for the San Miguel fault depends on the time interval over which the averaging is done. If the average is taken since Cretaceous, a value less than 0.01 mm/yr is obtained. On the other hand, if we assume

the displacement all occurred in the last one million years, a rate as high as .25 mm/yr is obtained. The higher rates would be consistent with the thesis that the OZD-San Miguel linear zone is a highly active incipient fault zone.

Thus, the 1956 San Miguel earthquake is further evidence that the slip rate methodology is invalid for estimating the maximum magnitude for the OZD. The occurrence of an M = 6.8 earthquake in this region, on a fault of such low total displacement, and with such a short historic record, argues that the maximum earthquake on the OZD could be considerably larger.

Fault Activity and Stress Away from the San Andreas Fault

A number of times the suggestion has been made that solely because of its distance from the main plate boundary and from the San Andreas fault, the OZD is likely to have a lower stress or to have a lower slip rate, lower seismic activity and lower maximum magnitude (SCE Response to FOE Interrog. 39, 62, 64). Concerning stress, there is no mechanical basis for the assumption that stress decreases away from the plate boundary and, in fact, there are tectonic models in which the opposite is true (Lachenbruch, 1981). The fact that the strength of faults increases with contact time in laboratory models suggests that faults of lower slip rate and thus longer intervals between earthquakes might have higher breaking strengths, higher stress drops and thus, higher magnitudes (for given fault geometry) than earthquakes along main fault boundaries. There are other suggestions that stresses away from plate boundaries are higher than those near plate boundaries (Sbar and Sykes, 1973). The high stress drops and high apparent stresses found along the San Miguel fault zone (see later section) are strong evidence that in this region stress is higher away from the main plate boundary.

Magnitude by Fractional Fault Length

Since it is a well-known fact that for any given fault zone larger earthquakes are less frequent than smaller earthquakes, it is obvious that the longer the observation time for a given fault zone the larger the maximum recorded earthquake is likely to be and, consequently, the larger the ratio of the largest observed rupture length to the total fault zone length, i.e., the larger the fractional fault length of the maximum event. For faults with slower slip rate, the observing time required to record a rupture with a given fractional fault length will be correspondingly longer than for a fault with faster slip rate.

The definition of total length of zone is not straightforward, as pointed out by Slemmons (1977 and SER). For example, almost 100% of the Imperial fault broke in 1940. However, we may extend the "fault zone" to which the Imperial fault belongs to include, to the north, the San Andreas fault, and, to the south, the whole set of transform faults in the Gulf of California, perhaps to the intersection with the East Pacific rise, and thus make the "fractional fault length" for the 1940 rupture anywhere between nearly 100% and less than 10%. Slemmons (SER) identifies the Imperial fault with the San Jacinto fault zone even though the San Jacinto fault is not believed to have been part of the main plate boundary in recent geologic time, while the Imperial fault has. Furthermore, the Imperial fault is offset from

a simple linear projection from the San Jacinto fault to the Cerro Prieto fault. There are, at present, no objective rules for deciding before-hand what the proper length of the total fault zone is. Decisions about how to define the total length of a zone, and what fraction of the zone to take have considerable arbitrariness at present and the method has not been subjected to sufficient critical review.

In the case of the OZD, the northern terminus is fairly clearly defined by the transverse ranges, but the southern end is less certain, as indicated by the various alternatives considered by Slemmons (SER). In fact, the right-stepping distance required to connect the Rose Canyon fault with the Coronado Banks-Agua Blanca system (~15 - 20 km) is about the same as that required to connect the Imperial fault with the Cerro Prieto fault, two faults which Slemmons (SER) includes in the same San Jacinto to Cerro Prieto zone. Thus, if we accept the Imperial and Cerro Prieto faults as part of the same zone for the purpose of estimating fractional fault lengths, there is no geometrical reason for not considering the Coronado Banks fault zone as part of the OZD.

For a given throughgoing linear fault zone, such as the OZD, there is no known reason why the rupture could not proceed along the whole length. Therefore, one might expect, given a long enough recording time, that each such more or less continuous straight zone would have increasing observed

fractional ruptures with time, approaching eventually some large fraction of the total length. The 1930 IZU strike-slip earthquake ruptured nearly 100% of its length (Allen, 1975). Given the short time of historical observations and the inherent difficulties in defining total fault zone length, it appears possible that many relatively straight continuous active fault zones such as the OZD will eventually generate ruptures over nearly their whole length. In this context, the fractional fault length method appears as a probabilistic method, not a determinsitic one. Since a rupture of the whole fault length is less likely than rupture of some fraction of the length, assuming a rupture of length of some fraction of the fault length for a design earthquake is simply less conservative than assuming rupture of the total fault length.

The probability of observing, in a given time period, a rupture along a given fraction of a fault zone can be calculated from the relationship between fault dimensions and magnitude and the recurrence relationship for earthquakes of various magnitudes. Caputo (1973; personal communication, 1981) has made such a calculation and finds that the probability of observing one half length is about 4 times greater than full length, the probability of observing one third length about 8 - 10 times greater than full length, and the probability of observing one quarter length about 15 - 20 times greater than full length. From this consideration, it is unlikely, with the present data sample, that we have recorded near the maximum rupture length possible on the OZD.

Estimating Magnitude from Rupture Length

Slemmons (SER, App. E) has used a regression curve developed by Slemmons (1977) to assign magnitudes to ruptures of a given length. In the calculations given by him in Appendix E, however, he uses the mean curve rather than the curve for a mean plus one standard deviation. Thus, the magnitude values he cites for a given rupture length would be expected to be exceeded 50% of the time. The mean plus one standard deviation value is .694 magnitude units higher than the mean for strike-slip earthquakes. For example, for an assumed rupture length of 62 km (SER, p. E-11) for SCOZD the mean estimated magnitude is 7.07 (expected to be exceeded 50% of the time), the mean plus one standard deviation is 7.77 (expected to be exceeded by about 16% of the data for faults with a rupture length of 62 km) and the mean plus two standard deviations is 8.46 (expected to be exceeded by about 2% of the data for faults with a rupture length of 62 km). These calculations suggest that for rupture lengths of only a fraction (∿1/4 to 1/3) of the length of the OZD an M = 7.5 event is possible.

Synthetic Slip Rate v. Magnitude Curve

Section 361.38(b) in the FSAR is a comparison of the slip rate and half-length methods for estimating maximum magnitudes. This is done using a synthetic slip rate vs. magnitude plot based on two correlations: the magnitude vs. rupture length correlation of Slemmons (1977) and a correlation of slip rate vs. length (Fig. 361.38-3) to obtain a synthetic one-half length line (Figs. 361.38-4, 361.38-5, 361-38.6). However, both of these correlations represent average values, and thus the synthetic slip rate vs. magnitude plot also represents an average line. If the data of Slemmons (1977) for strike slip faults is transferred in the same manner, 50% of the data will fall to the right of the curve, indicating that the bounding curve from the slip rate does not "bound" the data. A more conservative estimate would include a one standard deviation correction (±.694 magnitude units) giving a maximum magnitude of about 7.35. A two standard deviation correction would give a magnitude value slightly over 8.

Strictly speaking, in order to derive a valid "bounding curve", a bounding line should have been drawn for the data in Fig. 361.38-2, combined with a bounding curve for the magnitude vs. rupture length data, and transferred to Fig. 361.38-3. This curve would indicate a magnitude of about 8.5 for a slip rate of .5 mm/yr.

Japanese Earthquakes

In FSAR Question 361.46b it is stated that "The differences in mechanics of faulting between Southern California and Japan has led the Applicants to remove the Tanna fault from the data base together with eliminating all Japanese faults from consideration."

Removing the Japanese data from consideration has a serious effect on the conclusions concerning the slip rate method. Since the Japanese data represent most of the data at slow slip rates, the data base is weakened in precisely the range where it is most uncertain and where the data is most important to the conclusions concerning the maximum earthquake limit (MEL). Since much of the Japanese data exceeds the present proposed MEL, its elimination has shifted the MEL curve to lower magnitude values for slow slip rates.

Considering the claims made for the slip rate method by the Applicants and the NRC Staff, it is important to thoroughly justify such dismissal of data. There is no established reason why Japanese strike slip earthquake mechanics should be any different than California strike slip earthquake mechanics.

Differences between Japanese earthquakes and Southern California earthquakes mentioned in the FSAR include:

> Most strike-slip faults on land in Japan began in the early

Quaternary (about one million years ago) and have continued to move in the same direction with an average rate of a few millimeters per year (Matsuda and Okada, 1968). The total displacements are not greater than 12 kilometers. This small amount of displacement and youthfulness of the origin of their recent movement are characteristic of Japanese active faults and are in contrast with the history of such major faults as the San Andreas fault in California or the Alpine Fault in New Zealand (Matsuda, 1967). FSAR Question 361.46 b-2

Recurrence intervals of earthquakes on a given fault are long, and lie in the range of several hundred to a thousand years (Matsuda, 1967). FSAR Question 361.46 b-2

Japanese strike slip earthquakes differ from all other strike slip earthquakes in many ways. The most conspicuous differences are the large displacement relative to rupture length, the shortness of rupture length and overall fault length, the fact that commonly the entire mapped length of the fault breaks in one earthquake and that conjugate pairs of faults often rupture at the same time. FSAR Question 361.46 b-2

There is a trend of apparently low slip rates for some strike slip faults in Japan that produce large earthquakes. FSAR Question 361.50.

To the extent that strike slip faults in Japan and California fall into almost mutually exclusive groups when these fault properties are compared, the Applicants conclude that faulting of the kind that occurs in Japan cannot occur in California. Accordingly, it is inappropriate to include faults in Japan in an analyses of fault behavior of strike slip faults in California. FSAR Question 361.50.

Apparently, the Japanese data are disregarded because they commonly violate the thesis that is being tested. The data set for Japan is probably the best in the world, in terms of thoroughness and length of historic record. This, in part, may be the reason for its appearing anomalous to the Applicants. It seems to me premature to disregard the Japanese data until some mechanism is established justifying this, or until better data are obtained outside Japan. It may be noted, that many of the characteristics of Japanese earthquakes which are used to justify disregarding them are precisely those characteristics attributed by the Applicant to the OZD, e.g., youthful origin, low slip rates, small total displacements and long recurrence intervals on a given fault. Thus the Japanese data should not be disregarded in considering the seismic hazard from the OZD. Similar comments apply to data from Chinese earthquakes, which are dismissed. There are examples of Chinese earthquakes of large magnitude occurring on fault zones which have remained quiescent for periods much longer than the period of observation available for the OZD (Allen, 1975).

V. PEAK GROUND ACCELERATIONS

The Data Base for Estimating Peak Ground Accelerations

The data base for predicting peak accelerations very near (< 10 km) from large earthquakes ($M \sim 7$ and greater) is very limited, and thus any such predictions are open to large uncertainties. We will need about ten well-recorded earthquakes of magnitude near 7 and greater before we can be confident of such predictions. Present data do not preclude occasional peak horizontal accelerations of higher than 1 g at a distance of 8 km.

A similar conclusion (regarding possible peak horizontal acceleration) results from consideration of our physical understanding of earthquakes and generation of strong ground motion. We lack the necessary understanding of critical aspects of the rupture mechanism, e.g. level and variation of stress drop, complexity of rupture, focussing of energy by rupture propagation, and attenuation. Simple theories relating to peak acceleration to stress drop indicate accelerations higher than 1g are possible for stress drops of 100 bars, a value reasonable for earthquakes of magnitude 7.0. However, localized stress drops of higher than 500 bars have been inferred in some studies, thus, since peak acceleration is linearly related to stress drop in these simple models, near source accelerations of higher than 5g might occur. It is not known at the present time,, because of the lack of data, how adequate our present simple

models are for estimating ground accelerations, and it is not known what the probabilities of high stress concentrations are, nor how large a volume can experience large stress drops, and, thus, how far very high accelerations could extend away from the source of the concentrated stress drop.

Because of the uncertainties described above, it is not possible to establish with confidence the probabilities of high peak horizontal accelerations at a distance of 8 km from an M = 7 earthquake. Recent extrapolations of existing data obtain quite different results (TERA Report Campbell, 1980; NRC SER; USGS 81-365, Joyner, *et al.*, 1981). The differences in the estimates stem from small differences in choices of data base and in assumptions in the regression analyses. At present, it is not possible to establish with confidence which data and which assumptions are most appropriate and thus, the probabilities of high accelerations remains uncertain. From the results of the USGS Open File Report 81-365 correlation, the probability, for an M = 7 earthquake at 8 km, of accelerations over 1g is 11%, and for accelerations over 2g, 1%. These values are higher than for the other correlations but do not appear to be unreasonable, based on our present understanding of the data and of earthquake source mechanism. However, they must remain quite uncertain because of the limited data base.

Possibility of high stress drop

For other fault parameters constant, accelerations and velocities are proportional to stress drop. The average stress drop for large earthquakes is about 30 bars (Kanamori and Anderson, 1975) with a range up to about 100 bars.

Although most small earthquakes have stress drops of less than 100 bars, there is evidence from spectrum studies that in some circumstances stress drops can be as high as a kilobar, with consequent higher nearly source accelerations and velocities. Trifunac (1972 a and b) found stress drops of about 500 bars and 350 bars for aftershocks of the San Fernando and El Centro earthquakes, respectively. A stress drop of ∿600 bars was inferred by Hartzell and Brune (1977) for one earthquake in Brawley earthquake swarm of January 14 - 31, 1975. Fletcher et al., (1978) found a stress drop of over 400 bars for the Oroville, California earthquake of August 6, 1975. Hartzell et al., (1978) inferred a stress drop of over 1 kbar for the Acapulco, Mexico earthquake of October 6, 1974. The larger earthquakes in a recent Victoria, Baja California swarm apparently had stress drops of over 500 bars. Other examples of studies which found high stress drops include House and Boatwright (1980), 890 bars and 650 bars; and McGarr (1981 over 2 kbar for asperity stress drop).

The stress drop along a major fault during a large earthquake is probably quite variable, and thus even though the average stress drop is usually less than 100 bars, locally the stress drop could be considerably higher. Hanks (1974) inferred a stress drop of 350 - 1400 bars near the hypocenter of the 1978 San Fernando earthquake. Aki (1978) inferred a

local stress drop of 370 bars and associated near source accelerations of 1.5g for the 1857 California earthquake (based on variations of observed fault slip and a barrier theory of faulting). Trifunac (1972a) inferred a stress drop of about 350 bars for the southeast part of the rupture in the 1940 Imperial Valley earthquake. It is not known how large the areas of high stress drop along a fault can be, but it is possible that they could extend over more than a 10 km radius, thus leading to anomalously large accelerations and velocities at the surface and out to 10 km from the fault. Although our understanding of earthquake stress drop is not fully developed, and there are uncertainties in the above results, they collectively suggest that in some cases stress drops of a few hundred bars or more may occur over fault volumes of at least a few km dimension.

Whether or not large stress drops can extend to shallow depths (less than a few km) is not known. The study of Aki (1978) implied large stress drops extending to near the surface. Hartzell, *et al.* (1978) interpreted the strong motion record of the Acapulco earthquake of October 6, 1974 to be due to an earthquake with a stress drop of over 1 kbar at a depth of only about 1 km. The Norma 163 model of Heaton and Helmberger (1979) for the San Fernando earthquake

has two fault segments one at a depth of about 13 km with a relatively steep dip and a second at depths less than 5 km with a shallower dip. His contours of displacement on the shallower segment of the fault imply high stress drops at 2-3 km depth. Thus, there are indications that in some cases stress drops greater than a few hundred bars may extend to near the surface.

I conclude that large stress drops over relatively large volumes near the surface could cause anomalously high accelerations and velocities in some instances (greater than 2g accelerations and greater than 200 cm/sec velocities). The probabilities of occurrence for high stress drops is not known.

Effect of focussing of energy by rupture propagation (Directivity)

Focussing of energy in the direction of source propagation is a phenomenon that has been known and observed in nature for many years. In seismology, the effect has been termed directivity and has been observed for many earthquakes (Bakun, *et al.*. 1978), and most recently in the Livermore earthquake (Boore *et al.*, 1980), the Santa Barbara earthquake (CDMG, Special Report 144, 1979; Miller and Felszeghy (1978); and the Coyote Lake earthquake (Archuleta, 1979).

For wavelengths shorter than fault dimensions, the effect can lead to amplitudes in the direction of rupture several times higher than in directions away from (or near normal to) the direction of the rupture. The effect has been verified in physical models of spontaneous rupture (Archuleta and Brune, 1975) and in numerical similations (Boore, 1977; Archuleta and Frazier, 1979) as well as in numerical modelling of TERA-DELTA (page 6-1 , Supp. 1).

The concept of focussing or directivity is important in strong motion seismology not only because of the fact that it can lead to anomalously high ground velocity and acceleration in the focussed direction, but also because it can introduce a large range of scatter in the data close to faults, thus making it particularly difficult to estimate the true mean and standard deviation of peak velocities and accelerations from a limited sample of data. It may cause the distribution of peak accelerations to deviate significantly from a lognormal distribution.

Although, as mentioned above, effects of directivity have been observed for several recent earthquakes, there is no case of a well instrumented large earthquake (M_s near 7) where such effects are clearly evident (directivity effects are not obvious in acceleration data from the recent IV-79 earthquake, possibly because the source was not an approximate uniform rupture). Thus, the possibility remains that if

special circumstances leading to strong directivity for a large earthquake were to occur, horizontal accelerations could be considerably higher than any recorded to date. The probabilities of such occurrences are not known.

Arguments against high velocities and accelerations

I have discussed above, a number of points which suggest that near large earthquakes accelerations higher than 1g and velocities higher than 100 cm/sec may be common, and accelerations as high as 2g and velocities as high as 200 cm/sec are possible. I would now like to discuss some of the arguments which have been cited against the possibility of such high velocities and accelerations.

The fact that the data base is so small can be equally well used to argue that the above conclusion, that high velocities and accelerations will be occasionally expected, is not proven (burden of proof reversed), especially since no accelerations as high as 2g nor velocities as high as 200 cm/sec have yet been recorded. Also, a number of physical phenomenon might limit the velocities and accelerations observed, e.g. scattering, inhomogeneities in the rocks, incoherency in the fault rupture, low Q and high non-linear attenuation. Non-linear attenuation at high strains associated with high acceleration might be especially effective in

limiting accelerations in certain types of soil such as exists in the Imperial Valley. The fact that average stress drops (averaged over the fault plane) are commonly about 30 bars, and thus less than necessary for generating large accelerations and velocities, suggests that in most cases such large velocities and accelerations would not be expected (probabilistic argument). Also, perhaps many of the recently observed high values of acceleration and velocities have been unduly affected by special conditions which would not apply to San Onofre.

Building damage observed near large earthquakes has usually not been as great as engineers would have expected for such large accelerations and velocities. This observation suggests that such large accelerations and velocities are rare, if we accept these earlier expected correlations of building damage and acceleration. However, estimation of ground acceleration from building damage is a very uncertain procedure. Conversely, however, this line of reasoning, along with the low intensities associated with the accelerations recorded for IV-79, suggest that higher intensities, e.g., intensity IX, may be associated with <u>higher</u> accelerations than previously thought.

Finally, it can be reasonably argued that the very high values of accelerations and velocities require such a coincidence of deviations of variables away from their

average values as to be very unlikely for any given earthquake (probabalistic argument). For example, the highest accelerations are expected when a series of factors combine to lead to high accelerations (e.g. station azimuth and rupture propagation so as to maximize directivity focussing, anomalously high stress drop, relatively low attenuation and scattering, and anomalous surface amplification).

In my opinion, the above arguments do not outweigh the contrary arguments and evidence. They are especially weak if the burden of proof is assumed to lie with the contention that high velocities and accelerations are not possible.

Expectations for the near future

It is evident that our understanding of the nature of strong ground motion near large earthquakes is still in an uncertain stage. Deployment of large numbers of accelerographs near active faults began only a few years ago and the data base is as yet very limited. Each new large earthquake recorded usually has surprises. We may expect marked changes in our ideas once strong motion from several large earthquakes has been observed on a number of instruments in the near field. Also, our ability to do theoretical and numerical modelling is advancing rapidly and may lead to important insights in the near future.

Conclusion regarding the data base for ground motion

Based on our present limited data base for near source (< 10 km) ground motion for large earthquakes ($M \gtrsim 7$), and based on our present limited understanding of the seismic

wave generation and transmission, the design base horizontal ground acceleration of .67g could be exceeded by an M = 7 earthquake on the OZD. Under reasonable assumptions, maximum accelerations at a distance of 8 km could exceed 1g.

Although there are factors operating which might make large accelerations and velocities less probable, such limiting factors are not established by our present data base and theoretical understanding. A near certain conclusion is that if the burden of proof is assumed to lie with the thesis that very close (< 10 km) to large earthquakes ($M \sim 7$ and greater) accelerations of greater than .67g are not common, then the thesis has not been proven.

Numerical Modelling

Numerical modelling provides a method of extrapolating beyond our present data base to magnitudes and distances not represented in the data. This can be useful in understanding the possibilities and probabilities of high ground acceleration. However, to do this would require variation of the input parameters over ranges based on outside information. Since by itself the ground acceleration data at close distances and large magnitudes is so limited that different investigators can come up with quite different extrapolations beyond the data base, as described above, it is clear that, given the flexibility of assigning input parameters in the numerical modelling, it is also possible for different investigators using numerical modelling to obtain different conclusions about whether or not the 0.67g DBE for San Onofre is "conservative". The modelling studies of TERA-DELTA purport to show that the .67g DBE horizontal ground acceleration for San Onofre is "conservative" and to provide support for this conclusion beyond that provided by "empirical" study of the data base. I disagree with that conclusion because the study does not demonstrate that the parameters introduced into the numerical modelling are "conservative" and have been varied over reasonable ranges. Rather, the study shows that the parameters can be chosen in such a

way that the resultant accelerations and response spectra fall below the values for the DBE (at the appropriate distance and magnitude) and at the same time fit reasonably well limited data from other distances and for other magnitudes. It is not surprising that such a "solution" (i.e. selection of parameters) can be found, given the number of modelling parameters introduced, their uncertainties, and the lack of controlling data in the distance and magnitude range of interest. Unfortunately, such a result does not help very much since we already knew that a set of assumptions could be made, in extrapolating from the existing limited data base such that the DBE acceleration of .67g would be above the extrapolations.

However, the real value of numerical modelling is not taken advantage of in this approach. I believe that the proper approach is to assume a range of various input parameters, the range determined by information other than the strong motion data itself. Thus, parameters which are introduced for which there is little knowledge about their true values, would result in large variations in the result, but effort would be put into limiting their range using outside information. Given our present uncertainties in many of the parameters, it is obvious that many of the computer runs would show results over the .67g DBE value. This, by itself, would not necessarily mean that the DBE

acceleration was not adequate or conservative. However, we would be in a position to judge better the likelihood of these exceedences. Unfortunately, such calculations are so expensive that few organizations have the funds to carry them out. Given the fact that such a variation in parameters has not been carried out, I have to agree with Drs. Trifunac and Luco, in their testimony before the NRC Appeal Board hearings on the Diablo Canyon Nuclear Power Plant (October, 1980) that use of such calculations in the licensing process is premature. In the present case, with the present stage of development, the calculations presented by TERA-DELTA do not add a significant further constraint on expected values for ground motion over that available from statistical extrapolations from the data base itself

With these introductory remarks about the philosophy of numerical modelling, I would like to address the uncertainties of some of the parameters in the TERA-DELTA study. First, the values for standard deviations in the TERA-DELTA model do not represent the kind of standard deviations expected from real data where stress drops, rupture complexity and rupture propagation, as well as relationship of the rupture to local geologic irregularities, varies from earthquake to earthquake. In the real world, standard deviations of peak acceleration are considerably greater than the values

given in the TERA-DELTA study, which correspond to varying only certain randomness parameters in a controlled way.

Second, the effects of uncertain values of the attenuation parameter Q have not been adequately investigated. NRC Reviewer Luco (by report to the Nuclear Regulatory Commission, August, 1980) suggested they should be about a factor of 2 higher. I believe this is a reasonable factor for the uncertainty of Q. Low Q values assumed in the TERA-DELTA model may have excessively attenuated high frequency energy and thus reduced peak accelerations (as well as indirectly and artifically reduced the effects of focussing in the modelling).

Third, because of the assumed slip function in the TERA-DELTA model, it is difficult to infer what the effective value for dynamic stress drop is. A dynamic source study comparison by Swanger et al., (1981), indicates that it is only about 50 bars (see attached fiture). (There is no established basis for the assumption, made in the TERA-DELTA modeling, that the peak slip velocity parameter is constant for all earthquakes.) I feel that the average effective stress values should be varied by at least a factor of two and, in addition, the possibility of localized stress drops of up to 500 bars should be considered. The parameter studies by TERA-DELTA have shown that in the frequency range 2 to 10 Hz, the response spectra are essentially linear with with respect to dynamic stress drop. Thus, to take into

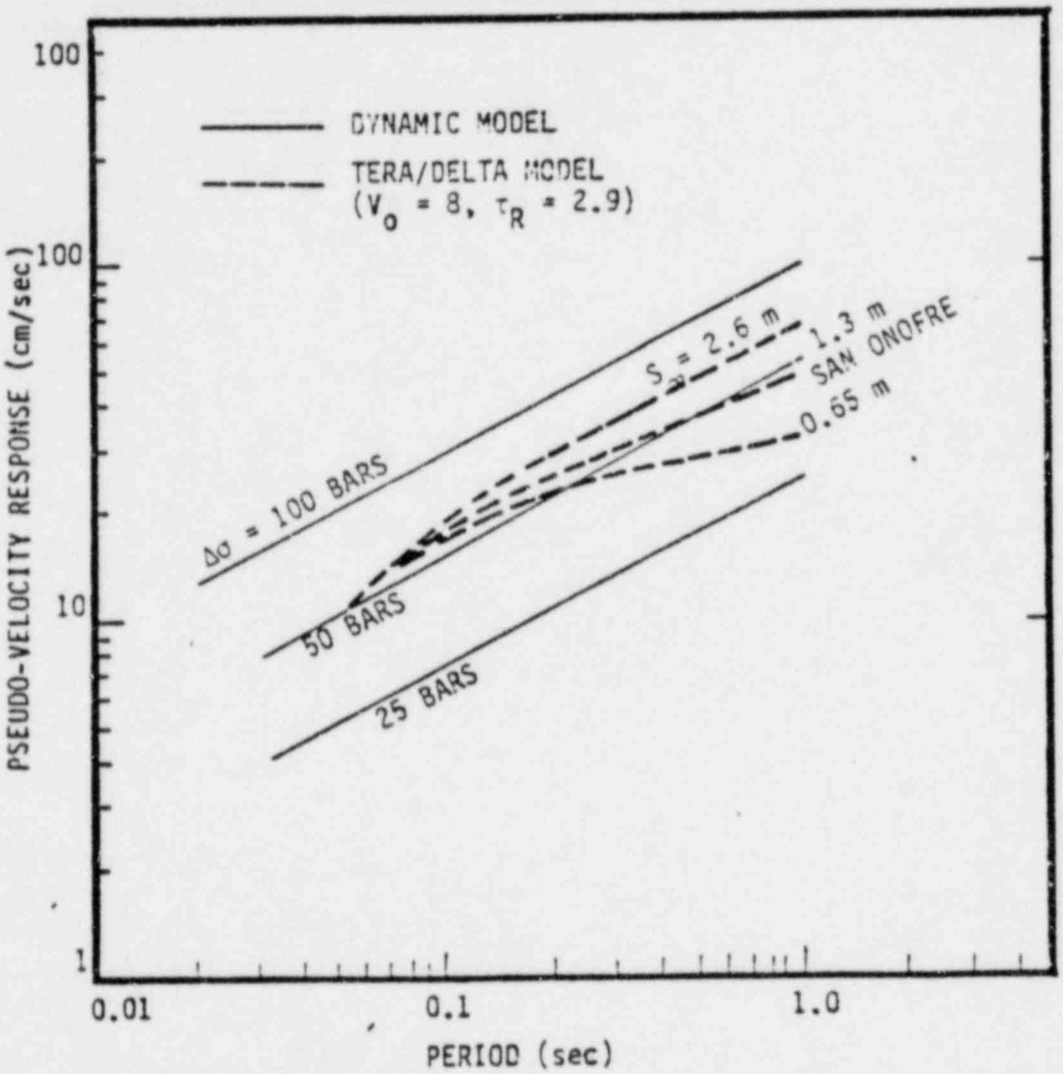


Figure 4.7. Comparison of response spectrum of the dynamic slip function with that of the TERA/DELTA San Onofre slip function.

account these higher stress drops, we should multiply the spectra in this frequency range by a factor of about two. Local zones of high stress drop would increase the spectra even more.

Fourth, the TERA-DELTA model does not adequately predict the accelerations actually observed in the Imperial Valley 1979 earthquake at stations a few kilometers from the fault, being approximately a factor of two too low (Luco, 1980). This indicates that the dynamic stress drop or some other parameter should be changed to increase the peak acceleration by about a factor of two in order to fit the Imperial Valley earthquake. Further increases would need to be made to go from the IV structure to the SONGS structure and to extend the results to the higher magnitude appropriate for the OZD.

In conclusion, the results of the computer modelling presented by TERA-DELTA indicate the feasibility and importance of doing a parameter study of expected strong motion at SONGS but, by themselves, do not constitute such a study, but rather, a limited range of calculations in which important parameters have been assigned values not established as conservative. If these parameters had been assigned more conservative values (based on our present uncertainties) the DBE spectra would have been substantially exceeded.

Requirements for an adequate modelling study

An important aspect of estimating earthquake hazard with the present limited data base is the use of computer modelling of fault rupture to simulate strong motion, without which we cannot make full use of the data. Modelling can provide improved understanding of the strong motion, especially the effects of focussing by rupture propagation and localized concentrations of high stress drop. Modelling can eventually significantly reduce uncertainties and provide a range of realistic models of ground motion records from which to predict expected ground motion which might occur at SONGS from a rupture on the OZD. To estimate the effects of uncertainties in the parameters for the SONGS modelling, reasonable variations in model parameters should be carried out to indicate conclusions for a range of degrees of conservatism.

The numerical modelling should include effects of rupture on all faults considered capable. In particular, the orientations and locations of fault ruptures should be varied to take into account possible effects of focussing, i.e. whether fault rupture could proceed toward the plant, thus focussing energy in that direction. A conservative approach in this case would be to choose fault orientations as close to the direction toward the plant as allowed by the data.

A distance of about 5 miles or 8 km with an uncertainty of the order of one kilometer has been cited by the Applicant and Staff as the distance to be considered for the design earthquake. However, at the time this decision was made, detailed seismic sounding information did not exist between the OZD and SONGS. A recent study by Greene and Kennedy (1980) shows a zone of deformation, called the Cristianitos Zone of Deformation (CZD) lying closer to SONGS at a distance of about 1500' offshore (2½ miles or 4 km). Since northwest of the zone is a "data void", it is reasonable that the zone may continue parallel to the coast and simply be a closer strand of the OZD (rather than a branch of the Cristianitos fault from onshore). Both Greene and Kennedy (personal communication 1981) have indicated that this is a reasonable interpretation of the existing data. If this possibility is established as credible, modelling should include calculations with rupture on this zone. It is also important to consider the effect of possible rupture on any other splay or branch faults considered capable. In particular, calculaton should be made for a rupture proceeding northwestward along the OZD and continuing onto the CZD towards SONGS. Focussing and directivity effects of such a rupture might lead to peak accelerations at SONGS considerably greater than the DBE.

A reasonable initial conservative model for an earthquake on the OZD would be a model with a more or less uniform stress drop of 100 bars (over the entire fault rupture), and

superimposed local stress drops of about 500 bars for local stress concentrations of about 5 km in radius, located at several points along the main fault branch and on splay or branch faults which are judged capable. Refined estimates for these values could be made when the initial results of the modelling are obtained.

A possible criticism of this proposed variation in input parameters for numerical modelling is that we already know from existing sensitivity studies and the large uncertainties (due to lack of controlling data) in input parameters, that such calculations will in some cases yield accelerations higher than the DBE, and thus, we do not need to spend the money. Nevertheless, it is clear that more sensitivity studies (in essence the same as variation in parameter studies) are needed before the full value of numerical modelling is realized.

Lessons from the Imperial Valley 1979 Earthquake

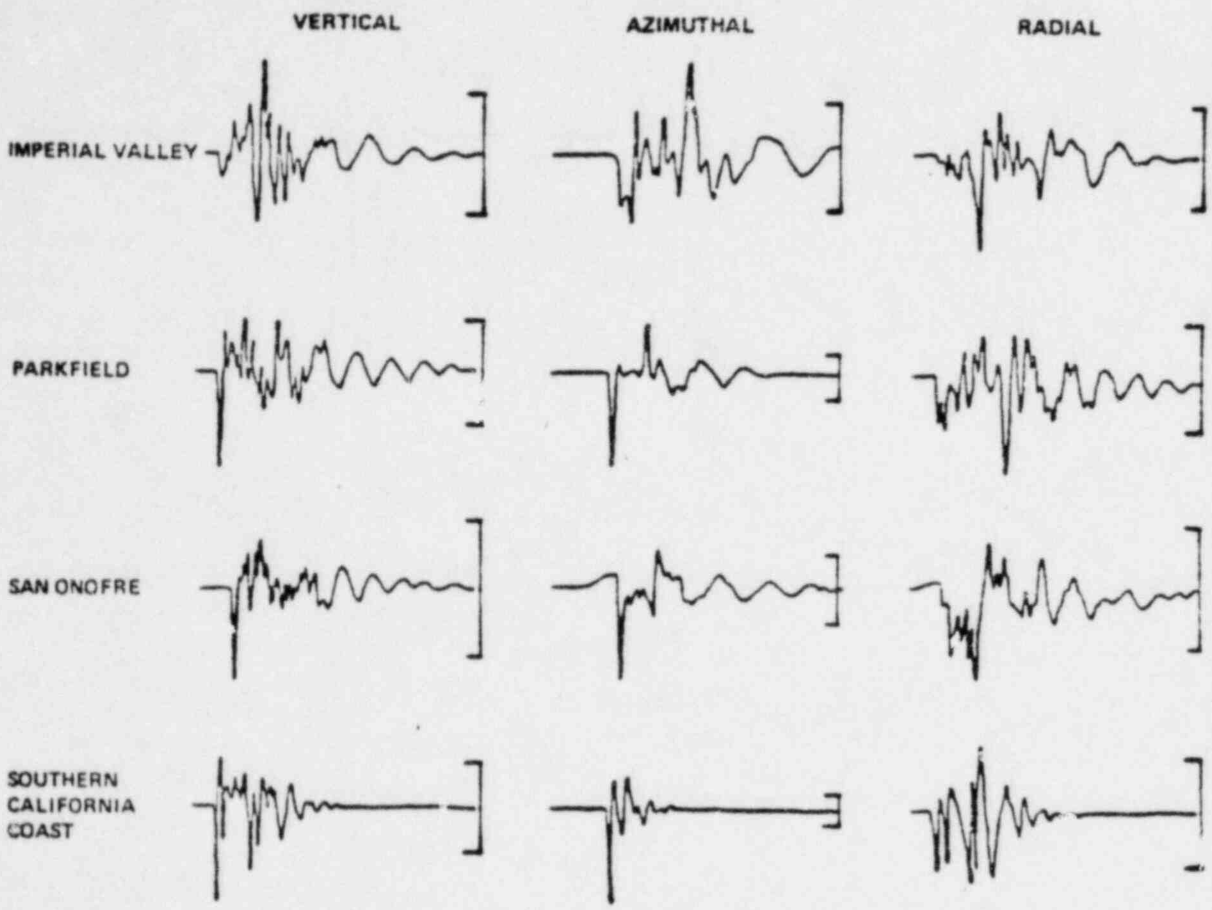
The Imperial Valley 1979 (IV-79) earthquake is the first well-recorded (in terms of near source strong motion) earthquake with M_s between 6 and 7. Accelerograms were recorded at sites along the fault trace, at sites in both directions from the epicenter, and on a cross array of stations near the northern end of the fault. Because it is unique in terms of instrumental coverage, there is a natural tendency to assume it is the "typical" earthquake for its magnitude, and use it as a basis for estimating strong ground motion for other magnitudes and other geologic settings. However, because the data base is so limited, we cannot confidently assume the IV-79 earthquake is "typical" or "not typical." We will need several more well-recorded earthquakes before we establish with certianty how "typical" it is. Furthermore, it will probably be at least a couple of years before the mechanism of the IV-79 earthquake is analyzed and understood well enough to clearly appreciate its implications for strong motion.

Horizontal accelerations near or above the San Onofre DBE value of .67 g were recorded at several stations: .72 g at Station 942, .81 g at Station 5054, .64 g at Station 958 and .61 g at Station 955 (Data taken from Table 2 of Joyner et al., 1981). Thus, the IV-79 earthquake data suggest that near source values of horizontal acceleration above .67 g may be quite common for earthquakes of this magnitude. There is no reason to expect that such high values could not occur

at San Onofre from a similar earthquake on the OZD. In fact, the TERA-DELTA modelling results indicate that for the same earthquake mechanism, the accelerations at San Onofre would be about 1.8 times higher, primarily because of lower attenuation in the SONGS structure as compared to the IV structure (Final Report, Fig. 4-13, attached).

GREEN'S FUNCTIONS

Source Depth 5 km
Distance 21 km



The height of the brackets correspond to equal absolute magnitudes for each trace. Hence, a large bracket corresponds to a small arrival.

FIGURE 4-12

COMPARISON OF GREEN'S FUNCTIONS
FOR THE FOUR GEOLOGIC MODELS

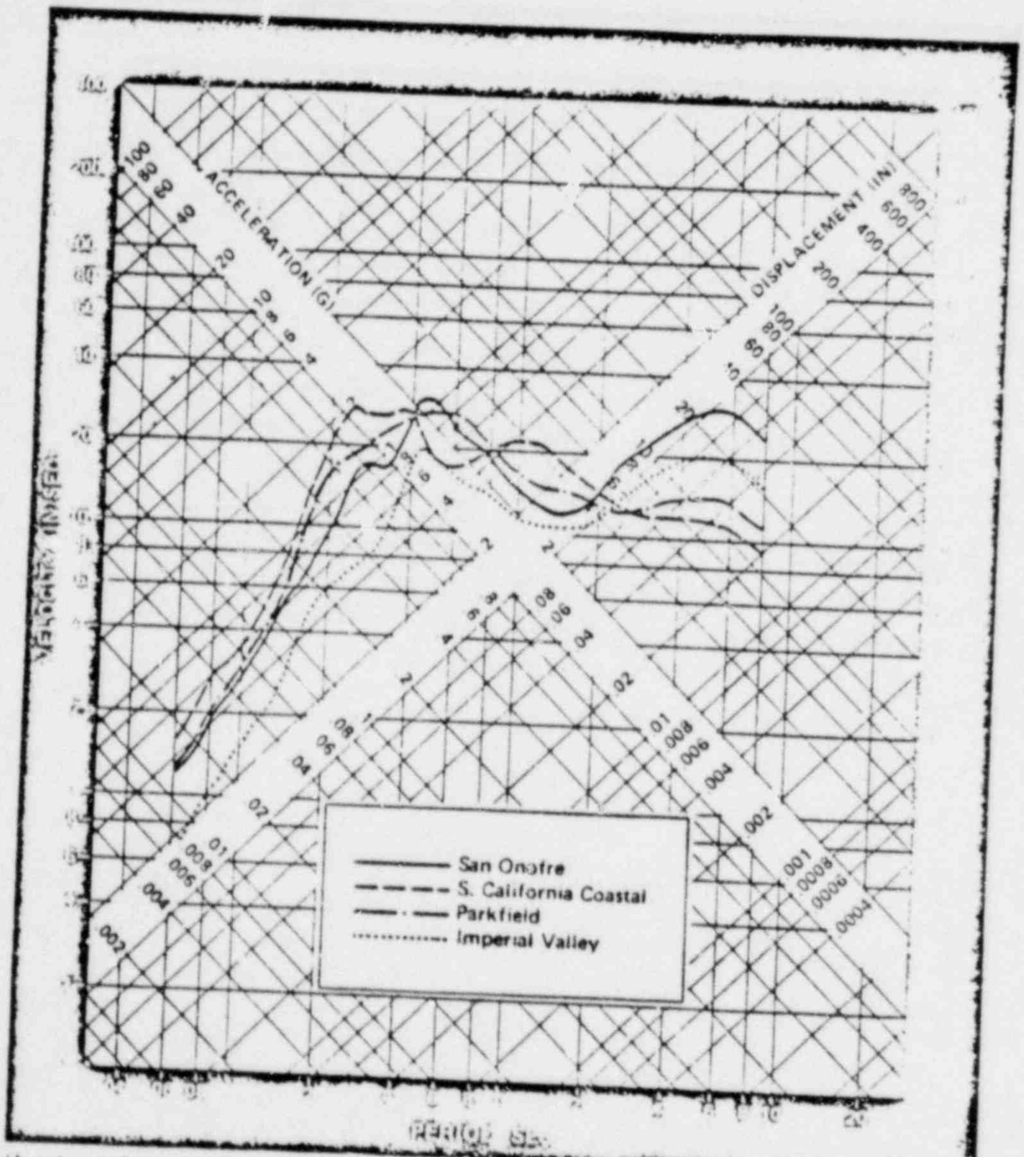
TERA-DELTA
FINAL REPORT
(1)

A DIVISION OF TERA CORPORATION

SITE SPECIFIC RESPONSE PARAMETER STUDY

EFFECT OF GEOLOGIC STRUCTURE

MAXIMUM HORIZONTAL COMPONENTS EARTHQUAKE D



Hypothetical Smoothed Two Percent Horizontal Response Spectra that Would Be Found Were the San Onofre Site Located on Each of the Geologic Structures Illustrated in Figure 4-11.

FIGURE 4-1_

EFFECT OF GEOLOGIC STRUCTURE ON SITE SPECIFIC RESPONSE

A DIVISION OF TERA CORPORATION

Magnitudes for the IV-79 event

The value of M_w, based on the seismic moment, for IV-79 is 6.5 as given by Kanamori and Regen (1981). The seismic moment is 6 x 10^{26} dyne-cm, considerably smaller than estimates for the 1940 Imperial Valley earthquake. (More recent results using IDA long period data give a moment value of 7 x 10^{25} dyne-cm (Kanamori, sonal communication, 1981)). Following Slemmons (1977), the average fault length and displacement inferred by Kanamori and Regen, 35 km and 57 cm, respectively, correspond to M = 6.44 using the equations relating LD^2 to M (Curve E, Table 15). The average offset of about 1/2 meter, confirmed by geodetic observations (Snay, 1981), corresponds to M $\sim$ 6.4 (Curve E, Table 11). Thus, we can see that the combination of average displacement and fault length correspond approximately to an M = 6.5 event, in agreement with the Kanamori and Regen value for M_w.

The M_s value reported in the USGS EDR, 6.9, is 0.4 units higher than M_w. However, this M_s value is unreliable and biased upward because of the heavy weighting of European stations (10 out of 13 stations used) at a narrow azimuth along a path where the attenuation is relatively low. If the European data are together taken as one determination of M_s (= 6,9) and averaged with the three other values, an M_s of 6.45 is obtained. The IV-79 magnitude is further increased by about .2 units, because the EDR uses the Prague Formula rather than the Gutenberg-Richter formula, which was used for the determination of M_s for IV-1940 (Kanamori, personal communication, 1981). Direct comparison of amplitudes

recorded at DeBilt and Stuttgart indicates that the IV-40 earthquake was larger in magnitude by .6 units (DeBilt) to .4 units (Stuttgart). Thus, this data confirms that the IV-40 event was about 1/2 magnitude unit higher than the IV-79 event. Considering the poor sampling resulting in the EDR M_s value of 6.9, a value of M_s and M_w of 6.5 for IV-79 seems more reliable and consistent with the observed displacement data, especially in relationship to the IV-40 earthquake with M_s and $M_w = 7.1$. There is no doubt that the displacement was much less for IV-79 than IV-40, and the corresponding M_s should also be less.

The local magnitude, M_L, for IV-79 reported by Pasadena, is 6.6. Calculations based on synthetic Wood-Anderson seismograph responses from strong motion records give $M_L \sim 6.3$ from Mexico stations (Brune <u>et</u> <u>al</u>., 1981) and $M_L \sim 6.2$ from United States stations (Kanamori and Regen, 1981). The local magnitude for the IV-40 event ranges in magnitude from 6.3 to 6.5 (Kanamori and Regen, 1981). Kanamori and Regen note that, since for the 1940 event only stations north of the border were available, there may be strong effects of rupture propagation. Since the 1940 event is believed to have ruptured to the southeast (Trifunac and Brune, 1970), away from the stations used to determine M_L, and away from the El Centro strong motion station, its true local magnitude may have been considerably higher (because of defocussing of energy in the direction of the U.S. stations).

Estimates of the maximum Modified Mercalli intensities in the near field of the IV-79 earthquake are less than corresponding intensities for the 1940 earthquake, about IX vs. VII (Reagor et al., 1981). This is, in part, because of the longer duration of shaking for the 1940 event (in turn associated with the longer fault length and greater fault slip). However, since there is a general correaltion of peak acceleration with intensity, it is probable that peak accelerations in the 1940 event, particularly to the soutehast nearer the large displacements and in the direction of rupture propagation, were considerably higher in 1940 than in 1979.

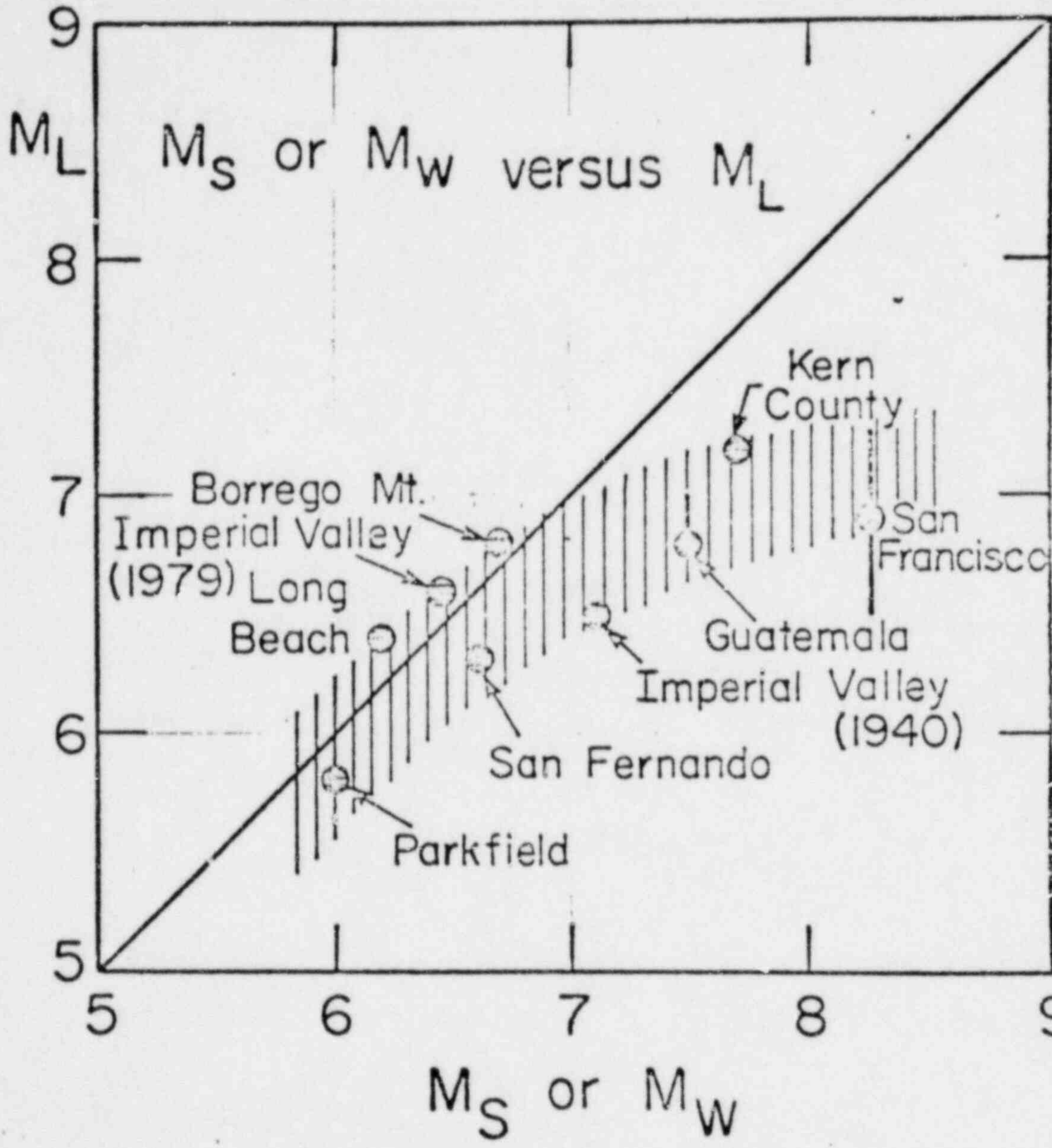


Fig. 4

KANAMORI & REGAN

Peak horizontal accelerations for IV-79

There are a range of ways of considering the peak acceleration data from the IV-79 earthquake. On the one hand, one might make the reasonable assumption that the high acceleration at Bonds Corner (.81 g) was the result of breaking of a local high stress drop asperity at some point along the fault, or the result of directivity due to rupture propagation (Hartzell, personal communication), or some other at present not understood effect, and take this observation as the most conservative point from which to extrapolate a maximal design acceleration for SONGS. Assuming an M_w for IV-79 of 6.5 (Kanamori and Regen, 1981) and a Design Base M_w for San Onofre of 7.0, along with the peak acceleration vs. magnitude dependence of about .3M given in USGS Open File Report 81-365 (and Hanks and McGuire, 1981) yields a peak acceleration of about 1.1 g. If a further correction for the lower attenuation at the SONGS site is compared to the attenuation in the Imperial Valley is made, $\sim$ 1.8, (TERA-DELTA, Final Report, Fig. 4-13), a peak acceleration at SONGS of $\sim$ 2 g is obtained. This sort of extremal analysis indicates, as do many other consideration, that accelerations over 1 g and up to 2 g are possible. Of course, for a hypothesized M = 7 on the OZD, such high accelerations are less likely than lower values, and hence if we are willing to be less conservative, we can reduce these values in a number of more or less arbitrary ways:

(1) We can assume that a "saturation" effect reduces the increase in acceleration between M = 6.5 and 7.5 to an average of only ~ .15 g, or even none at all (least conservative assumption).

(2) We can assume that the increase in peak acceleration between SONGS and IV-79 due to different attenuation (Q) is overestimated in the TERA-DELTA studies and reduce the correction to a factor of about 1.3 rather than 1.8 (FSAR Question 361.55 assumes a factor of 1.3 - 1.35), or even to 1.

(3) We can assume that for some as yet unestablished reason, the Bonds Corner record is unreliable and ignore it. For example, we might take .61 g at Station #4 seven km from the fault, as the basis for extrapolation:

(4) We may take average accelerations recorded at various stations close to the fault extending arbitrary distances along the fault away from the zone of maximum surface displacement, e.g., averages extending to the northern end of the Brawley fault and to the southern end of the Imperial fault, or some lesser or greater distance. This implies probabilistic considerations based on the fact that it is less likely that SONGS would happen to be situated at the "worse" location than at some more "average" location. The "average" can be reduced arbitrarily by extending the average to include stations farther and farther from Bonds Corner.

(5) We may consider probabilistic distribution and estimate the mean and one standard deviation in various ways, and assume that a mean plus one standard deviation value is sufficiently conservative.

The above considerations allow us a wide range of estimates of less conservative values for a DBE based on the IV-79 data, ranging from a value for peak acceleration anywhere between over 1 g and less than .5 g. However, these estimates cannot be rigorously justified from solely scientific considerations, there is some uncertain risk implied in the various assumptions made to obtain a given number.

Correlations of Peak Horizontal Acceleration with Magnitude and Distance

The most recent USGS correlation of peak horizontal acceleration and velocity from strong motion records, including records from the 1979 Imperial Valley, California earthquake, are given in USGS Open File Report 81-365 (Joyner, *et al.*,1981). This correlation is based on moment magnitude, M_{M_O}, which is closely related to M_s. Prior studies made correlations with M_L (Joyner, *et al.*, 1981; USGS Open File Report 80-115), and still earlier a correlation was given without the IV-79 data (Circular 795, Boore, *et al.*, 1978). In all cases, the data base is very limited, especially for large earthquakes at close distances, and the results must be "treated with caution".

The data base and understanding of ground motion is not sufficient to place much confidence in these correlations, or other such correlations based on our present data set. Reasonable assumptions in choice, weighting and elimination of data, in choice o_ curve shapes to fit to the data, and in ways of grouping the data can lead to quite different results. Thus, as might be expected, different persons can obtain significantly different estimations of the peak acceleration for an M_s = 7.0 event at 10 km. For example, TERA - Technical Report 80=1 (Campbell, 1980) obtained different results than USGS 81-365. Similarly, the WCC

"Empirical Approach" results referred to in the SER give still different results.

The value from the TERA report for the 50% value (mean) for M = 7.0 at 10 km is considerably less than the corresponding USGS values. The exact reasons for this are uncertain at present (Boore, personal communication, 1981). Note that one effect of the IV-79 data has been in part to lower the mean and one standard deviation curves for M = 6.5 (USGS Circular 795 vs. USGS 81-365 results). Among other things, this could be a result of the unusually high attenuation in the sediments of the Imperial Valley.

Data base and standard deviation

Because the IV-79 earthquake is the only earthquake of magnitude gerater than 6 for which there are such a large number of stations within 10 km of the rupture surface, there is a tendency to rely heavily upon its data. However, we have no basis for saying it is typical or that its accelerations are typical. In particular, there is no basis for assuming that the standard deviations estimated on the basis of our present data set represents the standard deviation of a population of different earthquakes of magnitude near 7.0 at distances near 10 km. At close distances we may find the standard deviation for a population of data from numerous earthquakes with different stress drop and rupture

characteristics, to be considerably greater than the standard deviation of multiple observations of a single earthquake, or of our present data set for only a few earthquakes, especially when most of the data is at larger distances. This could be particularly true if large earthquakes are characterized by a complex rupture process with large, high stress drop asperities, as many recent studies suggest. For this reason, I feel that the actual standard deviations could end up considerably greater than present estimates

Magnitude "saturation"

Magnitude saturation is often based on the assumption that the slip, stress drop, and energy release on a particular section of the fault near a site, assumed to control the strong motion, will not change with magnitude, i.e. that larger magnitudes will be associated only with longer rupture lengths and that the additional energy release from distant parts of the fault will not significantly change the strong motion near the site. However, it is well known that the amount of displacement on a fault increases with fault length, and magnitude, up to magnitudes greater than 7.5. For example, Slemmons' (1977) compilation of North American data indicates maximum surface fault slip

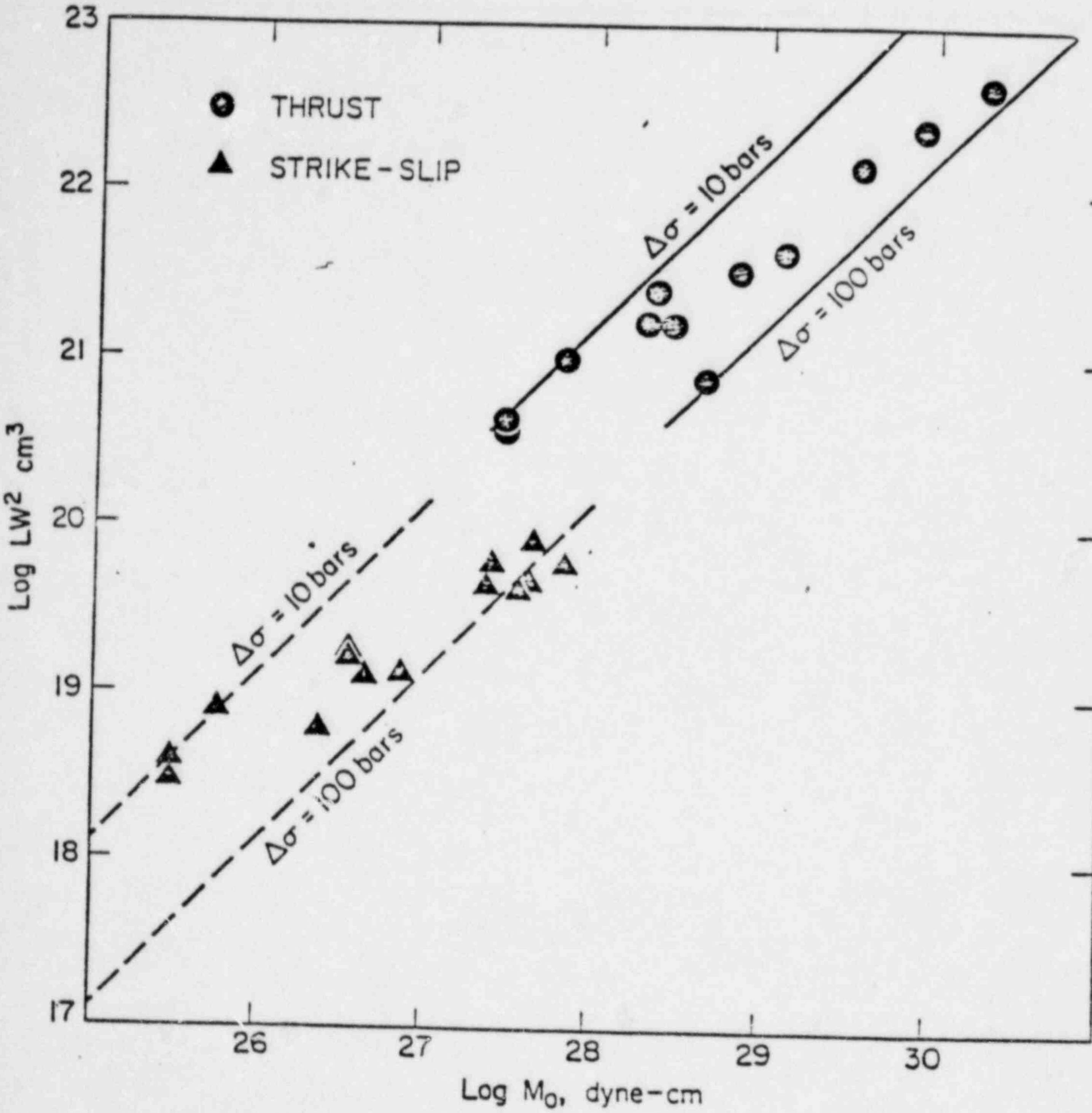


Figure 1

SCHOLZ, 1981

of .66 m for an M = 6.5 earthwuake, 1.71 m for an M = 7.0 earthquake, and 5.74 m for an M = 7.5 earthquake (Table 11, Curve E). Scholz (1981) (attached) has cast this result in terms of stress drop and fault slip as a function of fault length and moment (for strike slip earthquakes; $\sim$ 10 bars for a moment of 2×10^{25} dyne-cm, and $\sim$ 100 bars for a moment of 4×10^{27} dyne-cm). (See Scholz, Figure 1.) This data suggests that the amount of energy released on a given section of a fault (e.g., $\pm$ 20 km from a given point) may have a clear increase with magnitude. Thus, the amount of seismic energy a structure such as San Onofre would be exposed to would increase with magnitude. However, the rate at which peak acceleration would increase with magnitude depends on the details and coherence of the pattern of energy release.

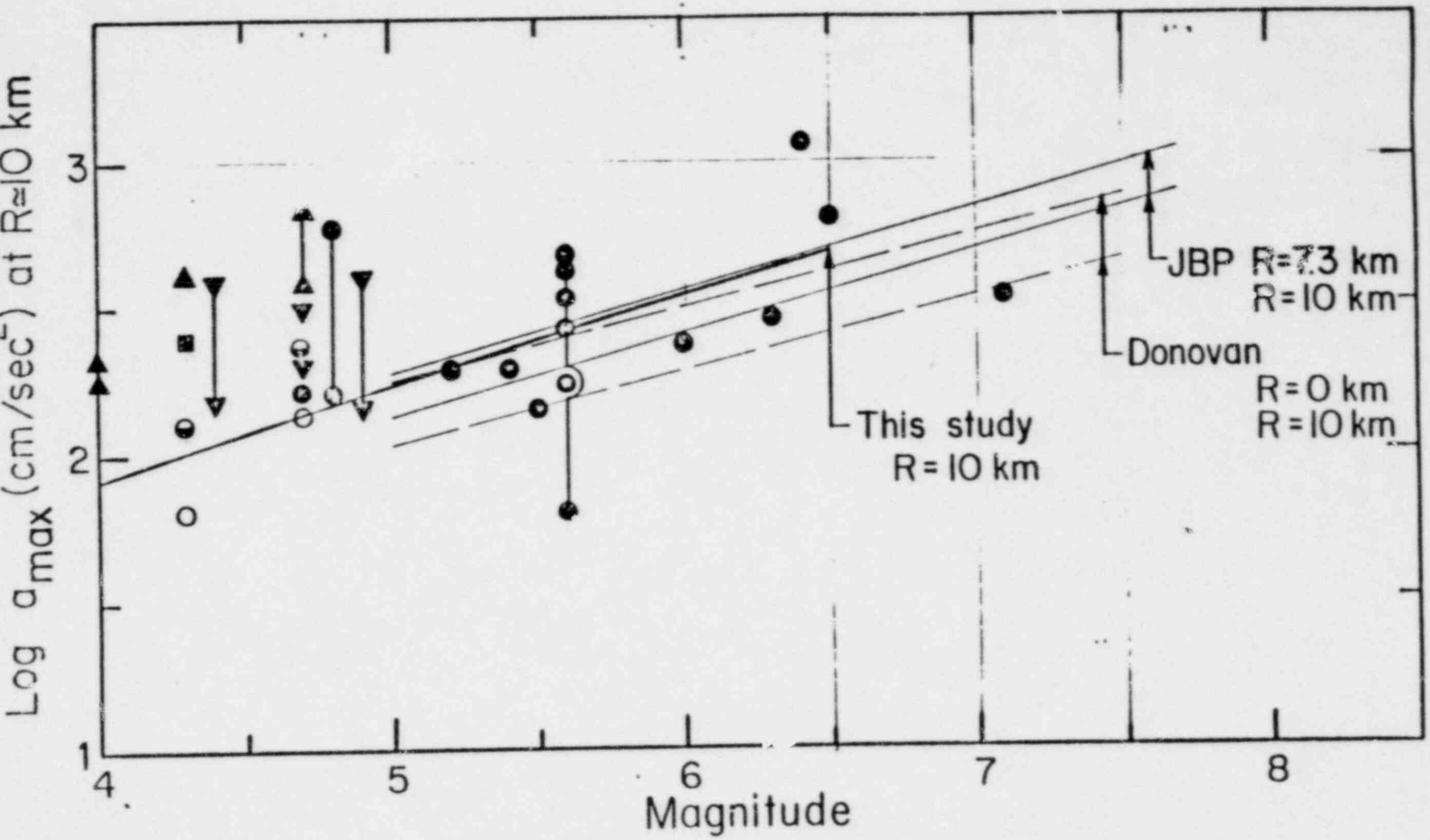
As Scholz (1981) has pointed out, the dependence of high frequency energy release per unit fault length with increasing fault length (and magnitude) depends on the mechanism causing the increase in fault displacement with fault length (and magnitude) and this is not understood at present. Thus, one cannot confidently assume that "magnitude saturation" of peak ground acceleration will occur at some magnitude below 7.5, although it could begin to be effective at M = 6.5 or M = 7.0. It is even possible, given the uncertainty about the mechanism responsible for the large increase in average slip between M = 6.5 and M = 7.5,

that the peak accelerations could increase somewhat faster with increasing magnitude between 6.5 and 7.5 than between 5.5 and 6.5, i.e., even faster than the Hanks and McGuire (1981) and Joyner *et al.*, (1981) results.

Extrapolations based on the Recent Hanks and McGuire Results

A recent Hanks and McGuire (1981) study has superseded the previous Hanks and Johnson (1976) study, often cited in discussions of magnitude saturation. Hanks and McGuire (1981) studied more than 300 horizontal components of ground acceleration from recent earthquakes and obtained the following result for the increase in average peak acceleration with magnitude: $\log a_{max} \sim 0.30\,M$ for $4 \leq M = M_L \leq 6.5$, "remarkably close to that recently determined empirically by Joyner, *et al.*, (1981) for $5.0 \leq M \leq 7.7$, their coefficient on M (moment magnitude) being 0.28 ± 0.04." Thus, based on their data, in the magnitude range 5.5 to 6.5 there is no longer any indication of "saturation."

The Hanks and McGuire curve for peak acceleration at R = 10 km (distance to fault $\sim$ 6.8 km) is significantly higher than the USGS 81-365 curve (but has approximately the same dependence on magnitude in the range 5.5 - 6.5). These results are based on a reasonable physical model for peak and RMS accelerations and thus may not be subject to quite the same arbitrariness in choosing curve shapes as the



HANKS & McGUIRE Fig 7

the other correlations. The mean value for M = 6.5 at R = 10 km (distance ∿ 6.8 km) is .50 g. If we assume a standard deviation factor of 1.5, a value for a mean plus one standard deviation horizontal acceleration for M = 6.5 is .75 g. Concerning extrapolations to higher magnitudes, Hanks and McGuire state that

> On the other handk, a_{max} need not increase by much above ∿ 1/2 g for M > 6.5 at close distances, and we expect it will not, the linear increase in log a_{max} above M = 6.5 assumed in the empirical relations of Donovan (1973) and Joyner et al., (1981) not withstanding.

In a recent personal communication, Hanks has explained that this was not meant to be a precise statement, and could include increases ranging between, on the one extreme, a continuous linear increase at the same slope as between 5.5 and 6.5 giving accelerations of about 0.9 g at M = 7.5 and R = 10, to on the other extreme, a corner and flattening at 1/2 g. Thus, to correct for an M = 7.0 event we could, on the one hand, assume the same slope on the acceleration versus magnitude curve between M = 7.0 and 6.0 as between M = 5.5 and 6.5, which would yield a mean acceleration of about .72 g and a mean plus one standard deviation acceleration of 1.06 g at M = 7.0, or on the other hand, we could assume complete flattening of the curve or complete "saturation" at M = 6.5 giving the same value at M = 7.0 as at 6.5 (.50 g). Assuming such complete saturation is not reasonable and would be the least conservative extreme. A more reasonable assumption is

is that the curve has a decreasing slope between M = 6.5 and M = 7 giving a value of about .6 g for the mean acceleration and a value of .90 g for the mean plus one standard deviation acceleration. For an M = 7.5 event, the expected accelerations would be still higher. Thus, the recent Hanks and McGuire results suggest peak horizontal accelerations could be considerably above the values quoted in the SER and also above the design base acceleration of 0.67 g for SONGS. The question of magnitude saturation cannot be solved by debate over the present data set, but must await accumulation of more data. The fact that the magnitude saturation originally hypothesized by Hanks and Johnson (1976) for $5.5 \leq M \leq 6.5$ has turned out to be incorrect suggests that we should not, without further verification, assume it applies between $6.5 \leq M \leq 7.5$. Note that if the design magnitude is increased from 7.0 to 7.5, still higher accelerations would be expected.

Conclusion regarding correlations of peak horizontal acceleration

The recent USGS Open File Report 81-365 represents an extrapolation based on one set of assumptions, but as the authors point out, "for distances less than 40 km from earthquakes with M greater than 6.6 the prediction equations are not constrained by data and the results should be treated with caution." The same applies to other attempted extrapolations. Whether mean peak horizontal accelerations increase with mangitude above $M_s = 6.5$ at the same rate they do below $M_s = 6.5$ depends on how the effects of increased energy release per unit fault length balance the near field tendency toward saturation due to fault size, and this is not known at present. Recent results of Hanks and McGuire give higher accelerations than USGS 81-365.

The above uncertainties, differences, and changes in the various probabilistic correlations, are to be expected in view of the limited data base, and are a reflection of true uncertainties in making estimates. The uncertainties cannot be eliminated by debate about various correlation methods and data selection procedures, but must await the accumulation of new data.

Vertical Accelerations

In a number of recent earthquakes, recorded vertical accelerations at close distances have been higher than corresponding horizontal accelerations, at variance with a common earlier assumption that vertical accelerations would be 2/3 the horizontal accelerations. The design vertical acceleration for SONGS is .44 g, 2/3 of the horizontal design acceleration of .67 g. Thus, the recent evidence that near faults in some situations vertical accelerations may be higher than horizontal accelerations is very significant. It further emphasizes the lack of understanding of earthquake strong motion and the fact that each new earthquake can yield unexpected results. In this situation there may be a tendency to look for "special explanations" for each unexpectedly high recording, with no corresponding effort ot look for "special explanations" for unusually low recordings, thus biasing the average data downward.

In the case of the high vertical accelerations recorded in the IV-79 earthquake, several explanations have been proposed. Because of the lack of data, we cannot be sure that such high vertical accelerations are unusual. Among the special explanations suggested for the IV-79 high vertical accelerations are special constructive interference in wave guides (Archuleta, 1981) and local site amplification (Mueller and Boore, 1981). The high vertical accelerations recorded for the Gazli, Russia earthquake are reported to have resulted "because the fault beneath the site ruptured

vertically upward towards the site (Hartzell, 1980)." (SER, p. 2.5 - 33). However, no rigorous modelling has been done to demonstrate this explanation. The Coyote Lake earthquake is reported to have "resulted in high vertical acceleration at one station because of S to P conversion at the interface between the soft alluvium and firm bedrock at depth" (Angstman et al., 1979). Other cases in which vertical accelerations have been reported higher than horizontal include the Naghan, Iran earthquake of April 6, 1977 (Ambrayseys, 1978) and the Victoria, Baja California earthquake of June 9, 1980 (Simons et al., 1981). Perhaps the most important example is the 1933 Long Beach earthquake, since it occurred on the OZD. Ratios of vertical to horizontal peak acceleration were 1.45, 1.00 and 0.60 at stations 6, 9, and 12 km from the fault (Luco, personal communication).

At the present time, we do not have a certain enough understanding of vertical ground motion to be sure that, at distances less than 10 km, high vertical accelerations will not be common. As one example, a recent model by Blandford (1975) attributed high P wave excitation to asperities on the fault surface. Models with asperities have been suggested in numerous recent studies. If any similar mechanism were operating in many earthquakes, then close to faults, vertical accelerations higher than corresponding horizontal accelerations could be quite common. Given our present lack of understanding, and lack of data, we cannot be sure of the degree of conservatism involved in the vertical design acceleration of 0.44 g.

It has already been exceeded in several earthquakes and there is no reason to believe it could not be exceeded during an earthquake on the OZD.

Special mention needs to be made of the explanation for the high vertical accelerations observed in IV-79 given by Frazier in his testimony regarding the Diablo Canyon Nuclear facility. As Dr. Frazier pointed out, "at close distance, the peak horizontal accelerations were as high, on the average, as the peak vertical accelerations." (Written testimony of Gerald Frazier, p. IV-1.) Dr. Frazier then goes on to explain the characteristics of P waves and S waves which relate to the ratio of vertical to horizontal accelerations. He describes the "echo chamber" effect due to the high near surface velocity gradient which "would tend to increase both P and S wave amplitudes...However, high frequency S waves are severely attenuated in the shallow sediments, thereby compensating for the amplification of the sedimentary basin. P waves, on the other hand, can travel efficiently in the soft surface materials. In the shallow sediments in the Imperial Valley, S waves above 10 Hz can be attenuated by a factor of ten within one kilometer, while P waves above 10 Hz are attenuated only about 20% over the same distance." (Written testimony of Gerald Frazier, Diablo Canyon transcript, p. IV-4.) Thus, in this explanation proposed by Dr. Frazier, it is suggested that the high vertical relative to horizontal accelerations occur because the S waves have been severely attenuated by the low

Q for shear waves. It follows that if an earthquake with the same stress drop had occurred in a structure with higher Q, but approximately the same shear wave structure, the peak horizontal accelerations could have been considerably higher. This would be an extreme case of higher accelerations on rock vs. soil sites.

Thus, reduced horizontal accelerations for the IV-79 earthquake may have occurred as a result of high material attenuation (low Q) in the sediments near the earthquake, especially for higher horizontal accelerations observed close in, where non-linear effects could result in the most attenuation. This possibility further indicates we should be cautious in assuming the peak accelerations observed in the IV-79 earthquake are typical.

Ground Motion Implications of Northern Baja California Earthquakes

Since the northern Baja California seismicity near the San Miguel fault zone is in a tectonic environment similar to that of the OZD (part of the same linear zone parallel to the plate boundary, recent activity, a smaller amount of total slip, probably in similar deep basement rocks) it is reasonable to assume that an earthquake similar to, but larger than, the 1968 El Alamo earthquake ($M = 6.8$) could occur on the OZD.

. This raises a question that has not been adequately dealt with in either the FSAR or the SER. Because of the special characteristics of earthquakes in the region, i.e. relatively high inferred stress drops, an earthquake with the same M_s as an earthquake in the Imperial Valley (e.g. IV-79 or IV-40) could generate much greater high frequency energy. A number of studies describe the characteristics of these earthquakes. Brune, *et al.*, (1963) noticed a region of anomalously low surface wave excitation for a given M_L (or, conversely, greater high frequency generation for a given M_s) in northern Baja California while studying events from the California-Nevada area. A similar result, expressed in terms of apparent stress, was found by Wyss

and Brune (1971). Typical values for apparent stress parameter defined by them were 140-710 bars in the region of the San Miguel Fault Zone, as compared to typical values from less than 10 up to 100 bars in the Salton Trough region. High apparent stresses were also observed in certain areas of southern California. Thatcher (1972) studied the regional variations of spectral parameters in northern Baja California from observations at Cal Tech stations in southern California especially BAR, PLM and PAS. The spectra were interpreted in terms of the source parameters moment, source dimension, stress drop and high frequency spectral fall-off. Thatcher inferred that northern Baja sources have dimensions that are typically a factor of four smaller than the dimensions of Gulf events of comparable local magnitude. Conversely, moments for the Gulf events were about an order of magnitude larger (for the same M_L) than those for northern Baja. The average stress drop for the Gulf earthquakes was found to be lower than the average for northern Baja California. Nava and Brune (1981) and Nava (1980) did a more detailed study of a sample earthquake from each region, the Pino Solo earthquake from northern Baja California and the Mesa de Andrade earthquake from the Colorado Delta. Both earthquakes have approximately the same local magnitude (determined from the maximum amplitude measured on standard Wood-Anderson short period torsion seismographs). However, the relative long

period excitation was much larger for the Mesa de Andrade earthquake.

To obtain a more quantitative comparison of the relative high frequency excitation of northern Baja events, we may compare the various figures and spectra shown by Thatcher (1972). The spectral amplitudes at 4 Hz are typically a factor of 10 or more higher for northern Baja events than for West Coast of Baja or Gulf of California events of about the same moment. We can also estimate this difference from Table 2 of Thatcher by extrapolation from the corner frequency using the tabulated slopes of the spectra. Again, we find that the amplitudes estimated at 4 Hz are typically 10 times higher for the high stress drop northern Baja events as compared to the Gulf of California events of about the same M (moment magnitude).

Since peak accelerations would be expected to be closely related to the excitation of 4 Hz energy, the above results are a clear indication that the near source peak accelerations for the norther Baja type high stress drop events can be considerably higher than for events of comparable M_s in the Imperial Valley. The actual difference in peak acceleration will depend on the nature of the rupture mechanism. Nava (1980) and Nava and Brune (1981) inferred that, in addition to the factors cited by Thatcher (1972) for northern Baja events, namely, lower source dimensions (for a given M_L), and higher stress drops,

another factor operating in some cases was the relative simplicity of rupture for the northern Baja events. Relatively lower average stress drops in the Imperial Valley might be expected on the basis of the high heat flow, thick sediments and high water content. Whatever the physical mechanics responsible for the differences between the Salton Trough type earthquakes and the northern Baja type earthquakes, the direct observation of greater high frequency excitation for northern Baja events indicates that assuming that the accelerations observed for the IV-79 earthquake are "typical" or "conservative" is unwarranted, and earthquakes of the same M_s can have considerably higher near source accelerations.

Implications of the El Alamo Earthquake of 1956

The 1956 El Alamo earthquake has been considered in FSAR Question 361.68. However, in that consideration, it was assumed to have the same characteristics as the 1979 Imperial Valley earthquake. In view of the above cited evidence for differences between northern Baja earthquakes and Salton Trough earthquakes, I do not believe the 1956 El Alamo earthquake has been adequately considered.

There is strong evidence that the El Alamo earthquake generated higher accelerations than the IV-79 earthquake at comparable distances. For example, the intensity map for the El Alamo earthquake shows an area of intensity VI about thirty times larger than the area of intensity VI for IV-79 (compare attached figures).

The high amplitude and high frequency content of the record at El Centro is in striking contrast to the records at similar distances from the IV-79 earthquake. It is mentioned in the FSAR Question 361.68 that the rupture in the El Alamo earthquake was to the southeast, away from San Diego. If the rupture had proceeded northwestward directively focussing of energy could have led to even higher intensities in southern California. Thus, available evidence indicates that the El Alamo earthquake had

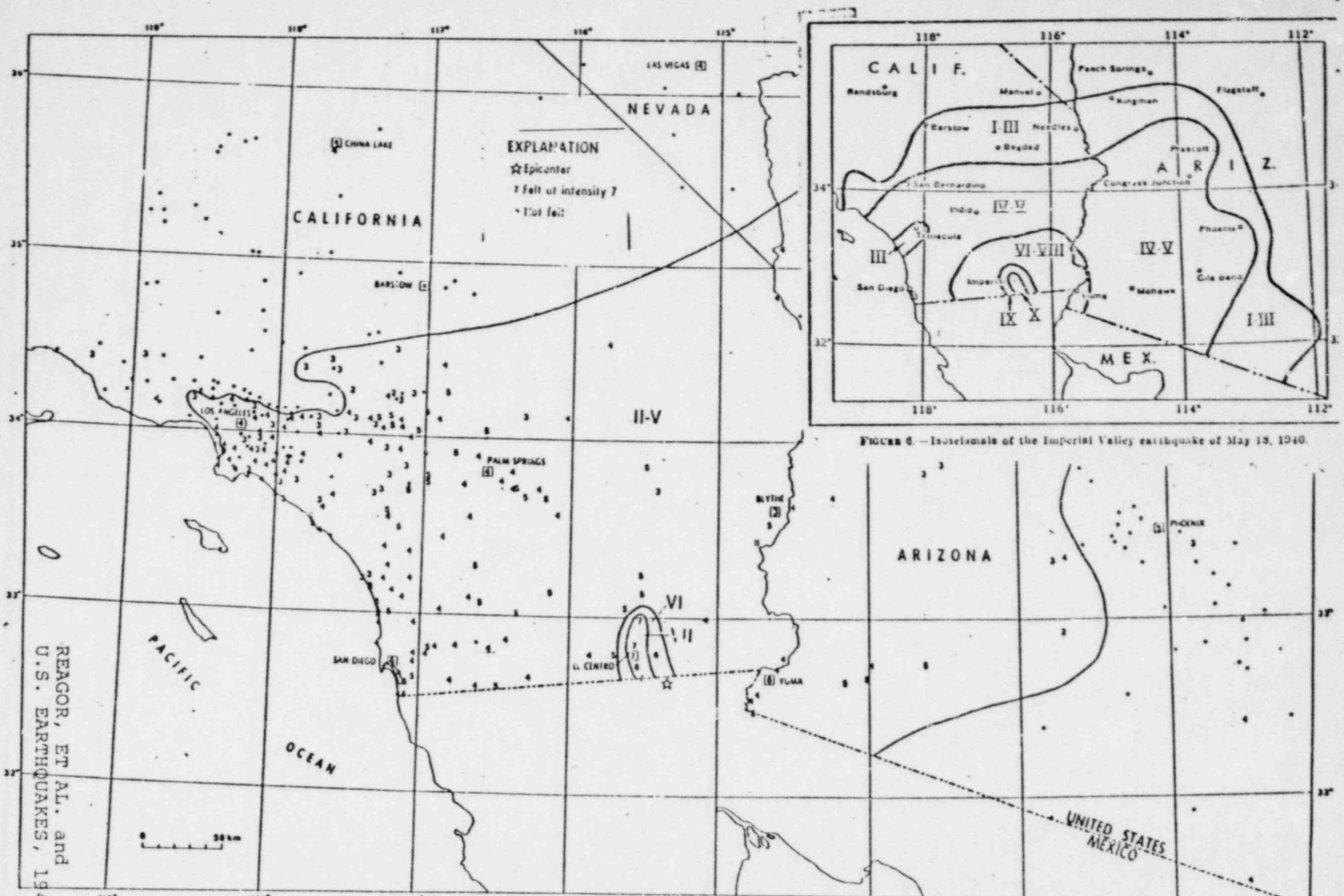


FIGURE 6.—Isoseismals of the Imperial Valley earthquake of May 18, 1940.

Figure 1.–Isoseismal map for the Imperial Valley earthquake of October 15, 1979.

REAGOR, ET AL. and
U.S. EARTHQUAKES, 1940
(Inset)

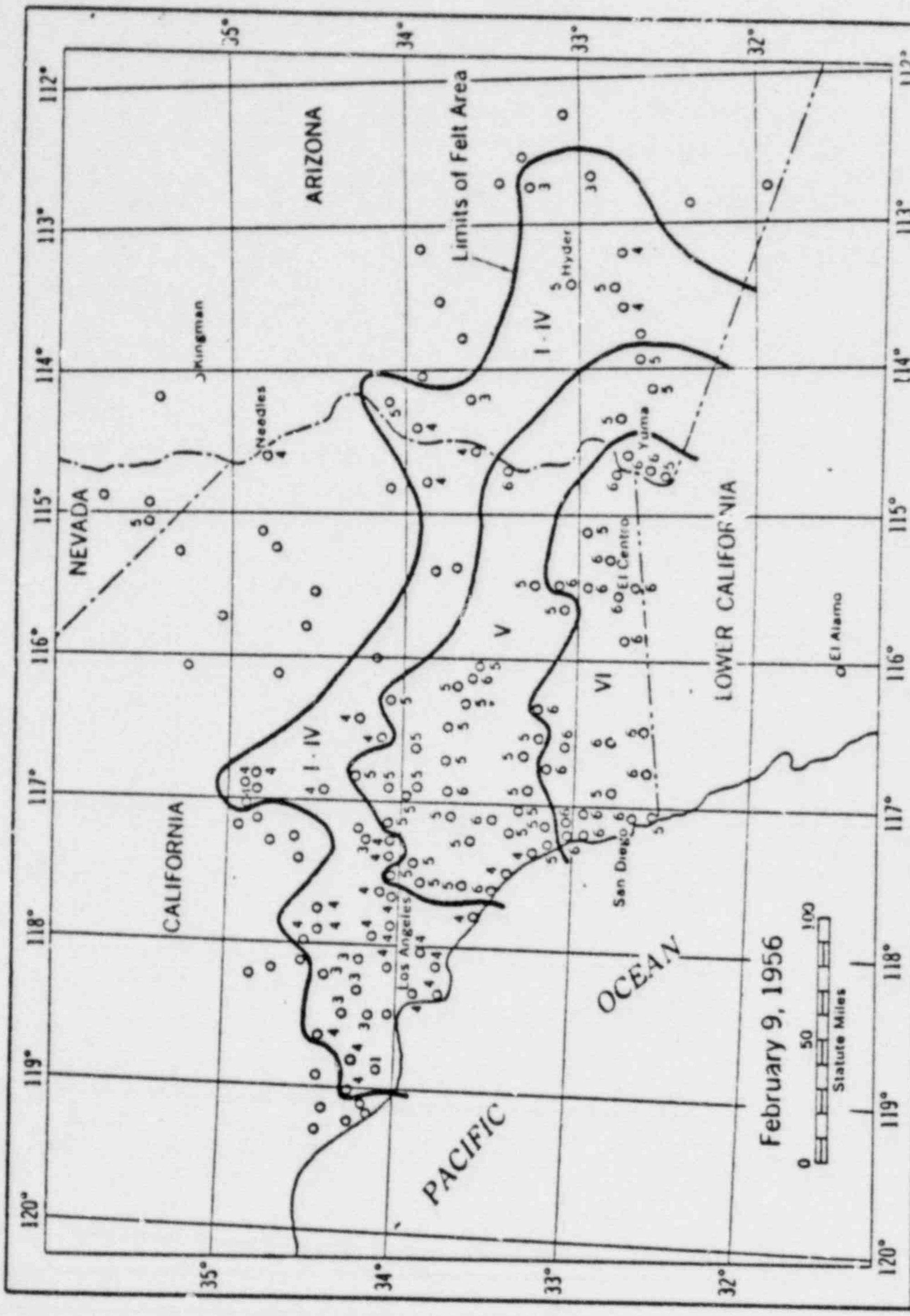


FIGURE 8.—Area affected by earthquake of February 9.

significantly higher near source accelerations than the IV-79 earthquake. A thorough study of the records of this earthquake should be made, rather than an unwarranted assumption that it was similar to IV-79.

Intensity Magnitude for the IV-79, IV-40 and El Alamo 1956 Earthquakes

We may use the area of intensity VI to define an Intensity Magnitude, M_I, using the relationship of Hanks, *et al.* (1975) between area of intensity VI and moment:

$$\text{Log } M_o = 1.97 \quad \text{Log } A_{VI} - 2.55$$

and the Hanks and Kanamori (1979) definition of Moment Magnitude:

$$M_{M_o} = \frac{2}{3} \log M_o - 10.7$$

Combining these relationships we define Intensity Magnitude as:

$$M_I = 1.31 \log A_{VI} - 12.4$$

Using this result we obtain, for the 1956 El Alamo earthquake $M_I = \sim 7.3$ $(A_{VI} \sim 10)^{15}$ cm^2), for the IV-40 earthquake, $M_I = 6.6$ (Hanks, *et al.*,1975) and for the IV-79 earthquake, $M_I = 5.3$ ($A_{VI} \sim 3 \times 10^{13}$ cm^2). This dramatically illustrates that the intensity of shaking, damage and high frequency energy in southern California was very low from the IV-79 earthquake and thus, the use of IV-79 peak accelerations as typical of southern California earthquakes is unjustified, and along with other evidence cited above, indicates that the assumption made in FSAR Question 361.68, namely, that the accelerations for the 1956 El Alamo earthquake were the same as for the IV-79 is also unjustified.

New Strong Motion Data

Victoria strong motion record of the June 9, 1980 northern Baja California earthquake (See attached Abstract, Simons, *et al.*, 1981)

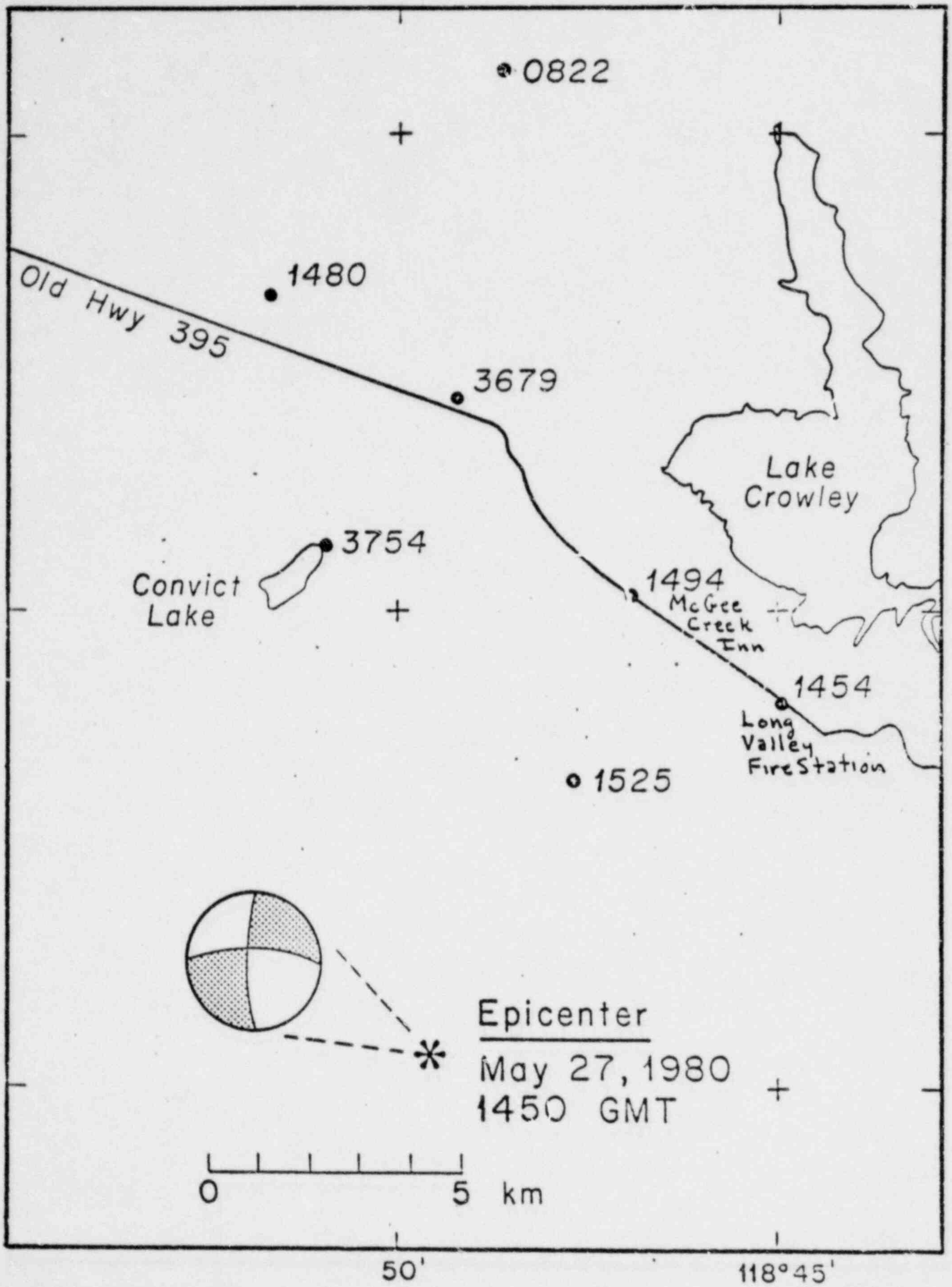
This recording for an $M_L = 6.2$, $M_s = 6.3$ event had peak vertical accelerations of over 1 g and horizontal accelerations of up to .85 g. It is of particular interest because it occurred in an environment similar to that of the IV-79 earthquake and, thus, indicates that peak horizontal accelerations similar to that recorded at the Bonds Corner station (.81 g), and vertical accelerations of over 1 g, are not unexpected for this magnitude ($M \sim 6\frac{1}{4}$) earthquake in the Imperial Valley. No surface break was observed along the primary fault (the Cerro Prieto fault) and at the present time, no fault plane solution or knowledge of rupture parameters is available. It is possible that the horizontal projection of the rupture at depth passed within less than 1 km of the station.

Previous earthquakes in this region, of magnitude less than 5, generated peak accelerations up to about .6 g at the same station and, thus, there may be local site conditions partially responsible for the high accelerations. Both

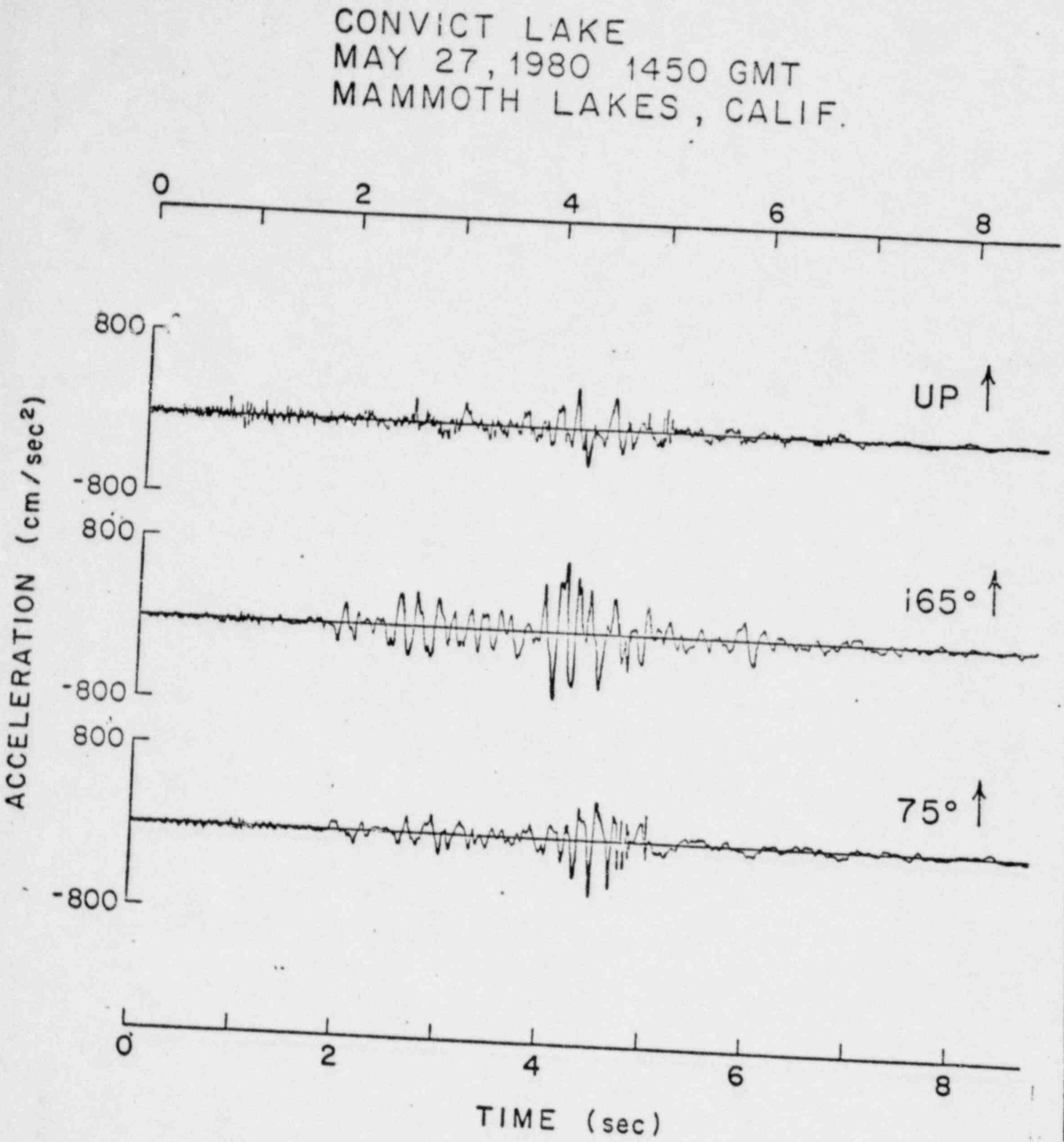
vertical and horizontal response spectra for this earthquake exceed the DBE for SONGS 2 and 3, at damping of 7%. A final assessment of the importance of this recording must await a better understanding of the rupture mechanism of the earthquake and better knowledge of the local structure.

Mammoth earthquake accelerations (John Anderson, personal communication, 1981)

The May 27, 1980 Mammoth earthquake, M_L = 6.2 (BRK) 6.3 (CIT), M_s = 6.0 (GS), M_b = 5.7 (GS) was recorded on three temporary strong motion stations at distances of approximately 10 km from the epicenter (See Map 1 attached). One station, "Convict Lake", located near the outflow of Convict Lake, recorded a peak horizontal acceleration of 0.72 g (See attached record). The other two stations, located at McGee Creek Inn and Long Valley Fire Station, recorded peak accelerations of 0.20 g and 0.35 g. The high acceleration recorded at the Convict Lake station may have in part been influenced by directivity, but a reliable interpretation of this data point must await a better understanding of the earthquake mechanism. The station was not located on any obvious topographic or structural feature which could explain the higher acceleration recorded. The instrument was resting on the ground, not anchored to a pier, and this may have led to some distortion of the signal.


0822
40'
1480
Old Hwy 395
3679
Lake Crowley
3754
Convict Lake
1494
McGee Creek Inn
35'
1454
Long Valley Fire Station
1525
Epicenter
May 27, 1980
1450 GMT
°30'
0
5 km
50'
118°45'

MAP 1


CONVICT LAKE
MAY 27, 1980 1450 GMT
MAMMOTH LAKES, CALIF.
0
2
4
6
8
800
-800
UP
165°
75°
ACCELERATION (cm/sec²)
TIME (sec)

Ground Motion from Possible Slip on the Cristianitos Fault

At the time of writing of this testimony, it has not, to my knowledge, been finally established whether or not the Cristianitos fault is to be judged capable. If it is, the distance to the fault trace would be reduced considerably. The expected peak ground motion would be increased. I believe that if the Cristianitos fault is judged capable, then the whole issue of ground motion must be re-opened, as the present FSAR and SER do not deal with this possibility.

above 15 Hz. The effect is frequency dependent, and does not show a significant reduction for frequencies below 5 Hz. For this particular situation, at a distance of about 5 km from the rupture surface of the fault, the reduction is more pronounced for horizontal components than for vertical.

AN EMPIRICAL ANALYSIS OF THE SOURCE OF ENERGY RELEASE DURING THE OCTOBER 15, 1979 IMPERIAL VALLEY EARTHQUAKE

CAMPBELL, KENNETH W. and MARK W. POLIT, TERA Corporation, 2150 Shattuck Avenue, Berkeley, CA 94704

The extensive set of strong-motion data recorded during the October 15, 1979 Imperial Valley Earthquake has been used to study statistically the probable sources of energy release on the fault rupture surface responsible for producing the peak ground motions at these stations. This was studied by regressing peak ground motion parameters on distance for a series of point sources, located both at the surface and at depth, distributed at equal intervals along the rupture from the epicenter to the northern end of the Imperial and Brawley Faults. Also investigated was the effect of considering multiple point sources. These results were compared to a similar expression which was based on the shortest distance from each station to the fault rupture surface.

Preliminary results for peak acceleration (PGA) indicate that no single point-source model can represent the attenuation characteristics of this parameter as well as the expression based on closest distance. Furthermore, these point-source models require a zone of constant acceleration within about one half a fault-length distance from the source in order to best fit the data. The point-source found to give the least scatter and best fit of all single sources investigated was located about 6 km. north of the epicenter. By eliminating data primarily south of the epicenter, this point was found to migrate some 15 to 20 km. north near the junction of the Imperial and Brawley Faults. By considering two point sources, a model almost as good as that for closest distance was obtained. These results suggest that PGA data cannot support a single, concentrated zone of energy release, but rather are consistent with either multiple zones or a single broad zone of energy release.

THE GUADALUPE VICTORIA STRONG MOTION RECORD FROM THE NEAR FIELD OF THE JUNE 9, 1980 NORTHERN BAJA CALIFORNIA EARTHQUAKE

SIMONS, R. S., BRUNE, J. N., ANDERSON, J. G., Institute of Geophysics and Planetary Physics, Scripps Institution of Oceanography, University of California, San Diego, La Jolla, California 92093; PRINCE, J., MENA, E., Institute of Engineering, Universidad Nacional Autonoma de Mexico, Apartado Postal 70-472, Mexico 20, D. F., Mexico

The June 9, 1980, "Victoria" earthquake (N. Baja California, M_L = 6.2, Cal Tech) was recorded on a strong-motion instrument (1 g digital Kinemetrics DSA-1) in the town of Guadalupe Victoria, ~11 km NW of the preliminary epicenter and close to the Cerro Prieto fault. The town of Guadalupe Victoria experienced severe damage.

The early part of the recording suffers from low magnetization level and consequent bit confusion. A combination of hardware and software techniques

has successfully recovered most of the data for the vertical and one hor- izontal (N 40° W) component. The recovered data have occasional gaps, one about .35 sec long and the rest less than .08 sec. The WWVB trace verifies the time scale. Filling the gaps with zero acceleration values yields accelerograms which integrate plausibly to velocity and displacement. Vertical accelerations reach or exceed 1 g several times in both the pos- itive and negative directions. In one 0.25 sec segment, the instantaneous vertical acceleration exceeds 1 g 4 times at frequencies as low as 10 Hz. The horizontal acceleration reaches 0.85 g and exceeds 0.5 g at times spanning an interval of ~9 sec. Horizontal peak velocities are ~40 cm/sec. The emergent beginnings of the records suggest a complicated source time history.

Acceleration response spectra have been computed from both available components, using a variety of damping factors. In all cases, the horiz- ontal response spectrum exceeds that for the Bond's Corner record of the October 15, 1979 Imperial Valley earthquake throughout most of the period range from 0.0 to 0.3 sec. This recording provides a unique opportunity to compare damage in the region (mainly adobe and brick masonry construc- tion) with recorded ground motion. The damage, corresponding to intensity ~VIII, was surprisingly moderate in view of the high response spectrum at short periods.

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SCALING LAWS FOR LARGE EARTHQUAKES

CONSEQUENCES FOR PHYSICAL MODELS

Christopher H. Scholz

Department of Geological Sciences and

Lamont-Doherty Geological Observatory

of Columbia University, Palisades, New York 10964

ABSTRACT

It is observed that the mean slip in large earthquakes correlates linearly with fault length L and is not related to fault width, W. If we interpret this in terms of an elastic model, it implies that static stress drop increases with aspect ratio (L/W). We also observe a tendency, particularly for strike-slip earthquakes, for aspect ratio, and hence static stress drop, to increase with seismic moment. Dynamic models of rupture of a rectangular fault in an elastic medium show that the final slip should be controlled by the fault width and scale with the dynamic stress drop. The only way these models can be reconciled with the observations is if dynamic stress drop correlates with fault length so that it is also nearly proportional to aspect ratio. This could only happen if fault length is determined by the dynamic stress drop. There are several serious objections to this, which lead us to suspect that these models may be poor representations of large earthquakes. Firstly, it conflicts with the observations for small earthquakes (modeled as circular sources) that stress drop is nearly constant and independent of source radius. Secondly,

it conflicts with the observation that fault length is often determined by rupture zones of previous earthquakes or tectonic complications. We speculate that the boundary condition at the base of the fault, that slip is zero, is unrealistic because that edge is in a ductile region at the base of the seismogenic layer. In a modal in which slip is not so constrained at the base of the fault nor at the top (the free surface), such that no healing wave originates from these edges, final slip would be determined by fault length. The observations would then be interpreted as meaning that the static and dynamic stress drops of large earthquakes are nearly constant. These two alternatives predict very different scaling of the dynamics of large earthquakes. The width-dependent model predicts that average particle velocities are larger for long ruptures but the rise time will be the same as in a shorter event of the same width. The length-dependent model predicts the opposite.

INTRODUCTION

A central problem in earthquake seismology has been to find scaling laws that relate the static parameters such as slip and stress drop to the dimensions of the rupture and to understand these relationships in terms of the dynamic parameters, the most fundamental of which are rupture velocity and dynamic stress drop.

In doing so, it is essential to distinguish between small earthquakes and large earthquakes. Tectonic earthquakes nucleate and are bounded within a region of the earth between the surface and a depth h_o, the seismogenic layer. The seismogenic depth, h_o, depends on the tectonic environment but in a given region the maximum width of an earthquake occurring on a fault of dip δ is $W_o = h_o/\sin\delta$. We will define a small earthquake as one with a source radius $r \leq W_o/2$ and a large earthquake as one in which $r > W_o/2$. Thus a small earthquake can be represented as a circular source in an elastic medium, whereas a large earthquake is more suitably treated as a rectangular rupture with one edge at the free surface.

It has been repeatedly demonstrated (e.g., Aki, 1972; Thatcher and Hanks, 1973; Hanks, 1977) that the stress drops of small earthquakes are nearly constant and independent of source dimensions. This result, when interpreted with dynamic models of finite circular ruptures (Madariaga, 1976; Archuleta, 1976; Das, 1980), simply means that the dynamic stress drop is constant.

If the same were true for large earthquakes, the dynamic models of rectangular faulting in an elastic medium (Day, 1979; Archuleta and Day,

OBSERVATIONS

For small earthquakes, using the definition of seismic moment, M_o, and the relationship

$$\Delta\sigma = \frac{7\pi}{16}\mu\frac{\bar{u}}{r}$$

where r is source radius, $\bar{u}$ is mean slip, and $\Delta\sigma$ is stress drop. If stress drop is constant, the relationship between M_o and fault area, A, is

$$M_o = \left(\frac{16\Delta\sigma}{7\pi^{3/2}}\right) A^{3/2} \quad . \tag{1}$$

Large earthquakes, however, are more nearly rectangular ruptures of width W and length L and in this case, for an elastic model in which slip is restricted to be within W,

$$\Delta\sigma = C\mu\frac{\bar{u}}{W} \tag{2}$$

where C is a geometrical constant.

If stress drop were constant, we would expect to find that

$$M_o = \frac{\Delta\sigma}{C}LW^2 \tag{3}$$

In Figure 1 we show a plot of log LW^2 vs. log M_o, for the large interplate thrust and strike-slip earthquakes from the data set of Sykes

and Quittmeyer (1981). These observations are listed in Table 1. The data for each type of earthquake define a line, but with a slope less than one, indicating that stress drop systematically increases with moment. The offset between the data for the strike-slip and thrust events is also an important feature that we will discuss later.

These data indicate that $\bar{u}$ is not simply related to W and that $\Delta\sigma$ is not constant for large earthquakes. On the contrary, many workers (e.g., Bonilla and Buchanan, 1970; Slemmons, 1977) have argued that $\bar{u}$ correlates with L, and recently Sykes and Quittmeyer (1981) have argued that the correlation is linear. Plots of $\bar{u}$ vs. L on linear scales are shown in Figures 2 and 3 for strike-slip and thrust earthquakes, respectively.

In view of the usual uncertainties in the estimates of $\bar{u}$ and L, and any naturally occurring variations in dynamic stress drop (with which slip should be expected to scale), the correlation between $\bar{u}$ and L is fairly strong. We fit it with a straight line with an intercept at the origin

$$\bar{u} = \alpha L \qquad (4)$$

and find that $\alpha \simeq 2 \times 10^{-5}$ for the thrust events and 1.25×10^{-5} for the strike-slip events. At least for the strike-slip events, slip is clearly not dependent on width because the widths of all the events in Figure 2 are between 10-15 km, i.e., they are essentially the same.

From this observation we would then expect that

$$M_o = \mu\alpha L^2 W \qquad (5)$$

which is confirmed in Figure 4. For reference, the line drawn through the data has a slope of one.

Since

$$L^2W \equiv A^{3/2}(\frac{L}{W})^{\frac{1}{2}}$$

and since the aspect ratio L/W varies only by a factor of about 20 in the data-set, we would have found a good correlation between M_o and $A^{3/2}$, as did Aki (1972) and Kanamori and Anderson (1975) had we plotted log A vs. log M_o. The question is not whether M_o correlates better with L^2W than with $A^{3/2}$. The issue of concern is that Kanamori and Anderson's interpretation of their correlation as meaning that stress drop is constant is only true if L/W is constant, because from (2) and (4), we have

$$\Delta\sigma = C\mu\alpha\frac{L}{W} \quad . \tag{6}$$

That L/W is a constant is an explicitly stated assumption of Aki (1967, 1972) and Kanamori and Anderson (1975), and although Abe (1975) and Geller (1976) attempted to observationally justify this assumption, it is not generally true. In Figures 5 and 6 we plot $\Delta\sigma$ vs. L/W for the two types of earthquakes. The correlation between them is very clear for the strike-slip events, and less so for the thrust events, for which there is a much smaller variation of aspect ratio. That L/W does not have a large variation for the thrust events seems to simply result from the fact that the seismogenic width of subduction zones, W_o, is about 100 km, so that only extremely large events can achieve high values of aspect ratio.

We can now understand why stress drop increases systematically with M_o, as shown in Figure 1. The width of large strike-slip earthquakes is

limited by the seismogenic depth to $W_o \simeq 15$ km so that they grow principally in the L direction. This results in a systematic increase in L/W, and hence $\Delta\sigma$, with M_o. The subduction zone thrust earthquakes have different widths but L increases faster than W with increasing moment, producing the same result, i.e., $\Delta\sigma$ increases with L/W or M_o. The offset between the data for thrust and strike-slip events in Figure 1 occurs simply because the widths of the thrust events are much greater than those of the strike-slip events. A strike-slip event must have a much greater aspect ratio, and hence stress drop, than a thrust event of the same moment.

PHYSICAL CONSEQUENCES

The principal feature of the observations that we wish to explain is the correlation between slip and fault length. It is a surprising observation because intuition would first lead one to expect slip to depend on width, yet this is not observed. This intuition is re-inforced by the results of dynamic models of rectangular faults in an elastic medium (Day, 1979; Archuleta and Day, 1980; Das, 1981). These models show that slip is controlled by the width of the fault and that it scales with dynamic stress drop.

The situation is illustrated in Figure 7, which shows surface slip along the fault for two representative strike-slip earthquakes. These earthquakes have essentially the same width, and differ only in length. If the dynamic stress drop were the same for these two earthquakes, then according to the theory, the Ft. Tejon earthquake would be the equivalent of six Mudurnu earthquakes placed end to end. Clearly that is not the case.

If the dynamic, elastic models are correct representations of earthquakes, then the only way they can be reconciled with the observations is if dynamic stress drop correlates with aspect ratio. Since the width of strike-slip events is nearly constant, and the width varies much less than length for the thrust events, this would be approximately true if dynamic stress drop correlates linearly with fault length. The only way this can happen without violating causality is if fault length is determined by dynamic stress drop. This is not an entirely unphysical proposition, because dynamic stress drop determines the stress intensity factor, which

is important in fracture growth. It is not obviously apparent, however, why L should increase linearly with $\Delta\sigma_d$, the dynamic stress drop.

There are several major objections to this interpretation. The first is that we have to assume that for large earthquakes $\Delta\sigma_d$ determines the rupture length, which directly contradicts the observations for small earthquakes. Although stress drop appears to increase with source radius over a limited range in some data sets (Aki, 1980), it shows no obvious variation with source radius over a very broad range (Hanks, 1977). We can offer no reasonable explanation for why large earthquakes should behave differently than small earthquakes in this important respect.

A second objection is that this assumption conflicts with the principal observations that led to the concept of seismic gaps: that the length of large earthquakes is often controlled by the rupture zones of previous earthquakes or by structural features transverse to the fault zone. Of course, one could soften the original assumption to: $\Delta\sigma_d$ determines the length unless the rupture encounters a rupture zone of a previous earthquake or a transverse feature. The rejoinder is that if the latter were as common as is thought, it would have the effect of destroying the correlation between $\bar{u}$ and L that is observed.

It is worth giving a specific example. If we compare the 1966 Parkfield earthquake (L = 30 km, $\bar{u}$ = 30 cm, W = 15 km) and the 1906 San Francisco earthquake (L = 450 km, $\bar{u}$ = 450 cm, W = 10 km) we need to explain the difference in $\bar{u}$ by a difference in $\Delta\sigma_d$ of about a factor of 15. Since the correlation between $\bar{u}$ and L is also good in these examples, we also need to argue that $\Delta\sigma_d$ determined L in these cases. On the other hand, it can be argued that the length of the 1966 earthquake was determined by the length of the gap between the rupture zone of the 1857 earthquake (or the

fault offset near Cholame) and the southern end of the creeping section of the San Andreas fault. Similarly, the 1906 earthquake filled the gap between the northern end of the creeping section at San Juan Bautista and the end of the fault at Cape Mendocino. If our argument that $\Delta\sigma_d$ determines L is true, then these latter observations are coincidences. Almost identical arguments can be made for many of the other earthquakes in our data set.

The third point is less an objection than a surprising consequence of this interpretation. The Hoei earthquake of 1707 ruptured about 500 km of the Nankai trough in Japan (Ando, 1975; Shimazaki and Nakata, 1980). The same plate boundary was ruptured twice subsequently, in two sets of delayed multiple events, the Ansei I and II events of 1854, and the Tonankai and Nankaido events of 1944 and 1946. In support of a time-predictable model of earthquake recurrence, Shimazaki and Nakata argued that the greater recurrence time between the first two sequences (147 years) and the second (91 years) is because the slip (and stress drop) were greater in 1707 than in either 1854 or 1946, the greater uplift at Muroto Point in 1707 (1.8 m) than in 1856 (1.2 m) or 1946 (1.15 m) being the evidence. The reason why this should happen is readily explained by the correlation between $\bar{u}$ and L. Thus the ratio of fault length of the Hoei and Ansei II earthquakes, 500 km/300 km = 1.7 can explain the ratio of uplift at Muroto Point, 1.8/1.2 = 1.5 and recurrence time, 147/91 = 1.6.

However, if this is interpreted as being due to a difference in dynamic stress drop, then one has to argue that a significant change in dynamic stress drop (50%) can occur on the same fault zone between successive earthquakes. One could argue that this could occur because the slip in one earthquake might change the relative position of asperities on the

stopping crack (Savage, 1965). A stopping phase cannot physically stop the slip in these models because such a wave will lose energy with distance whereas the results of the models are independent of dimension. A healing wave must be interpreted as a wave that propagates into the interior of the rupture in an analogous way, and for analogous physical reasons, as the stopping of cars on a highway propagates up the stream of traffic. Causality restricts it to travel at a velocity slower than a stopping phase. Thus Madariaga (1976, p. 648) observed, "It appears as if a 'healing' wave propagates inward from the edge of the fault some time after the P and S stopping phases."

Since slip is terminated by the healing wave, the rise time and final slip at any point on the fault is determined by the distance to the nearest boundary (Day, 1979; Das, 1981). Therefore it is easy to see why mean slip on a rectangular fault should be controlled by the fault width.

A healing wave is the result of the boundary condition that $u = 0$ at the edges of the fault. If the models are poor representations of large earthquakes, the most likely problem is that these boundary conditions are unrealistic. The models are of rectangular faults embedded in an elastic whole space. The boundary condition $u = 0$ is imposed on all edges of the fault and healing waves thus propagate from each edge. Since large earthquakes rupture the free surface, slip is unconstrained there and a healing wave will not propagate from that edge. However even if an elastic half-space model were available, we would still expect slip to be width-dependent since it would be controlled by the healing wave from the base of the fault.

In large earthquakes the base of the fault is at the bottom of the seismogenic layer. A plausible explanation for the seismogenic depth is

that it is the result of a brittle-ductile transition. Thus a large earthquake cannot propagate to greater depth because the energy at the crack tip is dissipated in plastic deformation. A more realistic model then may be one in which the base of the fault is in a plastic, rather than elastic, region and therefore the condition $u = 0$ is no longer valid at that edge.

We illustrate in Figure 8 the difference between an elastic model and an elastic-plastic model. The most significant difference is that in the elastic-plastic model (Figure 8b) slip at the base of the fault may be allowed to be greater than zero as a result of plastic deformation in a zone surrounding the rupture tip. This is simply the equivalent, in shear, of the blunting of a crack tip that occurs in tensile crack propagation in ductile materials. The plastic deformation around the base of the fault smooths out the stress singularity associated with finite slip there, and will continue as long as slip continues. This may have the effect of inhibiting a healing wave from originating at the base, and if healing waves propagate only from the ends of the fault, slip and rise time will depend on fault length, not width.

No model is available with these boundary conditions but we can approximate one. If we make the approximation that slip stops abruptly with the arrival of the healing wave, then the final slip on the fault will be, from (7),

$$u(x,y) = \dot{u}_o\left(t_h^{\,2} - \frac{(x^2+y^2)}{v^2}\right)^{1/2} \qquad (9)$$

which we can calculate. This is a 'quasidynamic' model (Boatwright, 1980), i.e., a kinematic model that simulates a dynamic model.

It can readily be shown for the circular case that (9) yields final slip values that are everywhere within 5% of that of the dynamic numerical models of Madariaga (1976) and Das (1980), and Day (1979) has shown that (9), when properly truncated, also yields a very good approximation to final slip in his rectangular models. We use it to simulate an elastic-plastic half-space model by simply assuming that no healing wave propagates from either the top or bottom of the fault.

The procedure we use is very similar to that used by Day (1979, pp. 23-26), and simply involves the calculation of t_h. We assumed $v = 0.9\beta$, for which the corresponding value of K is 0.81 (Dahlen, 1974), and that the velocity of the healing wave is $\sqrt{3}\beta$. In Figure 9 we show slip at the surface as a function of distance from the center of the fault for a bilateral case with L/W = 4. The mean slip is found to scale as

$$\bar{u} = \frac{1}{2}\frac{\Delta\sigma_d}{\mu}L \qquad (10)$$

so this model would lead to the interpretation that the linear correlation between $\bar{u}$ and L that is observed means that the dynamic stress drop for large interplate earthquakes is approximately constant. Equating (10) with (4) we obtain $\Delta\sigma_d \simeq 12$ bars and 7.5 bars for thrust and strike-slip earthquakes, respectively. Returning to Figure 4, the line drawn through the data is the prediction of this model for $\Delta\sigma_d = 10$ bars. Furthermore, in this model, where slip is unconstrained at top and bottom, static stress drop will also be a function of fault length, since the scale length that determines the strain change will be the fault length. The observation made earlier that $\Delta\sigma$ is a function of aspect ratio is due to the incorrect use of equation (2) to calculate it. According to this model, $\Delta\sigma$ is also approximately constant for these earthquakes.

DISCUSSION

The observation that slip increases with fault length in large earthquakes poses severe consequences when viewed in the light of dynamic rupture models. In conventional dynamic models (W models), slip is determined by fault width, rather than length. These models can only be reconciled with the observations if it is assumed that the dynamic stress drop determines the fault length, and the several major objections to this possibility were detailed earlier. With different assumptions concerning the boundary conditions at the base of the fault, it may be possible to construct a dynamic model in which slip depends on fault length (L model). This model avoids the objections raised to the W model but is based on a speculative, although not entirely ad hoc, assumption concerning the boundary conditions.

Furthermore, severe constraints are placed on L models from the geodetic data obtained for the 1906 San Francisco earthquake. The simplest form of L model is one in which slip is totally unconstrained at the base of the fault. If this were the case, strain release would extend out to distances comparable to fault length, rather than depth, but as Brune (1974) has pointed out, the strain release in 1906 was concentrated within a few tens of km from the fault. From angle changes in the Pt. Arena triangulation network [angle + from Thatcher (1975, Fig. 4)] one can estimate a strain drop of 8×10^{-5} within 12 km of the fault, a figure somewhat more consistent with a W model than an L model. Thus if L models are relevant, they must be models in which slip is only partially constrained at the base of the fault. In the absence of numerical modeling of

this type, one can't tell if this type of model will result in L scaling or hybrid scaling intermediate to the L and W extremes.

These L and W models represent, in many respects, opposite extremes concerning the mechanism of large earthquakes and so it is useful to discuss the contrasting way in which they scale. For earthquakes in which $L < 2W$, the models are indistinguishable in their gross manifestations. In Figure 10 we schematically show a comparison between an earthquake of dimensions about $L = 2W$ and one of the same width but about 15 times longer. Specifically, this might be a comparison of the 1966 Parkfield earthquake, say, and the 1906 San Francisco earthquake.

On the left of the figure we show a snapshot of slip on the fault during the smaller earthquake. We only show the part that is actually slipping during the snapshot. We also show the time history of slip at some representative point. For simplicity, it is simply shown as a ramp with a rise time, t_R. On the right is shown the predictions of the two models for the longer earthquake.

In a bilateral case, as shown, the W model predicts that the slipping portion of the fault splits into two patches of length $\sim W$ that propagate away from each other at a velocity 2v as they sweep over the fault surface. Since the rise time $t_R \approx W/2\beta$, remains the same but the slip is fifteen times greater, the dynamic stress drop, and hence particle velocity, must be fifteen times greater.

In the L model, the rupture sweeps out over the fault as an expanding patch, with slip continuing within its boundaries until after the final dimensions are reached. In that model, the dynamic stress drop and particle velocities are the same as in the smaller event, but the rise time, $t_R \approx L/2\beta$ is much longer.

In terms of predicting the strong ground motions for a 1906 size earthquake, say, from observed ground motions for a 1966 size earthquake, the difference between the W and L model is critical. The W model would predict that the average particle velocities would be much higher and the duration would be about the same. The L model would predict nearly the opposite.

Suppose we start with a square rupture of width W_o and consider how peak particle velocity, $\dot{u}_p$, and the asymptotic particle velocity, $\dot{u}_o$, increase for ruptures of greater length. For a square rupture with dynamic stress drop, $\Delta\sigma_d^S$, the maximum value of $\dot{u}_p$ and the asymptotic value $\dot{u}_o$ will be

$$\dot{u}_p^S \propto \Delta\sigma_d^S \sqrt{W_o}$$

and (11)

$$\dot{u}_o^S \propto \Delta\sigma_d^S$$

Using the W model, for a rupture of width W_o and length $L > W_o$, the stress drop will have to be greater by the ratio

$$\frac{\Delta\sigma_d^W}{\Delta\sigma_d^S} = \frac{L}{W_o}$$

so that

$$\dot{u}_p^W \propto \Delta\sigma_d^S \frac{L}{W_o} \sqrt{W_o}$$

and (12)

$$\dot{u}_o^{\,W} \propto \Delta\sigma_d^{\,S} \frac{L}{W_o}$$

For the L model, stress drop is the same but the scale length that determines the maximum peak velocity becomes L rather than W, so that

$$\dot{u}_p^{\,L} \propto \Delta\sigma_d^{\,S} \sqrt{L}$$

and (13)

$$\dot{u}_o^{\,L} \propto \Delta\sigma_d^{\,S}$$

Comparing (12) and (13), the two models differ in the ratios

$$\frac{\dot{u}_p^{\,L}}{\dot{u}_p^{\,W}} = \sqrt{\frac{W_o}{L}}$$

and (14)

$$\frac{\dot{u}_o^{\,L}}{\dot{u}_o^{\,W}} = \frac{W}{L}$$

So that with a W model, from (12), both peak and asymptotic velocities for a 1906 type earthquake would be about 15 times greater than for the Parkfield earthquake. For the L model, from (13), the peak velocities would at maximum be about $\sqrt{15}$ greater for a 1906 than a 1966 event, but the asymptotic value would be the same.

These remarks, of course, apply only to the simple case of a smoothly propagating rupture. Any heterogeneity will produce local high frequency variations in the velocities. However, they serve to point out the importance of determining if large earthquakes are better described by an L model or W model or by some intermediate case, if such can exist.

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TABLE 1

PARAMETERS OF LARGE INTERPLATE EARTHQUAKES
(AVERAGED FROM SYKES AND QUITTMEYER (1981))

No.	Date	Location	Mo 10^{27} dyne-cm	L km	W km	L/W	$\overline{u}$ cm	$\Delta\sigma$ bars
		Strike-Slip Earthquakes						
1.	10 Jul 1958	SE Alaska	4.3	350	12	29	325	26
2.	9 Jan 1857	S. California	7	380	12	32	465	36
3.	18 Apr 1906	San Francisco	4	450	10	45	450	44
4.	19 May 1940	Imperial Va., Ca.	0.23	60	10	6	125	13
5.	27 Jun 1966	Parkfield, Calif.	0.03	37	10	4	30	4
6.	9 Apr 1968	Borrego Mtn, Ca.	0.08	37	12	3	25	3
7.	15 Oct 1979	Imperial Va., Ca.	0.03	30	10	3	30	4
8.	4 Feb 1976	Guatemala	2.6	270	15	18	150	9
9.	16 Oct 1974	Gibbs F. Z.	0.45	75	12	6	170	14
10.	26 Dec 1939	Ercincan, Turkey	4.5	350	15	23	285	18
11.	20 Dec 1942	Erbaa Niksar, Turkey	0.35	70	15	5	112	8
12.	1 Feb 1944	Gerede-Bolu, Turkey	2.4	190	15	13	275	18
13.	18 Mar 1953	Gönen-Yenice, Turkey	0.73	58	15	4	280	21
14.	22 Jul 1967	Mudurnu, Turkey	0.36	80	15	5	100	7
		Thrust Earthquakes						
15.	6 Nov 1958	Etorofu, Kuriles	44	150	70	2.1	840	37
16.	13 Oct 1963	Eruppu, Kuriles	67	275	110	2.5	445	12
17.	16 May 1968	Tokachi-oki, Japan	28	150	105	1.4	355	10
18.	11 Aug 1969	Shikotan, Kuriles	22	230	105	2.2	180	5
19.	17 Jun 1973	Nemuro-oki, Japan	6.7	90	105	0.86	140	5
20.	4 Nov 1952	Kamchatka	350	450	175	2.6	890	14
21.	28 Mar 1964	Prince Wm Sound, Alaska	820	750	180	4.2	1215	18
22.	4 Feb 1965	Rat Island, Aleutians	125	650	80	8.1	480	10
23.	10 Jan 1973	Colima, Mexico	3	85	65	1.3	110	5
24.	29 Nov 1978	Oaxaco, Mexico	3	80	70	1.1	110	5
25.	22 May 1960	S. Chile	2000	1000	210	4.8	1900	21
26.	17 Oct 1966	C. Peru	20	80	140	0.6	360	12

FIGURE CAPTIONS

Figure 1. Plot of log LW^2 vs. log M_o for the large intraplate earthquakes from the data set of Sykes and Quittmeyer (1981). The lines of slope 1 are constant stress drop lines, assuming C = 0.6 for the thrust events, and 0.3 for the strike-slip events.

Figure 2. A plot of mean slip, $\bar{u}$, vs. fault length for the strike-slip events. The line drawn through the data has a slope of 1.25×10^{-5}. Numbers are references to Table 1.

Figure 3. The same as Figure 2, for the thrust events. The slope of the line is 2×10^{-5}.

Figure 4. A plot of log L^2W vs. log M_o. The line drawn through the data has a slope of 1, for reference.

Figure 5. Stress drop plotted vs. aspect ratio for the strike-slip earthquakes.

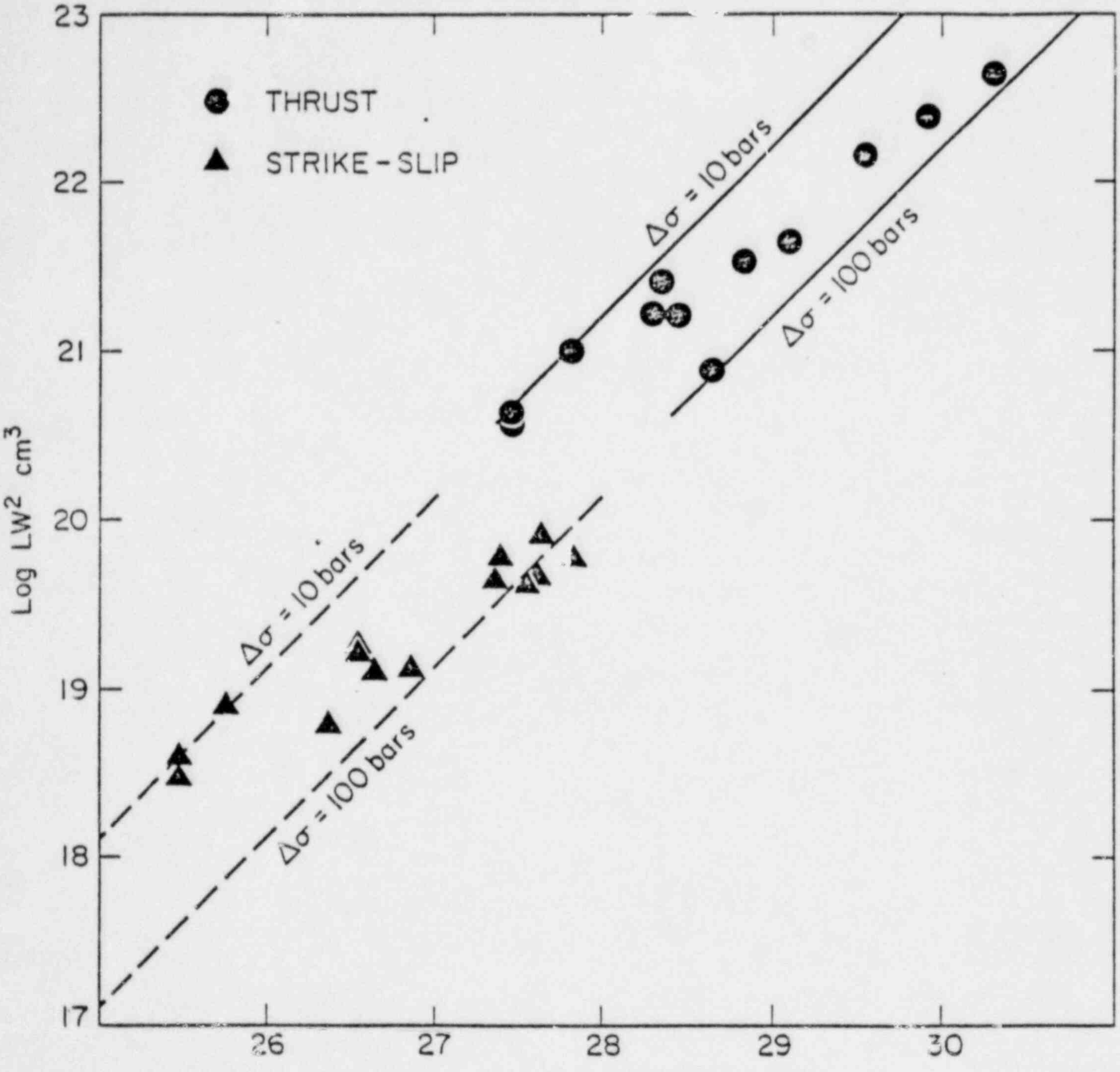
Figure 6. Stress drop vs. aspect ratio for the thrust earthquakes. Event 22 is an oblique slip event for which stress drop was calculated based only on the dip slip component and is hence underestimated. Event 15 is an anomalously deep event in the Kuriles (Sykes and Quittmeyer, 1981).

Figure 7. Schematic representation of two models of large earthquakes. In A, it is represented by rupture in an elastic half-space. The boundary condition at the base of the rupture is $u = 0$. In B, the rupture penetrates a ductile region. At the base $u > 0$, which is accommodated by plastic deformation in a zone surrounding the rupture tip.

Figure 8. Surface slip as a function of distance along the fault plane for two representative strike-slip earthquakes of similar width but different depth. Data for the Mudurnu earthquake is from Ambraseys (1969) and for the Ft. Tejon earthquake from Sieh (1978).

Figure 9. Dimensionless slip, u' vs. length, L', at the free surface from the center to the end of the fault. The model is a quasidynamic one that simulates a dynamic model with boundary conditions similar to those shown in Figure 7b, as described in the text. The normalization relations are $u = \frac{\Delta\sigma_d}{\mu} W u'$ and $L = WL'$. The case shown is bilateral with aspect ratio 4.

Figure 10. A schematic diagram to illustrate the contrasting way in which a model in which width determines the slip (W model) scales with length as compared to a length dependent model (L model).



THRUST
STRIKE-SLIP
Δσ = 10 bars
Δσ = 100 bars
Δσ = 10 bars
Δσ = 100 bars
23
22
21
20
19
18
17
26
27
28
29
30
Log LW² cm³
Log M₀, dyne-cm

Figure 1

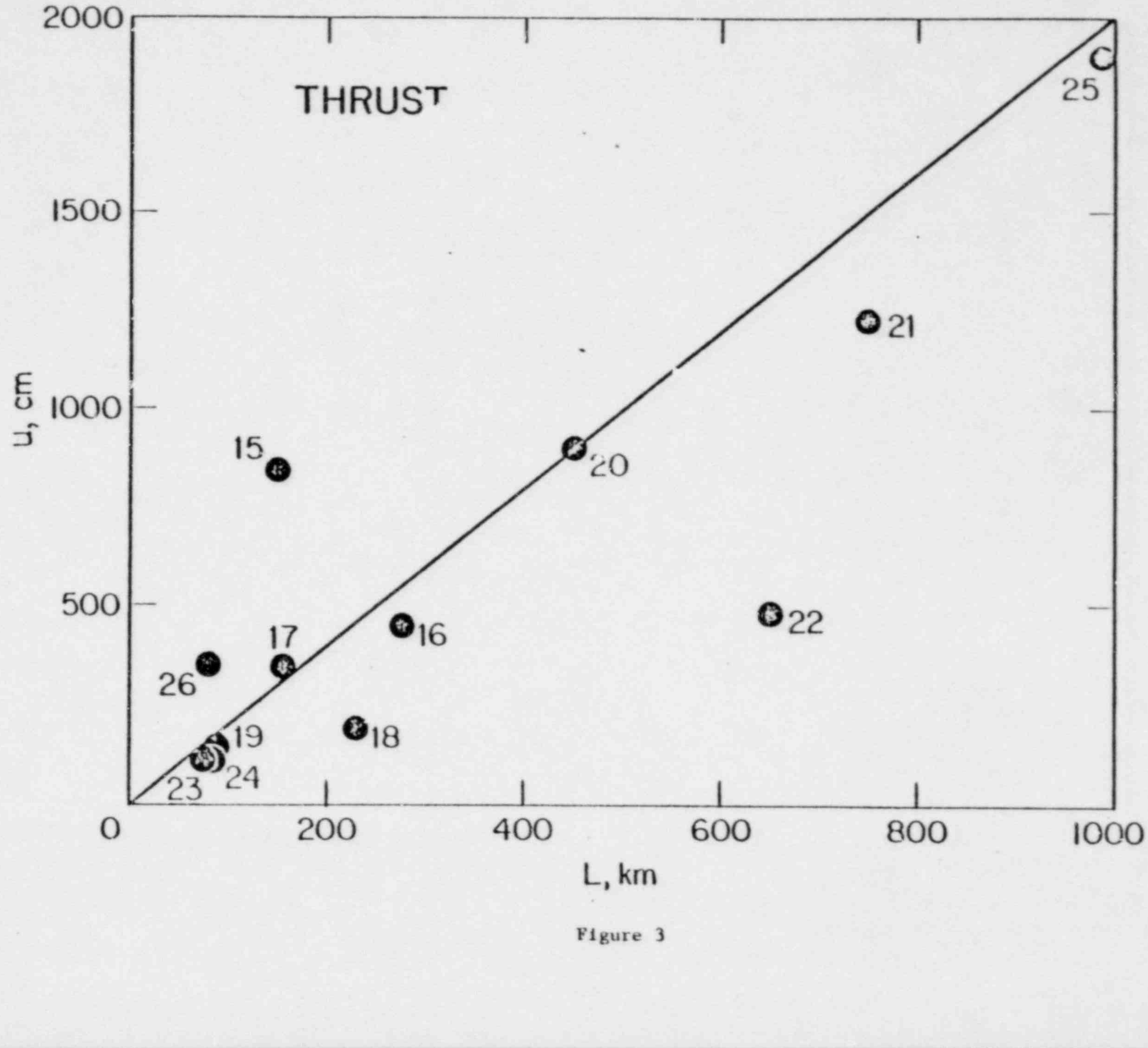
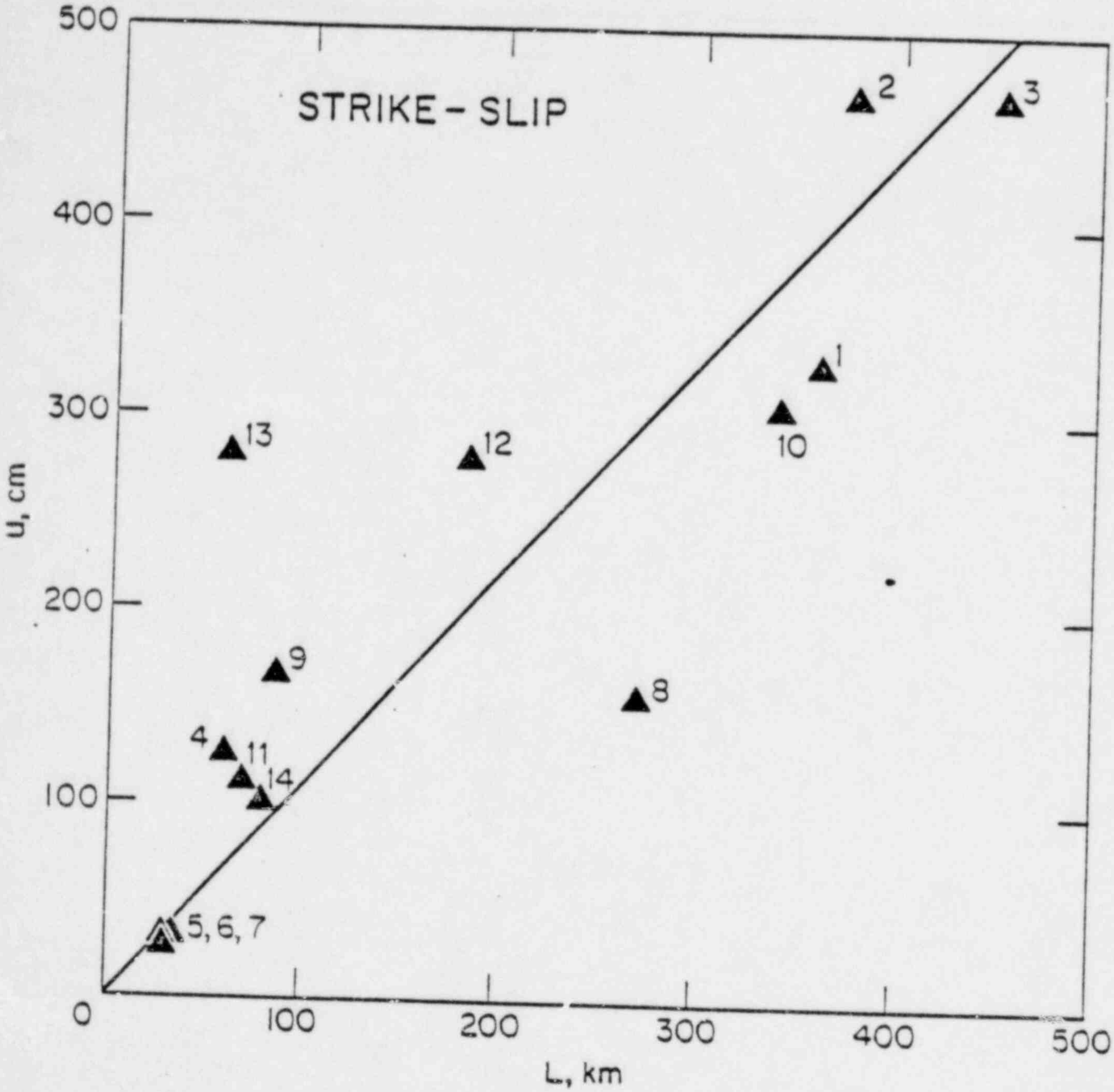

THRUST
u, cm
L, km
2000
1500
1000
500
0
200
400
600
800
1000
15
16
17
18
19
20
21
22
23
24
25
26

Figure 3



STRIKE - SLIP
500
400
300
200
100
0
100
200
300
400
500
u, cm
L, km
1
2
3
4
5, 6, 7
8
9
10
11
12
13
14

Figure 2

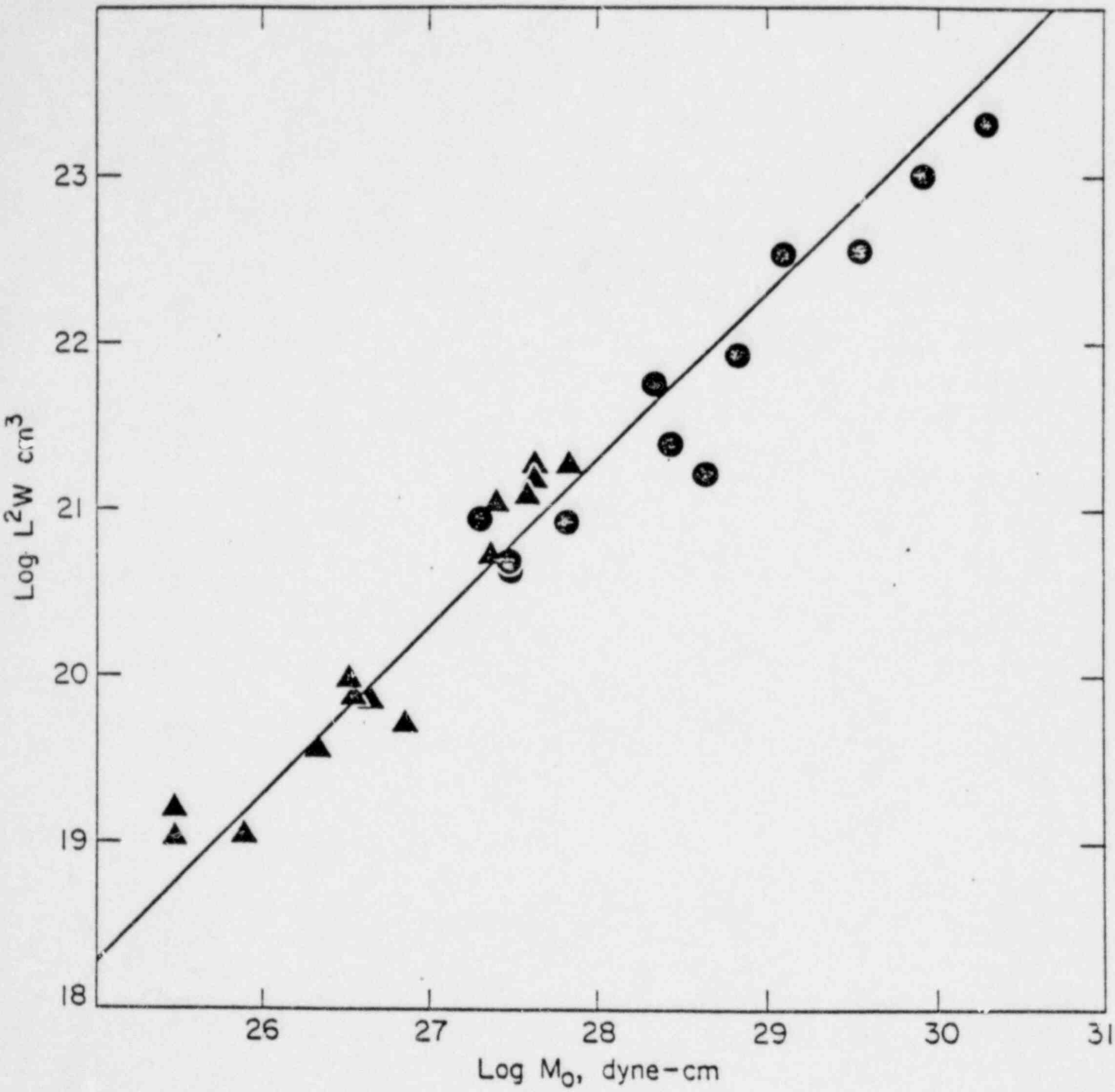


Figure 4

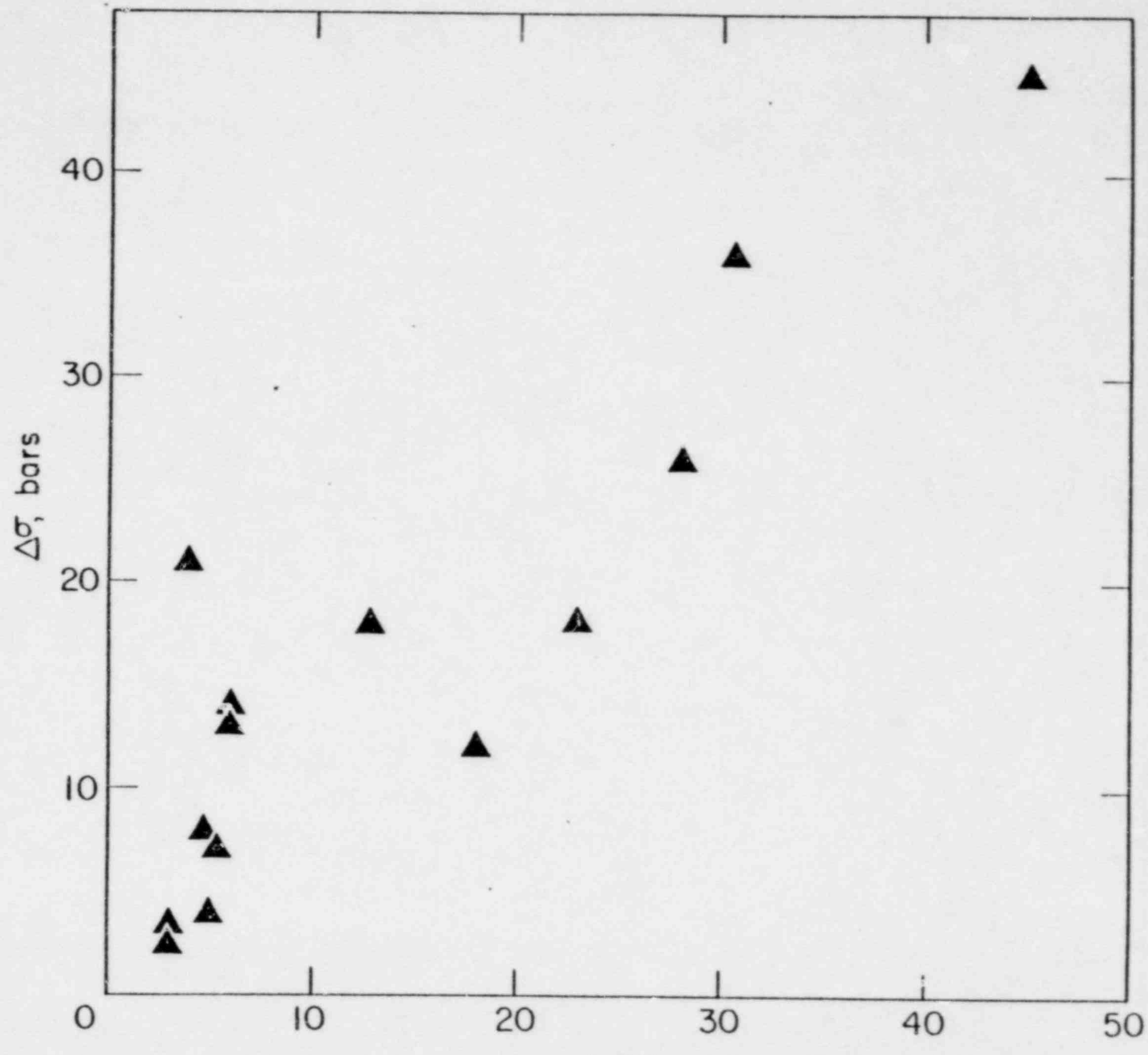


Figure 5

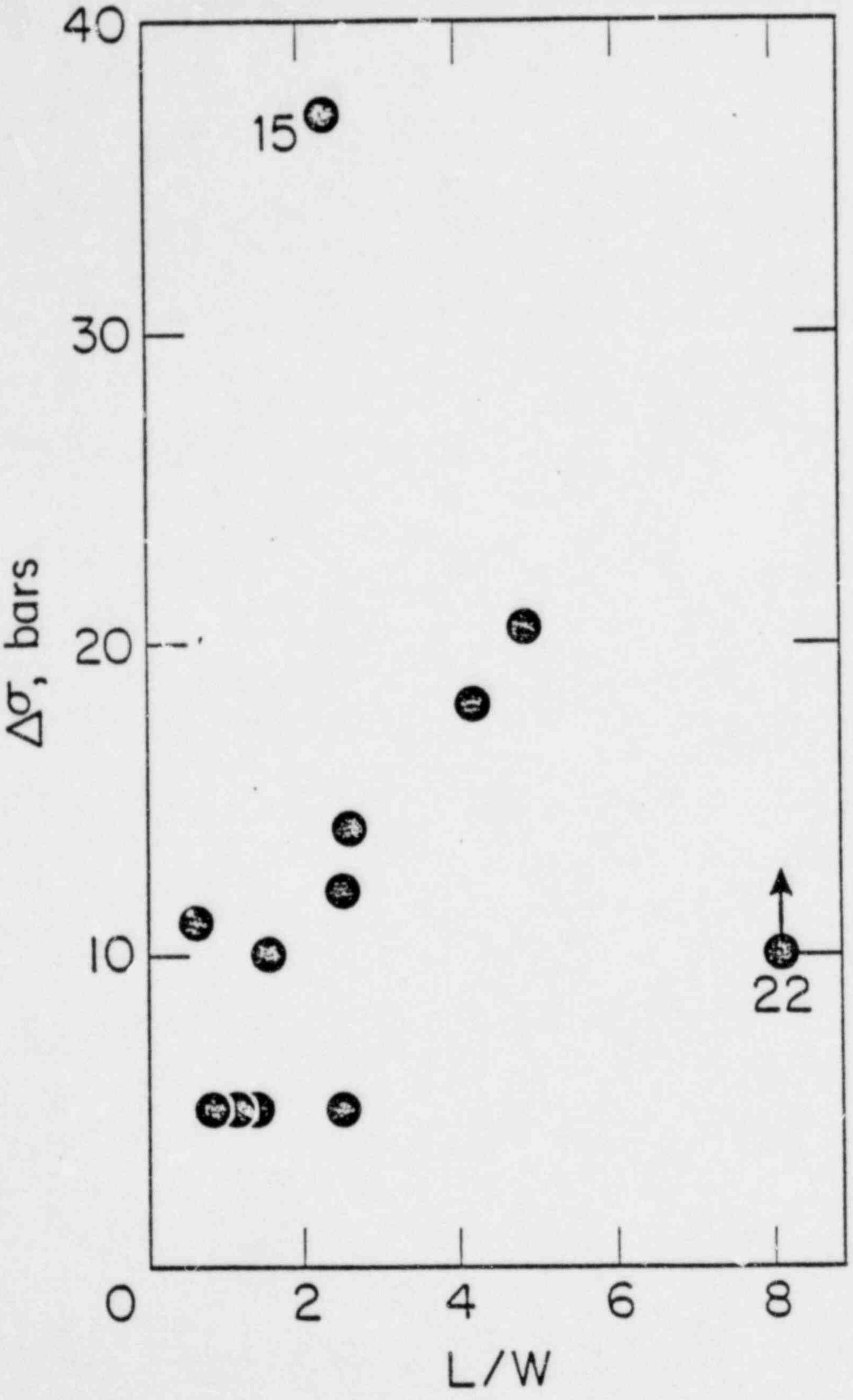


Figure 6

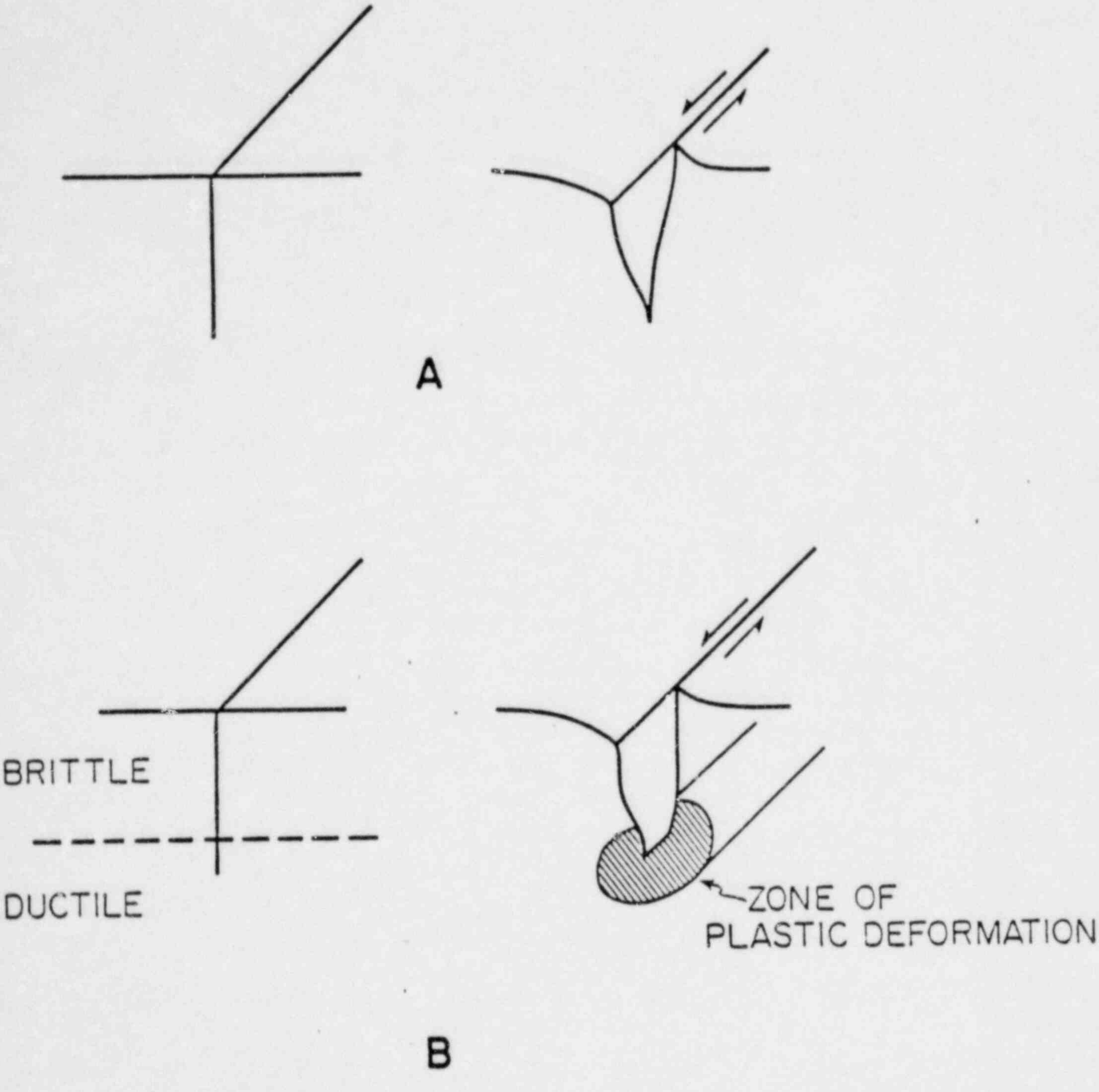


Figure 7

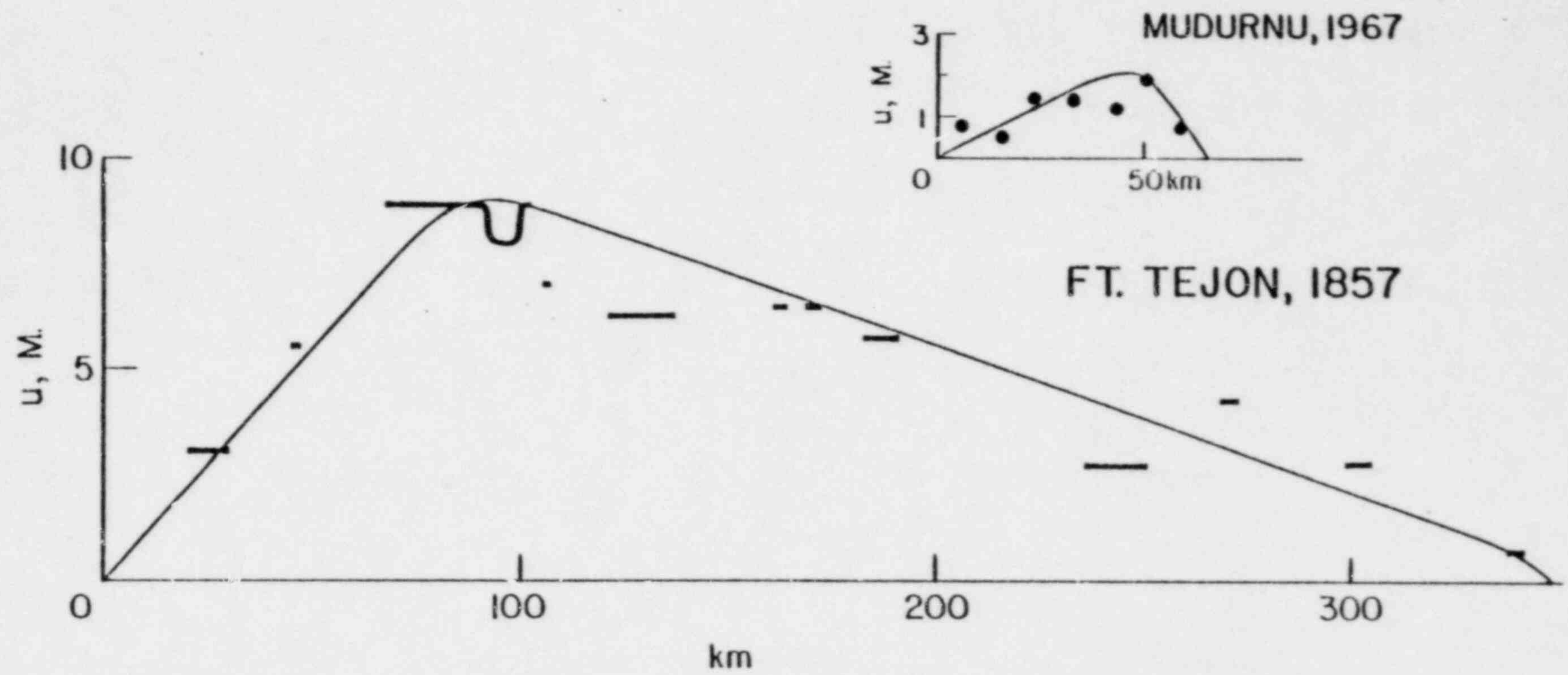


Figure 8

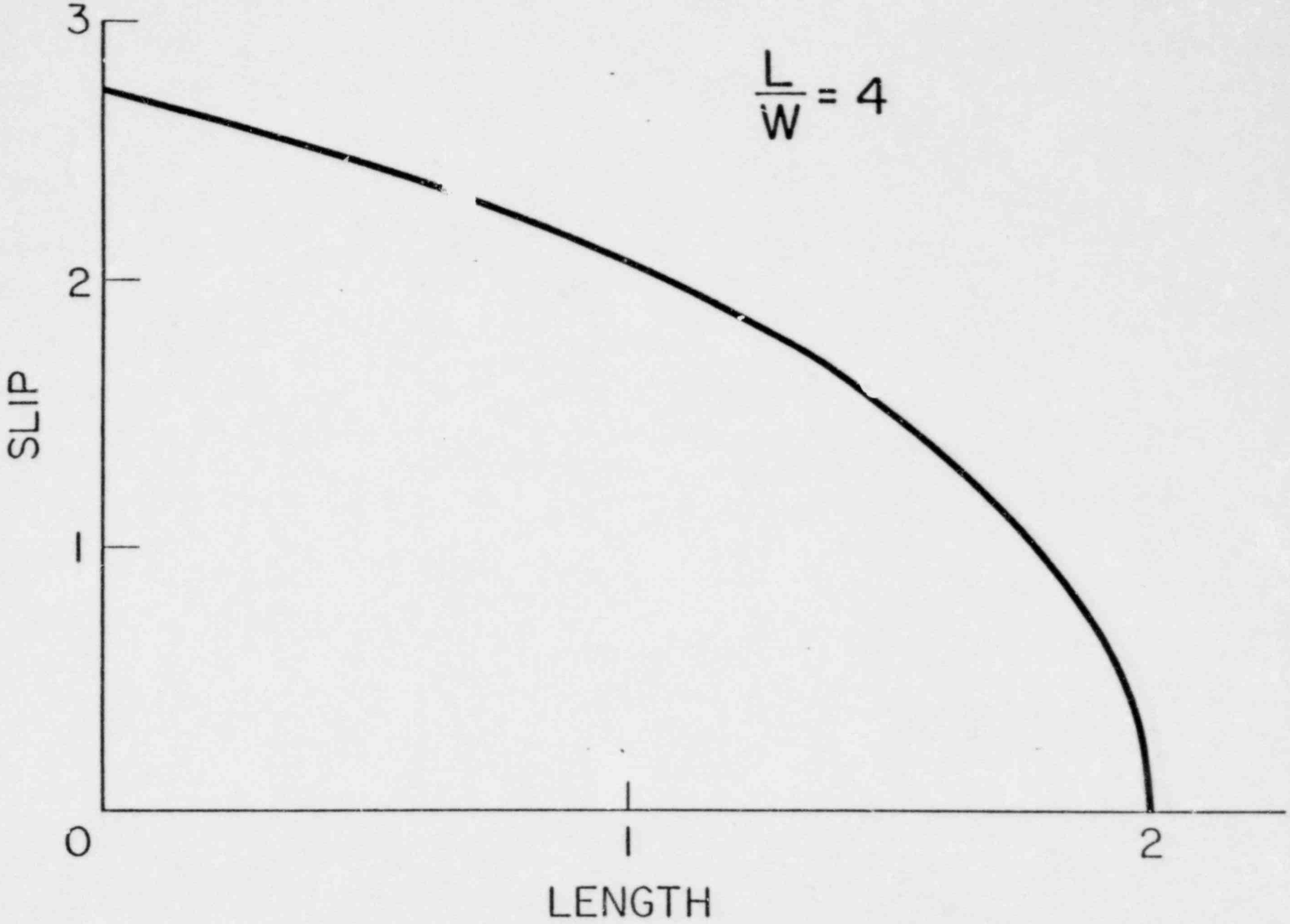


Figure 9

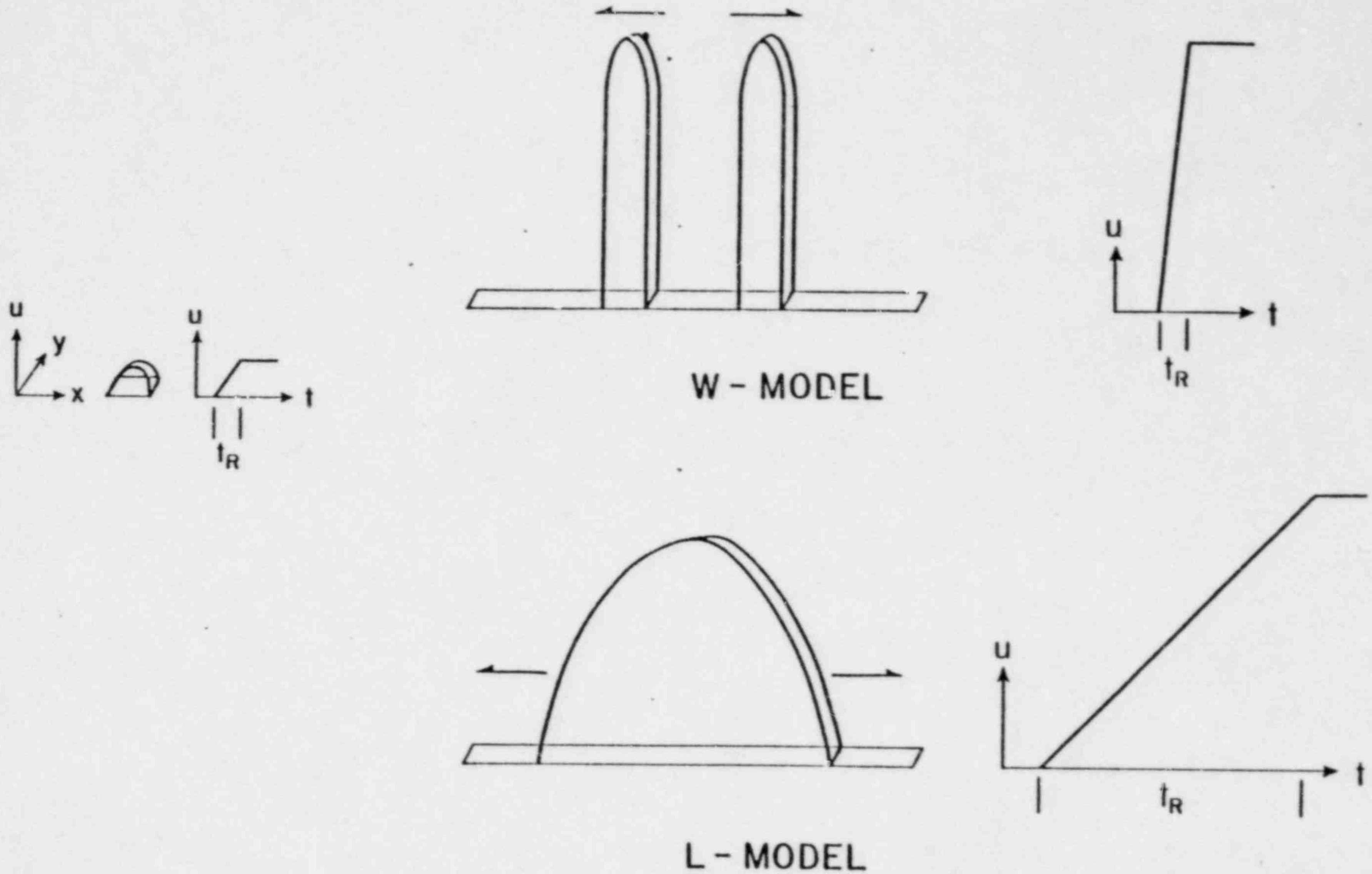


Figure 10

BIOGRAPHICAL SKETCH

(PROVIDE THE FOLLOWING INFORMATION FOR ALL PROFESSIONAL PERSONNEL ENGAGED ON THE PROJECT, BEGINNING WITH THE PRINCIPAL INVESTIGATOR)

NAME	BIRTHDATE (MO. DAY, YR)
JAMES N. BRUNE	November 23, 1934

PLACE OF BIRTH (CITY, STATE, COUNTRY)	PRESENT NATIONALITY (ALIENS INDICATE KIND OF VISA AND EXPIRATION DATE)
Modesto, California	U.S. Citizen

EDUCATION (BEGIN WITH BACCALAUREATE TRAINING AND INCLUDE POSTDOCTORAL)

DEGREE	YEAR CONFERRED	INSTITUTION AND LOCATION
B.Sc.	1956	University of Nevada, Reno, Nevada
Ph.D.	1961	Columbia University, New York City

HONORS AND AWARDS

Fellow, Geological Society of America, 1975.
President, Seismological Society of America, 1970.
Grove Karl Gilbert Award in Seismic Geology, 1967.
Nominated, New York Academy of Science, 1966; Member, 1970.
Fellow, American Geophysical Union, 1967.
First recipient of J.B. Macelwane Award by AGU, 1962

MAJOR RESEARCH INTEREST

Earthquake Source Mechanism
Tectonics
Earth Structure

RESEARCH AND/OR PROFESSIONAL EXPERIENCE (STARTING WITH PRESENT POSITION, LIST PROFESSIONAL BACKGROUND AND EMPLOYMENT)

Professor of Geophysics - University of California, San Diego, 1969 -
Associate Director, Institute of Geophysics and Planetary Physics, University of California, San Diego, 1973-1976.
Chairman, Geological Research Division, Scripps Institution of Oceanography, University of California, San Diego, 1974-1976.
Associate Professor of Geophysics, California Institute of Technology, 1965-1969.
Adjunct Associate Professor of Geology, Columbia University, 1964.
Geophysicist, U.S. Coast and Geodetic Survey, 1964.
Research Scientist, Columbia University, 1958-1963.
Exploration Research, Chevron Oil Company, 1957.
Exploration Geophysics, Chevron Oil Company, 1956.

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