DEPARTMENT OF THE INTERIOR

UNITED STATES GEOLOGICAL SURVEY

# PROCEEDINGS OF THE NATIONAL EARTHQUAKE PREDICTION EVALUATION COUNCIL

JUNE 3 - 4, 1993

HERNDON, VIRGINIA

by

Virgil A. Frizzell, Jr.

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This report is preliminary and has not been edited or reviewed for conformity with U.S.G.S. publication standards.

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## PREFACE

The National Earthquake Prediction Evaluation Council (NEPEC) was established in 1979 pursuant to the Earthquake Hazards Reduction Act of 1977 to advise the Director of the United States Geological Survey (USGS) about issuing any formal predictions or other information pertinent to the potential for the occurrence of a significant earthquake. The Director of the USGS is responsible for deciding whether and/or when to issue predictions or other information pertinent to a prediction.

A prediction is defined as a statement on the time of occurrence, location, and magnitude of a future significant earthquake including an analysis of the uncertainty of those factors. NEPEC advises the Director concerning the completeness and scientific validity of the available data and on related matters. Duties include the evaluation of predictions made by other scientists, from within or outside of government, rather than issuance of predictions based on data gathered by NEPEC itself.

According to its charter, NEPEC, also referred to in this document as the Council, is comprised of a chairman, vice chairman and from 8 to 12 other members appointed by the Director of the USGS. The chairman may not be a USGS employee and at least one-half of the membership must be other than USGS employees.

NEPEC generally functions through the use of working groups organized by the USGS at the request of NEPEC. Working groups often include representatives from private industry, academia, and the USGS. Members of NEPEC who participate in a working group do not vote during NEPEC's evaluation of the results of the working group. After concluding its evaluation, NEPEC presents its recommendations to the Director, who bears ultimate responsibility for a decision concerning issuance of a prediction or other information.

The USGS has published the proceedings of previous NEPEC meetings as open-file reports; these reports, listed and annotated in Frizzell (1993), are available from the USGS Open-File Distribution Center in Denver, Colorado. Phone 1 (800) USA-MAPS for information.

**NATIONAL EARTHQUAKE PREDICTION EVALUATION COUNCIL**
**June, 1993**

**Thomas V. McEvilly**, Chair
University of California

**Robert L. Wesson**, Vice-Chair
USGS, Reston

**Keiiti Aki**
University of Southern California

**William H. Bakun**
USGS, Menlo Park

**James F. Davis**
California Division of Mines and Geology

**James H. Dieterich**
USGS, Menlo Park

**Thomas H. Heaton**
USGS, Pasadena

**Arch C. Johnston**
Memphis State University

**Hiroo Kanamori**
California Institute of Technology

**William H. Prescott**
USGS, Menlo Park

**Kaye M. Shedlock**
USGS, Golden

**Joann M. Stock**
California Institute of Technology

**Ray J. Weldon**
University of Oregon

**Virgil A. Frizzell, Jr.**, Executive Secretary
USGS, Reston

**NATIONAL EARTHQUAKE PREDICTION EVALUATION COUNCIL**
**PROCEEDINGS OF THE MEETING OF JUNE 3 & 4, 1993**
**Herndon, Virginia**

**Council Members Present**

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|---|---|
| Thomas McEvilly, Chair | University of California |
| Robert Wesson, Vice-Chair | USGS, Reston |
| Keiiti Aki | University of Southern California |
| William Bakun | USGS, Menlo Park |
| James Davis | California Division of Mines and Geology |
| James Dieterich | USGS, Menlo Park |
| Thomas Heaton | USGS, Pasadena |
| Arch Johnston | Memphis State University |
| William Prescott | USGS, Menlo Park |
| Kaye Shedlock | USGS, Golden |
| Joann Stock | California Institute of Technology |
| Ray Weldon | Oregon State University |
| Virgil Frizzell, Executive Secretary | USGS, Reston |

**Guests**

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|---|---|
| Brian Atwater | USGS, Seattle |
| Jody Bourgeois | National Science Foundation |
| Carrye Brown | House Subcommittee on Science |
| David Clague | USGS, Hawaii |
| Joyce Costello | USGS, Reston |
| James Devine | USGS, Reston |
| Earl Eldridge | Gannett News Service |
| Brad Hager | Massachusetts Institute of Technology |
| Don Kelly | USGS, Reston |
| Peggy Randalow | USGS, Reston |
| Jim Riehle | USGS, Reston |
| Mark Russo | Federal Emergency Management Agency, FEMA, Washington, DC |
| Bob Shay | FEMA, Washington DC |
| Randy Updike | USGS, Reston |
| Craig Weaver | USGS, Seattle |
| James Whitcomb | National Science Foundation |
| Max Wyss | University of Alaska |
| Art Zeizel | FEMA, Washington, DC |

June 3, 1993
Morning Session

**T.McEVILLY**, Chair, and **R.WESSON**, Vice Chair, of the National Earthquake Prediction Evaluation Council (NEPEC) opened the Council meeting by welcoming Dr. Dallas Peck, Director, U.S. Geological Survey (USGS).

**D.PECK** briefly noted that work undertaken by NEPEC was important and that he appreciated both the advice offered from time to time by the Council and the large amounts of personal time spent on Council activities by NEPEC members.

**T.McEVILLY** asked that members comment on the advanced draft report that resulted in the proceedings volume covering the NEPEC meeting held in 1992 in Portland, Oregon (Frizzell, 1993). He noted that report contained an annotated list of NEPEC proceedings volumes and asked if the next volume might not contain an annotated list of NEPEC "pronouncements."

**T.McEVILLY** asked Council members, participants, and guests to introduce themselves. All members were in attendance except H.Kanamori.

**T.McEVILLY** presented the Agenda (Appendix A) and asked R.Wesson to proceed.

**R.WESSON** preceded an update of USGS issues and the future of NEPEC by noting that his tenure as Chief, USGS Office of Earthquakes, Volcanoes, and Engineering (OEVE), would be coming to an end relatively soon and that the terms of most NEPEC members were about to expire. In order to allow the next Chief, OEVE, full latitude in assisting the Director in the appointment of future members of NEPEC, paperwork was underway to reappoint members for a limited additional period so that NEPEC might be able to respond to an emergency. **R.WESSON** and **D.PECK** discussed issues related to the National Earthquake Hazards Reduction Program (NEHRP) structure, to NEHRP reauthorization, and to NEHRP funding.

On behalf of former Secretary of the Interior, Manuel Lujan, Jr., **D.PECK** presented certificates (Appendix B) to NEPEC Members in recognition of their significant contribution to the Administration and the Department of the Interior.

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**T.McEVILLY** introduced B.Hager and expressed appreciation for the intense and productive review of the Parkfield Prediction Experiment that he and his review committee, called the 1993 NEPEC Working Group on Parkfield, performed.

**B.HAGER** presented an historical perspective (See Appendix C for overheads provided). In about 1983, the USGS was transitioning from work scattered over the State of California to work on an earthquake experiment at a specific locale. Determining the location for a two-color geodemeter was a big question. The choices were either Parkfield or San Juan Bautista. W.Bakun and A.Lindh presented a prediction for an earthquake of 1985$\pm$5 years that was endorsed by NEPEC. When the State of California was notified of the prediction, it provided $1 million for the experiment, which changed it from a strictly geophysical experiment to one having significant public policy aspects.

In 1992, NEPEC chartered a working group to evaluate the experiment. The charge (Appendix D) asked us three specific questions: current assessment, what have we learned, and where should the experiment go in the future?

The report was organized around these questions, and **B.HAGER** briefly reviewed the findings. The working group confirmed that Parkfield still is the most likely locality to trap a moderate sized earthquake; the short average recurrence intervals obviously have some regularity. Loading appears to be unusually simple, somewhat intermediate in scale between the laboratory scale and that common for other earthquakes, in that the section to the northwest is creeping and providing a rather uniform loading rate. Current estimates cluster at about 10 percent per year.

The basis of the prediction, outlined by Bakun and Lindh (1985), involved a characteristic failure stress, a uniform stress drop for each event, and a uniform loading rate. The definitions and assumptions allowed the 1934 event to be excluded from the calculations, yielding a prediction of 1985±5 years.

This series of assumptions are now thought not to be correct, and the main lesson is that the original prediction should have included an assessment of the probability that the model assumptions were correct, as opposed to the statistical uncertainty of a particular model. Nevertheless, Parkfield remains the best place to trap an event.

**B.HAGER** noted that the major scientific results will accrue after the next event. Based upon our collective experience at Parkfield, we can now instrument other localities, such as the Hayward fault, more easily and cheaply. The real success, however, has been the interaction of the scientific community with the response community, which is very happy with the Parkfield experiment. This was a great place to explore such interaction, and the A-level alert that occurred in October, 1992, provided a realistic drill, definitely an added bonus. The plans that had been put in place actually worked. Thus, Parkfield has become a template for application in California and elsewhere.

The working group presented several suggestions for improvement and future success. The experiment should be considered a long-term commitment. The ±5-year uncertainty seemed to focus the experiment on the short term, but it may be necessary to maintain the experiment's equipment for a decade or more to follow the earthquake cycle to the next event and the post-seismic phenomena. A need exists for a long-term project scientist, as well as for a plan to replace and upgrade equipment. A process needs to be implemented to provide peer review in order to keep the experiment current with regards to any changes in the science or public policy. Long-term access needs to be assured. The warning thresholds need to be addressed, as do communications between scientific community and the public with regards both to the precision of the estimates of the probabilities and the fact that the A-level warning saturates at such a low probability.

Significant discussion and questions followed, as did some minor editing of the report.

**D.PECK** asked about the reasons that the event has not occurred. **B.HAGER** indicated that a number of factors seemed to be involved. Essentially, though, we don't know enough to say that the earthquake will happen when we hit the "characteristic" failure stress. Likewise, we don't really know the loading rate.

**T.HEATON** expressed some concern about the state of understanding of physics of the experiment. We can calculate stress drops on the order of 100 to 200 bars for events with magnitudes of 4.5, yet the average stress drop of a Parkfield event, if it repeats every 22 years, must only be about 2 bars.

**W.BAKUN** recalled that P.Segall and R.Harris (1986) calculated that the strain released in 1966 would most likely have been restored between 1984 to 1989, although maybe not until 1995.

**R.WELDON** asserted that what we are really discussing is the failure of the characteristic earthquake model, a model in which we had too much confidence. The experiment has renewed focus on the characteristic earthquake model; more recent work shows that the 1934 and 1966 events are, in fact, not exactly the same. As we learn more about the distribution of rupture on the southern San Andreas and other faults, our appreciation of the variability of rupture length with magnitude will increase. **W.BAKUN** countered that the model is still open to debate and noted that the term means different things to different people.

**B.HAGER** agreed that the term "characteristic earthquake" needs to be used with care, and **J.DIETERICH** expressed his concern that the term may not have much meaning anymore. **J.WHITCOMB** asked how the Coalinga event might affect the characteristic event at Parkfield, and **W.PRESCOTT** asked if the Coalinga event wasn't the predicted event.

In response to a question from the Director, **W.BAKUN** noted that in 1985 the early occurrence of the 1934 earthquake was ascribed to its being triggered by a large number of M4 to M5 earthquakes that occurred in the few days before the 1934 major event.

**T.HEATON** argued that activities since the 1985 prediction indicate that the model is much more complicated than thought at that time. Stress does not appear to be steadily renewed over the region. One of the important lessons of the October, 1992, events is that we must question the assumption that the stress would renew itself; this is important because this assumption is one of the underpinnings of the prediction.

**T.McEVILLY** noted that we are aware, especially at Parkfield, of high stress foreshocks with 2 km source dimensions that are associated with the nucleation process. This understanding allowed the development of the alert levels.

**W.BAKUN** agreed with T.Heaton, however, that we don't understand what is going on. Particularly since P.Segall's calculations indicate that the strain that was released in 1966 has been accumulated. How a M4.5 event that close to the expected hypocenter failed to initiate the next Parkfield event is a puzzle. The past six months have been remarkable for the high level of activity following a very quiet period from 1984 until October of 1992. Activity has been getting closer and closer to the expected hypocenter and increasing the stress in that area.

**B.HAGER** noted that the discussion in his working group's report emphasized the long-term need to continue the measurements to enable us to more fully describe what happened leading up to the next event. This would enable us to replace the debates with observations.

**B.HAGER** led a discussion of the recommendations presented in the draft report (Appendix E) and fielded comments and editorial suggestions from NEPEC Members concerning the report itself. All agreed to maintain a commitment to the experiment, to attempt to protect its funding, and to allow the USGS management team flexibility in how the experiment is integrated with the balance of program activities. NEPEC discussed the panel's concerns about the long-term commitment and the need to develop a fiscally reasonable replacement mechanism for monitoring equipment, as well as how to keep the experiment active and current. A couple members suggested that the USGS has the responsibility of converting the recommendations in the report into policy. The topic of publication of the report was discussed, and **J.DAVIS** argued for peer review before publication. The **DIRECTOR** envisioned a scenario in which the ad hoc working group presents its report to NEPEC, which would revise it, as needed, and present it to the Director for his consideration. **R.WESSON** indicated that the USGS welcomes the advise from the NEPEC and hopes for a wide distribution of the results of the panel deliberations. All agreed that the comments and suggestions of the day and any written comments that reach B.Hager within the week would be considered during the preparation of the "final" version of the report by the 1993 NEPEC Working Group on Parkfield. The report would then be sent to Chairman McEvilly to be circulated, with a draft letter of transmittal, by mail to NEPEC Members in anticipation of transmission to the Director. Upon acceptance by the Director, appropriate release methods would be undertaken to assure the widest distribution possible.

On behalf of the Council, **T.McEVILLY** thanked B.Hager and his committee for hard work, intense effort, and a really nice job.

June 3, 1993
Afternoon session

**K.AKI** distributed a preliminary draft "executive summary" (Appendix F) of the Landers Phase II report on future seismic hazards in Southern California.

**R.WESSON** presented a brief history of the post-earthquake activities that followed the Landers earthquake. In 1988, NEPEC charged the original working group on probabilities of earthquakes in California along the whole San Andreas fault system. Since the 1988 report (Working Group on California Probabilities, 1988) much work has been undertaken, much information has become available concerning recurrence intervals and many methodological issues have been addressed. After the 1989 Loma Prieta earthquake, NEPEC convened another working group to update the probabilities for the San Francisco bay area (Working Group on California Probabilities, 1990).

Shortly thereafter, at its meeting in Alta, Utah, in 1991 (Frizzell, 1992), NEPEC determined that an update in southern California seemed warranted. Because the Southern California Earthquake Center (SCEC) was initiating its activities, it clearly seemed important that SCEC play a role in whatever new estimates of probabilities would be developed. Then, in 1992, the Landers earthquake, the biggest event in California since 1952, occurred. In part because of Landers and its aftershocks and in part because of comments by some seismologists about the likelihood of additional big earthquakes, the populace was extremely concerned. At that time, NEPEC joined with the California Earthquake Prediction Evaluation Council (CEPEC) and SCEC to use a combined approach to address Landers and the broader probabilities in Southern California.

An ad hoc working group, principally under K.Aki's leadership, with representatives from NEPEC, CEPEC, SCEC, and the California Office of Emergency Services, was convened to address the situation. The Landers Phase I report (Ad Hoc Working Group, 1992) addressed the change in probability for earthquakes in Southern California as a consequence of the Landers event. Targeted for September 1, 1993, the Phase II investigation involves addressing the whole question of probabilities in southern California. K.Aki was asked to present an update of the Phase II process.

**K.AKI** indicated that Phase II activities started immediately after the Phase I report was released at the end of November, 1992. At the first meeting in late January, 1993, the outline of the chapters was determined, and groups were established to write the individual chapters.

An introductory chapter on the tectonic perspective addresses the tectonic regimen in southern California. A chapter on the geological data, which will contain a consensus concerning the parameters of the faults in southern California, started with a 1986 compilation that S.Wesnousky has continually updated. A draft of a table that will be contained therein (Appendix F, Table 1) contains the current estimates of fault parameters. A third group will use these data to derive probability estimates for fault failure using a variety of models. And, because we are concerned with the expected ground motion in parts of the basin at some distances from the faults, we will go somewhat beyond the scope of the 1988 working group report to produce a probabilistic seismic hazard map using every kind of information relevant to earthquake studies. Because some workers are very conservative, indicating that it is premature to produce such a hazard map, and because others have new ideas and want to apply them immediately, the process is very interesting.

The 1988 report provides the starting point for our efforts. One widely quoted conclusion from that report is that "a magnitude 7.5 or greater earthquake has a 60 percent probability of occurring somewhere on the southern San Andreas fault within the next 30 years." We address this number to determine if it is valid in terms of our understanding today.

A workshop will be held on June 8, 1993, at the University of Southern California, to address the summary that **K.AKI** present. The summary was written by D.Jackson of the University of California at Los Angeles and **K.AKI** with the intention of addressing the individuals working on the various aspects of the report and aggressively keeping everything, accompanied by the realization that many parts will be deleted during the review process. Hopefully, we will finish the first draft of the body of the report by the end of June. **K.AKI** expressed his appreciation for the opportunity to present the draft summary to NEPEC and noted that J.Dieterich has been very helpful in the exercise and asked him if he had any comments.

**J.DIETERICH** indicated that he had been involved in the computation of the segment probabilities which involves several more or less independent approaches and that he probably represents an "institutional memory" in the process. Using geologic data on fault offsets, recurrence intervals, and most recent date of movement from D.Schwartz and coworkers, **J.DIETERICH** has collaborated with J. Savage in computation of probabilities using the general procedures established by prior working groups (Appendix F, Table 2). A major issue and source of discussion concerns the intrinsic variability of earthquake recurrence on these characteristic fault segments. In prior working groups, we used a value of 0.21. We seem to have reached a consensus that this value is too low, but little agreement exists, in fact very little data can be applied, concerning an appropriate value. We simply agreed that a value of 0.5±0.2 would be used. J.Savage is addressing these calculations. D.Jackson is independently addressing Poissonian probabilities, assuming that there is no time-dependent change in probability. That is where we are today; we have not reached any consensus on any single method, but, **J.DIETERICH** indicated a desire to employ different methods to at least find consensus within each of the methods.

**K.AKI** indicated that a consensus exists (Appendix F, Table 1) among the geologists concerning the segments of the San Andreas and San Jacinto faults addressed by the 1988 working group, as well as five segments of the Elsinore fault not addressed by that group. **R.WELDON** noted that similar information has been compiled for other faults in the region and that a decision might be made concerning the use of such information, even if we don't know as much about these faults as the three addressed in this table. For instance, we may only have information concerning long-term slip and possible segmentation of some faults. Several schemes exist that would incorporate the probabilities for these other faults during the production of the ground motion map.

**T.HEATON** pointed out one of the advantages of using whatever information we have about these other faults in the production of the ground motion maps. Emphasizing ground motion probabilities and de-emphasizing specific earthquakes flags the possibility of damaging ground motion next to the faults that move regardless of the detail of how the faults break. In fact, three main techniques are being considered.

One approach involves conditional probabilities for repeating earthquakes and provides a long-term geologic scale. Another, which D.Jackson has been advocating, assumes that to understand the probability of events in the next five to ten years, one should address the past five to ten years; activity will occur where faults have been active. This is based strictly on the catalog. A third uses strain release rates, an assumed maximum sized earthquake, and some distribution to determine seismicity rate by region. All three techniques have been used and the working group is attempting to integrate the results.

The standard errors for each measurement can give some estimate of the reliability of the recurrence interval. The first working group (1988) used the value 0.21 as the standard error of the logarithm of recurrence interval, but the group involved in the current evaluation of the probabilities in southern California thinks that this should be more like 0.5. Areas where we have the most complete data yield high values. For instance, Pallett Creek data yield a value of 0.66. As the value approaches one, the results approach the Poissonian model, in which the time of the last event has no effect.

**K.AKI** discussed the preliminary probabilities for 5-, 30-, and 50-year periods using prior working group methods (Appendix F, Table 1) and correlations of variation with both parametric error and intrinsic errors. The 50-year period was used because it is cited in the Uniform Building Codes and the 30-year period was used so that we might compare our results with those from the work done in 1988.

The 30-year probabilities for both studies are more or less similar, but several segments have changed significantly. Probabilities for the Coachella segment of the San Andreas fault and the Anza segment of the San Jacinto fault are reduced by a factor of two because of the change in the coefficient of reliability from 0.2 to 0.5; these two segments happen to have events in the distant past. If the most recent events occur in the distant past, the probability is high if sigma is small; as sigma has increased, the probabilities have been reduced. On the other hand, the San Jacinto Valley segment increased significantly. The reason is two-fold: the estimate of the slip for the event is reduced by a factor of two, increasing the recurrence interval by a factor of two, and the most recent event is in the recent past. The overall cumulative probability for the San Andreas and San Jacinto faults has not changed much, being 60 percent before and 61 percent now.

**R.WELDON** pointed out that the 1988 probability was derived using a concept that the values for the segments could be combined as independent statistical elements, whereas we certainly know this to be false. We now know that, in most cases, segments move in concert. The big question remains about how, or if, the numbers should be combined. **J.DIETERICH** noted that the issue has been discussed, but not decided. Currently they are being treated as independent.

**K.AKI** noted that the increase in the coefficient of variation results in the time dependent probabilities very similar to the Poissonian probabilities. We can therefore extend this analysis to other faults for which we only have recurrence intervals and no other information. We are anticipating that the geologists will give us details on more faults on which we might apply the Poissonian evaluation.

**W.BAKUN** expressed concern that the previously used standard error of the logarithm of recurrence interval (0.21) does not fall within the error bars of the value used by the current working group (0.5$\pm$0.2).

**J.DIETERICH** noted that, while the question certainly was "fair," the answer quickly sinks to the arcane depths of the subject.

In response to a reasonable prod from the **Director**, a brief explanation of this important issue was presented by J.Dieterich, T.Heaton, and R.Wesson. Given a slip v. time plot of an imaginary sequence of events, if one had perfect observations, one would know the exact times of the events, and the variability in the observed intervals between earthquakes would be the intrinsic variability.

The measure of *intrinsic* uncertainty (variability) is the standard deviation of the recurrence times. The *parametric* uncertainty, essentially the error in measurement, represents all observational uncertainties in estimating dates, fault slip rates, and earthquake slip that yield recurrence intervals. The *net* uncertainty used in estimating conditional probabilities includes a combination of the parametric and intrinsic uncertainties. The correct model for combining these uncertainties is the subject of some debate.

All workers must admit to some element of interpretation. If one picks a small value for the intrinsic variability, one accepts or supports the characteristic earthquake hypothesis. If one picks a value of 1.0, one completely rejects the characteristic earthquake hypothesis. **R.WELDON** pointed out that the 0.5 value was used, in part, because we have two data sets from the southern San Andreas that yield this value and in part because the method used to determine the 0.21 value was partly incorrect.

**K.AKI** presented a map (Appendix F, Figure 1) depicting 58 seismic source zones that the working group developed to integrate the geologic, geodetic, and earthquake catalog data. In their analysis the working group would treat "characteristic" earthquakes on known segments as "line sources," while "non-characteristic" events derived from filtered catalogs (Appendix F, Table

3a-c) would be randomly distributed within the 58 seismic source zones.

No little amount of discussion followed the introduction of these tables and the smoothed maps depicting the probabilities (Appendix F, Figures 2, 2', 3, 4). The Council expressed some initial doubt about the probabilities for the Parkfield segment, for instance. **K.AKI** noted that this was an artifact of the relatively small area within the Parkfield seismic source zone, and **T.HEATON** suggested that scaling the zones by area might eliminate this problem.

Deformation measurements derived from GPS investigations over the past four years present a rather uniform distribution of strain (Appendix F, Figure 5). Note that the Los Angeles basin has very large strain, comparable even to the San Andreas fault, and that the Rose Canyon area shows significant strain accumulation.

**T.HEATON** concurred, noting that one of the key features is the closing of the Los Angeles and Ventura basins at rates of about 1 cm/yr, rates equal to the San Andreas. **K.AKI** explained that S.Ward used an unknown algorithm to partition the strain among the seismic source zones (Appendix F, Figure 6).

**B.HAGER** expressed concern that some of the polygons depicted on the maps do not correspond with observations that he has made in southern California. The Ventura basin, for instance, has 50 percent higher strain rate than the San Andreas fault, and the region to the south of that has essentially no strain. Some problem exists with the observations.

**W.PRESCOTT** pointed out that the interpretation would benefit from use of some 20 years of data that exist for several of the boxes. The argument would be much more robust with the longer record.

**K.AKI** next presented a table and map (Appendix F, Table 3d and Figure 7) produced by S.Ward showing the recurrence intervals and yearly frequency of earthquakes, respectively, for the seismic source zones derived by integrating the randomly distributed seismicity and the partitioned strain for the region.

**K.AKI** next showed, with some explanation concerning methodology, the tentative results of integration of the source characterizations derived from paleoseismology, earthquake catalog, and strain accumulation (Appendix F, Figures 8 - 10, respectively), using a model to calculate ground motion and thence exceedence probabilities for certain accelerations. The exceedence probabilities derived using the Ellsworth catalog (Appendix F, Figure 9) could be considered a lower bound and those integrating the strain rate data (Appendix F, Figure 10) could be considered an upper bound.

**T.HEATON** started some discussion by pointing out that the highest probabilities derived using the later model do not fall on the San Andreas fault, but on the Los Angeles basin. These results are influenced by the geometry of the polygons, the relationships at the edges of the polygons, and strain relationships within a polygon, as well as from assumptions concerning the likelihood of really large events.

**K.AKI** concluded his presentation by soliciting a rapid return of any comments on the draft presented by him.

**R.WESSON** suggested that he understood at the outset of this presentation that everything that has gone on during investigations and discussions by the ad hoc working group had been included in the draft summary document, and that much would be eliminated. He then opined that the first part, the extension and review of the 1988 report, likely would elicit few methodological objections, though some discussion concerning details of new information undoubtedly would occur. And although the method of translating this into the ground motion, using attenuation relationships, for instance, was not presented, this also likely would elicit little objection. With that said, however, **R.WESSON** suggested that the rest of the material presented, perhaps with the exception of the geodetic work and the ground motion prediction from that, seems to be on the outer limits and not really ready to be presented to the general public as a basis for estimating probabilities.

T.Heaton, J.Dieterich, and K.Aki participated in a short discussion concerning the catalogs. **T.HEATON** suggested that the catalogs were used for the preparation of the Phase I report, and that Kagan and Jackson were addressing the short-term probability, not the 50-, to 30-year probability, while **J.DIETERICH** said that he understood they were looking at 50 years. **K.AKI** said that they would use the Ellsworth catalog for 50 years, the Caltech catalog for 30 years, and the Harvard catalog for 5 years.

**R.WESSON** asked how the Harvard catalog could help determine the probability of M8 events, and **J.DIETERICH** did his best to explain. The Ellsworth catalog, with 150 years of record, seems appropriate for use in the Poissonian modeling. Because the Harvard catalog is a short-term catalog, it may be the best one to use for a short-term prediction. Caltech is intermediate between the catalogs and most appropriate for the intermediate-term projection. **J.DIETERICH**'s challenge, given the long catalog, is how many of the big earthquakes would show up using the method that Kagan and Jackson would like to apply?

**K.AKI** asserted that using a fault-specific model, after working group 1988, for a five-year prediction, one would fail for all the events since 1857 because they have not been on the San Andreas. Using the Harvard catalog, one can make predictions for the next five years. **T.HEATON** said that, while the gap model must work in the long run, in the short run one should use this cluster model to determine where the next event will occur. Kagan and Jackson have some good ideas how catalogs behave in time and space, we just need to figure out how to integrate the concepts with the longer geologic record.

**R.WELDON** commented that while they do have some success, on the order of 5 to 10 percent, not all events have precursory activity, and these events will not be predicted by the short catalog.

**T.HEATON** stated that is why the next report will have to blend the different techniques, because the technique that addresses geologic data says nothing about the recent catalog except for the conditional probability, when in fact clustering is important, and the clustering model says nothing about the geologic rates. He went on to discuss methodology of review. The Phase I report was subjected to five mail reviews. Will NEPEC want to undertake a similar elaborate review process with the Phase II report? If so, isn't now the time to plan for that process?

**R.WESSON** reminded all that the Phase I was designed to be very timely and respond to a perceived crisis situation. We all intend that the Phase II have some significant lasting value similar to the 1988 report. Perhaps the tables are the source of concern about the models involving the catalogs. K.Aki and T.Heaton agreed that the tables may be misleading and that the smoothing algorithms and the narrow polygons don't intuitively lead to the probabilities.

**R.WELDON** expressed concern about "signing off" on any of the methodologies, be they from S.Ward or Kagan and Jackson, without seeing the details. If we need to reach a consensus by September 1, 1993, a significant amount of work remains, including a lot of education concerning the various methods. One way to obtain the consensus would be to cut some of these items out, and another would be to extend the educational period somewhat.

**T.McEVILLY** noted that the comprehensive nature of the report had been a concern in January when its outline was presented to himself and J.Davis. At that time these two, Chairs of NEPEC and CEPEC, attempted to restrain the working group from taking such bold steps into the frontier of ground motion prediction based on conceptual ideas that are really just forming. The group proceeded anyway and now we wonder if we have a "Landers Phase II" or if we have a "Master Model I."

**W.PRESCOTT** expressed a counter view that the techniques that we now consider as fairly accepted and receiving wide consensus probably were not as widely accepted when they were first were used. As a community, we do want to keep trying to push the envelope.

**R.WESSON** concurred that we do want to push the envelope and added that we do not want to discourage the activities of the working group and SCEC. This is not a problem with science, but perhaps NEPEC isn't familiar with the techniques and needs to spend some time, perhaps a workshop, with the working group to gain the familiarity needed to meaningfully address the issues.

**J.DIETERICH** argued against a mail review, characterizing that process as ineffective. The final Phase I product was good because individuals contributed heroic effort. These open discussions are the best method to determine what really counts and what doesn't. Furthermore, the 1988 methods are well documented and understood, but no documentation yet exists for these newer methods.

**J.DAVIS** agreed that the review process will take some time because the group was not building directly on previous experience. We also must consider how we expect these documents to be used. As well as representing a consensus of the scientific community, what are we trying to communicate? This was commissioned as a report to contribute to public policy, as were the '88 and '90 reports. **J.DAVIS** lauded the effort and the variety of methodologies used as a valuable contribution, but he noted that analyses that end up with disparate conclusions will not serve the purposes needed. We also need to consider that when we convert these conclusions to probabilistic ground motions, we use dimensions used in the engineering community for engineering design. We should try to obtain the consensus of the engineering community for this to be an advisory document, and this would extend the review period even longer. In summary, we need to design a review process that converges on consistent conclusions, and this may characterize the type of conclusions that we try to reach in the document. **T.McEVILLY** agreed and pointed out that the range of conclusions were outlined in the preface to the Landers Phase I report.

**W.PRESCOTT** argued that NEPEC needs a more detailed description of the methods and a fair amount of time to do a good job reviewing the current state of the draft.

After some discussion, **K.AKI** agreed to send drafts of several articles "in press" and early drafts of the various chapters that result from working group meetings in June to attendees who might participate in a later workshop that would address the methodologies being evaluated.

**T.McEVILLY** asked the working group about the impact of the 20 percent gravity probability maps. These are important maps. How are they going to be presented to the engineering and emergency services communities in the State?

**K.AKI** answered that these first maps would not be considered final values in quantitative manner, but more an emphasis of relative importance. Thus, we would not include site effects at all.

**R.WESSON** expressed interest in this fact, noting that if peak acceleration were used all the way through the analysis, the group could still, on the map, use a different name, such as "relative hazard index," and the report wouldn't tend to create uncertainty with the engineering community, because it isn't characterized as being intended to be used for design. But it still would show the relative hazard across the region for public education and planning purposes.

The Council debated the efficacy of using a map of any sort. Some were concerned that even a map depicting relative hazard would be "misused" as the uninitiated attempt to find exactly which street corner the contour touches. Much of the collective experience with maps created for public consumption has been with maps that can be intuitively associated with a source. Such situations may be more recognizable to the users than a probabilistic map based upon multiple sources and second and third derivation analysis. An underlying discomfort with what has been presented today may derive, in part, from not fully understanding the analytical process, but **J.DAVIS** noted that NEPEC has not been fully appraised with the context of "here is why we have done it" and this is what it means to these specific user communities.

For instance, the California scenario maps, whose narrow purpose is providing a context for prioritizing the use of scarce resources in the 72-hour period after a major event, do not seem to have been misused, either in engineering practice or in the drawing priorities by local governments emergency response. What is practical, what is feasible, and what need of what readers does the Phase II report seek to fill? We shouldn't simply state the problem, but we should inform our intended audience how to deal with the problem. We commissioned a document to provide a background for public understanding of the seismic threat in Southern California and for some public policy statements and planning.

**R.WESSON** noted that a widely distributed newspaper insert was the document that outlined an appropriate public response to the 1990 report following the Loma Prieta event. If we recognize that the structural engineers, for the most part, already know about earthquake hazard in California, and that the audience that we are trying to reach is the "public," one could argue that, using whatever units we may to quantify the hazard, we could define what the hazard is in southern California relative to the rest of the country, delineate the geographic distribution of that hazard within southern California, and then determine some sort of newspaper-insert-like strategy. The map in the middle was one of the more effective parts of the San Francisco Bay Region insert.

This map showed geologic units and relative shaking that might be associated with the units. In Southern California we are addressing a much larger area, and we may not have the detail needed. But perhaps we can proceed from the probabilities and the faults that we are aware of to the use of a rock and/or soil motion index, showing how this varies across the region. The idea being to educate the public rather than impact the engineering community.

**K.AKI** pointed out that the site effects in the newspaper insert derived solely from geology, suggesting a correlation between geology and intensity developed by R.Borchard. Actually, with more quantitative data, we find no significant correlation between ground acceleration and site condition. This is not a simple issue if we go to peak ground acceleration. If we want to use intensity, we can always use the J.Evernden model, but it seems about time to go to a more quantitative approach.

**T.HEATON** noted an added benefit of the direction that SCEC wants to take the master model is that the input parameters can be modified depending upon what output one wants. If we address unreinforced masonry we would use different response spectral periods and durations than we would for a structural steel building. What should the first cut at this be? Peak acceleration is an easy thing to talk about and it gets us away from the site effects issues, but the down side is that the engineers might take umbrage. But we should be able to craft a suitable disclaimer to address the issue of suitable uses for the map. In fact, the number g in the building code has very little to do with the level of acceleration.

**K.AKI** mentioned that A.Cornell is working on this aspect of the report because SCEC wants to use the analysis for sensitivity study, to compare different source characterizations.

**W.BAKUN** interjected that a map such as this won't be used by the engineers, anyway. In the method just explained to him, the engineers will see how the map impacts the building code and then design a new code. They will intuit the R factor to whatever they need in order to get the acceleration down to where they feel comfortable to create safe buildings. So we are really preparing the report for the planning community and the general public.

In response to a question by T.McEvilly concerning the document's relation to Landers, **K.AKI** acknowledged that the relation was tenuous. Landers is just another event in the catalog. **T.McEVILLY** suggested that this really isn't a followon to the Landers Phase I., and that perhaps the title, "Landers Phase II," isn't correct.

**R.WESSON** summarized these several issues as two questions. What form should the final product take? What process do we need to undertake to become comfortable with the technical aspects? **J.WHITCOMB** asked, again, "what audience are we aiming at?," and **J.DAVIS** advocated for consistency between the conclusions drawn by one analytical method and those drawn by another. The question remains how to develop a consensus about the map and conclusions and then how to present the result in some finite period of time. A trade off exists between the level of consensus we want to attain and the length of time we might allow for the process. The consensus that we might attain by the fall will be considerably different from that we will attain over the next five or ten years as the master model becomes more fully developed. The group agreed to attend a workshop in August to learn about the process and then to reevaluate the schedule for publication of the Phase II document.

June 3, 1993
Evening session

**J.EBEL** presented a brief introduction to the seismic activity in the northeast and then described recent accomplishments of a group of investigators interested in this seismogenic region (see Appendix G for overhead illustrations provided).

An earthquake data sheet (Figure 1) compiled by the New England States Earthquake Consortium (NESEC) presents some perspective on activity in the northeast. About 1200 earthquakes are known for New England through 1989 from a catalog of about 4500 events for the entire northeast.

Some of the larger events in the century in the New England region include a M5.5 and a M6.6 in 1924 and 1925 in Quebec. In response to a question, **J.EBEL** indicated that most magnitudes are instrumental. Some from the Canadian side may be determined by other means. **A.JOHNSTON** noted the absence of the Grand Banks events, which include a M7.2 earthquake.

The seismic network, at its greatest extent in the mid-1980's (Figure 2), included about 90 to 100 stations run by the Weston Observatory, MIT, the Lamont-Doherty Earth Observatory, Woodward-Clyde, and Penn State. It is now down to one-half to two-thirds the number of stations, say 50 to 60, with Woodward-Clyde gone, Lamont having shut down the western New York stations, and Weston having reduced the number of stations in New England. This averages about a 50 km station spacing.

NESEC

**New England Earthquake Historical Data**

**New England States' Historical Earthquake Record to 1989**

| State | Years of Record | Number of Earthquakes |
|---|---|---|
| Connecticut | 1568-1989 | 137 |
| Maine | 1766-1989 | 391 |
| Massachusetts | 1627-1989 | 316 |
| New Hampshire | 1728-1989 | 270 |
| Rhode Island | 1766-1989 | 32 |
| Vermont | 1843-1989 | 60 |
| Total Number of Earthquakes within New England | | 1215 |

Total Number of Earthquakes Within Northeastern North America, 1538 thru 1989 = 4498

**New England Earthquakes with Magnitudes of 4.5 or Greater, 1924 - 1989**

| New England Location | Date | Magnitude |
|---|---|---|
| Ossipee, NH | Dec. 20, 1940 | 5.5 |
| Ossipee, NH | Dec. 24, 1940 | 5.5 |
| Dover-Foxcroft, ME | Dec. 28, 1947 | 4.5 |
| Kingstown, RI | Jun. 10, 1951 | 4.6 |
| Portland, ME | Apr. 26, 1957 | 4.7 |
| NH-Quebec Border | Jun. 14, 1973 | 4.8 |
| Gaza, NH | Jan. 18, 1982 | 4.5 |

**Northeastern Earthquakes with Magnitudes of 5.0 or Greater, 1924 - 1989**

| Northeast Location | Date | Magnitude |
|---|---|---|
| La Malbaie, Quebec | Sep. 30, 1924 | 5.5 |
| La Malbaie, Quebec | Mar. 1, 1925 | 6.6 |
| Lake George, NY | Apr. 20, 1931 | 5.0 |
| La Malbaie, Quebec | Jan. 8, 1931 | 5.4 |
| La Malbaie, Quebec | Oct. 19, 1939 | 5.8 |
| La Malbaie, Quebec | Oct. 27, 1939 | 5.2 |
| Miramichi, New Brunswick | Jan. 9, 1981 | 5.8 |
| Miramichi, New Brunswick | Jan. 9, 1981 | 5.1 |
| Miramichi, New Brunswick | Jan. 11, 1981 | 5.5 |
| Goodnow, NY | Oct. 7, 1983 | 5.1 |
| Saguenay, Quebec | Nov. 25, 1988 | 5.9 |

Figure 1. Sheet presenting historical data on earthquakes in New England prepared by New England States Earthquake Consortium. Data compiled from various sources.

The Weston Observatory historic catalog contains events from 1534 to 1974, when the network was initiated. This included the Cape Ann activity, activity around New York City, the western Quebec zone, and a scatter of other events. This historic catalog is complete in the M4 to M5 range. In the early 1980's, the catalog was probably complete to around M2. With the presently reduced station density and with digital trigger systems, the catalog is only complete to the M2.5 to M2.7 range in New England.

Our northeastern network report for the period 1975 to 1986 shows several regions of activity (Figure 3). For instance the Charlotte fault zone, the western Quebec zone , and the Adirondacks. These appear to be

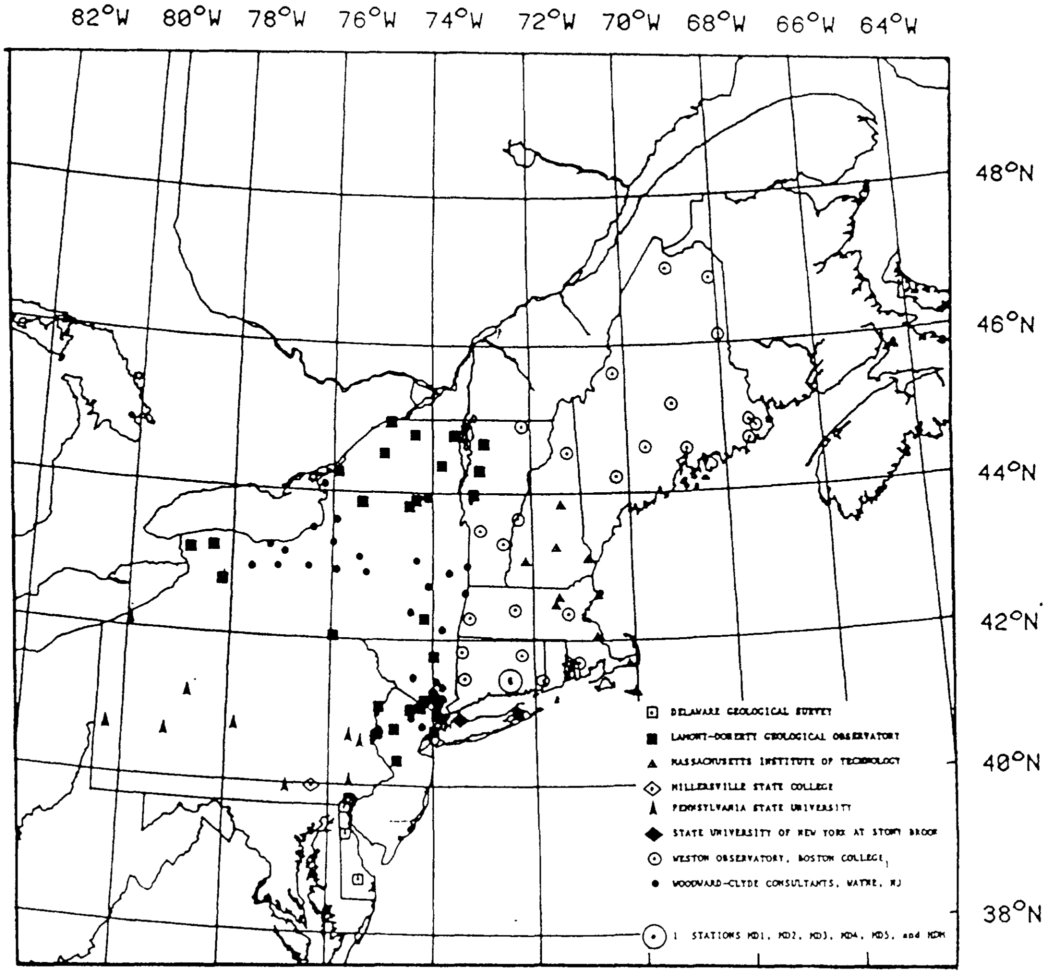


Figure 2. Map of seismic stations in the northeastern United States in September, 1985. Dotted squares, Delaware Geological Survey; filled squares, Lamont-Doherty Geological Observatory; filled triangles, Massachusetts Institute of Technology; dotted diamonds, Millersville State College; arrowheads, Pennsylvania State University; filled diamonds, State University of New York at Stony Brook; dotted circles, Weston Observatory; and filled circles, Woodward-Clyde.

somewhat more active than in the historic catalog because we did not have folks living in many of these areas at the beginning of the historic record. The modern instrumental zones of activity reproduce the historic seismicity pretty well. Thus, activity seems fairly consistent with time.

Cape Ann has not been active since the instrumental record was started, but there has been some activity in the northwestern suburbs of Boston, including a felt earthquake. There, one of the local names (Nashoba) derives from a indigenous word meaning "hill that shakes."

NESEC's activities are a direct response to the Federal Emergency Management Agency's (FEMA) effort to increase the awareness of earthquake hazards in the Northeast. NESEC is chartered by the six civil defense directors from the New England states. Dr. Louis Klotz, an engineer, is the director, and FEMA funds earthquake coordinators in three of the New England states: New Hampshire, Vermont, and Massachusetts.

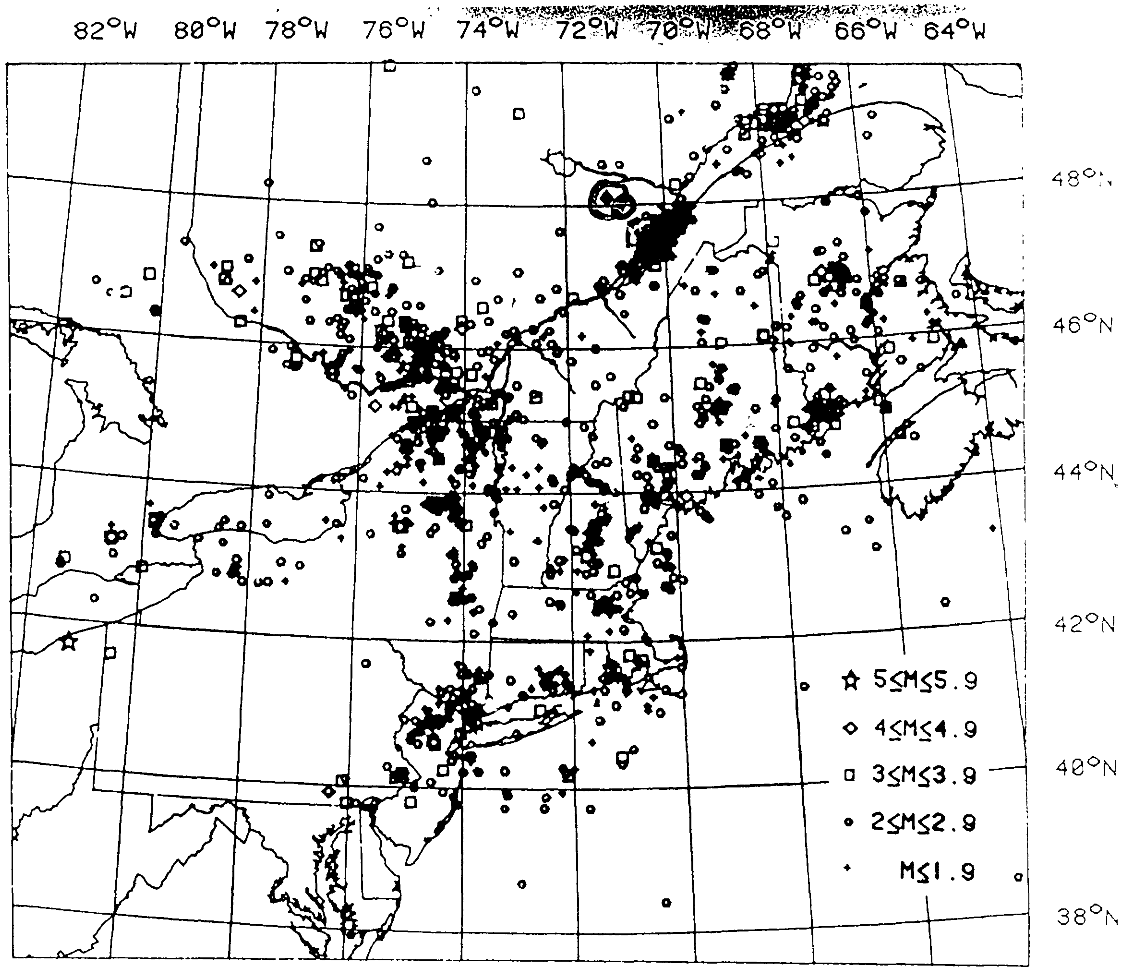


Figure 3. Earthquake epicenters in the northeastern United States for the period October, 1975, to December, 1986.

**J.EBEL** said that in an attempt to increase interest, understanding, and support in the region, NESEC's director drew on parts of an article (Ebel, 1984), in which he had looked at the instrumental catalog from 1975 to 1982 and generated some simple statistics. A recurrence curve or Gutenburg-Richter relation yields a straight line (Figure 4), which, when extrapolated to M6 from M4.5 or so, allows estimates of the probabilities for larger events (Table 1).

Applying a Poissonian model, the 200-year recurrence from the table indicates a 75 percent probability of a M6 (Table 1). Since it has been more than 200 years since the 1755 M6 Cape Ann event, it was reasoned by some that we must be up in the 75 percent or so probabilities for such an event. This is incorrect, however, because, in the Poissonian model, the probabilities know nothing about the date of the last event.

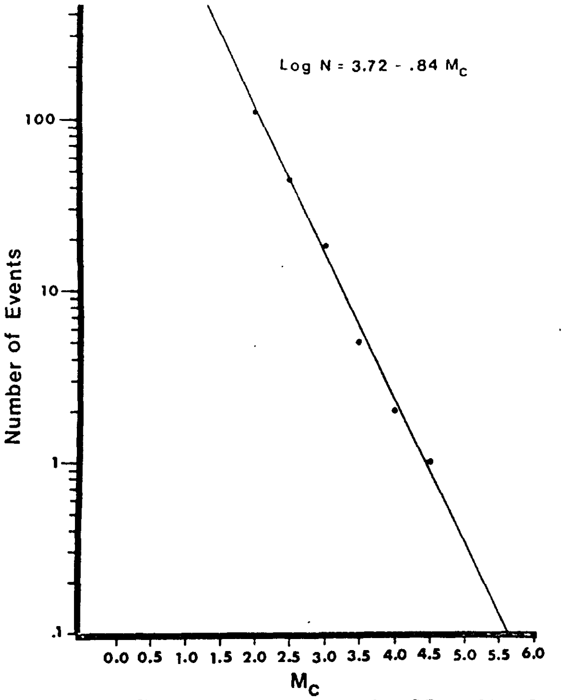


Figure 4. Recurrence curve for New England seismicity from Ebel (1984).

Nevertheless, a large probability of a M6 event before the turn of the century was quoted in the media. While wrong, this raised questions and provided an opportunity for **J.EBEL** to communicate directly with all involved about what the probabiliteis mean and to establish his collaboration with D.Perkins and S.Nishenko. It also led to the convening of a working group to improve the numbers.

What really got peoples attention was an earthquake loss study (URS Consultants, 1989) for Boston that was released right after the Loma Prieta event. The study addressed a postulated event with a M6.25 offshore near Cape Anne, some 40 to 60 km from Boston. The study determined that $4 billion in shaking damage would result from such an event. Estimates for damage ranged from $2 to $10 billion. The study estimated casualties, with a few hundred deaths and a few thousand injuries. Judging from what happened in the San Francisco Bay region in response to the Loma Prieta event, these values seem reasonable. This study did not consider liquefaction, post-earthquake loses due to fire, or aftershock damage, for instance. Most of the Boston area is on some sort of unengineered landfill, and many of the buildings are unreinforced masonry.

Using the numbers published in 1984, we (Nishenko and others, 1993) submitted an abstract to the Spring AGU meeting, indicating that we felt that we could derive some reasonable estimates of probabilities for earthquakes in the northeast.

Table 1 -- Probability of an earthquake of a particular magnitude occurring within the specified time period. Derived by extrapolation of a recurrence curve for New England seismicity (Ebel, 1984) for the period October, 1975, through November, 1982 (all events smaller than M4.5).

| Time (yrs) | 1 | 7 | 10 | 50 | 100 | 200 | 500 | 1000 |
|---|---|---|---|---|---|---|---|---|
| Magnitude | | | | | | | | |
| 4.6 | .10 | .51 | .64 | .99 | 1.00 | 1.00 | 1.00 | 1.00 |
| 5.0 | .05 | .28 | .38 | .91 | .99 | 1.00 | 1.00 | 1.00 |
| 5.2 | .03 | .20 | .28 | .80 | .96 | 1.00 | 1.00 | 1.00 |
| 5.5 | .01 | .12 | .17 | .60 | .84 | .97 | 1.00 | 1.00 |
| 5.8 | .01 | .07 | .10 | .40 | .64 | .87 | .99 | 1.00 |
| 6.0 | .01 | .05 | .07 | .29 | .50 | .75 | .97 | 1.00 |
| 6.4 | .003 | .02 | .03 | .15 | .27 | .47 | .80 | .96 |
| 6.5 | .003 | .02 | .03 | .12 | .23 | .41 | .73 | .93 |
| 7.0 | .001 | .01 | .01 | .05 | .10 | .18 | .40 | .63 |

Subsequently, with M.E.Williams of the USGS, we convened a working meeting at MIT at the end of April, 1994, to decide what we should do next (See Appendix G for participants and tasks). The group comprised a broad cross section of workers, including one from Canada. The tasks were to produce a consensus statement on probabilities for the northeastern part of the country, to produce a colorful seismicity map for the region, and to organize a subsequent workshop to disseminate this information, as well as to promote earthquake hazard mitigation and response measures. A proposal for this subsequent meeting has been submitted to FEMA, and our USGS colleagues think that agency might provide some support.

**J.EBEL** noted that the group initially only considered New England, because this was the region addressed by the controversial NESEC statements. Because of the composition of workers at the meeting at MIT and because of the continuity of seismicity between the regions, we decided to expand the region to included New York and New Jersey. We strongly felt that the catalogs need to be reexamined, especially for historic events above about M4.5. This would serve as a basis for the seismicity map. Determination of the probability of fore- and after-shocks would also be desirable.

After some discussion, we addressed the probabilities. Using data from various catalogues, we normalized to the area of New England and produced estimates of return times for various magnitude events up to M7 (Table 2). These resulted in linear extrapolations of the data and S.Nishenko produced Poissonian probabilities for events above M5, M6, and M7 in periods of 10 years, 20 years, and 50 years.

Table 2 -- Estimates of return times and Poissonian probabilities for various magnitude earthquakes in New England. Data from various catalogs; data normalized to the area of New England. Subscript denotes Possionian probabilities for 10, 20, and 50 year exposure windows.

| Magnitude | ≥5 | ≥6 | ≥7 |
|---|---|---|---|
| Return Times | 60-94 yrs | 447-1035 yrs | 4500-11,000 yrs |
| Probability | | | |
| $P_{10}$ | 0.10-0.15 | 0.01-0.02 | <0.01 |
| $P_{20}$ | 0.19-0.28 | 0.02-0.04 | <0.01 |
| $P_{50}$ | 0.41-0.56 | 0.05-0.11 | 0.01 |

It is interesting to note that my estimates for New England using data from 1975 to 1982 indicated a return time of about 20 years for M5, about 150 years for M6 to M7, and 1000 or something years for M7 and above and compared well with more recent estimates. The estimated return times produced during our collective effort are in the range of about 60 to 94 years for M5 in New England, about 450 to a little over 1000 years for M6 to M7, and several thousand to 11,000 years for M7 and above.

From the historic record in New England, about five or so events of estimated M≥5 have occurred. The uncertainty involves two events in Ossipee, New Hampshire in 1940, one on December 20, and one on December 24. Should we count these as one event or two? This yields a return time of about 40 or 50 years (266 years divided by 5 or 4 events), about the same ballpark as our instrumentally derived recurrence curves.

We used a catalog that only contained instrumental data, for the most part. J.Armbruster used two different catalogs for his determinations, one a Lamont catalog, the EPRI catalog and the EPRI catalog plus some other events that Lamont has come up with. J.Adams used the Canadian northeastern catalog and ratioed it down to the area of New England. Thus different catalogs were used, admittedly an overlapping set, but this presents the range of numbers. We felt that we could put together a consensus about return times and probabilities, and we did so, but we do have some continuing disagreements concerning the catalogues and estimated sizes about some of the older events therein.

**J.EBEL** guessed that even when the group is satisfied with the catalogs, the numbers won't change very much because the events in question are few.

Also presented at Baltimore was a comparison of work by L.Jones (1985) in southern California. From 1932 to 1983, southern California had about 89 events $M \geq 5$. New England, with roughly comparable surface areas, had about five such events from 1727 to 1992. This makes southern California about 91 times more active than New England at $M \geq 5$.

**J.EBEL** cited G.Bollinger's (Bollinger and others, 1993) estimates that the damage areas in the eastern U.S. for intensity VI is about 3.6 times greater than the western U.S. Intensity VII (M6) is about 6 times greater. The activity rate divided by these factors of intensity indicate that the hazard is actually greater in the east than the activity might indicate, because of the larger area over which one might get damage from a given event. The "hazard" in the eastern U.S., then, is 26 times lower at M5 and 14 times lower at M7 when compared to southern California.

In response to a question from A.Johnston, **J.EBEL** indicated that the magnitudes of the earlier events were derived from total felt area or intensity for area. Therein ensued a discussion of damage area ratios. **J.EBEL** indicated that he used G.Bollinger's numbers instead of those of A.Johnston (Hanks and Johnston, 1992) because the former recontoured the intensities of a lot of the events from the early part of this century.

**R.WELDON** posited that the hypothesis presented was incorrect. For instance, the number of deaths in New England from earthquakes does not approach 1/14 that of southern California. On the other hand, **R.WESSON** pointed out that we know that damage areas are larger for eastern earthquakes, as are felt areas. **J.DIETERICH** asked if we weren't addressing distinctions between geometric attenuation of some kind, that would be identical in both areas, and material attenuation, that would be different, and the strong ground motion would be close. Others pointed out that this may not be the same, because in the eastern U.S. numerous waves would be trapped and because of wave propagation differences between the east and west.

**J.EBEL** used the Saguenay, Quebec, event in 1988 which lead to damage to the City Hall in Montreal, as an example of the large damage areas in the east. **R.WESSON** pointed out that this event was felt in the Washington DC metro area.

**R.WESSON** continued by observing that one of the key problems in the whole US earthquake program is understanding the risk in the east coast relative to that in California. If we just try to use what we know, the critical link is the attenuation relationship. We don't know it well enough in the west, and we know very little about it in the east. If we try to produce a rational cost/benefit type analysis for how much the program should spend in New England versus California, it boils down to the attenuation relationship in the east.
In part, this is the rationale for the national seismic network so that we have some means to calibrate strong motion when a M6 event occurs in the east. We also need to develop a better understanding about the origin of the earthquakes. Some seem to be related to the opening of the St. Lawrence and some to the astrobleme. But, fundamentally, we are clueless about the others, including the Cape Anne 1755 event which is the model of the M6 event.

**J.EBEL** noted that L.Jones also derived an estimate of about 6 percent of southern California events have a comparable or larger shock within 5 days and 10 km. Foreshocks are present in the record for the east. In fact, the largest events since 1980 was the Saguenay, Quebec, with foreshocks of up to M4.7 and the January 1982, Miramichi, New Brunswick, earthquake which had a M3.1 foreshock. Using 7 days and 20 km, **J.EBEL** found that about 3 percent of earthquakes in the Northeast catalog are followed by a comparable or larger shock. A discussion ensued concerning the differences in assumptions and thresholds between the L.Jones work and that presented here, and **W.BAKUN** pointed out that P.Reasenberg's (Reasenberg and Jones, 1989) generic model would obtain different statistics also.

Before he started his work in the East, **J.EBEL** had been informed that the seismological environment was quite different in New England, compared to that in southern California, where he had trained. In fact, the areas do not appear to be as different statistically as anticipated. Aftershock patterns for the 1981 Miramichi event, for instance, follow the typical Omori decay with a slope of about 0.7 or 0.8.

The next tasks involve the catalog and a workshop in the northeast, which might occur in the winter of 1993-1994 in Connecticut, New York or western Massachusetts, involving a broad coalition of folks with interest in earthquakes and their effects.

At the meeting at MIT, R.Updike drafted a statement and, taking our discussions and suggestions at the meeting into account, redrafted it at the meeting. This was the first output of the group (Appendix H).

**J.EBEL** noted that he had presented statistics for New England this evening, but this statement presents the numbers for the entire northeast, to include New York and New Jersey. As it turns out, New England is a little more than half the surface area of the northeast. M.E.Williams is producing a draft of the proceedings, this is the first release of the statement outside of the two-day meeting.

Some discussion ensued concerning wording in the statement, especially the underlying assumptions, the geographic area, and the choice of a M5 for a threshold. Ultimately a press release will be prepared, and some sort of statement by the working group would be appropriate. Such a statement also would help start the workshop.

Many additional and wide-ranging comments were made, and much advise was offered, especially addressing the attenuation, zonation, construction, and public-outreach issues.

Given that the presentation was informational in nature and not action or decisionmaking oriented, the Council thanked J.Ebel and indicated that it was encouraged to see activities such as those described being undertaken in the region and asked that the Council be kept informed of progress as it occurs.

June 4, 1993
Morning Session

**C.WEAVER** introduced his presentation on the status of the ad hoc working group on the Cascadia subduction zone by discussing the Scotts Mills earthquake. This $M_L$5.6 event, which occurred on March 25, 1993, at about 5:30 am, local time, was the biggest crustal earthquake, since 1936. It was slightly larger than the M5.5 1981 Alkaloid Lake earthquake which occurred near St. Helens.

Unlike Alkaloid, however, this earthquake did 30 million dollars in damage in the Willamette Valley of Oregon. It produced about 15 million in uninsured damage to a church and abbey near the epicenter and about 13 to 15 million in damage to insured public buildings. The region was declared a federal disaster area.

The earthquake occurred about 35 km south of Portland, where about 25 instruments were deployed. A M3.2 event has been the largest in the typically poor aftershock sequence in the Pacific Northwest. The final depth is about 15 km for the thrust mechanism with east-west striking fault planes. Combining the short-period and broad-band data, it appears that the mechanism has a fault plane aligned northwest-southeast with the general distribution of the aftershocks with a combination of strike-slip and thrusting.

**C.WEAVER** continued that beginning on the 26th of March, the day after the Scotts Mills earthquake, predictions of subsequent events caused quite a stir. I.Madin, of the Oregon Department of Geology and Mineral Resources (DOGAMI), provided a summary (Appendix I) of the events from his agency's point of view, and **C.WEAVER** presented the following narrative from this and other anecdotal sources.

A column in the Portland Oregonian said that an earthquake larger than the Scotts Mills event would soon hit Portland. The next week, a "postdiction" for the Scotts Mills earthquake was claimed by an A.Trombley, and details for the prediction of the coming earthquakes over the next few months were given. This prompted a number of media inquiries both in Seattle and Portland, and state and federal agencies in Oregon and Washington discussed options by phone.

On April 1, 1993, at a post-earthquake coordination meeting to evaluate their response to the Scotts Mills event, the dozen or so federal and state agencies decided to take a wait-and-see attitude relative to the "predictions" because no "prediction information" had been printed in the previous day's column. They reasoned that, given the upcoming "timber summit" involving the President and five Cabinet Secretaries and the Vancouver summit between Yeltsin and the President, the local press would have little space or inclination to make a big deal about these so-called earthquake predictions.

Someone called the Coast Guard about the prediction, and a mistake in reporting the call was made, which, because of the link through Treasury, resulted in the Secret Service being informed that a large earthquake was going to strike Portland. This resulted in the Portland Fire Chief and various officials in Portland being informed that the Secret Service was going to "take over" because the President was in town.

At noon on Friday the 2nd, DOGAMI, the University of Washington, and the USGS released a statement addressing the Scott's Mills aftershock sequence, which had decayed nicely with events below M3. At about 1 pm, the Governor's office in Oregon called DOGAMI and requested a public statement about the predictions. DOGAMI called C.Weaver, and with the help of E.Roeloffs, R.Wesson, and S. Malone, a one-page statement (Appendix J) indicating that no predictions existed in the Pacific Northwest, was released at 4 pm, and it seemed to have slowed interest on the part of the press.

Council members found the ongoing activities on the part of those making the "predictions" to be somewhat troubling. One of the individuals, a D.Farnsworth, appears to be setting up a prediction installation somewhere in the Portland area, and he may have asked for equipment from Hewlett-Packard.

Council members mentioned and discussed reports of these individuals' activities in California, where they appear to have taken a tour to several seismically active regions of the state. They purport to have a "predictive tool" that involves a microphone pointed at the ground to detect microfractures in rock as well as magnetic signals. They are reported to have convinced some investors to put a million dollars into the venture to do earthquake prediction. Some members thought that it would be prudent to track the activities and "predictions" of these individuals for future reference.

The DOGAMI stance is now one of ignoring any "predictions" made by these individuals. Fortunately, the time-frame was much shorter than in the so-called "Browning prediction" of a few years ago, and we are nearly out of the window for the "predicted" large event. It is important that NEPEC not get in a position of having to respond to "predictions" such as those discussed today.

**C.WEAVER** noted that, in spite of the flap discussed above, much progress has been made with the media in the Pacific Northwest region. A month before the Mills Creek event, a news conference was held to discuss aeromagnetic data from the Portland region which hones the interpretation of the Portland Hills fault zone. After the earthquake, on April 25, a news conference was held to discuss what was learned after the earthquake. None of the "predictions" were mentioned. If the "predictions" had not occurred during the time of the two summits with the attendant national press coverage, they probably would have not had near so much coverage.

With regards to the report on the ad hoc working group on the Cascadia subduction zone (See charge, Appendix K), **C.WEAVER** indicated that much progress had been made. At the outset we sought a consensus opinion from the paleoseismologists concerning the best integrated interpretation of the data coming from the marshes along the Pacific coast. At the time, we thought that the working group report might consist of an extended executive summary of some 6 pages with three appendices, including the paleoseismic statement from B.Atwater, G.Carver, and others, a statement of supporting geophysical and framework issues, and a summary of outstanding problems and issues that have been raised. We continue to be concerned about who the document is being written for and why we were doing it.

Over the past several months iterations of the paleoseismic component have been forthcoming, and we have been considering a cleaner regional document with respect to a consensus opinion on the focusing on state of knowledge with respect to Cascadia earthquakes coming from the geology and downplaying the supporting geophysics and tectonic framework issues.

Such a document would be quite useful for supporting implementation efforts in both Oregon and Washington. It would not have clouding issues such as why the strain rates are so low, or the uncertainties concerning the source of the seismicity in the crust, or what is driving the crustal stresses, as well as leaving out some of the significant engineering problems such as duration and amplification.

**B.ATWATER** presented the main points of the current iteration of the consensus document (Appendix L) and outlined evidence for large prehistoric earthquakes in the Pacific Northwest (See Appendix M for illustrations presented and references cited in Appendix L). Much of the evidence points to subsidence followed by deposition of sand, presumably by tsunamis. Evidence for seismic shaking is not as widespread and does not appear to support very strong shaking. The intensity of shaking responsible for features along the Columbia River will be addressed this summer. See note from S.Obermier and S. Dickenson (Appendix N).

While recurrence poses a difficult problem, all things considered, it appears that we are dealing with hundreds of years between successive events. **B.ATWATER** indicated that during deliberations to attain a consensus, the variables and uncertainties weighed differently on different individuals. No consensus on how to handle this or how to compute probabilities from this sort of evidence was reached.

**B.ATWATER** showed some slides of the effects of tsunami following the 1983 earthquake in the Sea of Japan to broach the subject relative to Oregon, Washington, and northern California. He noted the evidence for tsunami within the Puget Sound, which may have to 6 to 7m runups.

Most communities have taken little action with regards to tsunami and are looking for guidance and support. The two communities that have undertaken some activities have systems that would protect more from an Alaskan event than from a local Cascadia event. One community evacuated in response to the Scotts Mills event and the local emergency services folks indicate that they learned quite a lot during the evacuation.

**B.ATWATER** concluded his presentation by stating that the draft document recommends that NEPEC help local and state implementation efforts by releasing public statements concerning the reality of a Cascadia earthquake and the tsunami threat.

**T.McEVILLY** indicated that the best statements that NEPEC has made have been endorsements of NEPEC generated and accepted reports on regions. An attempt to send a letter to responsible individuals in Utah did not work because, while plenty of good work has been done in the region, no NEPEC-generated document existed.

While the system may be slow, it appears to be the best way to obtain a NEPEC statement.

**C.WEAVER** indicated that what seems to be needed is a clean and simple document on the Cascadia event and tsunamis. Such a document would be useful to the engineers. He noted that a multi-issue professional paper was being published on the region. The state building code effort seems to be losing to federal NEHRP building codes. Essentially, two code maps exist, one for local construction and one for federal construction. The local engineers seem upset by the federal code pushed by FEMA, which would result in buildings being built to lower standards than local nonfederal buildings.

**J.EBEL** noted that he had participated in subcommittee activities and that different viewpoints exist. The zones are drawn based upon hazard calculations that consider source zones and wave propagation from the source zones to the sites. They really represent strength of ground shaking. If one just runs the zones north and south parallel to the coast, one tacitly makes the judgment about where the ground shaking will occur. If one ups the standards significantly, one requires design standards stronger than those for nuclear power plants in the area.

**C.WEAVER** noted that the local engineers were concerned that the maps essentially ignore, for example, the 1872 event, the possible Cascadia events, and the more recent Borah Peak event. **T.HEATON** noted that none of the nuclear plants in the Pacific Northwest benefited from our current understanding about the seismic environment, including our understanding of moderate crustal earthquakes within the Puget Lowland. This led to a discussion of segmentation of a Cascadia event, whether the whole zone released at one time or in discrete segments. **R.WELDON** noted that many models of release appear reasonable. **K.SHEDLOCK** noted that the 50-year maps lose most earthquakes on the Wasatch Front, which looks like central Texas on the maps.

**R.WESSON** agreed that C.Weaver and the team from the Pacific Northwest needed to meet with the BSSC hazard group, but that the issue seemed inappropriate to pursue any further at this meeting. **R.WESSON** urged that the group determine what sort of consensus statement NEPEC wanted, what feedback the group could provide B.Atwater and his colleagues, what sort of publication should be sought, and what sort of executive summary might be added.

**R.WESSON** suggested that the document was first and foremost for earth scientists. We need to have a point upon which we have good consensus. We need something to build on for a variety of purposes. NEPEC could accept this and attach a letter from the Director, stating that this is the state of our understanding. **B.ATWATER** asked if such a cover letter could include a statement such as "and NEPEC recommends that people take measures against possible tsunamis," such a document would be very helpful to those trying to implement hazards reductions measures. **R.WESSON** stated that such a statement was possible, but noted that we are in a long struggle. Nevertheless, we should go the next mile to make that sort of statement.

**J.DIETERICH** suggested that a consensus statement be prepared to summarize our level of understanding and the outstanding controversies involved. This would be very useful and lend some scientific credibility. Many members indicated that the 7 page document, with a few more figures and illustration, was a very good start for a consensus on the paleoseismology in the region.

**C.WEAVER** agreed that this document, with some editing, could be combined with a statement summarizing, for instance, where we stand with the strain observations, what the crustal earthquake distribution says, and other supporting issues relevant to the Cascadia earthquake concept.

**T.HEATON** recommended that the report specifically compare the Cascadia zone to other similar zones in the world, such as, the zones in southwestern Japan, Columbia, and in southern Chile, with which it shares many characteristics, and that compelling geologic evidence supports the occurrence of very large earthquakes and ensuing tsunamis such as those that have occurred in those other zones. The recommendation would be that communities consider the occurence of such earthquakes possible in the Pacific Northwest, and that plans should be made accordingly.

**B.ATWATER** indicated that substantive issues that deal with coastal geology easily could be addressed, but to try to get the whole group of participants to address other issues and agree to changes that are outside of their field of expertise would cause the level of consensus to plummet. Such statements summarizing the state of affairs in other fields could occur up front with different authorship.

**R.WESSON** supported B.Atwater's idea that the report be released with two components. The consensus about what the marshes say would be the second half or two-thirds and the first third or half would be written by a different set of authors and contain some interpretation that several folks would write are return to NEPEC.

The group agreed that an open-file format or circular format might be appropriate, and that a summary article in EOS might also be appropriate.

**T.McEVILLY** suggested that members address the consensus document on paleoseismology and that we consider accepting an embedding document that includes this material as well as summarizes other issues and our state of understanding with regards to those issues. He expressed concern about how long it might take to reach consensus with regards to the other issues, such as strong ground motion. **J.DAVIS** supports the near-term publication and asserts that it will be useful to both the scientific and the public policy communities, as have NEPEC's previous documents. This document deals with some questions that can be closed as well as notes areas where more work is needed, such as the recurrence interval. He agrees with J.Dieterich that more figures and illustrations would more thoroughly communicate the overall message.

Many members participated in a wide-ranging discussion during which they asked questions, discussed details, and made suggestions concerning the draft consensus document. They also discussed possible contents and thrust of the embedding document, which would consist of 10-pages, including supporting figures. Release would be some time this summer. The group encouraged inclusion of explicit statements concerning the status of the consensus with regards to different issues.

**T.McEVILLY** noted that the current document states no consensus was reached with regards to the magnitude issue, but a lot is included in the discussion of the uncertainties. **R.WESSON** probed a statement about the degree to which these inferred earthquakes exceeded M8 by asking how a consensus for a M8 was developed.

**B.ATWATER** noted that the group rounded to the nearest integer, and agreed that the current wording made it implicit that the events exceeded M8. Some minor rewording might be warranted.

**T.HEATON** asserted that planners are interested in the maximum credible earthquake. If we talk about M8 events, we have to say we are assuming the average M8 involves 150 to 200 km of rupture. It would take 5 to 6 M8 events to cover the zone or two to three M8.5 events. If the entire 1200 km long zone ruptured, we'd probably see a M9 event. But as soon as M8 events are discussed, the folks at EERI say that's all they are willing to address; they don't want to talk about anything greater than M8.

**T.McEVILLY** summarized that the group basically has agreed that, with some minor rewording, clarification, and simple illustrations, the report is quite acceptable, great in fact. Now the question involves what does NEPEC do with regards to the two recommendations that NEPEC (actually the Director) advise the public about great earthquakes and tsunamis?

**R.WESSON** suggested that they would be more easily addressed if they were in the form of a conclusion rather than recommendation. Participants suggested that the issues might be addressed in a discussion or implications section of the document. Perhaps a letter from the Director in the forward of the Circular or as a letter of transmittal could convey this message. The statements could be placed in the embedding document.

**J.DAVIS** suggested that the letter transmitting the document to the Director could characterize the recommendations as conclusions and state that we view these as a serious concern for public safety. This is the foundation upon which a whole series of actions might be based.

The group agreed that the proposed circular would consist of the consensus report concerning the evidence in the marshes, the embedding document, and a draft letter that would be the cover page for the circular. It would seem appropriate for the Director to send copies with transmittal letters to the three governors and to our Canadian colleagues for their use. The group hoped for a July 1, 1993, transmission of the embedding and consensus documents to NEPEC for review and a middle August transmittal to the Director.

**J.DAVIS** noted that the level of understanding of the tsunami issue is generally rather immature, both with regards to effects and warning time. A lot of denial exists. We should note minimal consequences in the introduction; we should indicate that warning time on tsunami can be very short in this environment because these are locally generated. This compares to many hours of advance warning for tsunamis generated in the Gulf of Alaska and announced through the NOAA network.

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**M.WYSS**, of the University of Alaska, Fairbanks and the acting state seismologist for Alaska, briefed the Council on the seismological environment in the Aleutians-Alaska Peninsula region and briefed the group on the recent Shumagin earthquake (see Appendix O for overhead illustrations presented).

After having won his election, former NEPEC member J.Davies has left his position as State Seismologist to join the Alaska State Legislature. A search committee is looking for a replacement, and **M.WYSS** sought members' assistance to fill this position with a top-level seismologist.

The May 13, 1993, Ms6.9 earthquake in the eastern Aleutians-Alaska Peninsula area (Figure 5) occurred in the Sand Point area (Figure 6). It occurred in the early morning hours and local residents evacuated to higher ground upon being awoken.

**J.RIEHLE** of the U.S.G.S. pointed out that all coastal Alaskan communities have a civil-defense-like warning system for tsunami warning, with the system triggering at M7 events.
A focal mechanism (Figure 7) from S.Jaume depicts a low dipping thrust.
Crudely sketched aftershock zones of great events that have occurred in the region (Figure 8) and the epicenter of the recent event give a regional picture. The epicenters on the map are located using two S-P times, one from Sand Point and one from the volcano observatory at Mt. Dutton. No other stations exist within 600 km of this event; Kodiak is the next closest station and these aftershocks are too small to be observed from that distance. The times from Dutton were calibrated with events from the former Shumagin network. This data indicates that the rupture was about 30 to 40 km in length, a length roughly appropriate for a M7 event.

The Shumagin gap is recognized as possibly one of the world's top contenders for receiving a M8 or larger. It has been designated by IASPEI as an international location for cooperation in earthquake studies in subduction zones. The Alaska net has 150 stations with an average interstation distance of 50 km. We are attempting to find ways to improve coverage in the Aleutians.

According to **M.WYSS**, Boyd and others (1988) presented evidence supporting segmentation of the arc (Figures 8, 9). The closest historic epicenter ruptured in 1917. Cross sections from S.Jaume (Figure 10) through the Sand Point and a region further west depict the hypocenters of events located during the time of the Lamont network through middle 1991. Earlier events in the middle-M6 range have fault plane solutions similar to that for the recent event. The 1993 event occurred about 30 to 35 km in depth and in the thrust zone.

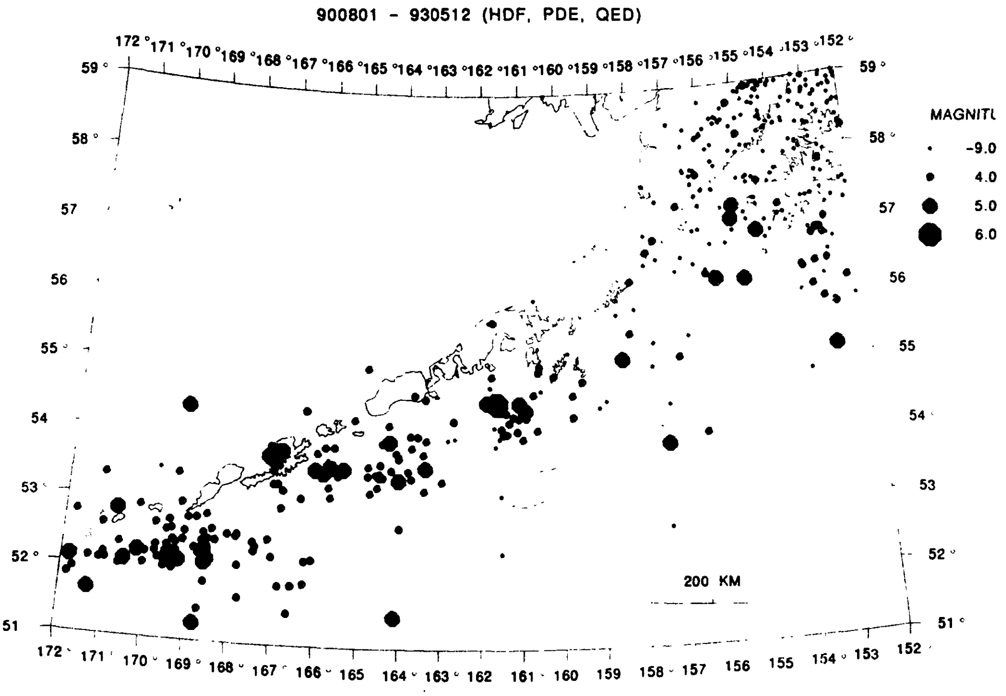



Figure 5. Seismicity map of the eastern Aleutians-Alaska Peninsula area using the PDE and weekly reports of the National Earthquake Information Center for 1 August 1990 through 28 May 1993. (from C. Stephens, USGS, Menlo Park).

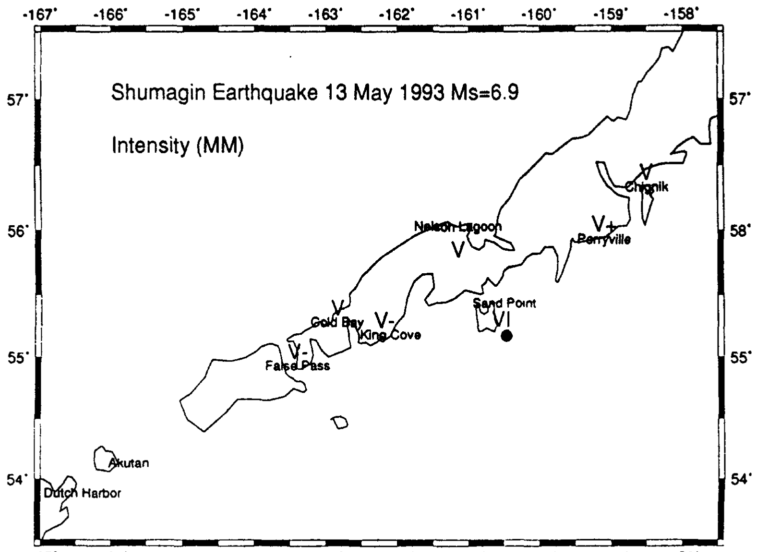



Figure 6. Macroseismic map for the Shumagin earthquake of 13 May 1993. Solid dot indicates epicenter relocated by E.R.Engdahl (USGS, Golden).

**M.WYSS** next presented information indicating that the recent event may not be the end of the story. A hypothesis exists that the outside of the Mogi donut, or a part of seismicity patterns that may develop before a main shock, could be represented by cumulative moment release. These curves (Figure 11) may have an exponential increase where a major earthquake could be predicted to occur at some future time. The group at Golden, C.Bufe, S.Nishenko, and D.Varnes have given a paper on this hypothesis.

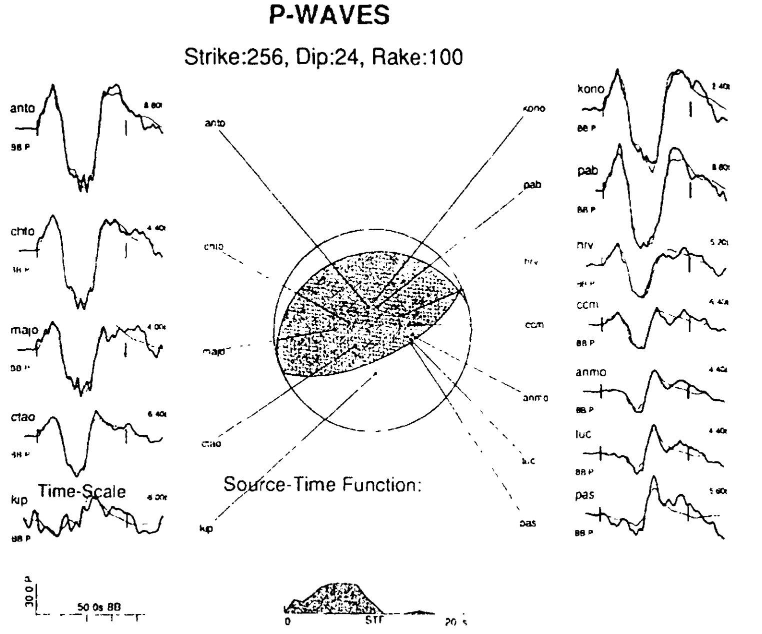



Figure 7. Moment tensor solution for the Shumagin earthquake of 13 May 1993 (from S.Jaume, Lamont-Doherty Earth Observatory).

**M.WYSS** has been skeptical of the hypothesis because the acceleration often depends on one event. This multi-event curve (Figure 11), however, does seem to look like an exponentially increasing curve. The workers from Golden determined a window of about four years for a M7.5 to M8 event in the gap, and we are in the window now.

The low b-values related to this event also demand attention (Figure 12). The general background b-value is 0.8 to 1.0. The b-value of 0.38 is extraordinarily low and the curve has a kink in it. In general, there are too many large earthquakes to the number of smaller events. A M7 event, on the average, has 30 M4 aftershocks, but this event has only been followed by 3 to 4 such events. We should expect 1000 M1 events, but we only have 200. Some foreshock sequences have a low b-value compared to the aftershocks. Whether or not this is a predictive tool has not been established. **M.WYSS** thinks that our colleague Smith, of New Zealand, would use this to indicate that this possibly was not an aftershock sequence, but rather a foreshock sequence.

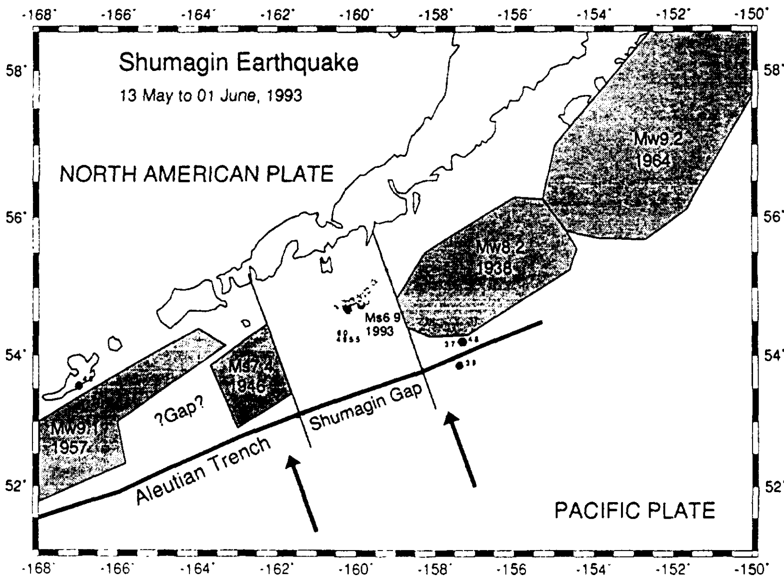



Figure 8. Map of the aftershock locations of the Shumagin earthquake (circles) during the first two weeks. Aftershock areas of large and great historic events are outlined and their year of occurrence and magnitude are shown. The mainshock epicenter depicted here was relocated by S.Jaume based upon the Sand Point S-P time.

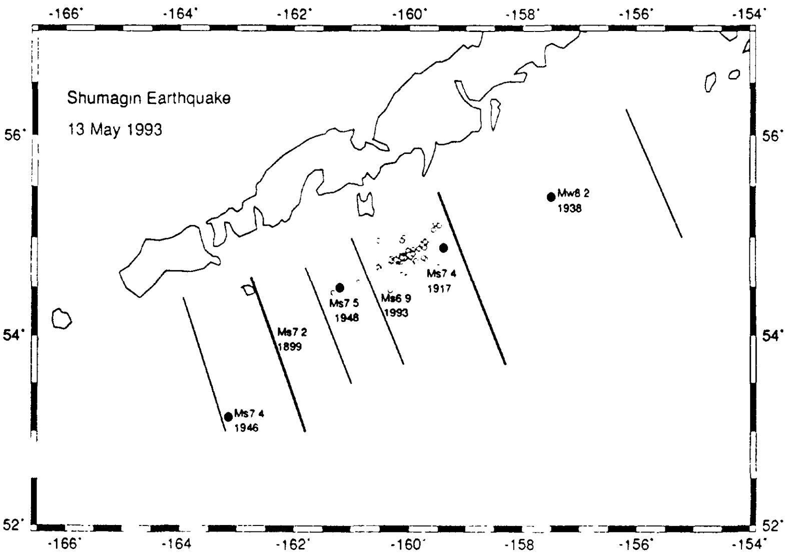



Figure 9. Aftershock area of the Shumagin earthquake of 13 May 1993 compared to the segments of the Aleutian arc in the vicinity of the Shumagin gap which have broken in historic earthquakes, delineated by lines perpendicular to the arc.

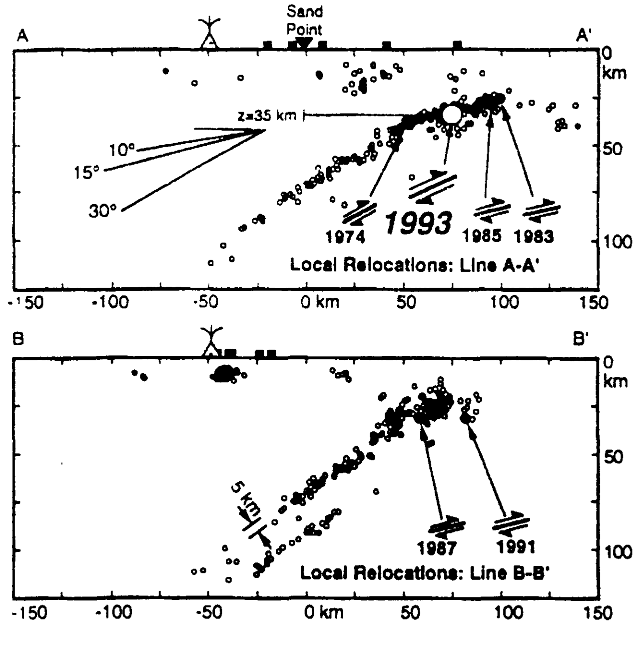



Figure 10. Cross-sections of the Aleutian subduction zone at the Shumagin Islands (AA") and at a location about 100 km further west (BB") showing fault planes and slip directions for larger events. Small circles: hypocenters of earthquakes located by the Shumagin seismograph network. Large circle: the fault plane solution and location of the May 13, 1993, event (from S.Jaume, Lamont-Doherty Earth Observatory).

The expense of locating temporary seismograph stations had seemed to preclude collection of aftershock data, but the above several lines of evidence convinced **M.WYSS** that these events could be a preliminary to larger event in the Shumagin gap. Once the decision was made, B.Busby of IRIS delivered 6 rapid response stations in two days. If the stations do not produce exciting results within a month they will be retrieved.

In response to a question, **M.WYSS** summarized the core of the gap hypothesis. It was thought for a while that the M7.5 1917 event broke the entire Shumagin gap (Figure 8). More recently, however, the magnitude of that event has been downgraded, so that it is now thought that the 1917 event may have only broken part of the gap. The average recurrence time in the region is estimated to be 60 to 75 years. The 1948 event is a special event, it may not have been a thrust event. Many new concepts and ideas have somewhat muddied the perception of a crisp gap theory in the region. Some have asserted that the feature is a permanent gap because the geodetic networks have not recorded strain accumulation.

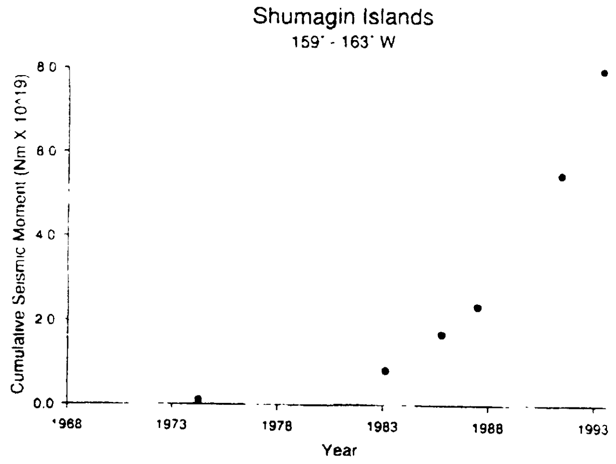
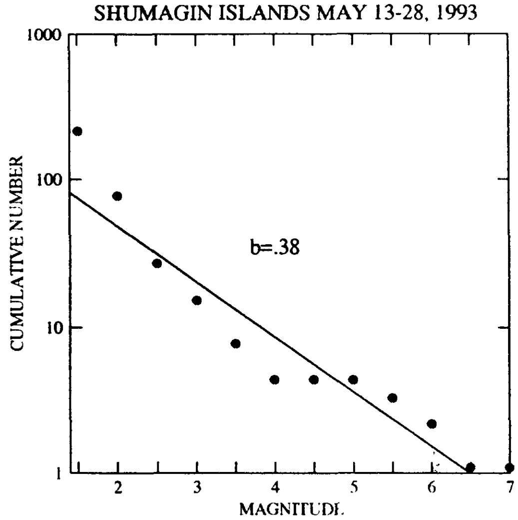



Figure 11. Cumulative seismic moment as a function of time in the Shumagin seismic gap area. The accelerating moment release rate is taken to indicate that a gap-filling earthquake may occur soon (from S.Jaume, Lamont-Doherty Earth Observatory).



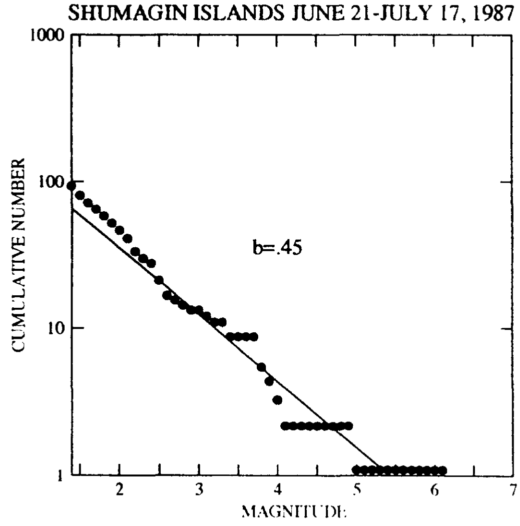




Figure 12. The b-value of the frequency-magnitude relation of the 13 May 1993 earthquake (b=0.38) is unusually low compared to b=1.0 of the background activity in the Shumagin area. However, the aftershock sequence of the M6.1 event of July, 1987, is also low. Low b-values are characteristic of foreshock sequences. The interpretation that the sequence that started on May 13, 1993, is a foreshock sequence (because of the low b-value) may be rejected on the grounds that aftershock sequences in the area may have low b-values in general.

June 4, 1993
Afternoon Session

**D.CLAGUE** delivered the first of three presentations on Hawaii which was a summary of the distribution and historic pattern of seismicity in the region.

While the earthquakes associated with eruptions are interesting and allow us to trace eruptions as they progress and dikes as they intrude, such events are small and of no consequence, in terms of seismic hazard.

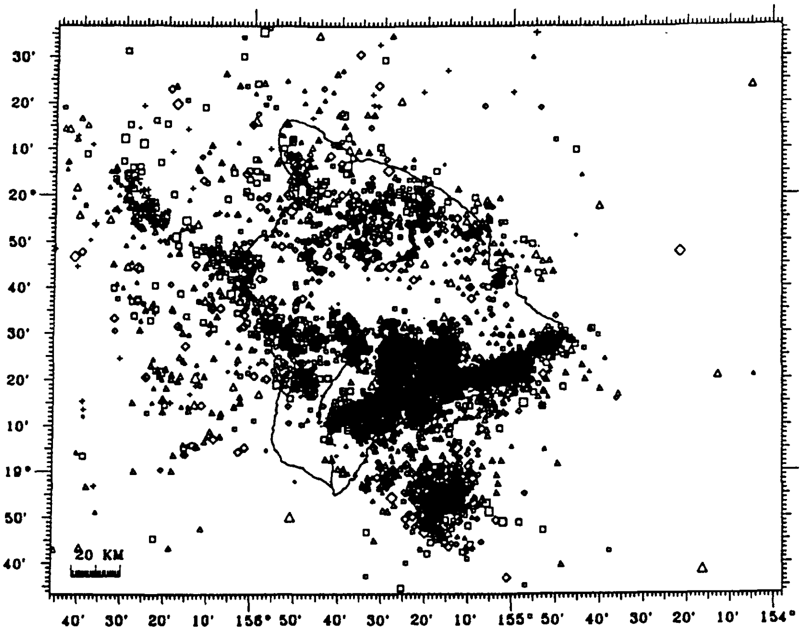



Figure 13. Map of Hawaii showing 22 year (1970 - 1991) record of epicenters for earthquakes M>2.5 (after Okubo and others, 1992).

A 22 year catalog of events greater than M2.5 shows (Figure 13) most seismicity occurring in the southern part of the island of Hawaii. The complete catalog contains some 20,000 events over the last twenty years. A cross section though the eastern side of the island show salient seismic features (Figure 14). Under Kilauea, seismicity provides evidence for magma migration. Below around 10 km, most earthquakes cut out. Below that depth only magmatic events and deep events related to lithospheric flexure occur. Many of the events on the south flank are related to movement on a basal decollement, as the south flank moves seaward.

Hawaii has had a lot of good sized earthquakes including many M6 and larger events (Figure 15). The 1973 M6.3 Honomu event was a deep earthquake, a flexural event, about 45 km in depth. These are related to the load of the volcano on the lithosphere. This event caused quite a lot of damage considering the relatively low population density. This type of event can occur anywhere around the Hawaiian Islands, and historic examples of comparable and even larger events have been recorded as far as Oahu. These pose the hazard in the Hawaiian Islands.

Contours of deflection (Figure 16) provide some feeling for the large amount of subsidence, greater than 6 km in the center, that the islands have imparted on the crust. This subsidence forms a paired flexural trough and arch that surrounds the islands. The younger islands are sinking at a rate of about 3 mm per year. The older islands are sinking at a rate on the order of 0.1 mm per year.

The attenuation of seismic waves in Hawaii is different from other parts of the world. Depending upon where one is, significant differences in ground effects can be experienced. The deep earthquakes are felt strongly on the old islands; they come up through

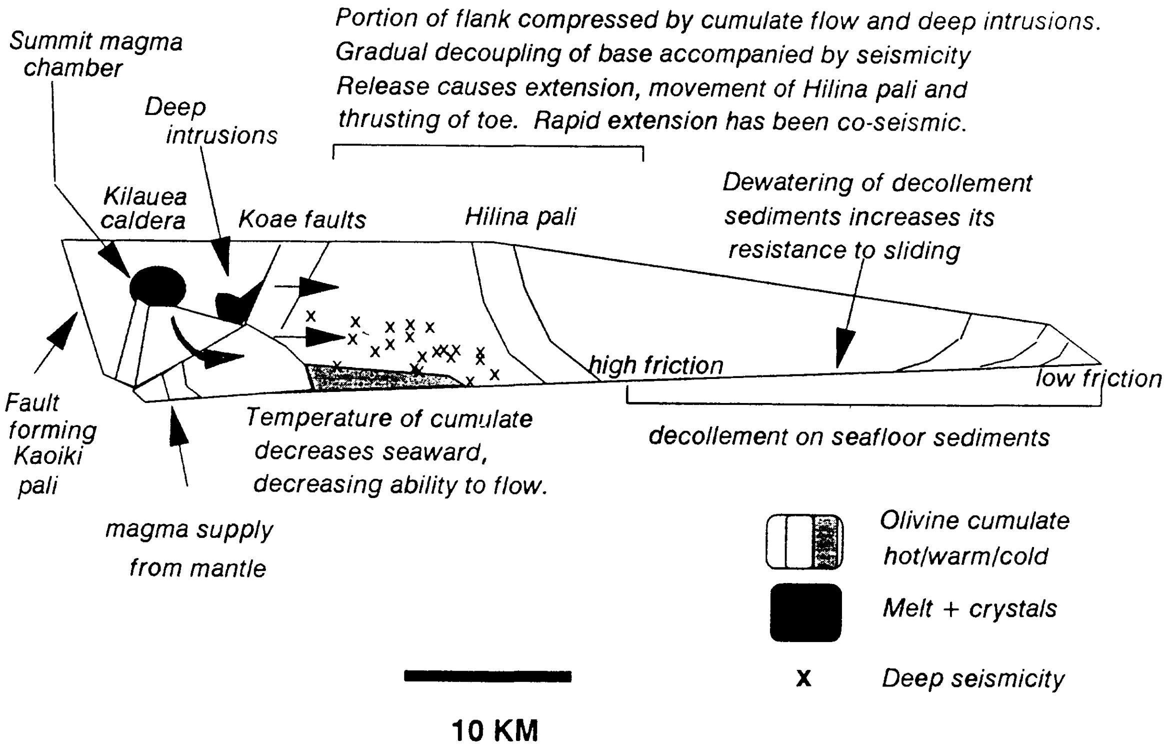



Figure 14. Mechanical model for south flank of Kilauea volcano, drawn through the summit caldera complex (from Clague and Denlinger, in review).

mantle rocks. One M4.1 event beneath west Hawaii was felt in Honolulu some 400 km away. M4 events on the south flank of Kilauea, within 5km of the Observatory, commonly are not felt because they are attenuated by the magma in the reservoir.

Another type of event may pose some hazard. In 1929, two events occurred under Haulalai volcano that appear to be related to magma intrusion within the volcano, which does not erupt very often. These events may indicate that intrusions into older volcanoes that don't erupt very often may generate pretty good sized earthquakes.

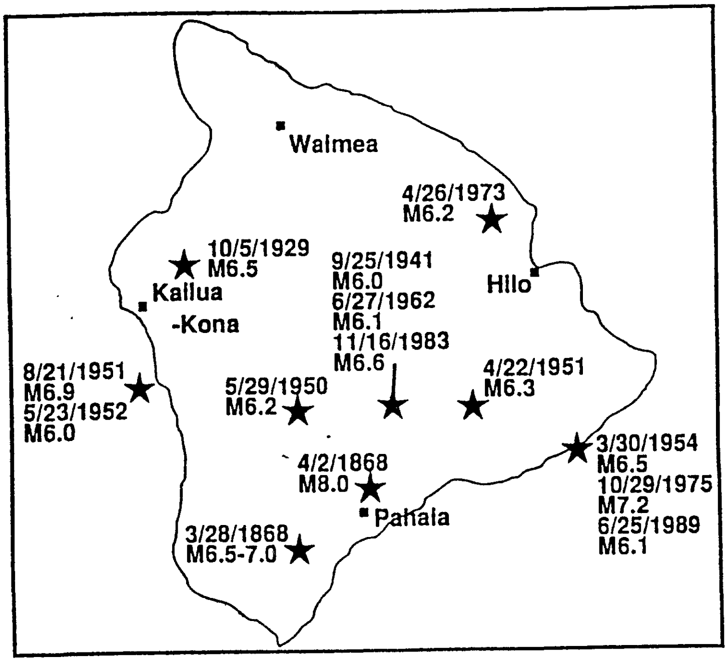



Figure 15. Distribution of earthquakes M≥6 (after Heliker, 1990, p. 35).

Thick ash deposits and soils on many of the islands and local liquefaction can cause some problems.

The 1868 main shock occurred somewhere in southern Hawaii with meizoseismal MMIs of XII and intensities of V in Honolulu. A 1975 event was accompanied by subsidence along the coast and lateral movement on the order of meters. Since that event, this section of the coast has continued to move seaward at a rate of about 10 cm per year.

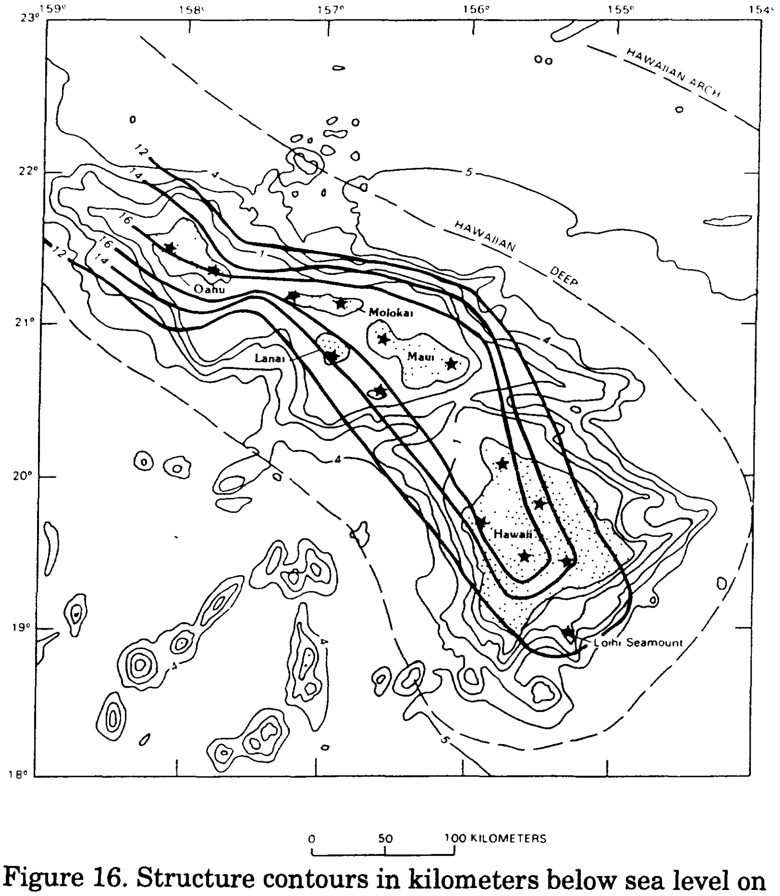



Figure 16. Structure contours in kilometers below sea level on the base of the crust (from Moore, 1987, fig. 2).

In 1975 we had a M7.2 event at Kalapana with meizoseismals MMIs of VIII, and with intensities of V on Maui. A M6.3 event took place in the same area in 1989. The occurrence of a M6 event within 14 years of the M7 suggests nonlinear strain accumulation rates.

An event in 1983 between Kilauea and Mauna Loa, on the Kaoiki fault system, caused failure along part of the rim of the Kilauea caldera. The event damaged the Observatory; a repeat of this event would put us out of business. It had MMI of IX on the flank of Mauna Loa with IV on Maui. These local intensities were greater than the 1975 event, but they were not transmitted as far.

We have a new framework in which to reference many of these earthquakes. This line drawing (Figure 17) based on GLORIA images depicts giant landslide deposits. The south flank of Kilauea is an active landslide complex that could lead to one of these giant events. J.Moore, **D.CLAGUE**, and many others compiled the locations and areas covered by these deposits around the islands. We found 17 easily identifiable deposits, and the relations can become very complicated.

Very little data exists for the age of the deposits. On Kauai, an unusual graben occurs on the south side of the caldera. The graben connects via a channel to a landslide. The graben formed when the volcano was actively growing. A landslide on the Kohala volcano indented the coast and lava ponded in a pull-apart basin in the summit.

One of the blocks in a landslide north of Oahu, as large as 30 km long, 5 km wide, and 2 km tall, was previously mapped as a separate volcano.

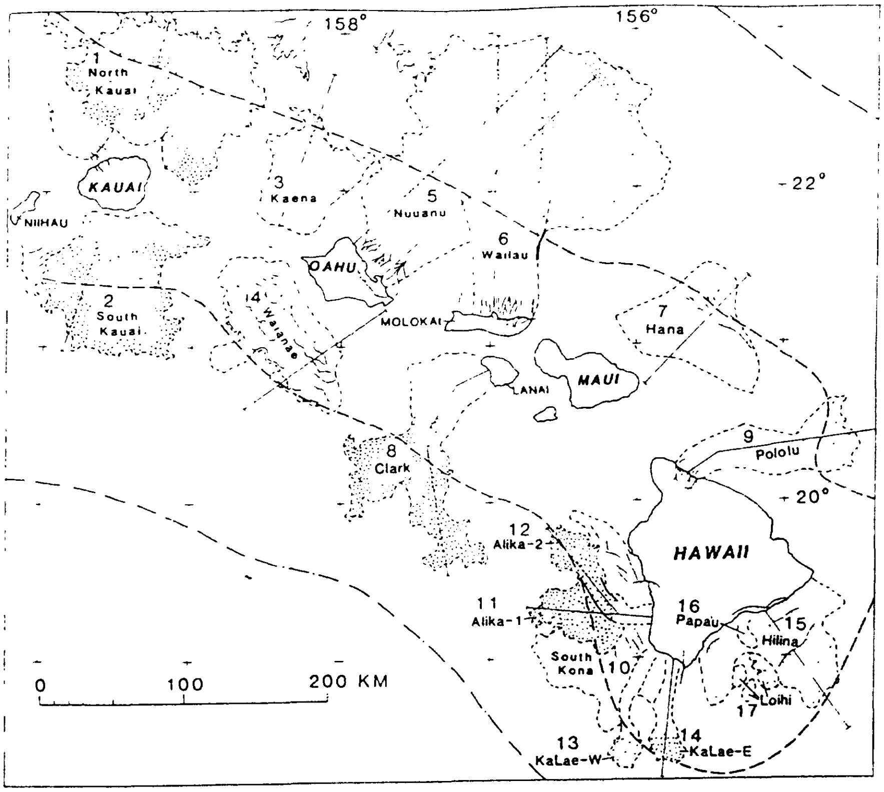



Figure 17. Map of the Hawaiian Islands showing major slides bounded by dashed lines (from Moore and others, 1989, fig. 2).

The relation between these landslides and earthquakes, whether earthquakes caused the landslides or whether the landslides caused earthquakes, is not known.

**J.DIETERICH** noted that one explanation being considered to explain the catastrophic landslide events involves a model in which the thermal processes outpace the heat diffusion process. The cores of the volcanoes become so soft that they can't hold themselves up.

**D.CLAGUE** noted that the trough that surrounds the islands did not affect many of the landslides. The deposits flowed downhill through the low and uphill out of the low. Two overtopped the flexural arch beyond the trough (Figure 17). The fact that the most distal blocks ran uphill 500 m and flowed some 100 km from the island suggests that the failure mode for these features is catastrophic.

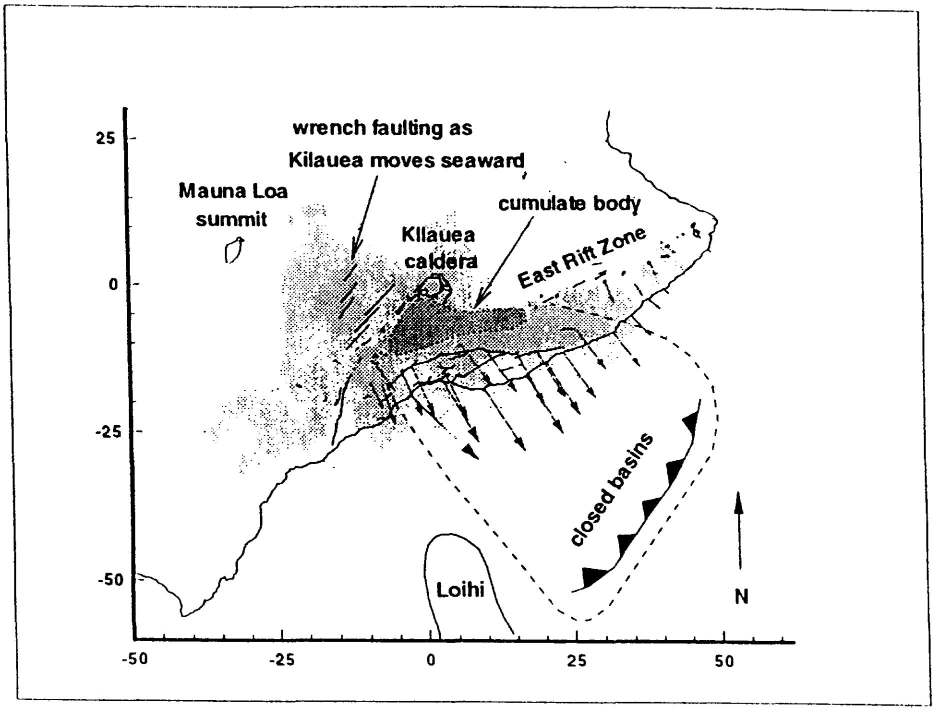



Figure 18. Map of south flank of Hawaii showing rotational slump on Kilauea Volcano. Longest vectors represent 10 m of movement between 1970 and 1989 ( from Clague and Denlinger, in review).

The Alika landslide has two ages of material and hence two landslides. **D.CLAGUE** showed tremendous detail for the younger feature and suggested that it may be responsible for a tsunami deposit on Lanai that washed coral boulders to the 1000 foot elevation level in a canyon. Some of the coral boulders have been dated at 110,000 years and probably approximate the age of the landslide. The coastline of Mauna Loa has been completely repaved by lava since this event, so we can't see the indentation any longer.

The indentation from some of the landslides are obvious. The Kohala landslide goes offshore and created a pull-apart basin at the top about 300,000 years ago. Many of the features cut all the way back to the caldera, and calderas cut in half by landslides are amazingly common; even some of the higher summits are affected.

The south flank of Kilauea is a huge landslide complex (Figures 17 & 18) characterized by extension across the headwall, which coincides with the summit caldera and rift zones, and thrusting at the distal toe. Offshore, the slide is bounded by a strike-slip zone on the western boundary and a zone of disturbed deformation on the eastern boundary. One important aspect of this configuration (Figure 14) is that the landslide slips on a nearly horizontal decollement that has a surface area large enough to generate M8 events, as occurred in 1686. This zone is also the site of the 1975 M7.2 and the 1989 M6.3 earthquakes.

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**M.WYSS** presented a brief overview of the historic seismicity of Hawaii and the Hawaiian catalog, a seismic gap that may exist on Hawaii, and the record of an earthquake that occurs at regular intervals (see Appendix P for overhead illustrations presented).

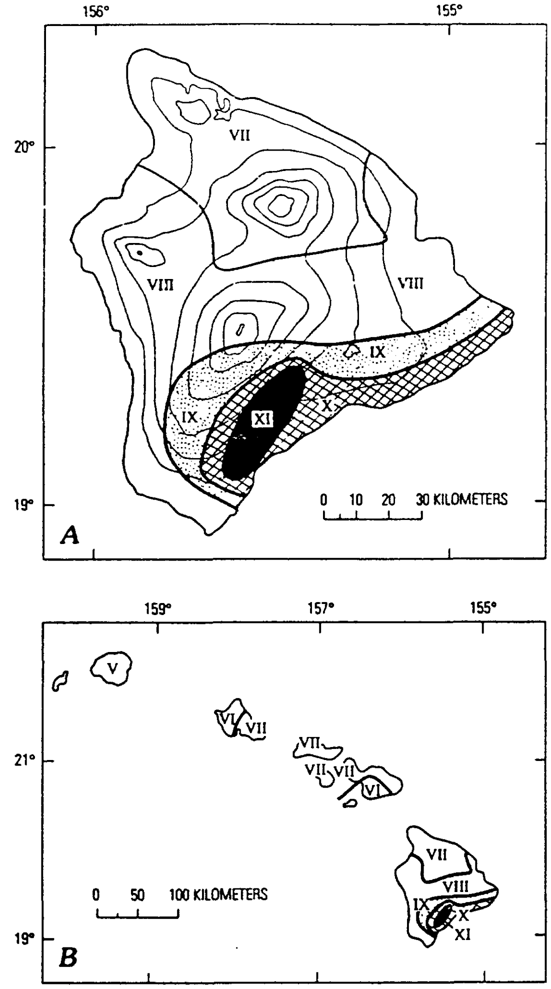
B.Koyanagi and **M.WYSS** have compiled a catalog that goes back to 1832. This catalog is probably complete to that time at the M6.5 level. They used macroseismic maps and compared them to recent events and developed a magnitude and felt-area relationship.

Most of the Island of Hawaii has historically experienced modified Mercalli intensity VIII and above (Figure 19), and the middle islands have experienced intensities VI to VII. The biggest earthquake generated intensity X shaking that caused severe destruction in the southern part of Hawaii. Thus, Hawaii has a significant earthquake problem.

Although most of the big earthquakes are on the Big Island, isoseismal maps for earthquakes that occurred in the middle islands indicate that significant events are not limited to Hawaii.

A structural cross section from earthquake experiment depicts the oceanic crust to be overlain bysediments which in turn are overlain the volcanic edifice. A decollement that has formed in the sediments, the interface between the oceanic crust and the volcanic pile, may be the source of many large earthquakes.

A possible seismic gap exists on Hawaii. A significant portion of the




southern part of Hawaii (Figure 21) ruptured in 1868. Events in 1929 and 1951 ruptured western coastal portions of Hawaii. Part of the zone ruptured in 1868 was again ruptured in 1975 and 1983. It appears that two "gaps" remain unruptured by historic seismicity.

The concept of seismic gap used here is somewhat different than our usual use of the term. While the phrase usually implies the zone should rupture relatively soon, that may not be the case here. In this gap, focal mechanisms indicate slip directions to the northwest. While these might be somewhat unloaded by rupture related to the 1868 event, certainly the 1951 event increased the load on the gap. The area is interesting and warrants some attention.

Figure 19 (adjacent). Map of the maximum historical modified Mercalli intensity for Hawaii and for the Hawaiian archipelago. (from Wyss and Koyanagi, 1992b).

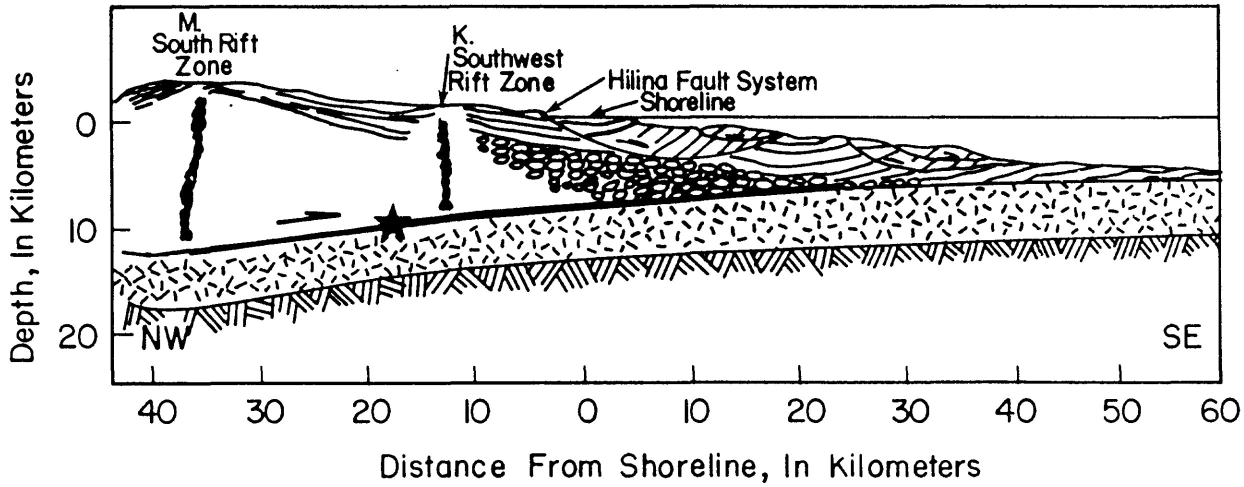



Figure 20. Structural cross-section of Hawaii (from Hill and Zucca, 1987) showing volcanic deposits resting on and depressing the oceanic crust. The oceanic sediments at the base of the edifice form a decollement plane, along which slip of the upper crust occurs in earthquakes. Arrow shows the direction of slip at the hypocenter of the 1868 great earthquake (shown by star, from Wyss, 1988).

Slip vectors (Figure 22) form part of the evidence suggesting that stress driven by magmatic intrusions forms the seismic environment for tectonic earthquakes. These occur in brittle fracture regimens more than 15 km from the nearest magma, as compared with the volcanic earthquakes which are associated directly with magma movement.

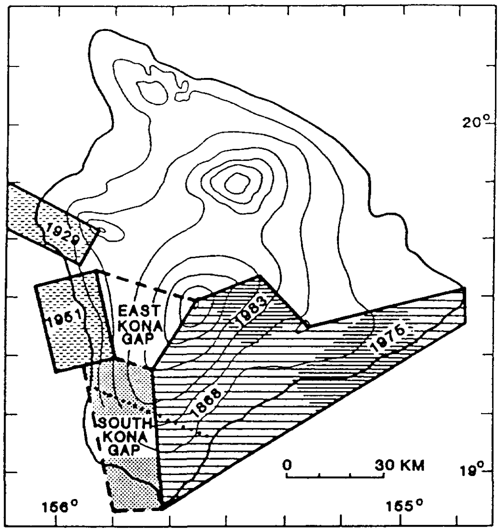



Figure 21. Map of approximate extent of historic rupture areas on Hawaii showing seismic gaps between these rupture areas. These gaps may be capable of M6.5 to M7.5 earthquakes. No microseismic activity is detected south of the dotted line (from Wyss and Koyanagi, 1992a).

**M.WYSS** next reported on the Kaoiki events, which appear to occur at regular intervals. A block caught between Mauna Loa and Kilauea (Figure 23) is under compression. Two types of focal mechanisms exist: near-horizontal, decollement-like events, and near vertical strike-slip events.

If one plots the 1868 event as the first in a series plotted on a linear graph, events from the region define a line, and they occur with amazing regularity (Figure 24). The interval between events is 10.4±1.5 years (Wyss, 1986). Every other event is

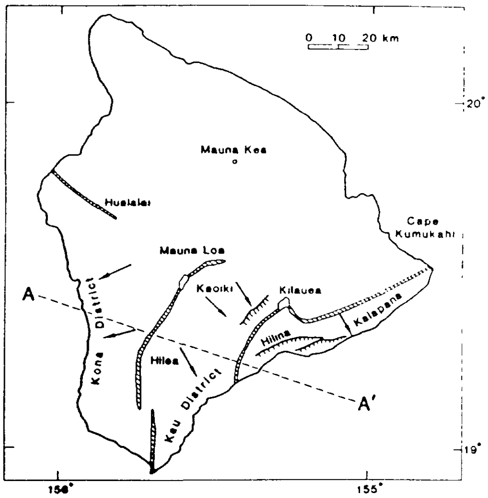



Figure 22. Map of the slip directions (arrows) of the upper crust of Hawaii on the 9 to 14 km deep decollement plane. Because this slip is directed away from Mauna Loa, the pressure due to a magmatic intrusions is probably the major driving force for large earthquakes in Hawaii (after Liang and Wyss, 1991, and Wyss and others, 1992).

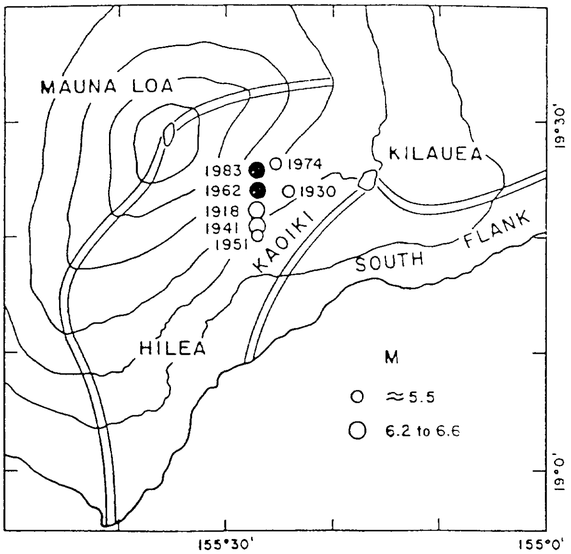



Figure 23. Schematic map of epicenters of the Kaoiki mainshocks with M≥5.5. Instrumentally calculated and macroseismic epicenters are marked by dots and circles, respectively.

larger than M6, with the alternate events in the M5.5 to M6 range. The event in 1983 initiated as a near-vertical strike-slip fault, but its aftershock volume was distributed over a circular area with a diameter of 10 km in which many aftershocks had the decollement-like mechanisms. Aftershocks to the 1973 event only exhibited strike-slip mechanisms. These small events appear to be confined to the upper crust; the larger events involve the decollement also. Based upon this pattern, the next earthquake would be expected in the middle of 1994.

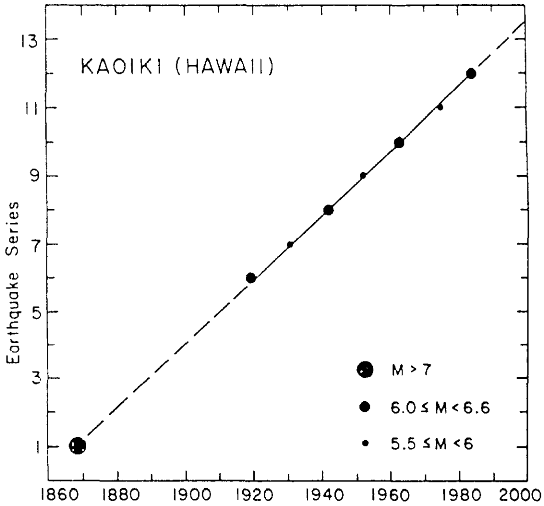



Figure 24. Event number as a function of time for Kaoiki earthquake sequence. Size of dots is proportional to size of event(after Wyss, 1986).

Table 3 -- Conditional probability estimates for earthquakes in the Hawaiian archipelago and the southern parts of the island of Hawaii. [M, magnitude; I, intensity. Parentheses enclose estimates for the southern parts of the island of Hawaii.]

| | 1990-2000 | 1990-2010 | 1990-2040 |
|---|---|---|---|
| M≥6 | 0.84 (0.71) | 0.97 (0.92) | 0.999 (0.998) |
| M≥6.5 | 0.50 (0.39) | 0.75 (0.63) | 0.97 (0.92) |
| M≥7 | 0.17 (0.17) | 0.31 (0.31) | 0.61 (0.61) |
| $I_{max}$≥VII | 0.67 (0.63) | 0.89 (0.86) | 0.997 (0.99) |
| $I_{max}$≥VIII | 0.50 (0.39) | 0.75 (0.63) | 0.97 (0.92) |

A determination of the conditional probability in the whole archipelago and in Hawaii for a M6 event, yields very high numbers (Table 3) indicating that this is a very active volume for its relatively small size. The seismogenic southern part of Hawaii has a diameter of 25 km.

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**J.DIETERICH** presented some ideas and observations that may indicate that a more thorough review of the earthquake hazard in Hawaii is warranted. The presentation continued the discussion, begun by M.Wyss, on the giant detachment fault under the south flank of Kilauea and Mauna Loa volcanoes.

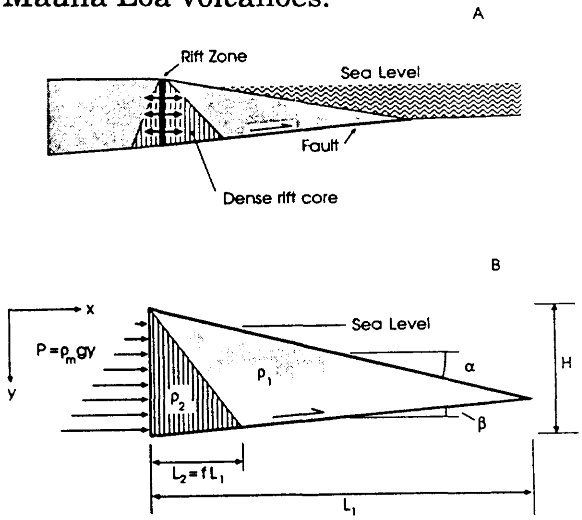


Figure 25. a) Idealized model for rift-flank interactions. b) Definition of parameters. The example shown is for the case where the rift expansion fault coincides with the prevolcano seafloor. If fault dip B does not coincide with seafloor dip, then the fault is within the body of the volcano. For all fault dips the analysis assumes that the fault extends from the base of the active part of the rift zone. From Dieterich, 1988.

A highly simplified model of the detachment fault and volcanic rift zones shows a wedge of volcanic material apparently being driven uphill along the fault (Figure 25). The model assumes that repeated injection of dikes into the rift zone drives the motion of the wedge. The force available to move the wedge is supplied by gravity and is the pressure of a standing column of magma in the volcanic rift zone. One can derive a simple model for the motion of the

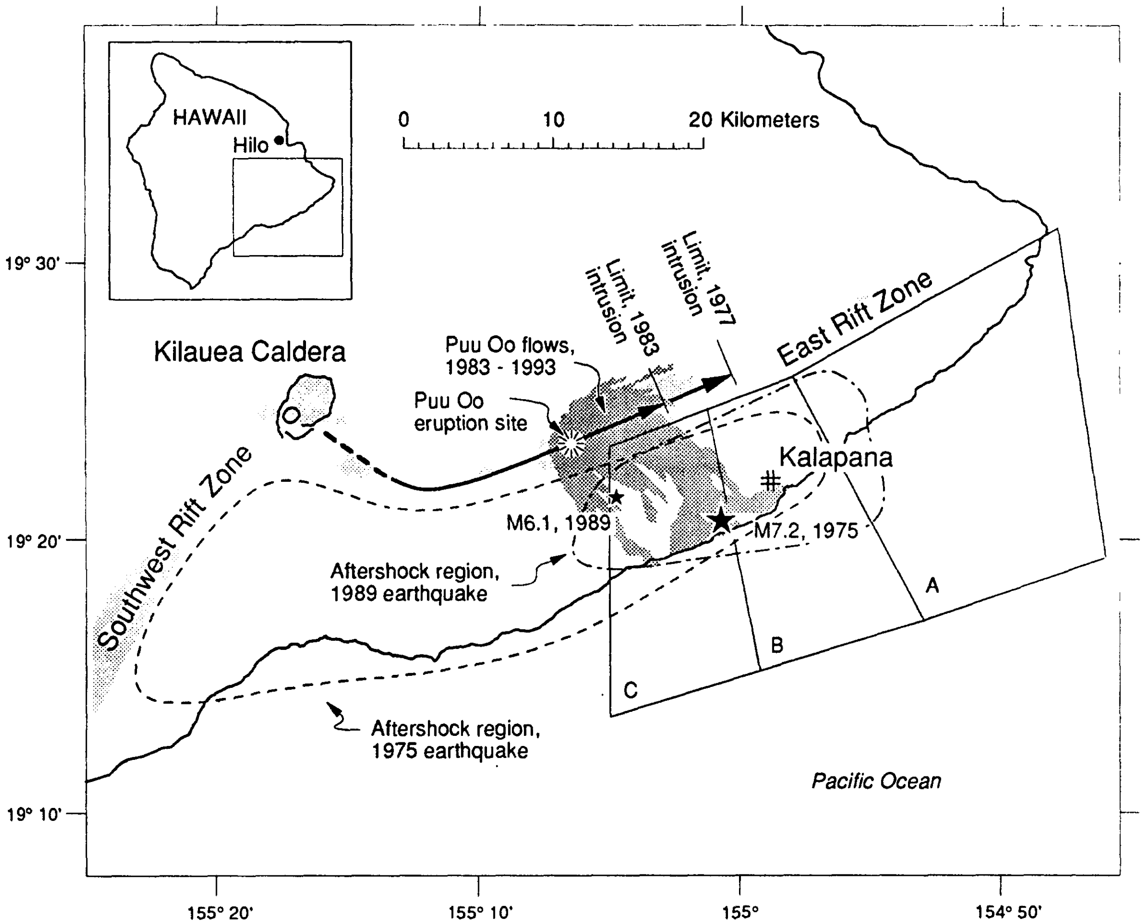


**Figure 26. South flank of Kilauea volcano showing aftershock regions of the two Kalapana mainshocks and polygons for Figure 27 earthquake plots.**

wedge that accounts for the weight of the wedge, dip of the fault, density of the magma in the rift zone, and friction on the fault (Dieterich, 1988). A coefficient of friction as high as 0.39 permits the wedge to slide assuming a normal hydrostatic pore fluid pressure in the fault zone. This coefficient of friction is somewhat low for normal crystalline rocks, but is reasonable if slip occurs within the clayey sea-floor sediments that underlay the volcano.

Based on the recent intrusion rates and exposures in eroded rift zones on the other islands, the volumes of the repetitively intruded dike material that constitute the rift zones may make up some 50 to 75 percent of the growth of the volcano. The long-term rate of rift opening and flank slip is estimated to be about 5-10 cm per year (Dieterich, 1988). However, modeling of recent geodetic strain data yield slip rates on the basal fault of 25cm/yr averaged over the interval 1983-1991 (Delaney and others, 1993) and over the interval 1990-1992 (Information supplied by P.Segall and S.Owen). These extraordinary rates of slip are about 10 times the rate of the San Andreas fault and suggest the possibility of very short return times for major earthquakes.

These observations of high deformation rates provide some background to an unusual pattern of seismic activity that P.Okubo and **J.DIETERICH** have discovered. Most portions of the south flank of Kilauea are dominated by the aftershocks to the 1975 M7.2 earthquake and show nothing particularly unusual (Figures 26 and 27). However, one region, near the epicenters of the 1975 earthquake and the 1989 M6.1 earthquake, shows a pattern of repeated seismic quiescence (Figure 27b). A pronounced period of quiescence preceded the 1975 earthquake and was recognized and reported by Wyss and others (1981). Quiescence also preceded the 1989 earthquake, and it appears that we are currently in another period of quiescence that began in 1990.

To our knowledge, this is the only place where a pattern of repeating quiescence has been observed. We don't know what the current quiescence means, but obviously it may represent a precursor to another large event. However, several other possibilities warrant attention. These include 1) quiescent episodes are spurious and represent man-made catalog problems; 2) the last two quiescent episodes represent the return to a new very low level of seismic activity following the end of the aftershock sequence to the 1975 earthquake; and 3) magmatic activity in the nearby rift zone has reduced the stress in this part of the wedge. The latter possibility arises because of the pronounced sensitivity of this area to rift zone intrusions in 1977 and 1983 (see Figures 26 and 27).

**R.WESSON** stated that these very interesting presentations refreshed some things that we knew about Hawaii. The forward movement in interpretations is impressive. The suggestions for near-term M6 events are interesting, but such events are not uncommon in this seismic region. This contrasts with the Pacific Northwest and New England, where such events are not common.

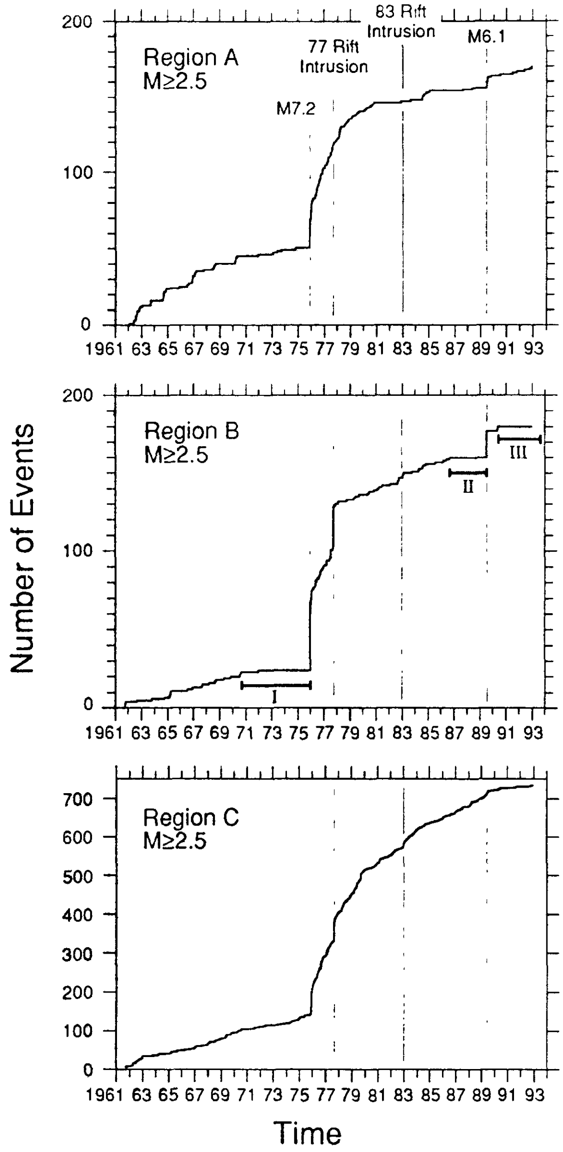


Figure 27. Cumulative seismicity plots, M≥2.5, from 1961 through 1992, for polygons A, B, and C, Figure 26.

Perhaps we could forward a statement to the Director conveying the fact that NEPEC is impressed with the continuing hazard in Hawaii, is impressed by the recent advances in understanding the processes, and feels that the situation bears a more thorough look from NEPEC as well as from the NEHRP.

**R.WESSON** proposed that it might be interesting to get individuals working on the quiescence issues together with those working on accelerating deformation issues to address the same data set to see what they find collectively.

With regards to the Shumagin gap issue, **T.McEVILLY** indicated that all NEPEC can do is inform the Director that the gap remains and there has been recent activity that pertains to it. It warrants watching, although there is a difficulty funding such activity.

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**T.McEVILLY** pointed out that we did not spend time addressing M8, but that a statement from J.Healy (Appendix Q) indicates that theexperiment continues. To date it is not working very well, having identified one in three events.

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**R.WESSON** pointed out that it was possible that this might be the last formal meeting of NEPEC. In the event that was the case, **R.WESSON** thanked the group on behalf of the Director for their individual and collective service. He noted that the T.McEvilly era has been particularly productive. In light of all their other responsibilities, **R.WESSON** personally thanked everyone for their attention and contributions to NEPEC.

**T.McEVILLY** adjourned the meeting at 3:35 pm

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## APPENDICES

| | |
|---|---|
| **Appendix A** | Agenda for the June 4 and 4, 1993, NEPEC meeting at Herndon, Virginia. |
| **Appendix B** | Representative citation presented to NEPEC members by Director Peck on behalf of Interior Secretary Lujan. |
| **Appendix C** | Illustrations presented to NEPEC by B.Hager. |
| **Appendix D** | Charge to 1993 NEPEC working group on Parkfield. |
| **Appendix E** | Document presented by B.Hager: an advance draft of the report of the 1993 NEPEC working group on Parkfield. |
| **Appendix F** | Document presented to NEPEC by K.Aki: draft executive summary of Phase II report with illustrations. |
| **Appendix G** | Illustrations presented to NEPEC by J.Ebel. |
| **Appendix H** | Statement presented to NEPEC by J.Ebel: consensus document produced at April 28, 1993, meeting. |
| **Appendix I** | May 26, 1993, statement by I.Madin and M.Mabey concerning predictions of earthquakes in Oregon. |
| **Appendix J** | April 2, 1993, news release citing lack of evidence for Oregon earthquake prediction. |
| **Appendix K** | Charge to the NEPEC working group on the Cascadia subduction zone. |
| **Appendix L** | Advance draft of the report of the NEPEC working group on the Cascadia subduction zone. |
| **Appendix M** | Illustrations presented to NEPEC by B.Atwater. |
| **Appendix N** | Document sent to NEPEC by S.Obermeier and S.Dickenson. |
| **Appendix O** | Illustrations used by M.Wyss during presentation on Shumagin event. |
| **Appendix P** | Illustrations used by M.Wyss during presentation on seismicity in Hawaii. |
| **Appendix Q** | Document sent to NEPEC by J.Healy in May, 1993: brief update on M8 earthquake prediction algorithm. |

**Appendix A**

Agenda for the June 3 and 4, 1993, NEPEC meeting at Herndon, Virginia.

National Earthquake Prediction Evaluation Council
Meeting of June 3 & 4, 1993
Herndon, Virginia

Thursday, June 3

| | | |
|---|---|---|
| 9:00 am | Introductory remarks | |
| | Tom McEvilly & Rob Wesson | |
| | Miscellaneous business | |
| | Portland minutes | Virgil Frizzell |
| | Reston agenda | Tom McEvilly |
| | USGS issues | Numerous |
| | Future of NEPEC | Wesson |
| | Portland 'prediction' | Weaver |
| | M8 | note from Healy |
| 11:00 am | report from ad hoc working group on Parkfield prediction experiment (status, technical issues, mode of publication, etc.) | |
| | Brad Hager, Joanne Stock, Ray Weldon | |
| 12:30 pm | Lunch | |
| 1:30 pm | report from ad hoc working group on Landers -- Phase II (status, technical issues, review process, authorship, mode of publication, etc.) | |
| | Kei Aki, Jim Dieterich, Ray Weldon | |
| 5:30 pm | Dinner | |
| 7:00 pm | East Coast issues | |
| | John Ebel | |
| 9:00 pm | adjourn | |

Friday, June 4

| | | |
|---|---|---|
| 8:30 am | report from ad hoc working group on Cascadia subduction zone (status, technical issues, review process, authorship, mode of publication, NEPEC public statement, etc.) | |
| | Craig Weaver, Brian Atwater | |
| 12:00 | briefing on recent events in the Shumigan gap | |
| | Max Wyss | |
| 12:30 pm | Lunch | |
| 1:30 pm | Hawaii (background, technical issues, predictions) | |
| | David Clague, Jim Dieterich, Max Wyss | |
| 3:00 pm | New business | |
| 3:15 pm | Executive session | |
| 3:30 pm | Adjourn | |

## Appendix B

Representative citation presented to NEPEC members by Director Peck on behalf of Interior Secretary Lujan.

# U.S. DEPARTMENT OF THE INTERIOR

OFFICE OF THE SECRETARY

*This certificate is awarded to*

## *Dr. James H. Dieterich*

*In recognition of your Significant Contribution to the Bush Administration and the U.S. Department of the Interior by serving on the National Earthquake Prediction Evaluation Council*

*January 15, 1993*
*Date*

*Secretary of the Interior*

**Appendix C**

Illustrations presented to NEPEC by B.Hager

# Earthquake Research at Parkfield, 1993 and Beyond —

1993

## Report of the NEPEC Parkfield Earthquake Prediction Experiment Review Committee

(or

Report of the Working Group to Evaluate the Parkfield Earthquake Prediction Experiment)


Bradford H. Hager (chair),
Massachusetts Institute of Technology

C. Allin Cornell,
Stanford University

William M. Medigovich,
Federal Emergency Management Agency

Kiyoo Mogi,
Nihon University, Japan

Robert M. Smith,
University of Utah

L. Thomas Tobin,
California Seismic Safety Commission

Jim Brika

Joann Stock,
California Institute of Technology

Ray Weldon,
University of Oregon

## Parkfield Earthquake Prediction Experiment—

## Historical Perspective

1857, 1881, 1901, 1922, 1934, 1966
- "Characteristic" earthquakes (?)

1983 ±
- USGS - "Whither 2-color geodimeter?"
  Parkfield or San Juan Bautista?

1984
- Bakun & Lindh prediction — endorsed by NEPEC

1985
- Bakun & Lindh *Science* article
  1988 ± 5 years, 95% confidence by end of '92
- State of California support begins
  public policy responsibility defined

1989
- Loma Prieta earthquake

1992
- NEPEC charters Working Group to Evaluate Expt
- "A"-level alert in October

SCIENCE

16 August 1985, Volume 229, Number 4714

# The Parkfield, California, Earthquake Prediction Experiment

W. H. Bakun and A. G. Lindh

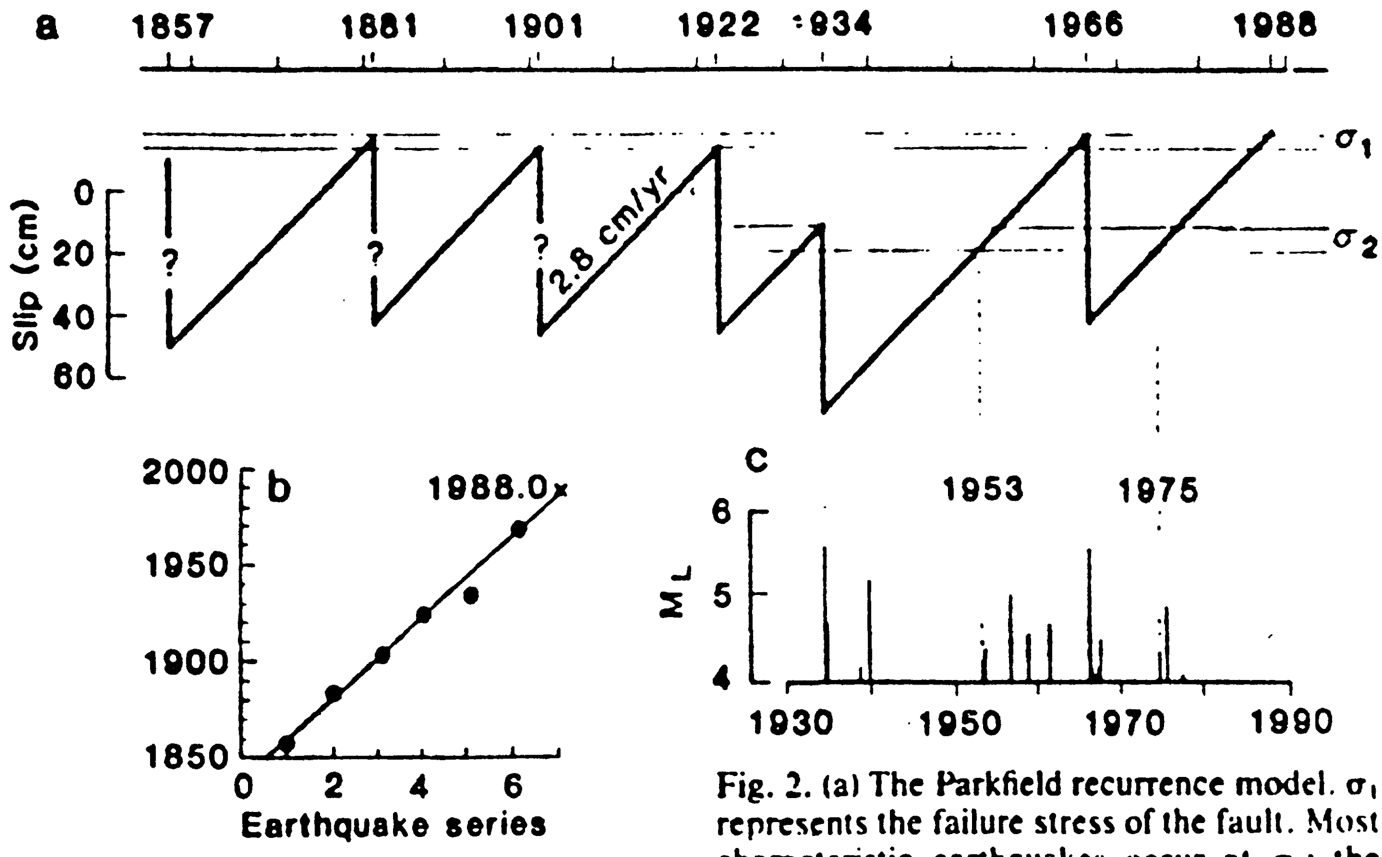


Fig. 2. (a) The Parkfield recurrence model. $\sigma_1$ represents the failure stress of the fault. Most characteristic earthquakes occur at $\sigma_1$: the 1934 shock occurred at $\sigma_2$. A constant loading rate of 2.8 cm per year and a coseismic slip of 60 m for the Parkfield earthquake sequences in 1881, 1901, 1922, 1934, and 1966 are assumed (56). (b) Series of earthquake sequences at Parkfield since 1850 [after (5)]. The line represents the linear regression of the time of the sequence obtained without the 1934 sequence. The anticipated time of the seventh (that is, the next) Parkfield sequence for the regression is January 1988. (c) Shocks of $M_L$ greater than 4 since 1930 have tended to occur when the stress exceeds $\sigma_2$.

States and from around the world. Parkfield research is supported by funds and grants from the USGS, the State of California, the National Science Foundation, the Electric Power Research Institute, and from several other private, government, and international sources.

### The Parkfield Working Group Composition:

NEPEC has been asked by the USGS Director to create a Working Group (WG) to advise him on the future course of the Parkfield experiment, beginning with WG participation in the program review Workshop to be held June 28 - July 01, 1992 in Santa Cruz, CA. Recommended membership in the WG includes B. Hager (Chair), R.B. Smith (or Gilbert), Mogi, R. McGuire (or Cornell), Medigovich (or Grew), plus NEPEC members J. Stock and R. Weldon. Members are selected in part on the basis of their having had no substantial prior connection with the Parkfield experiment.

### Questions for the Working Group:

Three classes of questions can be posed and answered in the Group's analysis of the program at Parkfield:

1. What is the current assessment of the Parkfield earthquake prediction? In 1985 NEPEC endorsed a prediction that an earthquake of about magnitude 6 had a 95% probability of occurring by 1993. In light of current knowledge, is it still considered highly likely that an earthquake will occur in the short term? If the earthquake has not occurred by the end of 1992, what does that tell us about the original prediction? Was the basis for the prediction in error?

2. What have we learned during the experiment? The experiment has had both scientific and response community aspects. What have been the principal benefits that have come from both of these aspects of the experiment?

3. Where should the experiment go in the future? In light of the reassessment of the likelihood of a Parkfield earthquake, how should we modify the scientific experiments taking place in Parkfield? Should the real-time surveillance and monitoring be continued, and if so, what changes should be made in the monitoring program? What modifications should be made to research priorities at Parkfield? What research efforts should receive the highest priority? Should there be any modification in the agreements that govern the interaction between the USGS and the State of California with regard to hazard warnings for an earthquake at Parkfield?

### Working Group Schedule:

It is hoped that the Working Group can conduct the bulk of its business in conjunction with the Workshop, with possibly one further meeting to formulate its report to NEPEC for the Director by late summer. Recommendations on this schedule will allow timely restructuring the research program at Parkfield.

## Parkfield Earthquake Prediction Experiment—

## Questions

*1) What is the current assessment of the prediction?*

*a) Is it likely that an M ~ 6 earthquake will occur in the short term?*

- most likely locality identified to "trap" a moderate sized earthquake.
- regularity, short average recurrence time
- loading is unusually simple,
- estimates of the probability of occurrence clustered around ~ 10%/year.

*b) Was the basis for the prediction in error?*

- based on a rather specific set of assumptions that allowed the 1934 event to be ignored
- now known to be too simple to apply to Parkfield.
- should have included an assessment of the probability that the model assumptions were correct.

**Parkfield Earthquake Prediction Experiment—**

**Questions**

*2) What have we learned, both from the scientific and response community aspects? What have been the principal benefits?*

- major scientific impact of the experiment will not occur until the next Parkfield earthquake is "trapped"
- technology transfer realized
- important benefits reaped from the real-time exercise created by the response community interacting with the scientific community.
- Parkfield has been an ideal location to begin this process.
- a public policy success.
- "A"-level alert in October, 1992, provided a realistic "fire drill"

**Parkfield Earthquake Prediction Experiment—**

**Questions**

*3) Where should the experiment go?*

- *What modifications should be made to scientific monitoring?*
- *What research efforts should receive highest priority?*
- *Should there be any modification in the agreements that govern the interaction between the USGS and California with regard to hazard warnings?*

- USGS should view the Experiment as a long-term commitment.
- committed, long-term Project Scientist
- long-term plan for replacing or upgrading equipment
- periodic peer review
- ensure access to monitoring sites
- commitment to continue the public policy aspects
- reassess warning thresholds

## Appendix D

Charge to 1993 NEPEC working group on Parkfield.

# EARTHQUAKE RESEARCH AT PARKFIELD - 1993 AND BEYOND

**Information and Charge to the NEPEC Working Group for the Parkfield Prediction Experiment**

## Introduction

On April 4, 1985, the State of California was advised by the U. S. Geological Survey (USGS) of the expectation that an earthquake of about magnitude 6 is likely to occur in the next several years on the San Andreas fault near the small community of Parkfield, California. The purpose of this notification was not to issue a hazard warning but to provide State and local officials with information that would be of use in hazard mitigation and emergency response planning. As a consequence of this announcement, the first coordinated, public attempt in the United States was begun to organize an operational system to issue a short-term warning of a potentially damaging earthquake. This short-term prediction experiment also served as a catalyst for a larger, more comprehensive experiment designed to capitalize on the anticipated occurrence of the earthquake by providing a natural laboratory to study the entire earthquake process.

According to statistical calculations made at that time, the earthquake should occur, with 95 percent probability, in the 1985-1993 time interval. Now that the end of this interval is approaching, the Director of the USGS has requested guidance from the scientific community regarding options for the future course of earthquake hazards reduction research at Parkfield.

## Background

The Parkfield segment of the San Andreas fault is widely recognized as a world-class locality for the study of strike-slip faulting and crustal earthquakes. Several factors contribute to the importance of Parkfield, including its relatively simple tectonic setting, high rate of strain accumulation, and long history of repetitive failure in moderate-magnitude earthquakes. The 1966 Parkfield earthquake, M 6, marked a watershed in our understanding of the earthquake source and led to the initiation of long-term observational studies of this part of the San Andreas fault, beginning less than 6 months after the occurrence of the event.

Formal studies directed toward the prediction of the next Parkfield earthquake began in 1978 with the creation of a small project headed by Allan Lindh of the USGS. This work was carried out under the National Earthquake Hazards Reduction Act of 1977, which called for "the implementation in all areas of high or moderate seismic risk, a system (including personnel and procedures) for predicting damaging earthquakes and for identifying, evaluating, and accurately characterizing seismic hazards." By 1979, William Bakun of the USGS and Thomas McEvilly of the University of California, Berkeley, proposed that earlier M 6 earthquakes at Parkfield in 1901, 1922, and 1934 were remarkably similar to the 1966 earthquake. In 1984 they published a recurrence model for Parkfield earthquakes and suggested an average interval of 22 years between M 6 earthquakes along this segment of the San Andreas fault.

In 1985, Bakun and Lindh published a paper forecasting that the next Parkfield earthquake should occur before 1993, based upon their analysis of intervals between earlier Parkfield earthquakes. In the parlance of earthquake prediction, their prediction can be classified as a long-term forecast, and it depended solely upon the statistics of the intervals between large earthquakes in the sequence. This hypothesis was presented to the National Earthquake Prediction Evaluation Council (NEPEC), an advisory body to the Director of the USGS, in November 1984, and NEPEC endorsed the general aspects of the prediction. In April 1985, the Director of the USGS formally advised the State of California of the prediction and obligated the USGS to attempt to provide a short-term warning of the anticipated earthquake.

Real-time monitoring of the Parkfield region, made possible with funding from a joint State-Federal funding agreement, provides automatic analysis of seismicity, strain in boreholes, movements of the ground water table, creep along the fault, and other geophysical parameters. A formal set of rules governs the interpretation of specific observational conditions as probabilistic estimates that the next M 6 earthquake will occur within 3 days. There rules were reviewed and endorsed by the California Earthquake Prediction Evaluation Council and by NEPEC. The State of California has the responsibility for issuing a public warning, based on the advice and recommendation of the USGS. The State and all of the counties in the affected region also have formal response plans tied to the USGS rules.

The earthquake prediction experiment at Parkfield also serves a larger and potentially more significant purpose. The perceived high likelihood of an M 6 earthquake at Parkfield makes it one of the best sites in the world to study the earthquake process. A major investment has been made at Parkfield to study the earthquake preparation process, to measure the dynamics of rupture in the next event, and to quantify the response of varying surficial geologic materials and engineered structures to the anticipated strong ground motion. New experiments continue to be installed as opportunity permits, including the recent deployment of ultra-low frequency radio receivers following the 1989 Loma Prieta, California, earthquake. Other opportunities for capitalizing on the Parkfield experiment, such as those outlined in a 1986 National Research Council report, have yet to be undertaken.

## Current Effort at Parkfield

Work now in progress at Parkfield can be classified approximately into three different types of activities: monitoring in support of the effort to issue a short-term prediction of the next M 6 event; basic research directed toward understanding the physics of earthquakes; and applied engineering experiments sited a Parkfield and designed to capitalize on the event when it occurs.

Monitoring of the Parkfield region is supported by a network of autonomous instruments equipped with real-time telemetry to the USGS offices in Menlo Park, California. Data are automatically analyzed, as they are received, by computers in Menlo Park that issue alert messages to project scientists. These same computers and personnel also perform many of the same functions for northern and central California using other instrumentation networks.

Basic and applied research at Parkfield spans a wide range of disciplines and involves researchers from government, universities, and the private sector, both in the United

States and from around the world. Parkfield research is supported by funds and grants from the USGS, the State of California, the National Science Foundation, the Electric Power Research Institute, and from several other private, government, and international sources.

**The Parkfield Working Group Composition:**

NEPEC has been asked by the USGS Director to create a Working Group (WG) to advise him on the future course of the Parkfield experiment, beginning with WG participation in the program review Workshop to be held June 28 - July 01, 1992 in Santa Cruz, CA. Recommended membership in the WG includes B. Hager (Chair), R.B. Smith (or Gilbert), Mogi, R. McGuire (or Cornell), Medigovich (or Grew), plus NEPEC members J. Stock and R. Weldon. Members are selected in part on the basis of their having had no substantial prior connection with the Parkfield experiment.

**Questions for the Working Group:**

Three classes of questions can be posed and answered in the Group's analysis of the program at Parkfield:

1. What is the current assessment of the Parkfield earthquake prediction? In 1985 NEPEC endorsed a prediction that an earthquake of about magnitude 6 had a 95% probability of occurring by 1993. In light of current knowledge, is it still considered highly likely that an earthquake will occur in the short term? If the earthquake has not occurred by the end of 1992, what does that tell us about the original prediction? Was the basis for the prediction in error?

2. What have we learned during the experiment? The experiment has had both scientific and response community aspects. What have been the principal benefits that have come from both of these aspects of the experiment?

3. Where should the experiment go in the future? In light of the reassessment of the likelihood of a Parkfield earthquake, how should we modify the scientific experiments taking place in Parkfield? Should the real-time surveillance and monitoring be continued, and if so, what changes should be made in the monitoring program? What modifications should be made to research priorities at Parkfield? What research efforts should receive the highest priority? Should there be any modification in the agreements that govern the interaction between the USGS and the State of California with regard to hazard warnings for an earthquake at Parkfield?

**Working Group Schedule:**

It is hoped that the Working Group can conduct the bulk of its business in conjunction with the Workshop, with possibly one further meeting to formulate its report to NEPEC for the Director by late summer. Recommendations on this schedule will allow timely restructuring the research program at Parkfield.

## Appendix E

Document presented by B.Hager: an advance draft of the report of the 1993 NEPEC working group on Parkfield.

# Earthquake Research at Parkfield, 1993 and Beyond — Report of the NEPEC Working Group to Evaluate the Parkfield Earthquake Prediction Experiment

## Summary

During the past century, earthquakes of M ~ 6 have occurred with remarkable regularity on the San Andreas fault at Parkfield, California. Events occurred in 1857, 1881, 1901, 1922, 1934, and 1966. At least two of these events were preceded by large foreshocks and there is evidence for precursory creep of the shallow segment of the fault prior to the 1966 event. In 1984 - 1985, scientists developed and published a prediction, based on a model of "characteristic" earthquakes, that the next M ~ 6 Parkfield event was expected in a time window centered on 1988, with 95% probability that the earthquake would occur by the end of 1992 [*Bakun and Lindh*, 1985].

Shortly after the publication of this prediction, with endorsement by NEPEC, the United States Geological Survey (USGS) initiated the Parkfield Earthquake Prediction Experiment (the Experiment). With additional support from the state of California, the Experiment took on a public services aspect, as well as a geophysical aspect.

By late summer, 1992, the predicted event had not yet occurred. NEPEC chartered a Working Group to evaluate the Parkfield Earthquake Prediction Experiment. This group was asked a series of questions which are summarized below, along with the responses of this Working Group.

*1) What is the current assessment of the prediction?*

*a) Is it still considered likely that an M ~ 6 earthquake will occur in the short term?*

Parkfield is still considered to be the most likely locality identified to "trap" a moderate sized earthquake. Empirically, no other location has demonstrated a sequence of earthquakes with as much regularity and as short an average recurrence time as the Parkfield sequence. The loading of the Parkfield segment of the San Andreas fault is unusually simple, with the creeping segment to the north leading to a continuous accumulation of strain on the locked segment near Parkfield. At this time, about as much strain has accumulated as was released in the previous event [*Segall and Harris*, 1986].

Estimates of the probability of the Parkfield earthquake occurring in the near future have been generated by a number of scientists. These estimates are based on a number of assumptions about the statistical behavior of faults, but all update the probability estimate to include the information that, as of the time of the estimate, the event has not yet occurred. Estimates of the probability of occurrence are clustered around a value of approximately 10%/year.

*b) If the event does not occur by the end of 1992, what does that indicate about the original prediction? Was the basis for the prediction in error?*

The original prediction was based on a rather specific set of assumptions. These include that the loading rate is constant, that failure of the same patch of the fault occurs at or below a threshold stress level, and that the stress drop is identical for each event

[*Bakun and Lindh*, 1985]. It was the adoption of this specific model that allowed the 1934 event to be ignored when evaluating the expected time of the next "characteristic earthquake," leading to a small uncertainty in the expected time of the event. This model is now known to be too simple to apply to Parkfield. In retrospect, the original prediction should have included an assessment of the probability that the model assumptions were correct, in addition to considering the uncertainties related to data noise in fitting the assumed model.

But failure of this specific prediction does not negate the consensus of the Working Group that Parkfield is still the most likely place in the United States to capture an earthquake and that there is a relatively high probability that this event will occur on a time scale of a few years. In addition, Parkfield is unique because the location of the likely nucleation point can be estimated. And there is a long baseline of measurements already established there.

*2) What have we learned during the experiment, both from the scientific and response community aspects? What have been the principal benefits that have come from both of these aspects of the experiment?*

While the major scientific impact of the experiment will not occur until the next Parkfield earthquake is "trapped" in the dense web of instrumentation operating there, there have already been important benefits reaped from the real-time exercise created by the response community interacting with the scientific community. Because of its low population density, Parkfield has been an ideal location to begin this process. The Experiment has been a public policy success, with positive implications for response and mobilization to possible future earthquake alerts elsewhere. Cooperation at Parkfield has produced the *California Short-Term Earthquake Prediction Response Plan*. The "A"-level alert in October, 1992, provided a realistic "fire drill" to test the implementation of this plan. The press has learned how to portray alert levels and associated probabilities.

Several scientific results are also notable. For example, geodetic data have shown resolvable differences between the 1966 and 1934 events; Parkfield events are similar, yet still show differences large enough to violate the specific assumptions of the model on which the original prediction was based. In addition, the fault zone and asperities have been imaged at unprecedented resolution, with the identification of a low-velocity zone, perhaps related to high fluid pressure.

There has also been substantial technology transfer resulting from the Experiment, including increased expertise in siting of borehole instruments, experience with real-time seismic networks, and improved instrument design. These advances have made the installation of other instrumentation, such as that monitoring the Hayward fault, more cost effective.

*3) Where should the experiment go in the future? What modifications should be made to scientific monitoring? What research efforts should receive highest priority? Should there be any modification in the agreements that govern the interaction between the USGS and the State of California with regard to hazard warnings?*

The science of understanding the earthquake source is limited by the dearth of observations throughout the earthquake cycle. Parkfield is the most likely place yet identified to capture a moderate earthquake in a densely instrumented region and the best locale identified to answer a number of important scientific questions about the seismic source. Because the Parkfield segment of the San Andreas fault is loaded by the creeping section at one end, it provides a setting intermediate in scale between the simplicity of the laboratory and the complexity of most other faults. Substantial resources have been invested in setting up the Experiment and the marginal costs associated with continued operation of the experiment are minimal. Although the estimated annual probability of about 10%/year is the highest proposed for any specific location, and high enough to make the area scientifically and societally interesting, it is not high enough to ensure that an earthquake will occur on a time scale of a few years. Thus the USGS should view the Experiment as a long-term commitment.

In this context, the Working Group recommends that USGS continue the Parkfield Experiment and assign it a high priority. This includes having a committed, long-term Project Scientist with sufficient resources available to deal with the scientific, response planning, and public relations priorities of the Experiment. These aspects of the Experiment are likely to be in a state of high activity simultaneously when alerts are called.

A long-term plan is required for replacing failed or obsolete equipment, in particular, strain meters. This long-term plan should also include periodic peer review of the Experiment, with possible redirection and reordering of priorities.

In recognition of the long-term aspects of monitoring the preparation zone throughout the earthquake cycle, and the problems due to transients and costs associated with new installations, the USGS needs to ensure access to monitoring sites. The USGS should attempt to acquire control of land where instruments are located to avoid disruption when landowners change.

There should also be a commitment to continue the public policy aspects of the Parkfield Experiment through the earthquake cycle. There is still much to be learned about public response to perceived false alarms, perception of risk assessment, and warning thresholds.

*Parkfield Earthquake Prediction Experiment Working Group:*

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## 1.0 Introduction

Before the mid-1980's, the United States' earthquake prediction program was in a reconnaissance mode, with monitoring programs broadly distributed across regions of the nation that had high seismic activity. In 1984, scientists at the USGS began to plan a spatially focused earthquake experiment, with a primary goal of evaluating possible precursory phenomena. The issue that had to be addressed at that time was where to use the limited resources available with the best chance for evaluating these phenomena. The immediate question that had to be answered was where to deploy the recently developed dual-frequency laser geodimeter. Sites under consideration included Parkfield and San Juan Bautista.

In 1985, scientists at the USGS published a prediction that the next Parkfield earthquake was expected in a time window centered on 1988, with a 95% probability that the earthquake would occur by the end of 1992 [*Bakun and Lindh*, 1985]. This prediction was based on a specific model of the "characteristic" Parkfield earthquake, discussed in more detail below. NEPEC reviewed the prediction favorably and the USGS decided to locate their focused experiment in Parkfield.

In April, 1985, the Director of the USGS sent a letter to the Governor of California informing him about the possibility of an earthquake at Parkfield. California and USGS each contributed $1M to begin instrumentation of the Parkfield area. Over 20 observational networks have been installed, including seismometers, creep meters, borehole strain meters, the two-color laser geodimeter, water wells, and magnetometers. Five of these networks are monitored in real time.

There were two scientific goals for the Parkfield Earthquake Prediction Experiment: 1) To record the geophysical details before and after the expected earthquake; and 2) To issue a short-term prediction. In addition, with the involvement of the State of California, the Experiment took on an important public policy aspect, serving as a test bed for communication between earthquake scientists and public officials. The rural nature of the community made it an ideal location to carry out the Experiment.

A formal series of alert levels has been set up, triggered by phenomena such as fault creep or earthquake activity in the "preparation zone." Alert levels range from D (triggered about 100 times since June, 1985) to A (triggered once, in October, 1992, by a $M = 4.5$ earthquake; this alert corresponds to an expected probability of 37% of the forecast event occurring within 72 hours).

## 1.1 Charge to the Working Group

In mid-1992, with the pending expiration of the prediction window, NEPEC chartered a working group to evaluate the original prediction. The charge to the working group was to evaluate the Parkfield Prediction Experiment in light of the following questions:

*1) What is the current assessment of the prediction?*

- *a) Is it still considered likely that an earthquake will occur in the short term?*
- *b) If the event does not occur by the end of 1992, what does that indicate about the original prediction? Was the basis for the prediction in error?*

*2) What have we learned during the experiment, both from the scientific and response community aspects? What have been the principal benefits that have come from both of these aspects of the experiment?*

*3) Where should the experiment go in the future? What modifications should be made to scientific monitoring? What research efforts should receive highest priority? Should there be any modification in the agreements that govern the interaction between the USGS and the State of California with regard to hazard warnings?*

## 2.0 Assessment of the Prediction

The Working Group addressed two main questions in assessing the Parkfield Prediction. The more important one is whether Parkfield is still viewed as being the most likely place to trap a moderate earthquake. The secondary question is whether the specific prediction that expired at the end of 1992 was correct.

Parkfield is thought to be more likely to experience a moderate earthquake than any other place in the United States. During the past century, earthquakes of $M \sim 6$ have occurred there with remarkable regularity. Events occurred in 1857, 1881, 1901, 1922, 1934, and 1966. The time between earthquakes ranged from 12 to 32 years, with an average of 22 years. The relatively short time between events may result from a near-uniform rate of loading of the Parkfield segment by slip on the creeping segment of the fault just to the northwest.

With the demonstration that for the last two $M = 6$ Parkfield earthquakes the total (co- and post-seismic) surface displacements determined from geodesy are resolvably different [*Segall and Du*, 1993], it is now understood that the assumptions of the specific model on which the prediction was based do not hold. There is no consensus on the correct physical model to be used for evaluating the probability of the Parkfield event occurring within any given time window. The computation of statistical estimates of occurrence probabilities has become almost a cottage industry, with a variety of models produced using different assumptions about the nature of the appropriate statistics. Most recent statistical models update the estimated probability to include the information that the earthquake has not yet happened as of the time of the estimate. While the details vary, estimates of the probability of occurrence of the event cluster around a value of about 10%/year [e.g., *Kagan and Jackson*, 1991; *Savage*, 1991]. Such a probability is high enough that continued monitoring of the Parkfield segment is important.

In addition to Parkfield being the most likely place identified to trap a moderate earthquake, Parkfield has other features that make it a good place for a prediction experiment. First, a variety of events that may have been precursors occurred before previous $M \sim 6$ earthquakes there. At least in 1934 and 1966, $M > 5$ foreshocks occurred. In 1966, cracks were observed in the ground and a pipe broke days before the event. The epicenters of the 1922, 1934, and 1966 events are all located close to each other, providing a clear target for siting instrumentation to capture the next event.

From a social and economic perspective, Parkfield is an ideal location for a prediction experiment. The area is sparsely populated and the residents are well educated and relaxed about the occurrence of moderate earthquakes.

The Experiment has made important contributions both to geophysical science and to public policy. But advances in our understanding suggest that the model on which the original prediction was based is too simplistic. The original model of constant loading rate, uniform moment release, and constant stress drop, controversial when it was first proposed, has now been shown not to apply to Parkfield. In retrospect, an estimate of the reliability of this hypothesis should have been included in the original calculation, and would have broadened the window of predicted recurrence. The relatively narrow window that was stated in the original prediction has led to expectations that the experiment would be over relatively quickly, leading to the misconception that the experiment has now somehow "failed" because the narrow time window has closed.

## 3.0 What We Have Learned from the Experiment

## 3.1 Lessons for the Scientific Community

### *3.1.1 Improvements in Monitoring*

The Parkfield project was the first concerted effort in the US to implement real-time monitoring of a variety of possible precursory signals and geophysical information in a single earthquake source region. This forced the development of a coherent plan for monitoring instrumentation: alignment arrays, a two-color laser geodimeter, creep meters, ground-water-level transducers, tilt meters, surface and borehole seismometers, volumetric strain meters, tensor strain meters, leveling lines, borehole temperature measurements, an earth resistivity monitoring network, Global Positioning System (GPS) geodetic measurements, magnetic field measurements, ULF electromagnetic measurements, ground water radon measurements, and soil hydrogen measurements. In addition, arrays to measure strong ground motion, liquefaction, coseismic slip, and pipeline response were installed. The result has been the most thoroughly instrumented earthquake source region in the world. The technology required to record and download these data (often in real time) has been a challenge to implement, and during the Parkfield experiment, the knowledge of how to run such a system has improved.

The monitoring done in Parkfield since the start of the earthquake experiment has yielded a very detailed baseline of behavior of all of these phenomena, which will be invaluable for comparison with post-earthquake observations. The baseline observations have permitted a good understanding of the transient results of rainfall events, slow strain events, and tides. In addition, they have provided observations regarding the response of the Parkfield region to smaller earthquakes nearby, and to larger earthquakes at some distance from the region (e.g. Kettleman Hills; Loma Prieta; Coalinga; and Landers). These observations are extremely relevant to the design of monitoring installations and establishment of alert levels in other regions; they will also contribute to the revision of alert levels for Parkfield.

### *3.1.2 Scientific Results to Date*

The scientific experiments at Parkfield fall into three classes: those designed to monitor possible earthquake precursors, those designed to monitor the behavior of the region around the anticipated rupture nucleation point, and those designed to study the effects of earthquake-induced ground shaking on both natural and manmade structures. Many of the experiments will not yield their full scientific value until the earthquake has occurred, when the difference in baseline before and after the earthquake can be determined, and the effect of the earthquake on manmade structures can be analyzed. However, some of the instruments installed at Parkfield have shown temporal variations (particularly in levels of seismicity) that have led to important scientific advances even during the current pre-earthquake monitoring period.

One such example is the borehole seismic array that has been installed at Parkfield. Because of the extremely low noise present on the downhole seismometers, a complete seismic catalog down to magnitude 1.0 has been recorded for the region. These seismic observations have permitted the recognition of slow, microseismic slip events [*Malin and Alvarez*, 1992] and the identification of periods of higher seismicity that may be related to the locations of future slip events [*Roeloffs and Langbein*, 1992]. Such events would have been impossible to identify with conventional surface seismic arrays, and have illustrated the importance of including downhole installations in other prediction arrays.

Because of the focus on Parkfield as a center of earthquake prediction efforts, the seismic history of the region has been scrutinized during the years since 1985. This has led to major advances in our understanding of the details of the previous Parkfield earthquakes. For example, differences in the extent of the rupture plane between the 1934 event and the 1966 event are now recognized [*Segall and Du*, 1993]. The pre-1930 historical events in the region have been more closely studied, resulting in the recognition of 10 previously unrecognized events with magnitudes above 5.5 within 100 km of Parkfield [e.g., *Toppozada et al*, 1990]. This new information has led to debate about just how similar the 1934 event and the 1966 event really are, and whether all of the earlier events were on the same fault patch, casting doubt on the validity of applying the "characteristic earthquake" hypothesis, in its simplest form, at Parkfield.

The focus on Parkfield has also resulted in reexamination of the predicted earthquake recurrence interval there based on the simplistic idea of uniform loading rate. For example, earthquakes on nearby faults, such as the 1983 Coalinga event, may have an effect [e.g., *Simpson et al.*, 1988; *Tullis et al.*, 1990]. In addition, the results of viscoelastic relaxation following the great 1857 earthquake may lead to a decrease in recurrence time during the longer-term San Andreas seismic cycle [*Ben-Zion et al.*, 1993]. The possibilities that the segment boundaries at Parkfield are not geometrically well defined [*Nishioka and Michael*, 1990] nor visible as steps in the seismically defined fault zone [*Eberhart-Phillips and Michael*, 1992; *Michael and Eberhart-Phillips*, 1991] and that successive ruptures may overlap in spatial extent has been recognized, along with the tendency for earthquakes to cluster [*Kagan and Jackson*, 1991]. The rate of strain accumulation since the 1966 event has also been examined, and used to constrain the moment deficit since 1966, which can be used for estimates of recurrence time [see summary by *Roeloffs and Langbein*, 1992].

Partially due to the focus on Parkfield, understanding of the statistics of "earthquake prediction" (in terms of estimation of recurrence times and their uncertainties) has advanced considerably. If we were to calculate the mean recurrence time and probability of the Parkfield earthquake as of today, we would do it by a more sophisticated technique, and using different uncertainties, than used in 1984. We would also recognize that various models of earthquake recurrence are possible. The question of whether to include the 1934 event in the probability calculation, and how to estimate the variability in the recurrence time ("shape factor") has led to further evaluations of the details of probabilistic calculations [*Nishenko and Buland*, 1987; *Savage*, 1991; *Roeloffs and Langbein*, 1992]. These advances in understanding of methods of estimation of earthquake probabilities have been applied to other regions along the San Andreas fault system [e.g., *Working Group on California Earthquake Probabilities*, 1988, 1990; *NEPEC/CEPEC/SCEC Working Group*, 1992; *Jones et al.*, 1991] and elsewhere, including volcanic unrest at Long Valley Caldera [*Hill et al.*, 1991] and at Mt. Pinatubo.

Regional studies of crustal structure around Parkfield have been carried out as part of the Experiment. These studies are aimed at (1) characterizing the three-dimensional velocity structure for improved hypocentral determinations and (2) identification of temporal and/or spatial variations in seismic velocity that may be related to fault zone material properties or fluid pressure buildup in the fault zone. The three-dimensional velocity structure obtained by *Eberhart-Phillips and Michael* [1992] and *Michael and Eberhart-Phillips* [1991] suggests the presence of a body with low compressional wave velocity and low resistivity, near the northeast side of the fault, beneath Middle Mountain. They infer that this material may contain high fluid pressure. Low shear wave velocity is also inferred at depths of 5-9 km near the 1966 hypocenter, possibly indicating high pore pressure [*Michelini and McEvilly*, 1991]. In addition, the quarterly vibroseis (polarized shear wave) investigations have shown temporal variations in seismic velocity that may be correlated to resistivity changes and/or slow creep events [*Karageorgi et al.*, 1992].

Possible precursory phenomena have been scrutinized. The Parkfield project required a careful evaluation of various possible precursors, in order to decide what to measure and record at the start of the experiment. During the course of the experiment, it has become clear that some possible precursory phenomena were not being studied (e.g. the ULF electromagnetic signals) and that some other measurements related to precursory phenomena may be relatively problematic (e.g., rainfall-induced creep events). Theoretical modeling of expected strain accumulation prior to rupture [e.g., *Tullis and Stuart*, 1992] has forced a careful look at the time scales and spatial scales over which precursory signals might be visible. This has shown that, for certain scenarios, we would expect to capture precursory signals on the currently designed and located instruments, but for other scenarios we would not be able to resolve these precursory signals. Thus, we have improved our knowledge of optimal experiment design for this location, and can now apply these techniques to future experiment design in other regions.

### *3.1.3 Technology Transfer*

The Parkfield experiment required a working collection of monitoring instrumentation designed to observe long-term and short-term changes in the fault near the inferred point of rupture initiation. As the experiment progressed, equipment failure and changes required modification of both the hardware (e.g., the cable connections on some downhole equipment) and the science plan (e.g., the alert levels triggered by creep events during heavy rainfall). Many of the bugs related to real-time operation of this monitoring system, and to design and installation improvements, are now worked out. Similar monitoring systems are now being installed along the Hayward fault system, near San Francisco, and along parts of the southern California fault systems. There has been a great savings in cost for these installations because of the experience gained at Parkfield. The real-time aspect of such data collection has been greatly advanced by the expertise gained in the Parkfield experiment, so that Parkfield serves as a starting model for the design of similar, but younger and more sophisticated, systems.

## 3.2 Lessons for the Response Community

### *3.2.1 Parkfield Successes*

The Parkfield Earthquake Prediction Experiment has been a success from the response community perspective as demonstrated by the response of emergency management services, in conjunction with the scientific and media communities, to the October, 1992, "A"-level alert. During the Experiment, the emergency response community has been able to prepare and exercise its response plan for the "A"- and "B"-level alerts, including the notification of local governments. Cooperation at Parkfield between the USGS and the California Office of Emergency Services (OES) has resulted in the State of California Governor's Office of Emergency Services *Parkfield Earthquake Prediction Response Plan* [1988], which was the basis for the response actions during the "A"-level alert. Based on the *Parkfield Earthquake Prediction Response Plan,* California OES developed and published the *California Short-Term Earthquake Prediction Response Plan* [October, 1990]. This second document describes state agency and county government actions to be taken in response to any scientifically-driven earthquake alert or prediction at any other location in the state.

The Experiment has brought scientists together with state and local officials, emergency managers, and the media, in a productive, mutually beneficial relationship. The state established the first scientifically-based state emergency management protocol for a specific predicted earthquake. This interaction has permitted emergency managers to understand the earthquake hazard as well as to understand the perspective of the scientific community regarding earthquake prediction. USGS scientists now understand the importance of providing timely interpretation of earthquake data to the state and local emergency managers and the public.

### *3.2.2 Benefits From the Parkfield "A"-Level Alert, October 20 - 22, 1992*

The "A"-level alert was mostly a positive experience. California OES received an initial alert from the USGS regarding a "B"-level alert 18 minutes after the initial earthquake. Six minutes later, the USGS notified OES of the "A"-level alert, triggered by the M = 4.7 earthquake at Middle Mountain. Eight minutes later, OES broadcast the alert to state agencies and local governments over the California Warning System and the California Mutual Aid System. Kern County was the first county to activate its Emergency Operations Center, 47 minutes after the OES alert. OES completed its alert of local government and response officials in less than one hour following the earthquake. OES staff went to the USGS offices at Menlo Park, and to the town of Cholame, near Parkfield.

Planning and exercising of the *Parkfield Earthquake Prediction Response Plan* allowed for this timely activation and mobilization of local government and OES. OES' overall assessment of the notification is that they "could not have done it much quicker." The operational response plan worked well because it prescribes simple and straightforward actions. As a result of evaluation of the state and local response to the "A"-level alert, the process of formal notification and validation did not need to be changed. However, key OES staff now carry the USGS alert pager, which will notify OES staff simultaneously, along with the USGS personnel, of Parkfield earthquake activity.

Interaction with the press was one of most valuable benefits of the "A"-level alert. OES was able to explain the purpose of the alert and associated response to the media. The seven counties' response was overwhelmingly positive. Local officials and politicians appreciated receiving the warning and the continuous flow of information. The county after-action reports were very positive — nothing of substance was reported as negative.

The widespread news coverage of the Experiment and the October "A"-level alert has sensitized the public in California to the possibility of future earthquake alerts. For example news media gave the alert front-page and lead-story status in the San Francisco Bay Area. Just as significant, the media also announced the end of the alert window three days later. To prompt media closure of the 72-hour alert window, the USGS, OES, and the California Division of Mines and Geology (CDMG) conducted a news conference. Fortuitously, the next week's "B"-level alert, October 26, 1992, following the "A"-level alert, emphasized to the public that different levels of alert exist with different associated earthquake probability percentages. Again, the media did a good job in portraying these alert levels with their associated probabilities. The alerts emphasized the prediction issue and hazard reduction issues, both of which will be raised in other contexts during future events.

The "A"-level alert underscored the key steps that local, state, and federal government officials must take when a prediction or alert is issued to the public. As a result, some local governments are now in the process of developing formal short-term earthquake prediction response plans.

*3.2.3 Benefits Advancing Public Policy*

The Experiment resulted in the development of the following beneficial response-related actions integrating scientific information with public policy:

1) It offered the first opportunity for the California Earthquake Prediction Evaluation Council (CEPEC) to validate an earthquake prediction;

2) It prompted the California OES to write and issue a public information brochure to the people of the area;

3) It prompted the National Science Foundation to fund a study to gauge the effectiveness of this public information brochure;

4) It prompted the California OES to write and issue written press statements on the alert prior to the "A"-level alert in October, 1992;

5) It contributed to the development of the City of Los Angeles' *Earthquake Prediction Response Plan;*

6) It led to the passage of California Assembly Bill 938 in 1985 (California Public Resources Code 2800 *et seq.*), which authorized California to contribute $1 million in state funds to the Experiment;

7) It attracted international public and private scientific participation in the Experiment through the CDMG-sponsored *Turkey Flat Strong Motion Experiment*;

and

8) It offered the first opportunity to engage the liability immunity provisions of California's law for state and local agencies and officials involved in response activities. (California Government Code Section 955.1, *et seq.*)

We believe that the California OES and the USGS plan to continue supporting the Experiment. The seven counties also are willing to continue participation in the prediction aspects.

**3.3 Problems Identified**

Although the Parkfield Earthquake Prediction Experiment has been very successful, there have been a few problems that the NEPEC Working Group has identified. These should be addressed as the Experiment evolves.

*3.3.1 Lack Of Ongoing External Scientific and Emergency Response Review*

Perhaps because of initial haste prompted by anxiety that the anticipated earthquake might occur before the Experiment was fully operational, there has been little provision for ongoing review. The annual expenditure and the importance of this project to the prediction community requires periodic external review (suggested annually) to assure its viability and credibility. An assessment of the progress and quality of the project by a panel of experts would ensure that appropriate scientific and response objectives are kept at the forefront of the project and that the Experiment changes in response to new developments, as well as doing much to dispel criticism of the experiment. The review might be done by a subset of the NEHRP Review Panel, which could oversee the general worth of the project in the context of the NEHRP program and give advice on the direction and disposition of financial support.

*3.3.2 Scientific Data Not Readily Accessible*

There is a perception that some investigators funded under the Parkfield project have been too busy, inadequately supported, or perhaps reluctant to release their data in a timely manner. These delays have reduced the usefulness of the data to the Parkfield team. An implicit philosophy of the Experiment is that all investigators must work in a cooperative effort in order to provide the data in a timely fashion for time-dependent decisions regarding designations of alerts and to assure timely review of data. If data gathered within the Parkfield experiment are important enough to be funded, they are important enough to be made available on a short enough time frame to contribute to the decision making process.

*3.3.3 Data Management*

The question of data accessibility is also related to the philosophy of data management. It is problematic that there is not now a modern data base management scheme implemented for the Parkfield data. Individuals who wish to examine the data must be familiar with the location(s) of particular data files in USGS computers (which require secure passwords) in order to retrieve the data. The user then must plot the data with his or her own software and has little information on attributes or an understanding of how the data may relate to the earthquake process. This is a serious limitation and does not allow ready access to the user.

While we realize that data acquired in sophisticated experiments must be carefully processed and scrutinized by individual investigators, we suggest that because of the specific NEHRP financial commitment and national stature of the Parkfield Experiment that a modern Data Base Management System be implemented for archiving and managing the Parkfield data. This requires that all on-line recorded data be available immediately after processing and that off-line and low-rate data, requiring editing and processing, should be archived within a sufficiently short time to be useful to the decision makers. This aspect is also important for providing on-line access to designated users. A user-friendly GIS system available with sub-licenses to the participating PIs would also be useful to manage, correlate, and display the archived data.

We also note that some additional data, now collected at Parkfield, should be considered for transmission and recording at Menlo Park. These include the electrical resistivity array and the ULF EM measurements.

A possible restriction in the Parkfield data distribution scheme is that the downhole and acoustic data, which are recorded on tape at Parkfield, are then sent to Duke University and UC Berkeley for analysis. Because of these time-consuming steps they are not available to the USGS team in a timely or efficient manner. A plan should be made to make these data more readily available to the USGS in Menlo Park quickly enough to be useful in a short-term alert and prediction mode.

#### *3.3.4 Overemphasis by the Public and Media of the Prediction Aspects of the Experiment*

The original objectives as stated by *Bakun and Lindh* [1985] were that the Parkfield experiment was to monitor the details of the final stages of the earthquake preparation process. The instrumental aspect of the Parkfield project was designed primarily as a surveillance project. However, with the involvement of California funding, the Experiment took on a short-term earthquake prediction objective that is perceived by the scientific and emergency response community as the "national" prediction experiment.

The general public now perceives the Experiment primarily as a short-term earthquake forecasting project with an inherent expectation to accurately predict an earthquake, while the scientific community views it not only as a short-term prediction experiment, but also as an effort to "trap" a moderate earthquake within a densely instrumented network. It is important to educate the public that there is great value to this monitoring effort even if the prediction effort is unsuccessful.

### 4.0 How Should the Experiment be Modified in the Future?

The Working Group recommends that the Experiment continue as a specific coordinated scientific effort in monitoring through the earthquake cycle, as well as in earthquake prediction. Although the annual probability of the expected characteristic event occurring, about 10%/year, is sufficiently high that the Experiment should continue, it is not so high that we can expect the Experiment to be over on a time scale of a few years. Thus, it is prudent to take a long-term perspective in contemplating the future of the Experiment. Issues to be addressed include costs, relocating the Experiment, mechanisms for ongoing evaluation of the Experiment, and future USGS response efforts.

### 4.1 What are the Incremental Costs Associated with the Experiment?

In addressing possible modifications of the Experiment, it is important to place the budget in context. The internal budget for the Parkfield Experiment is $1.4 million/year, but this amount is much more than the true incremental cost of the Experiment. The great majority of these expenditures are for salaries of scientists (prorated based on estimates of the fraction of time spent on the Experiment). Since these salaries would continue to be paid if the Experiment were ended, termination of the Experiment would not allow salary costs to be cut, although the effort could be deployed elsewhere. Real-time monitoring is highly automated, making use of computer systems that already exist. In the judgment of the Working Group, turning off the Experiment completely would save less than $200,000/year in the internal program. The external program has a budget of ~$400,000/year. Thus the total incremental cost of the Experiment is approximately $600,000/year — just over 1% of the NEHRP budget.

### 4.2 Should the Experiment be Moved?

USGS is under tremendous pressure to "do something" in more heavily populated areas, such as near the Hayward fault. The argument is made "How can we justify spending money trying to predict an earthquake in an area with as few taxpayers as Parkfield, when so many more people would be affected by an earthquake on the Hayward fault?" In answering this question it is important to remember that we do not at this time know which, if any, of the instruments monitoring pre-earthquake activity will measure premonitory signals. Thus, we do not know the requirements for instrumentation to install to be useful for earthquake prediction in heavily populated areas. It is extremely important to answer this question as quickly as possible. Impatient as we may be with the lack of the expected event, Parkfield remains the most likely place identified to capture a moderate earthquake. It is probable that precursors would only be detected by instruments located quite close to the earthquake preparation area. Parkfield represents one of only a few places where this preparation area has been specifically identified. In addition, the long baseline of measurements already made at Parkfield represents an investment that should be used, not walked away from.

While there are benefits to having the monitoring effort visible to the public, evaluating the possibility of precursors should have highest priority. The best place to find out if they exist is at Parkfield, where the target area is well defined. It is important that adequate resources be made available to keep the Experiment alive and evolving, not placed in mothballs.

### 4.3 Long-Term Aspects of the Project

#### *4.3.1 Commitment for Long-Term Management*

There has been an apparent lack of commitment by the USGS for long-term management and identification of importance for the Parkfield project. The project has included several Project Chiefs whose terms have been less than that of their corresponding USGS Branch Chiefs, with some in that position for as little as one year. The Experiment is sufficiently important that it deserves a commitment by the USGS for a long-term management team with the requirement that those individuals become totally familiar with the objectives and results, and have a good working relationship with participants. A Parkfield Project Chief should remain in that position for sufficient time to be able to make critical decisions based on a broad experience of observations and predictive models. Further she/he should have the status of major contributor of the NEHRP program and be involved in its planning and implementation.

#### *4.3.2 Acquisition of Land Rights at Parkfield*

There is apparently a problem with long-term rights of access to instrumentation sites at Parkfield. We believe that the importance of this experiment merits a cooperative commitment from federal, state, and county officials to assure land accessibility and that an effort should be made with private land owners to gain long-term commitments and accessibility. This aspect of the project would be enhanced by a local coordinating committee with members from the Parkfield community. This may require consideration of the instrumentation sites as designated land within a state or federal easement program, as a national earthquake study area, or other designated scientific establishment similar to the Stanford Linear Accelerator, the Superconducting Super-Collider site, etc.

### 4.4 Reassess Project Periodically to Modify, Upgrade and Acquire New Equipment

The scientific objectives of the Parkfield experiment are now reasonably well supported with modern instrumentation. It is imperative that these instruments, particularly the strain meters, be maintained, upgraded, and replaced when they fail. In addition, there are some methodologies that deserve additional consideration at Parkfield:

*Broad Band Seismological Studies* — An important issue is that there is only one broad-band, continuously recording system at Parkfield, installed in late 1991; it is not apparent to the Working Group how data from this instrument are used by those involved in the Parkfield Experiment. In the past few years, broad-band seismic data, 50 Hz to 0.03 Hz, recorded on wide-dynamic range digital recorders (either on site or via digital transmission) have been shown to be very useful for retrieving source and transmission properties of moderate to large earthquakes. In the recent earthquake for which the "A"-level alert was issued, broad-band techniques could have been used to rapidly obtain accurate source properties (e.g., rupture geometry, time and space properties, relationships to geometrical features of the proposed nucleation site at Middle Mountain, etc.). The Committee was unaware of to what extend, and how rapidly, broad-band data had been used in evaluating this event.

With the availability of new wide dynamic range broad band seismometers (such as Guralps, Streckheisens or equivalent) and 24-bit recording for example with RefTeks, there is a wealth of new information that can be gained by large dynamic range-wide band technology. For example, attenuation (weak vs. strong ground motion) over broad ranges of magnitudes and distances, source properties of small to moderate events, and accurate foreshock and aftershock assessment may yield important information on earthquake precursors. We suggest that a component of wide-dynamic range broad band coverage of the Parkfield area be implemented.

While real-time recording is perceived as costly to some, modern seismological instrumentation has been designed for on-line recording and is standard throughout the world's seismological community. These data would cost the same, if not more, if they were recorded on site or at Menlo Park in a time-delay mode. Moreover, if they were recorded on-site and transmitted to Menlo Park, then the timely nature of the data would be negated for warnings and alert. We suggest that real-time recording of seismological and related high sample-rate data be continued.

*Parkfield Network Calibration* — A general problem common to most short period seismic networks, including CALNET and hence Parkfield, is the lack of a systematic absolute calibration of the complete seismometer-recording systems for true ground motion. The USGS indicates that calibration pulses are recorded daily on the short period seismometers, but in reality little is done with the calibration data. The short period seismometers are calibrated only when they are brought to Menlo Park for bench tests where they can be absolutely calibrated (presumably very infrequently, i.e. every few years). If there is a large earthquake at Parkfield and the short period instruments have not been calibrated accurately, a major source of information, namely accurate ground motions (for the on-scale events for even small to moderate events), will be lost. This information is especially useful for assessing large ground motions and attenuation.

Network and other instrument calibrations should become part of the Parkfield archive accessible to all users. Additional information in a data base should include a history of each station, model numbers of components, maps of station locations, instrument modifications, calibration constants, etc. The public must be assured that the data acquired from this and any similar project are validated.

*Paleoseismicity* — The basic premise of the Parkfield experiment is the recurrence of "characteristic" M ~ 6 events. However, because there is little trenching information in the Parkfield area (in part due to geography and other logistical problems) the long-term Holocene record has not been evaluated. Thus, we do not know if the typical event size and recurrence interval assumed from the six historic events also dominates the longer-term seismic patterns. For example, if paleoseismological studies revealed the presence of larger events, this would change our view of the typical Parkfield event. This information is crucial for making statistical assessments of earthquake probability and for evaluating the characteristic earthquake model: Some studies have been carried out addressing the feasibility of trenching near Parkfield. These studies need to be documented and critically assessed.

### 4.5 Assessments of Costs and Productivity of the Experiment

The Experiment should continue to evolve, and external review should be part of this process. We suggest that the project be reviewed periodically by a panel of independent scientists. An annual report, discussing productivity and cost, should be prepared and submitted to this panel. The report should contain a summary of the scientific and emergency response aspects of the project, as well as a listing of products (reports, catalogs, data archives, papers, presentations, etc.) supported in part or totally by the Parkfield experiment. This part of the report should be presented at a national meeting such as the

AGU or SSA, with consideration of publication in a journal such as *Eos*, to inform the community of the status of the project.

An accurate assessment of the cost of the Parkfield experiments should be included in the report to the panel, assessing individual project costs and related salaries. A review of funds for USGS employees and contract employees should be scrutinized and the USGS management should ensure that funds for the Parkfield project are expended according to their intended use. Equipment acquisitions should be listed and a comparison of operating costs of the network and other systems should be made and reported periodically to the review panel. Further, the report should provide a list of instruments or projects that were considered each year for implementation, continuation, or deletion.

### 4.6 Recommendations for Future USGS Response Efforts

#### *4.6.1 Strengthen the USGS Response Role*

The USGS should recognize and provide support for the Experiment as a scientific experiment in the broader integrated context of an actual public policy activity. In general, the USGS must recognize the importance of developing written plans to provide the scientific and public information support that is needed during all stages of public-alerts. The USGS should also consider institutional recognition for scientists who are committed to devoting portions of their careers to public policy and education without jeopardizing potential career advancement. The USGS should consider formal and institutional recognition of the important public policy role that scientists can play for all phases of natural hazards prediction and response.

The USGS should establish a formal protocol describing agency functions and personnel functions required to adequately support the media, the state and local governments, and the emergency management community, after issuing earthquake predictions. This protocol should be written in the form of a *Standard Operating Procedure* which describes the tasks to be performed by dedicated USGS personnel during earthquake alerts. The USGS should develop these *Standard Operating Procedures* by querying representatives of the media, state and local governments, and emergency management who depend on earth science information during alert periods. In the case of Parkfield, the USGS should be committed, as an institution, to preparing for an "A"-level alert. To date, the USGS has relied on individual scientists to provide a presence and continuity for interaction with state and local government officials. Even though individual Parkfield scientists have had a continuous commitment to providing earth science information to the media, the USGS was unable to provide adequate representation to address media questions regarding the prediction during the "A"-level alert.

The USGS has strengthened its response role since the October, 1992 "A"-level alert by recognizing that the Parkfield Chief Scientist, Project Chief, and the Public Information Officer roles must be adequately covered by three scientists rather than one scientist, as has been the case for most of the duration of the Experiment. The Parkfield scientists at the USGS, Menlo Park, have already implemented this additional coverage of duties at least until the closure of the fiscal year, September 30, 1993. For additional "A"-level alerts, one scientist will run the field laser experiments, one scientist will coordinate the

Menlo Park operations, and a three-scientist team will mobilize to Parkfield to provide earth science information to the media.

*4.6.2 Review the Threshold Criteria Determining Alert Levels*

The USGS should review the probability percentages, which act as threshold criteria for the Experiment alert levels, with respect to the following three concerns:

1) the level of accuracy and the statistical uncertainty associated with these probability percentages;

2) the effect of "false alarms" on the credibility of the earthquake prediction process; and

3) the appropriate actions for the public and response community as reflected by these probability percentages and alert levels.

An "A"-level alert for the Experiment has been assigned a 37% probability for the occurrence of an earthquake of M ~ 6 within 72 hours. The public and the media infer that the accuracy of this percentage is very high, reasoning that it has not been stated as, e.g., 36% or 38%. Since the uncertainty in this estimate is much larger than the reliability associated with a probability expressed to two significant digits, the scientists are implying to the public unrealistic accuracy. This leads to the misperception that scientists believe their probabilities to be more accurate than they are.

There is also a problem in the public perception of the Experiment created by the highest level alert — the "A"-level alert — having a fairly low absolute probability associated with it. The alert scale is saturated at a low probability level. It is easy (albeit incorrect) to associate the highest available alert level with a high absolute level of probability and the need for media attention. When the expected event does not follow the posting of the highest available alert level (as should usually be the case for the alert levels as defined), the alert is easy to perceive as a false alarm. Will the frequency of *perceived* false alarms associated with future alerts diminish the intended response and preparedness actions of the public, as well as begin to discredit the scientific credibility of the earthquake prediction process? The false alarm rate needs to be more fully understood by local governments and the public. Perhaps the most straightforward remedy would be to define a new alert scale, e.g., I - V, with the current "A"-level assigned level II. (Level I might be triggered, for example, by the "early warning" system under study.) Although this is more of a public relations issue than a science issue, public relations are an important aspect of the Experiment.

An "A"-level alert results in notification of the state and local governments and the public, and triggers response agency mobilization. Are the public and response actions appropriate? Should future notification and mobilization be triggered at higher probability percentages to minimize false alarms and justify mobilization? For example, should an "A"-level alert be the basis for canceling employees' leave, or lead to the evacuation of hazardous buildings for one week? Are the social, economic, and political impacts appropriate? The USGS must work closely with state and local governments and private industry to review and possibly devise alert levels which are sensitive to the actions that the alerts inspire.

*4.6.3 Fund Understanding of Societal Impacts of False Alarms*

Repeated future alerts at Parkfield, which result in perceived false alarms, could have a negative "cry wolf" effect on the public. Since the Experiment will continue to teach us about the impact of perceived false alarms on the public, funding should be provided by some agency to determine the societal impact of false alarms. The societal impact of earthquake prediction and associated public alerts also includes unknown political and economic consequences. California OES believes that emergency managers still have a great deal to learn about the implications of "A"-level alerts in terms of what occurs as the public and emergency management goes through these cycles.

*4.6.4 Improve Communication of the Hazard Potential to the Public*

The USGS should support studying how to most effectively communicate the predicted earthquake hazard to the potentially affected population, as well as to populations located beyond the area of concern. During the "A"-level alert, some people who were unfamiliar with the Experiment and located long distances from Parkfield became unduly concerned for their safety. The alert stirred some to call for a shut down of Diablo Canyon nuclear power plant, located 50 miles southwest of Parkfield. Residents were concerned as far away as San Francisco. The OES *Earthquake Safety Information Center Hotline*, located in Pasadena, received numerous calls from residents in southern California counties. During alerts, the message to the public might also include appropriate warning and preparedness information to those located beyond the areas likely to be affected. Since there is virtually no part of California that is immune to earthquake damage, the public needs to receive the message that they can reduce potential damage from future earthquakes by mitigating risk and that they cannot "go to some place where they will be safe from the shaking."

The USGS might convey information using a map format to define areas of expected strong ground shaking and provide damage estimates out to the probable limits of felt ground shaking. Map displays that show where the earthquake will *not* be felt should be offered to the public as well. The USGS should investigate how to better communicate risk information to the public, the media and local government officials, not just earth science specialists.

## 5.0 Concluding Remarks

Parkfield remains the best identified locale to trap an earthquake. The consensus is that the annual probability for the expected "characteristic" event is about 10%/year. At this level, the Working Group concludes that the Experiment should be continued, both for its geophysical and its public response benefits.

Although this probability is relatively high, it is not so high that we can have confidence that the event will occur on a time-scale of only a few years. Thus the Experiment should be viewed with a long-term perspective. The Experiment should not stagnate: rather it should continue to evolve. This will require adequate resources. In view of the long-term commitment required, consideration should be given to a separate NEHRP funding status. Further, the project be periodically reviewed for its merits and progress .

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## Appendix F

Document presented to NEPEC by K.Aki: draft executive summary of Phase II report with illustrations.

# DRAFT EXECUTIVE SUMMARY (June 1, 1993)

## Future Seismic Hazards in Southern California

## Phase-II

§1. We estimate here the probabilities and likely ground motion resulting from large earthquakes in southern California. This report supplements an earlier report, WGCEP 88, which examined earthquake hazard on the San Andreas, San Jacinto, and Imperial faults. One widely quoted conclusion was that a M7.5 or larger earthquake had a 60% probability of occurring somewhere on the southern San Andreas fault within the next 30 years. The present report includes new data, new methods, and broader objectives than the 1988 report:

(1) A major earthquake occurred at Landers in 1992.

(2) We have new paleoseismic data on the San Andreas and other faults in southern California.

(3) We have new data on the tectonic deformation of the region from the Global Positioning System (GPS).

(4) We have new statistical methods for dating with uncertainty in earthquake recurrence times.

(5) We have a better understanding of fault interaction and better analytical tools for estimating the stresses that earthquakes cause on nearby faults.

(6) We have better, more complete high quality earthquake catalogs.

(7) We recognize the need to include risks from other faults including blind faults that were not considered in the 1988 report.

(8) We need to address not only the probability of earthquakes but also the likely ground motion resulting from these events.

§2. An ad-hoc working group was formed in July 1992, immediately after the Landers earthquake (M7.5) of June 28, to evaluate how the recent sequence of earthquakes in the Landers-Big Bear region might affect future large earthquakes along major faults in southern California. The group estimated the yearly probability of a M7 or larger earthquake in southern California to be at least 5% and may be as high as 12%, an increase from about 4% estimated prior to 1985. These larger values reflect the recent increase in seismicity in southern California, the effect of stress redistribution by the Landers earthquake sequence, and the ripeness for failure of portions of the southern San Andreas fault. The data and methodology used for estimating the yearly probability were published as the Phase-I report in November, 1992. The report included a list of plausible large (M>7) earthquakes within 100 km of the Landers rupture in the next few years together with maps of intensity of ground shaking for selected earthquake scenarios.

The present Phase-II report will address the estimation of future seismic hazards in the broader region, covering the whole southern California and for the longer term up to 30 years. Like the Phase-I report, this report is intended for public safety personnel, officials responsible for construction and maintenance of structures and life lines, engineers, science writers and interested members of the public, as well as members of the earth science community.

§3. Long-term probabilities for failure of major earthquake faults in southern California were estimated in 1988 by the Working Group on California Earthquake Probabilities (WGCEP 88). The estimation was based on the identification of a characteristic earthquake for a given fault segment, and requires paleoseismological data on the statistical distribution of time intervals between consecutive characteristic earthquakes, and the occurrence time of most recent one. WGCEP 88 considered only the San Andreas and San Jacinto fault because of the lack of paleoseismological data.

In order to improve the WGCEP 88 estimates, we tried to expand the list of faults for which the WGCEP 88 method can be applied, in addition to revising the parameters for the San Andreas fault and the San Jacinto fault. For this purpose, a workshop was held among earthquake geologists to reach a consensus on the up-dated compilation of geologic dates on faults in southern California. As a result, the probability of failure for the Elsinore fault can now be estimated by the WGCEP 88 method.

Some potentially dangerous faults cannot be treated by the above method because inadequate data exist. In fact, most of the damaging earthquakes in southern California since 1857, including the Long Beach earthquake of 1933, the San Fernando earthquake of 1971 and the Whittier Narrows earthquake of 1987, occurred on faults for which characteristic earthquake size and frequency cannot be estimated with currently available data.

In order to remedy the lack of geologic data for potential earthquake faults, we shall supplement the WGCEP 88 method by consideration of catalogs of past earthquakes and recent crustal deformation data now available from the GPS network in southern California.

In order to combine the geologic, geodetic and earthquake catalog data, we need to introduce seismic source zones for earthquake source characterization. For the purpose of the present report, we adopted 58 seismic source zones as shown in Fig. 1. Out of 58 zones, 33 zones are named after the fault they contain, among which segments of the San Andreas, San Jacinto and Elsinore faults constitute only 12 zones. We distinguish three types of source zones: Type A seismic source zones each contain one major fault segment, treatable by the characteristic earthquake methods of WGCEP 88. Type B seismic source zones contain well known active faults for which segmentation and recurrence data are not available or rather uncertain. Type C zones are not dominated by any single major fault, but may contain diverse and/or hidden faults.

Our general strategy is to combine the contributions from faults with sufficient paleoseismic data and those from the earthquake catalog data in such a way that the total displacement across southern California is consistent with the relative motion between the Pacific and the North American tectonic plate. The result will be compared with the one based on the crustal deformation data in order to examine the deficiency in paleoseismic and catalog data.

§4. Recently updated paleoseismological data for faults in southern California were reviewed by a group of earthquake geologists, and consensus estimates of slip rate, amount of slip in an earthquake, recurrence interval, dates of past earthquakes, and the date of most recent one were summarized in Table 1 for various segments of the San Andreas, San Jacinto and Elsinore fault. The table lists also the standard error for each estimated parameter.

Following the methodology used by WGCEP 88, we then estimated the probability of failure for each segment using the parameters listed in Table 1. The resultant 5-year and 30-year probabilities are shown in Table 2. The revised probability estimates are very similar to those given in 1988, with a few exceptions: we revise downward the estimated hazard for the Coachella segment of the San Andreas fault (from 0.4 to 0.2 in 30 years) and the Anza segment of the San Jacinto fault (from 0.3 to 0.13 in 30 years). We revise upward the hazard estimate for the San Jacinto Valley segment of the San Jacinto fault (from 0.1 to 0.42 in 30 years).

The primary cause of the above revisions is a fundamental parameter called "coefficient of variance" which is the standard error of the logarithm of recurrence interval. This parameter is composed of two terms, one is due to the intrinsic nature of earthquake process and the other to the measurement error. The intrinsic part is attributed to the erratic behavior of dynamic process of earthquake rupture and unknown interaction from other faults in its neighborhood. With the increase in high quality paleoseismic data as well as the advances in computer modeling of fault interactions, we now believe that the value of this parameter used by WGCEP 88 was too small. Revising this parameter to a larger value, the periodicity of earthquake occurrence is weakened, and a consequence is that the probability of failure was overestimated for the segment where the last event occurred in the distant past, and underestimated for the segment where the last event occurred in a recent past relative to the mean recurrence time. The reduced probabilities for the Coachella and Anza segment are due to the long time since the last event on these segments. The increased probabilities for the San Jacinto Valley segment is due to a combination of the relatively short time since the last event and the revision of the mean recurrence time to nearly half the original estimate.

Despite some differences for individual segments, the probability of failure of any one of the segments of southern San Andreas fault is unchanged from that given by WGCEP 88. For example, the 30-year probability remains about the same (61%).

This high probability is robust, because, even if we assume that the probability is independent of the time since last earthquake (Poisson model), we obtain about the same estimate.

§5. In order to extend our analysis to the entire southern California, our starting point is the fact that the fault slip on the three major faults considered so far amounts to about 60% of the total displacement between the North American and the Pacific tectonic plate. We need to consider the contribution of the remaining 40% to the seismic hazard in southern California. The nature of seismic hazard will be very different if the displacement is carried by few large earthquakes, such as an M7.5 earthquake on east-west running thrust faults along the northern margin of the Los Angeles basin, or by a large number of M6 earthquakes distributed throughout southern California.

In the present report, we addressed the above issue by the following two approaches. In one of them, we assume that the total seismic hazard comes from two types of source: characteristic earthquakes on known fault segments, and other, more random "non-characteristic" events. We treated the characteristic earthquakes as "line sources", and we assumed a random distribution of the non-characteristic earthquakes within the 58 seismic source zones. To incorporate non-characteristic earthquakes, we used earthquake catalogs, edited to delete those large earthquakes already included on defining characteristic fault segments, to define the spatial variation of earthquake occurrence.. Three different catalogs are chosen, covering three different periods, namely, 1850-1992, 1932-1992, and 1977-1992. We construct a map of spatially smoothed yearly frequency of earthquakes per (100 km)$^2$ with M>6.0 as shown in Figs. 2, 3 and 4. Then, we calculate the yearly frequency for each seismic source zone (Fig. 1) as listed in Table 3a, 3b and 3c. Assuming the Gutenberg-Richter magnitude-frequency distribution truncated at a $M_{max}$, we can find the rate of earthquake occurrence for a given magnitude for each zone by adjusting $M_{max}$ to meet the requirement for the total displacement. Table 3a, b, c, list the case in which $M_{max}$ is 8.25 for all zones. The above form of magnitude-frequency relation does not consider any earthquake characteristic to a fault segment, and represents distributing the total displacement over relatively smaller earthquakes. As a result, the recurrence time for M>7 estimated for Type A seismic zones is considerably greater than that assigned for a characteristic earthquake based on paleoseismological data.

§6. Our second approach is based on the geodetic data on crustal strain recently acquired by the use of GPS. The strain change in the past 4 years observed by the GPS network in southern California is shown in Fig. 5. It shows significant strain accumulation broadly throughout southern California. This measured strain accumulation was distributed among seismic source zones as shown in Fig. 6 and Table 3d with greater concentration in Type A and B source zones relative to Type C source zones. We assume again the Gutenberg-Richter magnitude-frequency distribution truncated at $M_{max}$, but in this case, we select $M_{max}$ corresponding to the magnitude of earthquake with the fault length equal to the maximum length of each zone, as listed in Table 3d. The recurrence time for a given magnitude for each zone can then be determined to account for the strain accumulated in each zone. Fig. 7 shows the corresponding yearly frequency of earthquakes with M>6.0 per (100 km)$^2$.

In comparison with the catalog-based approach, this approach produced higher frequency for Type A and B source zones. The recurrence interval for M>7.0 is also much shorter for these zones than the case of catalog-based approach, but still somewhat longer for type A zone than that obtained from paleoseismological data.

§7. In order to integrate the above three approaches based on paleoseismological, earthquake catalog, and strain accumulation data, it is useful to compare seismic hazard maps predicted by them. We estimated combined seismic hazard using three different models. The first model assumes an exponential distribution of recurrence times (i.e., a Poisson process) both for characteristic and non-characteristic earthquakes. This model is statistically robust, in that it makes no assumption about the regularity of earthquake occurrence, and it does not rely on the date of the last earthquake on a particular segment. The second method assumes that characteristic earthquakes have a log-normal distribution of recurrence times, with a total coefficient of variation (including both intrinsic and paerameters uncertainty) of 0.5. Non-characteristic earthquakes are assumed to be Poissonian. This model is closest in spirit to that used in the 1988 report. The third model, based on the geodetic data, assumes Poissonian reoccurrence for all earthquakes.

The seismic hazard maps can be constructed using a modular approach. For example, in method 1, we sum separately the effects of Poissonian characteristic earthquakes (line sources) and Poissonian non-characteristic earthquakes distributed in seismic source zones. Similarly, in method 2 we can combine the effects of log-normal characteristic earthquakes with the effects of Poissonian non characteristic earthquakes. As of June 1, 1993, we have the separate pieces (e.g. Figures 8, 9) but we have not yet combined them.

Fig. 8, 9 and 10 show the map of probability of exceedance for 0.2g peak ground acceleration at the basement rock (when the ground is shaken at the acceleration of 0.2g, you will be shaken by the force equal to 20% of your weight) in 30 years for southern California based on the paleoseismological data, earthquake catalog (1850-1992) and strain accumulation data, respectively.

The paleoseismological data (log normal model) on earthquake characteristics to segments of major faults give the highest value of exceedance probability, 60% near the junction of the San Andreas and the San Jacinto fault as shown in Fig. 8 . The probability decreases rapidly with the distance from these faults.

On the other hand, the catalog data predict diffusely distributed exceedance probability slowly decaying with the distance from the San Andreas-San Jacinto fault zone. Since the catalog data were interpreted without regard to any fault-specific characteristic earthquake, it is reasonable to combine Fig. 8 and Fig. 9 (after adjusting $M_{max}$ for the plate displacement constraint) to represent a total picture of seismic hazard in southern California. This picture, however, would disregard the role of any earthquake characteristic to a given fault, other than considered in Fig. 8. In this sense, it would represent a lower bound probability for Type B seismic source zones.

The exceedance probability based on the strain data shown in Fig. 10 gives lower estimates than the paleoseismological estimates for the zones considered by the latter method (Type A source zones), but gives higher estimates than the method combining the paleoseismological data and catalog data for Type B source zones. These high values are the consequence of the high strain accumulation rate observed broadly throughout southern California, as well as the particular procedure used for assigning the rate to individual seismic source zones. Since this approach is based on the assumption that all the accumulated strain is released seismically, the map obtained by this approach may give an upper bound probability for Type B source zones.

§8. For 5-year probability, we replace the catalog (1850-1992) used for the 30-year probability estimation by the one for 1977-1992, to reflect the clustering of earthquakes in space and time. This approach is qualitatively consistent with the short-term probability estimates made in the Phase-I report.

§9. In the present report, we considered only the peak ground acceleration at the basement rock for describing the shaking because the report is focused on the characterization of earthquake sources in southern California. In the future, we shall include other ground motion parameters such as peak ground velocity, duration of shaking and response spectra at various frequencies. For these parameters, we need to include the effects of local geology and topography on the ground shaking.

We also need to accumulate more paleoseismological, earthquake catalog and geodetic strain data in order to make our prediction more accurate and reliable. In particular, we need to validate our assumption that the accumulating strain is totally seismic.

Writing Assignment (Each about 5 pages)

§1&2. Tom Henyey (need to add tectonic perspective)

§3. Dave Jackson

§4. Dave Schwartz/Jim Dieterich/Dave Jackson

§5. Dave Jackson

§6. Steven Ward

§7. Mehrdad Mahdyiar/Allin Cornell

§8. Dave Jackson (need expansion)

§9. Kei Aki/Dave Jackson (need expansion)

| | |
|---|---|
| **DEADLINE:** | June 21 (M) |
| **EDITORIAL WORKSHOP:** | Henyey, Jackson, Aki, June 22? |
| **FINISH:** | June 30 |

Fig. 1 Seismic Source Zones

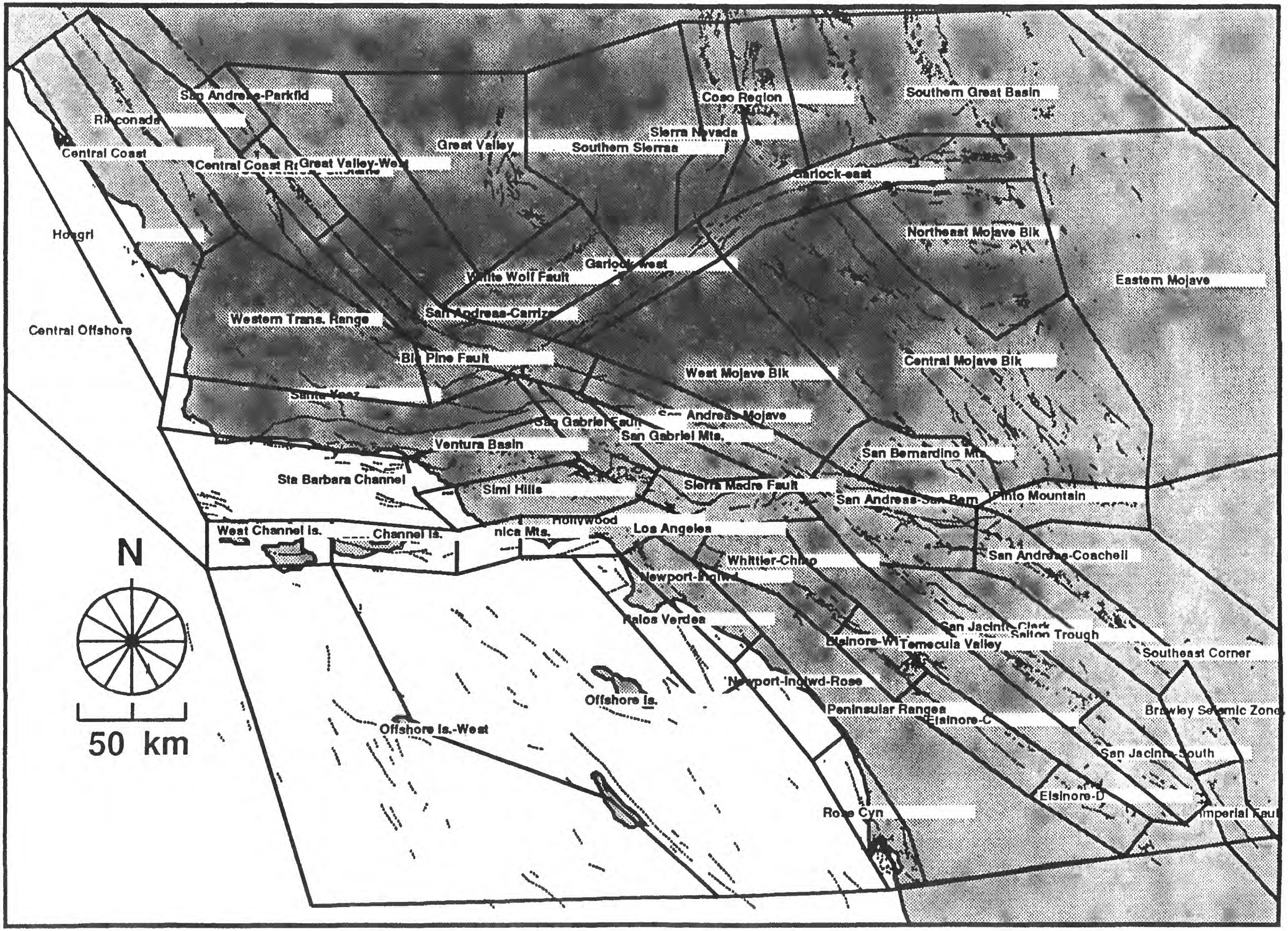


Fig. 2* S. California Seismicity Forecast (Ellsworth, 1850-1992)

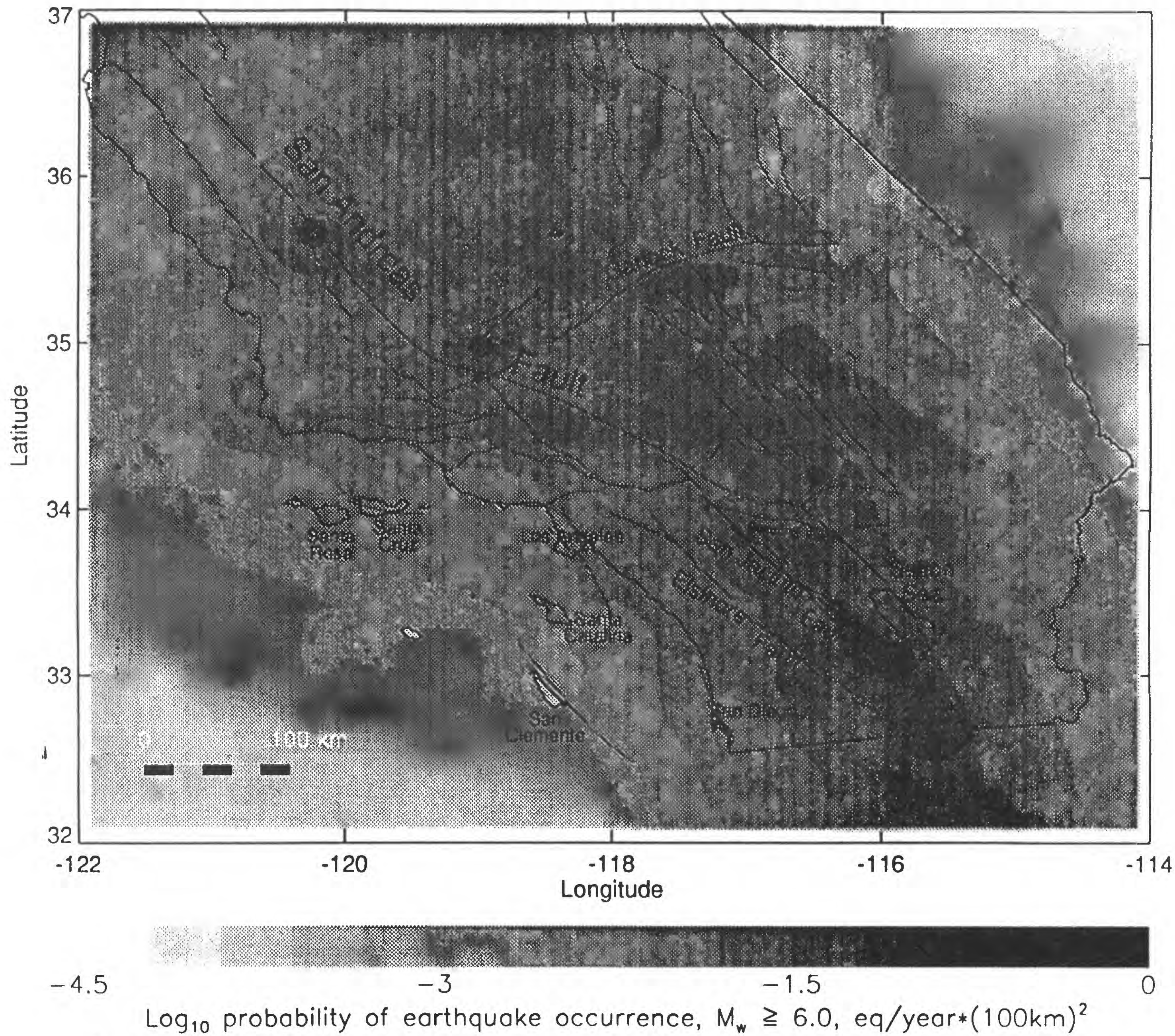


Fig. 2'

# S. California Seismicity Forecast (Toppozada, 1850-1992)

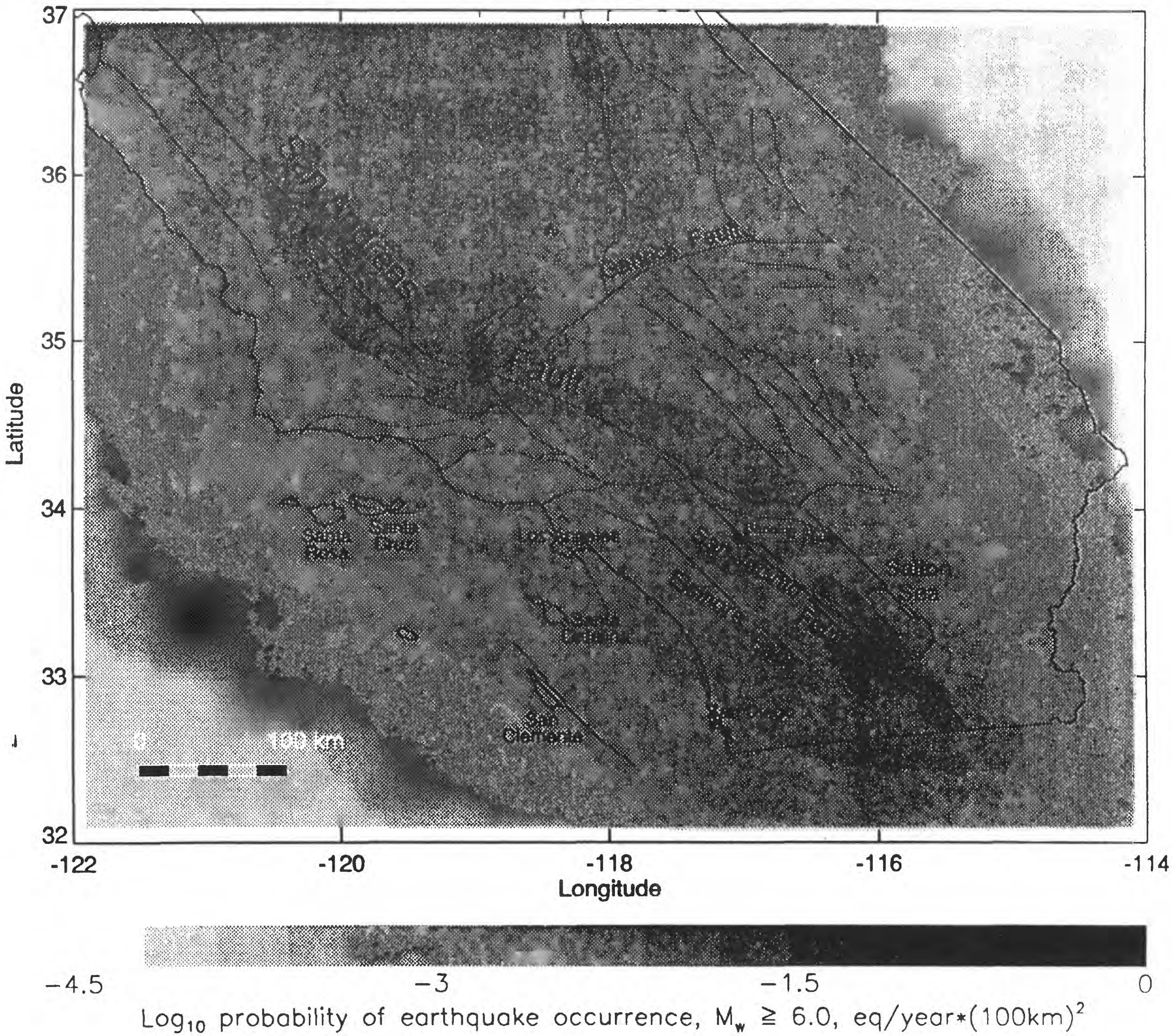
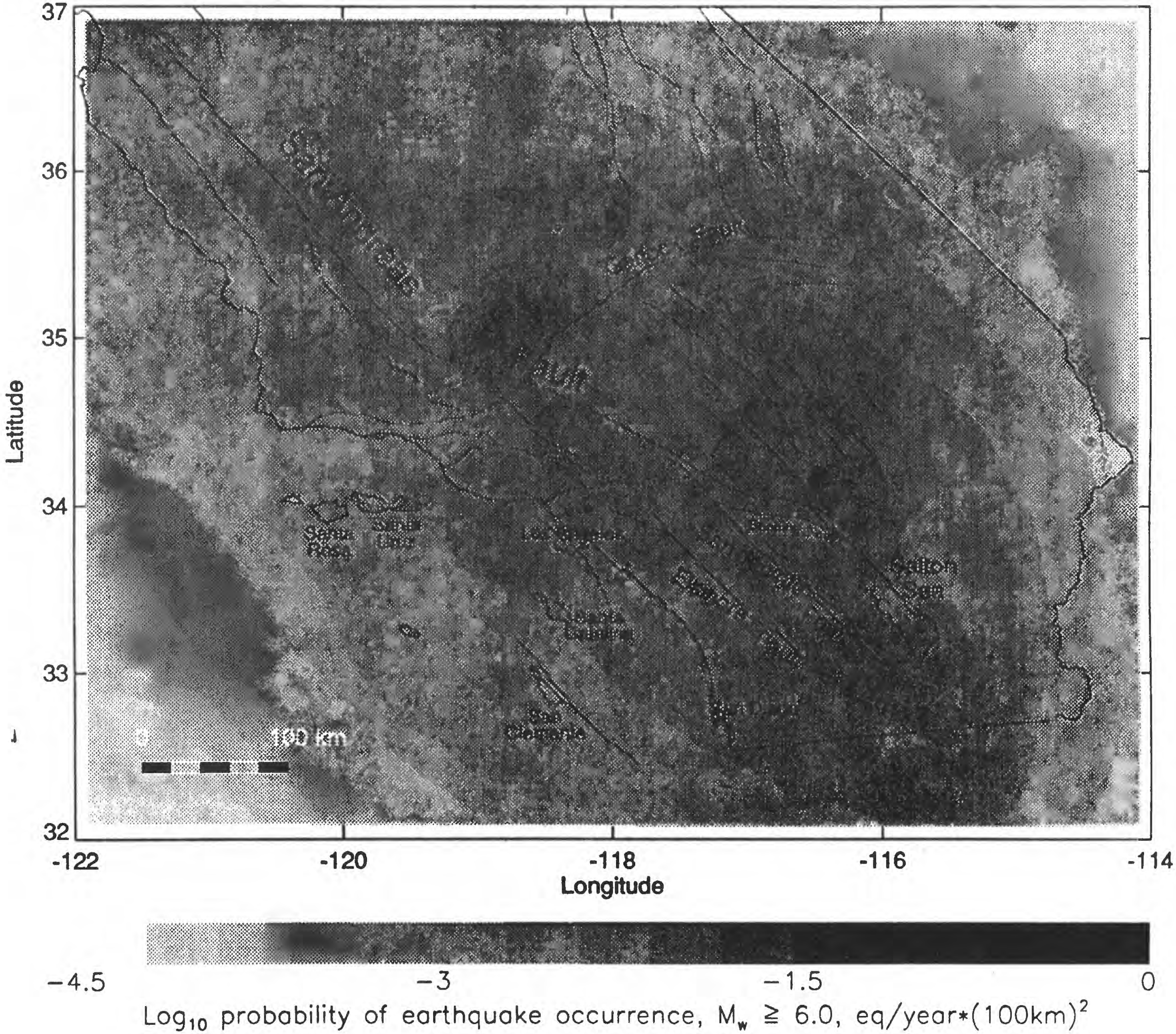


Fig. 3 S. California Seismicity Forecast (CIT catalog, 1932-1992)




100

Fig. 4. S. California Seismicity Forecast (Harvard CMT, 1977-1992)

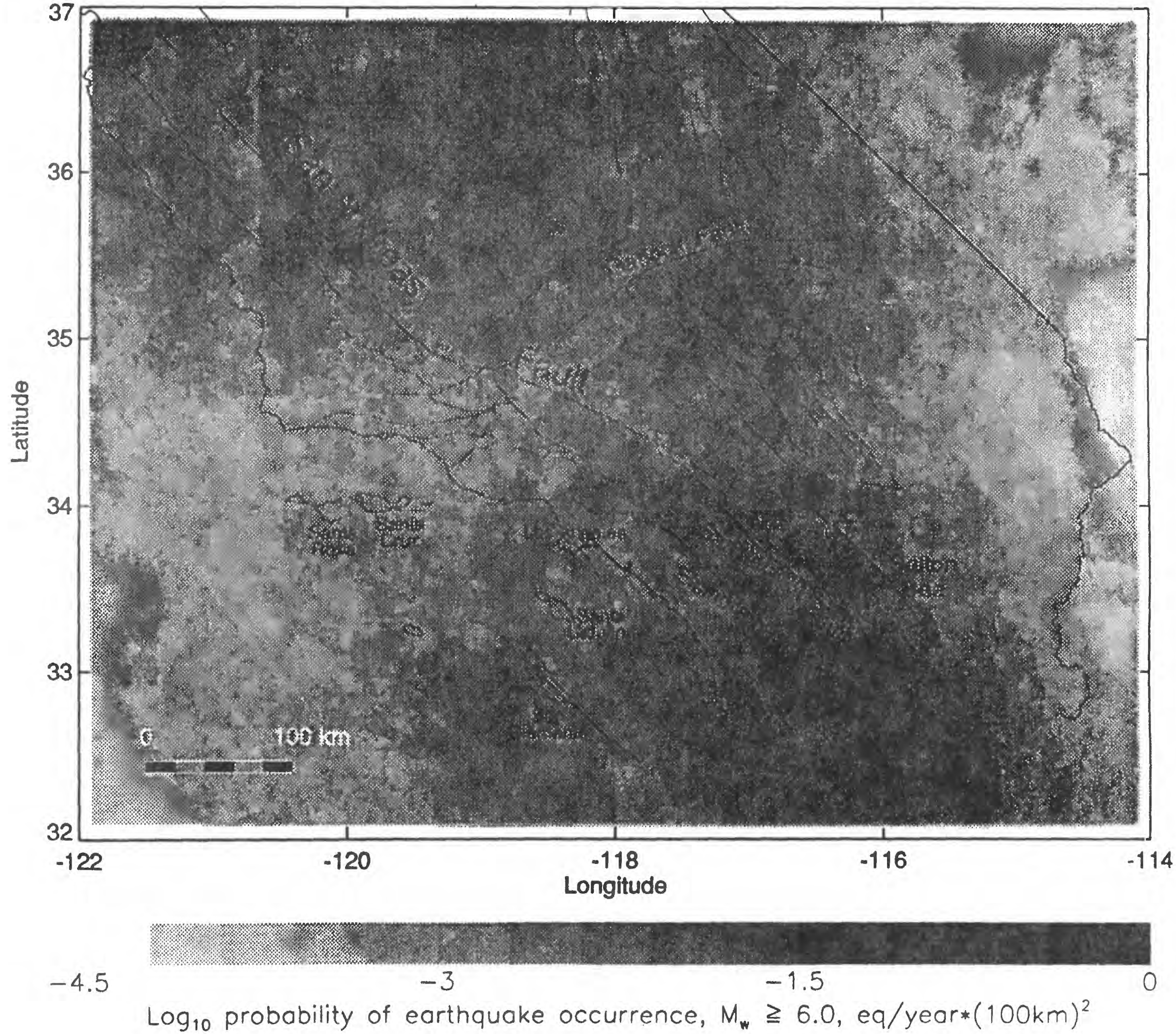


Fig. 5 Strain Rate

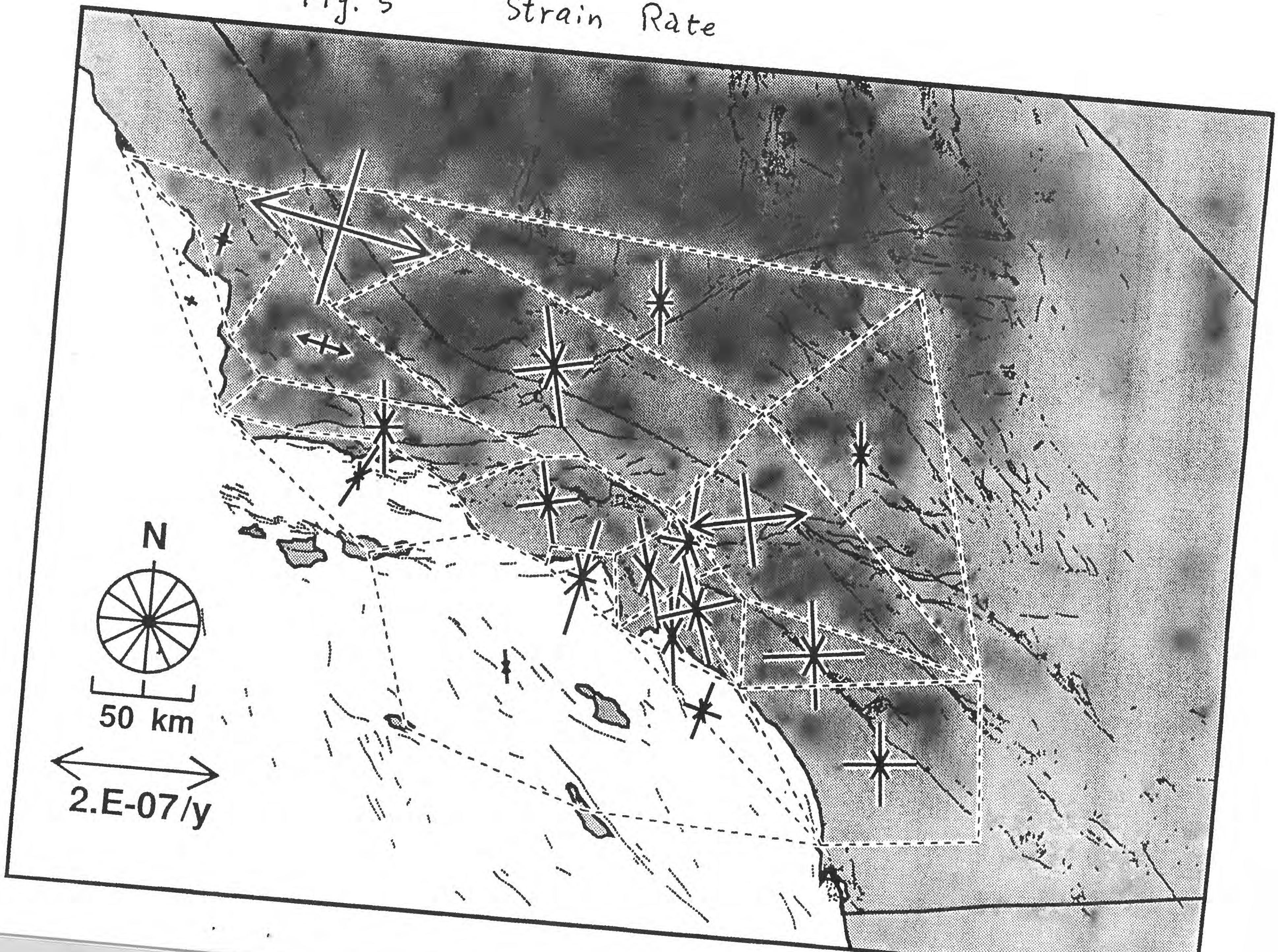


Fig. 6: Observed GPS strain rates. (small # interpolated) in $10^{-8}/y$

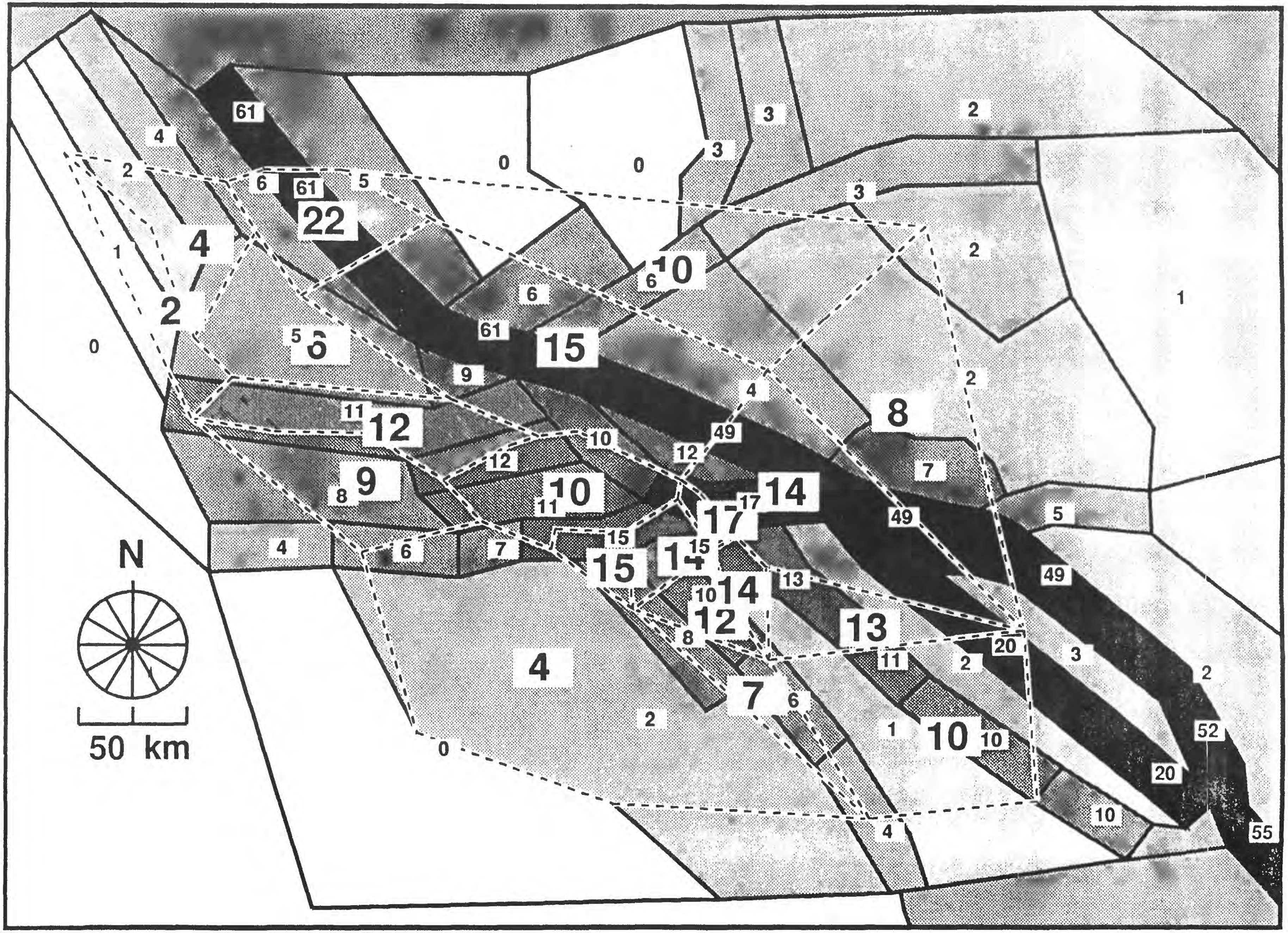
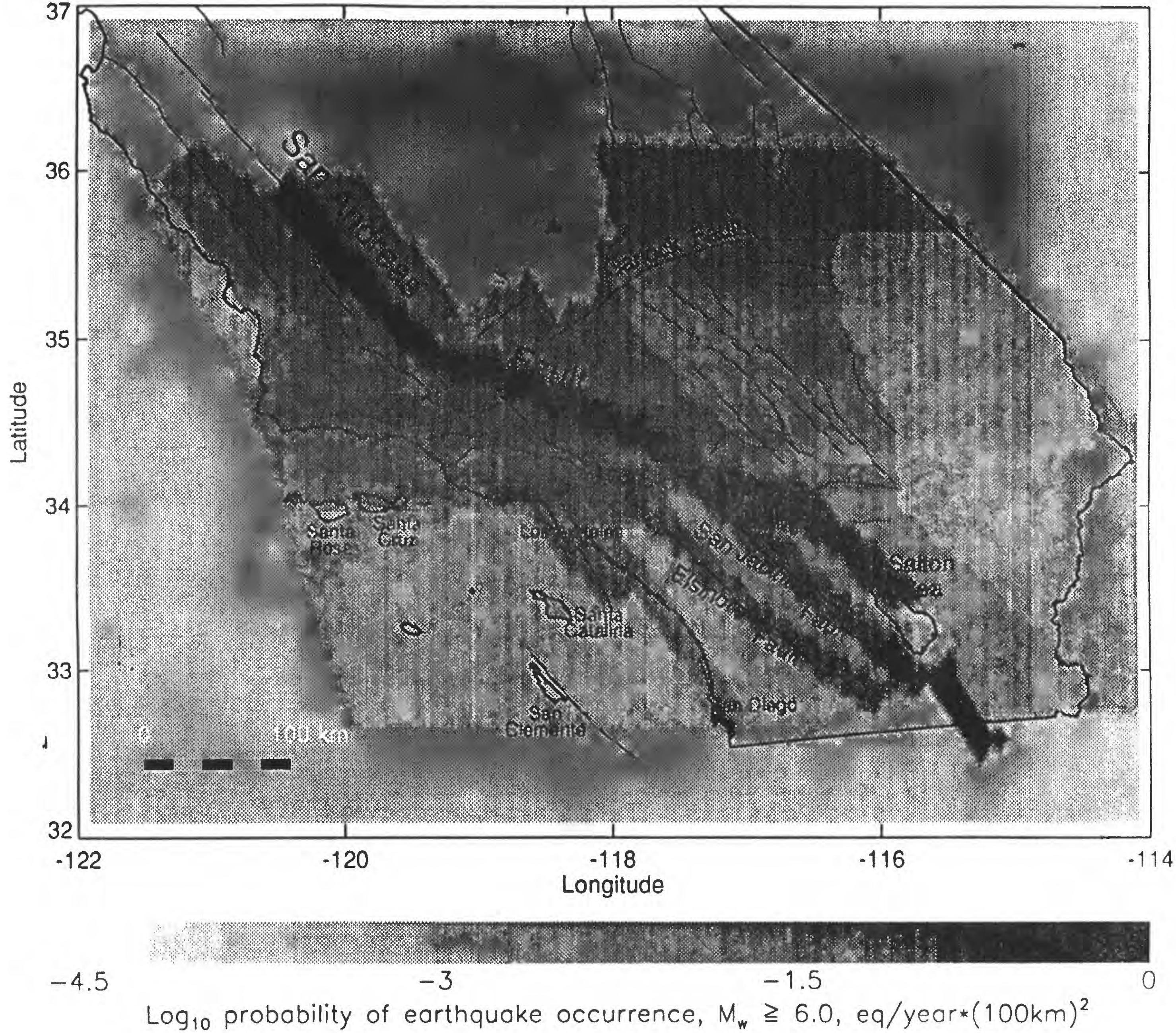


Fig. 7. S. California Seismicity Forecast (Ward: geodesy results)



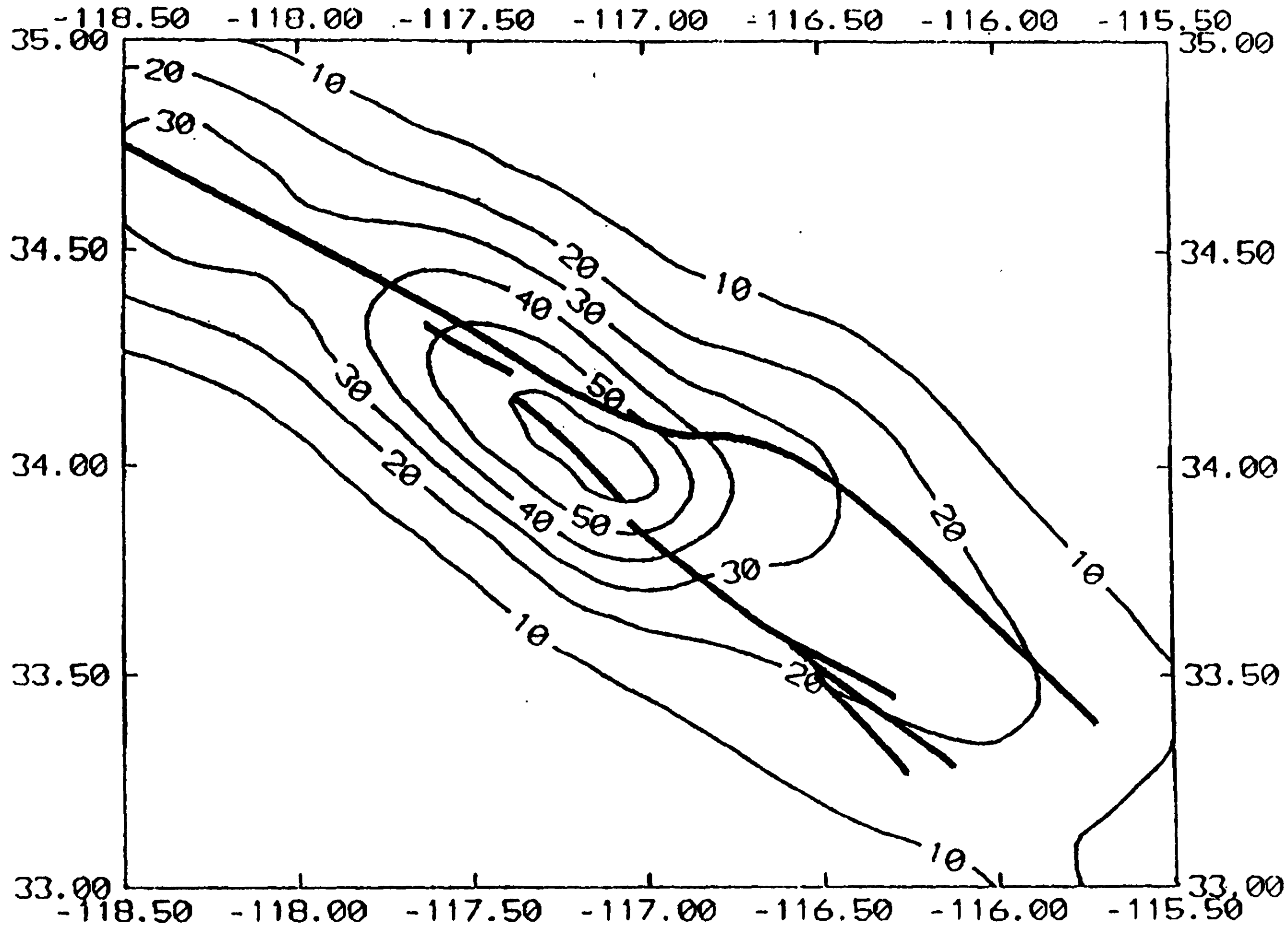


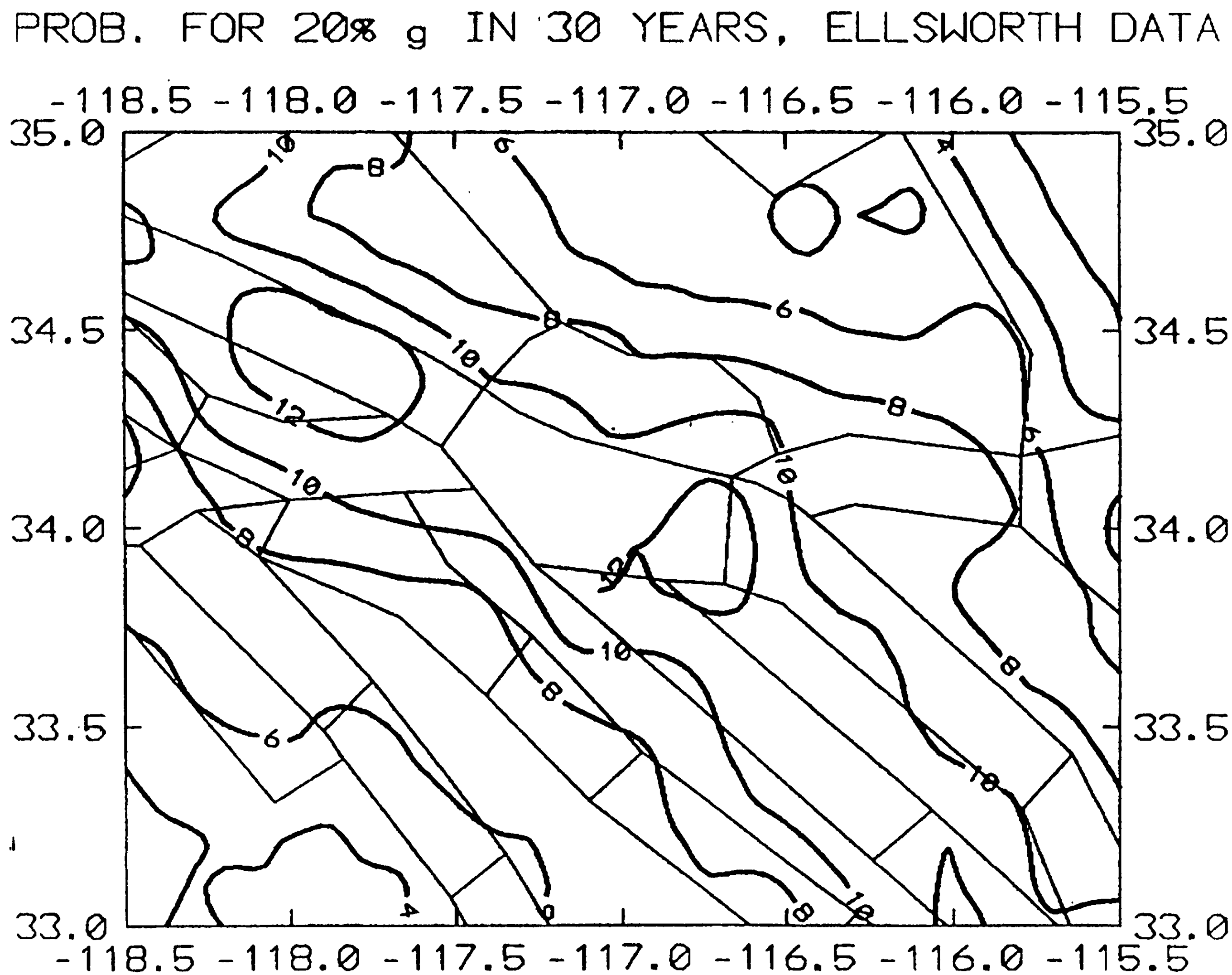

Fig. 8

**Probability of exceedance for 20% g in 30 years due to earthquakes on the San Andreas and San Jacinto faults.**

**Conditional probability (lognormal) for MCE, $\sigma$ = .55**
**Earthquakes with M < MCE are not included**

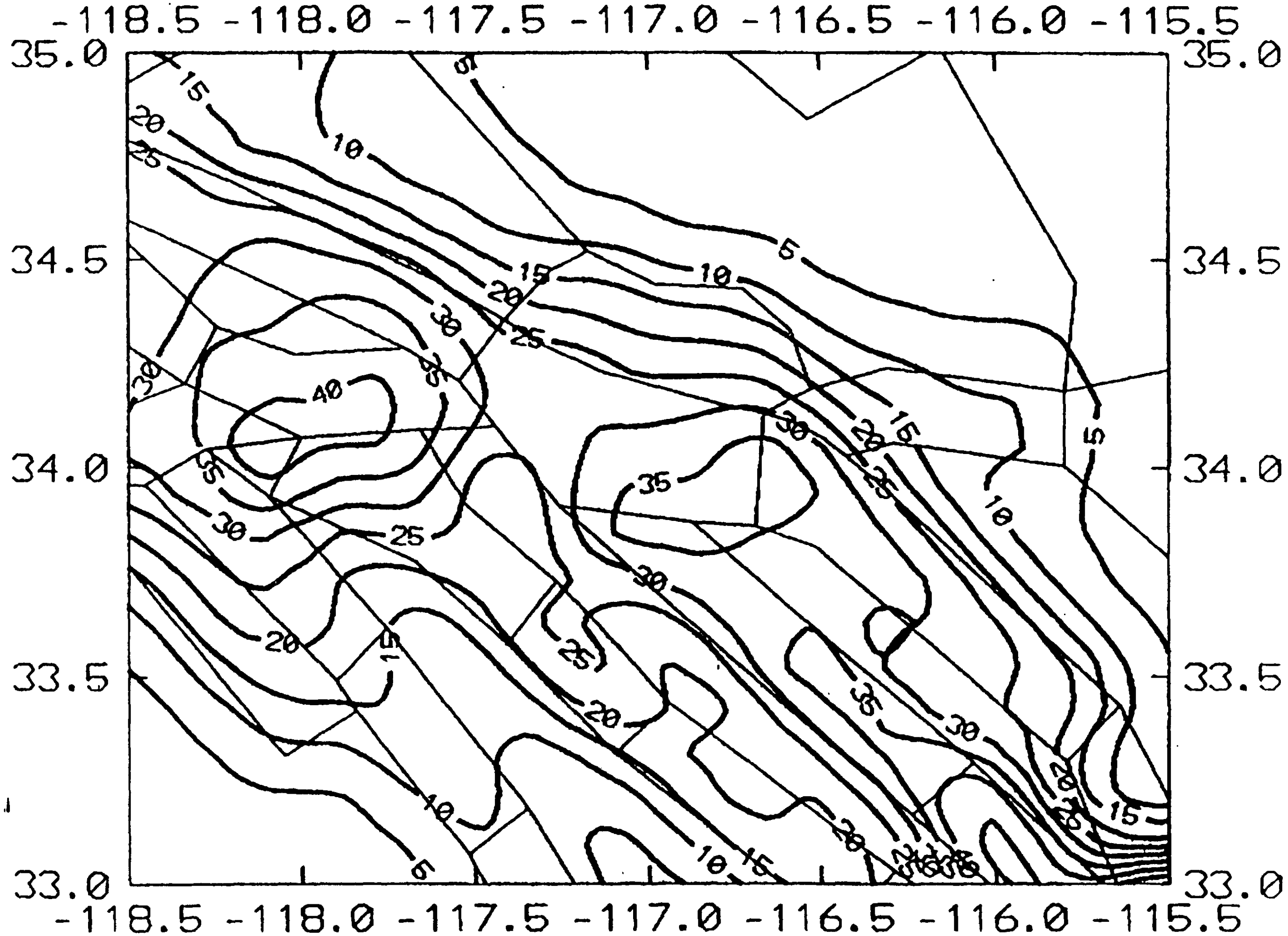
— To be expanded to cover the whole Southern California —

Fig. 9




— To be expanded to cover the whole southern California —

Fig. 10. PROB. FOR 20% g IN 30 YEARS, STRAIN RATE DATA




— to be expanded to cover whole southern California —

FROM : Dr. C. Allin Cornell Portola Valley,CA 94028 PHONE NO. : 415 854 8053



20 '93 11:49AM OEVE USGS MENLO PARK OEVE USGS M

Table 1.

Chap. 4

212, +78, -41 sR
212, +190, -171 p

mean(?) **

Jim: looks like error

| σ input / output | fault/segment | slip rate | slip/event | recurrence int | event dates AD | last event date MRE |
|---|---|---|---|---|---|---|
| | **SAN ANDR** | | | | | |
| (.11) .37 | Carrizo | 33.0±2.0 | 7.0 +2,-1 | 212 +190,-171sr<br>212 +78,-91 p | 1857, 1505, 1367,1231, 1001 | 1857 |
| (.2) .33 | Mojave | 30±5 | 4.0 +2,-1 | 133 +107,-47 sr<br>104 +26,-59p | dates from Ray | 1857 |
| (.18) .35 | San Bern Mt | 24±5 | 3.5±1.0 | 145+145,-60 | 1812, 1690 | 1812 |
| ? | Coachella | 24±5 | -- | 223+73,-57 p | 1020, 1300, 1450, 1690 | 1690 |
| | | | | | | |
| | **SAN JAC** | | | | | |
| (.28) .40 | San Bern V | *12±6* | *1.2±0.3* | *100+150,-50 sr* | -- | 1890? |
| (.27) .39 | San Jac V | *12±6* | *1.0±0.2* | *83+117,-38 sr* | -- | 1918 |
| (.3) .45 | Anza | 12 +7,-5 | *3.0±1.0* | *250+321-145sr*<br>270±50 p | 1210, 1530, 1750 | ≈1750 |
| no MRE | Coyote Cr | 4±1 | *0.7±0.3* | *175+158,-95 sr* | -- | -- |
| (.15) .31 | Borrego Mtn | 4±1 | 0.7±0.1 | 175+91,-55 sr | -- | 1968 |
| (.19) .37 | Super Hills | 4±2 | 1.0±0.3 | 250+225,-134 sr<br>225±75 p | -- | 1987 |
| (.15) .35 | Super Mts | 4±1 | 2.0±0.3 | 300+266,-160sr | 1430,≈1300 | ~1430 |
| | | | | | | |
| | **ELSINORE** | | | | | |
| (.11) .31 | Whittier | 2.5±0.5 | 1.9±0.2 | 760+290,-193sr | -- | 1300 min |
| (.29) .43 | Glen Ivy | 5±2 | 0.7±0.3 | 140+190,-82 sr<br>212±? p | post-1660, 1360-1660, ≈1300,1260, ≈1060 | 1910 |
| (.24) .39 | Temecula | 5±2 | *1.2±0.3* | *240+260,-111 sr* | -- | 1818 hist min |
| (.21) .37 | Julian | 5±2 | *1.7±*[illegible] 0.2 | *340+290,-125 sr* | -- | 1892 hist min |
| (.27) .41 | Coyote Mts | 4±2 | 2.7±0.5 | 675+925,308sr<br>800±300p | ≤1600, ≈1000, ~50 | 1892 hist min |

***data point in italics:**
a)slip rates. No actual rates for this segment. Rate is extrapolated from adjacent segment(s).
b)slip/event. No independent geologic observations. Value based on new length/M, displacement/M, and length/displacement regressions of Wells and Coppersmith (in review, BSSA).
c)recurrence interval. Calculated value when slip rate and/or slip per event are not independent geologic observations.

****recurrence interval:**
sr=recurence calculated using slip rate and slip/event. +,- is range using end-member uncertainties; p=paleoseismologically derived recurrence. ?

Table 2.

Working group 93 (WG)

Dieterich

Preliminary probabilities using prior WG methodology

| Segment | Tbar | Cond. Time | CV-p | CV-I | P 5yr | P 30yr | 88 WG | P 50yr |
|---|---|---|---|---|---|---|---|---|
| **SAN ANDREAS** | | | | | | | | |
| Carrizo | 212 | 136.7 | 0.11 | 0.5 | 0.03 | 0.15 | .1 | 0.26 |
| Mojave | 133 | 136.7 | 0.20 | 0.5 | 0.06 | 0.30 | .3 | 0.45 |
| San Bernardino Mtns | 145 | 181.5 | 0.18 | 0.5 | 0.06 | 0.29 | .2 | 0.44 |
| Coachella | 223 | 313.5 | 0.15 | 0.5 | 0.04 | 0.21 | .4 | 0.32 |
| **SAN JACINTO** | | | | | | | | |
| San Bernardino Valley | 100 | 103 | 0.28 | 0.5 | 0.07 | 0.36 | .2 | 0.52 |
| San Jacinto Valley | 83 | 85 | 0.27 | 0.5 | 0.08 | 0.42 | .1 | 0.60 |
| Anza | 250 | 163 | 0.30 | 0.5 | 0.02 | 0.13 | .3 | 0.21 |
| Coyote Cr | | | | | | | | |
| Borrego | 175 | 25 | 0.15 | 0.5 | <.01 | 0.01 | <.1 | 0.05 |
| Superstition Hills | 250 | 6 | 0.19 | 0.5 | <.01 | <.01 | | <.01 |
| Superstition Mtns | 500 | 563 | 0.15 | 0.5 | 0.02 | 0.09 | | 0.15 |
| **ELSINORE** | | | | | | | | |
| Whittier | 760 | 693 | 0.11 | 0.5 | 0.01 | 0.06 | | 0.09 |
| Glen Ivy | 140 | 83 | 0.29 | 0.5 | 0.03 | 0.21 | | 0.35 |
| Temecula | 240 | [175] | 0.24 | 0.5 | 0.02 | 0.15 | | 0.24 |
| Julian | 340 | [101] | 0.21 | 0.5 | <.01 | 0.03 | | 0.06 |
| Coyote Mtns | 670 | [101] | 0.27 | 0.5 | <.01 | <.01 | | <.01 |

Toppozada 1850-1992 Table 3-a

| Zone No. | | Area km^2 | Zone name | Probability M>6.0 eq/year | Return M>7.0 | Moment rate 10^15Nm/y | Moment rate per km^2 10^15Nm/(y*km^2) |
|---|---|---|---|---|---|---|---|
| 1 | 11 | 881 | San Andreas Fault-Parkfield | 0.0019 | 5298.8 | 44.9 | 0.0510 |
| 2 | 13 | 1045 | San Andreas Fault-Cholame | 0.0027 | 3744.9 | 63.6 | 0.0609 |
| 3 | 35 | 2833 | San Andreas Fault-Carrizo | 0.0069 | 1441.0 | 165.3 | 0.0583 |
| 4 | 28 | 2279 | San Andreas Fault-Mojave | 0.0051 | 1969.5 | 120.9 | 0.0531 |
| 5 | 32 | 2618 | San Andreas Fault-San Bernardi | 0.0061 | 1631.8 | 146.0 | 0.0558 |
| 6 | 34 | 2793 | San Andreas Fault-Coachella | 0.0043 | 2324.0 | 102.5 | 0.0367 |
| 7 | 29 | 2387 | San Jacinto Fault-Clark | 0.0047 | 2121.2 | 112.3 | 0.0470 |
| 8 | 16 | 1325 | San Jacinto Fault-South | 0.0028 | 3608.2 | 66.0 | 0.0498 |
| 9 | 20 | 1640 | Whittier-Chino-Glen Ivy Faults | 0.0026 | 3871.0 | 61.5 | 0.0375 |
| 10 | 10 | 823 | Elsinore Fault-Wildomar | 0.0012 | 8495.6 | 28.0 | 0.0341 |
| 11 | 16 | 1323 | Elsinore Fault-C | 0.0017 | 5847.2 | 40.7 | 0.0308 |
| 12 | 14 | 1162 | Elsinore Fault-D | 0.0018 | 5512.3 | 43.2 | 0.0372 |
| 13 | 16 | 1313 | Newport-Inglewood Fault | 0.0017 | 5791.3 | 41.1 | 0.0313 |
| 14 | 14 | 1155 | Newport-Inglewood-Rose Canyon | 0.0013 | 7880.1 | 30.2 | 0.0262 |
| 15 | 17 | 1411 | Rose Canyon Fault | 0.0012 | 8603.6 | 27.7 | 0.0196 |
| 16 | 17 | 1413 | Imperial Fault | 0.0021 | 4687.9 | 50.8 | 0.0359 |
| 17 | 30 | 2423 | White Wolf Fault | 0.0053 | 1879.7 | 126.7 | 0.0523 |
| 18 | 7 | 568 | Big Pine Fault | 0.0011 | 9448.4 | 25.2 | 0.0444 |
| 19 | 21 | 1697 | Garlock Fault-west | 0.0028 | 3618.5 | 65.8 | 0.0388 |
| 20 | 35 | 2813 | Garlock Fault-east | 0.0023 | 4356.3 | 54.7 | 0.0194 |
| 21 | 15 | 1226 | Pinto Mountain Fault | 0.0016 | 6147.4 | 38.7 | 0.0316 |
| 22 | 12 | 992 | Brawley Seismic Zone | 0.0013 | 7626.7 | 31.2 | 0.0315 |
| 23 | 20 | 1634 | Sierra Madre Fault | 0.0030 | 3389.5 | 70.3 | 0.0430 |
| 24 | 15 | 1221 | San Gabriel Fault | 0.0019 | 5214.0 | 45.7 | 0.0374 |
| 25 | 18 | 1473 | Hollywood-Santa Monica-Malibu | 0.0017 | 5845.6 | 40.7 | 0.0277 |
| 26 | 21 | 1727 | Palos Verdes Fault | 0.0019 | 5390.3 | 44.2 | 0.0256 |
| 27 | 7 | 573 | Santa Monica Mts.-Channel Isla | 0.0006 | 17225.3 | 13.8 | 0.0241 |
| 28 | 13 | 1064 | Eastern Channel Islands | 0.0009 | 10904.9 | 21.8 | 0.0205 |
| 29 | 16 | 1309 | Western Channel Islands | 0.0009 | 11010.9 | 21.6 | 0.0165 |
| 30 | 30 | 2404 | Rinconada Fault | 0.0028 | 3622.1 | 65.8 | 0.0274 |
| 31 | 42 | 3384 | Hosgri Fault | 0.0030 | 3357.4 | 70.9 | 0.0210 |
| 32 | 55 | 4473 | Santa Ynez Fault System | 0.0058 | 1709.4 | 139.3 | 0.0311 |
| 33 | 26 | 2082 | Sierra Nevada Fault | 0.0023 | 4365.2 | 54.6 | 0.0262 |
| 34 | 25 | 2039 | San Bernardino Mts. Region | 0.0038 | 2652.9 | 89.8 | 0.0440 |
| 35 | 64 | 5191 | West Mojave Block | 0.0083 | 1201.7 | 198.2 | 0.0382 |
| 36 | 157 | 12736 | Central Mojave Block | 0.0159 | 628.4 | 379.0 | 0.0298 |
| 37 | 24 | 1977 | Salton Trough | 0.0032 | 3118.6 | 76.4 | 0.0386 |
| 38 | 18 | 1467 | Ventura Basin | 0.0020 | 5019.2 | 47.5 | 0.0323 |
| 39 | 61 | 4920 | Northeast Mojave Block | 0.0038 | 2604.6 | 91.4 | 0.0186 |
| 40 | 27 | 2161 | Coso Region | 0.0019 | 5258.1 | 45.3 | 0.0210 |
| 41 | 13 | 1060 | San Gabriel Mts. | 0.0019 | 5347.5 | 44.5 | 0.0420 |
| 42 | 36 | 2894 | Central Coast Range | 0.0052 | 1911.8 | 124.6 | 0.0430 |
| 43 | 37 | 2972 | Central Coast | 0.0029 | 3473.5 | 68.6 | 0.0231 |
| 44 | 57 | 4578 | Great Valley-Western Hills | 0.0079 | 1267.0 | 188.0 | 0.0411 |
| 45 | 71 | 5695 | Great Valley | 0.0080 | 1249.7 | 190.6 | 0.0335 |
| 46 | 22 | 1797 | Simi Hills-San Fernando Valley | 0.0022 | 4630.6 | 51.4 | 0.0286 |
| 47 | 46 | 3754 | Santa Barbara Channel | 0.0036 | 2804.2 | 84.9 | 0.0226 |
| 48 | 425 | 35125 | Offshore Islands | 0.0166 | 604.0 | 394.3 | 0.0112 |
| 49 | 159 | 12859 | Central Offshore | 0.0082 | 1217.9 | 195.6 | 0.0152 |
| 50 | 84 | 6952 | Peninsular Ranges | 0.0081 | 1240.5 | 192.0 | 0.0276 |
| 51 | 43 | 3544 | Temecula Valley | 0.0061 | 1639.9 | 145.2 | 0.0410 |
| 52 | 79 | 6334 | Southern Sierras | 0.0078 | 1285.7 | 185.2 | 0.0292 |
| 53 | 142 | 11355 | Southern Great Basin | 0.0043 | 2331.0 | 102.2 | 0.0090 |
| 54 | 254 | 20564 | Eastern Mojave | 0.0073 | 1373.1 | 173.5 | 0.0084 |
| 55 | 236 | 19376 | Colorado Corridor | 0.0083 | 1201.5 | 198.2 | 0.0102 |
| 56 | 137 | 11316 | Southeast Corner | 0.0096 | 1038.7 | 229.3 | 0.0203 |
| 57 | 69 | 5588 | Western Transverse Ranges | 0.0075 | 1328.7 | 179.3 | 0.0321 |
| 58 | 2 | 164 | Los Angeles Basin | 0.0002 | 46975.6 | 5.1 | 0.0310 |
| 58 | 2993 | 243883 | So. California, all 58 zones | 0.2417 | 41.4 | 5755.9 | 0.0236 |

Gamma distribution, Mmax = 2.3714E+21 Nm, magmax = 8.25

Toppozada catalog 1850-1992; 1857 earthquake rupture is subdivided into 8 segments (K. Sieh, BSSA, 68, 1421, 1978).

rew 4/25/93

Caltech Catalog 1932 1992 Table 3-6.

| Zone No. | | Area km^2 | Zone name | Probability M>6.0 eq/year | Return M>7.0 | Moment rate 10^15Nm/y | Moment rate per km^2 10^15Nm/(y*km^2) |
|---|---|---|---|---|---|---|---|
| 1 | 11 | 881 | San Andreas Fault-Parkfield | 0.0011 | 9185.9 | 25.9 | 0.0294 |
| 2 | 13 | 1045 | San Andreas Fault-Cholame | 0.0009 | 10972.3 | 21.7 | 0.0208 |
| 3 | 35 | 2833 | San Andreas Fault-Carrizo | 0.0041 | 2458.0 | 96.9 | 0.0342 |
| 4 | 28 | 2279 | San Andreas Fault-Mojave | 0.0036 | 2752.9 | 86.5 | 0.0380 |
| 5 | 32 | 2618 | San Andreas Fault-San Bernardi | 0.0053 | 1874.2 | 127.1 | 0.0485 |
| 6 | 34 | 2793 | San Andreas Fault-Coachella | 0.0064 | 1569.7 | 151.7 | 0.0543 |
| 7 | 29 | 2387 | San Jacinto Fault-Clark | 0.0051 | 1964.2 | 121.3 | 0.0508 |
| 8 | 16 | 1325 | San Jacinto Fault-South | 0.0036 | 2806.6 | 84.9 | 0.0640 |
| 9 | 20 | 1640 | Whittier-Chino-Glen Ivy Faults | 0.0029 | 3466.2 | 68.7 | 0.0419 |
| 10 | 10 | 823 | Elsinore Fault-Wildomar | 0.0012 | 8177.1 | 29.1 | 0.0354 |
| 11 | 16 | 1323 | Elsinore Fault-C | 0.0021 | 4775.0 | 49.9 | 0.0377 |
| 12 | 14 | 1162 | Elsinore Fault-D | 0.0021 | 4847.4 | 49.1 | 0.0423 |
| 13 | 16 | 1313 | Newport-Inglewood Fault | 0.0029 | 3419.1 | 69.7 | 0.0530 |
| 14 | 14 | 1155 | Newport-Inglewood-Rose Canyon | 0.0015 | 6725.9 | 35.4 | 0.0307 |
| 15 | 17 | 1411 | Rose Canyon Fault | 0.0015 | 6573.0 | 36.2 | 0.0257 |
| 16 | 17 | 1413 | Imperial Fault | 0.0027 | 3705.5 | 64.3 | 0.0455 |
| 17 | 30 | 2423 | White Wolf Fault | 0.0057 | 1758.4 | 135.4 | 0.0559 |
| 18 | 7 | 568 | Big Pine Fault | 0.0007 | 13890.7 | 17.1 | 0.0302 |
| 19 | 21 | 1697 | Garlock Fault-west | 0.0028 | 3561.0 | 66.9 | 0.0394 |
| 20 | 35 | 2813 | Garlock Fault-east | 0.0024 | 4114.0 | 57.9 | 0.0206 |
| 21 | 15 | 1226 | Pinto Mountain Fault | 0.0031 | 3257.4 | 73.1 | 0.0596 |
| 22 | 12 | 992 | Brawley Seismic Zone | 0.0024 | 4106.5 | 58.0 | 0.0585 |
| 23 | 20 | 1634 | Sierra Madre Fault | 0.0030 | 3326.6 | 71.6 | 0.0438 |
| 24 | 15 | 1221 | San Gabriel Fault | 0.0021 | 4695.5 | 50.7 | 0.0415 |
| 25 | 18 | 1473 | Hollywood-Santa Monica-Malibu | 0.0022 | 4581.2 | 52.0 | 0.0353 |
| 26 | 21 | 1727 | Palos Verdes Fault | 0.0028 | 3542.4 | 67.2 | 0.0389 |
| 27 | 7 | 573 | Santa Monica Mts.-Channel Isla | 0.0006 | 15746.6 | 15.1 | 0.0264 |
| 28 | 13 | 1064 | Eastern Channel Islands | 0.0009 | 11236.7 | 21.2 | 0.0199 |
| 29 | 16 | 1309 | Western Channel Islands | 0.0007 | 14808.1 | 16.1 | 0.0123 |
| 30 | 30 | 2404 | Rinconada Fault | 0.0016 | 6144.3 | 38.8 | 0.0161 |
| 31 | 42 | 3384 | Hosgri Fault | 0.0016 | 6074.3 | 39.2 | 0.0116 |
| 32 | 55 | 4473 | Santa Ynez Fault System | 0.0039 | 2568.0 | 92.7 | 0.0207 |
| 33 | 26 | 2082 | Sierra Nevada Fault | 0.0022 | 4497.5 | 53.0 | 0.0254 |
| 34 | 25 | 2039 | San Bernardino Mts. Region | 0.0046 | 2187.3 | 108.9 | 0.0534 |
| 35 | 64 | 5191 | West Mojave Block | 0.0073 | 1363.0 | 174.7 | 0.0337 |
| 36 | 157 | 12736 | Central Mojave Block | 0.0186 | 537.0 | 443.5 | 0.0348 |
| 37 | 24 | 1977 | Salton Trough | 0.0044 | 2286.5 | 104.2 | 0.0527 |
| 38 | 18 | 1467 | Ventura Basin | 0.0017 | 5970.1 | 39.9 | 0.0272 |
| 39 | 61 | 4920 | Northeast Mojave Block | 0.0045 | 2207.4 | 107.9 | 0.0219 |
| 40 | 27 | 2161 | Coso Region | 0.0018 | 5694.9 | 41.8 | 0.0194 |
| 41 | 13 | 1060 | San Gabriel Mts. | 0.0019 | 5279.2 | 45.1 | 0.0426 |
| 42 | 36 | 2894 | Central Coast Range | 0.0024 | 4164.7 | 57.2 | 0.0198 |
| 43 | 37 | 2972 | Central Coast | 0.0018 | 5684.4 | 41.9 | 0.0141 |
| 44 | 57 | 4578 | Great Valley-Western Hills | 0.0046 | 2165.8 | 110.0 | 0.0240 |
| 45 | 71 | 5695 | Great Valley | 0.0060 | 1672.1 | 142.4 | 0.0250 |
| 46 | 22 | 1797 | Simi Hills-San Fernando Valley | 0.0022 | 4490.6 | 53.0 | 0.0295 |
| 47 | 46 | 3754 | Santa Barbara Channel | 0.0025 | 3931.3 | 60.6 | 0.0161 |
| 48 | 425 | 35125 | Offshore Islands | 0.0208 | 481.3 | 494.8 | 0.0141 |
| 49 | 159 | 12859 | Central Offshore | 0.0041 | 2457.9 | 96.9 | 0.0075 |
| 50 | 84 | 6952 | Peninsular Ranges | 0.0094 | 1066.9 | 223.2 | 0.0321 |
| 51 | 43 | 3544 | Temecula Valley | 0.0065 | 1540.9 | 154.6 | 0.0436 |
| 52 | 79 | 6334 | Southern Sierras | 0.0075 | 1330.8 | 179.0 | 0.0283 |
| 53 | 142 | 11355 | Southern Great Basin | 0.0042 | 2354.3 | 101.2 | 0.0089 |
| 54 | 254 | 20564 | Eastern Mojave | 0.0110 | 911.4 | 261.3 | 0.0127 |
| 55 | 236 | 19376 | Colorado Corridor | 0.0129 | 773.2 | 308.0 | 0.0159 |
| 56 | 137 | 11316 | Southeast Corner | 0.0146 | 685.5 | 347.5 | 0.0307 |
| 57 | 69 | 5588 | Western Transverse Ranges | 0.0041 | 2431.5 | 97.9 | 0.0175 |
| 58 | 2 | 164 | Los Angeles Basin | 0.0003 | 29601.0 | 8.0 | 0.0491 |
| 58 | 2993 | 243883 | So. California, all 58 zones | 0.2455 | 40.7 | 5848.0 | 0.0240 |

Gamma distribution, Mmax = 2.3714E+21 Nm, magmax = 8.25

Caltech catalog 1932-1992.

Harvard CMT Catalog 1977-1992 Table 3-c

| Zone No. | | Area km^2 | Zone name | Probability M>5.5 eq/year | T-axis Pl | T-axis Az | P-axis Pl | P-axis Az | Rotation angle | Return M>7.0 | Moment rate 10^15Nm/y | Moment rate per km^2 10^15Nm/(y*km^2 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 11 | 881 | San Andreas Fault-Parkfield | 0.0101 | 66 | 190 | 21 | 40 | 30.8 | 3137.0 | 75.9 | 0.0862 |
| 2 | 13 | 1045 | San Andreas Fault-Cholame | 0.0061 | 68 | 160 | 7 | 51 | 35.7 | 5151.8 | 46.2 | 0.0442 |
| 3 | 35 | 2833 | San Andreas Fault-Carrizo | 0.0110 | 78 | 122 | 4 | 15 | 47.7 | 2865.3 | 83.1 | 0.0293 |
| 4 | 28 | 2279 | San Andreas Fault-Mojave | 0.0167 | 8 | 278 | 5 | 187 | 36.3 | 1898.6 | 125.4 | 0.0551 |
| 5 | 32 | 2618 | San Andreas Fault-San Bernardi | 0.0431 | 9 | 282 | 4 | 192 | 30.5 | 733.6 | 324.7 | 0.1240 |
| 6 | 34 | 2793 | San Andreas Fault-Coachella | 0.0445 | 8 | 284 | 4 | 193 | 32.2 | 710.9 | 335.0 | 0.1200 |
| 7 | 29 | 2387 | San Jacinto Fault-Clark | 0.0394 | 12 | 277 | 5 | 186 | 34.9 | 802.9 | 296.6 | 0.1243 |
| 8 | 16 | 1325 | San Jacinto Fault-South | 0.0269 | 9 | 93 | 4 | 2 | 24.0 | 1176.5 | 202.4 | 0.1528 |
| 9 | 20 | 1640 | Whittier-Chino-Glen Ivy Faults | 0.0173 | 23 | 278 | 6 | 186 | 40.4 | 1825.2 | 130.5 | 0.0796 |
| 10 | 10 | 823 | Elsinore Fault-Wildomar | 0.0092 | 8 | 279 | 3 | 189 | 37.0 | 3440.4 | 69.2 | 0.0841 |
| 11 | 16 | 1323 | Elsinore Fault-C | 0.0159 | 1 | 98 | 1 | 188 | 31.7 | 1983.2 | 120.1 | 0.0908 |
| 12 | 14 | 1162 | Elsinore Fault-D | 0.0193 | 10 | 101 | 0 | 191 | 29.0 | 1636.2 | 145.6 | 0.1253 |
| 13 | 16 | 1313 | Newport-Inglewood Fault | 0.0111 | 51 | 287 | 8 | 186 | 41.7 | 2855.4 | 83.4 | 0.0635 |
| 14 | 14 | 1155 | Newport-Inglewood-Rose Canyon | 0.0093 | 9 | 280 | 3 | 190 | 40.3 | 3413.8 | 69.8 | 0.0604 |
| 15 | 17 | 1411 | Rose Canyon Fault | 0.0120 | 11 | 102 | 2 | 11 | 42.4 | 2644.2 | 90.1 | 0.0638 |
| 16 | 17 | 1413 | Imperial Fault | 0.0166 | 12 | 91 | 7 | 359 | 27.2 | 1901.6 | 125.2 | 0.0886 |
| 17 | 30 | 2423 | White Wolf Fault | 0.0087 | 16 | 277 | 6 | 8 | 50.4 | 3649.7 | 65.3 | 0.0269 |
| 18 | 7 | 568 | Big Pine Fault | 0.0020 | 83 | 135 | 5 | 6 | 53.2 | 15573.7 | 15.3 | 0.0269 |
| 19 | 21 | 1697 | Garlock Fault-west | 0.0064 | 1 | 280 | 4 | 190 | 33.0 | 4911.1 | 48.5 | 0.0286 |
| 20 | 35 | 2813 | Garlock Fault-east | 0.0127 | 6 | 288 | 9 | 197 | 32.0 | 2485.6 | 95.8 | 0.0341 |
| 21 | 15 | 1226 | Pinto Mountain Fault | 0.0196 | 11 | 291 | 2 | 201 | 36.2 | 1617.0 | 147.3 | 0.1201 |
| 22 | 12 | 992 | Brawley Seismic Zone | 0.0124 | 5 | 93 | 0 | 3 | 27.9 | 2557.9 | 93.1 | 0.0938 |
| 23 | 20 | 1634 | Sierra Madre Fault | 0.0197 | 8 | 272 | 5 | 182 | 42.8 | 1604.5 | 148.4 | 0.0908 |
| 24 | 15 | 1221 | San Gabriel Fault | 0.0073 | 7 | 92 | 4 | 183 | 44.9 | 4336.7 | 54.9 | 0.0450 |
| 25 | 18 | 1473 | Hollywood-Santa Monica-Malibu | 0.0120 | 72 | 295 | 7 | 181 | 43.4 | 2641.6 | 90.2 | 0.0612 |
| 26 | 21 | 1727 | Palos Verdes Fault | 0.0106 | 37 | 285 | 6 | 190 | 46.7 | 2972.8 | 80.1 | 0.0464 |
| 27 | 7 | 573 | Santa Monica Mts.-Channel Isla | 0.0023 | 66 | 76 | 6 | 181 | 45.7 | 13634.0 | 17.5 | 0.0305 |
| 28 | 13 | 1064 | Eastern Channel Islands | 0.0037 | 61 | 81 | 7 | 184 | 41.2 | 8611.6 | 27.7 | 0.0260 |
| 29 | 16 | 1309 | Western Channel Islands | 0.0035 | 69 | 85 | 5 | 188 | 44.8 | 9103.8 | 26.2 | 0.0200 |
| 30 | 30 | 2404 | Rinconada Fault | 0.0163 | 80 | 160 | 4 | 44 | 42.8 | 1936.6 | 123.0 | 0.0512 |
| 31 | 42 | 3384 | Hosgri Fault | 0.0179 | 81 | 308 | 5 | 69 | 38.4 | 1764.1 | 135.0 | 0.0399 |
| 32 | 55 | 4473 | Santa Ynez Fault System | 0.0145 | 84 | 133 | 3 | 13 | 49.5 | 2178.3 | 109.3 | 0.0244 |
| 33 | 26 | 2082 | Sierra Nevada Fault | 0.0087 | 2 | 276 | 1 | 6 | 39.6 | 3642.6 | 65.4 | 0.0314 |
| 34 | 25 | 2039 | San Bernardino Mts. Region | 0.0323 | 8 | 285 | 5 | 194 | 26.2 | 977.8 | 243.6 | 0.1194 |
| 35 | 64 | 5191 | West Mojave Block | 0.0320 | 9 | 281 | 5 | 190 | 30.0 | 989.3 | 240.7 | 0.0464 |
| 36 | 157 | 12736 | Central Mojave Block | 0.1122 | 10 | 289 | 10 | 197 | 27.3 | 281.8 | 845.3 | 0.0664 |
| 37 | 24 | 1977 | Salton Trough | 0.0327 | 8 | 276 | 4 | 185 | 32.5 | 966.2 | 246.5 | 0.1247 |
| 38 | 18 | 1467 | Ventura Basin | 0.0058 | 54 | 86 | 4 | 181 | 47.4 | 5476.6 | 43.5 | 0.0296 |
| 39 | 61 | 4920 | Northeast Mojave Block | 0.0272 | 10 | 291 | 12 | 199 | 29.5 | 1160.8 | 205.2 | 0.0417 |
| 40 | 27 | 2161 | Coso Region | 0.0091 | 3 | 278 | 8 | 188 | 37.4 | 3463.3 | 68.8 | 0.0318 |
| 41 | 13 | 1060 | San Gabriel Mts. | 0.0101 | 15 | 92 | 4 | 183 | 42.8 | 3142.9 | 75.8 | 0.0715 |
| 42 | 36 | 2894 | Central Coast Range | 0.0169 | 74 | 159 | 7 | 44 | 37.8 | 1876.0 | 127.0 | 0.0439 |
| 43 | 37 | 2972 | Central Coast | 0.0174 | 85 | 287 | 3 | 60 | 41.7 | 1817.4 | 131.0 | 0.0441 |
| 44 | 57 | 4578 | Great Valley-Western Hills | 0.0244 | 75 | 167 | 10 | 36 | 46.4 | 1296.9 | 183.6 | 0.0401 |
| 45 | 71 | 5695 | Great Valley | 0.0234 | 60 | 248 | 14 | 3 | 63.1 | 1353.1 | 176.0 | 0.0309 |
| 46 | 22 | 1797 | Simi Hills-San Fernando Valley | 0.0091 | 70 | 74 | 6 | 181 | 48.5 | 3461.6 | 68.8 | 0.0383 |
| 47 | 46 | 3754 | Santa Barbara Channel | 0.0111 | 82 | 105 | 1 | 9 | 49.9 | 2858.1 | 83.3 | 0.0222 |
| 48 | 425 | 35125 | Offshore Islands | 0.1348 | 32 | 95 | 3 | 187 | 39.2 | 234.6 | 1015.2 | 0.0289 |
| 49 | 159 | 12859 | Central Offshore | 0.0401 | 88 | 326 | 0 | 61 | 39.7 | 788.1 | 302.2 | 0.0235 |
| 50 | 84 | 6952 | Peninsular Ranges | 0.0705 | 4 | 101 | 4 | 191 | 36.5 | 448.6 | 530.9 | 0.0764 |
| 51 | 43 | 3544 | Temecula Valley | 0.0508 | 2 | 278 | 2 | 187 | 32.0 | 622.4 | 382.6 | 0.1080 |
| 52 | 79 | 6334 | Southern Sierras | 0.0252 | 4 | 272 | 15 | 3 | 48.8 | 1252.4 | 190.2 | 0.0300 |
| 53 | 142 | 11355 | Southern Great Basin | 0.0321 | 4 | 288 | 13 | 197 | 39.5 | 985.6 | 241.6 | 0.0213 |
| 54 | 254 | 20564 | Eastern Mojave | 0.0484 | 13 | 293 | 4 | 202 | 34.3 | 653.2 | 364.6 | 0.0177 |
| 55 | 236 | 19376 | Colorado Corridor | 0.0593 | 1 | 280 | 4 | 190 | 30.3 | 533.6 | 446.3 | 0.0230 |
| 56 | 137 | 11316 | Southeast Corner | 0.0812 | 3 | 97 | 3 | 187 | 29.6 | 389.6 | 611.3 | 0.0540 |
| 57 | 69 | 5588 | Western Transverse Ranges | 0.0193 | 83 | 174 | 5 | 42 | 40.8 | 1641.8 | 145.1 | 0.0260 |
| 58 | 2 | 164 | Los Angeles Basin | 0.0023 | 70 | 301 | 11 | 180 | 33.7 | 13637.3 | 17.5 | 0.1066 |
| 58 | 2993 | 243883 | So. California, all 58 zones | 1.4144 | 4 | 282 | 2 | 192 | 35.8 | 22.4 | 10652.8 | 0.0437 |

mma distribution, Mmax = 2.3714E+21 Nm, magmax = 8.25

rvard CMT catalog 1977-1992.

Table 3-d.



| Box | Area | Strain rate | $L_{max}$ | Moment rate | $M_{max}$ | Bval | Recurrence Interval $M >$ 6.0 | 6.5 | 7.0 | 7.5 | 8.0 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| SAF-Parkfield | 731 | 61 | 294 | 43 | 8.09 | 1.07 | 76 | 264 | 961 | 4074 | 55429 |
| SAF-Cholame | 1101 | 61 | 443 | 58 | 8.09 | 0.98 | 59 | 185 | 606 | 2315 | 28399 |
| SAF-Carrizo | 2882 | 61 | 1160 | 142 | 8.05 | 0.59 | 46 | 93 | 196 | 491 | 6402 |
| SAF-Mojave | 2313 | 49 | 748 | 117 | 8.03 | 0.74 | 51 | 123 | 309 | 931 | 23060 |
| SAF-San Bernardino | 2570 | 49 | 831 | 88 | 8.02 | 0.68 | 51 | 114 | 269 | 761 | 25828 |
| SAF-Coachella | 2651 | 49 | 857 | 123 | 8.02 | 0.77 | 42 | 103 | 268 | 843 | 31718 |
| Brawley Seismic Zone | 985 | 52 | 0 | 53 | 6.82 | 0.80 | – | – | – | – | – |
| Imperial F. | 1409 | 55 | 512 | 77 | 7.51 | 1.11 | 25 | 96 | 452 | 49200 | – |
| SJF-Clark | 2327 | 20 | 307 | 130 | 7.27 | 0.80 | 34 | 97 | 442 | – | – |
| SJF-South | 1403 | 20 | 185 | 73 | 6.98 | 0.80 | 38 | 126 | – | – | – |
| Whittier-Chino | 1814 | 13 | 156 | 80 | 7.27 | 0.80 | 68 | 194 | 867 | – | – |
| Elsinore-Wildomar | 876 | 11 | 64 | 49 | 7.05 | 0.80 | 121 | 384 | 6194 | – | – |
| Elsinore-C | 1546 | 10 | 102 | 82 | 7.28 | 0.80 | 105 | 300 | 1308 | – | – |
| Elsinore-D | 1048 | 10 | 69 | 57 | 7.12 | 0.80 | 123 | 374 | 2879 | – | – |
| Nwpt-Inglwd | 1434 | 10 | 95 | 75 | 7.25 | 0.80 | 108 | 310 | 1466 | – | – |
| Nwpt-Inglwd/Rose Cyn | 1194 | 6 | 47 | 64 | 7.18 | 0.80 | 194 | 575 | 3414 | – | – |
| Rose Cyn | 1498 | 4 | 40 | 79 | 7.27 | 0.80 | 266 | 761 | 3419 | – | – |
| White Wolf F | 2451 | 6 | 97 | 89 | 7.32 | 0.80 | 117 | 330 | 1340 | – | – |
| Big Pine F | 592 | 9 | 35 | 48 | 7.04 | 0.80 | 216 | 690 | 12858 | – | – |
| Garlock-W | 1607 | 6 | 64 | 98 | 7.37 | 0.80 | 190 | 530 | 2018 | – | – |
| Garlock-E | 3208 | 3 | 64 | 160 | 7.59 | 0.80 | 269 | 717 | 2226 | 22011 | – |
| Pinto Mt | 1351 | 5 | 45 | 81 | 7.28 | 0.80 | 239 | 681 | 3005 | – | – |
| Sierra Madre F | 1464 | 17 | 164 | 84 | 7.30 | 0.80 | 66 | 189 | 805 | – | – |
| San Gabriel F | 1342 | 10 | 89 | 84 | 7.30 | 0.80 | 123 | 350 | 1494 | – | – |
| Hollywood | 1414 | 15 | 140 | 89 | 7.32 | 0.80 | 81 | 228 | 930 | – | – |
| Palos Verdes | 1782 | 8 | 94 | 99 | 7.37 | 0.80 | 130 | 362 | 1364 | – | – |
| Sta Monica Mts | 580 | 7 | 27 | 40 | 6.96 | 0.80 | 252 | 865 | – | – | – |
| E Channel Is | 1201 | 6 | 48 | 64 | 7.17 | 0.80 | 191 | 569 | 3437 | – | – |
| W Channel Is | 1250 | 4 | 33 | 62 | 7.16 | 0.80 | 271 | 811 | 5142 | – | – |
| Rinconada | 2418 | 4 | 64 | 126 | 7.48 | 0.80 | 226 | 613 | 2060 | – | – |
| Hosgri | 3429 | 1 | 23 | 173 | 7.63 | 0.80 | 797 | 2113 | 6435 | 49155 | – |
| Santa Ynez | 4168 | 11 | 303 | 179 | 7.64 | 0.80 | 61 | 162 | 488 | 3412 | – |
| Sierra Nevada | 1941 | 3 | 38 | 102 | 7.39 | 0.80 | 326 | 901 | 3338 | – | – |
| San Bernardino Mts | 1995 | 7 | 92 | 84 | 7.30 | 0.80 | 119 | 336 | 1433 | – | – |
| W Mojave Blk | 5464 | 4 | 144 | 141 | 7.53 | 0.80 | 108 | 291 | 939 | 22858 | – |
| Cent Mojave Blk | 12672 | 2 | 167 | 224 | 7.70 | 0.80 | 121 | 318 | 936 | 5099 | – |
| Salton Trough | 2059 | 3 | 41 | 154 | 7.57 | 0.80 | 407 | 1086 | 3414 | 41442 | – |
| Ventura Basin | 1481 | 12 | 117 | 81 | 7.28 | 0.80 | 91 | 259 | 1143 | – | – |
| NE Mojave Blk | 4662 | 2 | 62 | 112 | 7.43 | 0.80 | 216 | 593 | 2096 | – | – |
| Coso | 2065 | 3 | 41 | 93 | 7.34 | 0.80 | 286 | 802 | 3157 | – | – |
| San Gabriel Mts | 894 | 12 | 71 | 96 | 7.36 | 0.80 | 169 | 471 | 1818 | – | – |
| Cent Coast Rng | 2954 | 6 | 117 | 216 | 7.70 | 0.80 | 173 | 455 | 1338 | 7291 | – |
| Cent Coast | 2942 | 2 | 39 | 144 | 7.54 | 0.80 | 409 | 1097 | 3511 | 66479 | – |
| Great Vly-W | 4436 | 5 | 146 | 154 | 7.57 | 0.80 | 114 | 303 | 952 | 11299 | – |
| Great Vly | 5377 | 0 | 0 | 126 | 7.48 | 0.80 | – | – | – | – | – |
| Simi Hills | 1904 | 11 | 138 | 104 | 7.40 | 0.80 | 92 | 253 | 928 | – | – |
| Sta Barbara Chan | 3980 | 8 | 210 | 146 | 7.55 | 0.80 | 76 | 204 | 651 | 11237 | – |
| Offshore Is | 18915 | 2 | 250 | 299 | 7.70 | 0.80 | 81 | 213 | 627 | 3416 | – |
| Offshore Is-W | 16808 | 0 | 0 | 281 | 7.70 | 0.80 | – | – | – | – | – |
| Cent Offshore | 12285 | 0 | 0 | 262 | 7.70 | 0.80 | – | – | – | – | – |
| Peninsular Rngs | 6744 | 1 | 45 | 243 | 7.70 | 0.80 | 455 | 1196 | 3517 | 19164 | – |
| Temecula Vly | 3513 | 2 | 46 | 235 | 7.70 | 0.80 | 437 | 1148 | 3376 | 18394 | – |
| S Sierras | 6188 | 0 | 0 | 118 | 7.45 | 0.80 | – | – | – | – | – |
| S Great Basin | 10424 | 2 | 438 | 222 | 7.70 | 0.80 | 147 | 387 | 1138 | 6199 | – |
| E Mojave | 20964 | 1 | 138 | 248 | 7.70 | 0.80 | 146 | 385 | 1131 | 6165 | – |
| Colorado Corridor | 18405 | 1 | 121 | 236 | 7.70 | 0.80 | 167 | 438 | 1289 | 7022 | – |
| SE Corner | 11070 | 2 | 146 | 270 | 7.70 | 0.80 | 139 | 364 | 1071 | 5837 | – |
| W Trans Rngs | 5782 | 5 | 191 | 127 | 7.49 | 0.80 | 76 | 206 | 691 | – | – |
| LA | 187 | 15 | 19 | 26 | 6.76 | 0.80 | 286 | 1309 | – | – | – |
| ALL S CALIF | 242150 | | | 9715 | | | 2 | 5 | 19 | 121 | 3095 |
| ALL S CALIF | 242150 | | | 9715 | 8.04 | -1.04 | 2 | 8 | 27 | 116 | 3096 |

Table 1. Statistical summary of box parameters and seismicity.

**Appendix G**

Illustrations presented to NEPEC by J.Ebel.

NESEC

# New England Earthquake Historical Data

## New England States' Historical Earthquake Record to 1989

| State | Years of Record | Number of Earthquakes |
|---|---|---|
| Connecticut | 1568-1989 | 137 |
| Maine | 1766-1989 | 391 |
| Massachusetts | 1627-1989 | 316 |
| New Hampshire | 1728-1989 | 270 |
| Rhode Island | 1766-1989 | 32 |
| Vermont | 1843-1989 | 60 |
| Total Number of Earthquakes within New England | | 1215 |

Total Number of Earthquakes Within Northeastern North America, 1538 thru 1989 = 4498

## New England Earthquakes with Magnitudes of 4.5 or Greater, 1924 - 1989

| New England Location | Date | Magnitude |
|---|---|---|
| Ossipee, NH | Dec. 20, 1940 | 5.5 |
| Ossipee, NH | Dec. 24, 1940 | 5.5 |
| Dover-Foxcroft, ME | Dec. 28, 1947 | 4.5 |
| Kingstown, RI | Jun. 10, 1951 | 4.6 |
| Portland, ME | Apr. 26, 1957 | 4.7 |
| NH-Quebec Border | Jun. 14, 1973 | 4.8 |
| Gaza, NH | Jan. 18, 1982 | 4.5 |

## Northeastern Earthquakes with Magnitudes of 5.0 or Greater, 1924 - 1989

| Northeast Location | Date | Magnitude |
|---|---|---|
| La Malbaie, Quebec | Sep. 30, 1924 | 5.5 |
| La Malbaie, Quebec | Mar. 1, 1925 | 6.6 |
| Lake George, NY | Apr. 20, 1931 | 5.0 |
| La Malbaie, Quebec | Jan. 8, 1931 | 5.4 |
| La Malbaie, Quebec | Oct. 19, 1939 | 5.8 |
| La Malbaie, Quebec | Oct. 27, 1939 | 5.2 |
| Miramichi, New Brunswick | Jan. 9, 1981 | 5.8 |
| Miramichi, New Brunswick | Jan. 9, 1981 | 5.1 |
| Miramichi, New Brunswick | Jan. 11, 1981 | 5.5 |
| Goodnow, NY | Oct. 7, 1983 | 5.1 |
| Saguenay, Quebec | Nov. 25, 1988 | 5.9 |

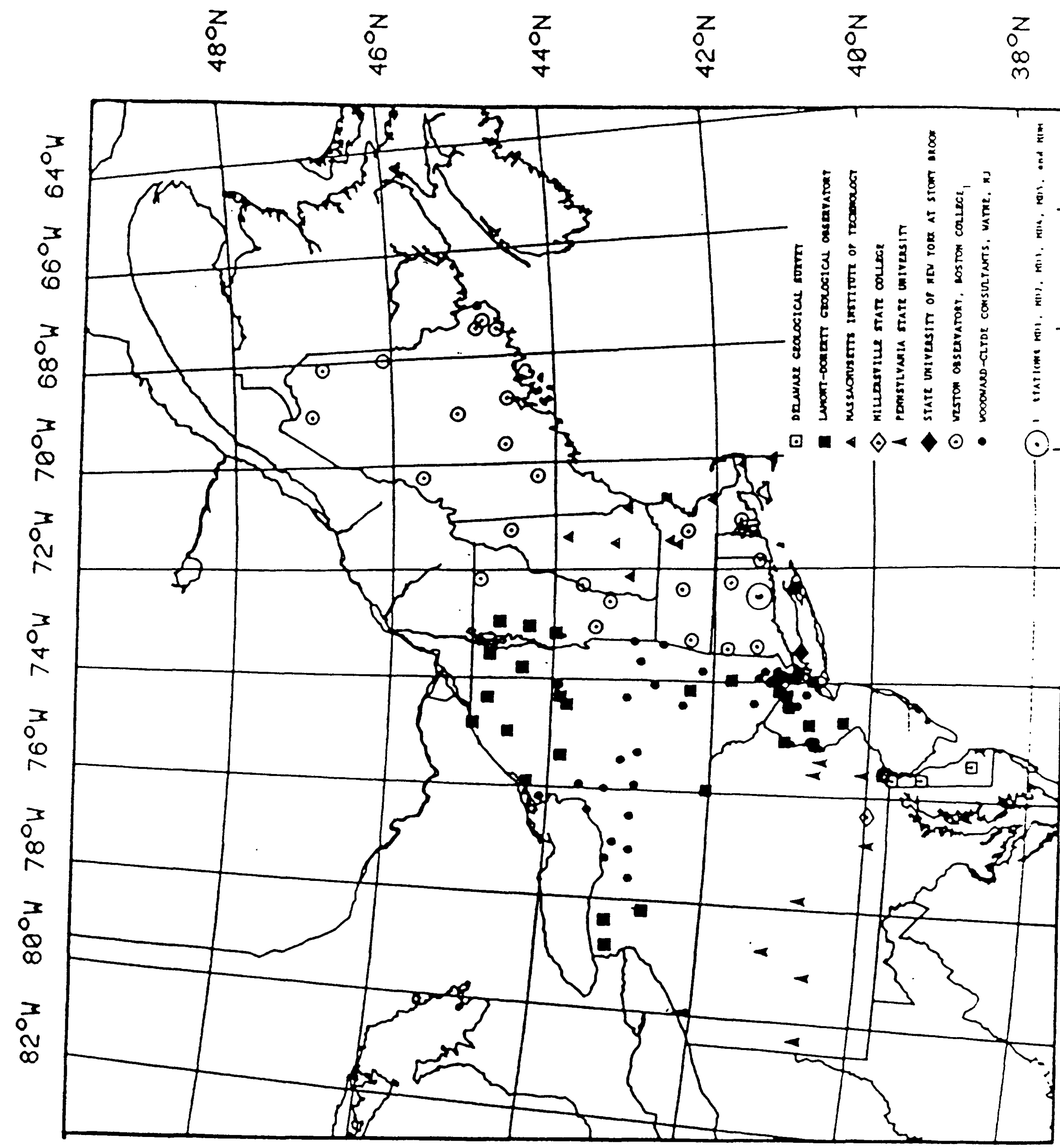
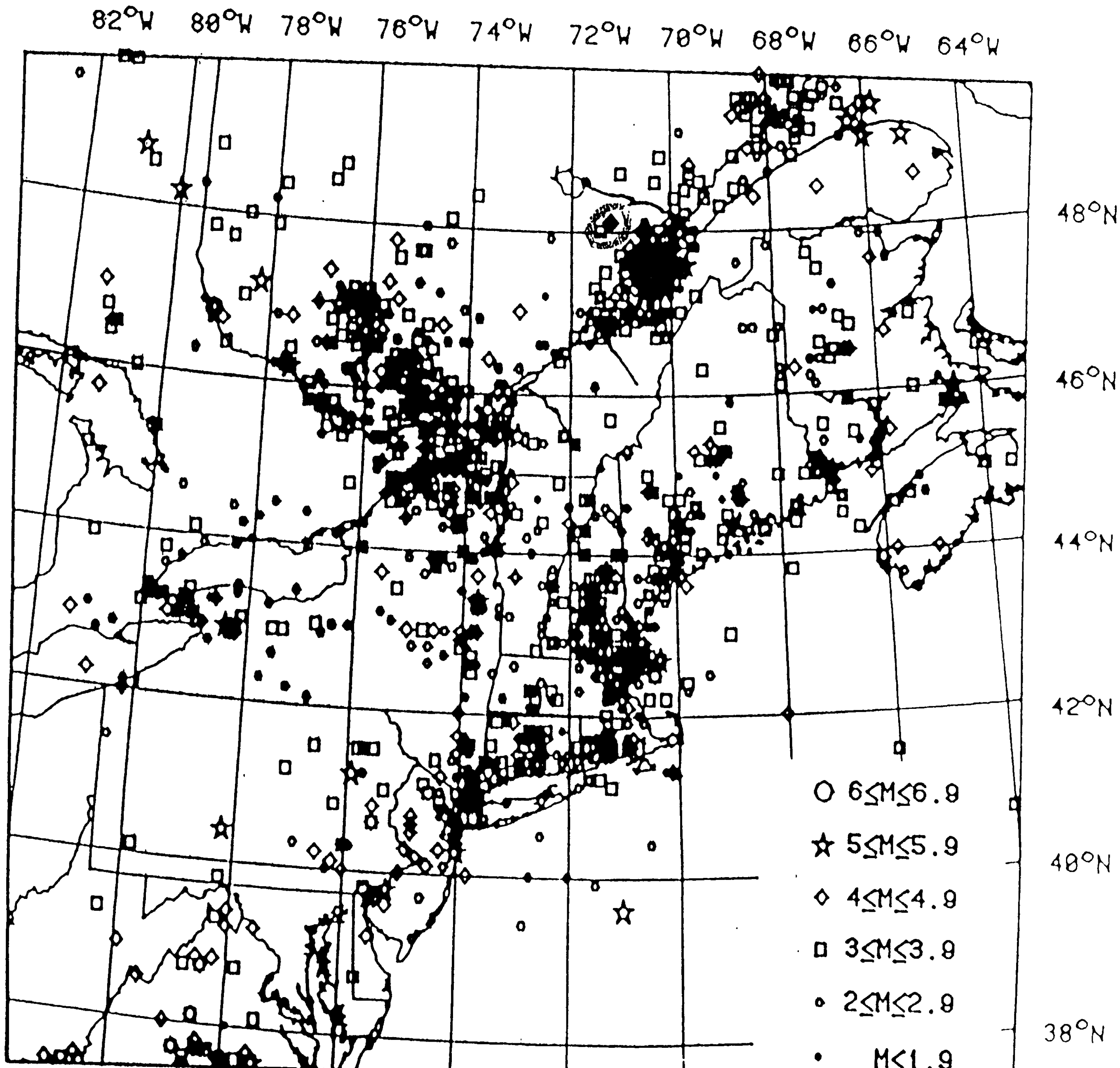



Figure 1. Map of the seismic stations in the northeastern United States in September, 1985.


82°W
80°W
78°W
76°W
74°W
72°W
70°W
68°W
66°W
64°W
48°N
46°N
44°N
42°N
40°N
38°N
6≤M≤6.9
5≤M≤5.9
4≤M≤4.9
3≤M≤3.9
2≤M≤2.9
M≤1.9

1534-1974

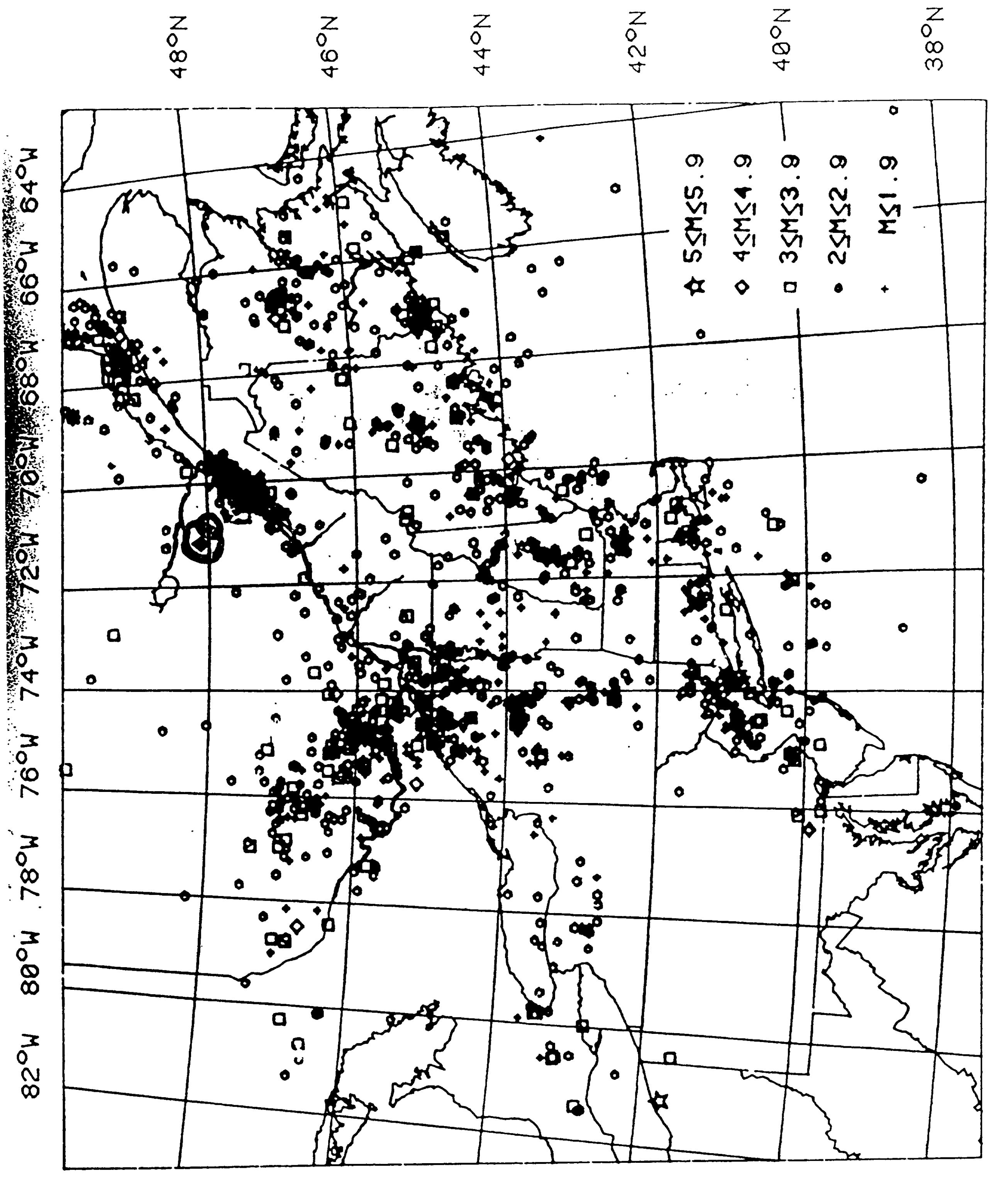



FIGURE 3. Earthquake Epicenters during the period

OCTOBER 1975 - DECEMBER 1986

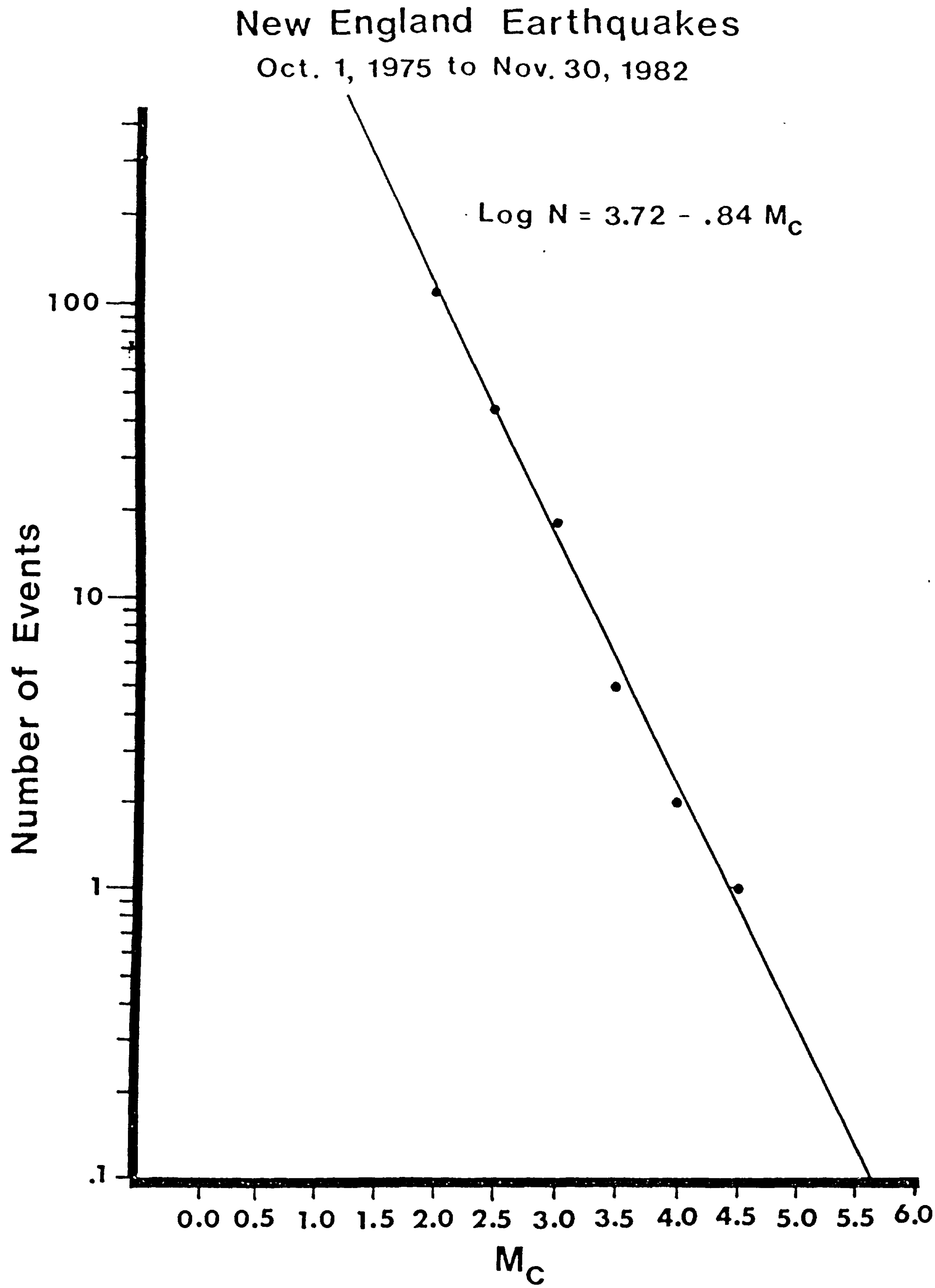



Figure 6. Recurrence curve for New England seismicity from Ebel (1984).



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Table 3

New England Seismic Hazard

Values in Table represent probability of an earthquake of a particular magnitude in the specified time period.

| Time (yrs) | 1 | 7 | 10 | 50 | 100 | 200 | 500 | 1000 |
|---|---|---|---|---|---|---|---|---|
| Magnitude | | | | | | | | |
| 4.6 | .10 | .51 | .64 | .99 | 1.00 | 1.00 | 1.00 | 1.00 |
| 5.0 | .05 | .28 | .38 | .91 | .99 | 1.00 | 1.00 | 1.00 |
| 5.2 | .03 | .20 | .28 | .80 | .96 | 1.00 | 1.00 | 1.00 |
| 5.5 | .01 | .12 | .17 | .60 | .84 | .97 | 1.00 | 1.00 |
| 5.8 | .01 | .07 | .10 | .40 | .64 | .87 | .99 | 1.00 |
| 6.0 | .01 | .05 | .07 | .29 | .50 | .75 | .97 | 1.00 |
| 6.4 | .003 | .02 | .03 | .15 | .27 | .47 | .80 | .96 |
| 6.5 | .003 | .02 | .03 | .12 | .23 | .41 | .73 | .93 |
| 7.0 | .001 | .01 | .01 | .05 | .10 | .18 | .40 | .63 |

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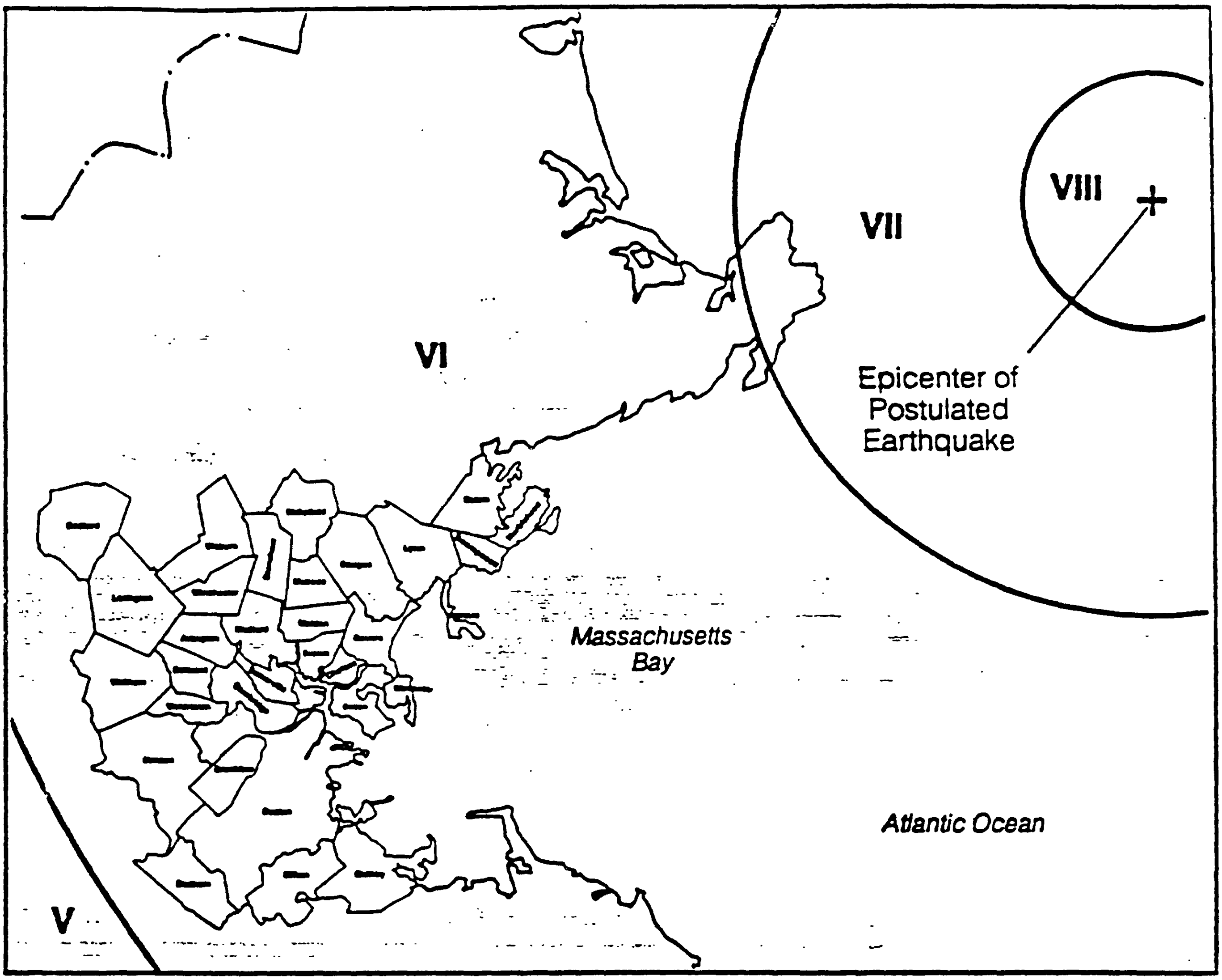
# Metropolitan Boston Area
# Earthquake Loss Study




**December 1989**

Prepared for
**The Massachusetts Civil Defense Agency**

Prepared by
**URS Consultants, Inc./John A. Blume & Associates, Engineers**
San Francisco, California

**Attachment 5-K**

**SUMMARY OF DAMAGE AND**

**72-HOUR POSTEARTHQUAKE FUNCTIONALITIES EVALUATION**

| Facility Group | Damage ($ millions) | Damage Factor Range (D/RV,%) | Functionality Range (%) |
|---|---|---|---|
| Medical Facilities | 96.5 | 2.1 -15.2 | 14 - 26 |
| Transportation Facilities and Systems | 70.8 | 1.0 - 8.9 | 47 - 100 |
| Gas and Petroleum Fuel Utilities | 6.2 | 1.6 - 12.1 | 47 - 95 |
| Water and Sewerage Utilities | 6.7 | 0.2 - 3.3 | 65 - 98 |
| Electrical Power Utility | 57.7 | 1.6 - 5.0 | 74 - 84 |
| Communications Network | 14.5 | 4.6 - 9.0 | 35 - 74 |
| Emergency Public Facilities | 9.7 | 3.3 - 14.4 | 16 - 26 |
| Residential Building | 2,500 | 2.3 - 10.0 | 57 - 62 |
| School Buildings | 992 | 0.7 - 15.5 | 11 - 19 |
| Special Facilities: | | | |
| -- Dams | .095 | 0.4 - 1.3 | 69 - 72 |
| -- Tall Buildings | 390 | 10.3 | 27 |
| Total | 4,150 | | |

# Working Group Scientists

John Adams, Canadian Geological Survey
John Armbruster, Lamont Doherty Earth Observatory
John Ebel, Weston Observatory of Boston College
Klaus Jacob, Lamont Doherty Earth Observatory
Alan Kafka, Weston Observatory of Boston College
Louis Klotz, New England State Earthquake Consortium
Stuart Nishenko, U.S. Geological Survey (Denver)
David Perkins, U.S. Geological Survey (Denver)
Nafi Toksoz, Massachusetts Institute of Technology
Randy Updike, U.S. Geological Survey (Reston)
Daniele Veneziano, Massachusetts Institute of Technology
Russell Wheeler, U.S. Geological Survey (Denver)
Mary Ellen Williams, U.S. Geological Survey (Woods Hole)

# Working Group Tasks

1. Determine earthquake probabilities for the Northeastern U.S. (New England, New York and New Jersey).
2. Assemble a single concensus regional earthquake catalog reflecting latest work on historic and recent events (John Adams, John Armbruster, John Ebel, and Stuart Nishenko).
3. Compile and produce a color seismicity map for the region.
4. Examine probabilities of foreshocks and aftershocks for earthquakes in the region.
5. Organize a northeastern workshop on earthquake probabilities, earthquake hazard and earthquake risk (late 1993 or early 1994).

Support for group: USGS and FEMA

# New England Earthquake Hazard Estimates

by

Stuart P. Nishenko, USGS, Denver

John E. Ebel, Weston Observatory of Boston College

David Perkins, USGS, Denver

**Study Tasks:**

1. **Form working group to put together concensus statement of earthquake probabilities for the northeastern U.S.**

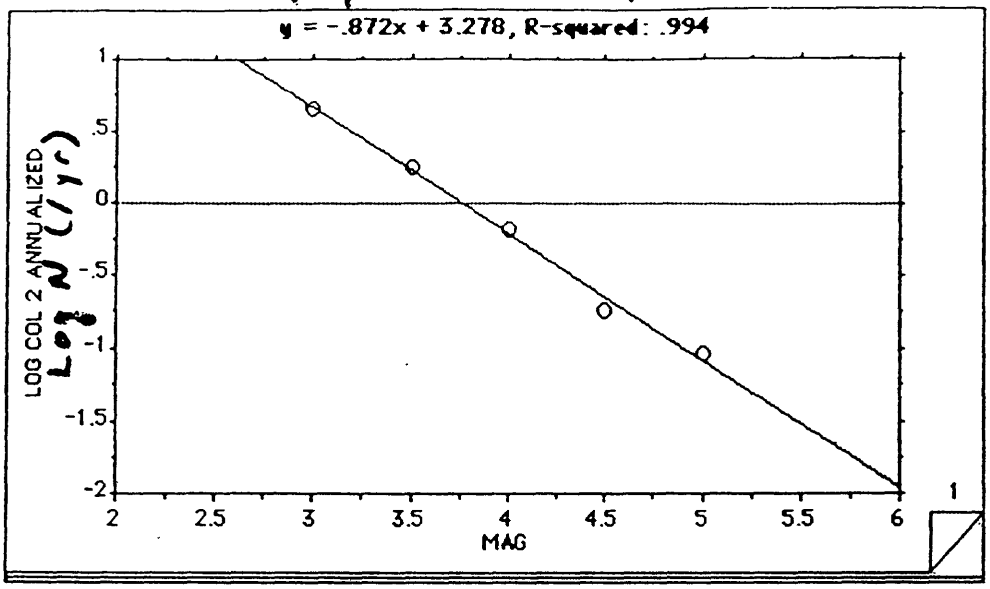
2. **Coordinate products such as a color USGS seismicity map for the northeastern U.S. for general distribution**

3. **Organize and convene workshop to publicly disseminate and discuss earthquake probabilities, earthquake hazard, and response measures for the northeastern U.S.**

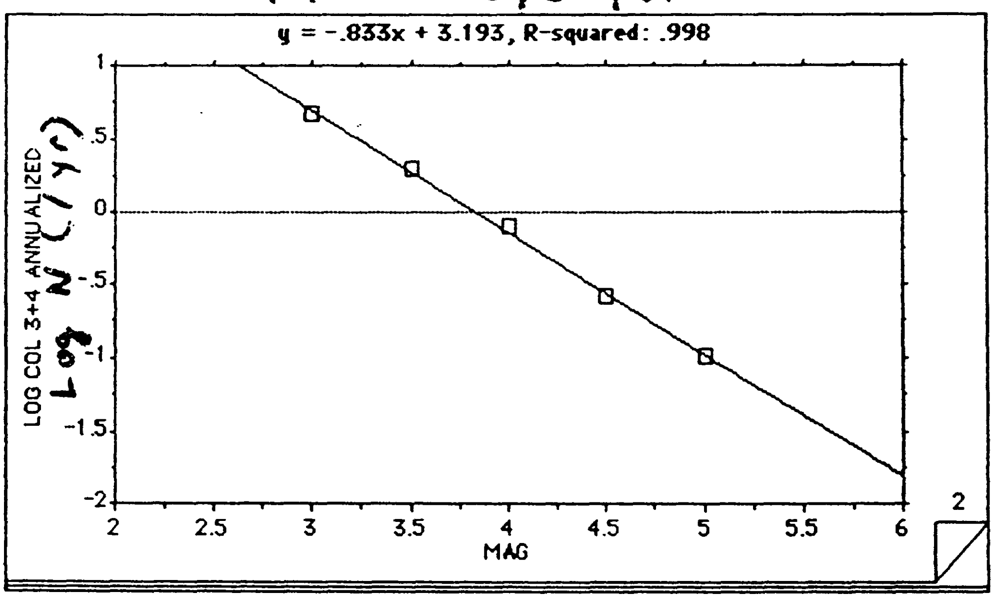
NEUS (ANNUALIZED)

Northeastern United States

10/1/75 – 6/86




1/1/38 – 6/30/86




**Range Restrictions**

| | Column Name: | Restriction: |
|---|---|---|
| AND | MAG | 3 ≤ X ≤ 5 |

TOP: 10/1/75 - 6/30/86; BOTTOM: 1/1/38 - 6/30/86

# New England Earthquake Probability Estimates

## Frequency-Magnitude Extrapolations

| Magnitude | ≥5 | ≥6 | ≥7 |
|---|---|---|---|
| Return Times | 60-94 yrs | 447-1035 yrs | 4500-11,000 yrs |
| Probability | | | |
| $P_{10}$ | 0.10-0.15 | 0.01-0.02 | <0.01 |
| $P_{20}$ | 0.19-0.28 | 0.02-0.04 | <0.01 |
| $P_{50}$ | 0.41-0.56 | 0.05-0.11 | 0.01 |

## Direct Rate Estimates

*4 to 5 events ≥ M 5 in 266 years*

| Probability | |
|---|---|
| $P_{10}$ | 0.14-0.17 |
| $P_{20}$ | 0.26-0.31 |
| $P_{50}$ | 0.53-0.61 |

$P_{10}$ , $P_{20}$ , and $P_{50}$ are Poisson probabilities for 10, 20, and 50 year exposure windows

**Earthquake Catalogs:**
Ebel (1987) 1938-1986
Armbruster (1993) {a} 1725-1985 {b} EPRI-EUS
Adams (1993) 1727-1985

# Earthquake Hazard Comparison: New England and California

Activity rate, California and New England

California: 89 earthquakes $M \geq 5$, 1932-1983 -- 1.71 events/year

New England: 5 earthquakes $M \geq 5$, 1727-1992 -- .019 events/year

**--> Activity Rate (Cal./NE) = 91.1 times greater**

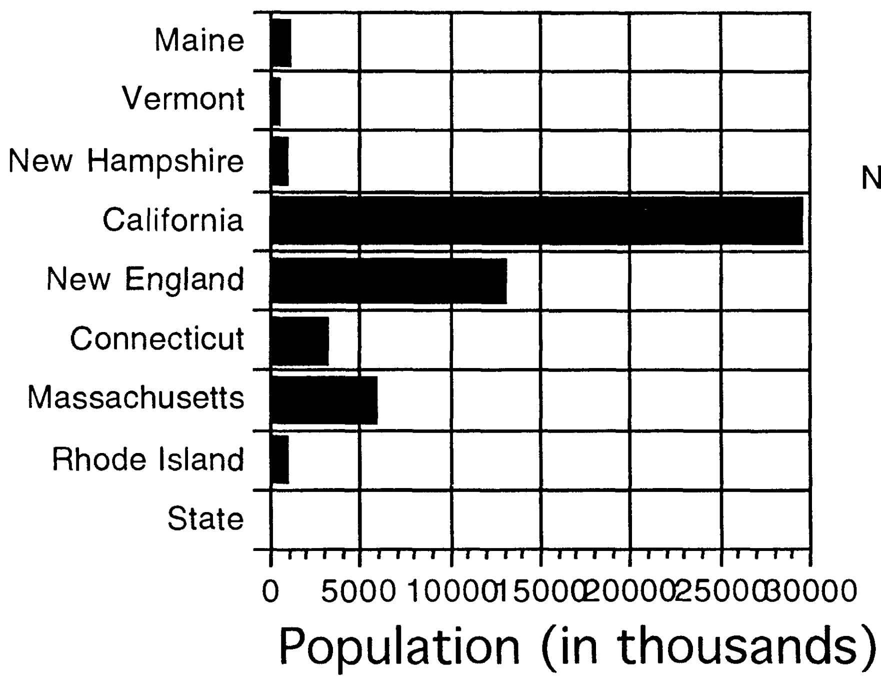
Damage area ratios, EUS/WUS

3.6 at IMM=VI (M=5 event); 6.3 at IMM=VII (M=6 event)

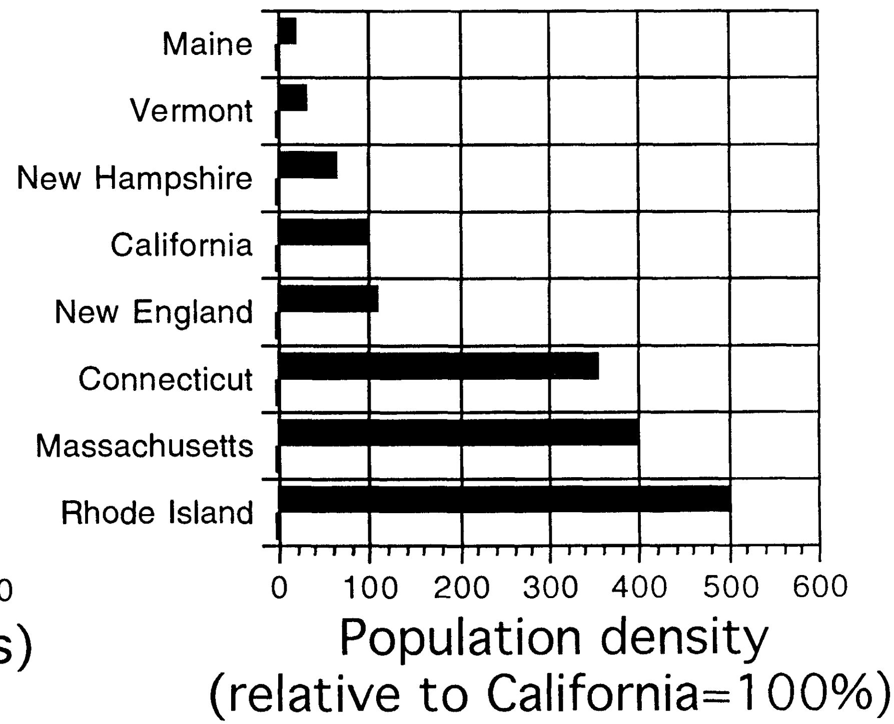
**--> Hazard Rate (Activity rate/Damage area ratio; Cal./NE): 26 times greater at IMM=VI (M=5 event); 14 times greater at IMM=VII (M=6 event)**

# At Earthquake Risk: New England versus California

## Total Population




## Population Density



# Foreshock Activity Rates

- About 6% of Southern California earthquakes are followed by a comparable or larger shock within 5 days and 10 km (1938-1983)

- About 3% of Northeastern North America earthquakes are followed by a comparable or larger shock within 7 days and 20 km (1938-1989) (preliminary estimate)

## Appendix H

Statement presented to NEPEC by J.Ebel:
consensus document produced at April 28, 1993, meeting.

DRAFT OF APRIL 28, 1993

# NORTHEASTERN U.S. WORKING GROUP ON EARTHQUAKES

Based upon a one-day review of the Seismological data available to the Group, we conclude the following: For the northeastern U.S. region (Maine, Vermont, New Hampshire, Connecticut, Rhode Island, Massachusetts, New York, and New Jersey), the probabilities of a potentially damaging earthquake, i.e. magnitude 5 or greater, is about 40 percent within the next 20 years and about 75 percent in the next 50 years.

If an earthquake of this magnitude were to occur in a heavily populated area of the region, substantial damage (millions to billions of dollars loss), injuries, and, perhaps, loss of life would result.

The Working Group will collaborate with the Federal Emergency Management Agency and the U.S. Geological Survey to enhance and improve these conclusions by convening a user-oriented workshop in late 1993. Prior to that workshop, scientists from Boston College, Columbia University, the USGS, and the Geological Survey of Canada will strive to produce an improved, uniform catalogue of earthquakes in the northeastern region, which will be used to produce a new earthquake map of the entire region.

This statement was produced as a consensus document at a meeting attended by John Adams (GSC), John Armbruster (L-DEO), John Ebel, Co-Chairman (BC), Klaus Jacob (L-DEO), Alan Kafka (BC), Louis Klotz (NESEC), Sturat Nishenko, Co-Chairman (USGS), Nafi Toksoz (MIT), Randy Updike (USGS), Daniele Veneziano (MIT), Russ Wheeler (USGS), and Mary Ellen Williams (USGS). It was distributed in hand-written format to a meeting of non-expert users of such information to convey the ad hoc nature of the statement.

## Appendix I

May 26, 1993, statement by I.Madin and M.Mabey concerning predictions of earthquakes in Oregon.

Oregon

DEPARTMENT OF
GEOLOGY AND
MINERAL
INDUSTRIES

ADMINISTRATIVE
OFFICE

Memo

To: Craig Weaver May 26, 1993

From: Ian Madin, Matthew Mabey

Subject: Earthquake Predictions following Scotts Mills earthquake.

On March 26, 1993, the day after the Scotts Mills earthquake, a columnist for the Oregonian (Phil Stanford) reported on a conversation a local person had on March 18, with Norm Paulhus of the Department of Transportation in Washington D.C.. Bob Behnke was the local person who had traveled to Washington, D.C.. In this conversation Mr. Paulhus was reported to have informed Mr. Behnke about an earthquake being predicted for the Pacific Northwest during the week of March 21-27. Mr. Stanford reports that on the day of the earthquake he called the Institute for Advanced Studies in Aspen, Colorado and spoke with Adam Tromley. Mr. Trombley is reported to have said that on March 24th they knew it was going to hit the "Portland area." Mr. Trombley is further quoted predicting increasing seismic activity all over the world and specifically a series of large events on the West Coast over the next few months. In a column on March 29, 1993, the following Monday, Mr. Stanford reported a conversation with a Cmdr. Mike Egan of the Coast Guard office of strategic planning. Egan reported to Stanford that a John Peterson of the Arlington Institute (Arlington Va. ) had received a fax, on March 24, from Adam Trombley and David Farnsworth of the Institute for Advanced Studies in Aspen, CO which predicted the March 25th event. Stanford also reported that these individuals felt that an earthquake of M 7.5 or larger was "shaping up" for Oregon within 2 to 3 months. In a subsequent column Stanford elaborated on the "brilliance" of Farnsworth and Trombley and pointed out that they were being ignored by the mainstream scientists. He also stated that he would publish any further predictions they forwarded to him. He has published none to date of which we are aware.

DOGAMI spoke with Egan (202-267-2690) on March 31, when he reported that he had received notification from the Arlington institute 24 hours before the quake that an earthquake was imminent between Mt. Rainier and Portland. He contacted FEMA (John Parkal) and Tracy Sinclair of Coast Guard in Seattle. Apparently the Coast Guard did take some action on the basis of this prediction.

Since that time, several local TV stations have done interviews with Trombley and Farnsworth. One, Mike Donahue (KOIN TV) announced (After the fact) that Trombley and Farnsworth had predicted a M 3 event in "Washington County " for the weekend of May 1st and 2nd, and that such and event did take place. UW reported a M 2 event that weekend in the general area. Mr. Donahue's coverage included an on-camera interview with not only Trombley and Farnsworth, but a commander or captain in the Coast Guard (in dress whites) who ascribed great credibility to Trombley/Farnsworth and said they were "batting 1000." Mike Donahue also has communicated in a phone conversation with DOGAMI that he is continuing to receive numerous predictions through the Coast Guard which he is tracking for a few months out of curiosity. He cited NEIC as the source of his magnitude 3 number for the earthquake which did occur. He has agreed to let DOGAMI know the results of his tracking.

We have not directly heard any subsequent predictions from these two, and indirectly only after the fact. In various rumors and news stories they are attributed with having predicted Petrolia, Loma Prieta, Landers, the recent Independence event and the series of events in Alaska, etc. We have spoken to one individual in the geoscience community

Suite 965
800 NE Oregon Street # 28
Portland, OR 97232
(503) 731-4100
FAX (503) 731-4066

who has spoken directly with Trombley (Chuck Hedel, ex USGS now with CH2M-Hill). Chuck said he was ultimately screamed at and hung up on. Numerous articles also appeared, largely in small Oregon newspapers detailing or describing their "electromagnetic" techniques and prediction claims. Mr. Donahue filmed various "coils" that are part of their sensing technique and reportedly was allowed to see equipment which they would not let him film.

In addition, there was a widely publicized prediction of a massive earthquake to strike Portland on May 3, made by a self-described street preacher. The "rumor" of this prediction was widespread before any main stream media coverage began, perhaps due to a large mailing which the street preacher sent out. The copious coverage of this prediction by the Oregonian included the statement that Portland would suffer this catastrophe because it is a well known locus of satanism, with human sacrifice practiced regularly. May 4, the lack of this event made the front page in the Oregonian and received similar coverage on TV.

There was also an astrologically based prediction aired on a TV series called "Sightings" which predicted a massive earthquake and tsunami for Oregon for May 8 $\pm$ 3 days.

Somewhere along the line, Nostradamus weighed in, but I don't have any specifics.

Most amazingly, there was a major flap the day of the President's timber summit. As reported in the Oregonian, an individual called the Coast Guard asking them to confirm a rumored prediction for the day of the summit. the Coast Guard misinterpreted his question as a prediction, and alerted their people and Portland emergency management. (There are actually several different versions of this story from equally credible sources. It is clear that the whole truth of the origin of what is about to be described is not known to DOGAMI.) Portland emergency management activated their EOC in response to this information. What other action Emergency responders actually took is unclear. Some of the rumors include fire engines being pulled into the street. An effort was made by some authority to put local hospitals on "earthquake alert", whatever the hell that is, some actually took steps while others wisely called DOGAMI and were told to ignore it. Portland emergency management later told us that the Secret Service was sufficiently concerned that they traced the mess back to the source. Portland emergency management also volunteered the statement that if the prediction had indeed been from Farnsworth and Trombley, the secret service would have taken it seriously enough to have taken some undefined action. We find this somewhat disturbing if true.

The volume of phone calls received by DOGAMI for about three hours around the noon hour of April 2nd equaled the maximum volume that occurred on the day of the Scotts Mills earthquake. Rumors were rampant that this or that radio or TV station had broadcast the rumor. All stations that were contacted denied it and said that to the contrary they had been broadcasting disclaimers stating that it was only a rumor. It is apparent from all conversations and phone calls that the rumor spread by word of mouth rather than the mass media. The media mentioning the rumor, even in an attempt to refute it, seems to have lent credibility to instead.

## Appendix J

April 2, 1993, news release citing lack
of evidence for Oregon earthquake prediction.

United States
Department of the Interior
Geological Survey, Western Region
Menlo Park, California 94025

Public Affairs Office Pat Jorgenson (415) 329-4000

For release: UPON RECEIPT (Mailed April 2, 1993)

EDITORS NOTE: **For comments and interviews on material in this news release you may call Dr. Evelyn Roeloffs at the USGS, 206-696-7912 or 503-288-9470; or Dr. Steve Malone at the University of Washington, 206-543-7010.**

## EARTHQUAKE "PREDICTION" UNSUPPORTED BY SCIENTIFIC EVIDENCE

No scientific evidence has been put forth to support the forecast of a major earthquake in the Pacific Northwest in the next two to three months. This forecast was referred to in a Portland, Ore., newspaper on March 26, 29 and 31, and April 2.

Adam Trombley and David Farnsworth are described as having observed electromagnetic signals over a wide frequency band that they interpret to indicate that a magnitude (M) 7.5 earthquake will take place within the next two to three months. Over the last several years, in many conversations with government and university scientists engaged in earthquake prediction research, Farnsworth has never revealed his measurement techniques in sufficient detail to enable their validity to be independently ascertained. Furthermore, there is no record showing that Farnsworth issued a prediction of the 1989 Loma Prieta (Calif.) earthquake.

There is always some possibility that the M 5.6 earthquake near Woodburn, Ore., on March 25 could be followed by a larger earthquake. The likelihood of such an event cannot be estimated. Based on the historical record of earthquakes in this region, however, we judge that a higher level of preparedness is not warranted at this particular time.

Earthquake activity in Oregon and Washington is monitored continuously by the University of Washington Geophysics Program. In the event of unusual activity, the USGS and the university's geophysicists will immediately notify the public if heightened preparedness is appropriate. No such activity has occurred to date.

The USGS and the UW geophysics program remind residents of Washington and Oregon that there is always the potential for a damaging earthquake to occur in the region. Residents should contact their local American Red Cross for information on inexpensive and effective ways to minimize their risk of injury or property damage during such an event.

* * * USGS * * *

## Appendix K

Charge to the NEPEC working group
on the Cascadia subduction zone.

# EARTHQUAKE HAZARDS IN THE PACIFIC NORTHWEST:

### Information and Charge to the NEPEC Working Group on the Cascadia Subduction Zone

## Background

The tectonic and geologic setting of the Pacific Northwest (PNW) includes the active Cascadia Subduction Zone (CSZ), similar to those elsewhere in the world where great earthquakes occur. No known great earthquakes (M>8) from the CSZ have been recorded by seismographic networks, and none are known from the historical record. The late Holocene geologic record found in numerous coastal intertidal marshes however, contains evidence consistent with the occurrence of great earthquakes. This evidence includes multiple buried peat horizons, each of which may be interpreted to represent a previous soil surface that was suddenly submerged during a great earthquake. At some sites along the coast of the PNW, tsunami-like sands have been deposited directly on these submerged soils, supporting the interpretation that burial was the result of a great earthquake. Other data consistent with, but not nearly as unequivocal as the interpretation made from the marsh subsidence records, include landslides that occur with a frequency similar to the marsh subsidence events and geophysical interpretations of crustal strain data.

The repeat time for these great earthquakes and their probable magnitudes remain uncertain. Resolution of both issues requires demonstration of synchronicity among the specific marsh horizons at multiple sites along the coast, a difficult experimental task. Furthermore, little evidence exists to indicate that strong ground shaking accompanied the most recent event, estimated to have occurred about 300 years ago. Despite these uncertainties, the available data, within their tectonic and geologic setting, constitute ample evidence that the Pacific Northwest is subject to great subduction zone earthquakes, that these events occur on the average of a few times per thousand years, that they involve lengths of the coast sufficient to produce earthquakes of magnitude at least 8, and that the data permit events as large as magnitude 9.

Two other sources for major damaging earthquakes exist in the Pacific Northwest: those within the subducting Juan de Fuca and Gorda plates, and those within the crust of the North American plate. These events have provided the primary model for earthquake hazard assessments in the region. Recent studies have concluded that earthquakes should be expected anywhere within the subducting plates at depths comparable to those known this century (40-60 km), and that a realistic magnitude for planning purposes for these events is in the range of 7 to 7.5. Crustal earthquakes in the North American plate may be a major urban hazard in the Puget Sound basin because M=7 crustal events are known to occur in similar settings elsewhere. Considerable additional geologic and geophysical studies are needed on this issue, and current geological investigations in the Puget Sound region should improve our understanding of major crustal events there.



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## Further Studies

Recognition of the possibility of great CSZ earthquakes in the PNW calls for more emphasis on direct hazards implications, particularly in the urban areas of the Puget Sound basin and the Willamette Valley. Modeling of strong ground motion, on scales from whole sedimentary basins such as Puget Sound to representative local sites, is needed for the spectrum of sources expected in the region. Additional work is needed to understand potential long-period motions associated with the very long fault breaks expected during a great CSZ earthquake. Finally, because of the danger of locally-generated tsunamis, both along the coast and within Puget Sound, efforts need to be made to map the limits of paleo-tsunami runups and to model future wave heights. Public awareness of all these earthquake-related issues must be increased as an integral part of hazards reduction and mitigation.

## Summary and NEPEC Response

The overview presented to the Council on research progress and current hypotheses on the earthquake potential of the CSZ calls for immediate action. Earthquakes of the size permissible under plausible models for the region represent a most serious threat to the U.S. Pacific Northwest from Cape Mendocino to the Canadian border. NEPEC therefore is chartering a CSZ Working Group on this issue to bring together and to summarize current evidence on possible modes of failure of the CSZ and to present the consequent implications for earthquake hazard assessment in the region.

## CSZ Working Group Composition

The recommended CSZWG membership includes C. Weaver (Chair), Kanamori, Nelson, Carver, Caruer, Plafker, Malone, Atwater, Savage and Weldon. E. Bernard of NOAA is available for consultation on tsunami issues.

## CSZ Working Group Charge

The charge to the CSZWG is to develop an objective assessment of all evidence and hypotheses for and counter to the proposed repeated great (M = 8.5-9) CSZ earthquakes, and to propose a best effort assessment of the possibility of future such earthquakes in the PNW and their potential effects on land. Specific questions surround issues of the frequency of the great earthquakes, the likely mode(s) of failure of the CSZ, and the implications of the plausible scenarios for earthquake preparedness. In addition, recommendations should be made for any specific investigative steps that hold promise for reducing uncertainties in the conclusions drawn from the available evidence.

## Working Group Schedule

The CSZWG should strive for early completion of their review and assessment, given the potential impact of their conclusions. If at all possible, a draft consensus report should be developed by the end of 1992.

## Appendix L

Advance draft of the report of the NEPEC working group on the Cascadia subduction zone.

# PAST OCCURRENCE OF GREAT EARTHQUAKES AT THE CASCADIA SUBDUCTION ZONE


Brian F. Atwater[1] (compiler), Peter T. Bobrowsky[2], Joanne Bourgeois[3], Gary A. Carver[4] (compiler), John J. Clague[5], Mark E. Darienzo[6], Wendy C. Grant[7], Eileen Hemphill-Haley[8], Harvey M. Kelsey[9], Alan R. Nelson[10] (compiler), Stuart P. Nishenko[11], Stephen F. Obermeier[12], Curt D. Peterson[13], Mary Ann Reinhart[14], David K. Yamaguchi[15]


Prepared for the National Earthquake Prediction Evaluation Council
May 18, 1993

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1 U.S. Geological Survey at Department of Geological Sciences, University of Washington, Seattle, WA 98195 206-553-2927, fax -8350, atwater@u.washington.edu

2 Geological Survey Branch, Ministry of Energy, Mines, and Petroleum Research, 553 Superior Street, Victoria, B.C. V8V 1X4 604-356-7763

3 Department of Geological Sciences, University of Washington AJ-20, Seattle, WA 98195; currently at National Science Foundation, 1800 G Street N.W., Washington, D.C. 20550 202-357-7806, fax -0364

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10 U.S. Geological Survey, MS 966, Box 25046, Denver, CO 80225-0046 303-273-8592, fax -8600, anelson@gldvxa.cr.usgs.gov

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## ABSTRACT

**Coastal geology shows that great (magnitude >8) plate-boundary earthquakes have occurred in the past few thousand years at the Cascadia subduction zone.**

Such earthquakes are indicated by buried marsh and forest soils at more than a dozen Pacific Coast estuaries between central Vancouver Island and Cape Mendocino. The burial of some of these soils is best explained by sudden tectonic subsidence of coastal land. Sand sheets on some of the soils in British Columbia, Washington, and Oregon show that the sudden subsidence sometimes coincided with tsunamis. Liquefaction features demonstrate that shaking accompanied the most recent of the subsidence events near the mouth of the Columbia River, but the intensity of this shaking remains to be determined. The documented evidence for the past occurrence of great earthquakes is strongest for southern Washington and weakest for British Columbia and northern Washington.

**The greatest of the inferred earthquakes may have been near magnitude 8 but could have exceeded magnitude 9.**

High-precision radiocarbon ages on earthquake-killed trees are statistically similar among two Pacific Coast estuaries in southern Washington, one estuary in northern Oregon, and an estuary in northern California. This similarity is consistent




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with a 10- to 20-year-long series of earthquakes near magnitude 8 but could also signify a single earthquake near magnitude 9.

**Recurrence intervals at individual sites have probably been on the order of centuries but may have been a millenium or more in some cases.**

The intervals are poorly known because of uncertainties about the number, timing, and extent of great Cascadia earthquakes in the past few thousand years. These uncertainties result in broad ranges of probabilities that a great earthquake will occur at the Cascadia subduction zone in the next 50 years.

**The National Earthquake Prediction Evaluation Council should adopt and publicize the position that the Cascadia subduction zone is capable of producing great earthquakes.**

An official advisory of great earthquakes would reinforce efforts to improve earthquake preparedness in the region. These efforts include recent changes in building codes for western Oregon and parts of western Washington--changes the advisory should commend.

**The Council should also put out an advisory about tsunami hazards at the Cascadia subduction zone.**

The tsunami advisory should instruct seaside residents and visitors to seek high ground upon feeling an earthquake. It would apply to the Pacific coast from Vancouver Island to Cape Mendocino, and it could also apply to Puget Sound. It might be prepared and issued jointly by several U.S. and Canadian agencies,



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in consultation with emergency managers from coastal towns and counties.

## INTRODUCTION

This report presents our consensus about the past occurrence of great (magnitude >8) plate-boundary earthquakes at the Cascadia subduction zone. It also discusses the estimation of magnitudes and recurrence intervals, and it concludes with recommendations for public statements from the National Earthquake Prediction Evaluation Council.

Statements in the text are based on articles in refereed journals and books *(cited with prefix A)*, other reports *(B)*, abstracts *(C)*, and unpublished work *(D)*. None of us necessarily endorse every interpretation in the cited material, but all of us have read the entire report and concur in the following findings and recommendations.

## FINDINGS

### Occurrence

Great (magnitude 8 or larger) plate-boundary earthquakes have almost certainly occurred at the Cascadia subduction zone in the past few thousand years. The inferred earthquakes generated sudden land-level changes, tsunamis, and shaking that are recorded by coastal deposits and landforms.

Sudden land-level change. The main evidence for the past occurrence of great Cascadia earthquakes consists of buried marsh or forest soils beneath the intertidal mud of Pacific coast estuaries. We have found such buried soils at more than a dozen estuaries between central Vancouver Island and Cape Mendocino. At many of these estuaries, fossils and sediment type show that tidal submergence caused the burial of one or more of the soils, and that this submergence probably resulted from at least 1/3 m of sudden tectonic subsidence *(A2, A4, A5, A8, A12, A13, A24, A26, A28, B9, B14, B16, B17, B19, B20, B22, B23, C4-C8, C10, D3)*. The main exception is an estuary in southern Oregon at which thick peaty deposits provide evidence against sudden land-level changes much greater than 1/3 m in the past two thousand years *(A24, A26)*.

Some of the individual subsidence events probably lowered much of the southern Washington and northern Oregon coasts *(A4, A7, B9, D1)*. The most recent subsidence in southern Washington was caused by an earthquake, or earthquakes, that ruptured at least 55 km of the plate boundary along the Cascadia subduction zone about 300 years ago *(A7)*. The subsidence did not extend inland to northern Puget Sound or the Strait of Georgia *(A14, B1, B8)*. The most recent coastal subsidence in southern Washington and northern Oregon is probably analogous to the widespread coseismic subsidence that flanked the rupture areas of great historical earthquakes along subduction zones in south-central Alaska, southern Chile, and southwestern Japan *(references cited in A12, A13, A26)*.

The significance of sudden subsidence is less certain for southern Oregon and northern California because there the subsidence appears mostly restricted to Quaternary synclines in the overriding North America plate *(A12, A20, A24, A26, B3, B21, B23, C2, C14)*. In southern Oregon some areas subsided while others escaped major land-level change *(A24, A26, B3, C2)*. Subsidence along synclines in northern California may have accompanied uplift on nearby anticlines *(A12, A21, B6, B7, B18, C3, C9, C11, C12)* and may have also coincided with surface rupture on nearby thrust faults *(A12, B4, B5)*. Such localized deformation might have occurred with or without concurrent slip at the plate boundary *(A12, A15, A24, A26, C2)*.

The Cascadia subduction zone offers little geologic evidence for coseismic uplift outside northern California. Meters of late Holocene uplift have been inferred for Cape Blanco, Oregon *(A19)*, western Vancouver Island *(A11, B11, B15)*, and Puget Sound *(A9)*. But the Holocene uplift at Cape Blanco now appears doubtful *(D3, D4)*, and the Holocene uplift at western Vancouver Island has probably occurred gradually and has been punctuated by coseismic subsidence *(C5)*. As for Puget Sound, the uplift there accompanied one or more shallow inland earthquakes that did not necessarily coincide with plate-boundary slip *(A9)*.

The overall scarcity of coseismically uplifted shorelines contrasts with the 1980s expectation that widespread coastal uplift would have accompanied great Cascadia earthquakes *(A30, A31)*. But that expectation is too simplistic to negate the abundant paleoseismic evidence now recognized at the Cascadia subduction zone *(A3, A12, A26, A22)*.

Tsunamis. Sandy beds probably deposited by tsunamis mantle some of the buried soils in British Columbia *(B2, C1, C5, D3)*, Washington *(A2, A4, A5, A8, C16, C17)*, and Oregon *(A13, B3, B9, B10, B12, C7, C8)*. These beds accumulated where little if any sand was deposited by the tsunami from the 1964 Alaska earthquake, the largest far-traveled tsunami that has struck the region historically. Plant fossils at some estuaries show that the sandy bed on the uppermost buried soil was deposited no more than a few years after the subsidence recorded by the soil *(A4, A7)*. Such prehistoric sandy beds imply that tsunamis were generated by earthquakes that caused sudden coastal subsidence along the Cascadia subduction zone.

Shaking. Seismic shaking during sudden subsidence best explains sand that intruded and vented onto the uppermost buried soil along the lower



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Columbia River *(D5)*. Shaking from great Cascadia earthquakes has also been inferred from Holocene turbidites off the Pacific coast of Washington and Oregon *(A1)*, from Holocene debris-flow deposits in northern coastal Oregon *(B12)*, and from Pleistocene liquefaction features in coastal Washington and Oregon *(C15)*. In addition, great Cascadia earthquakes are among the possible explanations for Holocene liquefaction features near Vancouver, British Columbia *(A10)* and Holocene turbidites and landslides in the Puget Sound area *(A16, A18, A29)*.

Despite these signs of shaking, it is unclear whether plate-boundary earthquakes have produced strong coastal shaking during the past few thousand years along the Cascadia subduction zone. Earthquakes from within the North America plate could explain most of the evidence for shaking cited in the previous paragraph. Only the liquefaction features along the lower Columbia River convincingly correlate with independent evidence for great plate-boundary earthquakes at the Cascadia subduction zone. Morever, little is known about the intensity of the shaking that produced the liquefaction features along the Columbia River *(D5)*, and none of the Holocene shaking in southern coastal Washington has been sufficient to produce widespread liquefaction features in gravelly alluvium *(D6)*.

## Magnitude

We reached no consensus about the degree to which any of the inferred earthquakes exceeded magnitude 8. This lack of consensus partly reflects uncertainties, discussed above, about the intensity of coastal shaking near the Columbia River and about the significance of localized subsidence in



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southern Oregon and northern California. The lack of consensus also reflects uncertainties in radiocarbon and tree-ring dating of the inferred earthquakes.

The uncertainties in dating, which commonly exceed several centuries *(A23)*, only rarely can be limited to a few decades *(A7, C4, C18, D1)*, and even this minimum uncertainty far exceeds the time that can separate successive earthquakes along different parts of a subduction zone. Therefore the dating is not necessarily capable of distinguishing between two very different possibilities: the synchronous subsidence of a large part of the Cascadia coast during a single earthquake of magnitude 9, and the nearly synchronous subsidence of much smaller parts of the coast during a series of earthquakes of magnitude 8.

The most precise of the radiocarbon ages leave intact the magnitude-9 hypothesis for the most recent earthquake. These ages fail to rule out the possibility that subsidence was synchronous among two areas in southern Washington *(A7)*, a site in northern Oregon *(D1)*, and a site in northern California *(C4)* (Fig. 1, localities with arrows). The distance between the northernmost and southernmost of the dated localities is 680 km.

## Recurrence and Probability

Recurrence intervals of great earthquakes at individual sites have probably been on the order of centuries but may have exceeded a millenium *(A4, A5, A12, A13, A24, A26, A28, B9, B12, B19, B22, B23, C6, C15)*. At some sites the intervals may have varied as much as tenfold *(A4, A5)*.

Several issues kept us from reaching a consensus on how to use these ballpark estimates to infer probabilities of future great earthquakes at the Cascadia subduction zone.



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(i) Completeness of record. We could underestimate the probability of a future earthquake in areas where some of the past earthquakes did not leave a lasting geologic record, as in the case of buried soils widely destroyed by erosion or oxidation *(A4, A5, A24)*. Conversely, we could overestimate the probability by inferring coseismic subsidence from buried soils that are unrelated to great earthquakes *(compare A24 with A20 and C15)*. Some buried soils might record earthquakes from structures within the North America plate, not only in southern Oregon *(A24, A26, A27)* and northern California *(A12)* but also near faults that cross the continental shelf off northern Oregon *(A15, B13)*. Moreover, nearly all the estuaries have buried soils that might represent submergence from rapid sea-level rise, breaching of tide-restricting bars, or non-seismic changes in sedimentation rate *(A24, A26)*. We have barely begun to evaluate non-seismic alternatives through detailed studies of sedimentary environments and microscopic fossils *(A17, A25, B14, C10)*.

(ii) Uncertainty in age. In some cases the total uncertainty in age--the sum of geological and analytical errors in our dating of the inferred earthquakes--is larger than some of the recurrence intervals *(A4, A23)*.

(iii) Statistical distribution of events. Because of uncertainties in the inference and dating of great Cascadia earthquakes, we do not know whether such earthquakes at a given place recur periodically, randomly, or in clusters. Nor have do we know whether the recurrence depends on the time elapsed since the preceding event.

(iv) Segmentation of rupture. Although radiocarbon dating thus far gives no evidence for segmentation between southern Washington and northern California about 300 years ago *(A7, C4, D1)*, the dating does not rule out the possibility of persistent barriers to the propagation of plate-boundary ruptures at the



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Cascadia subduction zone *(A26 and references therein)*. Such barriers could increase the total number of great Cascadia earthquakes in the coastal geologic record, with a consequent increase in the calculated probability that another great earthquake will occur somewhere along the subduction zone.

We struggled with the above four issues while attempting to estimate conditional probabilities from sequences of buried soils at the Copalis River, Washington *(A4)*; at the Nehalem River *(C6)*, Netarts Bay *(A13, B9)*, and the Coos Bay area *(A24, A26, B3, C14)*, Oregon; and at northern Humboldt Bay, California *(A12, B23)* (Fig. 1, starred localities).

(i) Completeness of record. Uncertainties in equating soils with earthquakes resulted in a correlation between probability and personality. We experimented with uniform criteria for the inference of earthquakes *(C13)*, but even with these criteria the number of inferred earthquakes depended not only on the number of observed soils but also on the cautiousness of the geologist. For most sites we made two estimates of the number of earthquakes in the past 2000 years--one stingy, the other generous.

(ii) Uncertainty in age. For convenience we ignored uncertainty in age and calculated repeat times from the midpoints of age ranges mostly uncorrected for geological uncertainties in dating.

(iii) Statistical distribution of events. Because the intervals appear variable at some sites, we dealt with the statistics of recurrence by treating earthquakes as memoryless (we used a Poisson distribution), and we simply used average repeat times estimated from the numbers of earthquakes inferred for the last 2000 years.



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(iv) Segmentation of rupture. We arbitrarily treated the subduction zone as a single segment for one group of calculations, and as three independent segments for a second group.

The result was a broad, speculative range--from about 3 to 30 percent--of conditional probabilities that a great earthquake will occur at the Cascadia subduction zone during the next 50 years.

## RECOMMENDATIONS

### Advisory about Great Earthquakes

The National Earthquake Prediction Evaluation Council should advise the public that the Cascadia subduction zone is capable of producing great earthquakes. Such an advisory would provide politicians and emergency managers with an official mandate for promoting earthquake preparedness. The advisory should applaud the seismic-risk upgrade--from zone 2 to zone 3--that structural engineers have recently approved for western Oregon and parts of western Washington.

The Council's advisory on great Cascadia earthquakes should give only broad ranges for magnitudes, recurrence intervals, and conditional probability. Such ranges should be consistent with the uncertainties discussed above. We ask the Council to reconsider Cascadia probabilities several years from now, on the chance that well-founded estimates will have appeared and survived scrutiny in peer-reviewed publications.

## Advisory about Tsunamis

The National Earthquake Prediction Evaluation Council should promote the issuance of a long-term tsunami advisory for the Pacific coast between central Vancouver Island and Cape Mendocino. The advisory should mention widespread geologic evidence for large tsunamis from great earthquakes at the Cascadia subduction zone during the past few thousand years. The advisory could also mention that tsunamis accounted for most of the loss of life from such great subduction-zone earthquakes as 1960 Chile and 1964 Alaska. The advisory could be extended to inland waters of southwestern British Columbia and western Washington, in recognition that faulting within the North America plate generated a tsunami in Puget Sound about 1000 years ago (*46*). Drafts could be reviewed by emergency managers of coastal jurisdictions and by staff of the Alaska tsunami warning center.

The main goal of the advisory would be to provide public officials with an official mandate for instructing coastal residents and visitors to move quickly to high ground or strong buildings after feeling an earthquake. Such education may be the most cost-effective way to minimize loss of life from future great earthquakes at the Cascadia subduction zone. Both the earthquake and the tsunami advisories might be issued jointly by American and Canadian governments.

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C8 Grant, W.C., and Minor, R., 1991, Paleoseismic evidence and prehistoric occupation associated with late Holocene sudden submergence, northern Oregon coast: Eos, v. 72, p. 313.

C9 Lajoie, K.R., 1992, Holocene coastal uplift in the region of the 1992 Petrolia earthquake, northern California: Eos, v. 73, p. 497.

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C12 Merritts, D.J., Dunklin, T., and Brustolon, A., 1992, Holocene surface uplift, Mendocino triple junction: Eos, v. 73, p. 497.

C13 Nelson, A.R., 1993, Stratigraphic and paleoecologic criteria that distinguish coseismically submerged from gradually submerged tidal-wetland deposits, Oregon and Washington: Geological Society of America, Abstracts with Programs, v. 25, no. 5 (in press).

C14 Peterson, C.D., and Darienzo, M.E., 1989, Episodic, abrupt tectonic subsidence recorded in late Holocene deposits of the South Slough syncline--an on-land expression of shelf fold belt deformation: Geological Society of America Abstracts with Programs, v. 21, no. 5, p. 129.

C15 Peterson, C.D., Hansen, M., and Jones, D., 1991, Widespread evidence of paleoliquefaction in late-Pleistocene marine terraces from the Oregon and Washington margins of the Cascadia subduction zone: Eos, v. 72, p. 313.

C16 Reinhart, M.A., and Bourgeois, Joanne, 1987, Distribution of anomalous sand at Willapa Bay, Washington--evidence for large-scale landward-directed processes: Eos, v. 68, p. 1469.

C17 Reinhart, M.A., and Bourgeois, Joanne, 1989, Tsunami favored over storm or seiche for sand deposit overlying buried Holocene peat, Willapa Bay, WA: Eos, v. 70, p. 1331.

C18 Yamaguchi, D.K., Woodhouse, C.A., and Reid, M.S., 1989, Tree-ring evidence for synchronous rapid submergence of the southwestern Washington coast about 300 years ago: Eos, v. 70, p. 1332.

**Unpublished work**

D1 Atwater, B.F., Stuiver, Minze, and Grant, W.C.: High-precision radiocarbon ages for the most recent sudden subsidence along the Nehalem River, Oregon. Ages measured on rings from bark-bearing stumps of Sitka spruce and crabapple(?) that are rooted in uppermost buried soil about 1 km upstream from Wheeler. Sudden subsidence is indicated for this soil by wide outer rings in the dated stumps and by stems of Potentilla pacifica that are rooted in the soil and entombed in the overlying mud, which contains rhizomes of Triglochin maritimum. The ages are not statistically different from high-precision ages of comparable rings from trees killed by the most recent sudden subsidence in southern Washington *(A7)* and northern California *(C4)*.

| LAB NO. | STUMP | RING NO. OF DATED RINGS (RING 1 ADJOINS BARK) | AGE IN 14C YEARS BEFORE A.D. 1950 |
|---|---|---|---|
| QL-4640 | E6 | 1-10 | 124 +/ 14 |
| QL-4641 | E7 | 3-7 | 127 +/ 14 |
| QL-4642 | E8 | 1-8 | 132 +/ 11 |
| QL-4643 | E8 | 39-41 | 211 +/ 13 |

D2 Clague, J.J., and Bobrowsky, P.T.: Evidence for prehistoric tsunamis at Port Alberni, British Columbia. The tsunamis are indicated by layers of sand and gravelly sand in peaty tidal-marsh deposits. The tsunami from the 1964 Alaska earthquake deposited a layer of fine sand a few centimeters thick *(C1)*. Some of the prehistoric tsunamis are indicated by thicker layers of gravelly sand. The youngest such layer is less than 500 years old, as judged by preliminary radiocarbon dating of plant detritus.

D3 Clague, J.J., and Bobrowsky, P.T.: Radiocarbon ages for the uppermost buried soil near Tofino and Ucluelet, British Columbia. The ages, measured on plant detritus, indicate that burial began sometime in the past 500 years. The soil records sudden subsidence that was attended by a tsunami *(C5)*.

D4 Kelsey, H.M.: Evidence for and against late Holocene uplift near Cape Blanco, Oregon. A cobble berm south of the cape may have been uplifted as much as 15 m in the past 2000 years *(A20)*. But such uplift now seems inconsistent with Holocene stratigraphy exposed nearby along the Sixes River. There, two buried forest soils suggest two abrupt rises in base level. Also along the lower Sixes River, sand dikes intrude the substrate of lower soil, and a sand layer less than 1000 years old was probably deposited by a tsunami.

D5 Obermeier, S.F.: Evidence for shaking along the lower Columbia River, Oregon and Washington. Intrusive and extrusive bodies of sand are exposed at low tide at the edges of islands within 50 km of the coast. The largest dike found is 30 cm wide. The sand vented onto the uppermost buried soil at or within a few years of probable subsidence of the soil. Age of this subsidence bracketed between 200 years ago (ring counts of spruce trees rooted above the buried soil) and 500 years ago (preliminary identification of a tephra layer below the buried soil). The dikes may be too few and thin to indicate that the shaking was strong.

D6 Obermeier, S.F.: Evidence against strong Holocene shaking in southern coastal Washington. Few if any liquefaction features are present in gravelly alluvium along many kilometers of streams tributary to Grays Harbor.

Figure 1. Index map of the Cascadia subduction zone.

EXPLANATION

Structural features

\` ^ Thrust fault at boundary between oceanic and continental plates--Sawteeth show direction of dip

---- Other fault

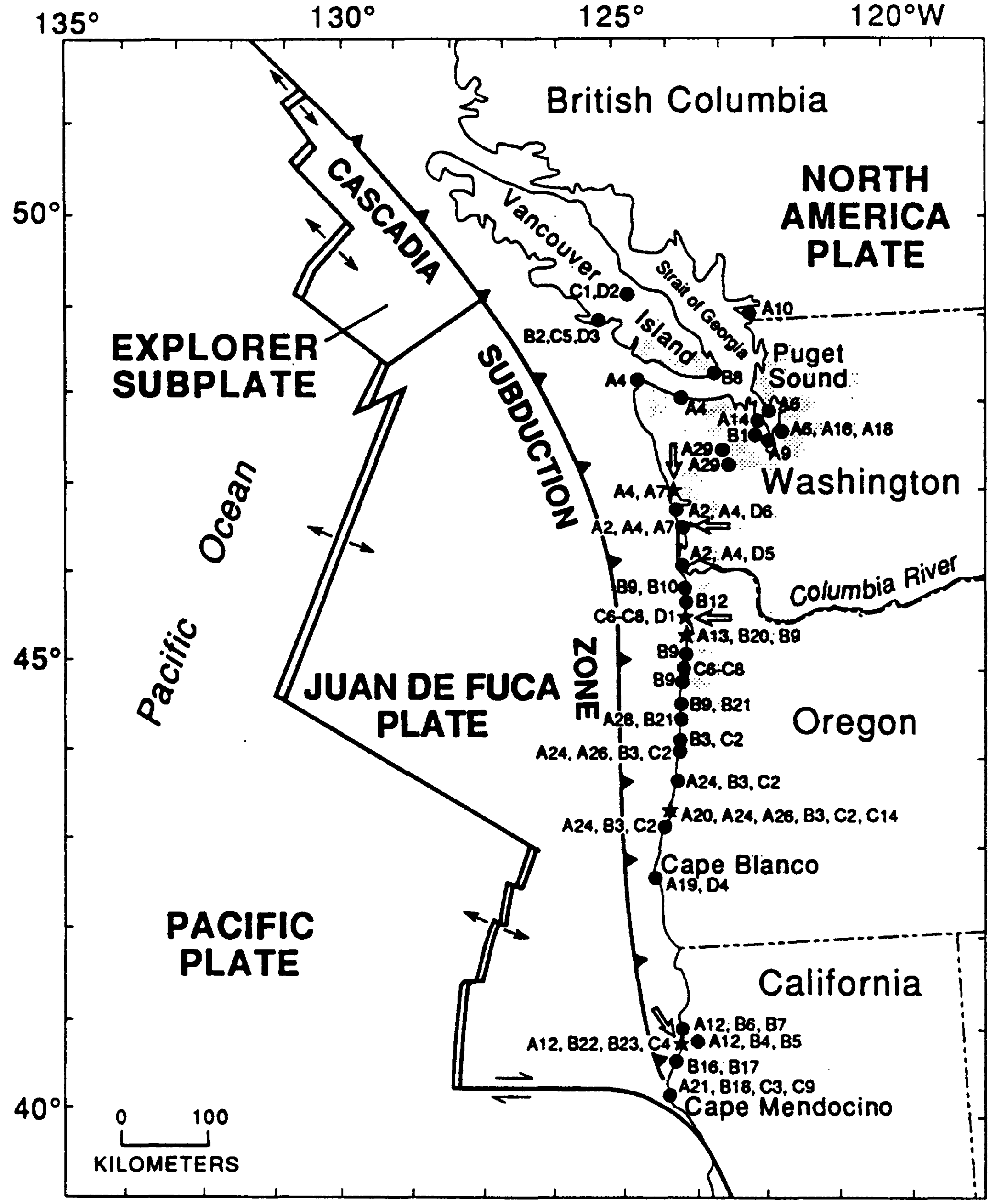
==== Spreading ridge

Localities

A13 Reference cited in text

--> High-precision radiocarbon ages

* Trial estimate of recurrence and probability



135°
130°
125°
120°W
50°
45°
40°
British Columbia
NORTH AMERICA PLATE
CASCADIA
SUBDUCTION
ZONE
EXPLORER SUBPLATE
Vancouver Island
Strait of Georgia
Puget Sound
Washington
Columbia River
Oregon
California
Cape Blanco
Cape Mendocino
Pacific Ocean
JUAN DE FUCA PLATE
PACIFIC PLATE
0 100 KILOMETERS
C1,D2
B2,C5,D3
A10
A4
B8
A4
A6
A14
B1
A6, A16, A18
A9
A29
A29
A4, A7
A2, A4, D6
A2, A4, A7
A2, A4, D5
B9, B10
B12
C6-C8, D1
A13, B20, B9
B9
C6-C8
B9
B9, B21
A28, B21
B3, C2
A24, A26, B3, C2
A24, B3, C2
A20, A24, A26, B3, C2, C14
A24, B3, C2
A19, D4
A12, B6, B7
A12, B22, B23, C4
A12, B4, B5
B16, B17
A21, B18, C3, C9

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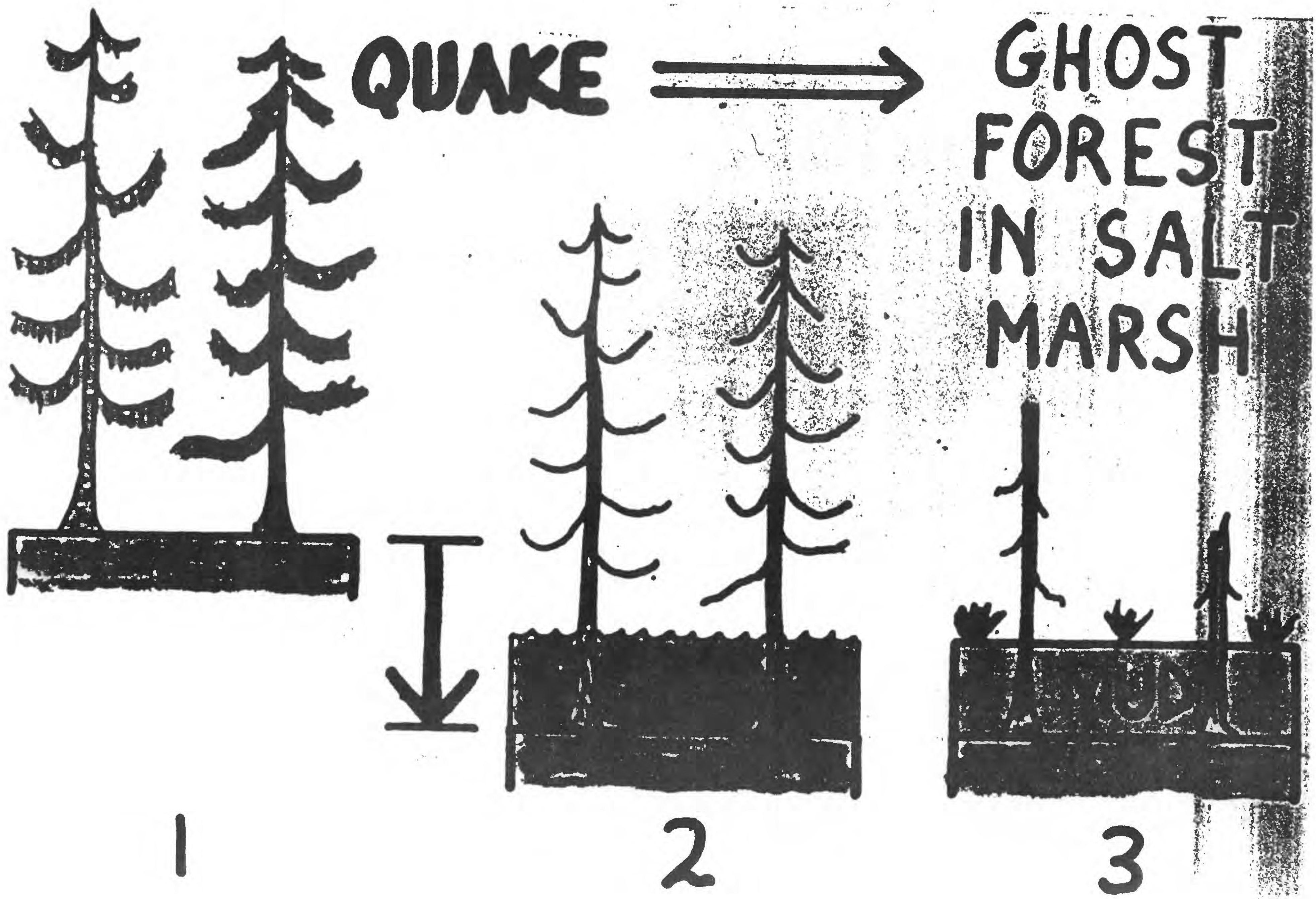
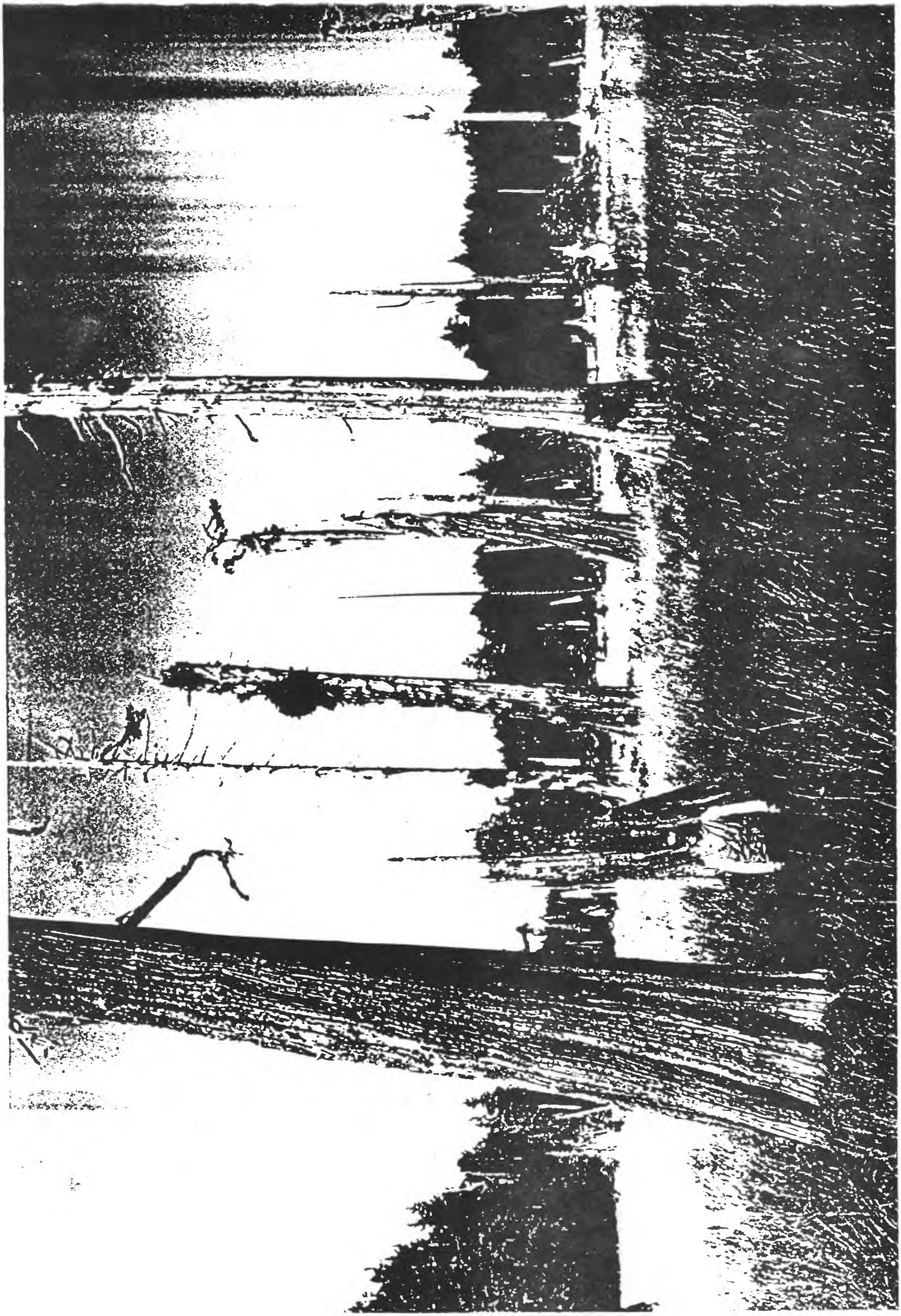
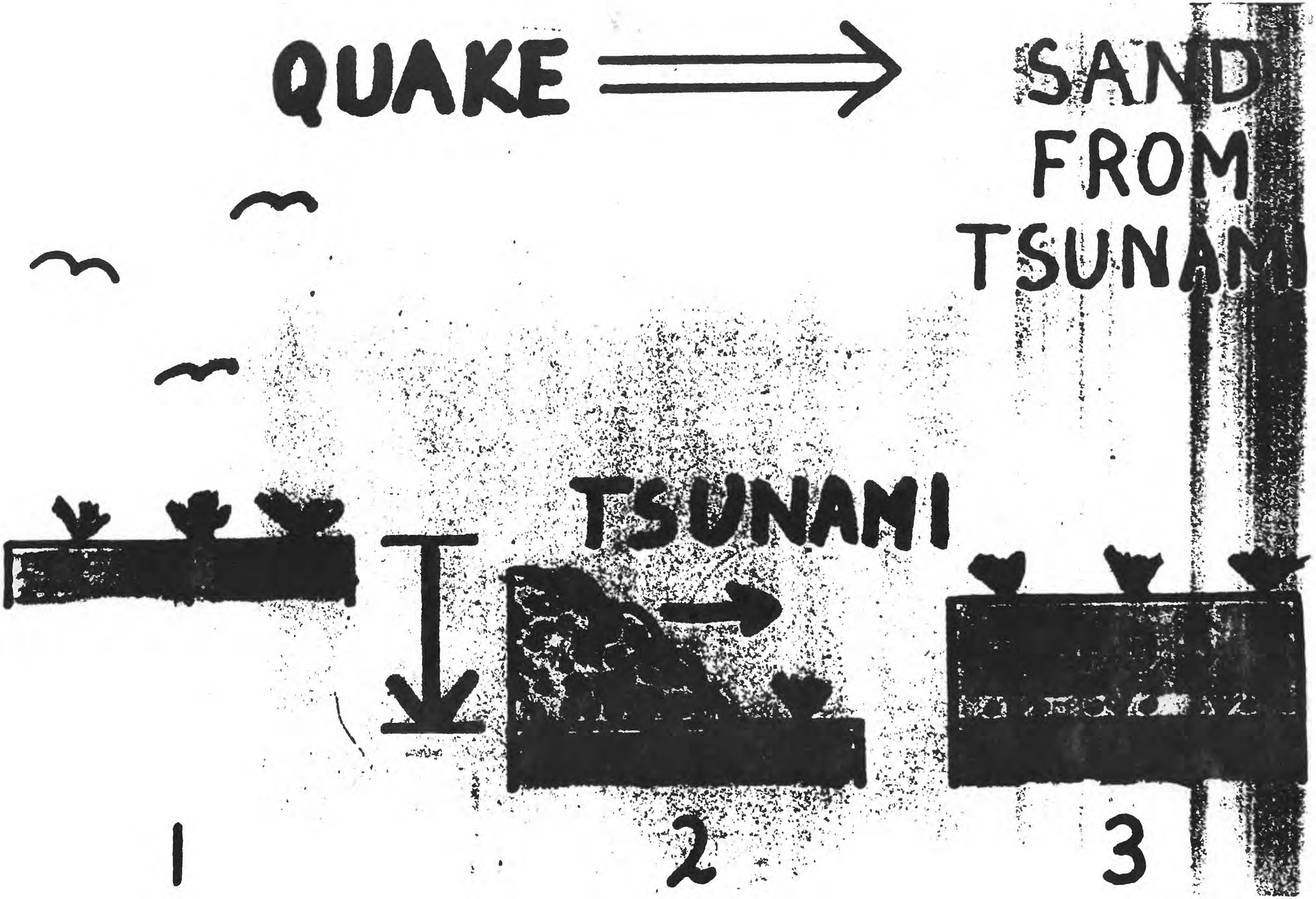
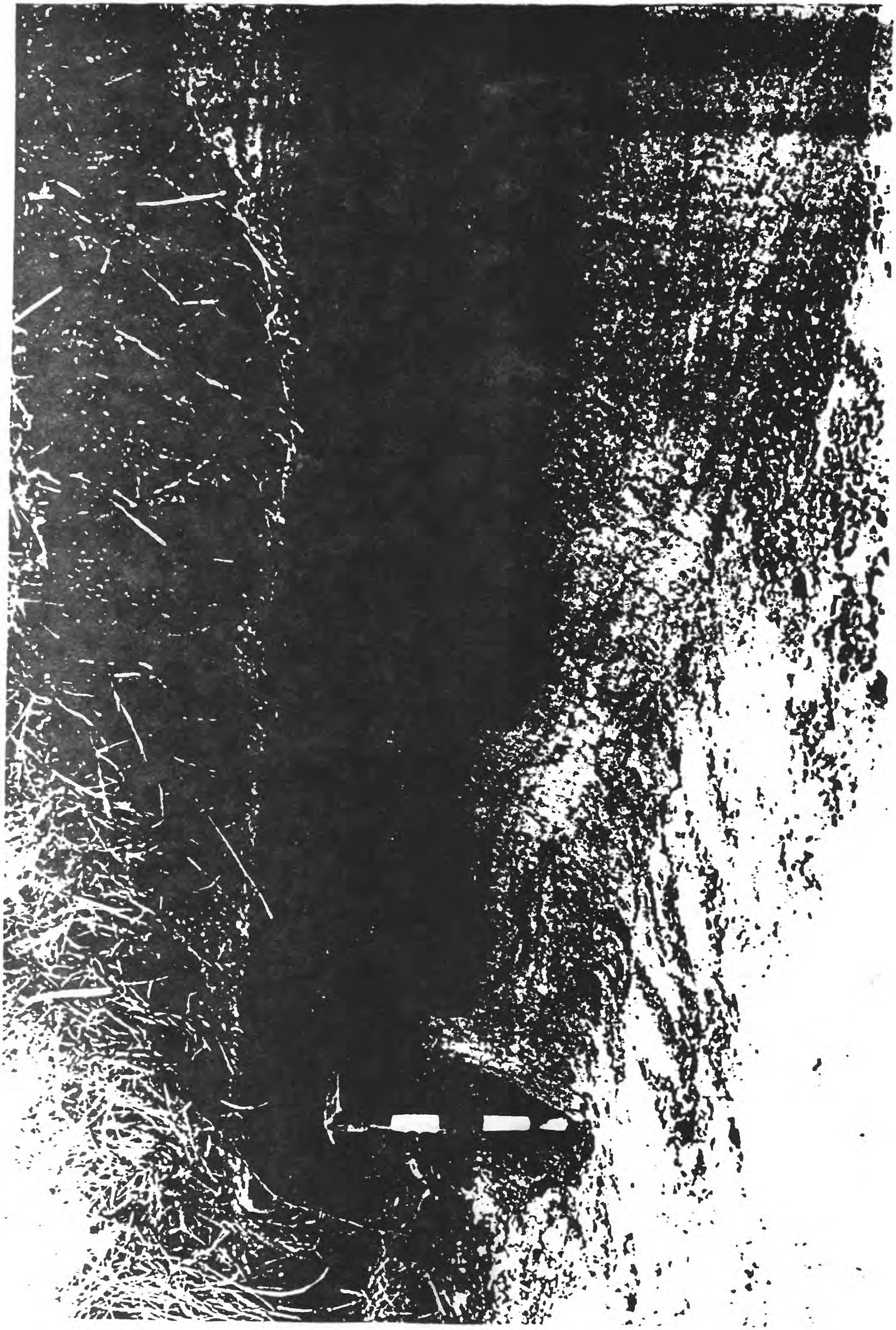
## Appendix M

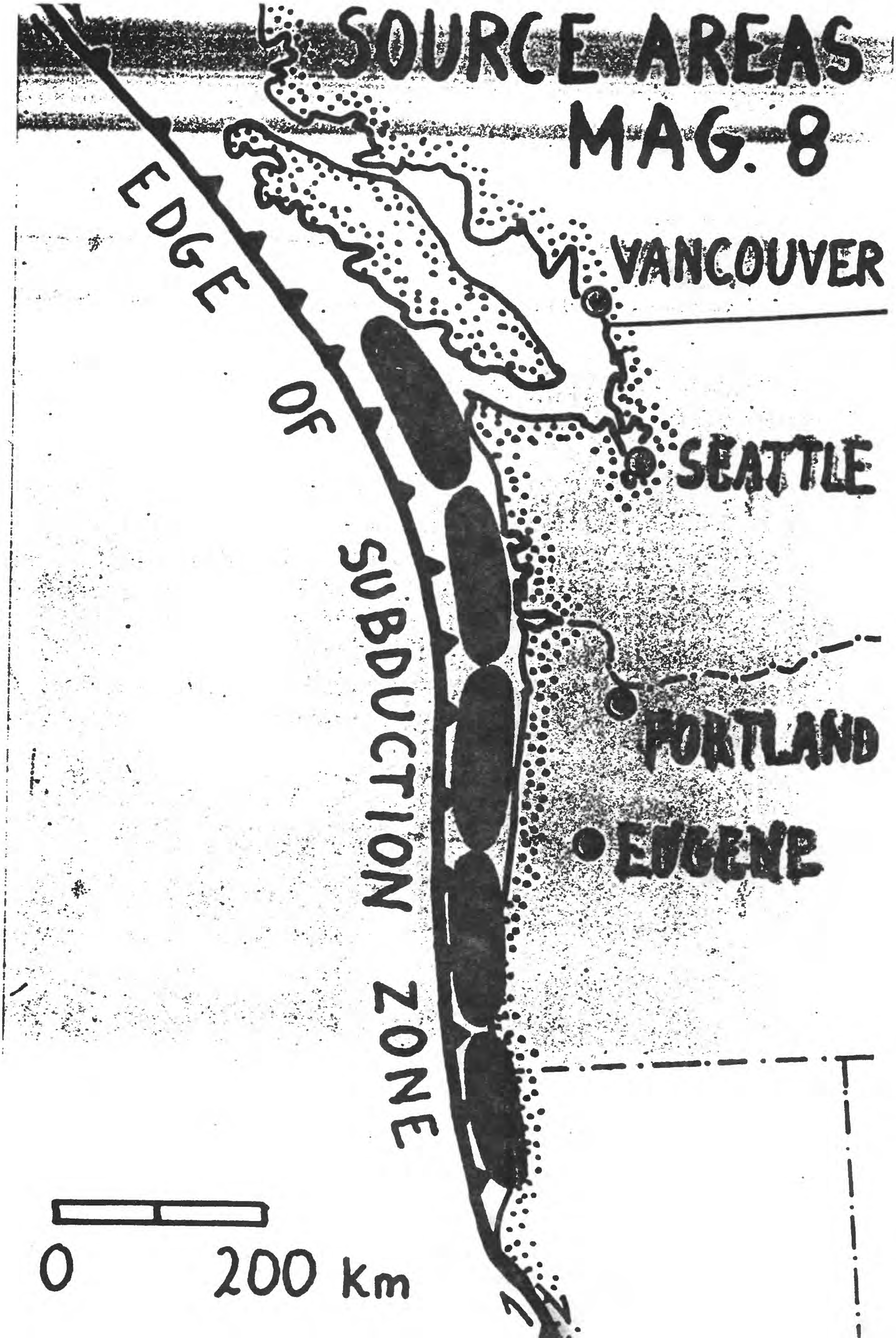
Illustrations presented to NEPEC by B.Atwater.


QUAKE
GHOST FOREST IN SALT MARSH
1
2
3



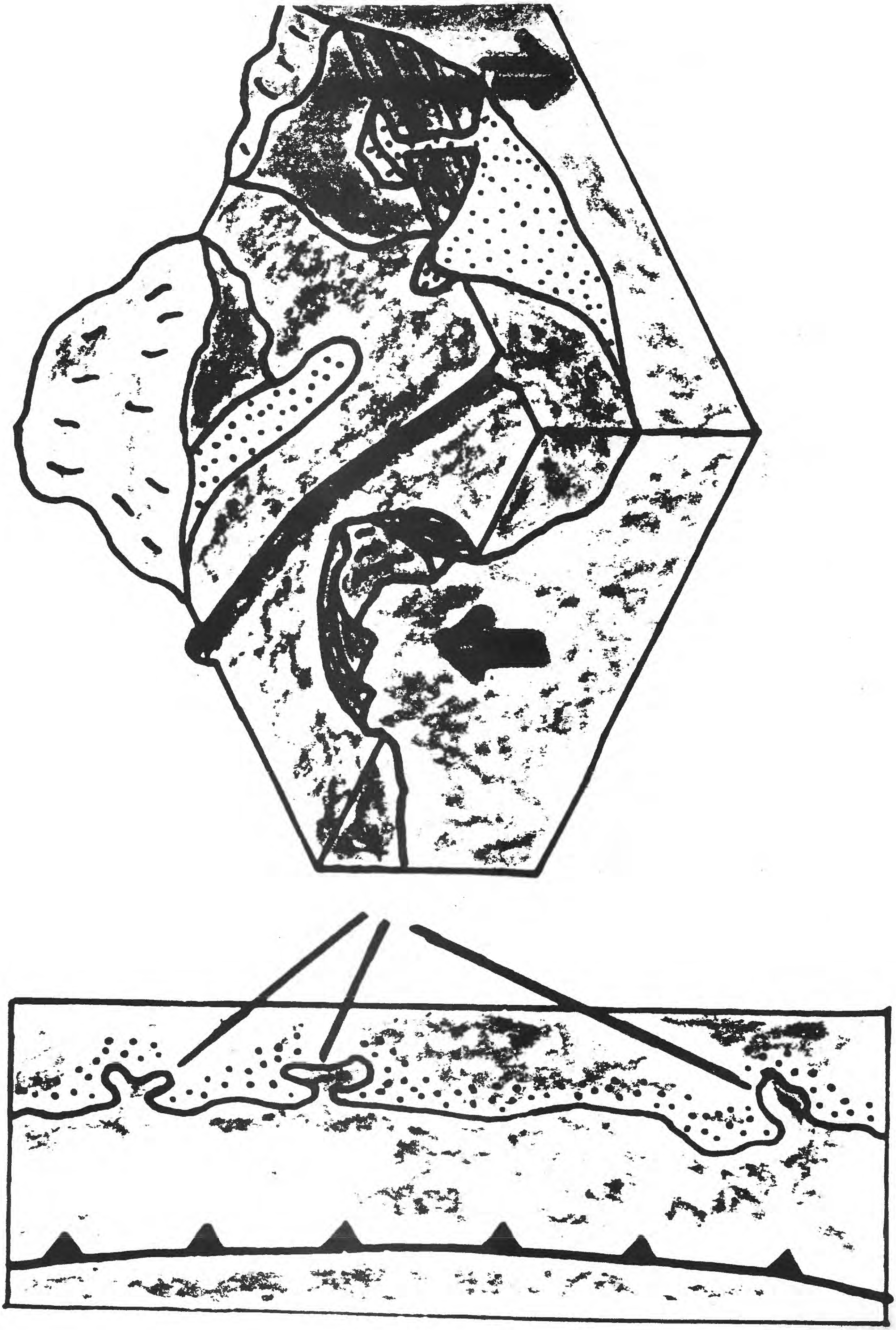

QUAKE
SAND FROM TSUNAMI
TSUNAMI
1
2
3

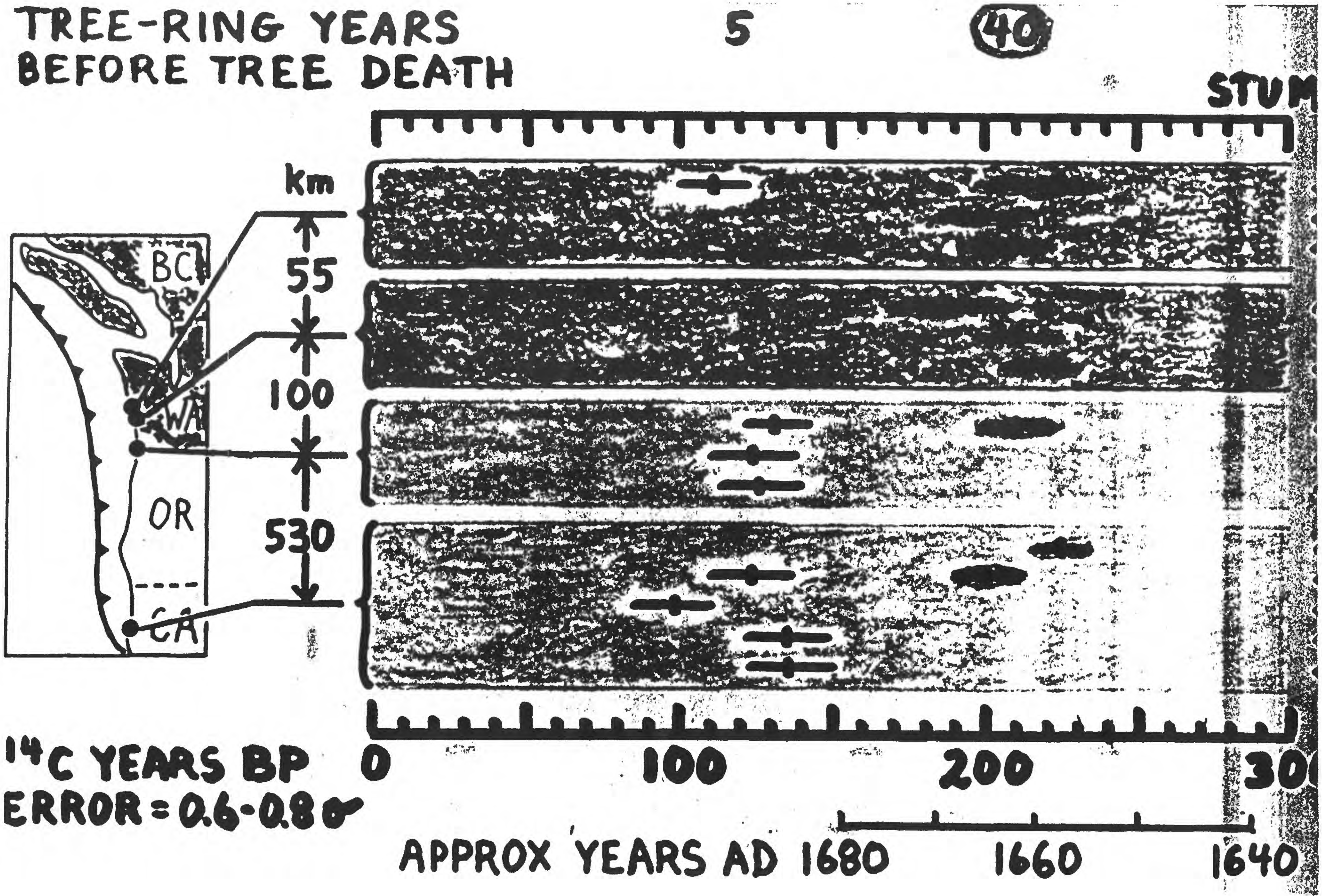



SOURCE AREAS
MAG. 8
VANCOUVER
SEATTLE
PORTLAND
EUGENE
EDGE OF SUBDUCTION ZONE
0 200 Km

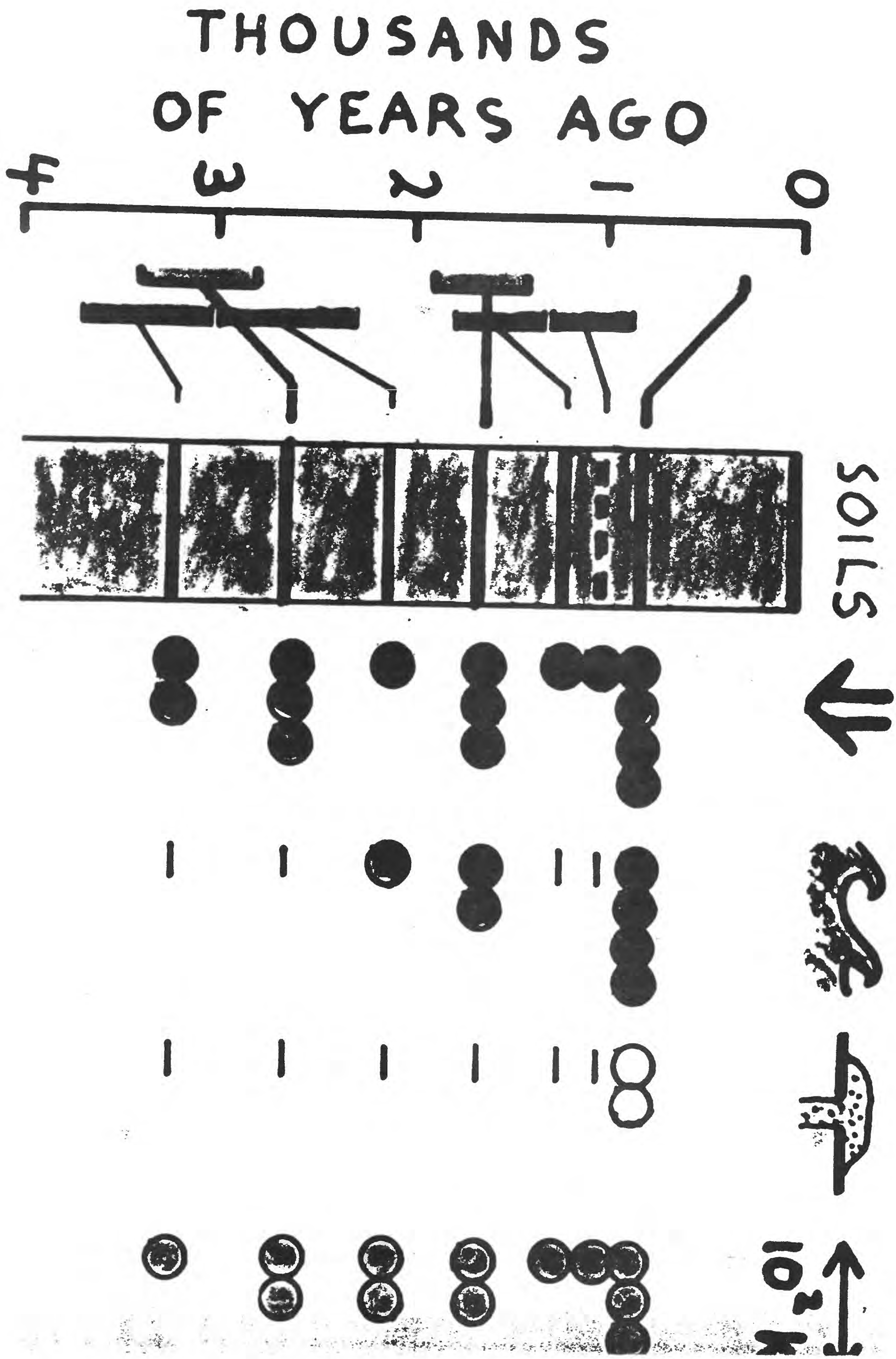
170


SOURCE AREA
MAG. 9
VANCOUVER
SEATTLE
PORTLAND
EUGENE
EDGE OF SUBDUCTION ZONE
0
200 km




TREE-RING YEARS
BEFORE TREE DEATH
5
40
STUM
km
55
100
530
BC
WA
OR
CA
14C YEARS BP
ERROR=0.6-0.8σ
0
100
200
30
APPROX YEARS AD 1680
1660
1640

173

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## Appendix N

Document sent to NEPEC by S.Obermeier and S.Dickenson.

*Executive Summary of the Draft Report*

# Some Limits For The Strength Of Subduction Zone Earthquake Shaking In Part Of Coastal Washington, During Late Holocene Time

Stephen F. Obermeier[1] and Stephen E. Dickenson[2]

Earthquake-induced liquefaction features discovered in islands in the lower Columbia River document strong shaking from a subduction zone earthquake that occurred near the coast about 300 years ago. However, the field evidence suggests that the region of strong shaking, from an engineering perspective, did not extend far inland. The region near Portland, Oregon, most likely experienced only minor shaking. The limited age of the sediments exposed in the Columbia River islands did not permit evaluation of the strength of shaking more than about 800 to 1000 years before present.

The liquefaction dikes in the Columbia River islands allow a preliminary assessment of the strength of earthquake shaking. The dikes systematically diminish in size and abundance upstream from Wallace Island. Wallace Island, located 60 km from the coast, is an especially important site because liquefaction effects are only marginally developed there, indicating that the threshold for liquefaction of the loose sandy soils was only slightly exceeded. Such slight development permits estimation of the surface strength of shaking, based on comparisons of liquefaction behavior of highly susceptible sediments worldwide. Downstream from Wallace Island, where liquefaction effects are much more abundant and the dikes are much larger, such comparison with worldwide observations permits only a more qualitative inference of the strength of shaking.

The extent of the liquefaction features discovered to date along the Columbia River and several smaller rivers and ascribed to the 300 year down-dropping event do not appear to follow the patterns observed following several large earthquakes in the western U.S. and Japan. First, the features discovered at the coast are much smaller than would be expected based on empirical relationships which describe the extent of liquefaction features as a function of both earthquake magnitude and the distance from the seismic energy source. Also, the isolated, diminutive features found at Wallace Island indicate that the threshold of liquefaction was just exceeded inferring an acceleration level on the order of 0.1g. Finally, no liquefaction features have been located more than 60 km inland. This field evidence is interpreted to indicate that the ground shaking in the lower Columbia River region associated with the postulated 300 year down-dropping event was less intense than has previously been assumed.

[1]S.F. Obermeier, Branch of Earthquake and Landslide Hazards, U.S. Geological Survey, Reston, VA 22092

[2]S.E. Dickenson, Department of Civil Engineering, Oregon State University, Corvallis, OR 97331

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The lack of field evidence for liquefaction along smaller rivers in the coastal areas of Washington suggests that there has not been exceptionally strong shaking along the coastal portions of Washington and northwestern Oregon during late-Holocene time (≈1000 y.b.p.). In addition, the lack of abundant, large-sized liquefaction features in highly susceptible soils throughout the coastal portion of the Columbia River that has been searched by Obermeier, seems to support the assumption that strong shaking has not extended very far inland. The field studies to date indicate that the extent of liquefaction related features is much less pronounced along the coastal rivers than would be anticipated based on experience from historic western U.S. earthquakes. Additionally, the accelerations associated with the formation of the observed liquefaction features indicate that the bedrock ground motions were much less intense than currently associated with postulated $M_w = 8$ Cascadia Subduction zone events. These preliminary findings have several possible implications for the specification of ground motions for engineered projects in the lower Columbia River region; (1) the large earthquake associated with the 300 y.b.p. down-dropping event was centered further offshore than is currently postulated, (2) the attenuation of ground motions is more pronounced in this region than is currently assumed, or (3) the magnitude of the 300 y.b.p. down-dropping event was less than $M_w = 8$. This data also seems to preclude the late-Holocene (last 1000 years) occurrence of a giant ($M_w \geq 9$) Cascadia subduction zone earthquake.

The field observations have been augmented with preliminary dynamic soil response analyses in an attempt to estimate the levels of ground shaking necessary to produce the liquefaction features discovered on Wallace Island. Recent ground motion studies of the Cascadia subduction zone provide estimates for the intensity and duration of strong ground motions generated during large to great subduction zone earthquakes. Ground motions at rock sites generated during a $M_w = 8$ event in the vicinity of the lower Columbia River are estimated to vary from approximately 0.18 to 0.25g at the coast to approximately 0.17g at Wallace Island. The duration of shaking associated with this event is on the order of 50 to 70 seconds. The intensity and extended duration of these ground motions would be expected to produce widespread liquefaction in the very loose to medium dense sandy soils which have been deposited along the rivers investigated in this study.

It is well established that bedrock motions are modified as they propagate through soil deposits. At the low to moderate levels of shaking at Wallace Island associated with the 300 y.b.p. subduction zone earthquake, moderate to deep deposits of loose or soft soils would tend to amplify the peak accelerations and enhance the duration of the ground motions. This dynamic soil behavior was exhibited during the 1949 Olympia, Washington earthquake ($M_s$=7.3) as the intensity levels were observed to increase at sites located on deep soils sites along the Columbia River. This increased intensity occurred despite an increasing distance from the rupture zone. The dynamic behavior of the soil deposits at the liquefaction sites must be assessed if limits on the intensity and attenuation of bedrock motions are to be ascertained. The field evidence of liquefaction is used to estimate the range of horizontal ground accelerations which occurred at the surface of the site. These estimates are used in conjunction with dynamic soil response analyses to deconvolve the surface motions to bedrock.

Dynamic soil response analyses have been performed for the Wallace Island site where a limited number of very small liquefaction features were discovered. Based on this field evidence it is inferred that the peak horizontal ground accelerations at this site were on the order of 0.1g. A suite of bedrock motions, which included recorded motions from large crustal earthquakes, as well as, simulated motions for a $M_w$=8.0 Cascadia subduction zone event, were used in the one-dimensional, equivalent linear dynamic soil response analyses. Based on ground motion attenuation relationships developed for Cascadia subduction zone earthquakes, the peak horizontal bedrock acceleration was taken as 0.17g. The computed surface accelerations ranged from 0.25g to 0.40g. This is in contrast to the estimate of 0.1g made based on the field evidence of minor liquefaction at the site. Parametric studies of the soil properties used as input for the dynamic response analyses were performed to assess the sensitivity of the analyses. The calculated horizontal accelerations consistently fell within the broad range listed. The results of these response studies complement the relations proposed in recent ground motion studies which estimate the ground motions on soil sites to range between 0.22g for firm soils of various thickness, and 0.28-0.37g for general alluvial soil profiles of various thickness. At the estimated levels of ground shaking the extent of the liquefaction features discovered at Wallace Island would most likely have been much more pronounced.

Subsequent response analyses were performed to estimate the level of bedrock motions required to produce peak surface accelerations of 0.1g. The computed bedrock accelerations ranged from 0.04g to 0.06g, much lower than that predicted by the current attenuation relationships for a $M_w$ = 8 event located near the coast. The use of the dynamic response analyses in conjunction with the field evidence of liquefaction effects is used to infer that strong ground motions generated during the 300 y.b.p. down-dropping event must not have extended very far inland.

Radiocarbon age dates of organic material obtained from soils at a study site on Wallace Island indicate that the soils at this site were deposited at least 830 y.b.p. The age of the soil profile combined with the lack of liquefaction features found to date at this site are tentatively interpreted as evidence that ground shaking levels have been relatively small in this area over the past 1000 years. A considerable amount of in situ soil data will have to be collected before the intensity of ground motions at Wallace Island can be more reliably estimated. A pilot project is scheduled for the summer of 1993 to characterize the engineering properties of the near surface soils at Wallace Island and adjacent sites.

## Appendix O

Illustrations used by M.Wyss during presentation on Shumagin event.

**THE SHUMAGIN ISLANDS EARTHQUAKE OF 13 MAY 1993**

Captions for Figures presented to NEPEC by
Max Wyss
Geophysical Institute, University of Alaska
4 June 1993

**Figure 1:** Seismicity map of the eastern Aleutians-Alaska Peninsula area using the PDE and weekly reports of NEIC for 1 August 1990 through 28 May 1993. Stars denote epicenters of May 1993. (Figure supplied by C. Stephens, USGS, Menlo Park).

**Figure 2:** Macroseismic map for the Shumagin earthquake of 13 May 1993. Solid dot indicates the epicenter relocated by E. R. Engdahl (USGS, Golden).

**Figure 3:** Moment tensor solution for the Shumagin earthquake of 13 May 1993. (Figure supplied by S. Jaume, Lamont Doherty Earth Observatory).

**Figure 4:** Map of the aftershock locations of the Shumagin earthquake (circles) during the first two weeks. Locations were calculated from S-P travel times observed at stations Sand Point and Dutton (Figure 9). Dots with magnitudes next to them mark the epicenters of the larger events that could be located teleseismically. The events at 100 to 300 km distance from the mainshock occurred on May 27/28 and suggest a general increase of activity in the area triggered by the mainshock. Aftershock areas of large and great historic earthquakes are outlined and their years of occurrence and magnitudes are given. The mainshock epicenter is relocated by Jaume based on the Sand Point S-P time.

**Figure 5:** Aftershock area of the Shumagin earthquake of 13 May 1993 compared to the segments of the Aleutian arc in the vicinity of the Shumagin gap which have broken in historic earthquakes, delineated by lines perpendicular to the arc. It appears that the recent mainshock re-ruptured approximately the segment that ruptured in 1917.

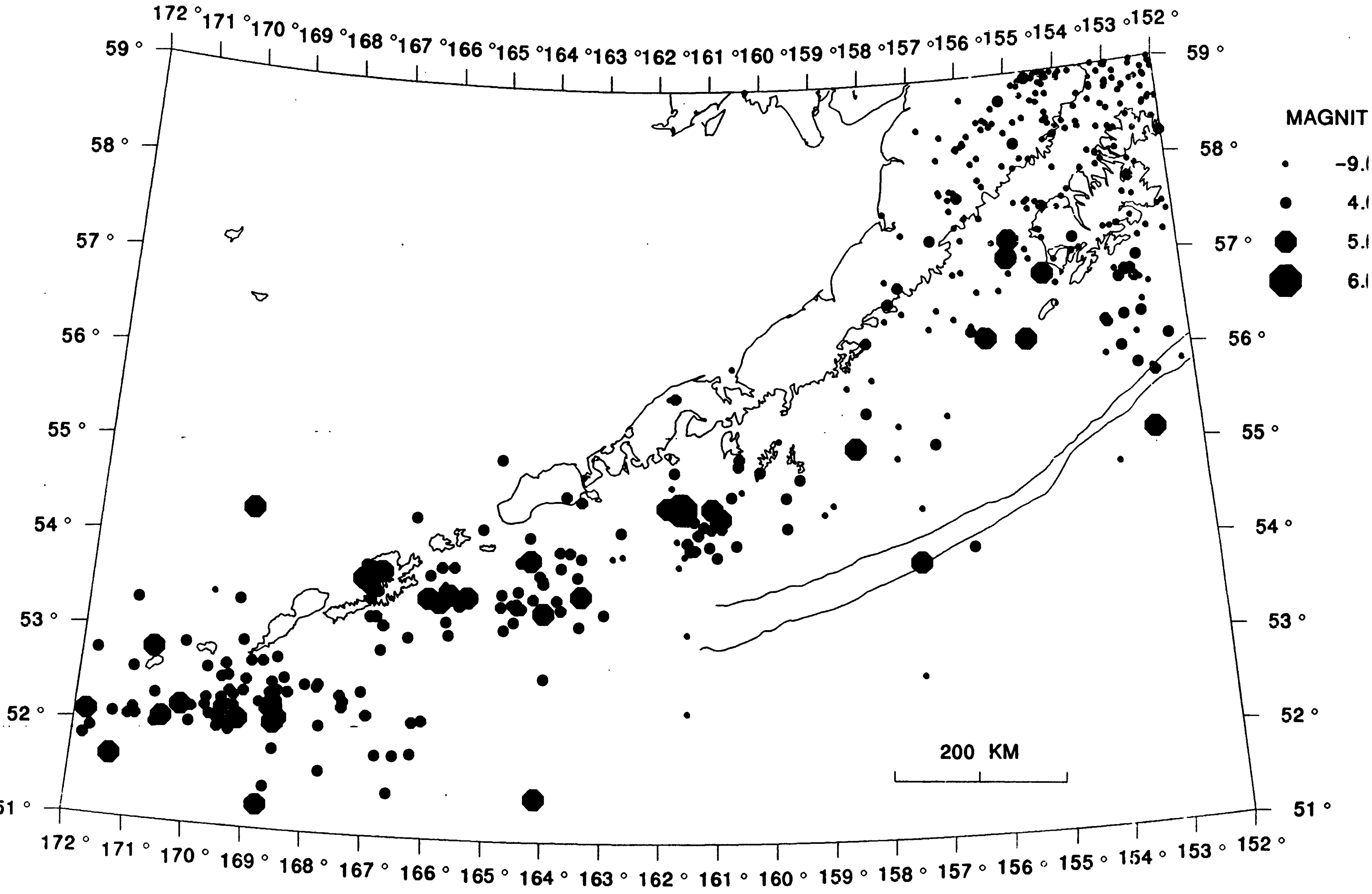
**Figure 6:** Cross-section of the Aleutian subduction zone at the Shumagin Islands (AA') and at a location about 100 km further west (BB'). Small circles show hypocenters of earthquakes located by the Shumagin seismograph network operated to mid-1991 by Lamont. The fault planes and slip directions for larger events are shown. The fault plane solution and location (large circle) of the May 13, 1993 earthquake show that it was a rupture on the main subduction thrust plane. (Figure supplied by S. Jaume, Lamont).

**Figure 7:** The b-value of the frequency-magnitude relation of the 13 May 1993 Shumagin earthquake (b=.38) is unusually low compared to b=1.0 of the background activity in the Shumagin area. However, the aftershock sequence of the M=6.1 Shumagin earthquake of July 1987 (for epicenter see Figure 4) is also low. Low b-values are characteristic of foreshock sequences. The interpretation that the sequence that started on May 13, 1993 is a foreshock sequence (because of the low b-value) may be rejected on the grounds that aftershock sequences in the Shumagin area may have low b-values in general, as in July 1987.

**Figure 8:** Cumulative seismic moment as a function of time in the Shumagin seismic gap area. The accelerating moment release rate is taken to indicate that a gap-filling earthquake may occur soon. (Figure supplied by S. Jaume, Lamont).

**Figure 9:** Map showing the location of Sand Point (SDN) and Mt. Dutton (DTN), the only two seismographs (triangles) that were operating within about 600 km of the May 13, 1993 epicenter. DTN records on paper only. Six portable REFTECH seismographs supplied by IRIS were deployed at the locations marked by diamonds. The three locations in the Shumagin Islands are on Nagai, Chernabura, and Semenof.

900801 - 930512 (HDF, PDE, QED)




C. Stephens

Fig. 1

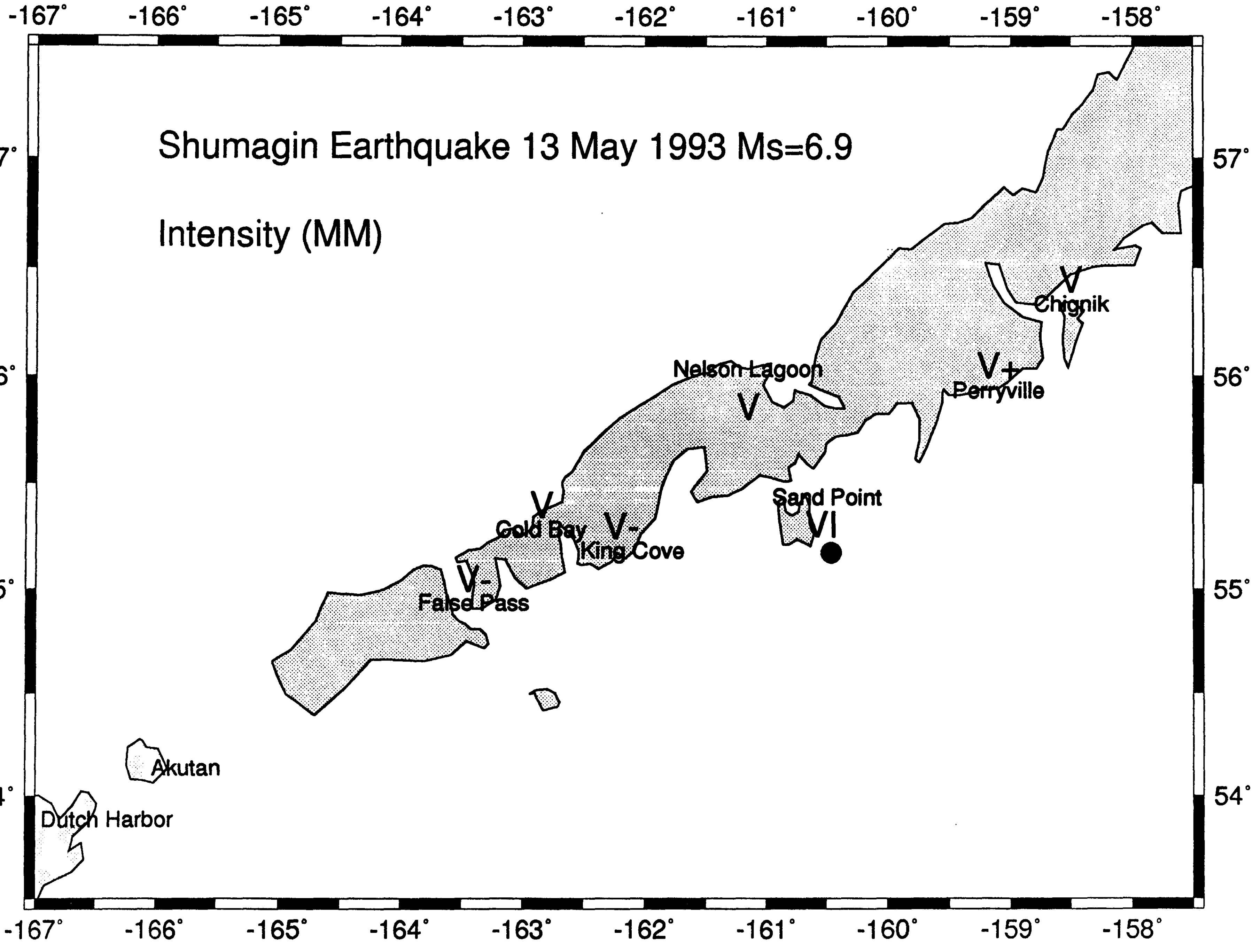

Shumagin Earthquake 13 May 1993 Ms=6.9
Intensity (MM)
-167° -166° -165° -164° -163° -162° -161° -160° -159° -158°
57° 56° 55° 54°
V
Chignik
V+
Perryville
Nelson Lagoon
V
Sand Point
VI
V
Cold Bay
V-
King Cove
V-
False Pass
Akutan
Dutch Harbor

Fig. 2

# P-WAVES

Strike:256, Dip:24, Rake:100

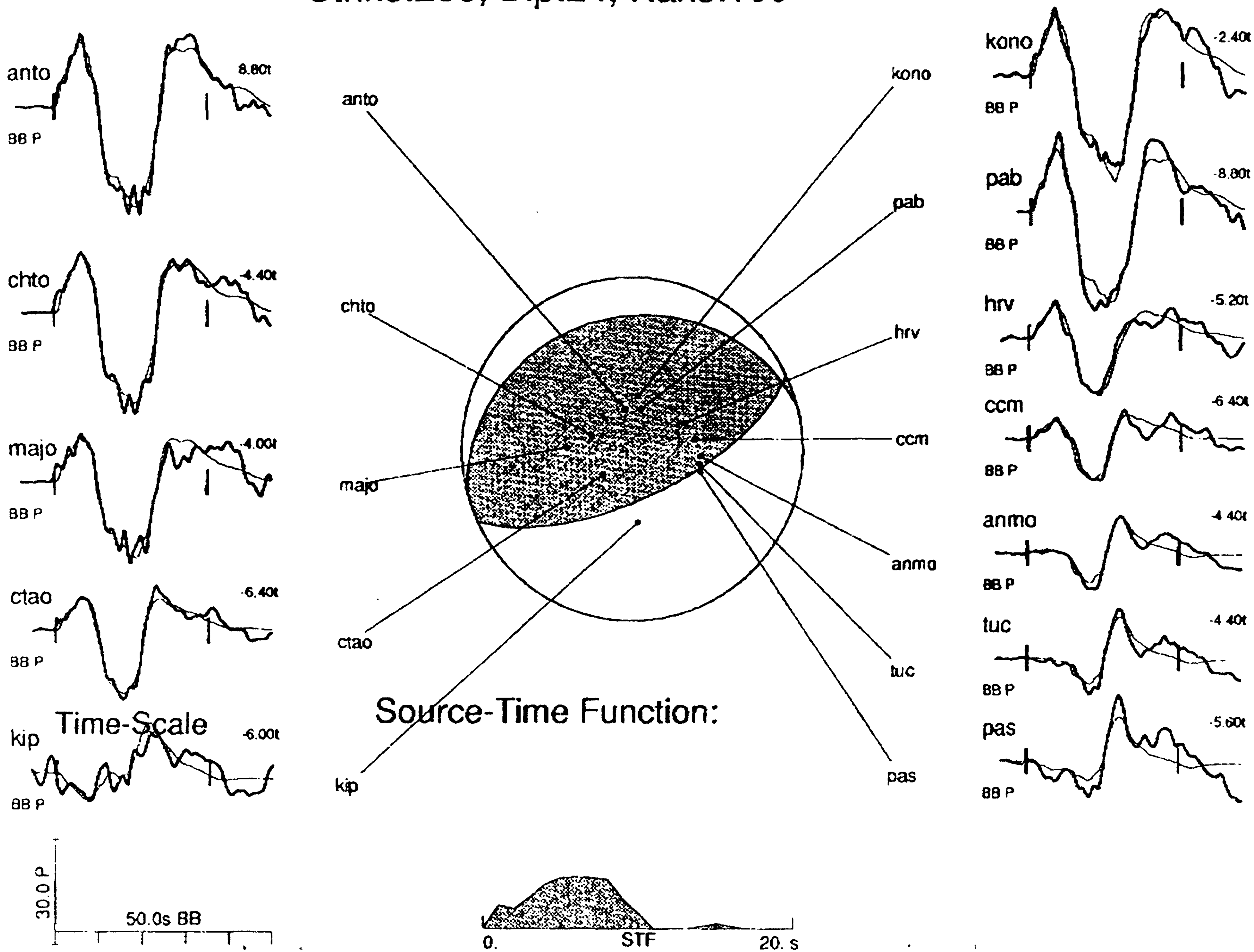



Fig 3

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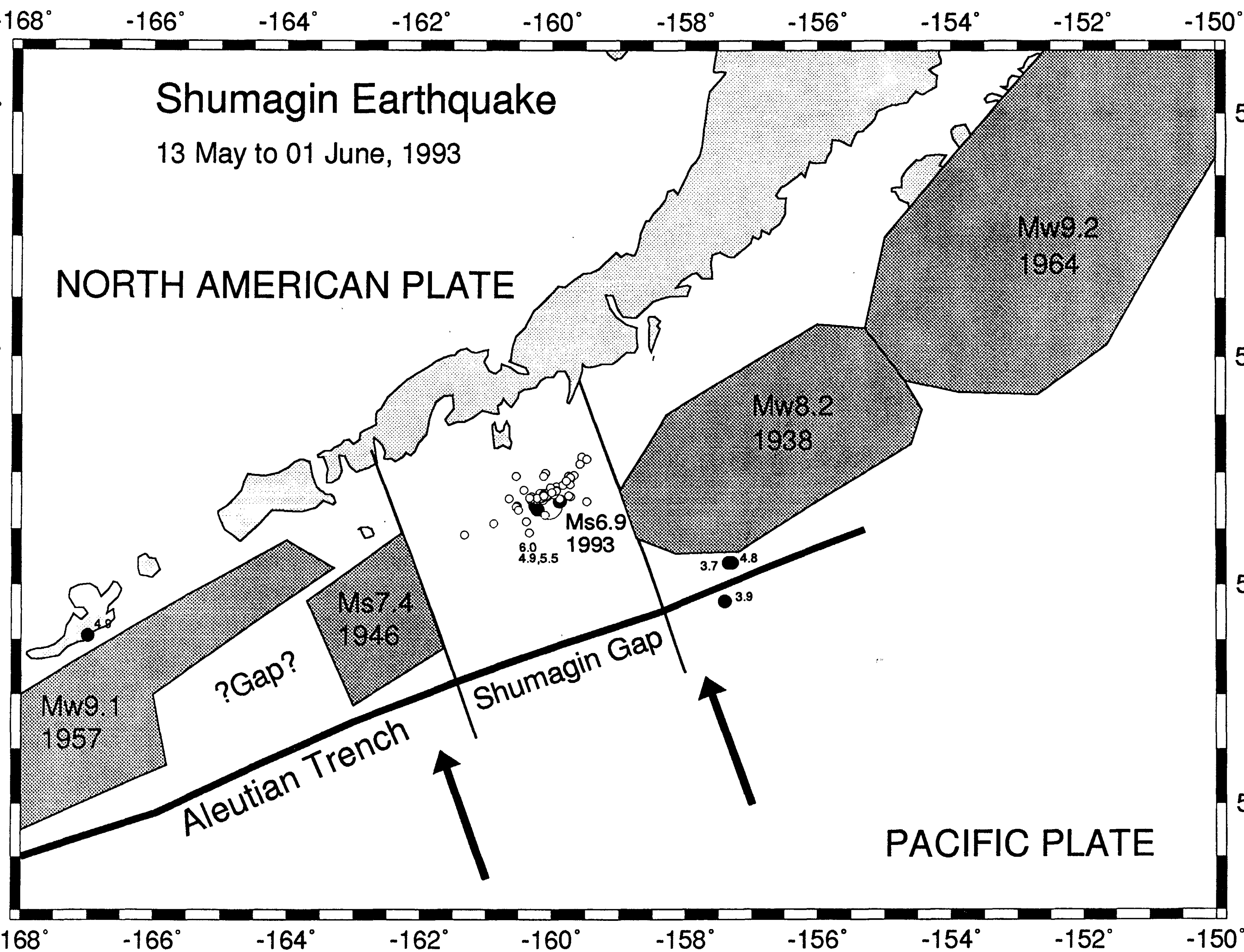
Shumagin Earthquake
13 May to 01 June, 1993
NORTH AMERICAN PLATE
PACIFIC PLATE
Mw9.2
1964
Mw8.2
1938
Ms6.9
1993
Ms7.4
1946
Mw9.1
1957
?Gap?
Shumagin Gap
Aleutian Trench
6.0
4.9,5.5
3.7
4.8
3.9
-168°
-166°
-164°
-162°
-160°
-158°
-156°
-154°
-152°
-150°
58°
56°
54°
52°

Fig. 4

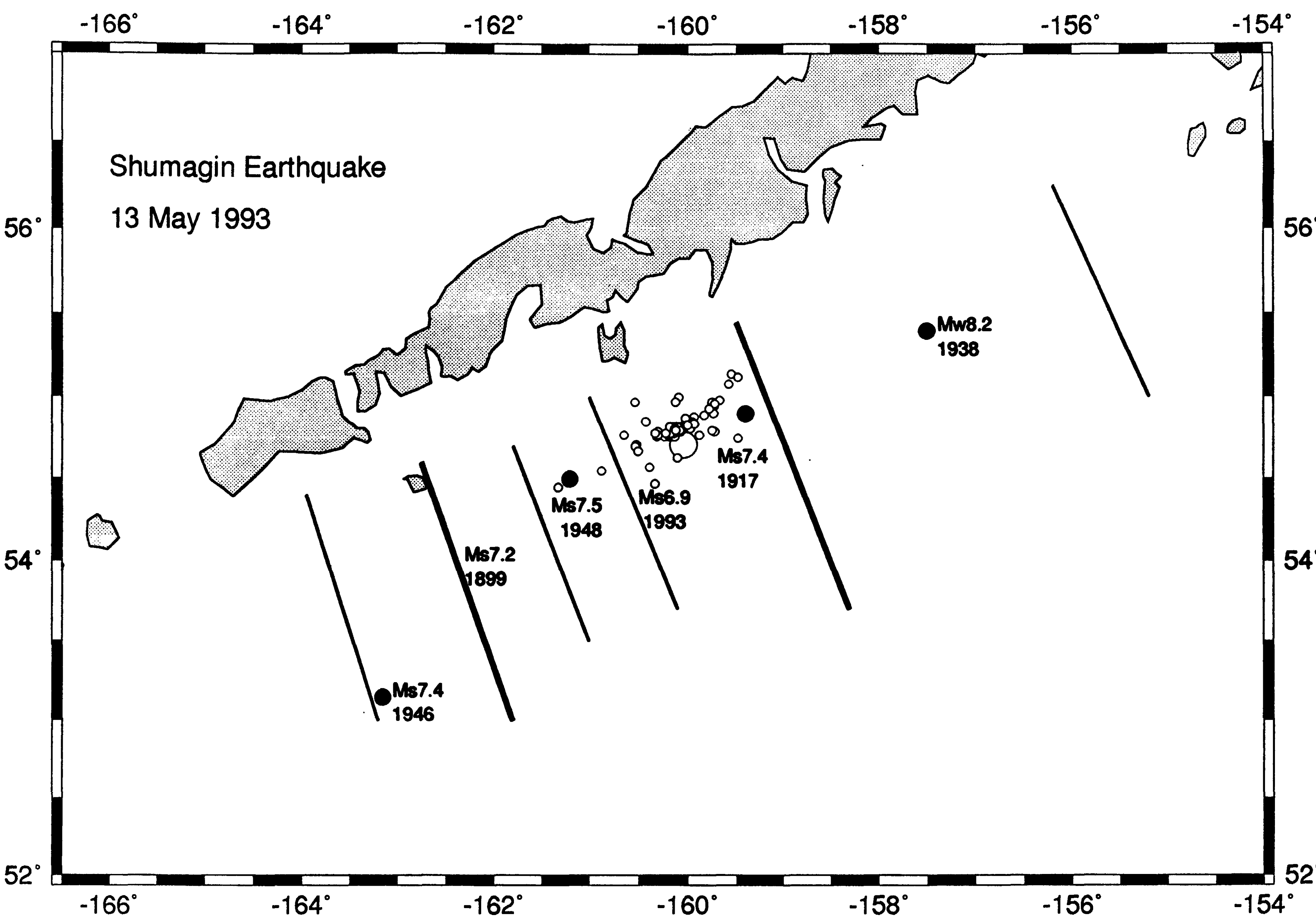
Shumagin Earthquake
13 May 1993
-166°
-164°
-162°
-160°
-158°
-156°
-154°
56°
54°
52°
Mw8.2
1938
Ms7.4
1917
Ms6.9
1993
Ms7.5
1948
Ms7.2
1899
Ms7.4
1946

Fig. 5

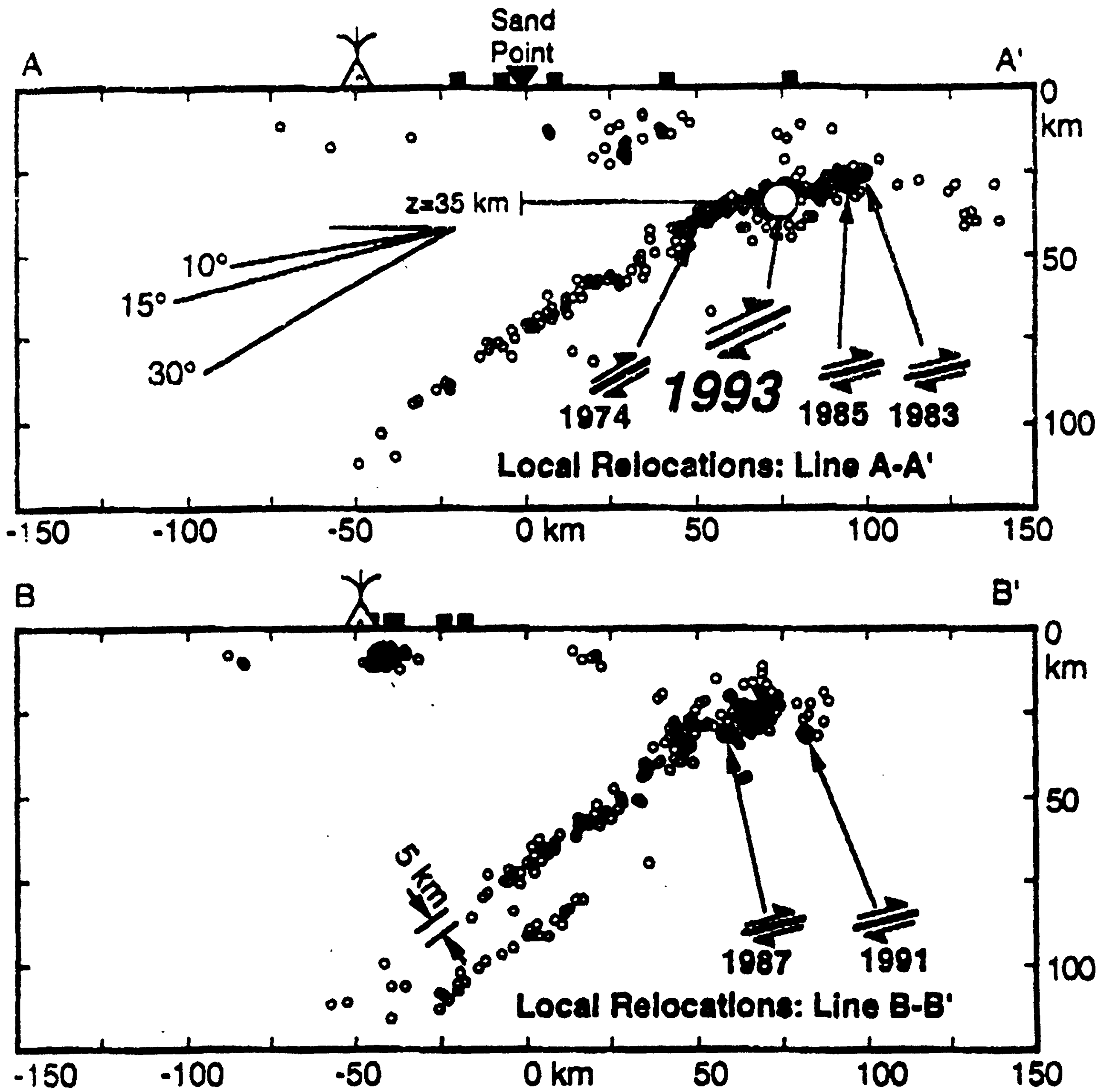
Sand
Point
A
A'
0
km
z=35 km
10°
15°
30°
50
1974
1993
1985
1983
100
Local Relocations: Line A-A'
-150
-100
-50
0 km
50
100
150
B
B'
0
km
50
5 km
1987
1991
100
Local Relocations: Line B-B'
-150
-100
-50
0 km
50
100
150

Fig. 6

186

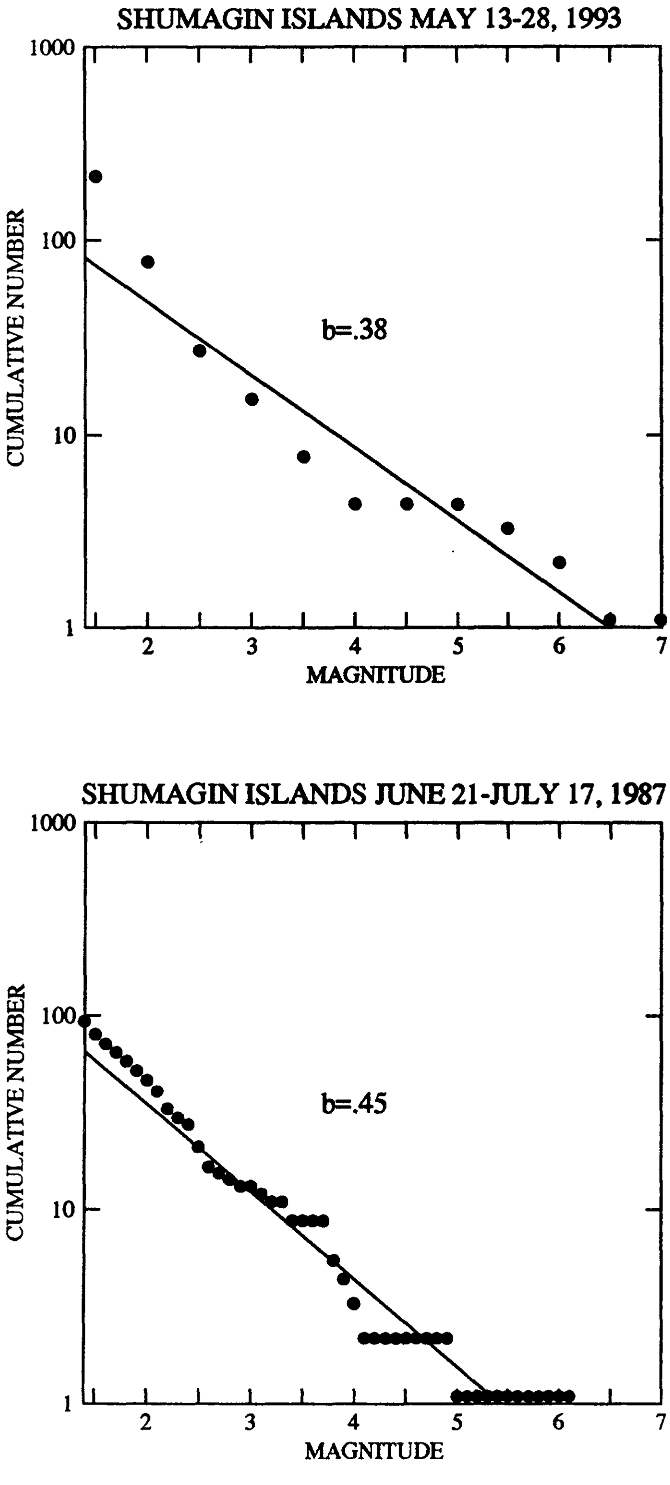
SHUMAGIN ISLANDS MAY 13-28, 1993
1000
100
10
1
CUMULATIVE NUMBER
b=.38
2
3
4
5
6
7
MAGNITUDE
SHUMAGIN ISLANDS JUNE 21-JULY 17, 1987
1000
100
10
1
CUMULATIVE NUMBER
b=.45
2
3
4
5
6
7
MAGNITUDE

Fig. 7

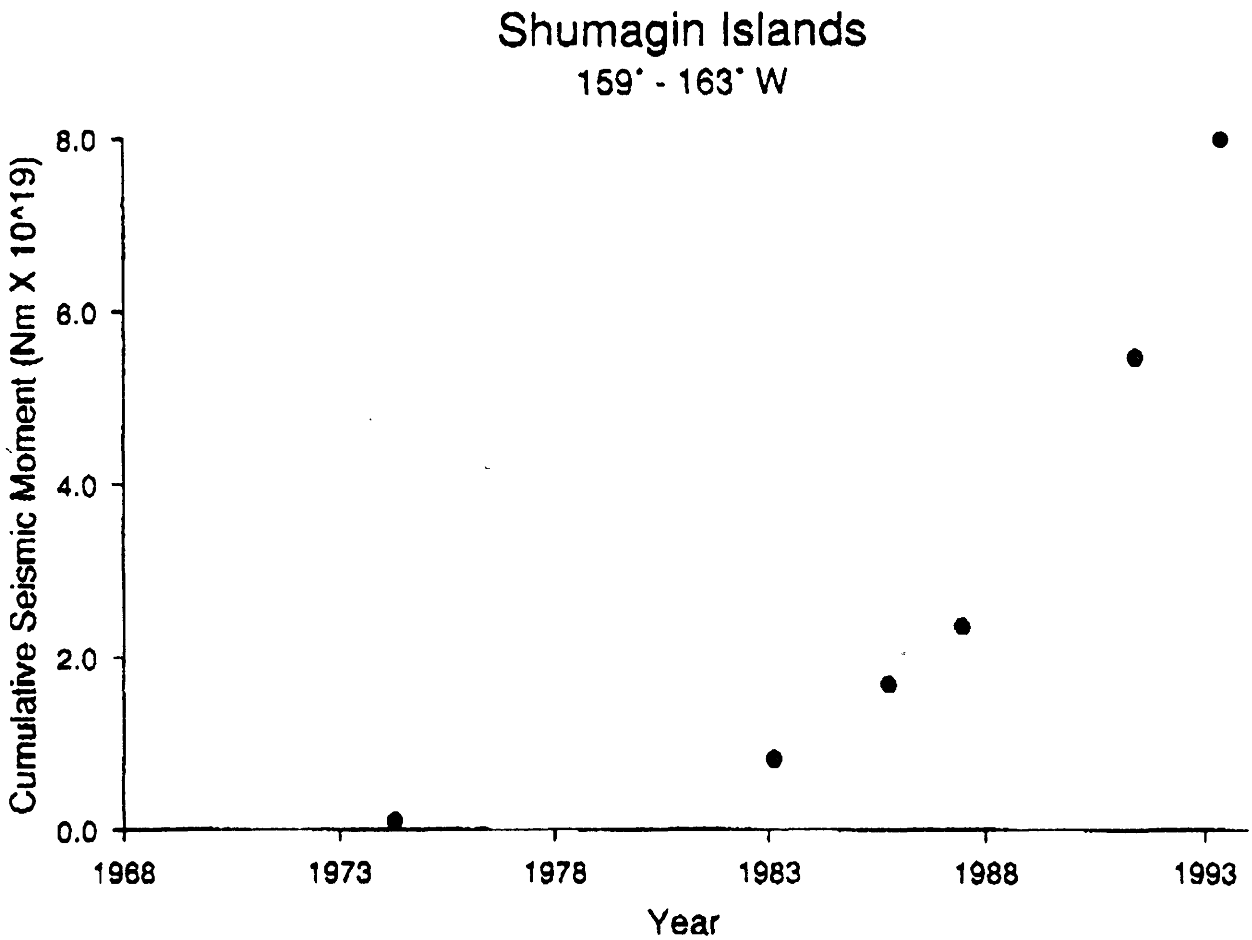
Shumagin Islands
159° - 163° W
Cumulative Seismic Moment (Nm X 10^19)
8.0
6.0
4.0
2.0
0.0
1968
1973
1978
1983
1988
1993
Year

Fig. 8

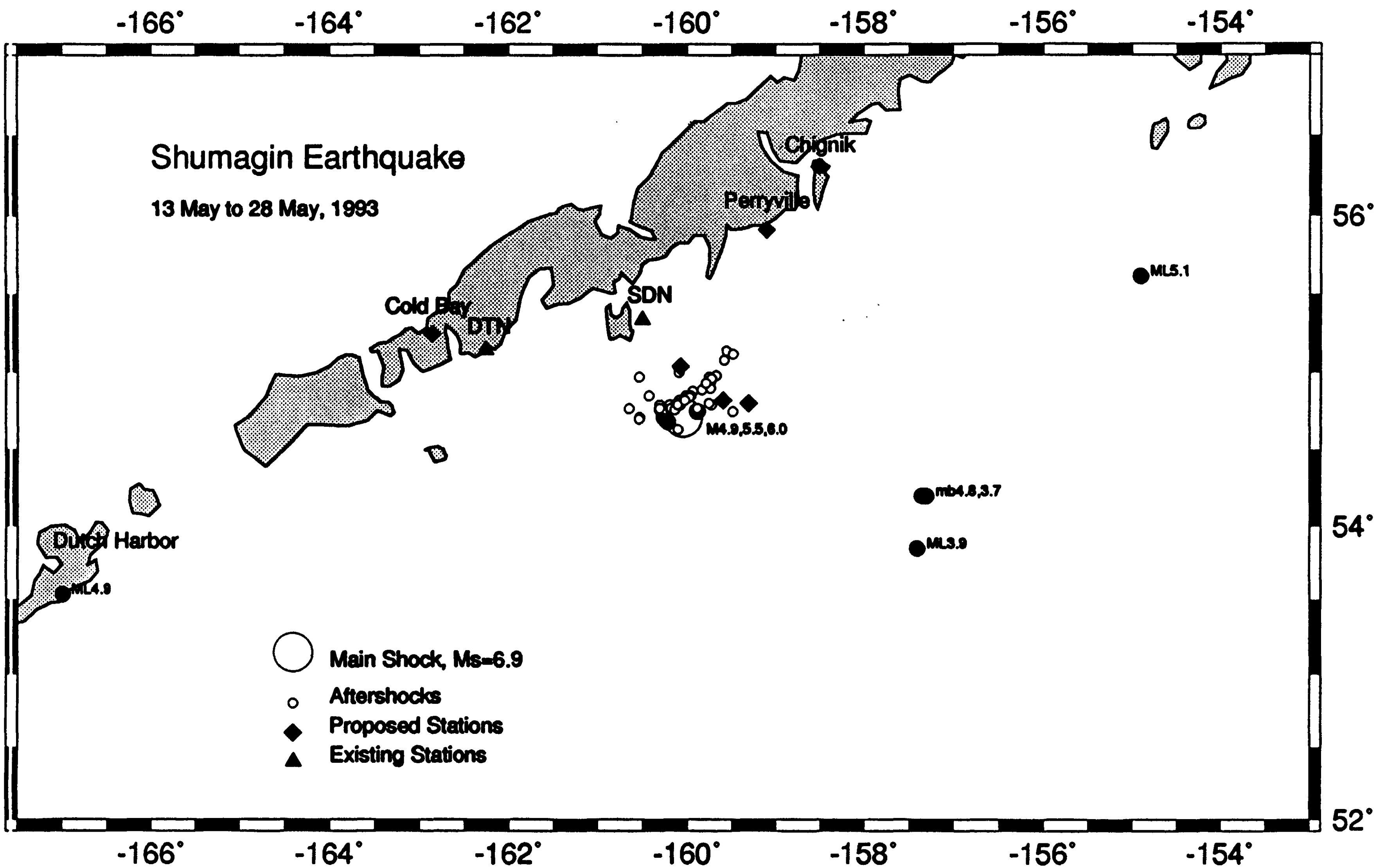
-166°
-164°
-162°
-160°
-158°
-156°
-154°
56°
54°
52°
Shumagin Earthquake
13 May to 28 May, 1993
Chignik
Perryville
ML5.1
Cold Bay
DTN
SDN
M4.9,5.5,6.0
mb4.8,3.7
ML3.9
Dutch Harbor
ML4.9
Main Shock, Ms=6.9
Aftershocks
Proposed Stations
Existing Stations

Fig. 9

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## Appendix P

Illustrations used by M.Wyss during presentation on seismicity in Hawaii.

**SEISMIC HAZARD IN HAWAII**

Captions for Figures Presented to NEPEC by
Max Wyss
Geophysical Institute, University of Alaska
4 June 1993

**Figure 1:** Map of the maximum historically reported modified Mercalli intensity for Hawaii and for the Hawaiian archipelago. Most of the areas have experienced shaking of intensity VIII and above (from Wyss and Koyanagi, 1992b).

**Figure 2:** Macroseismic map of the strong ground shaking due to the largest historical earthquake in Hawaii, 2 April 1868 M7.9. The southern part of Hawaii experienced severe destruction (from Wyss, 1988).

**Figure 3:** Macroseismic map for the earthquake of 22 January 1938, M6 3/4, located near the center of the archipelago. The high intensities show that a serious seismic hazard exists in the center of the archipelago. Although no major earthquakes occurred in this area during the last 55 years, several of them occurred during the second half of the last century (from Wyss and Koyanagi, 1992b).

**Figure 4:** Map of epicenters with year of occurrence for earthquakes on Hawaii for which macroseismic maps could be constructed (period 1832 to 1991, Wyss and Koyanagi (1992b). The southern part of Hawaii is seismically most active. Dots, circles and asterisks mark epicenters located instrumentally, macroseismically and at sub-crustal depth.

**Figure 5:** Structural cross section of Hawaii (from Hill and Zucca, 1987) showing the volcanic deposits which form the edifice of the volcano that rest on and depress the oceanic crust. The oceanic sediments at the base of the edifice form a decollement plane, along which slip of the upper crust occurs in earthquakes, away from the volcanic centers (the arrow shows the direction of slip at the hypocenter of the 1868 great earthquake). The hypocenter of the great earthquake of March 1868 is shown by a star (from Wyss, 1988).

**Figure 6:** The map of approximate extent of historic rupture areas on Hawaii shows seismic gaps between these rupture areas. These gaps may be capable of M6.5 to M7.5 earthquakes. No microseismic activity is detected south of the dotted line. (From Wyss and Koyanagi, 1992a).

**Figure 7:** Cross section (EW) through southern Hawaii showing the segment of the decollement plane that is identified as a seismic gap in Figure 6 (shaded). The years of rupture are indicated in the segments of the decollement plane that have ruptured. The directions of greatest and least principal stress as calculated by Gillard et al. (1992) and Liang and Wyss (1991) are also shown (from Wyss and Koyanagi, 1992a).

**Figure 8:** Map of the slip directions (arrows) of the upper crust of Hawaii on the 9 to 14 km deep decollement plane. Because this slip is directed away from Mauna Loa the pressure due to magmatic intrusions is probably the major driving force for large earthquakes in Hawaii (Figure modified after Liang and Wyss, 1991 and Wyss et al., 1992).

**Figure 9:** Schematic map of the historic epicenters of the Kaoiki mainshocks with M>=5.5. Instrumentally calculated and macroseismic epicenters are marked by dots and circles, respectively.

**Figure 10:** Event number as a function of time for the Kaoiki earthquake sequence. The hypothesis that Kaoiki mainshocks occur at regular intervals was formulated on the basis of the post-1940 data and tested

successfully by investigating macroseismic reports on Hawaiian earthquakes back to 1912 (Wyss, 1986). The size of the dots is proportional to the magnitude of these mainshocks which appear to alternate in size. The last two Kaoiki mainshocks caused surface ruptures of right lateral slip along near vertical NE striking planes. The smaller of these in 1973 was followed exclusively by aftershocks of strike-slip type outlining the NE-striking rupture plane. The larger 1983 mainshock was followed by mainly aftershocks of decollement type in a 10 km radius around the epicenter. Hence the proposal that the Kaoiki mainshocks with M<6 are strike-slip events only, whereas M>6 events involve the strike slip as well as the decollement plains. The average interevent time between these mainshocks is 10.4 years with a standard deviation of 1.5 years. Adding this interevent time to the last occurrence date suggests that mid-1994 is the most likely time for the next Kaoiki mainshock. (Figure extended from Wyss, 1986b).

**Figure 11:** The cumulative number of earthquakes as a function of time for a subvolume of the Kaoiki area shows a pronounced seismic quiescence during two years before the November 1983 mainshock (occurrence time at the right edge of the plot). The standard deviate z, which is proportional to the significance of the rate decrease, shows very high values (lower curve) which indicate that the change can be identified with high confidence. If the next Kaoiki mainshock is of the smaller variety, it may be difficult or impossible to resolve seismic quiescence, because of the small volume of the expected anomaly. (from Wyss, 1986a).

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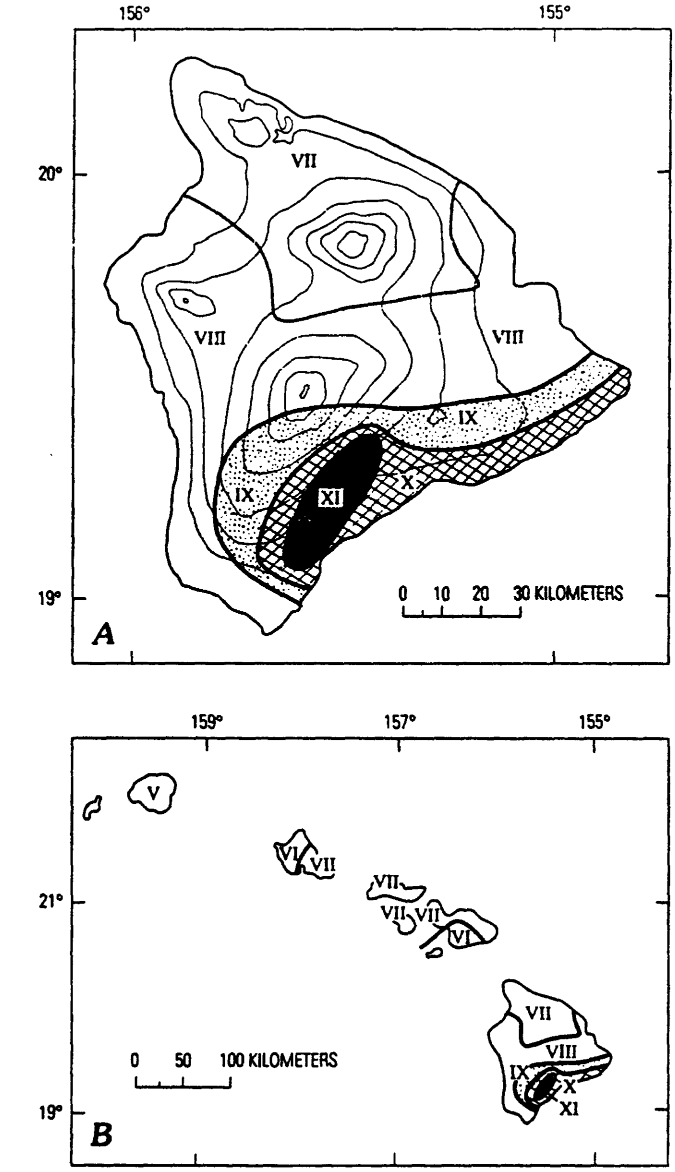
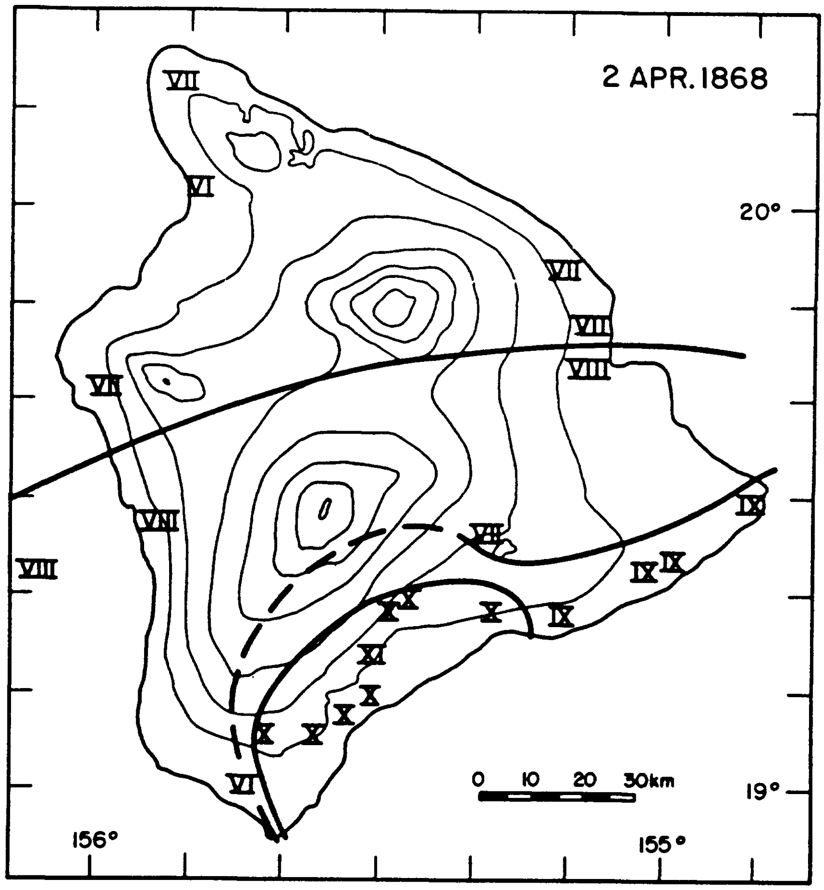

156°
155°
20°
19°
VII
VIII
VIII
IX
IX
X
XI
0 10 20 30 KILOMETERS
A
159°
157°
155°
21°
19°
V
VI
VII
VII
VII
VII
VI
VII
VIII
IX
X
XI
0 50 100 KILOMETERS
B

Fig. 1


2 APR. 1868
20°
19°
156°
155°
0 10 20 30km
VII
VI
VIII
IX
X
XI

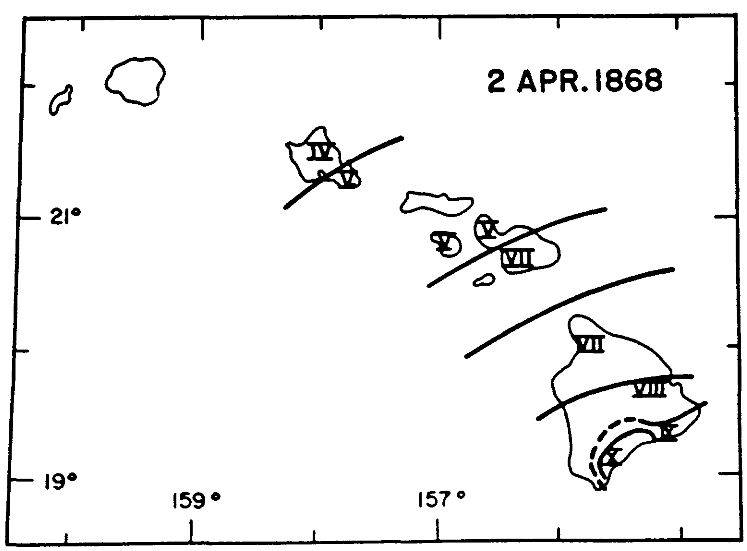
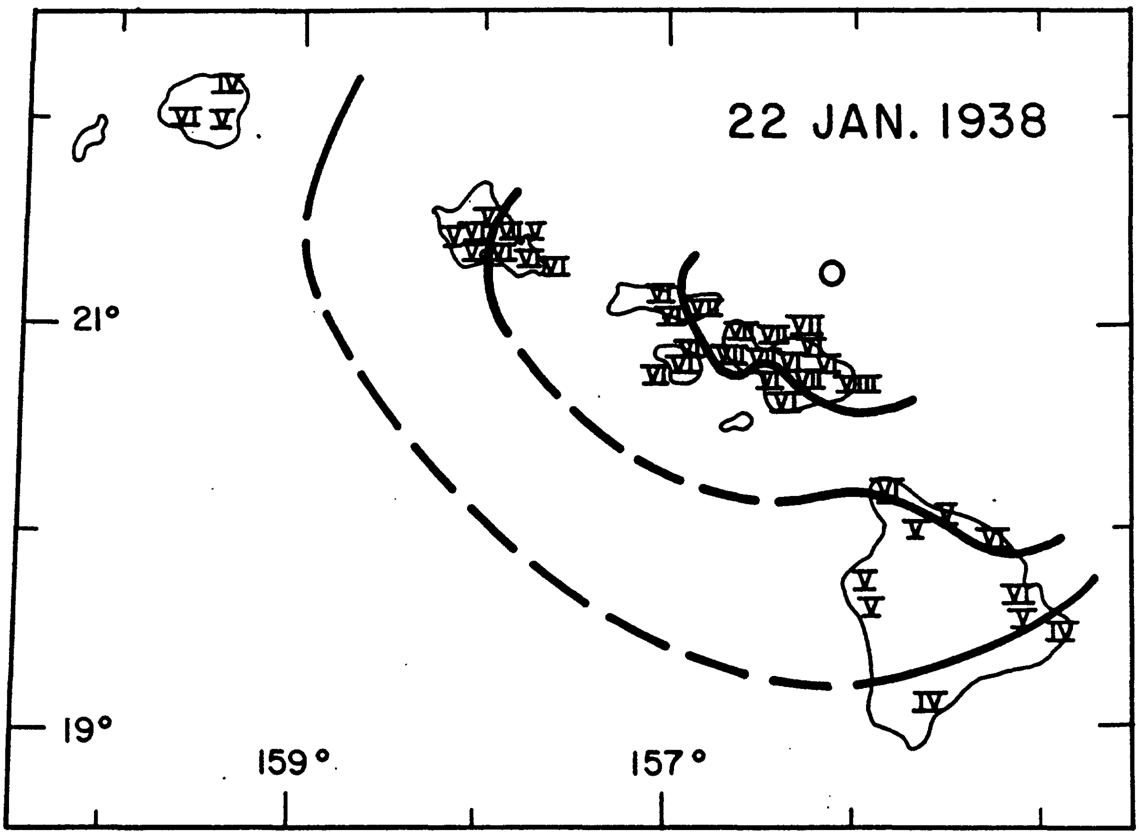

2 APR. 1868
21°
19°
159°
157°
IV
V
VII
VIII
IX

Fig. 2

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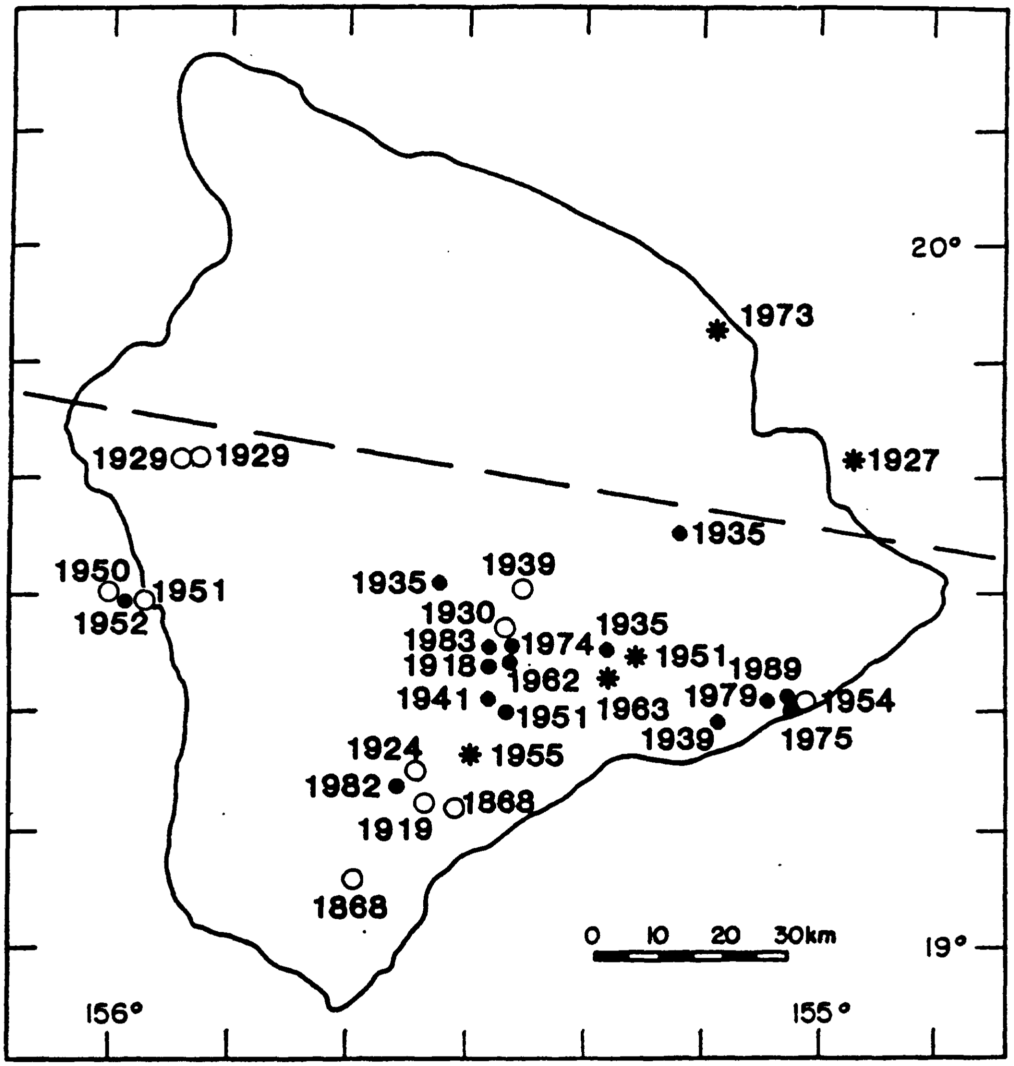

20°
1973
1927
1929
1929
1935
1950
1951
1952
1935
1939
1930
1983
1974
1935
1951
1989
1918
1962
1941
1951
1963
1979
1954
1939
1975
1924
1955
1982
1868
1919
1868
0 10 20 30km
19°
156°
155°

Fig. 4

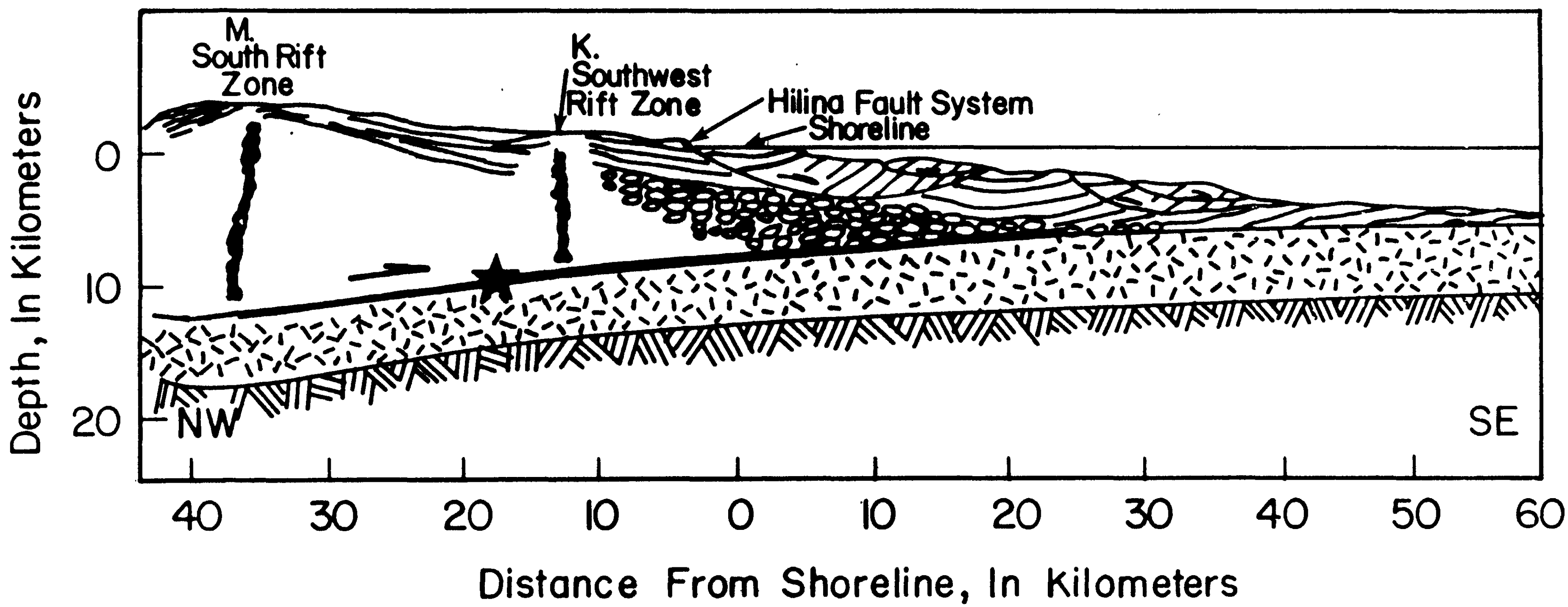

M.
South Rift
Zone
K.
Southwest
Rift Zone
Hilina Fault System
Shoreline
NW
SE
Depth, In Kilometers
0
10
20
40
30
20
10
0
10
20
30
40
50
60
Distance From Shoreline, In Kilometers

Fig. 5

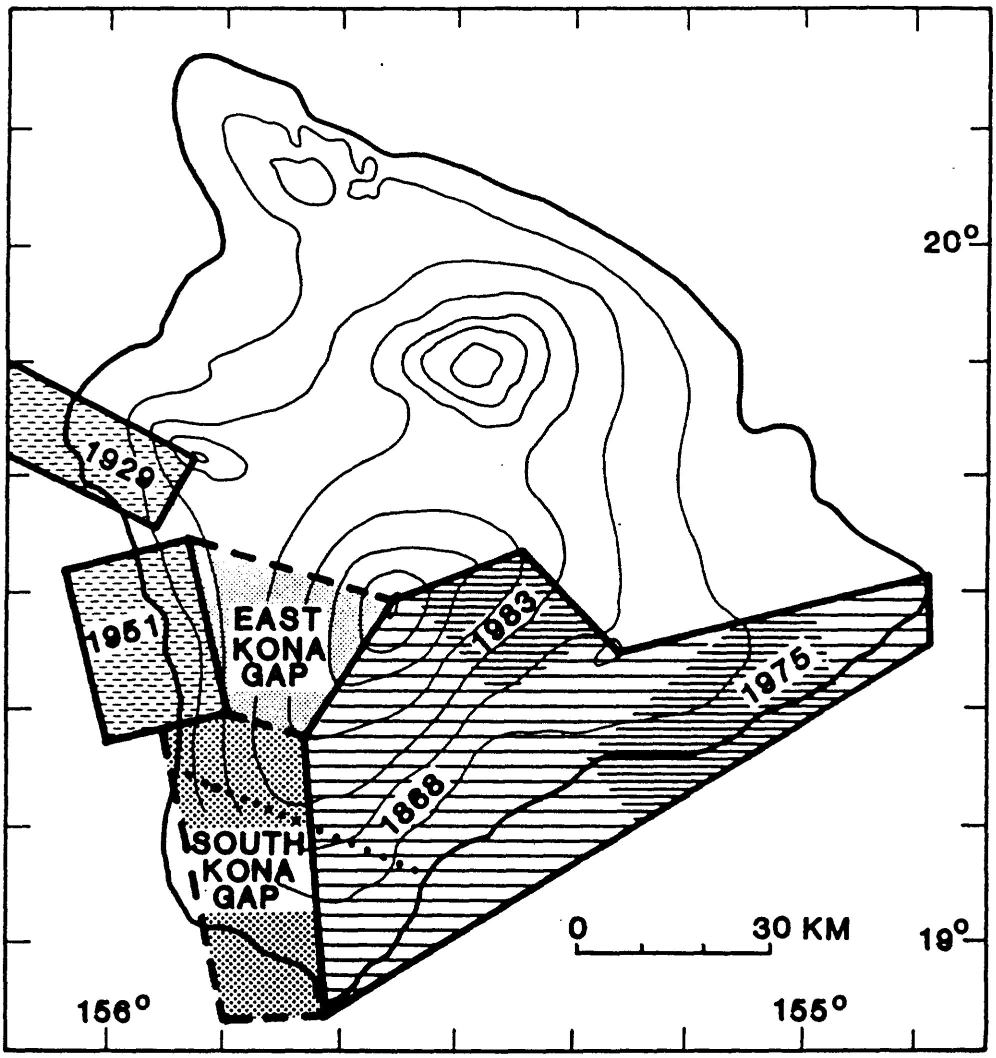
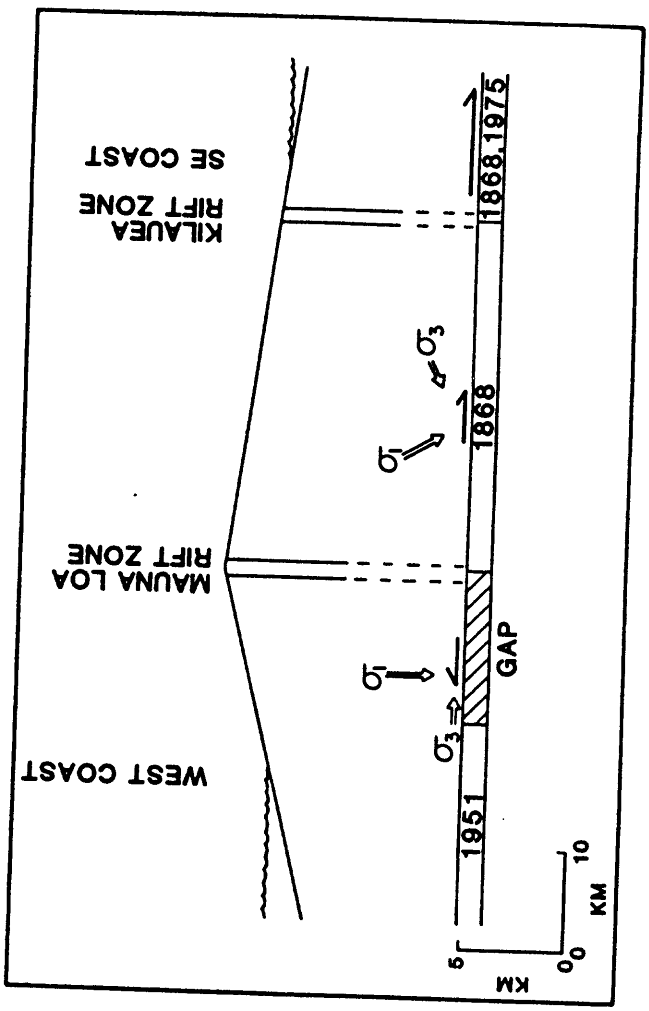

20°
1929
1951
EAST
KONA
GAP
1983
1975
1868
SOUTH
KONA
GAP
0
30 KM
19°
156°
155°

Fig. 6



SE COAST
KILAUEA
RIFT ZONE
MAUNA LOA
RIFT ZONE
WEST COAST
1868,1975
1868
GAP
1951
KM
5
0
0
10
KM


Fig. 7

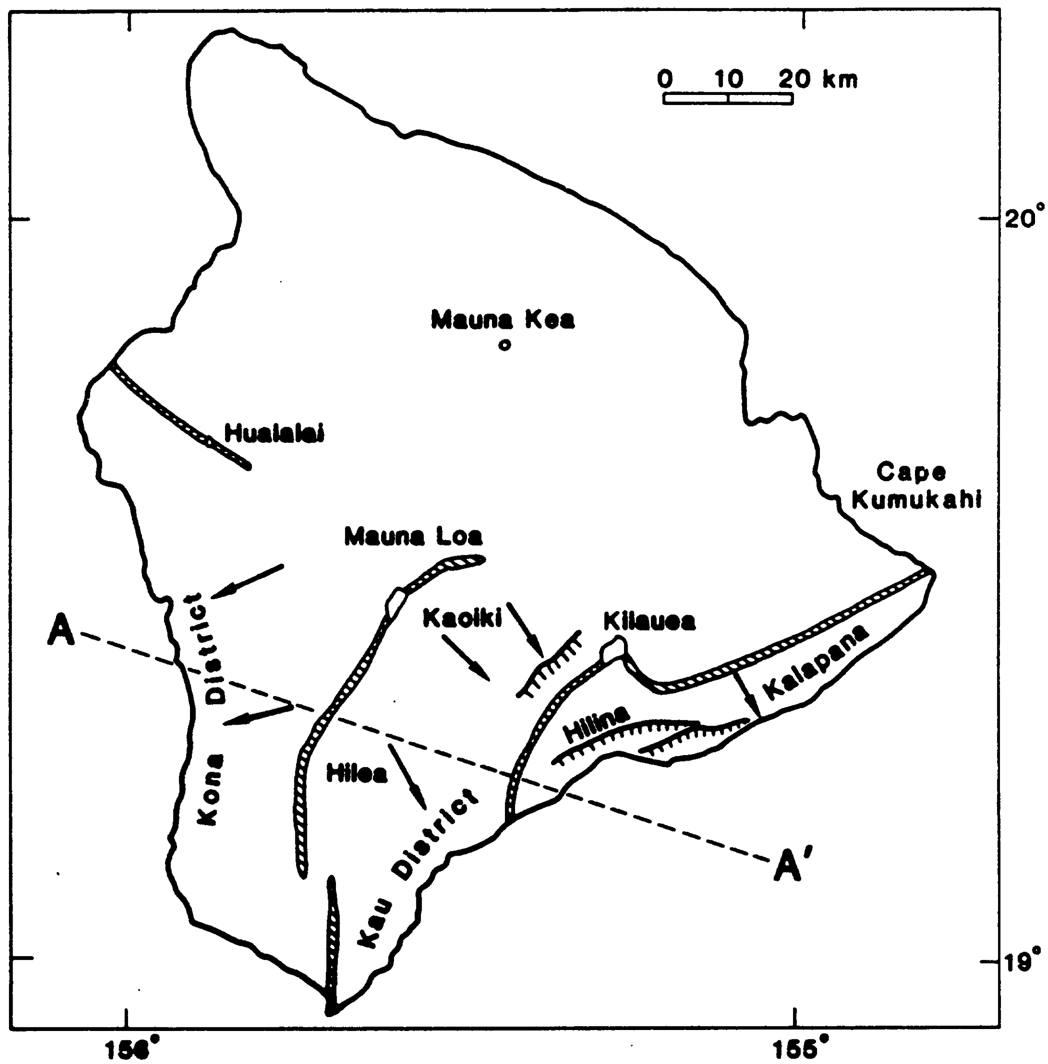

0 10 20 km
20°
Mauna Kea
Hualalai
Cape
Kumukahi
Mauna Loa
Kaoiki
Kilauea
A
A'
Kona District
Hilea
Hilina
Kalapana
Kau District
19°
156°
155°

Fig. 8

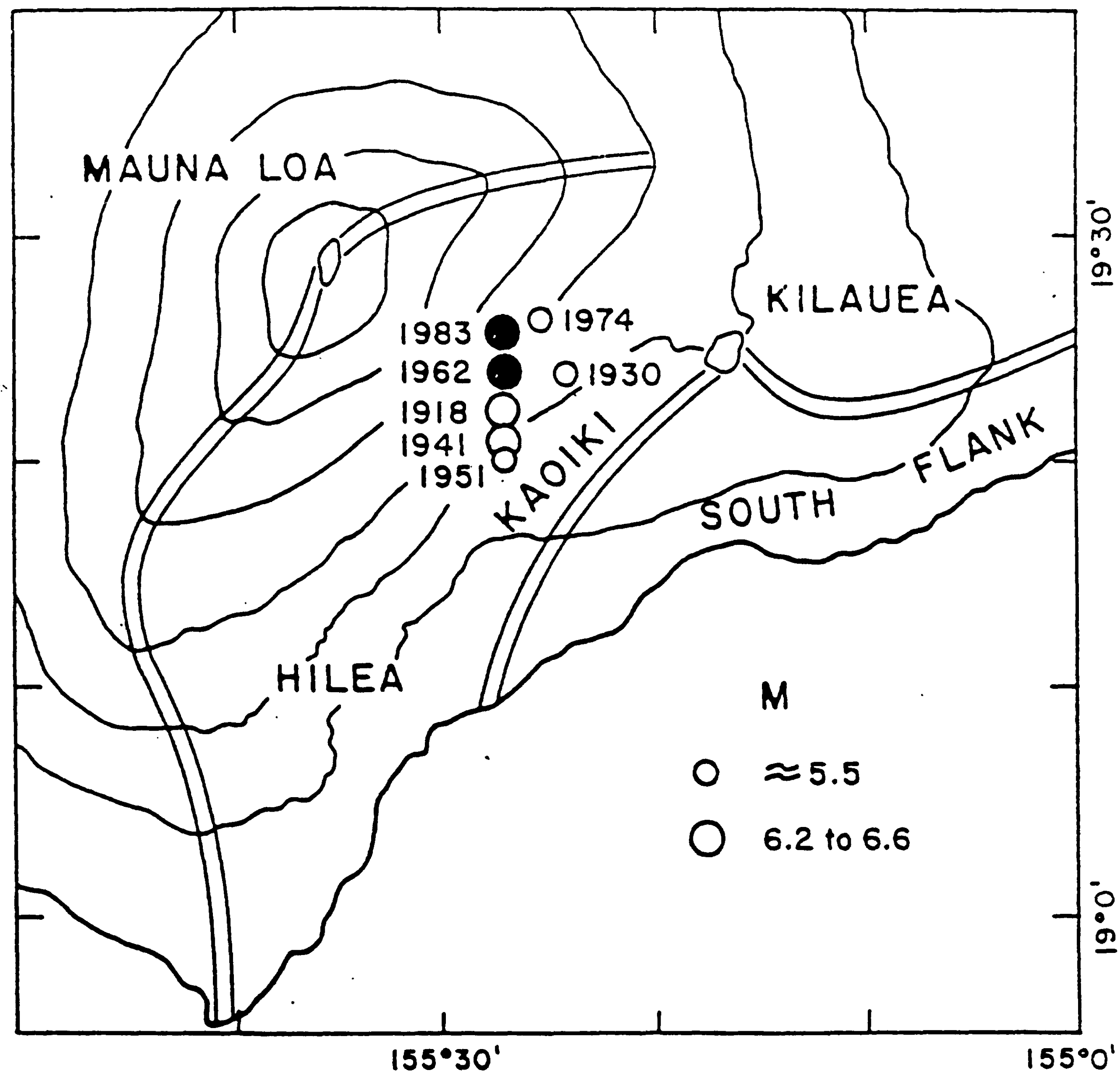

MAUNA LOA
KILAUEA
1983
1962
1918
1941
1951
1974
1930
KAOIKI
SOUTH FLANK
HILEA
M
≈5.5
6.2 to 6.6
19°30'
19°0'
155°30'
155°0'

Fig. 9

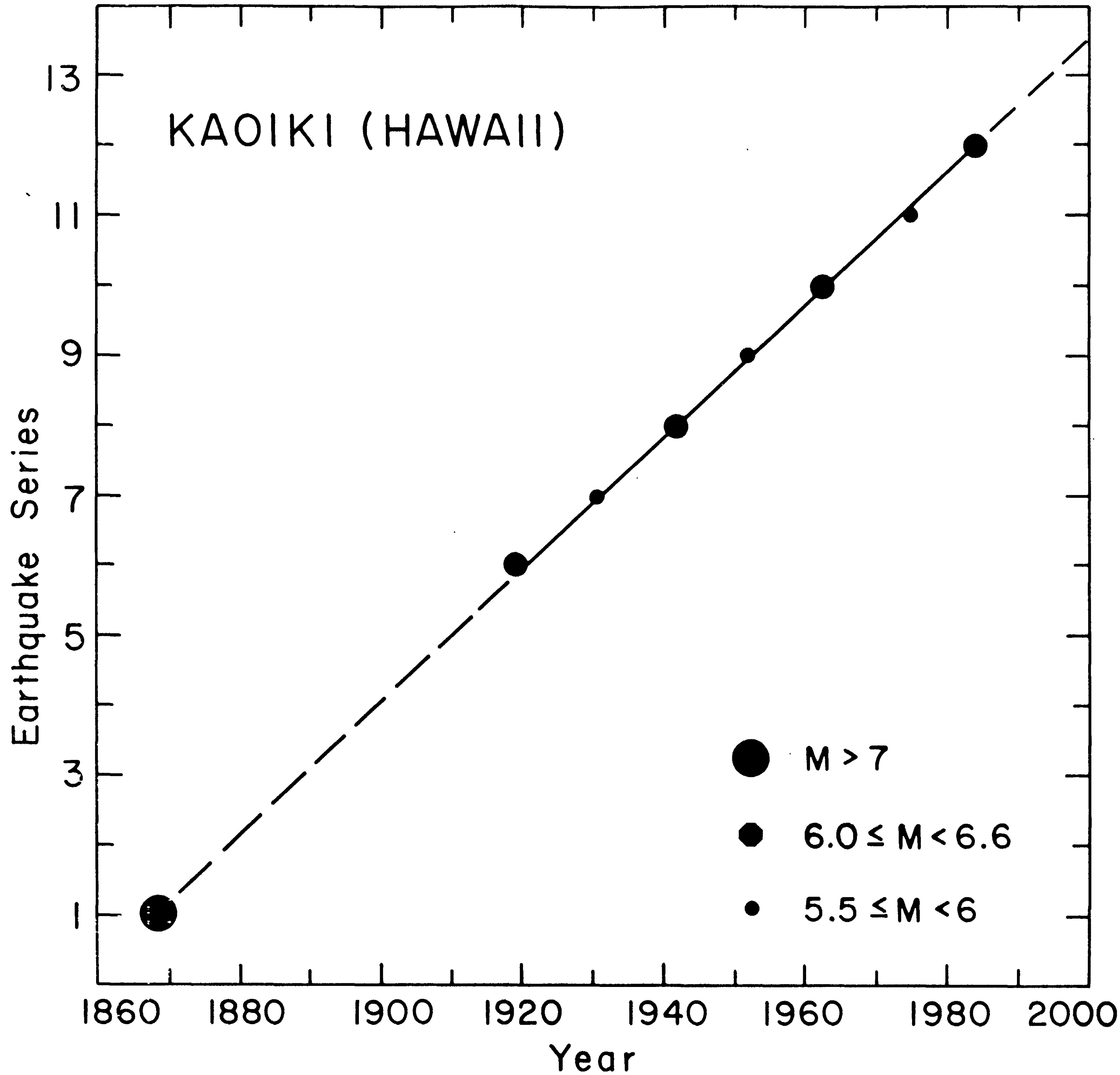

KAOIKI (HAWAII)
Earthquake Series
Year
13
11
9
7
5
3
1
1860
1880
1900
1920
1940
1960
1980
2000
M > 7
6.0 ≤ M < 6.6
5.5 ≤ M < 6

Fig. 10

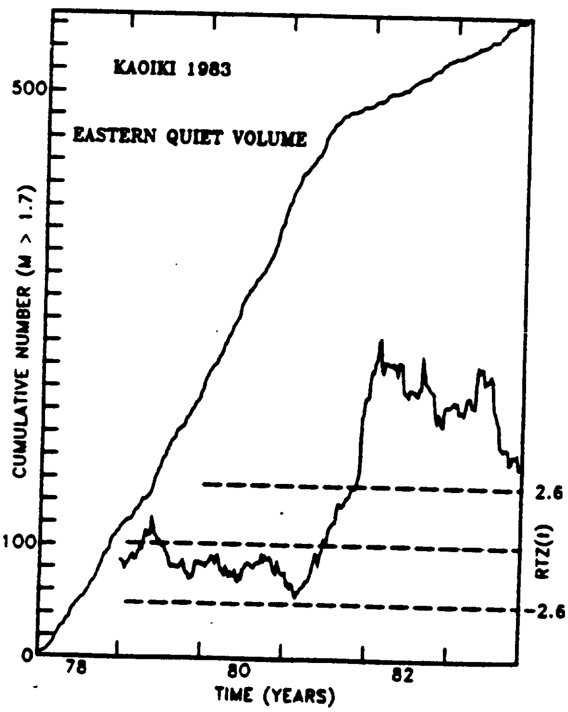

KAOIKI 1983
EASTERN QUIET VOLUME
CUMULATIVE NUMBER (M > 1.7)
500
100
0
78
80
82
TIME (YEARS)
RTZ(t)
2.6
-2.6

Fig. 11

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## Appendix Q

Document sent to NEPEC by J.Healy in May, 1993:
brief update on M8 earthquake prediction algorithm.

# TESTING THE M8 ALGORITHM

John H. Healy

## Introduction

A group of mathematicians in Moscow headed by V. I. Keilis-Borok have pursued the study of earthquake statistics for more than twenty years. In cooperation with U. S. Scientists they and their colleagues have made major advances in this field of study. We now routinely predict the probability of aftershocks and we identify foreshocks and use them as an important criteria in our attempts to predict earthquakes and volcanic eruptions. There is certainly reason to expect that continued study will contribute to a greatly improved capability to forecast and predict earthquakes.

The USGS has initiated a project to evaluate Earthquake Prediction Algorithms developed in the Soviet Union at the International Institute for Earthquake Prediction and Mathematical Geophysics in Moscow. The goal of the project is to design rigorous tests to evaluate any earthquake prediction algorithm. Important ideas about earthquake prediction are often rejected because we do not understand the proposed method or believe on other grounds that the method cannot work. Some predictions are rejected because the method is so poorly described or so dependent on the intuition of the predictor that the predictions cannot be reproduced by other investigators. If a method of prediction cannot be reproduced by other investigators it cannot be tested. Even if a predictor makes some successful predictions we have no way of evaluating future performance. Such methods are rejected because we have no scientific way to evaluate them.

The advantage of the Russian algorithms is that they are well described and presented in computer codes so that they can be reproduced and tested by other investigators. The algorithms are based on valid physical and mathematical concepts which are amenable to independent testing. In a formal test of an algorithm it is not necessary that we understand the algorithm or the underlying physical assumptions. In fact in a formal test the algorithm should be considered as a "black box" which takes data as input and produces predictions as output. When an algorithm shows promise in an unbiased test then a physical model which explains the successful performance increases our confidence in the method.

## Results

Jim Dewey, V. I. Kossobokov and Jack Healy have developed a program to test one of the Russian algorithms known as M8. The algorithm is applied to 147 circles of investigation in the Circum Pacific Seismic Zone figure 1. When the seismicity is anomalous high in a circle the algorithm declares a time of increased probability for an earthquake of magnitude 7.5 or greater. Operating in a post prediction mode, updating the prediction every six months, the algorithm "predicted" eight out of ten earthquakes in the period from January 1985 to January 1992. The earthquakes were predicted in the sense that they occurred in a time of increased probability declared by the algorithm. On the average 27.7% of the time was in this state of warning so there are many false alarms. Nevertheless, the probability of achieving these successful predictions by random guessing is less than three percent.

Encouraged by these results we set up a procedure for forward predictions and we plan to run the test in a forward mode for five years. In the updates we have completed the algorithm has predicted one earthquake and failed to predict two earthquakes, including the Landers earthquake.

## Problems

In my view the physics behind the algorithm is very simple. The algorithm is based on the theory that a strong earthquake will be preceded by a period of high seismicity in a large region on a time scale of about five years. The fact that the anomalous seismicity may occur at large distances from the predicted quake suggests that there is regional interaction between seismic zones that is not explained by generally accepted models of the earthquake process. The complicated parts of the algorithm and data preparation can be viewed as a problem of bookkeeping needed to correct and compensate for errors in earthquake catalogs.

We have tried to design a test that would avoid all possibility of cheating or self deception. Accordingly we have agreed to run the test independently at three locations Moscow, Denver, and Menlo Park. We are having some difficulty obtaining identical answers at the three sites and this problem is compounded by lack of funding for visits by our Soviet collaborators. These problems can be resolved if we can arrange for a minimal number of exchanges.

A more serious problem was discovered on close examination of the results from the forward test. We found that most of the earthquakes that were missed by the algorithm would have been predicted if the circles chosen for the test were moved to a slightly different position. This means that small adjustments of the circle positions could make the results appear either overwhelmingly significant or insignificant. We must develop a new test that automates the procedure for the circle selection. and we must develop suitable null hypotheses for this new test.

## Conclusion

The M8 algorithm is clearly detecting regions of anomalous seismicity that would not have been previously identified as either foreshocks or aftershocks. Strong earthquakes are related to these seismicity anomalies. The M8 algorithm in its present form does not provide a sufficient probability gain for use in a public warning system, but the approach appears to be promising and we believe that improved algorithms of this type will be useful in the future.