DEPARTMENT OF THE INTERIOR
U.S. GEOLOGICAL SURVEY

MINUTES OF THE NATIONAL EARTHQUAKE PREDICTION EVALUATION COUNCIL and THE SAN FRANCISCO BAY REGION SPECIAL STUDY AREAS WORKSHOP

February 26-March 1, 1986
Menlo Park, California

edited and compiled by
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Open File Report 86-630

This report is preliminary and has not been edited or reviewed for conformity with U.S. Geological Survey publication standards and stratigraphic nomenclature.

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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 U.S. Geological Survey (USGS) in issuing any formal predictions or other information pertinent to the potential for the occurrence of a significant earthquake. It is the Director of the USGS who is responsible for the decision whether and when to issue such a prediction or information.

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

The USGS routinely publishes the minutes of NEPEC meetings. The meeting was held in conjunction with the San Francisco Bay Region Special Study Areas Workshop, February 26 to March 1, 1986. This open-file report combines both the proceedings of the Council meeting and a summary of the San Francisco Bay Region workshop.

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National Earthquake Prediction Evaluation Council
Minutes of the Meeting
March 1, 1986
Menlo Park, California

Council Members Present

Dr. Lynn R. Sykes, Chairman, Lamont-Doherty Geological Observatory
Dr. John R. Filson, Vice Chairman, U.S. Geological Survey
Dr. Clement F. Shearer, Executive Secretary, U.S. Geological Survey
Dr. Keiiti Aki, University of Southern California
Dr. John N. Davies, Alaska Department of Natural Resources
Dr. James H. Dieterich, U.S. Geological Survey
Dr. William L. Ellsworth, U.S. Geological Survey
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Dr. I. Selwyn Sacks, Carnegie Institute of Washington
Dr. Wayne Thatcher, U.S. Geological Survey
Dr. Robert E. Wallace, U.S. Geological Survey
Dr. Robert L. Wesson, U.S. Geological Survey
Dr. Mark D. Zoback, Stanford University

Observers

Mr. James Goltz, California Office of Emergency Services
Dr. Allan Lindh, U.S. Geological Survey
Dr. David Schwartz, U.S. Geological Survey
Dr. William Bakun, U.S. Geological Survey
Dr. E. Roeloffs, U.S. Geological Survey

EXECUTIVE SESSION

Lynn Sykes opened the Executive Session by outlining the day's agenda. The Council began with an attempt to summarize its position regarding the Bay Area review presented in the earlier workshop. The Council also discussed the Parkfield Earthquake Prediction Scenarios and Response Plans; a proposed Red Book Conference on Intermediate-Term Earthquake Precursors; reports by three of the southern California special study area working groups (the Mojave, San Jacinto, and Indio segments of the San Andreas fault); recent earthquakes near the southern end of the Calaveras fault and assessment of the Wyss-Burford earthquake prediction; an update of recent activity in the Shumagin Islands, Alaska; the future direction of the Council; and a short summary of an Office of Science and Technology Policy briefing on the Parkfield experiment.

Discussion of Bay Area Review

Thatcher presented a brief summary of the San Francisco Bay Region Workshop held on the previous 2 days. A workshop summary and copies of papers presented at the workshop are given in the appendix to this report. Thatcher further encouraged Council members to submit their assessment and recommendations on what further studies are needed in the San Francisco Bay Region.

In light of the Council's general conclusion that the communities around the Hayward and Calaveras faults are vulnerable to significant losses from earthquakes of moderate magnitude, **Wallace** suggested that the Council should make the risk to East Bay communities clear to the public; possibly recommending that the U.S. Geological Survey send an informational letter to the Governor of California. **Wesson** recommended that instrumentation to measure strain in the area be improved and that a scientist-in-charge to coordinate the effort be named. Wesson expressed the opinion that any changes would be subtle and the value of this study would be primarily scientific. **Zoback** agreed that a denser network to assess whether strain is accumulating as rapidly in this area as it is farther to the south would be appropriate.

Dieterich suggested that the time predictable model used for some areas may not be useful for areas of multiple strand faults. **Wallace** suggested that more paleoseismological studies through trenching and close-in geodesy, at a minimum, would help with understanding the partitioning of slip among the various faults in the East Bay.

Filson requested that the Council try to classify or rank the hazard or level of concern in the areas of the State that have been discussed at the workshop (Hayward fault, Peninsular San Andreas fault, Alum Rock area), and to discuss what might be done by the Council and others. His concern is that the State officials have some sense of the relative importance of those areas that the Council has singled out for concern so that the State's limited resources can be used most effectively. **Aki** offered that a quantitative map would be difficult to make, but that perhaps a map showing the ranges of hazard based on various hypotheses, such as Sykes' proposal for the San Andreas given during the earlier workshop, could be prepared.

Zoback and **Thatcher** raised the issue of the disparity in slip between the ground surface and at depth. **Dieterich** offered two interpretations, either the entire 3 1/2 cm/yr of available slip occurs across the San Andreas fault zone and is accessible to the Hayward or the stress is transferred and taken up locally. **Zoback** offered that the way to resolve this problem is to adopt a geodetic net of Parkfield's density so that attention can be focussed on the seismogenic zone. **Kanamori** believes that the top priority is high resolution geophysical data. The present data and experiments are not suitable for M 6 earthquake analyses in the short or intermediate term as they are presently configured in too coarse of a network.

Wesson, in reference to the Peninsular San Andreas Fault, stated that there are two ways to approach the problem of discrepancy in slip rates. From a scientific point of view, we know from worldwide examples that not all the slip determined by geodetic or seismic means comes to the surface so that the hypothesis that we have 2 1/2 meters of slip at depth and none at the surface is perfectly reasonable. From the public policy point of view, though, it may be desirable to assume that we have only 1 1/2 meters of slip. **Dieterich** noted that the long-term slip rate has to be factored into the discussion. The 12 mm/yr is not incompatible with geodetic measurements. He feels that Sykes summary of different models and probabilities given at the workshop's conclusion was appropriate.

Wesson was concerned about the issue of fault interaction in the East Bay. The decrease in observed slip, whether measured geodetically or at the surface, going north into the Peninsular San Andreas fault may be related to bifurcation and transfer of slip. **Sykes** observed that a slip rate of 8 mm/yr for the San Andreas fault on the San Francisco Peninsula, mentioned in several places during the workshop, is based on a single unpublished geological estimate and therefore is more difficult to assess. He believes that the rate of 12 mm/yr obtained by T. Hall is more firmly established. Attempts to use rates of 8 to 12 mm/yr are weighted on poor evidence. Some members believe that a slip rate of 12 mm/yr is a minimum rate since Hall's measurement was obtained near an en echelon offset of the San Andreas fault. However, there is still some disagreement regarding slip rates, some arguing for 12 mm/yr and others arguing for even higher rates. **Dieterich** has doubts about how much weight should be placed on any of those figures because of the nearness of the faults. He contends that at the stress rates at one strand does not necessarily reflect the stress rate and probability of something happening in that stress field.

Ellsworth spoke regarding Wesson's public policy concerns. Although there are lots of questions about the detail of how slip occurred in 1906, how much strain has accumulated, and what the long-term slip rate is, it is fairly clear that moving along the 1906 break one goes from an area of high hazard in the south end to one of low hazard at the northern end (Golden Gate). But there are some questions about the details on the location of this transition. There is a clear potential for an earthquake of M 6 to M 7 within the zone; we can't specify its probability, but, given what we know about the East Bay, we can assess the hazard as being roughly equal. Further, we can't resolve this question with the available data and it is going to require new work on the geology, strain accumulation since 1906, and probably hard modelling of the data.

Sykes offered a table of various scenarios for that section of the San Andreas fault from mid-Peninsula (Black Mountain to San Juan Bautista), assuming different slip rates and characteristic slip.

San Andreas Fault
Black Mountain to San Juan Bautista

Repeat Time	Characteristic Slip	Probability, 1985-2005
68 yrs.(1906-1838)		60 %
93 yrs. (1.4 m/1.5 cm/yr.)	1.4 m	30 %
117 yrs. (1.4 m/1.2 cm/yr.)	1.4 m	20 %
208 yrs. (2.5 m/1.2 cm/yr.)	2.5 m	10 %
100 yrs. (2.0 m/2.0 cm/yr.)	2.0 m	30 %

The Council adopted a statement on Peninsular San Andreas earthquake hazards. It is cited in the appendix to this report. In summary, though, the Council believes that the earthquake hazard is high in the Bay region; its highest concern is for M 6 to M 7 events for the Hayward fault, the southern end of the 1906 break, and the Alum Rock gap; M 6+ events could occur on other faults; and based on current data, the Council isn't prepared to quantify a ranking for these areas of concern, and substantially more work needs to be done before detailed probabilistic estimates can be made.

Parkfield Prediction Scenarios

William Bakun introduced the latest draft of the "Parkfield Earthquake Prediction Scenarios and Response Plan." The decision matrix for a quick response, possibly with an earthquake prediction, to changing conditions at Parkfield, California, was developed at the Council's request and to meet a State of California requirement, for a matching fund agreement for instrumentation, that the USGS make a serious attempt to predict the next Parkfield earthquake. This latest draft attempts to address these issues. Because of the evolving state-of-the-art in earthquake prediction and limited understanding of earthquake processes, the paper represents the continuing development of an operational project and lacks much of the scientific rigor and documentation of many scientific articles.

The Council discussed the different levels of alert, in particular at what level the probability is sufficient to recommend action by the State of California. The California Office of Emergency Services observer, James Goltz, forwarded his agency's recommendation that his office be informed at every change in alarm level because notification at only the highest levels would leave just 24 hours for the State to take action, and 24 hours may not be adequate for a proper response.

The Council also discussed the definition of the response criteria, how to include the results of additional instrumentation, and how the different types of instrumentation and criteria--dilatometers, creepmeters, seismology, etc.--combine to produce the different response levels. **Wesson** suggested that the document be adopted as a provisional plan that would expire and require update in about 6 months. He further suggested that the USGS try to evolve the threshold criteria for instruments not yet included in the combination rules. The Council then adopted this recommendation.

Office of Science and Technology Policy Briefing

Ellsworth described a briefing on "A Proposed Initiative for Capitalizing on the Parkfield, California, Earthquake Prediction" (1986) given to the Office of Science and Technology Policy (OSTP). The briefing, held at the National Academy of Sciences on February 28, 1986, was at the special invitation of the Acting Director of OSTP. The Parkfield issue was of particular interest to the President's Science Advisor because of potential participation in the project by the Peoples Republic of China. Clarence Allen gave the briefing, which explained what the situation is at Parkfield, why there is an important opportunity that might be lost, how we might succeed in predicting the earthquake but not understand why, and that there is an opportunity to try new experiments. On the latter, he particulary noted deep drilling experiments into or adjacent to the fault zone in order to make measurements of material properties and to track the evolution of the conditions at depth leading to an earthquake. A proposed initiative on this issue is appended to this report.

Intermediate-Term Precursors Conference

Aki described the status of three classes of earthquake prediction. Long-term prediction is being addressed by seismicity and paleoseismicity studies, short-term prediction by the Parkfield Experiment, but intermediate-term prediction is receiving less attention. He proposes to organize a conference entitled "Observational and Physical Basis for Intermediate-Term Earthquake Prediction" to review the state-of-the-art and suggest future directions. Tentative subjects include: physical models of the fault zone; laboratory evidence for precursors; seismicity patterns; mechanical properties of the crust (elasticity, anelasticity, heterogeneities); crustal deformation; hydrogeology and geochemistry. Names of potential reviewers for these subjects were given by Aki. The very earliest the conference could be held is in 6 months.

Southern California Special Study Areas

A year ago at a workshop in San Diego scientists tried to identify some areas likely to have moderate or large earthquakes over the next several years in southern California and that would be amenable to further instrumentation and earthquake prediction studies.

Three areas were identified: the Mojave segment of the San Andreas fault, the San Jacinto fault, and the Indio segment of the San Andreas fault. Each segment was the topic of a further workshop to summarize knowledge of the segment and its earthquake potential and to begin to identify what types of investigations or instrumentation are required in order to initiate focused study such as that of the Parkfield Experiment.

Indio Segment

Rob Wesson and **Clarence Allen** cochaired the workshop on the Indio segment. This segment is the least understood of the three segments and so the findings are likely to be more general than those of the other workshops. There has been no great historic earthquake along this segment of the San Andreas fault and no historic earthquake that has broken the ground, as has been the case for the other two segments. Further, there are relatively few small earthquakes along this section. Many earthquakes have occurred at the northern end at the San Gorgonio knot, but they don't seem to be directly associated with the San Andreas fault itself. The fault is creeping along part of this section. A section of the Coachella Valley canal has been offset about 7 cm since the early 1950's. We don't know how to bound this segment of the fault. At the northern end there is a complicated tectonic knot in San Gorgonio pass, but along the main part of the fault, from Indio south, there is a fairly straight single fault segment. Whether an earthquake could actually break through the San Gorgonio knot is not known. At the southern end of the segment is the Brawley seismic zone. The group identified some needed investigations: continuation of neotectonic framework studies, paleoseismicity studies, improvement in the geodetic network--particularly with intermediate line lengths and additional creepmeters. Attempts will be made to have a symposium on this topic at the December AGU meeting and for the working group to continue to meet. A report on the workshop is included in the appendix to this open-file report.

San Jacinto Fault

The San Jacinto group is chaired by **Hiroo Kanamori** and **Jim Brune.** They held a workshop in early October 1985 to discuss the seismic potential of the San Jacinto fault zone and to frame recommendations for siting intensified earthquake prediction monitoring experiments. The entire segment of the southern San Jacinto fault ruptured between 1942 and 1969. The northern half of the fault should be given higher priority based on both the potential impact and elapsed time since the last earthquake. The most critical short-term recommendations are: improvement of slip rate estimates and most recent event characterization by paleoseismic studies in the Anza Gap, and development of methodologies for using digital seismic data in real time monitoring. Recommended long-term projects are: investigations of the spacial variation of slip rates; upgrading of the regional seismic network; expansion of the geodetic network to about 5 km in resolution; crustal studies including heatflow determinations; installation of strong motion instruments; and special studies in the Cahuilla swarm area.

Mojave Segment

K. Aki and **D. Schwartz** chaired a September 1985 workshop on new earthquake prediction research on the Mojave segment of the San Andreas fault from Tejon Pass to Cajon Pass. In order to develop specific recommendations the workshop was divided into three sections: geology, short-baseline borehole measurement and crustal structure, and network seismology and geodetics.

The participants agreed that to best understand the Mojave segment, the fault section from Cajon Pass to San Gorgonio Pass should be included as part of the Mojave segment. The group also endorsed investigations associated with the Cajon Pass drillhole. Further, the workshop identified four areas as potential nucleation sites for rupture in a large shock and, therefore, sites for further investigations. These areas are: the Tejon Pass area, a structurally complex zone that includes the big bend of the San Andreas fault and the intersection of the Garlock, White Wolf-Pleito, and Big Pine fault zones; Lake Hughes, the location of the proposed change in the amount of slip during the 1857 earthquake; the Cajon Pass area, a structurally complex zone representing the intersection of the San Andreas, San Jacinto, and Cucamonga Fault zones; and San Gorgonio Pass, a complex zone of step-overs and splay faults. The group strongly recommended improvements in monitoring, additional tectonic framework and paleoseismic studies, as well as some new efforts in dendochronology.

Recent Central California Earthquakes in the Wyss-Burford Predictions

Bob Wallace gave a brief description of recent earthquakes in central California in light of the Wyss-Burford prediction of an earthquake on the San Andreas fault near San Juan Bautista (see Minutes of the National Earthquake Prediction Evaluation Council, July 26-27, 1985, USGS Open-File Report 85-754). Essentially, these recent earthquakes don't satisfy the Wyss-Burford prediction. One earthquake occurred on January 26, 1986, probably on, perhaps, the Bradley fault and measured M_L 5.5. The other occurred on January 14, 1986, and was measured at M_L 4.5.

UPDATE: Burford reports that an earthquake of M 4.8 occurred May 31, 1986, within polygon 386 defined by Wyss and Burford. NEPEC has not reviewed the data.

Update on Recent Earthquake Activity in the Shumagin Islands

Davies described a series of earthquakes in the Shumagin Gap. Five earthquakes occurred from October 9 to November 15, 1985, all above M 5.0. The series can be interpreted as two independent main shock-after shock sequences. The State of Alaska, in consultation with the U.S. Geological Survey and National Earthquake Prediction Evaluation Council representatives, decided to monitor the events through its own geological survey. The Office of the Governor and the Department of Emergency Services of the State of Alaska were alerted. The Governor issued a press release and directive to the Department of Emergency Services to contact each of the affected communities and hold workshops if possible. This was done. The area has been seismically quiet since the November M 5.6 event.

Future Council Activities

Sykes observed that the Council has completed its review of the top priority geographic areas, which were determined at its original meeting in November of 1984. Now the Council should determine where to go, what type of meetings it should conduct, etc. **Filson** believes the workshops and discussions fostered by the Council and the USGS are very helpful and should continue. Further, the Council's work over the past 18 months has been admirable and should be reviewed, assessed, and summarized to help guide the group for its future work. **Dieterich, Thatcher,** and others believe an assessment and recommendations should be detailed in a single document.

Filson raised the issue of whether the Council should look at some other areas of the country. **Wallace** said that looking at the prediction of eastern U.S. earthquakes might lend a new perspective to earthquake-prediction studies. **Sykes** believes that the eastern United States may be an appropriate area of investigation but earthquake prediction work in that area should not be placed on the Council's agenda for a few years. **Wesson** offered that the Utah and Nevada intermountain area may be reasonable and appropriate for Council consideration. Also, other than the global issues, the Council could look at the methodology of probabilistic estimates and approaches to decision trees. And, the Council could take the southern California working group through an exercise in how to respond to anomalous behavior. **Dieterich** suggested meetings to get a better evaluation of methodologies, such as quiescence, or meetings on data within the USGS and how they are being handled, e.g., uniformity of catalogs and data availability. **Ellsworth** thinks the Cascadia subduction zone off Washington State should be a high priority for Council deliberation. **Sykes** and **McEvilly** included other faults - specifically, the Garlock, Newport-Inglewood faults. **Zoback** is concerned regarding the EPRI (Electric Power Research Institute) study in the East and the Midwest and the methodology EPRI is using. He thinks the USGS and subsequently the Council may be asked to review EPRI's work. The Council decided to review the Cascadia subduction zone at one of its next meetings.

San Francisco Bay Special Study Areas Workshop
February 27-28, 1986

INTRODUCTION

The San Francisco Bay Region Special Study Areas Workshop is one of several workshops convened by the U.S. Geological Survey (USGS) to consider the requirements and potential locations for detailed earthquake prediction studies. These workshops are one result of an unpublished paper, "Option Paper for Earthquake Prediction Strategy," prepared by the USGS. An earlier workshop was held in San Diego, California, in February and March 1985, similarly to consider special study areas in southern California.

Three overview presentations were given during the morning of the first day - "An Overview of Geodetic Deformation" by Will Prescott," An Overview of Late Quaternary/Holocene Fault Activity" by Darrell Herd and Tim Hall, and "An Overview of Seismicity/Historic Earthquakes" by Allan Lindh. The remainder of the first day's session was devoted to discussions of the Hayward and Calaveras Fault Systems. The second day was devoted to presentations and discussions on the Peninsular San Andreas Fault and a summary session for the entire workshop.

Overview of Geodetic Deformation

In his presentation, Will Prescott summarized some results of repeated geodetic measurements in the San Francisco Bay Region. Most of the observations were made annually or less often with a single color geodolite, using aircraft to measure refractivity so that strain could be determined with a precision of $2x10^{-7}$. Additional measurements were made by triangulation and creepmeters. Total slip is determined to be about 33 mm/yr - 10 mm/yr on the San Andreas fault, 9 mm/yr on the Hayward, and 6 mm/yr on the Calaveras with the remaining 8 or 9 mm/yr occurring east of the Calaveras fault. The mechanism operating on these faults varies; with distributive strain near the San Andreas fault; creep on the Hayward fault; block rotation east of the Calaveras fault; and with an undetermined mechanism on the Calaveras fault. Prescott discussed results from the regional network starting with the southern part of the San Andreas fault and ending with the northern part of the Hayward fault. His discussion and illustrations are reprinted in the appendix of this report.

Overview of Late Quaternary/Holocene Fault Activity

Darrell Herd and Tim Hall gave an overview of Late Quaternary and Holocene fault activity in the San Francisco Bay Region. Based largely on an unpublished map compiled by himself and others, Herd described in some detail the location and character of faults in the East Bay area. His presentation included an assessment of slip rates for some of these faults. However, he cautioned that some of his values are old and are likely outdated. There are no measurements of actual offset of any East Bay area fault, neither are there any good geologic constraints on most of the faults in the area. Tim Hall's presentation was on the western part of the San Francisco Bay area, particularly the San Andreas fault from Seal Cove to the San Gregorio fault. He noted that very little is known about the Neogene slip rate of the San Andreas fault in that area or for the San Gregorio fault. A long-term slip rate is available for the central San Andreas fault near the San Andreas Reservoir, while the geodetic rate is about 12 mm/yr. Otherwise data on Holocene slip rates along the San Andreas fault are sparse or non-existent. Bob Wallace reviewed some published dendochronologic data from along the northern San Andreas fault.

Overview of Seismicity/Historic Earthquakes

The overview session was concluded by Allan Lindh's discussion of seismicity and historic earthquakes in the San Francisco Bay Region. Lindh and J. Olsen divided the Bay area faults into characteristic segments based on historic seismicity, slip variations along strike of the 1906 earthquake, and discontinuities along strike and microseismicity and fault structure. The historic seismicity of the Bay Area is dominated by the M 8 San Francisco earthquake and its seismic cycle. Microseismicity is dominated by that occurring along the three main branches of the area's plate boundary - the San Andreas, the Calaveras, and the Hayward faults. Lindh attempted to identify those segments most likely to fail in the next 30 years, assigned slip rates to the major fault branches, and listed conditional probabilities of failure for these segments for periods of 1, 10, 20, and 30 years from the present.

OPEN SESSIONS

Hayward/Calaveras Fault System

During a general discussion, the participants tried to reach agreement on which sections of the fault system most deserved attention. The following types of investigations were associated with three sectors of the East Bay fault system.

Hayward	Alum Rock Gap	Other Parts of Calaveras Fault
geodetic lines	geodetic lines	paleoseismicity
creepmeters	creepmeters	Tertiary-Quaternary Geology
paleoseismicity	two-color laser	
two-color laser	dilatometers	
Quaternary geology		

There was significant and lively discussion of both the importance and lack of good, current geologic data on long-term slip rates for the Hayward and Calaveras faults. Several participants noted the absence of a person responsible for overseeing a coordinated study of the San Francisco Bay regional geology.

Peninsular San Francisco Bay Discussion

Several of the attendees led informal discussions of relevant investigations of the Peninsular San Francisco Bay Region. **Wayne Thatcher** described work on the geodetic network and attempts to determine slip on the San Andreas fault from Page Mill Road to San Juan Bautista. He concluded that he can't significantly decrease the geodetically determined slip below about 2.5 m due to individual variation in slip from segment to segment. He further discussed a discrepancy between surface slip and geodetically determined slip associated with the 1906 earthquake and concluded that there had to be more slip at depth than there was at the surface. **Roger Bilham** agreed with Thatcher's point on slip discrepancy. He divided the 1906 rupture into 33 straight line segments and discovered a puzzling relationship between fault strike and amount of slip. The relationship implies that somehow the fault strike is controlling the amount of slip at a given place. Bilham sees his analysis as possible evidence for deep slip that isn't reflected at the ground surface. **Carol Prentice** discussed her work north of the San Andreas-San Gregorio fault junction to estimate San Andreas fault slip rates. The work has recently begun, but she is optimistic that three areas will yield good results - one of these areas is near Ft. Ross and the other two areas are near Point Arena. She will be investigating offset landslides at the Ft. Ross site and a terrace offset at one of the Point Arena sites, and will do trenching at the third site. **Ken Lajoie** summarized work on the San Gregorio fault. He and his colleague tried to determine slip rates from analyses of marine terrace. Although study of the marine terraces yielded a lot of data on fault character, it isn't at all conclusive on slip rates or actual recurrence intervals. **Tim Hall** presented a reinterpretation of G. K. Gilbert's notes and photographs of the 1906 San Andreas fault rupture zone in Marin County. The purpose of the study was to find out exactly where faulting in 1906 occurred.

Workshop Discussion

The general workshop discussion acknowledged that more work needs to be done in the East Bay area but wasn't able to formulate a very focussed consensus on what should be done immediately with current limited resources and what should be part of an expanded program. Many participants cited better understanding of regional geology as a high priority for immediate work and increased geodetic control as a prime long-term goal. There was also general agreement on the need for better determinations of slip rates and slip in past earthquakes and better understanding of the relationship between slip at depth and slip at the ground surface. Several of the participants stressed a need to construct models that explain the distribution of stress among several fault strands. Participants did agree that there is a significant risk in the East Bay from moderate earthquakes (and, of course, from larger shocks). Some of the participants have agreed to meet informally as a working group to pursue some of these issues.

HOLOCENE SLIP RATES ON THE SAN ANDREAS FAULT IN NORTHERN CALIFORNIA

by N. Timothy Hall[1]

Introduction

During the past decade, geologists have begun to piece together the late Holocene paleoseismic history of the San Andreas fault and several of its branches. In particular significant data are now available for the south-central segment of the San Andreas fault at Pallett Creek near Palmdale (Sieh, 1978) and at Wallace Creek (Sieh and Jahns, 1984). Fewer data, however, are available for the northern segment of the fault which experienced well-documented surface rupture during the great San Francisco earthquake of 1906 (Lawson, 1908). In this paper I will discuss two sites, one in Marin County and one on the San Francisco Peninsula, which have produced some preliminary information on the neotectonic behavior of the northern San Andreas fault.

Dogtown Research Site, Marin County

Cotton et al (1982) performed a reconnaissance study of the northern locked segment of the fault from San Juan Bautista on the south to Point Arena on the Mendocino County coast on the north, a distance of about 300 kilometers, and identified a site for paleoseismic investigations in Marin County 30 kilometers northwest of San Francisco near the small community of Dogtown (Figure 1). The Dogtown research site is located within the boundaries of Point Reyes National Seashore on a reach of the San Andreas fault where slip in 1906 occurred on a single trace. The location of the 1906 trace is well constrained at Dogtown between offset fences at the Strain Ranch on the north and a row of offset eucalyptus trees on the south (Figure 2). These displaced features were well documented by G. K. Gilbert after the earthquake (Lawson, 1908). A low scarp along the west edge of a sag pond and an offset channel bank of Pine Gulch Creek just north of the site also serve to mark the 1906 rupture trace at Dogtown.

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In 1979 and 1981, fifteen backhoe trenches were excavated at the Dogtown site. Eleven crossed the active trace and four were parallel to the fault (Figure 3). The trenches exposed a heterogeneous and laterally variable sequence of unconsolidated to semi-consolidated fluvial and lacustrine sediments of late Holocene age. Channel gravels and overbank silts from Pine Gulch Creek and its tributaries were found to interfinger with fine-grained marsh and sag pond deposits creating complex facies changes throughout the area.

Attempts to obtain geologic slip rates, recognize pre-1906 faulting events and calculate recurrence intervals from geologic evidence preserved in these Holocene sediments at Dogtown met with varying degrees of success (or frustration) as outlined below:

1. Slip rate. Two trenches were excavated parallel to the fault, one on each side, in order to search for displaced piercing points (Figure 3). Unfortunately, former channels of Pine Gulch Creek tend to follow the active fault trace, not cut across it. Consequently, owing both to changes in lithology caused primarily by fluvial scour and fill, and to the lack of linear features transverse to the fault, no displaced features could be matched across it. To my knowledge, there are at present no well documented Holocene slip rates for the San Andreas fault north of San Francisco.

2. Pre-1906 events. At Dogtown the Holocene sediments have not liquefied during strong seismic shaking, so the best method of recognizing individual ground rupturing events in the subsurface was by identifying local unconformably truncated fault splays that developed by post-earthquake sedimentation across a mole track. Discrete tectonic events leave a decipherable record when succeeding ruptures occur along slightly different splays so that one slip event is not overprinted on another (Figure 4). Unfortunately, unconformably truncated fault splays cannot be used to place upper limits on either the amount of horizontal displacement per event or on earthquake magnitude. All that can be said is that each fault splay is probably caused by a seismic event in the $M_L = 5.5$. to 6.0 range or greater, large enough to produce ground rupture.

Based upon trenches opened in 1979, Hay et al (1981) initially (and probably optimistically) reported three pre-1906 ground rupture events for the Dogtown site

(Figure 5). Rupture splays for one pre-1906 earthquake (Event 3) were found in three different trenches, while the evidence for both Event 1 and Event 2 was less convincing. During the 1981 investigation additional evidence for the existence of Event 3 was found, but Events 1 and 2 could not be reconfirmed. At least two explanations are possible for this lack of confirmation. First, the 1979 logs depict Events 1 and 2 as fault splays whose upward truncations occur against the base of a gravel layer. A splay from the 1906 event might have propagated into these coarse-grained beds and been "absorbed" by intergranular rotation, giving the false impression that a pre-1906 fault splay was truncated by the base of the gravel. Such a misinterpretation leads to an overestimation of the number of slip events that had occurred. Alternatively, there is the possibility that pre-1906 slip events might go unrecognized. Due to the branching and braided pattern of fault splays within the San Andreas fault zone, individual splays that locally diverge from the main rupture surface have a finite length. It is therefore unlikely that a limited number of trench wall exposures will reveal all slip events or that the evidence for a given slip event will necessarily be seen in several trenches, particularly if displacement on the splay has been small.

3. Recurrence intervals. Errors in recurrence interval calculations can occur in several ways. First, as discussed above, the number of slip events may be improperly estimated. Second, radiocarbon dates on detrital wood and charcoal might not accurately reflect the age of the layer in which they are found. On one hand, some dates might be too young owing to sample contamination by rootlets, or by humic acids introduced by post-depositional gravitional water and ground water flucuations. This source of error can be greatly reduced by standard pretreatment procedures used by most radiocarbon labs. On the other hand, it is more likely that ages computed from detrital charcoal and wood are greater than the age of the enclosing sediments, particularly within a drainage basin like Pine Gulch Creek which contains redwood trees a thousand or more years old.

Two types of age discrepancies are present within the radiocarbon dates currently available for Dogtown (Hall, et al, 1984). First, multiple dates for a given horizon typically show a large spread of values. For example, samples of detrital charcoal within the Upper Red Gravel (fluvial channel deposits) ranged in age from 490 ± 150 to 2130 ± 90 years B.P. (Table 1). A more puzzling discrepancy exists within the dates for the Dark Gray Clay (marsh/sag pond deposits). Detrital charcoal gave an age of 1690 ± 210 years B.P. for this layer, while five radiocarbon dates on detrital wood from the same horizon ranged from 195 ± 75 to 260 ± 90 years B.P. (Table 1). I know of no reason why dates on detrital wood should yield systematically younger dates than detrital charcoal. Before credible recurrence frequency intervals can be estimated for the northern segment of the San Andreas fault from the prehistoric record at Dogtown, ambiguities in both the number of prehistoric slip events recorded here and in the ages of the layers that bracket these events need to be resolved.

San Andreas Dam, San Mateo County

In contrast to the Dogtown trenches which, so far, have yielded mostly equivocal data on the Holocene history of the northern San Andreas fault, the reach of the fault between San Andreas Lake and Crystal Springs Reservoir on the San Francisco Peninsula (Figure 6) contains a site from which I was able to estimate a late Holocene slip rate (Hall, 1984). This site consists of alluvium that has ponded behind a shutter ridge which developed between a left-stepping en echelon break in the fault trace (Figure 7). Deposition of alluvium ceased here approximately 1130 ± 160 radiocarbon years ago when the source channel became deeply incised into the alluvial surface.

Whether a slip event removed the shutter ridge from the path of the stream (scenario 1) or a flood added a large volume of sediment to the surface of the impounded alluvial fan (scenario 2), the ridge was ultimately overtopped by alluvium southeast of the road (Figure 7). This enabled the stream to flow across the fault and connect with the axial stream in the rift valley with a much shorter and steeper channel than it had previously. This stream formerly flowed in a northwesterly direction around the shutter ridge and carved what is now the abandoned channel spanned by the offset pipeline. After it overtopped the ridge, the stream incised itself into its own alluvial deposits and carved a minature water gap into the Franciscan bedrock. Within the last 1130 ± 160 radiocarbon years B.P., this channel has been displaced 44 feet by slip along the active trace of the San Andreas fault. If 1906 with its approximately 7 to 9 feet of right typical slip was a slip event for this reach of the San Andreas, then approximately five such events have occurred since the stream established its new channel across the fault.

In order to compute a slip rate from these data, several corrections and assumptions must be made. First, the youngest date available from near the top of the ponded alluvium 1130 ± 160 yr B.P., must be converted to calendar years using the curves of Stuvier (1982). Second, it must also be recognized that, because detrital charcoal is always older than the layer containing it, because the charcoal was collected at least 6 inches below the surface, and because some time may have elapsed between last deposition and offset, the calculated strain rate will be a minimum value and average recurrence interval estimates will be maximum values.

The 44 feet of measured right lateral displacement on the stream channel is also not without some inherent uncertainty. In addition to faulting, stream channels in this area may also be partly deflected or modified by other causes or combinations of causes, including slumping of the banks, vegetation growth (including fallen trees), and the fortuitous encountering of resistant blocks or "knockers" within the Franciscan melange. While these nontectonic modifiers of channel geometry probably operate randomly, a systematic under-estimation of the characteristic amount of slip for this reach of the fault might occur at this site because the deflected channel is near the end of a fault strand within a few hundred feet of the left step over between the en echelon traces. For this reason, an estimate of slip rate based upon this deflected channel might also be a minimum value.

Estimating a recurrence frequency requires an additional assumption: that the 1906 event, with its 7 to 9 feet of right slip, is typical for this reach of the fault. Until individual pre-1906 earthquakes can be recognized in the stratigraphic record of the San Francisco Peninsula, the assumption that the 1906 event is characteristic remains highly speculative. In June 1838, an earthquake with an intensity comparable to 1906 struck the San Francisco Bay area and produced ground rupture along the San Andreas fault in the form of a fissure which local residents reported as stretching from near San Francisco southward to Mission Santa Clara (Louderback, 1947). Louderback suggested that the greater intensity reported at Monterey in 1838 than during the 1906 earthquake might indicate that fault rupture extended further southeast in 1838 than it did in 1906. Unfortunately, the amount of displacement that occurred on the San Andreas fault in 1838 is not known. Consequently, the following recurrence calculations, which assume the 1906 event is characteristic, yield time intervals which are clearly maximums.

The two scenarios mentioned above can be considered end members of the likely geologic histories that led to the cessation of deposition of the ponded alluvium and the establishment of the more direct channel route across the fault. For both recurrence interval and strain rate calculations, it is important to estimate how many periods of elastic strain accumulation can be associated with a given total displacement or interval of time.

In the case of scenario 1, wherein a seismic event moved the shutter ridge aside, the stream developed a new straight path across the fault and deposition on the fan surface ceased. The geomorphic strain recorder was thus turned on at the time of an earthquake, or at the beginning of a new strain accumulation interval. After the fifth slip event in 1906, the initially straight channel that developed sometime after 910 $\pm$ 30 A.D. (Table 2) has been displaced a total of 44 ft.

For scenario 2, a flood buried the shutter ridge with alluvium and the stream established its new straighter and steeper path to the axis of the rift valley. In this case, the geomorphic strain recorder was turned on between earthquakes or within a strain accumulation interval, perhaps near the time of the younger earthquake. The first of five 1906 magnitude slip events occurred, and the channel was displaced approximately 9 feet. If, in the extreme case, this first earthquake occurred just after the establishment of the new channel, then the strain rate can be estimated by considering that only four complete strain accumulation cycles have occurred between 910 A.D. and 1906 and have produced 35 ft of slip during that 996-yr period. Strain rate and recurrence interval calculations based upon these two scenarios are summarized in Table 2. The incised abandoned channel shown in Figure 7 suggest that scenario 2 is the more probable since it is unlikely that a seismic event alone would have forced the stream to abandon it's channel. The prominent stone line shown at the base of the A horizon at the northeastern end of the trench may in fact be the basal deposit of the flood which overtopped the shutter ridge (Figure 8). Since there is no record of when, in the first strain accumulations cycle, the steam developed its new channel, the strain rate and recurrence interval for this reach of the fault are estimated by averaging the two end members for the date of 910 A.D. The results are a strain of 1.2 cm/yr and an average recurrence interval of 224 $\pm$ 25 yr. This geologic estimate of strain rate is in remarkable accord with the geodetic strain rate of 1.2 cm/yr across the San Andreas fault on the San Francisco Peninsula as determined by Prescott et al. (1981) for the decade between 1970 and 1980.

References Cited

Cotton, W. R., Hall, N. T., and Hay, E. A., 1982, Holocene behavior of the San Andreas fault at Dogtown, Point Reyes National Seashore, California: Final Tech. Rept. for U.S.G.S. Contract No. 14-08-0001-19841, 33 p.

Hall, N. T., 1984, Holocene history of the San Andreas fault between Crystal Springs Reservoir and San Andreas Dam, San Mateo County, California: Seis. Soc. America Bull., v. 74, No. 1, pp. 281-299.

Hall, N. T., Hay, E. A., and Cotton, W. R., 1984, Problems in the application of ^{14}C dates to slip rate determinations on the San Andreas fault: (abst.) Cordilleran Sect., Seis. Soc. America, Anchorage, Alaska, Earthquake Notes, V. 55, No. 1, p. 8.

Hay, E. A., Cotton, W. R., and Hall, N. T., 1981, Holocene behavior of the San Andreas fault in Northern California: (abst.) Cordilleran Sect., Geol. Soc. America, Hermosillo, Sonora, Mexico, v. 13, No. 2, p. 60.

Lawson, A. C., et al, 1908, The California earthquake of April 18, 1906; Report of the State Earthquake Investigation Commission: Carnegie Inst. Wash. Publ. 87, 451 p.

Louderback, G. D., 1947, Central California earthquake of the 1830's: Seis. Soc America Bull., v. 37, pp. 33-74.

Prescott, W. H., Lisowski, M., and Savage, J. C., 1981, Geodetic measurement of crustal deformation on the San Andreas, Hayward, and Calaveras faults near San Francisco, California: Jour. Geophys. Res., v. 86, pp. 10853-10869.

Sieh, K. E., 1978, Prehistoric large earthquakes produced by slip on the San Andreas fault at Pallett Creek, California: Jour. Geophys. Res., v. 83, pp. 3907-39.

Sieh, K. E., and Jahns, R. H., 1984, Holocene activity of the San Andreas fault at Wallace Creek, California: Geol. Soc. America Bull., v. 95, pp. 883-896.

Stuvier, M., 1982, A high-precision calibration at the AD radiocarbon time scale: Radiocarbon, v. 24, pp. 1-26.

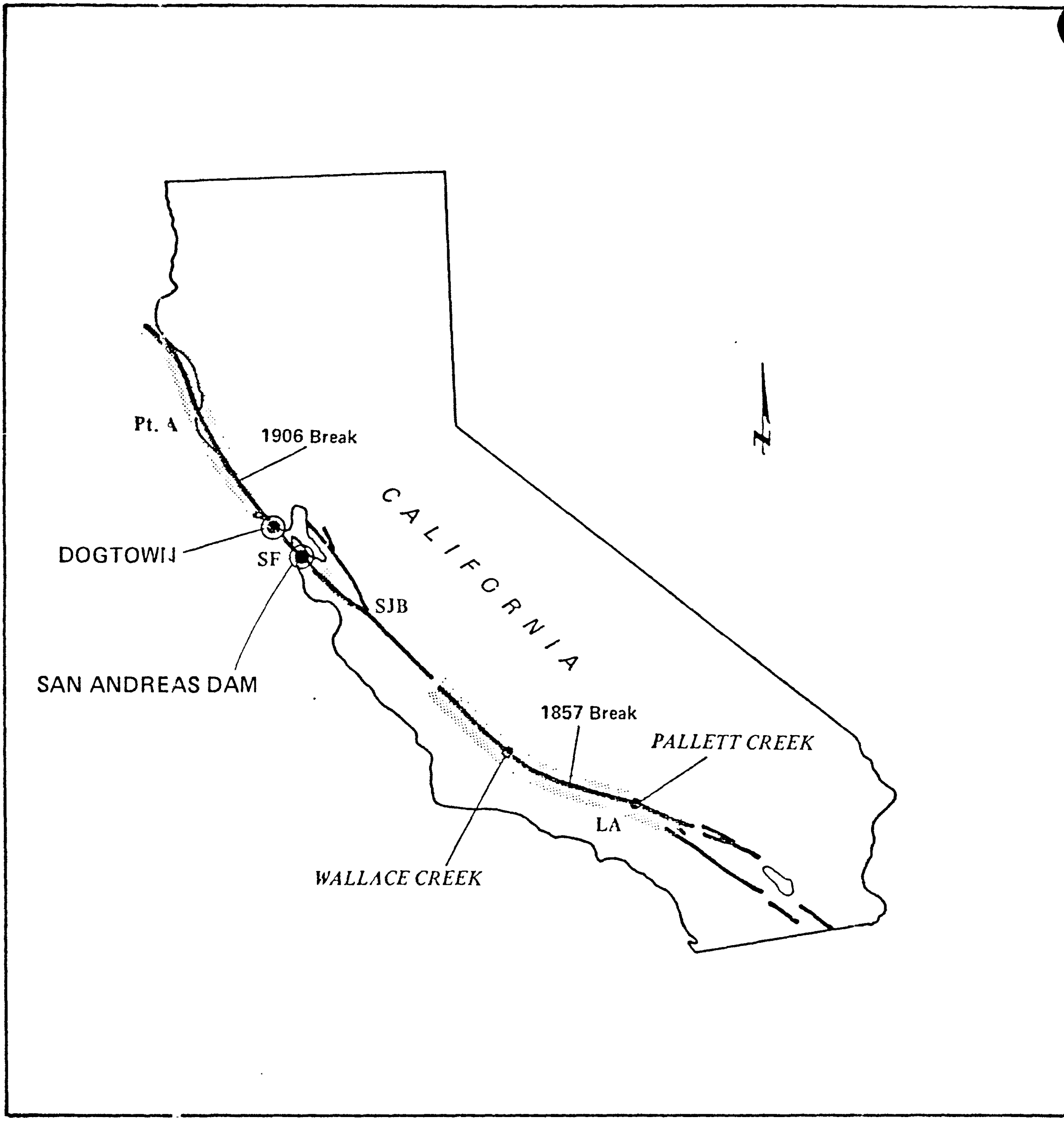


Figure 1. Locations of the Dogtown Research Site and the historically active segments of the San Andreas fault system. (LA-Los Angeles, SF-San Francisco, SJB-San Juan Bautista, Pt.A-Point Arean.)

ORIGINAL

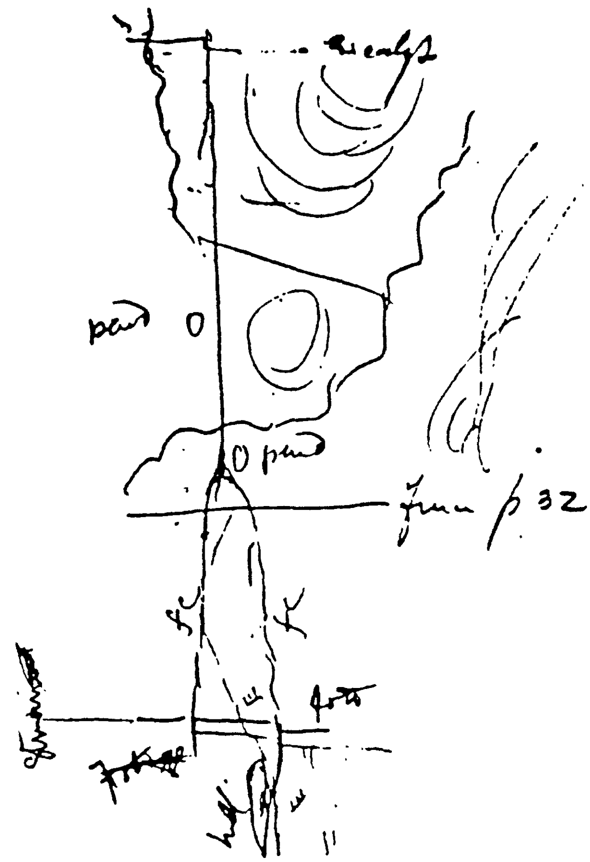


Figure 2A. G. K. Gilbert's Sketch Map of Dogtown

INTERPRETATION

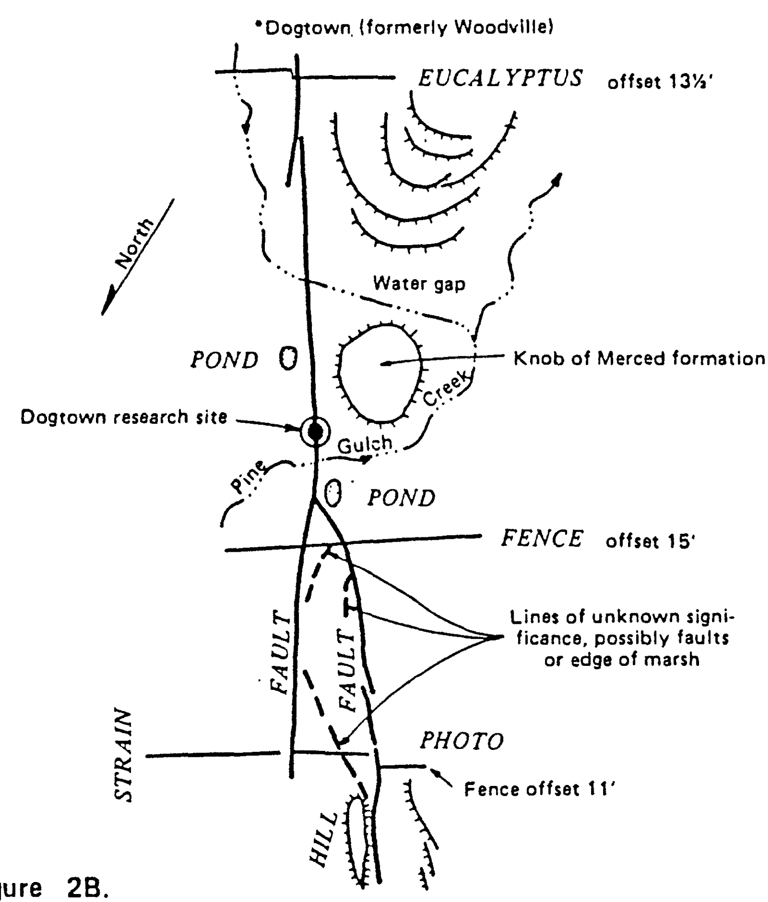


Figure 2B.

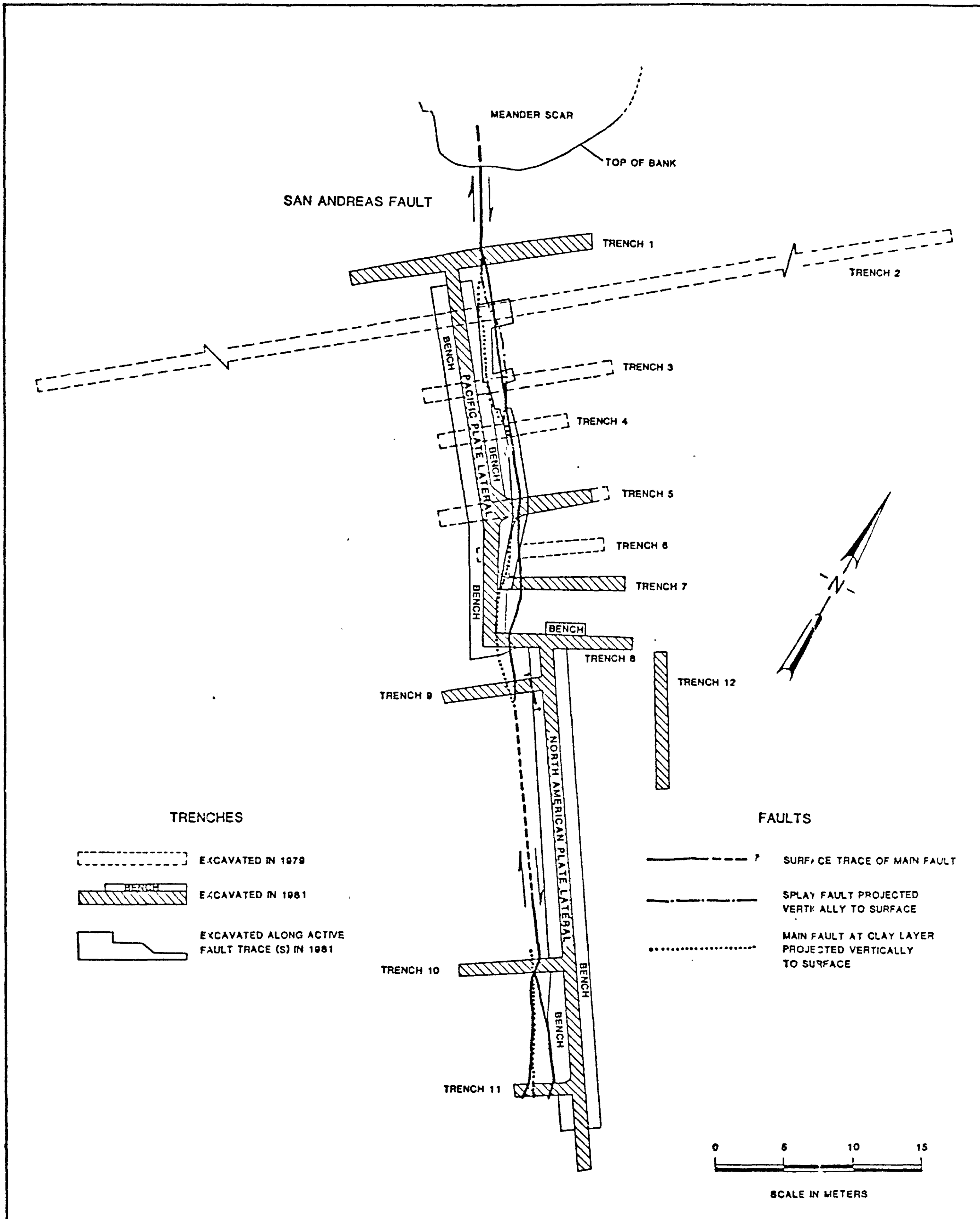


Figure 3. Location of Dogtown Trenches.

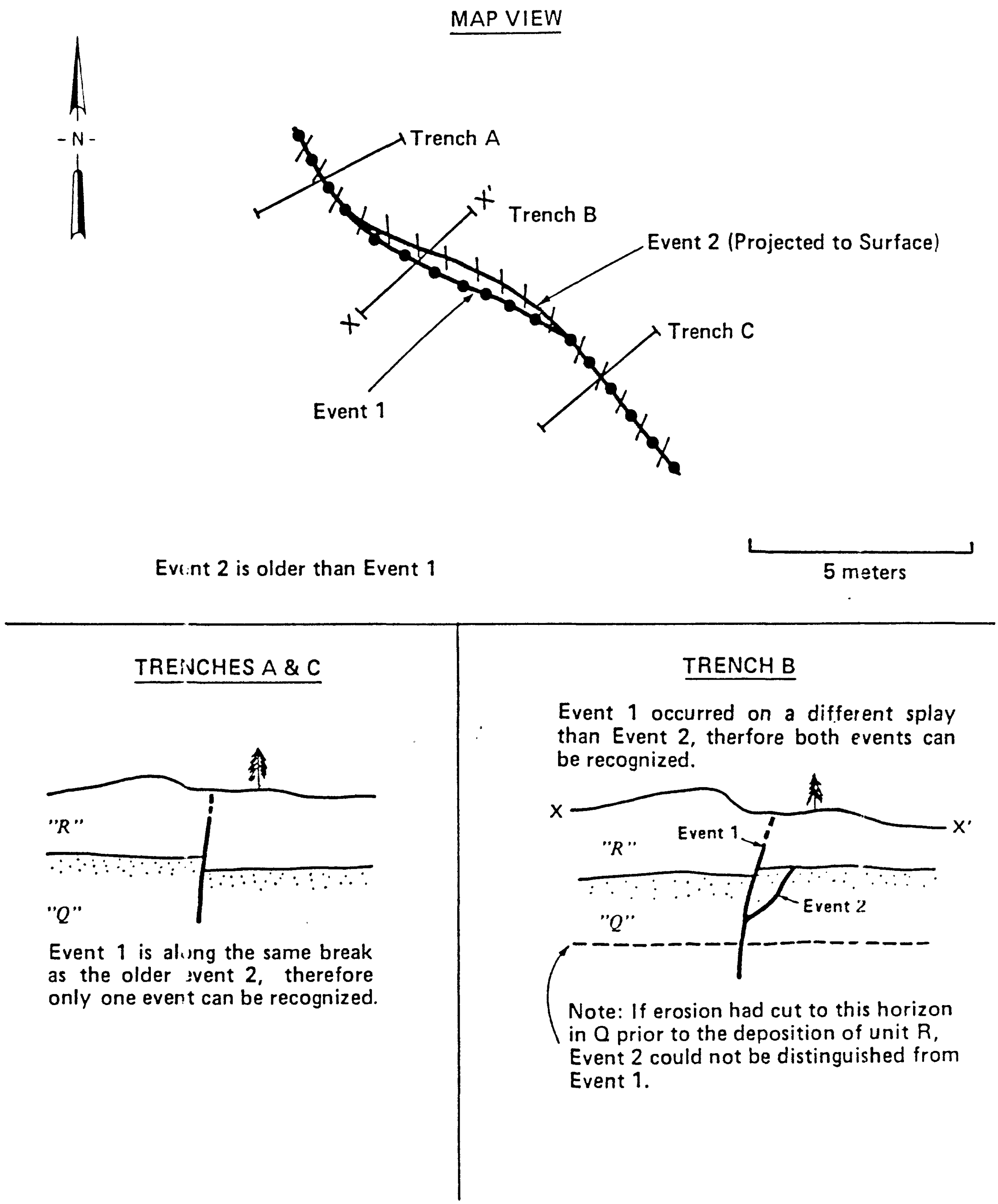
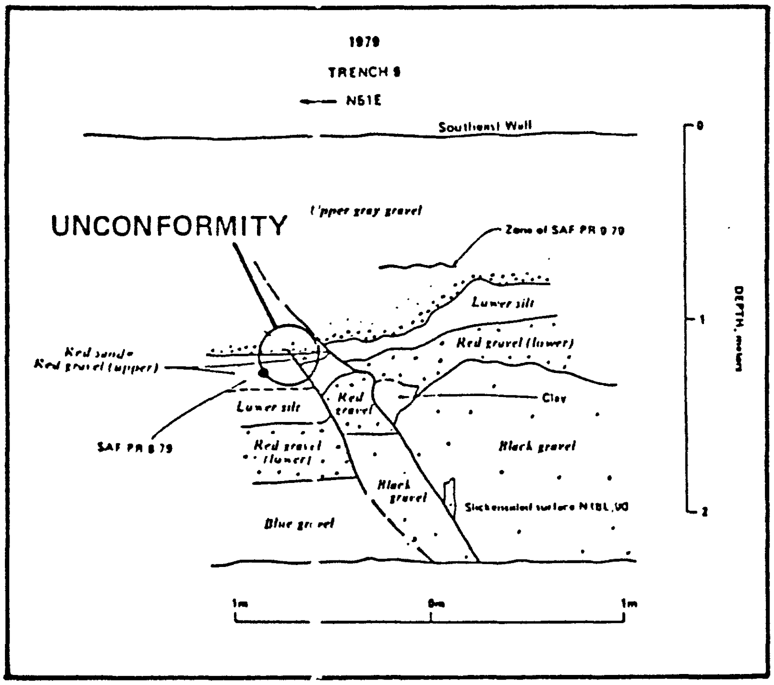


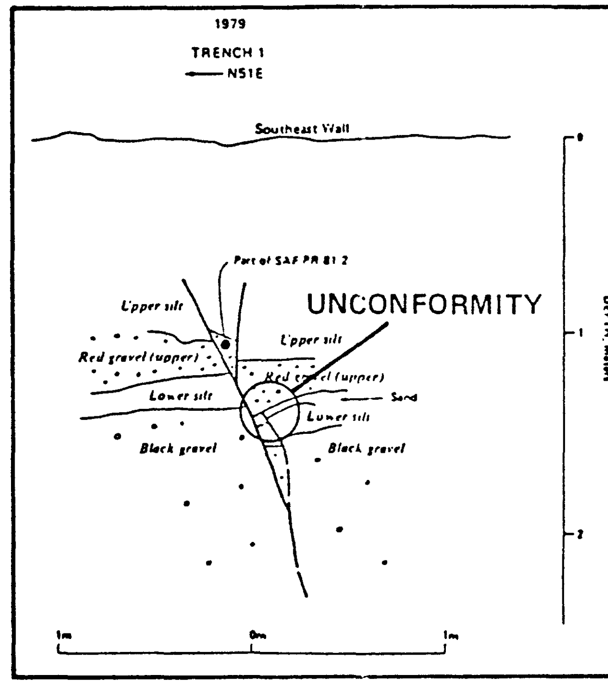
Figure 4. Diagrams Showing Difficulties in Recognizing Individual Paleoseismic Events.

A. Pre-1906
EVENT 1
Between 38) ± 85 and 490 ± 150 B.P.



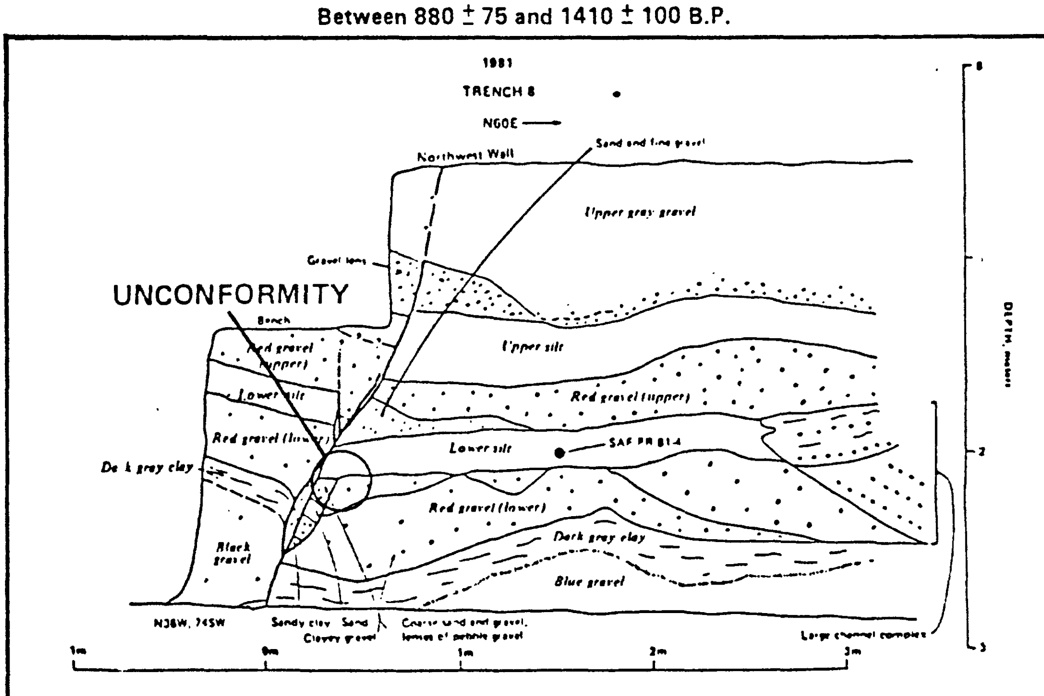
Trench 9, SE Wall, (1979)

B. Pre-1906
EVENT 2
Between 490 ± 150 and 880 ± 75 B.P.



Trench 1, SE Wall, (1979)

C. Pre-1906
EVENT 3
Between 880 ± 75 and 1410 ± 100 B.P.



Trench 8, NW Wall, (1981)

Figure 5. Trench logs showing evidence for pre-1906 ground-rupture events at Dogtown.

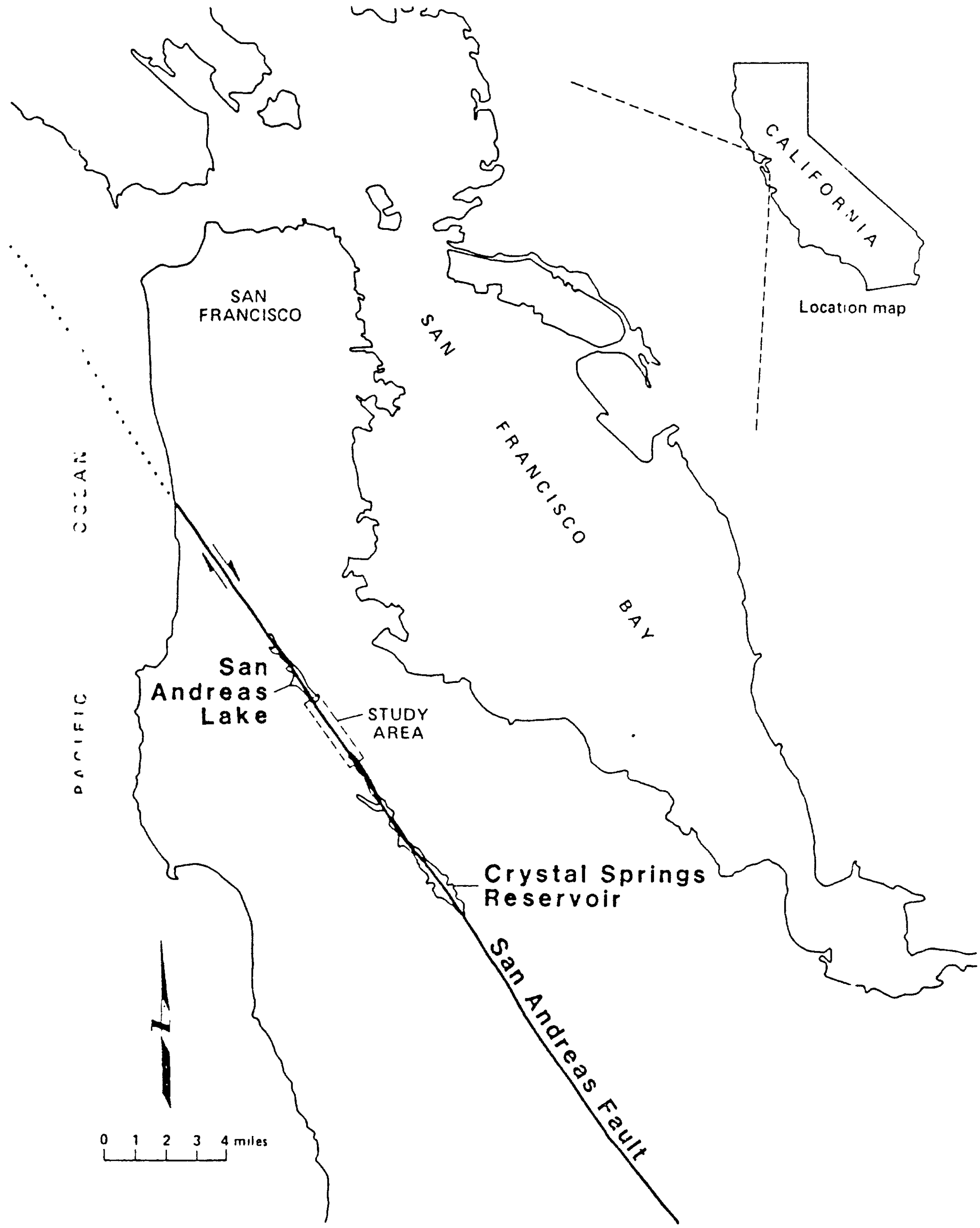


FIG. 6 Map of the San Andreas fault on the San Francisco Peninsula, California, showing location of the study area.

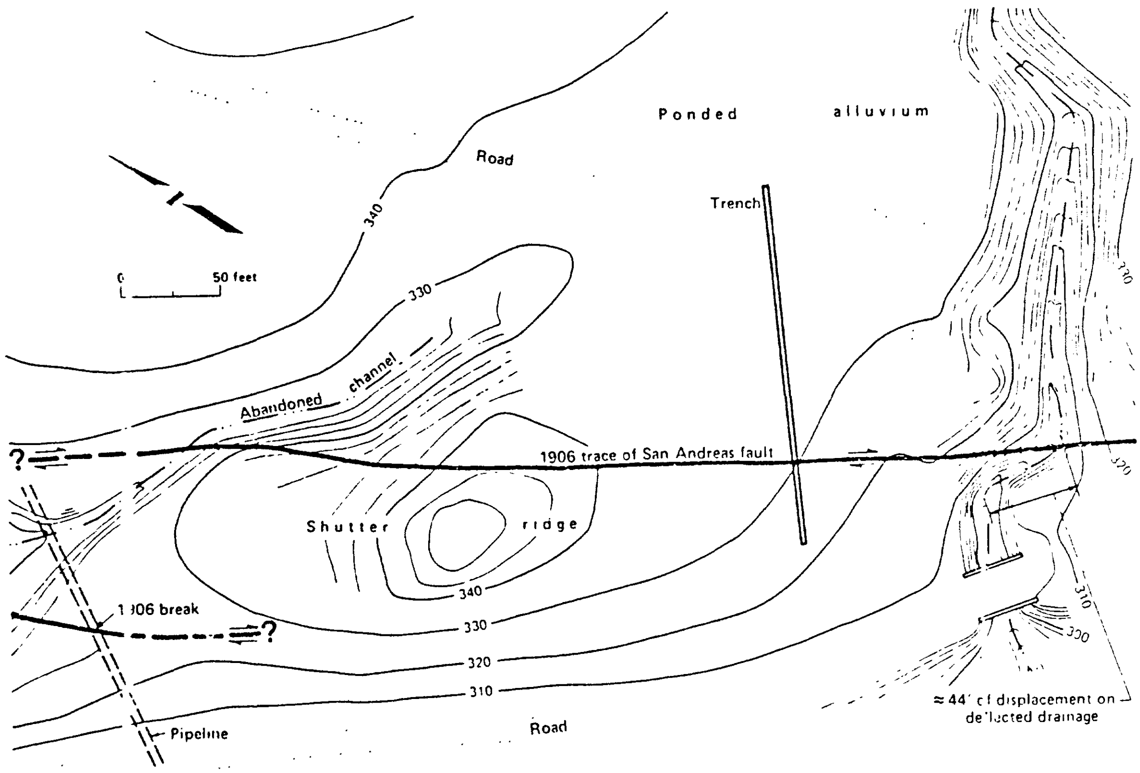


FIG. 7. Topographic map showing a shutter ridge, ponded alluvium, and a tectonically deflected stream channel located approximately 8500 ft southeast of San Andreas Dam.

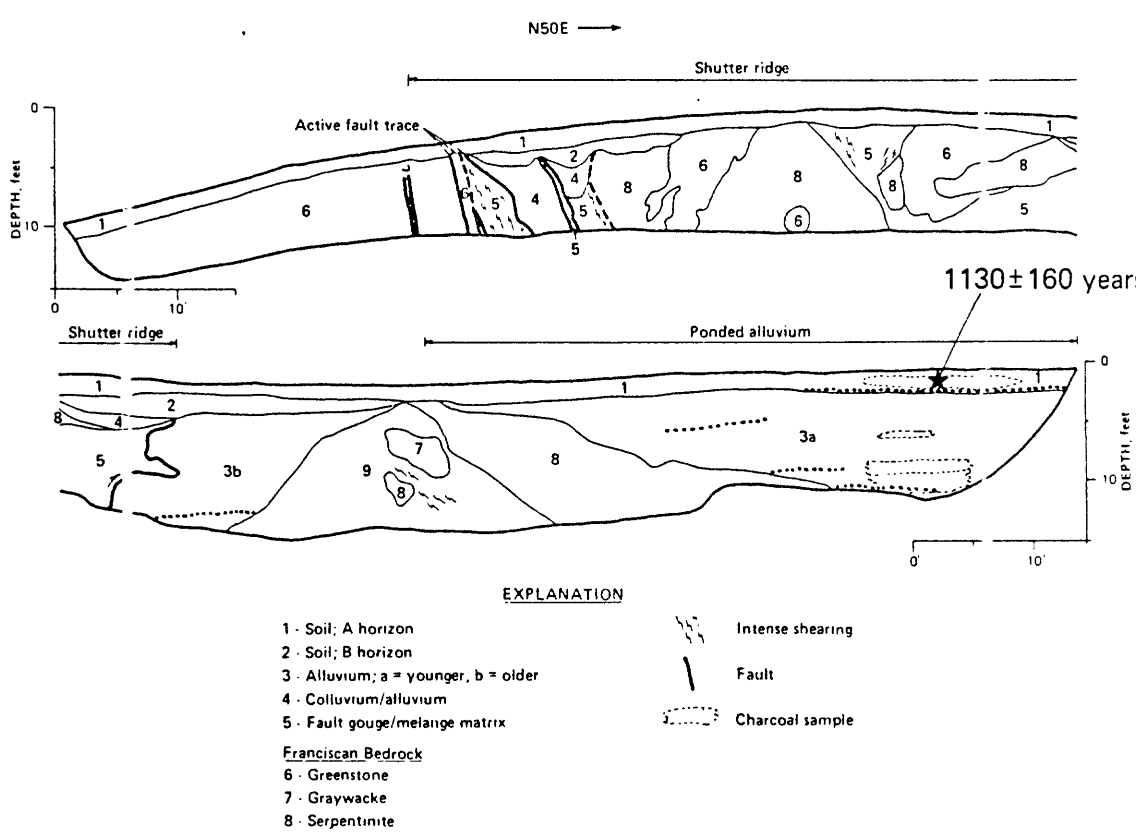


FIG. 8. Diagrammatic log of northwest wall of backhoe trench through shutter ridge and ponded alluvium 8500 ft southeast of San Andreas Dam.

TABLE I

RADIOCARBON DATES

UNIT (in stratigraphic order)	SAMPLE #	MATERIAL DATED*	RADIOCARBON AGE (years before 1950)
Upper Gray Gravel	1979-9	C	380 ± 85
Upper Silt	1979-1	C	740 ± 115
Red Gravel (Upper)	1979-8	C	1245 ± 105
	1979-2	C	2130 ± 90
	1981-1	C	490 ± 150
	1981-2	C	780 ± 110
Lower Silt	1979-5	C	880 ± 75
	1981-3	C	1330 ± 120
	1981-4	C	1230 ± 130
Red Gravel (Lower)	-	-	-
Dark Gray Clay	1981-6	C	1690 ± 210
	1981-10	W	270 ± 75
	1981-12	W	195 ± 75 **
		W	215 ± 75 **
	1981-13	W	205 ± 75
	1981-18	W	260 ± 90
Black Gravel	1979-3	C	2230 ± 105
	1981-5	C	1890 ± 100
Blue Gravel	1979-4	C	1410 ± 100
Blue-Gray Clay	-	-	-

* C = Charcoal
W = Wood

** split sample

TABLE 2

GEOLOGIC INTERPRETATION OF FIGURE 7

Deposition of ponded alluvium ceased after 1130 ± 160 radiocarbon years B.P. ago			SCENARIO I 5 seismic events, 5 periods of strain accumulation, 44′ of offset		SCENARIO II 4 seismic events, 4 periods of strain accumulation, 35′ of offset	
Radiocarbon Age B.P. (1950)	Time Calendar Years	Date A.D.	R.I.* (yr)	Strain Rate (cm/yr)	R.I.* (yr)	Strain Rate (cm/yr)
1130–160	Minimum 856 yr	1040	171	1.57	214	1.25
1130	Calculated date 996 yr	910 ± 30	199	1.35	249	1.07
1130 + 160	Maximum 1226 yr	680	245	1.09	307	0.87

* Recurrence interval.

Bay Area Workshop Summary—Geodetics
27 February 1986

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In this discussion I have attempted to briefly summarize some of the results that have been obtained from repeated geodetic measurements in the San Francisco Bay area. In the course of this discussion, hopefully, a picture will emerge of a plate boundary region with the relative motion spread over a distance of almost 80 km normal to the direction of relative plate motion. No single fault accounts for more than about ⅓ of the total motion. There is a great deal of variety in the ways that the motion is accommodated on the individual faults. The discussion is focussed on the figures. They are not numbered, but the text follows the order of the figures strictly.

Figure 1 Map of all lines in Bay Area

This map contains nearly all the lines that are measured in the Bay area. The network extends from the vicinity of Hollister on the south to the Farallons on the west, and nearly the central valley on the east.

Most of the observations to be discussed were made with a single color geodolite, using an aircraft to measure refractivity and providing a precision of about 2×10^{-7}. With few exceptions, observations are annual or less often in frequency. In addition to distance measurements, there is some triangulation in the area that I am not going to discuss, and some creep observations that I will touch on briefly.

The discussion will move around the Bay in a counter clockwise direction beginning with the southern part of the San Andreas fault and ending with the northern part of the Hayward fault.

Figure 2 Map of lines in south bay

This is an enlarged view of the lines at the southern end of San Francisco Bay. Note that the network spans all three major faults, the San Andreas, the Hayward, and the Calaveras. It also crosses the San Gregorio, but barely. A bird's eye view of displacement across the entire zone can be obtained by looking at a profile of the velocity of stations. Rather than plotting the velocity vectors, it is more informative to look at the vector components. I resolved the vectors into components parallel and normal to the fault. (Incidentally this is old figure, but there is no obvious evidence of change except for Morgan Hill earthquake).

Figure 3 Profile of displacement $\parallel$ and $\perp$ along a cross section.

Not surprisingly, the parallel component is much larger than the normal. There is an overall right lateral shear. The total slip is about 33 mm/yr. About 10 occurs on San Andreas, 9 on Hayward, mm/yr. About 10 occurs on San Andreas, 9 on Hayward, and 6 on Calaveras. Balance of 8 or 9 mm/yr occurs east of Calaveras. There is a diversity of mechanism: Distributed shear on San Andreas, Creep on Hayward, ? on Calaveras, and block rotation east of Calaveras.

West Bay

Figure 4 Map of lines
This is an expanded view of the lines in the vicinity of the San Andreas fault. To summarize our findings along the San Andreas—there is a high strain rate (0.6 µrad/yr) near the fault, within 5 km or so. Further away the strain rate drops to a more typical 0.30 µrad/yr. The total rate appears to be about 10-15 mm/yr, integrated across the shearing boundary. The geodetic data suggests a fairly constant behavior along the San Andreas fault from Hollister to Point Reyes (off this figure at the southern and northern ends respectively). The following half dozen figures will be the data for these lines and plots of strain accumulation derived from the observations.

Figure 5 Plots of data
Plots of the data for this sub-network are included as samples of the data for the whole area. Some of the plots are smooth, others are not. The large decreases in the 1980 to 1983 time period are probably real. We will look at this time period in more detail later.

Figure 6 Plots of data
Figure 7 Plots of data
Figure 8 Plots of data
Figure 9 Plots of data

Figure 10 Strain versus time
The final time plot contains three components of strain, northwest-southeast shear, east-west shear, and dilatation. The small drop in shear in 1982 and the somewhat larger drop in dilatation are both the result of the decrease in line length observed on many of the lines.

Figure 11 Strain rate table and principal strains
This table summarizes the average rates obtained along this section of the San Andreas fault. The deformation is primarily shear—across N41°W ± 2°. The dilatation is small but significant. The integrated slip across the network is 10 ± 0.3 mm/yr.

Morgan Hill Update

Figure 12 Loma net diagram
At the southern end of San Francisco bay we have been observing

three lines on a weekly or monthly basis for about four years. The line lengths are 43 km, 31 km, and 31 km. The lines cross all three of the major faults in the area.

Figure 13 Frequent history of Loma Prieta lines
Frequent observations began in 1981. The most obvious change observed is that associated with the Morgan Hill earthquake in April 1984. The earthquake produced a 25 mm offset in the length of the line Loma Prieta-Hamilton Not surprisingly, there has been a nearly uniform decrease in the length of the line to Allison, a predictable consequence of the right lateral shear to which the region is subjected. What is surprising is that the line to Eagle Rock did not show a corresponding increase in length during the initial observations.

Figure 14 Loma-Eagle Rock—long term & recent
This is an expanded plot of the data for the line Loma Prieta to Eagle Rock. The inset plot contains the entire history of the line. The larger plot is just the observations since we increased the frequency of observation in 1981. In both plots the observed line lengths are shown as filled diamonds. The open diamonds are the boundaries of a 95% confidence window about a smoothed version of the data (a seven point running mean was used to smooth the error bounds). The rate since about the beginning of 1983 has been quite consistent with the long term rate. But the rate from late 1981 until the end of 1982 was quite different. Interpretation of the reversal is complicated by the fact that it shows up in some of the other lines from station Loma Prieta, but not in all of them that a simple model would predict.

Figure 15 Aftershock zone and diagram
The location of most of the geodetic lines in the vicinity of the 1984 Morgan Hill earthquake are shown in this figure. The two lines of most interest are Mt. Hamilton-Loma Prieta and Mt. Hamilton-Llagas. Because the first line is part of the Loma monitor net we had observations of it just before (one the day preceding and one the week preceding) the earthquake.

Figure 16 Hamilton-Loma & Llagas—co seismic data
There is no evidence of any anomally in the data before the earthquake. There was a large change associated with the event; the postseismic deformation continued long after the time period shown on this figure, and perhaps is still continuing today.

Figure 17 Probability
One way to quantify the statement that no precursory slip occurred prior to the earthquake is to plot the probability of a slip event of specified size given the absence of any apparent anomally in the observations. As the figure indicates, at the 95% confidence

level we can rule out a precursory slip event greater than 90 to 140 mm. Smaller precursory events can not be excluded with confidence. There are two lessons to be learned from this experience: 1) For this particular M6+ earthquake no detectable anomally occurred; and 2) the prospects of detecting anomallies with geodetic data of this nature seem bleak. The earthquake occurred practically right under one of our most frequently observed stations and we had an observation just 24 hours prior to the event, not a likely occurrence, given our monthly observation schedule.

Figure 18 Hamilton-Loma—long term history

Figure 19 Hamilton-Loma—post seismic data
The rate of change of this line has continued at an accelerated pace since the earthquake. It is still not back to its pre-earthquake rate.

Figure 20 Hamil Ec-Llagas—long term history
This line was the most favorably oriented for detecting slip in this earthquake. It also has continued to slip since the earthquake.

Figure 21 Hamil Ec-Llagas—post seismic data
This plot includes the last observation before the Morgan Hill event and all the data since. Note that although the rate appears to have slowed down significantly it is still not back to the long-term value.

Figure 22 Table of Co-seismic slip—trial models
We have attempted to find the slip surface most consistent with the co-seismic changes in the length of all the lines in the area. All models extended 25 km from the vicinity of Mt. Hamilton to the vicinity of Morgan Hill (the aftershock zone). Various depths and fault widths were tried. The preferred models are 4-10 km or 6-12 km. The last column is the sum of the weighted $(o\text{-}c)^2$, a measure of the goodness of fit.

Figure 23 Table of Post-seismic slip—trial models
This figure is similar to the preceding one. In this case we fit model to all of the change in line length from the time of the earthquake through the most recent survey.

Figure 24 Table of Co & Post-seismic slip and moment—final
This table summarizes the preferred models. The total slip associated with this earthquake is over 1 meter. 57% of the total offset has occurred since the event and it appears that there is still some accelerated slip occurring.

Figure 25 Whole Bay diagram
Unlike the west side of the San Francisco bay, the faults on the east side of the bay are characterized by creep. The creep rate on the Calaveras fault near Hollister (Latitude 36°50'), as measured geodetically is about 15 mm/yr. Further north, along the side of San Francisco bay this slip is probably split between the Hayward and the Calaveras faults.

Figure 26 South Bay diagram
This figure is similar to the second one except that it shows the location of a new station between the Hayward and Calaveras faults near the southern end of the Hayward fault.

Figure 27 South Bay displacement profile—repeat
The displacement profile clearly indicates an offset of about 15 mm/yr across the combined Hayward-Calaveras faults. There is a single station between the faults in this profile. We have since added another station to better define where the transfer of slip from 100% Calaveras to approximately 50% on each takes place. As yet there is inadequate data for the new station.

Figure 28 Grant Ranch—diagram and fault crossing lines
At Grant Ranch, on the Calaveras fault near the northern end of the Morgan Hill rupture the rate is only about 10 mm/yr suggesting that some of the slip has already transferred to the Hayward fault. However the seismicity continues on the Calaveras fault somewhat further north.

Figure 29 Data for three Grant Ranch fault crossing lines
Notice that although nearly a meter of slip has occurred along the Morgan Hill rupture just to the south in the past year and a half, there is no evidence of any significance slip at the surface.

Figure 30 Calaveras reservoir net diagram
Calaveras Reservoir is located about 10 km north of Grant Ranch, but the slip rate on the fault trace is down to about 3 mm/yr with perhaps another 3 mm/yr distributed relatively close to the fault.

Figure 31 Hayward Net diagram
Along the Hayward there is a geodetic network including three low angle fault crossing lines, there are four functioning creepmeters (not shown), a number of small aperture geodetic networks and numerous offset cultural features.

Figure 32 Bald-San Pablo, Redhill-Skyline, Redhill-Allison data
The northern and southern lines both indicate a Hayward slip rate of 7-8 mm/yr. The line Red Hill Top-Skyline has consistently shown a lower rate. The apparent reversal about 1981-82 has not

increased the rate shown by this line. Overall its rate is now 3.0 mm/yr. In an attempt to ascertain whether this signifies a low rate on the fault in the central section, we have installed a small aperture network across the Hayward fault near station Skyline.

Figure 33 Hayward slip rate table

	dL/dT (mm/yr)	Azi.	Slip (mm/yr)
Red Hill-Allison	5.5±0.3	287°	7.9±0.4
Red Hill-Skyline	-2.9±0.7	347°	3.0±0.7
Bald-San Pablo	5.5±1.0	296°	6.7±1.2
Chabot 3-Seneca	2.3±4.7	137°	2.4±4.9
Merritt-Seneca	-11.7±4.4	177°	12.8±4.8
	(μrad/yr)		(mm/yr)
Strain result	.30±.01	33 km	10.0±0.3

The two lines into Seneca are from the new small aperture net near Skyline. At present the result is ambiguous. The last line of the table is derived from a uniform strain fit to all of the lines in the East bay.

Figure 34 Hayward slip—longitudinal profile (R. Burford)

The Hayward fault traverses a very densely populated and built up area. Consequently it is one of the most life threatening hazards of any fault in California, but it also provides an abundant supply of cultural features for estimating slip over the past 100 years. The northern end of the Hayward fault is located at 0 km on the horizontal axis and the southern end at 70 km. These rates are generally somewhat below the currently measured geodetic rates. They are perhaps more consistent with creep rates (4 mm/yr through 1980, 5-6 mm/yr since 1980 from S. Schulz).

Figure 35 Hayward slip—longitudinal profile (R. Burford)

These are just the better culturally-determined slip rates on the Hayward fault.

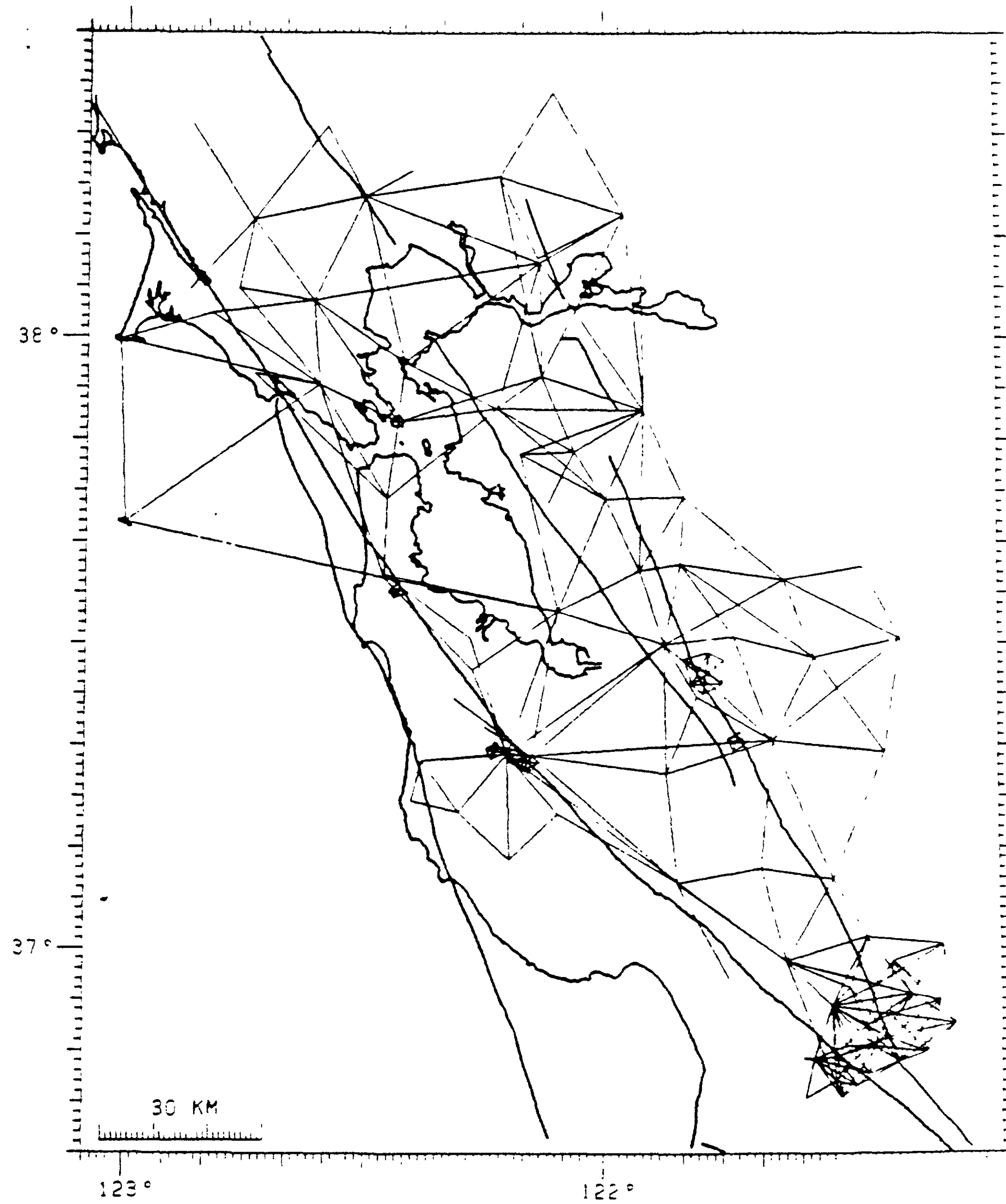


Figure 1

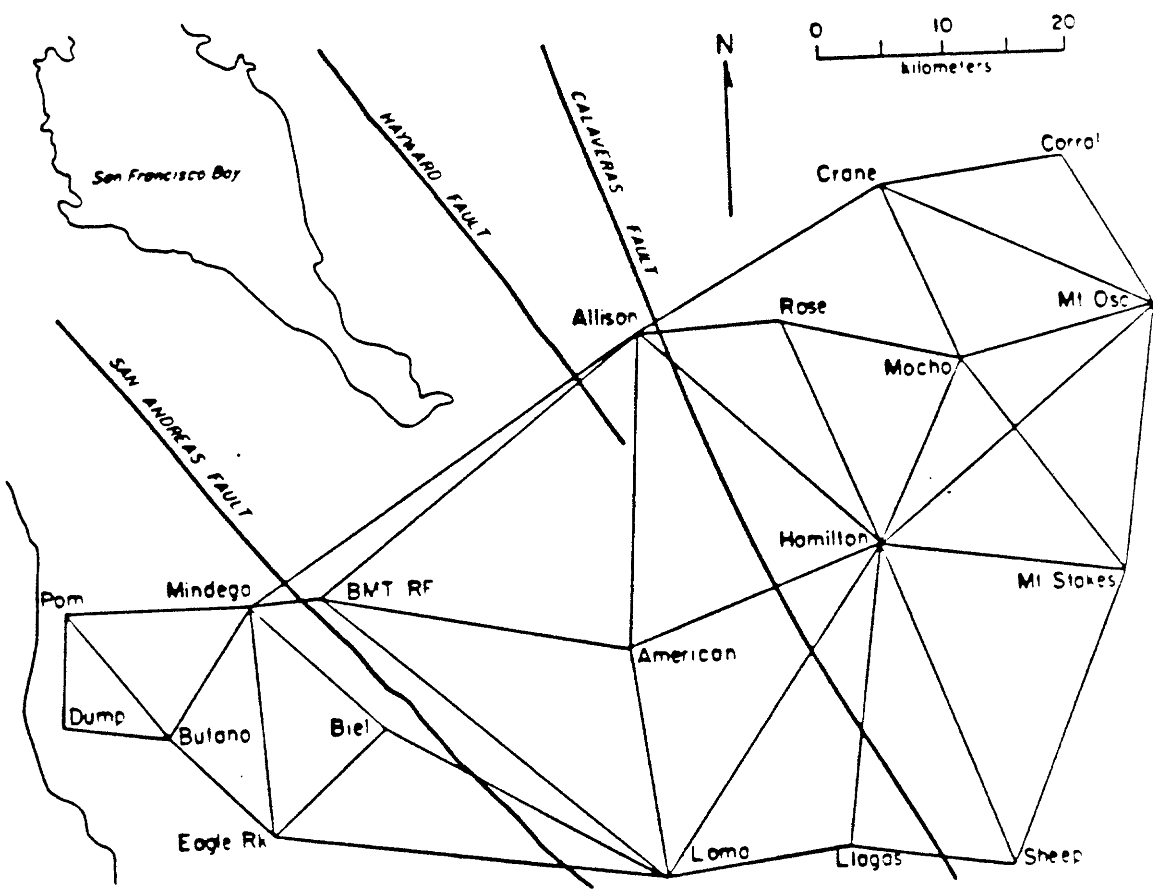


Figure 2 Diagram of the network at the southern end of San Francisco Bay

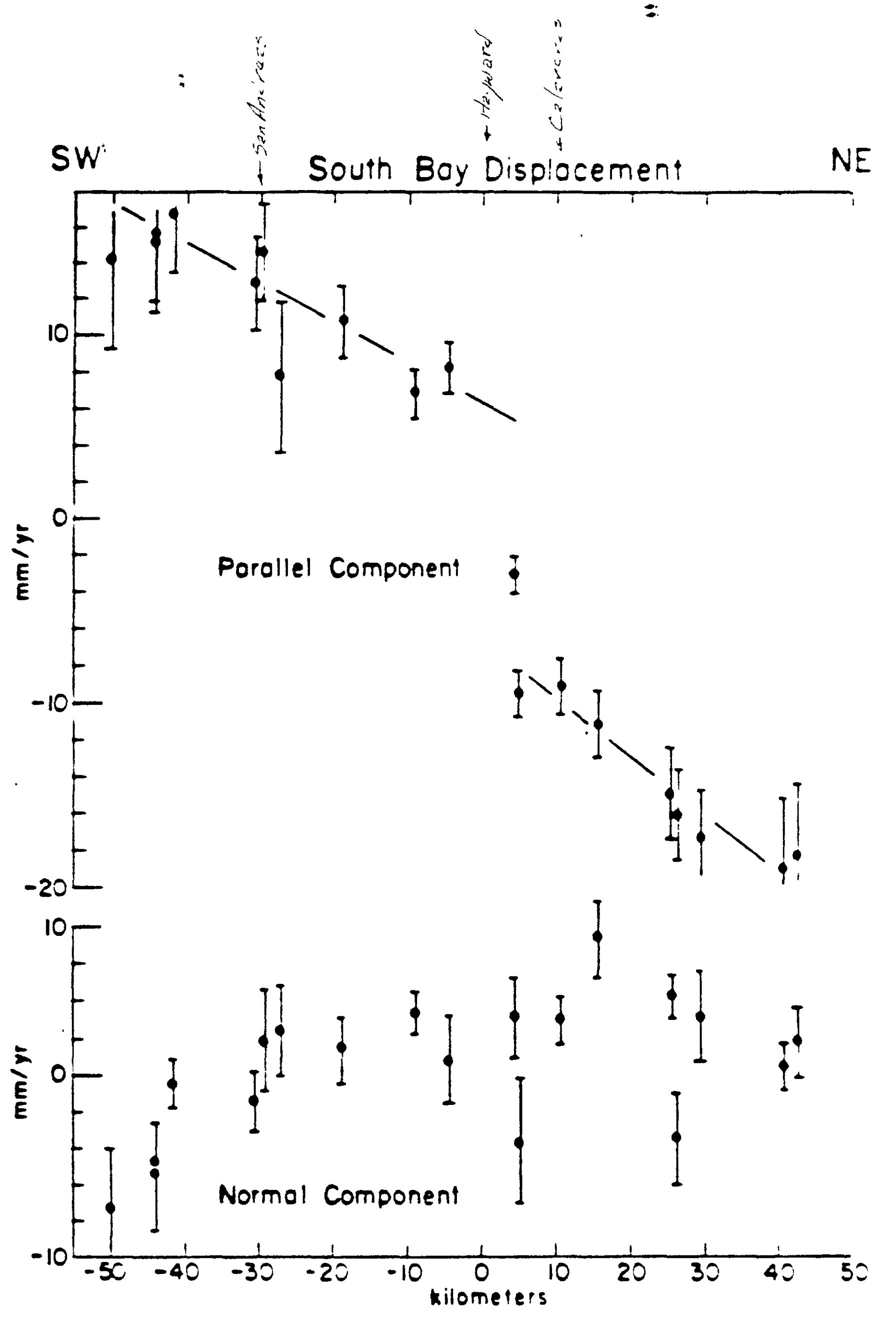


Figure 3 Profile of components of the velocity vector for stations of the network in Fig. 2 At top is the component of motion parallel to the faults; at bottom is the component of motion perpendicular to the faults. Both components are plotted as a function of distance along a normal to the fault plane (with arbitrary origin on both axes).

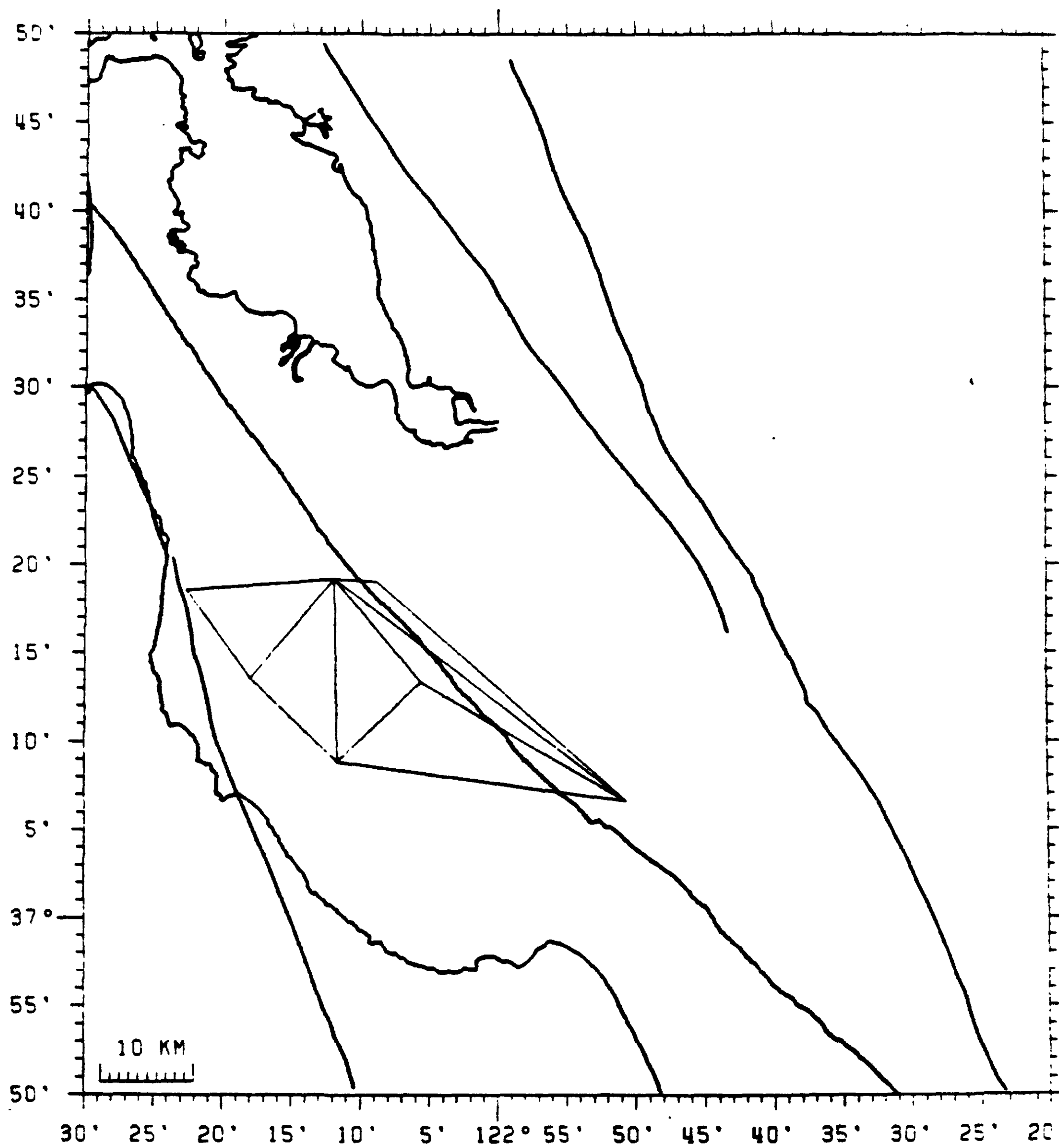

50'
45'
40'
35'
30'
25'
20'
15'
10'
5'
37°
55'
50'
10 KM
30'
25'
20'
15'
10'
5'
122° 55'
50'
45'
40'
35'
30'
25'
20'

Figure 4

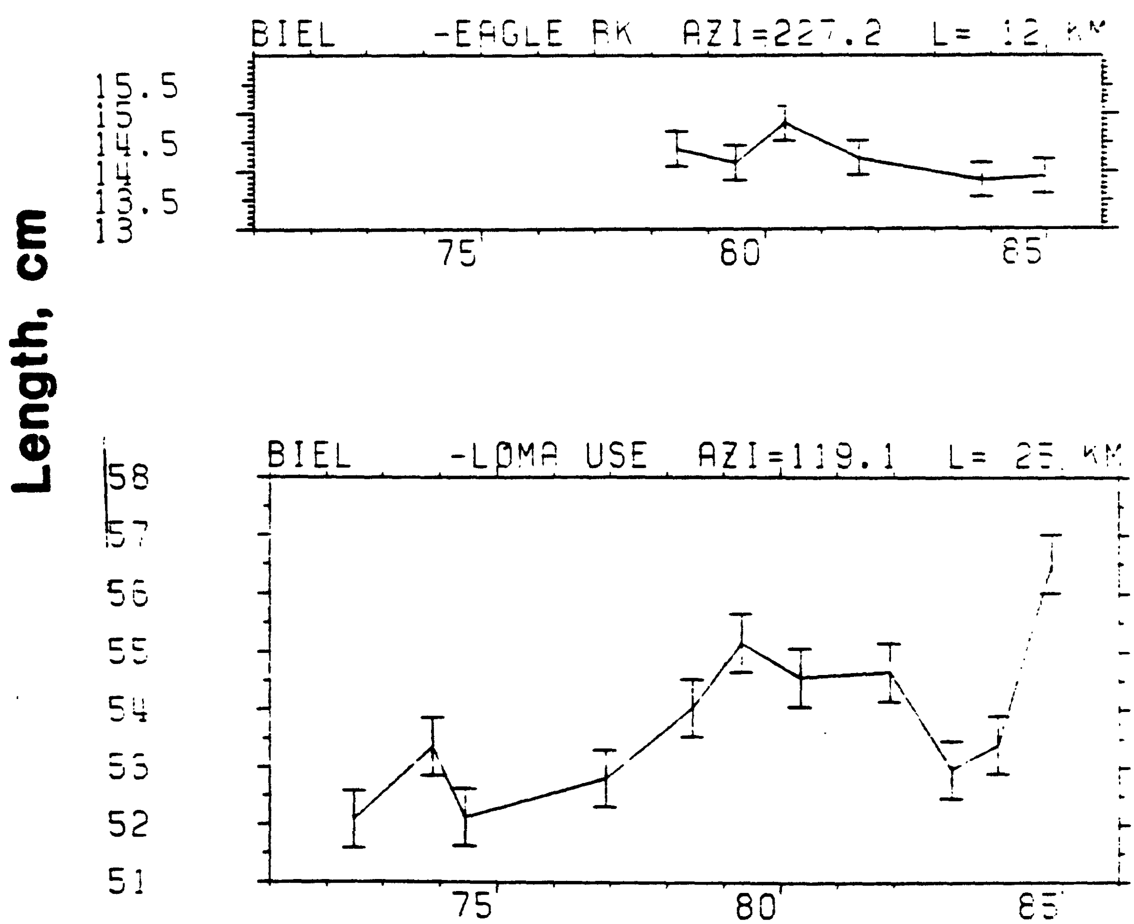

BIEL -EAGLE RK AZI=227.2
15.5
14.5
13.5
75
80
85
Length, cm
BIEL -LØMA USE AZI=119.1
58
57
56
55
54
53
52
51
75
80
85

Figure 5

Figure 6

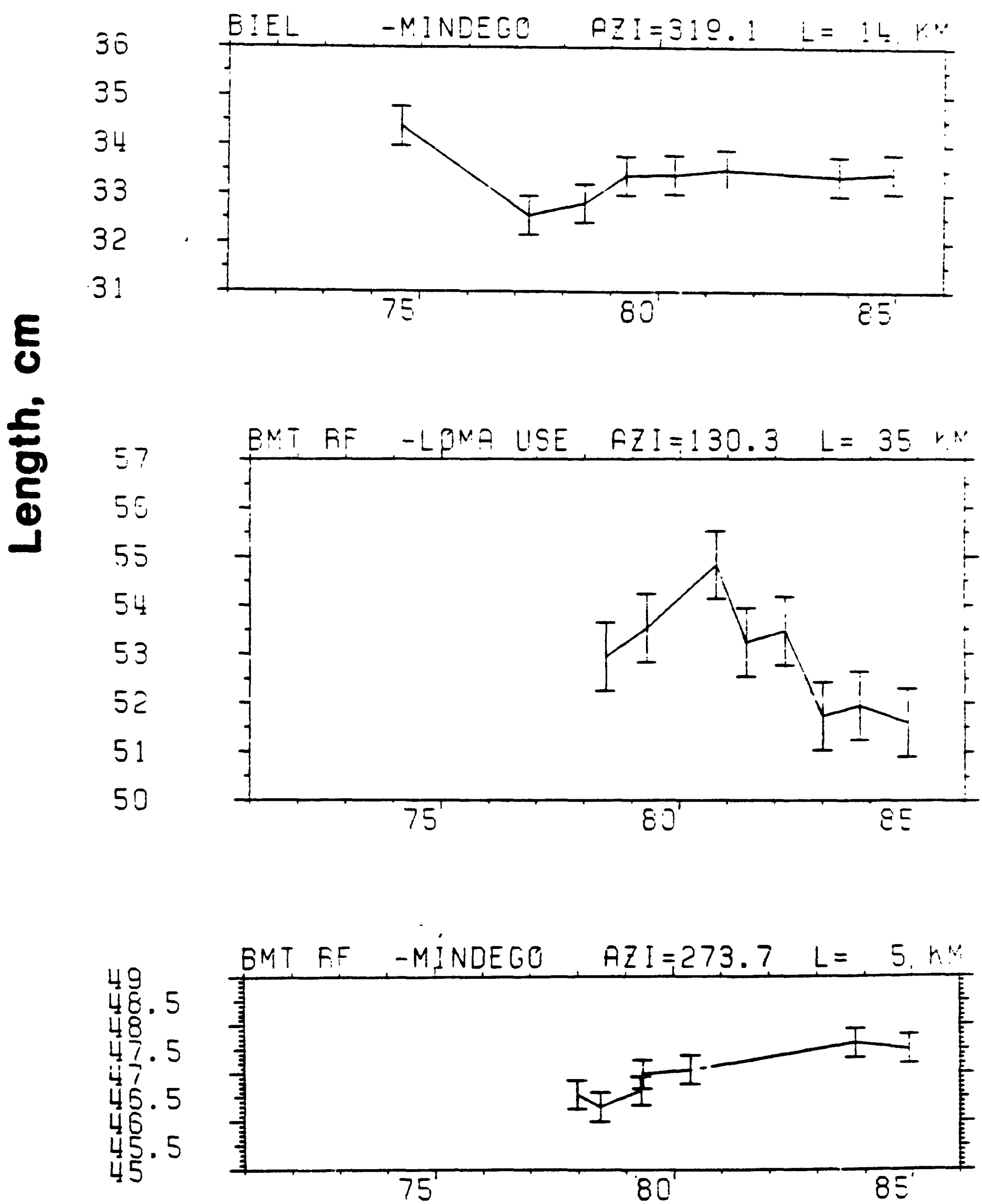
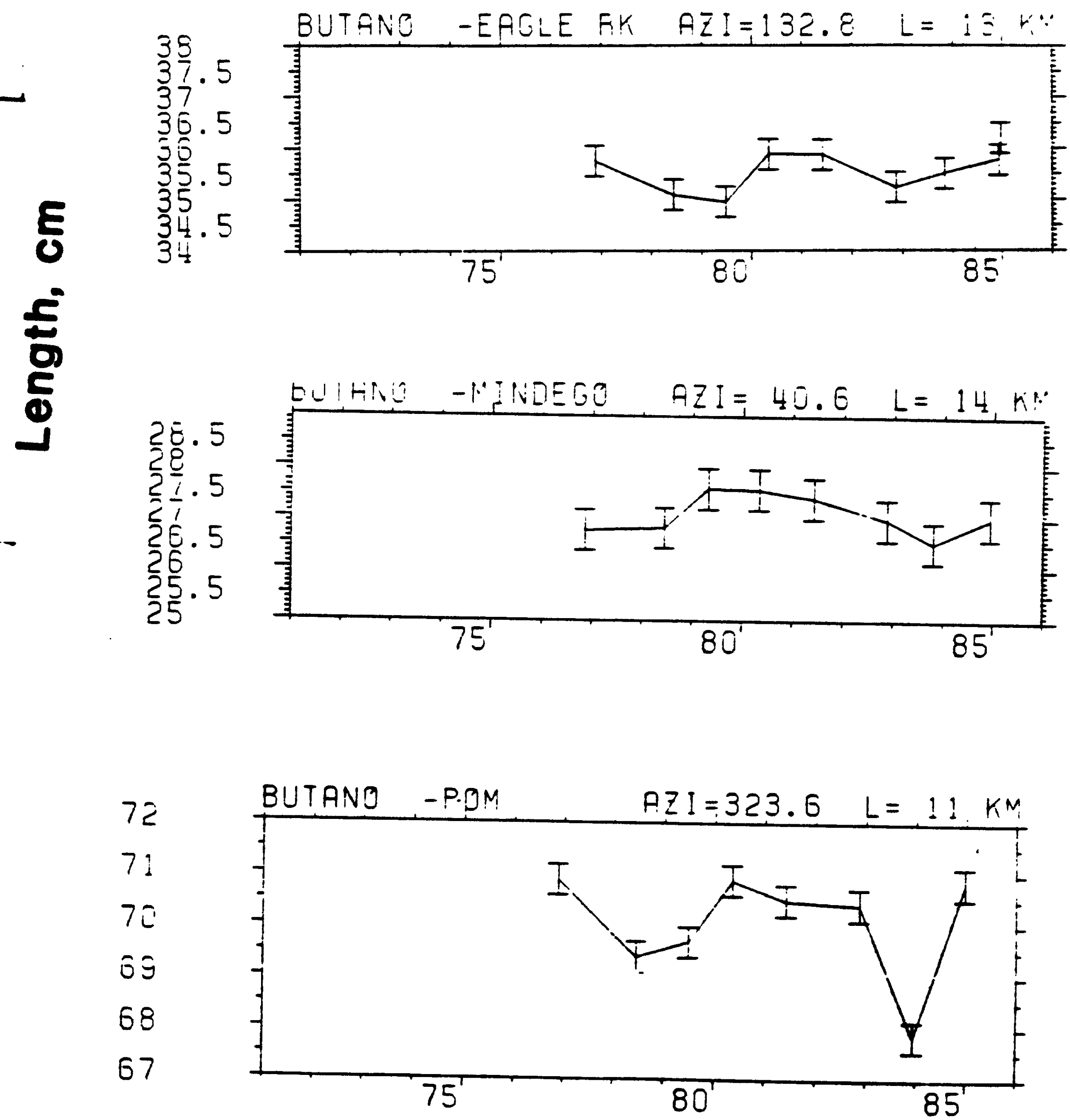

Length, cm
BIEL -MINDEGO AZI=319.1 L= 14 KM
36
35
34
33
32
31
75
80
85
BMT RF -LOMA USE AZI=130.3 L= 35 KM
57
56
55
54
53
52
51
50
75
80
85
BMT RF -MINDEGO AZI=273.7 L= 5 KM
75
80
85

Figure 7



Length, cm
BUTANO -EAGLE RK AZI=132.8
75
80
85
AZI= 40.6 L= 14 KM
75
80
85
BUTANO -PDM AZI=323.6 L= 11 KM
72
71
70
69
68
67
75
80
85

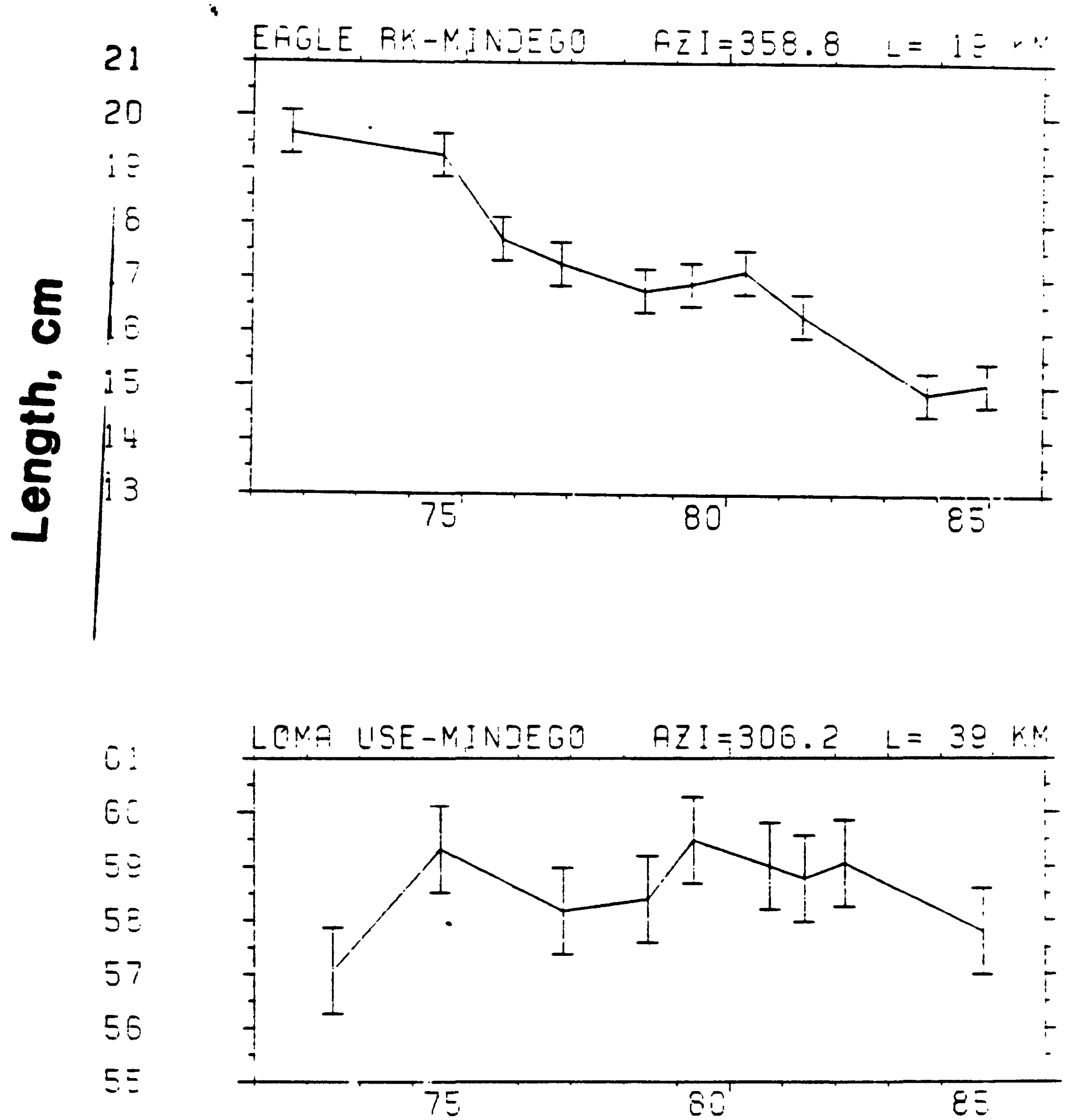

EAGLE RK-MINDEGO
AZI=358.8
LOMA USE-MINDEGO
AZI=306.2
L= 39 KM
Length, cm
75
80
85

Figure 8

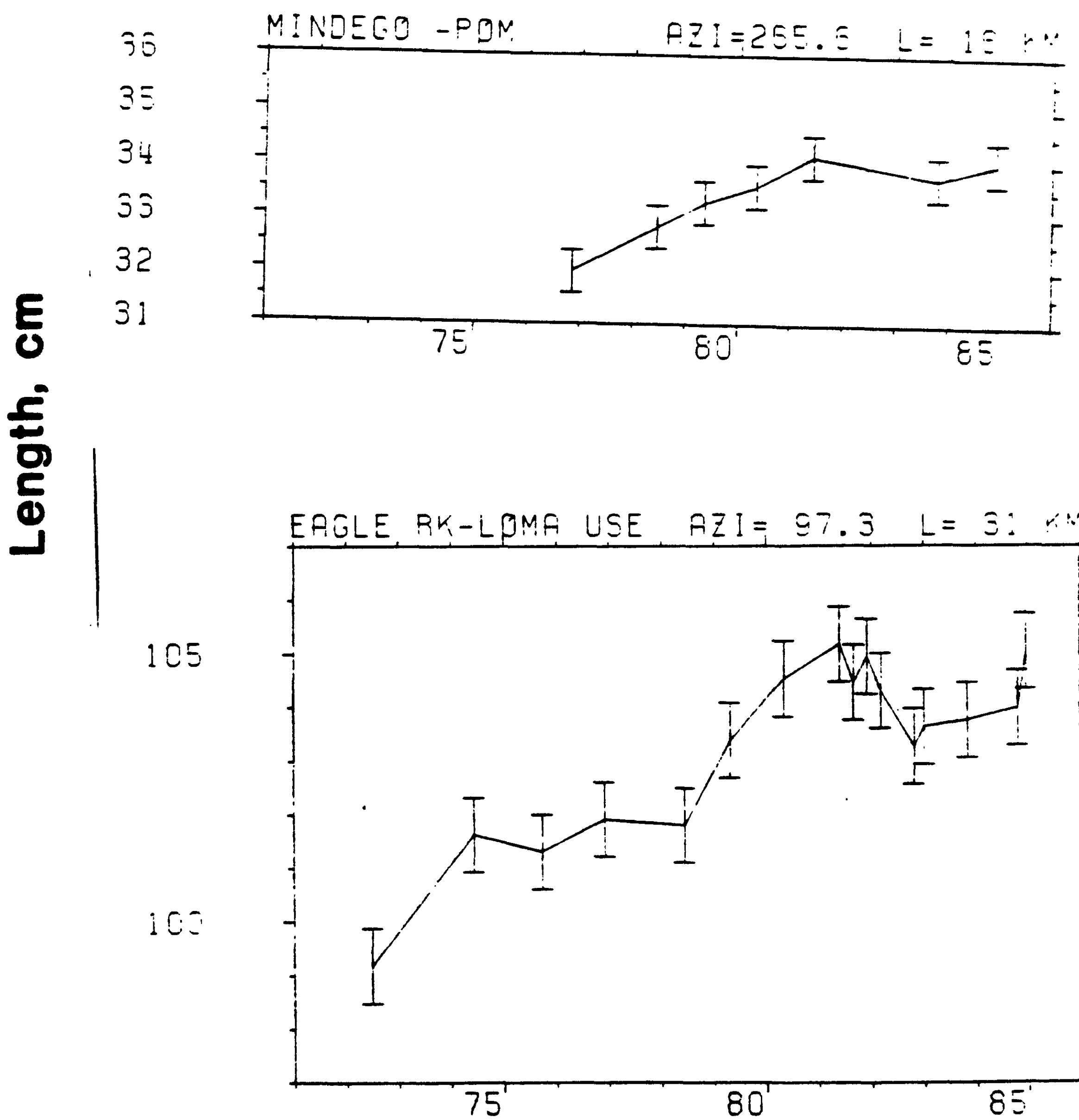
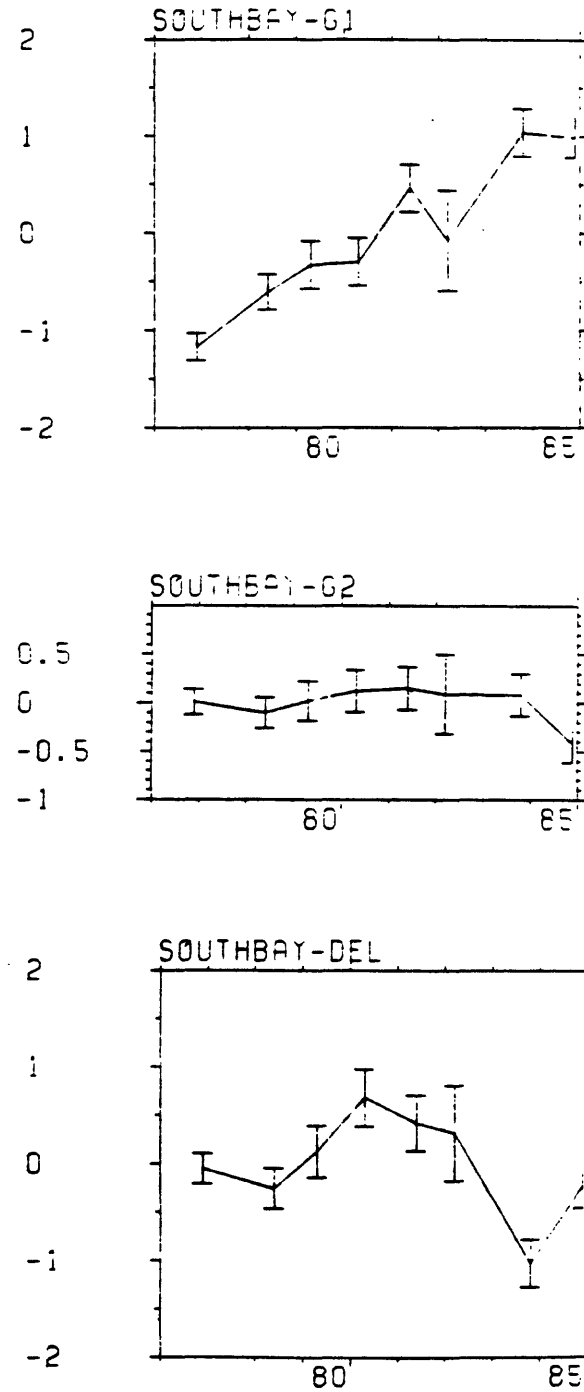

Length, cm
MINDEGO -PQM
AZI=265.6
36
35
34
33
32
31
75
80
85
EAGLE RK-LOMA USE
AZI= 97.3
L= 31 KM
105
100
75
80
85

Figure 9

Figure 10


SOUTHBAY-G1
2
1
0
-1
-2
80
85
SOUTHBAY-G2
0.5
0
-0.5
-1
80
85
SOUTHBAY-DEL
2
1
0
-1
-2
80
85

Southern San Andreas Strain Rates

$\dot{\gamma}_1$	$\dot{\gamma}_2$	$\dot{\Delta}$
0.31	-0.04	-0.07
±0.02	±0.03	±0.02
μrad/yr	μrad/yr	ppm/yr

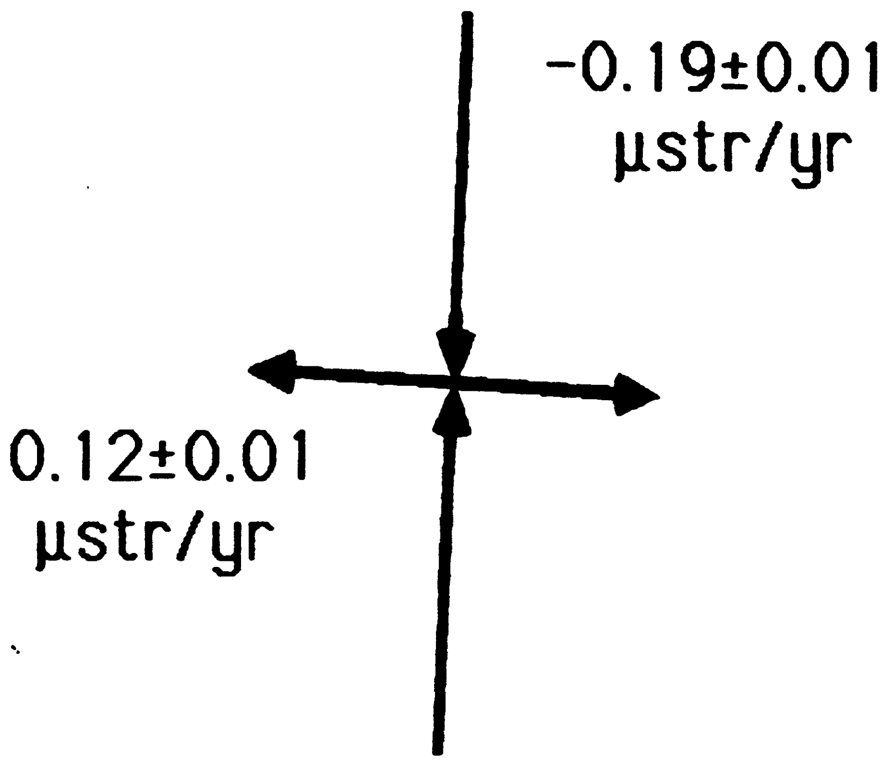


Figure 11

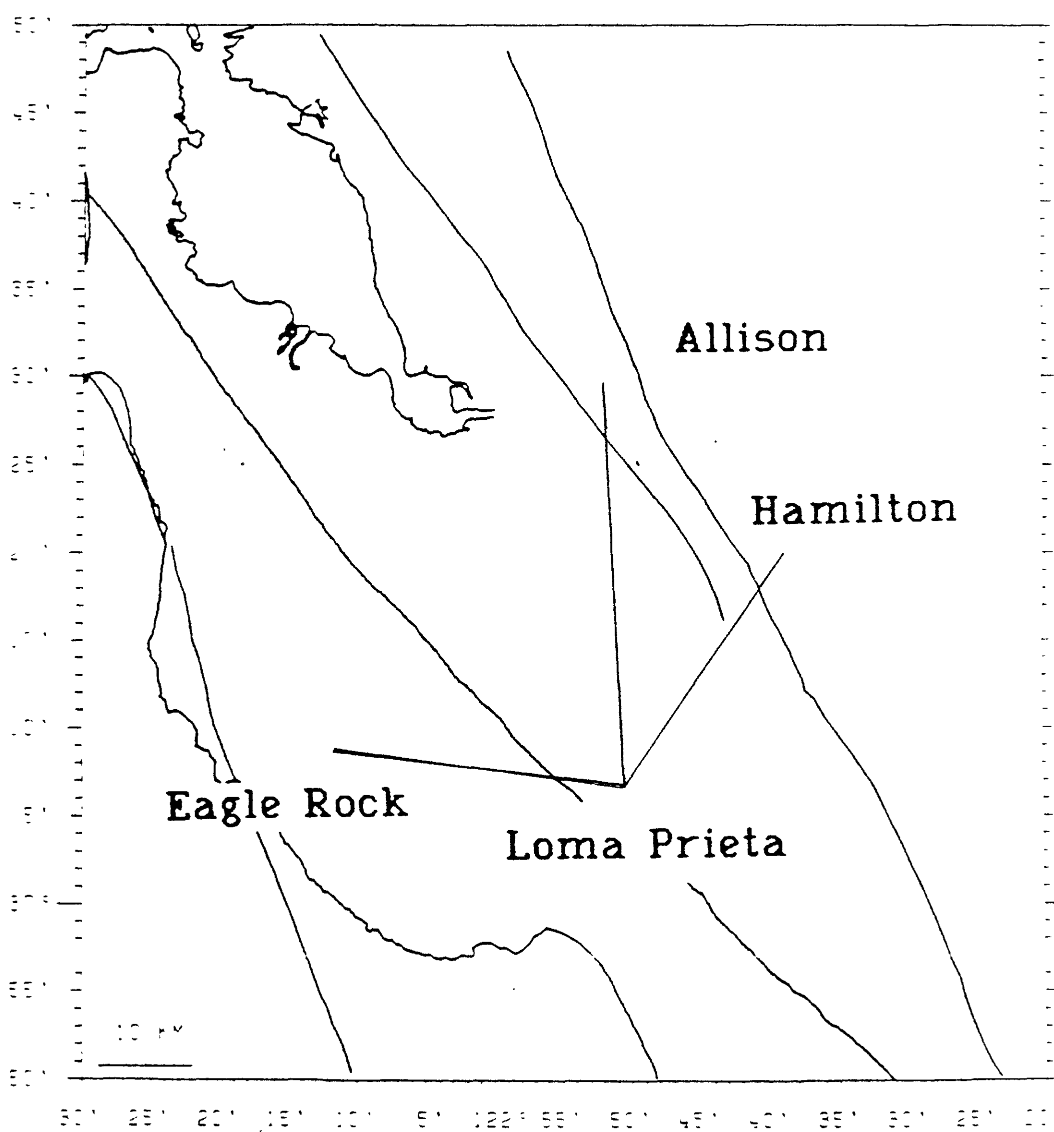


Figure 12

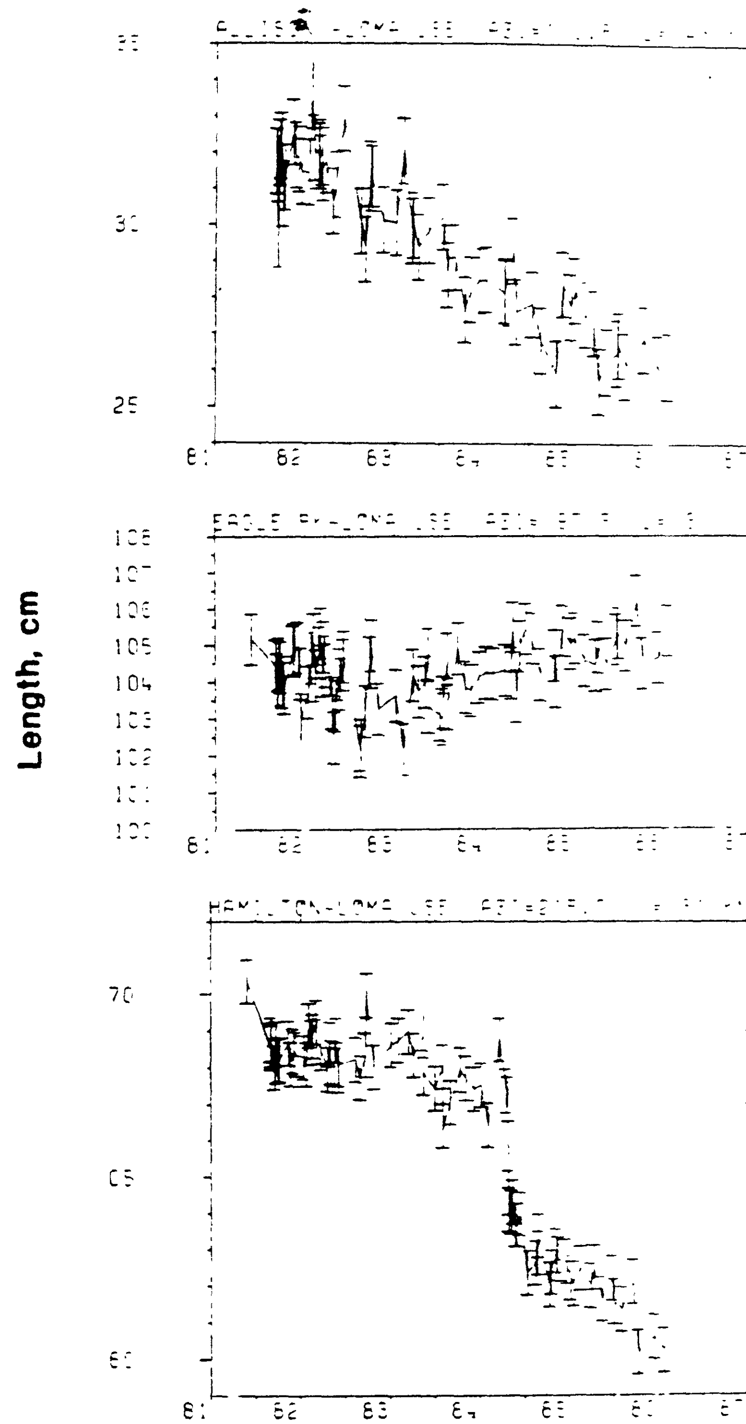
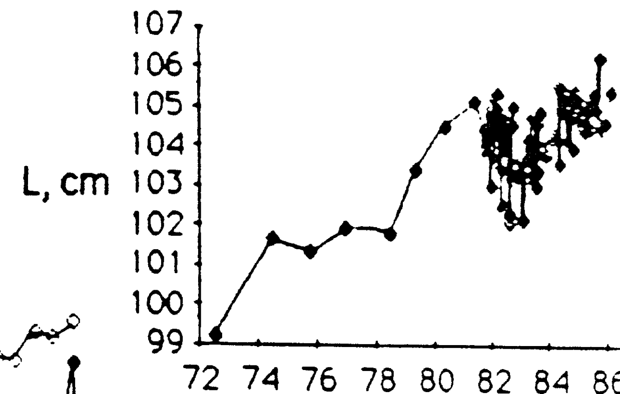


Figure 13

Eagle Rock



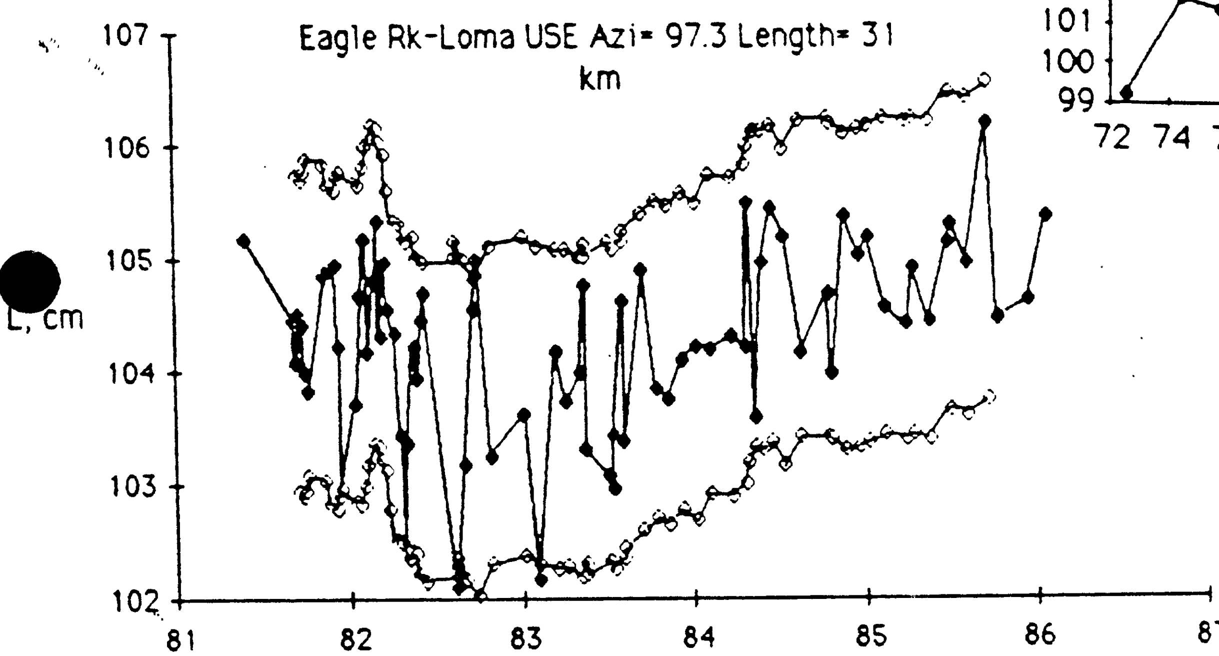


Figure 14

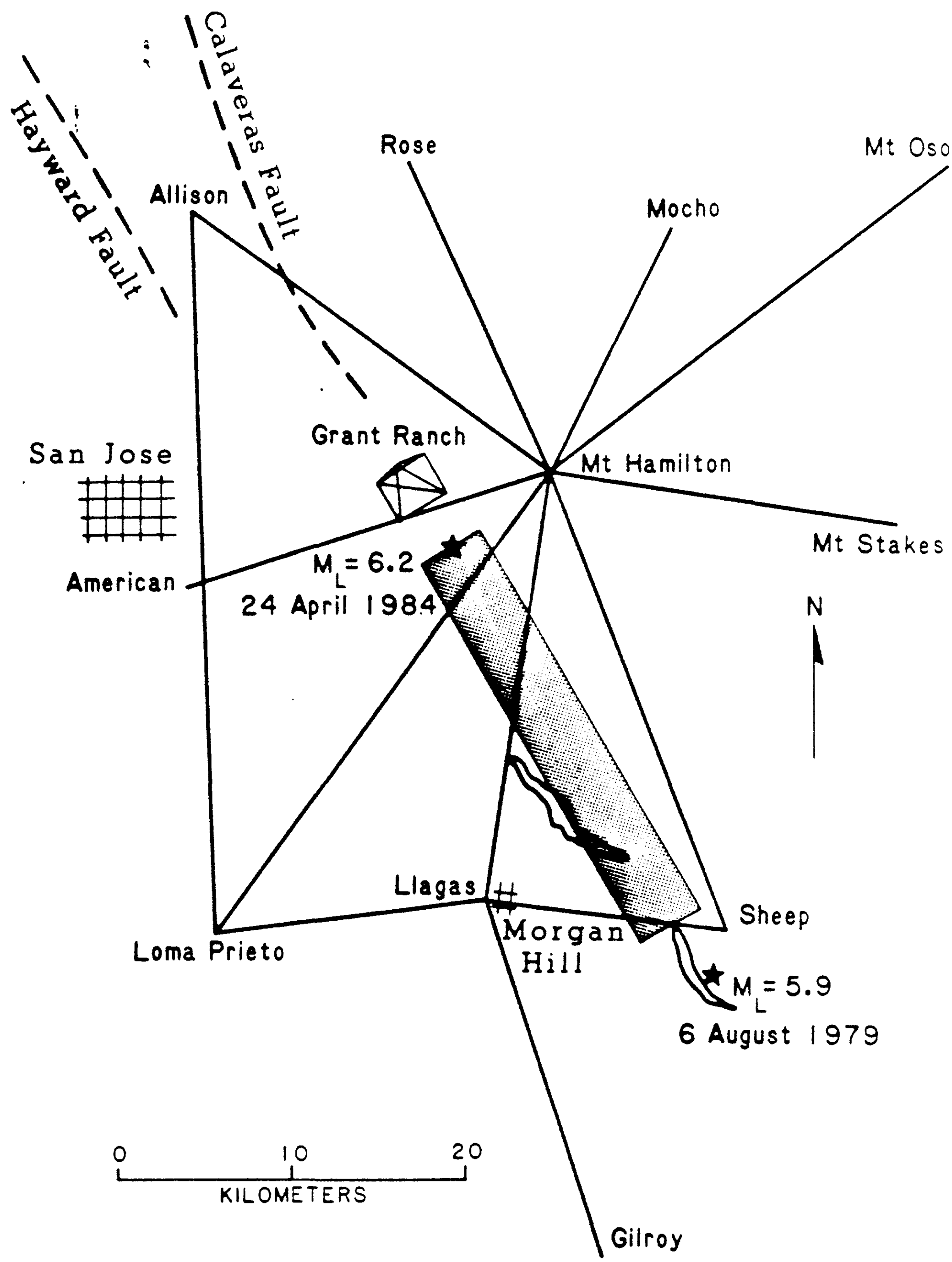
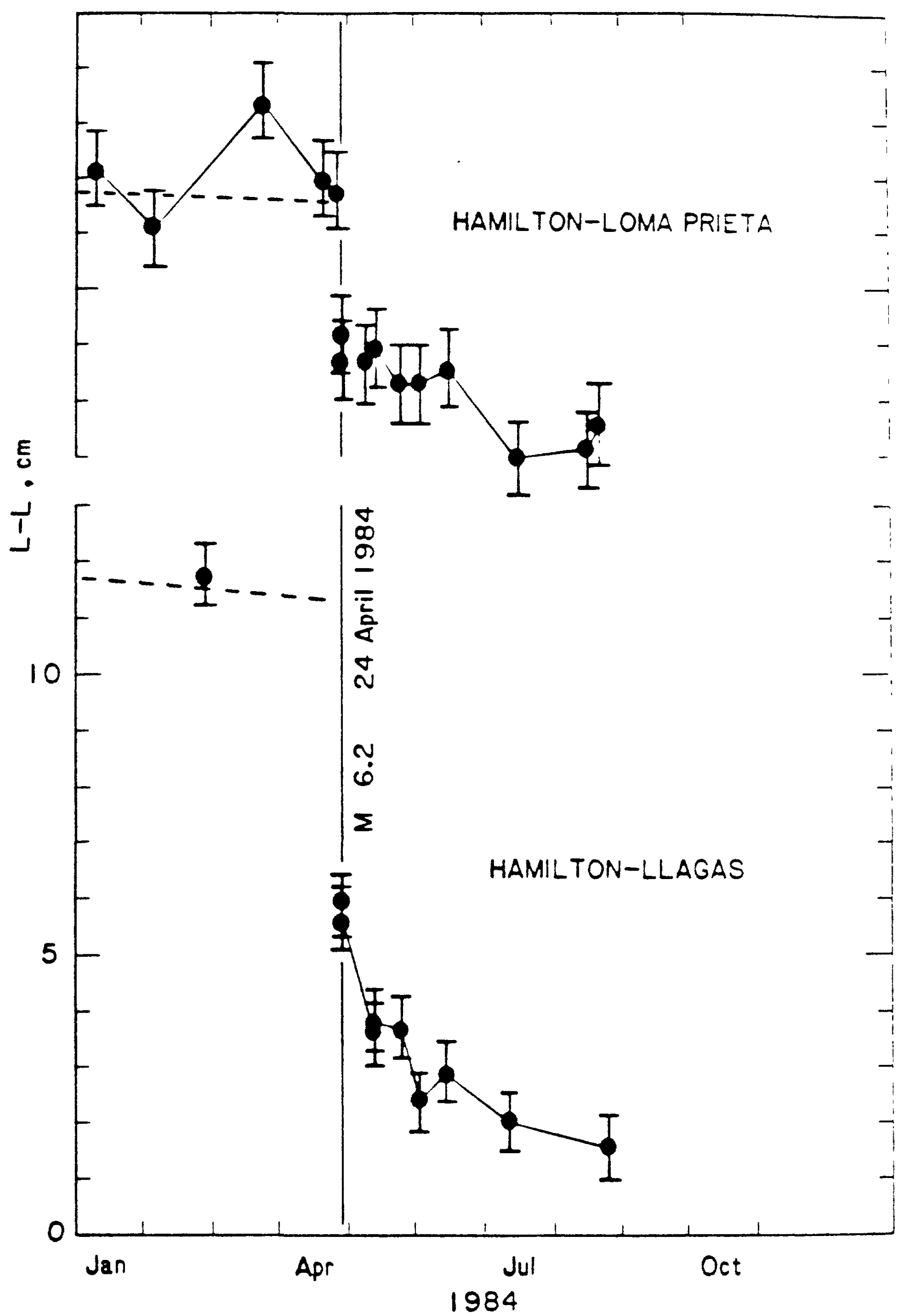

Hayward Fault
Calaveras Fault
Rose
Mt Oso
Mocho
Allison
Grant Ranch
San Jose
Mt Hamilton
Mt Stakes
American
$M_L = 6.2$
24 April 1984
N
Llagas
Sheep
Morgan Hill
Loma Prieto
$M_L = 5.9$
6 August 1979
0
10
20
KILOMETERS
Gilroy

Figure 15

Figure 16


HAMILTON–LOMA PRIETA
HAMILTON–LLAGAS
L-L , cm
M 6.2 24 April 1984
10
5
0
Jan
Apr
Jul
Oct
1984

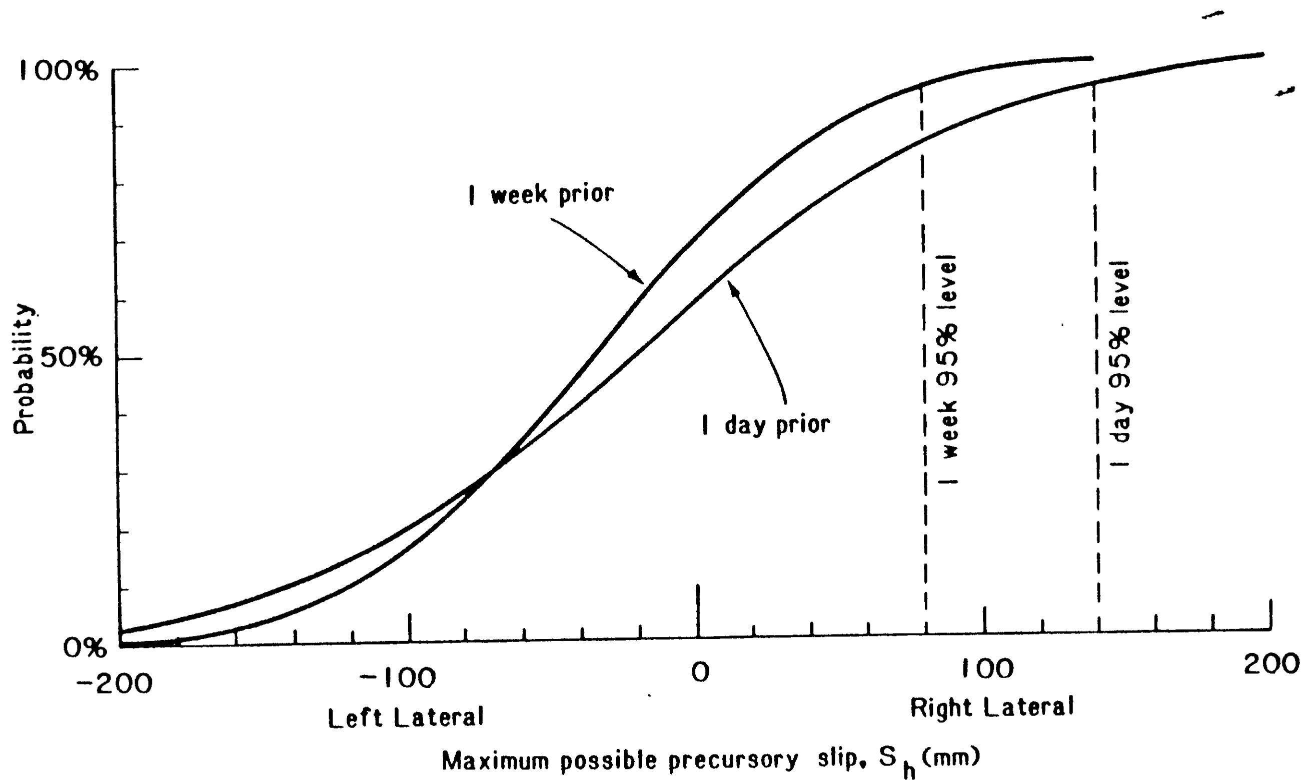

100%
50%
0%
Probability
I week prior
I day prior
I week 95% level
I day 95% level
-200
-100
0
100
200
Left Lateral
Right Lateral
Maximum possible precursory slip, S_h (mm)

Figure 17

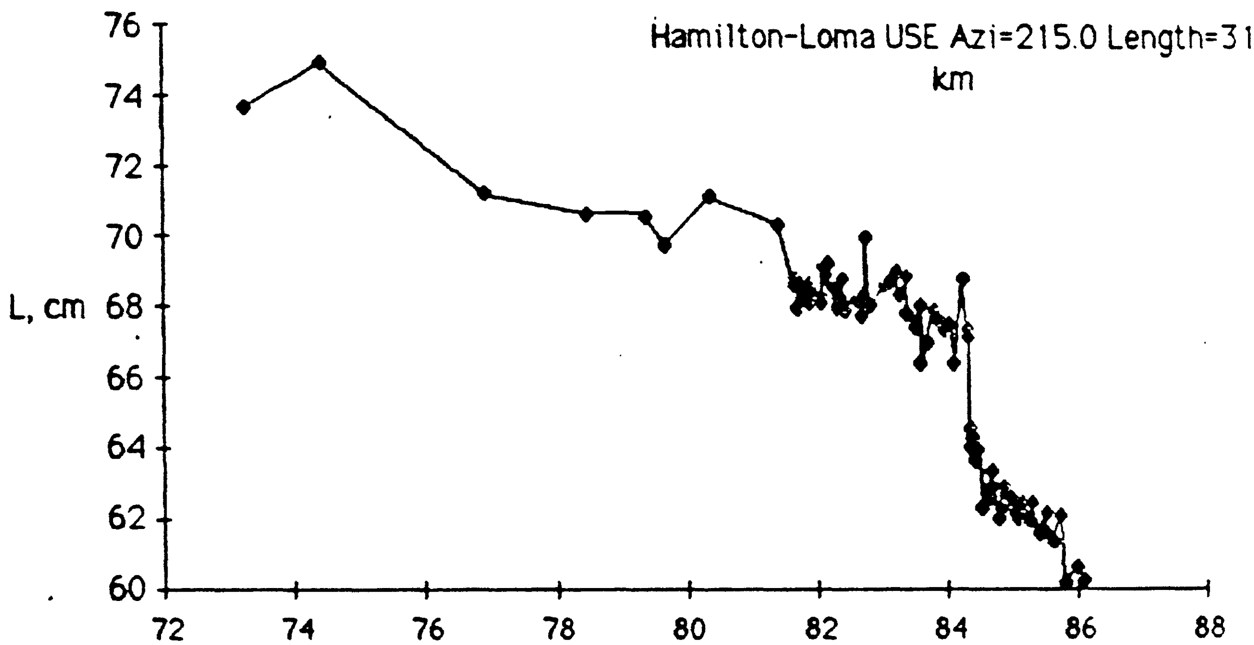

Hamilton-Loma USE Azi=215.0 Length=31 km
L, cm
76
74
72
70
68
66
64
62
60
72
74
76
78
80
82
84
86
88

Figure 18

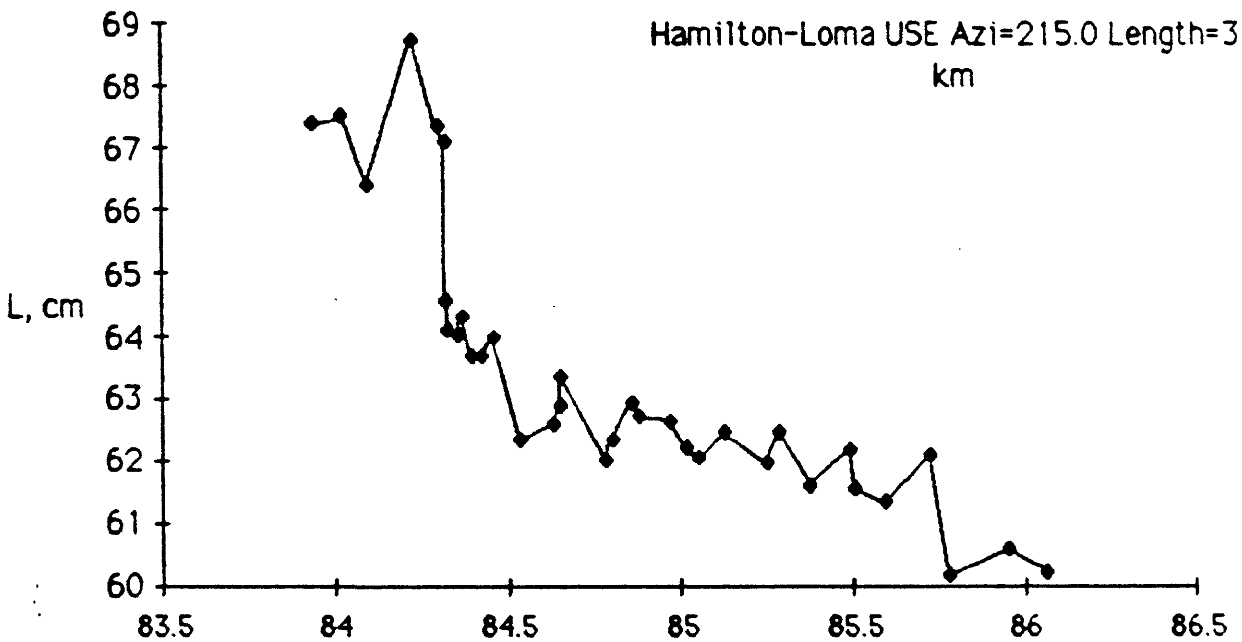

Hamilton-Loma USE Azi=215.0 Length=31 km
L, cm
69
68
67
66
65
64
63
62
61
60
83.5
84
84.5
85
85.5
86
86.5

Figure 19

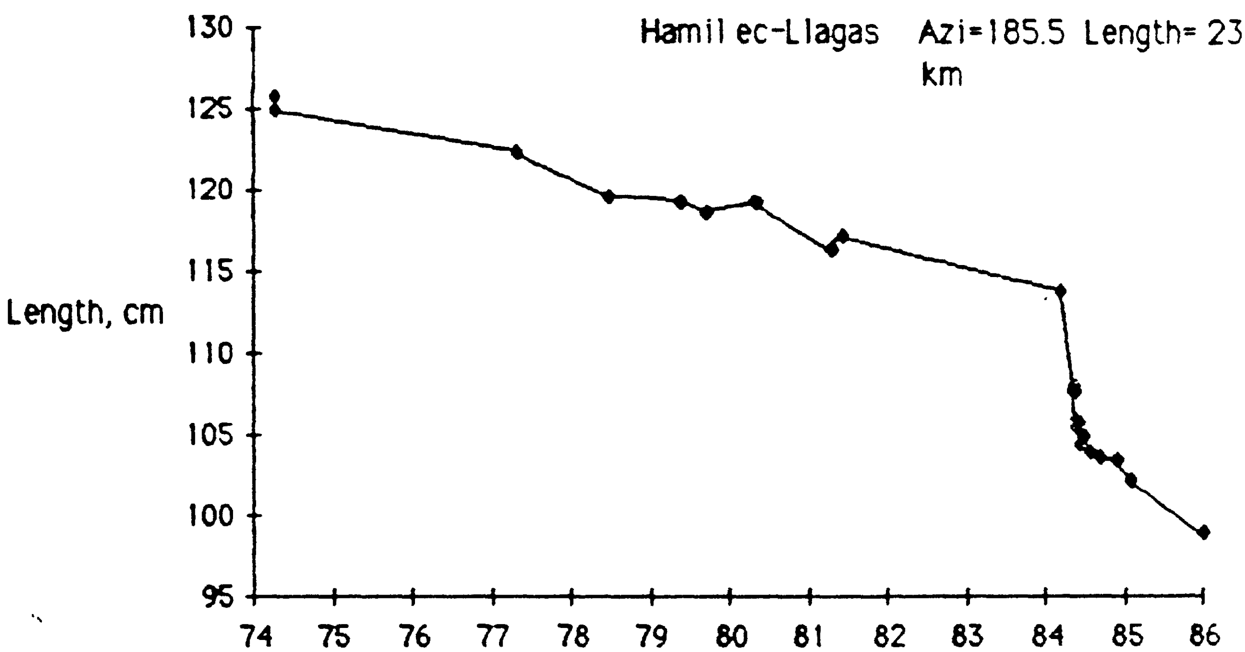

Hamil ec-Llagas Azi=185.5 Length= 23 km
Length, cm
130
125
120
115
110
105
100
95
74
75
76
77
78
79
80
81
82
83
84
85
86

Figure 20

Hamilton-LLagas

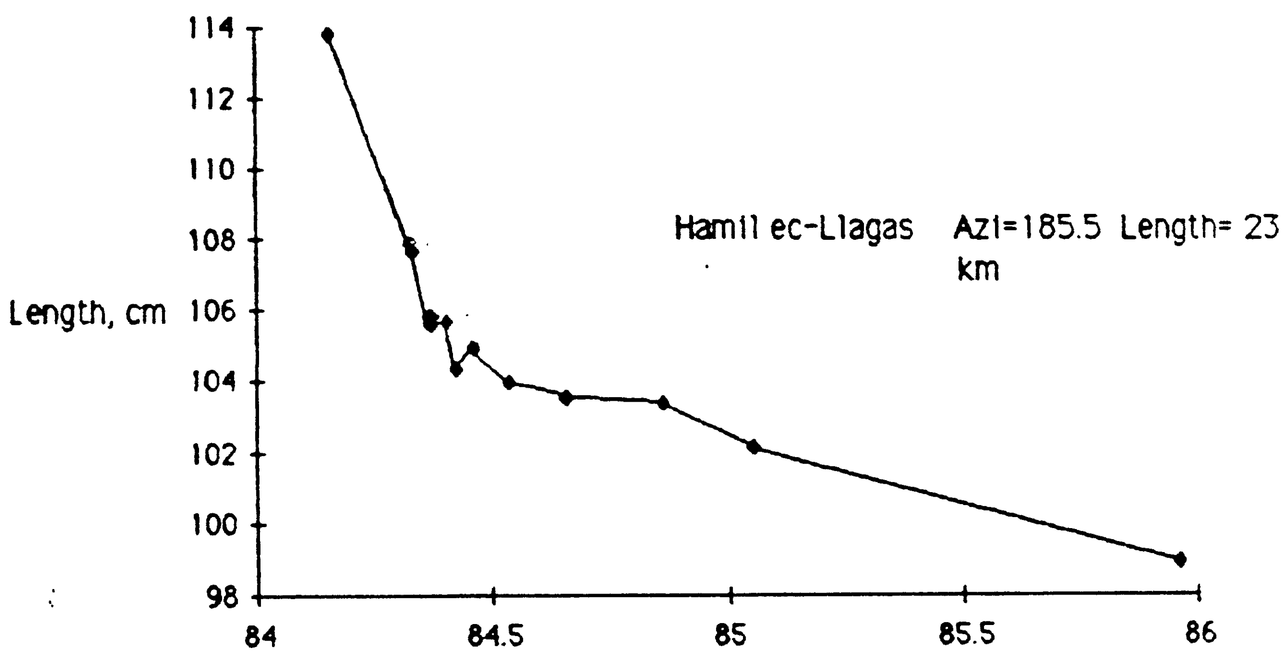


Figure 21

Coseismic Slip

Model	Slip	Sigma	Sum
2-8 km	240	± 22 mm	56
2-10 km	219	± 20 mm	52
2-12 km	207	± 19 mm	48
4-8 km	500	± 45 mm	47
4-10 km	408	± 36 mm	43
4-12 km	365	± 32 mm	40
6-8 km	1335	± 118 mm	41
6-10 km	818	± 71 mm	37
6-12 km	645	± 56 mm	35

Figure 22

Post Seismic Slip

Model	Slip	Sigma	Sum
2-8 km	324	± 22 mm	68
2-10 km	294	± 20 mm	61
2-12 km	278	± 19 mm	56
4-8 km	674	± 45 mm	54
4-10 km	549	± 36 mm	47
4-12 km	489	± 32 mm	43
6-8 km	1794	± 118 mm	43
6-10 km	1096	± 71 mm	39
6-12 km	866	± 56 mm	36
6-14 km	752	± 48 mm	36
6-16 km	683	± 44 mm	37
6-18 km	638	± 41 mm	38

Figure 23

Model	Slip	Sigma	Moment
Coseismic Slip			
4-10 km	408	± 36 mm	1.8×10^{25} dyne-cm
6-12 km	645	± 56 mm	2.9×10^{25} dyne-cm
Post Seismic Slip			
4-10 km	549	± 36 mm	2.5×10^{25} dyne-cm
6-12 km	866	± 56 mm	3.9×10^{25} dyne-cm
Total Slip	1.0 - 1.5 meters		$4.3\text{-}6.8 \times 10^{25}$ dyne-cm

Figure 24

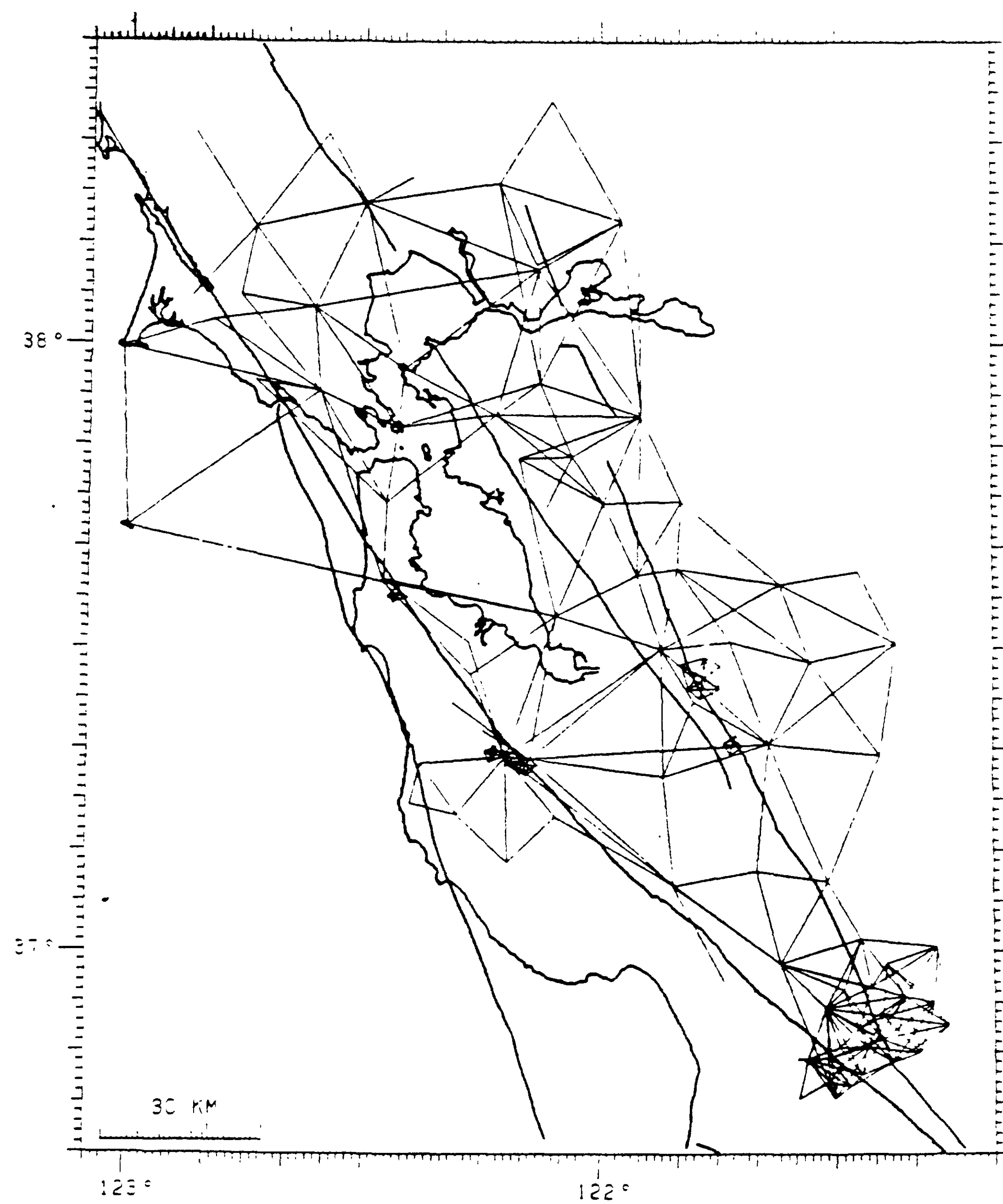


Figure 25

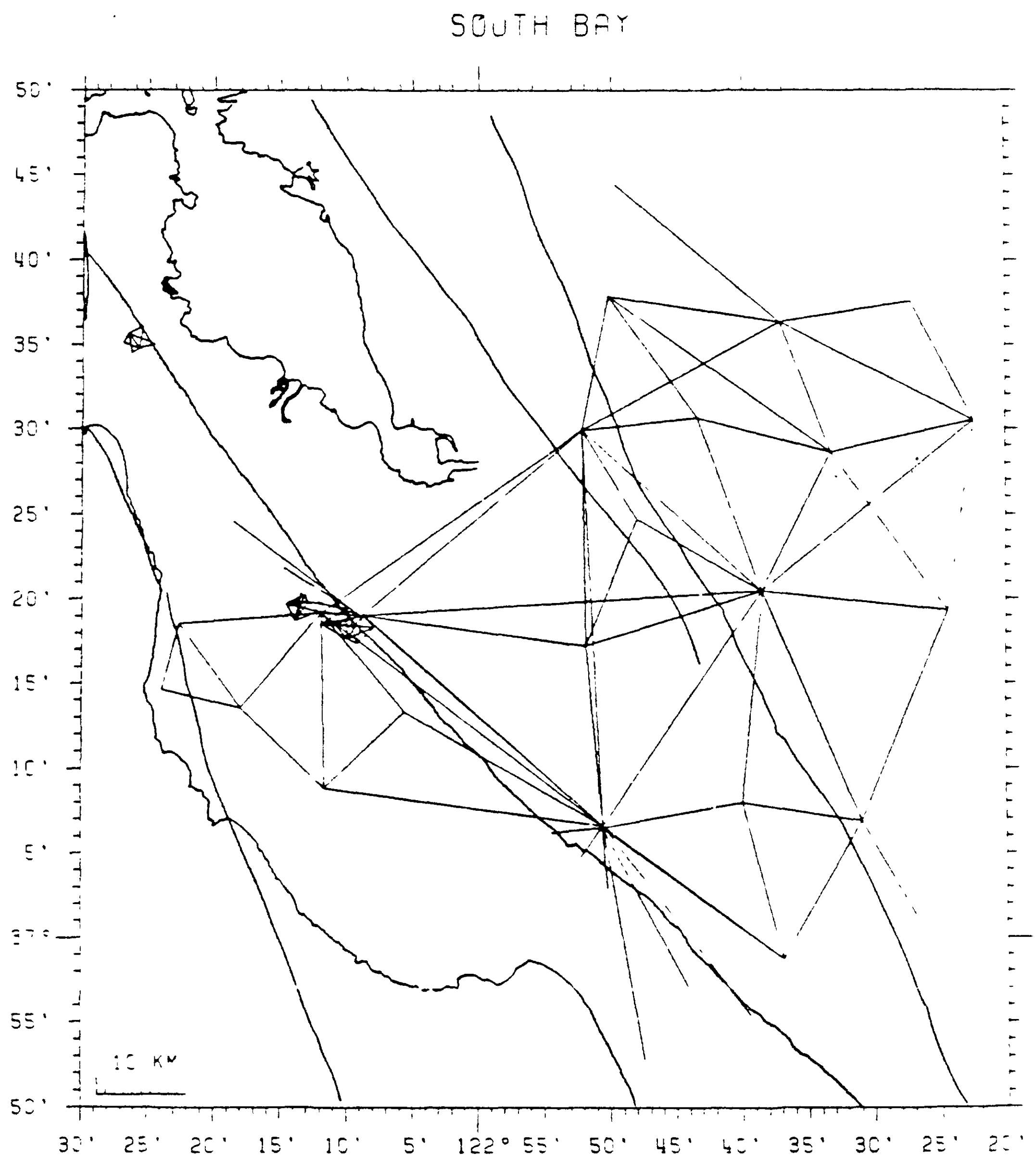
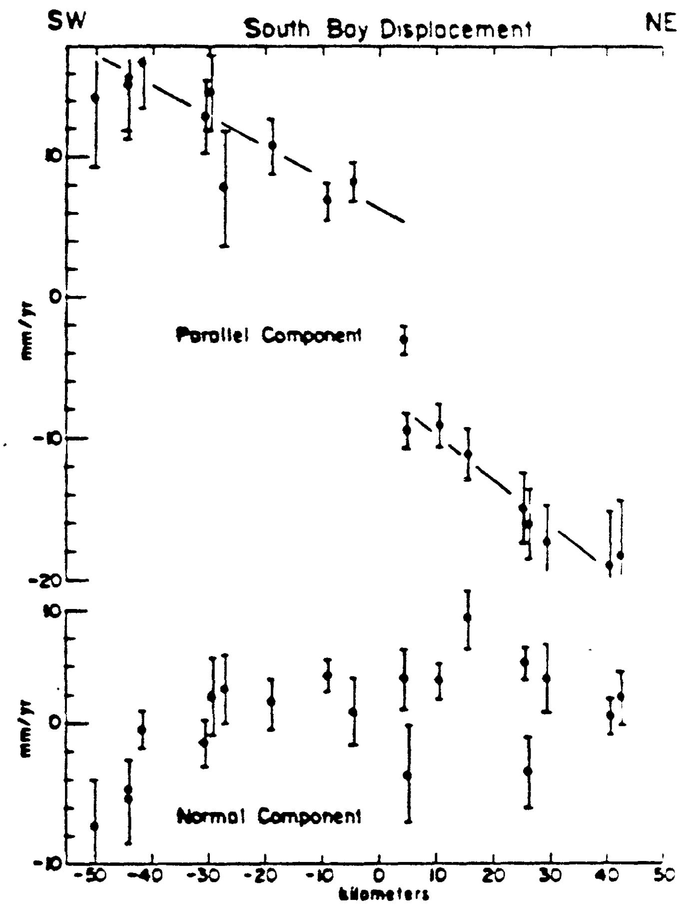


Figure 26

Figure 27



Profile of components of the velocity vector for stations of the network in Fig. 2 At top is the component of motion parallel to the faults; at bottom is the component of motion perpendicular to the faults. Both components are plotted as a function of distance along a normal to the fault plane (with arbitrary origin on both axes).

Grant Ranch	Slip Rate (mm/yr)
Eastern Trace	6.4 ± 0.3
Western Trace	3.0 ± 0.3

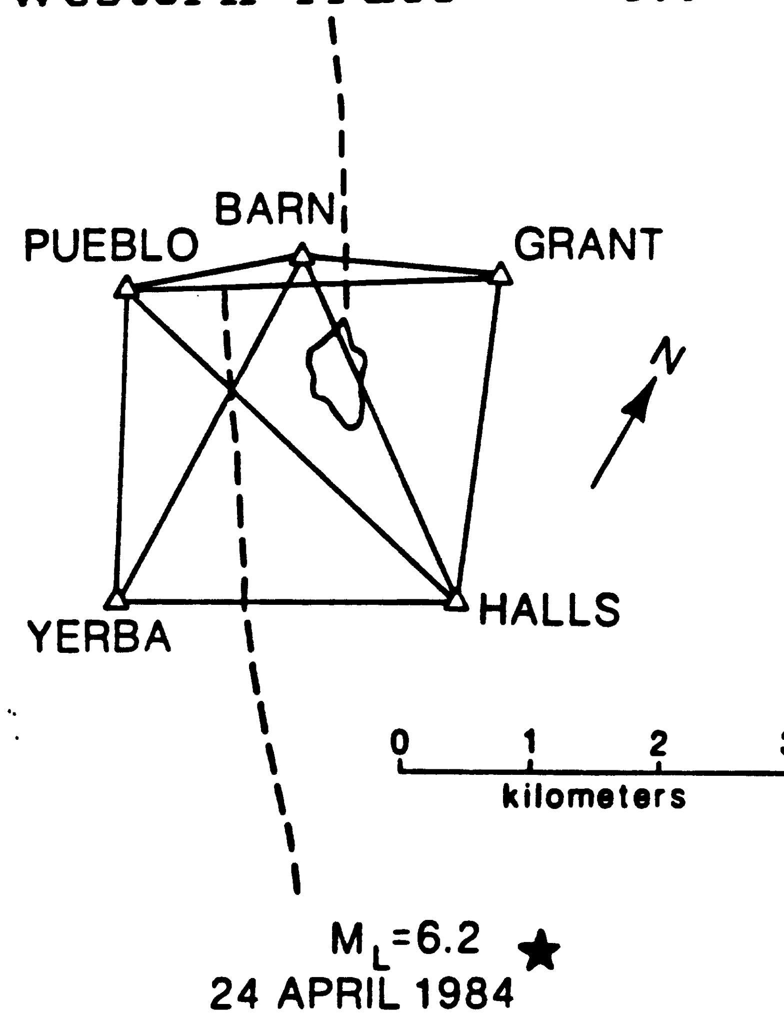


Figure 28

Figure 29

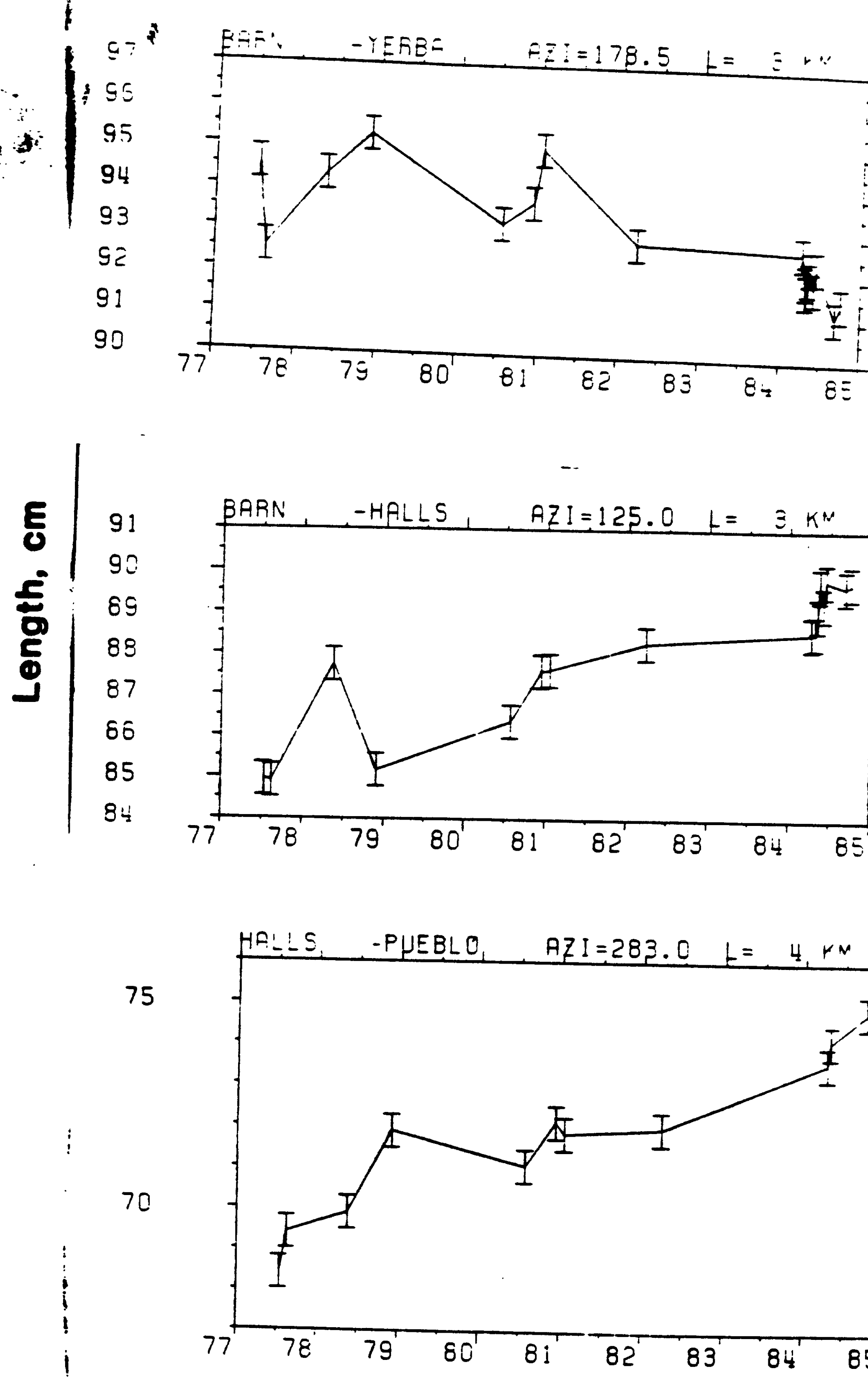

Length, cm
BARN -YERBA AZI=178.5 L= 3 KM
97
96
95
94
93
92
91
90
77 78 79 80 81 82 83 84 85
BARN -HALLS AZI=125.0 L= 3 KM
91
90
89
88
87
86
85
84
77 78 79 80 81 82 83 84 85
HALLS -PUEBLO AZI=283.0 L= 4 KM
75
70
77 78 79 80 81 82 83 84 85

Figure 30

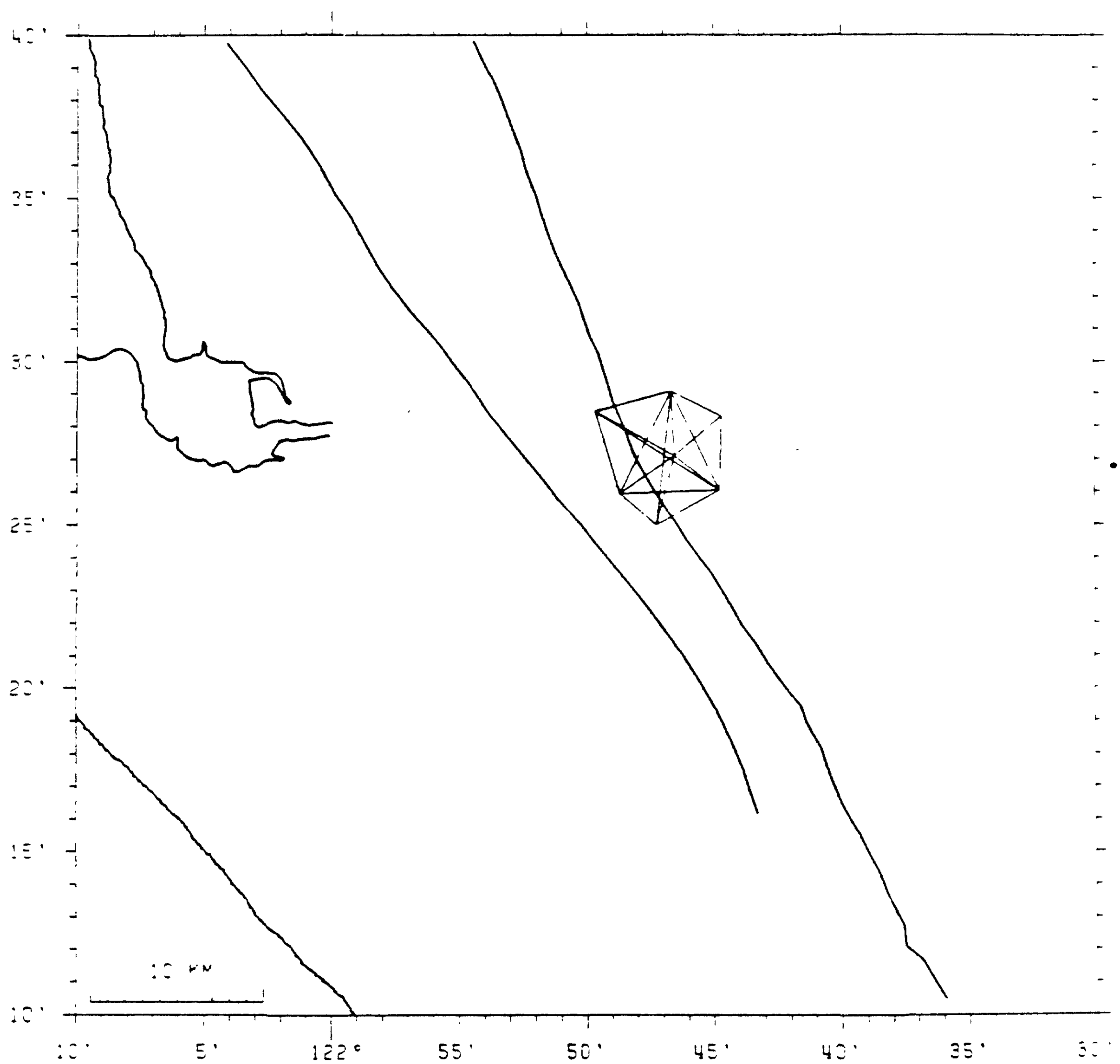
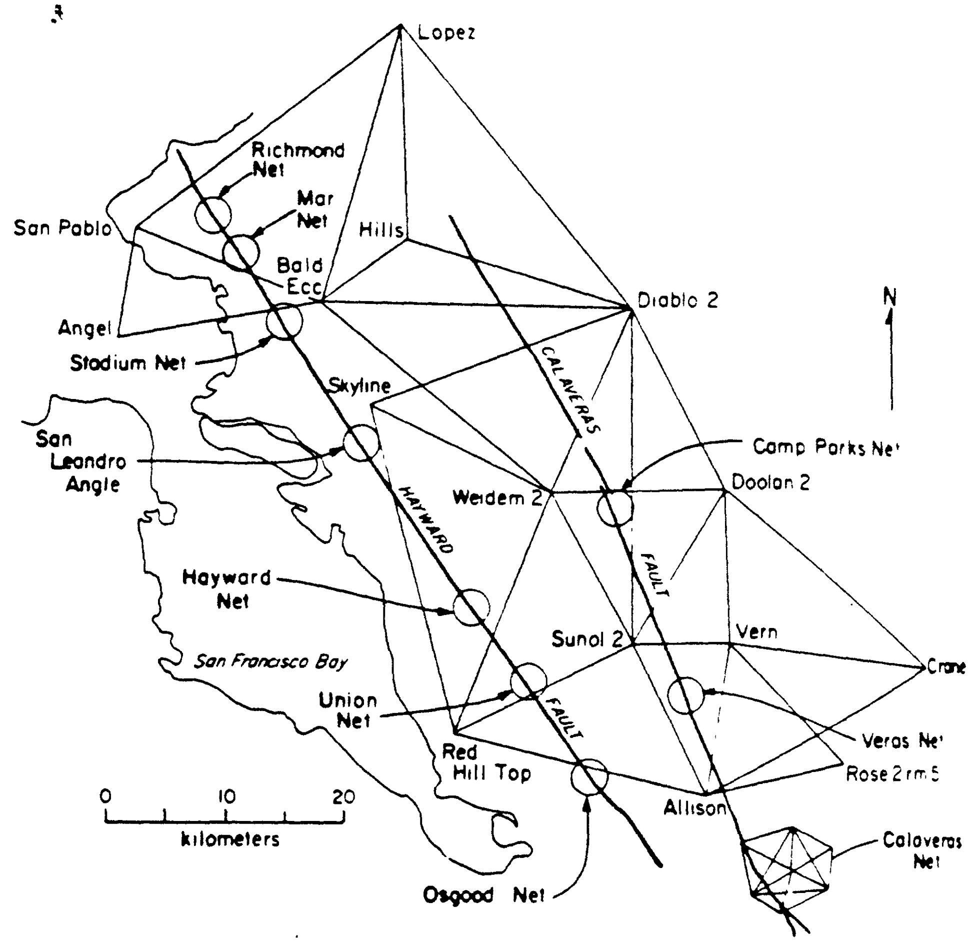


Figure 31

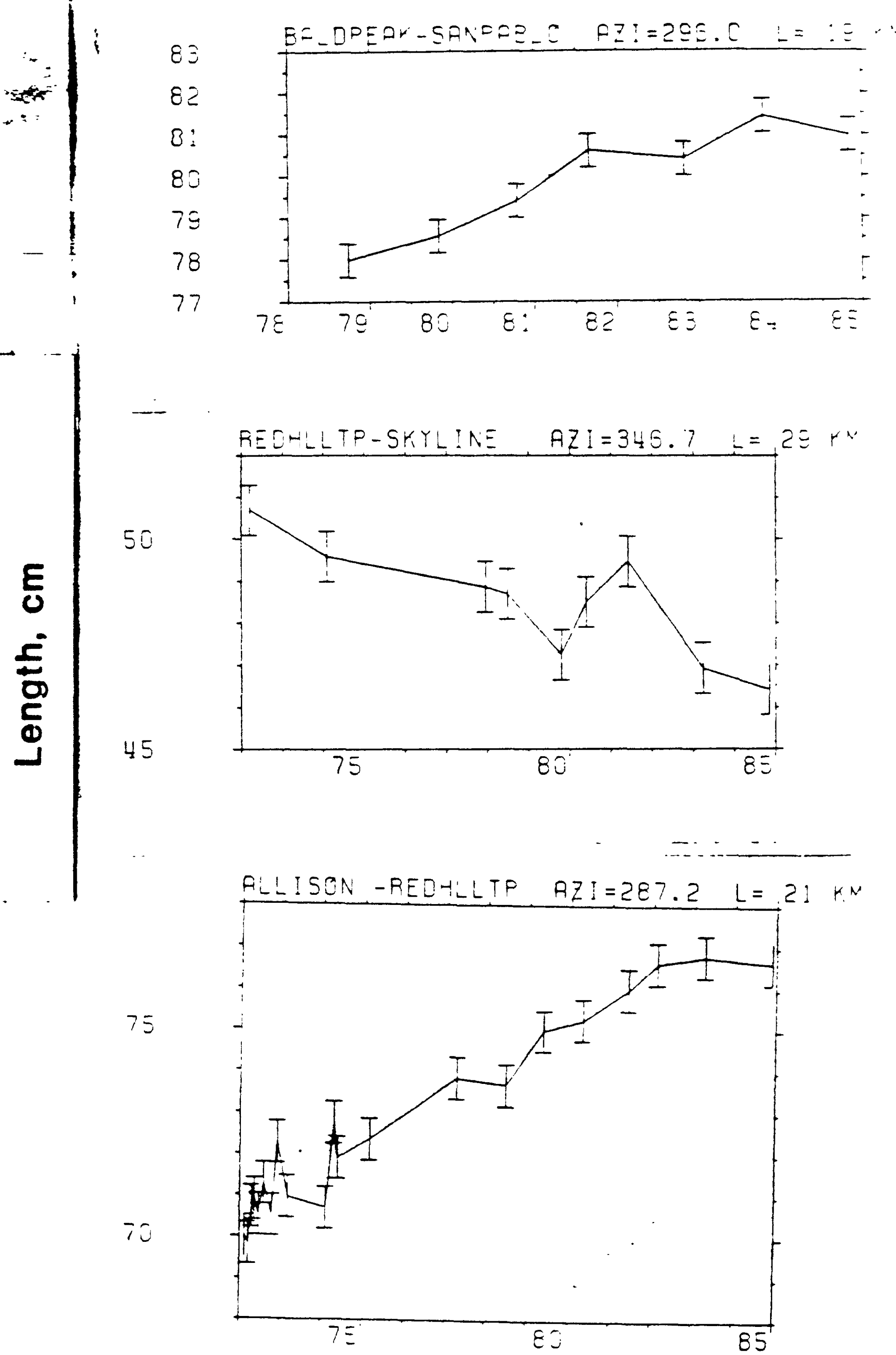


Map of the area east of San Francisco Bay. Straight lines indicate observed distances. Circles indicate networks of a number of short lines or, in one case, an angle.

Observations of some of these nets were first made during the 1960's by the U.S. Coast and Geodetic Survey (Parkin, 1965; Miller, 1966, 1967, 1968, 1970a, b, c, d; Mosier, 1977). Additional networks were installed by the U.S. Geological Survey during the 1970's and all of the networks have been reobserved recently by the U.S. Geological Survey. The second data set consists of observations of 32 geodolite lines ranging in length from 8 to 33 km. These lines (Figure 1) have been measured about once a year by the U.S. Geological Survey.

The precision of the short lines was discussed by Lisowski and Prescott (1981) and the precision of the longer lines by Savage and Prescott (1973).

Figure 32


Length, cm
REDHLLTP-SKYLINE AZI=346.7
ALLISON -REDHLLTP AZI=287.2 L= 21
83
82
81
80
79
78
77
78 79 80 81 82 83 85
50
45
75 80 85
75
70
75 80 85

Hayward Fault	Slip Rate (mm/yr)
Red Hill-Allison	7.9 ± 0.4
Red Hill-Skyline	3.0 ± 0.7
Bald-San Pablo	6.7 ± 1.2
Chabot 3-Seneca	2.4 ± 4.9
Merritt-Seneca	12.8 ± 4.8
Strain net	10.0 ± 0.3

Figure 33

HAYWARD FAULT
LONG TERM AVERAGE FOR EACH FEATURE

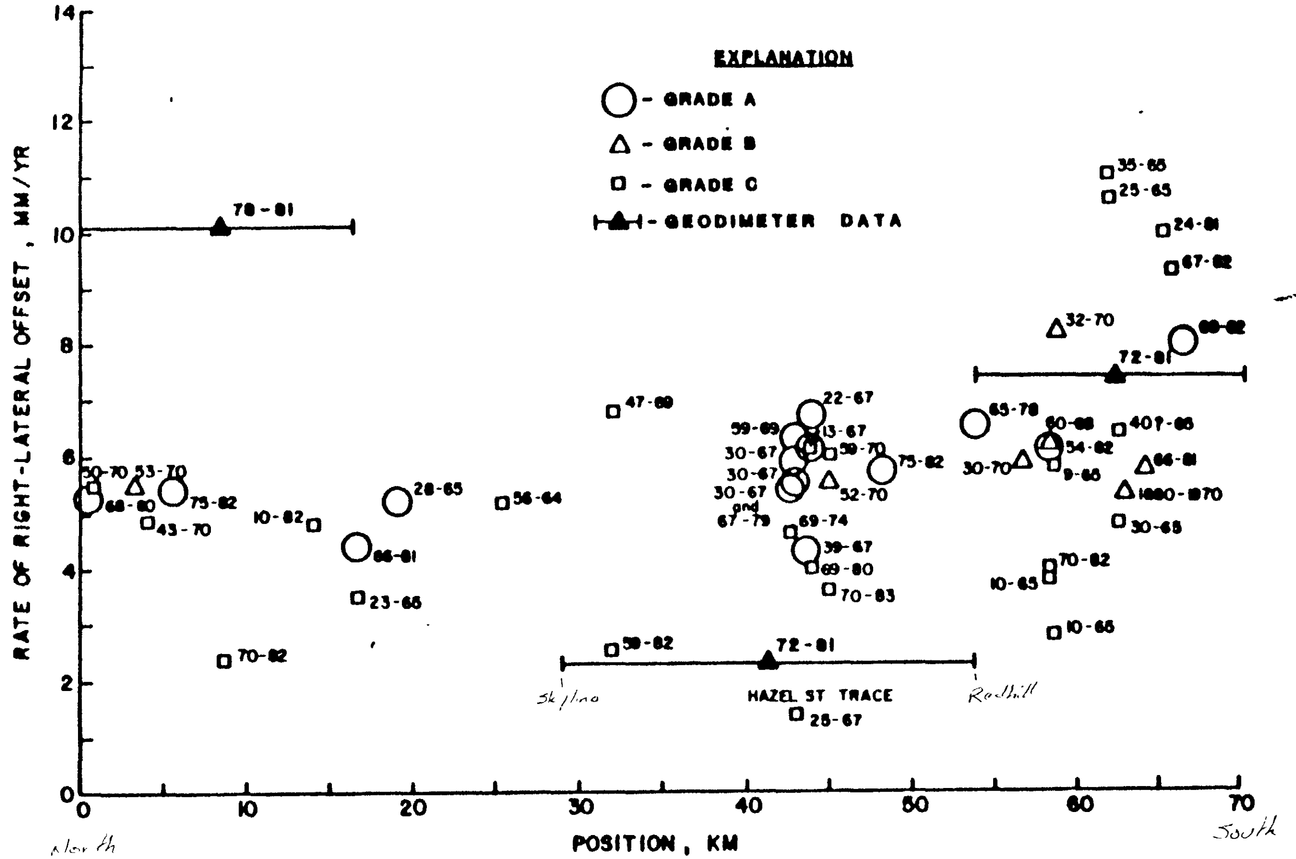


Figure 34

HAYWARD FAULT
RATE vs. POSITION – GRADE A

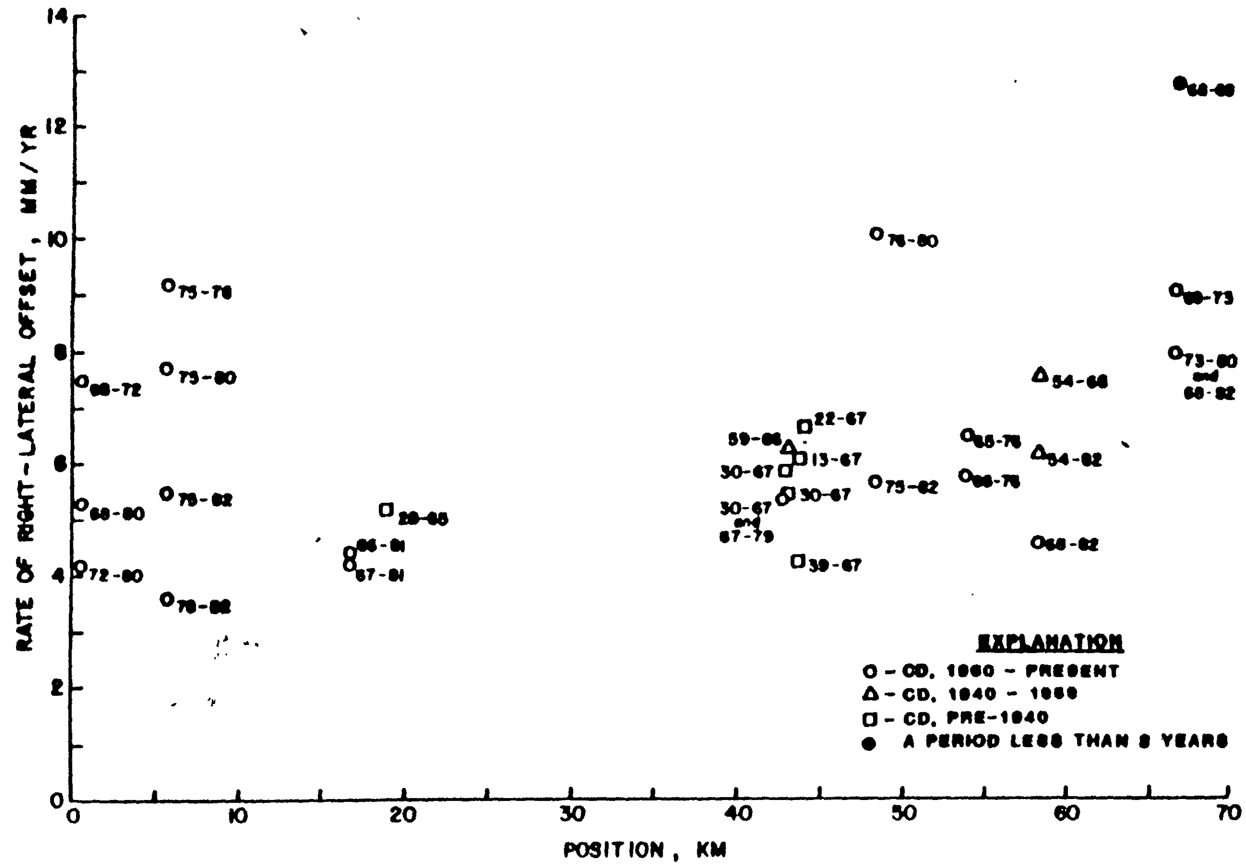


Figure 35

27 February 1986

Seismicity of the San Francisco Bay Area

Allan G. Lindh, Jean Olson, Rob Cockerham

To facilitate discussion, we have divided the plate-boundary faults in the Bay Area into characteristic segments (Figure 3) on the basis of:

1. Historic macroseismicity. Historic events of approximate M6 and larger have been "assigned" segments, wherever the data permitted. (See dates and magnitudes adjacent to each segment in Figure 3.)

2. Variations in slip along strike of the 1906 earthquake. The differing interpretations of Lindh (1983), Sykes and Nishenko (1984), and Scholz (1985) for the region north of San Juan Bautista have been taken into account and are represented by a dashed segment near Black Mtn.

3. Discontinuities along strike in the character of the microseismicity, fault structure, and in a small way, geology and crustal structure.

We wish to emphasize that this is a very subjective reading of the tectonics and seismicity of the Bay area. We believe it is an interpretation permitted by the data, and is about as plausible as most of the alternatives. It should be considered as a hypothesis at this point, one which should be judged by it's usefullness in designing future experiments.

The historic seismicity of the Bay area is dominated by the M8 San Francisco earthquake and it's accompanying seismic cycle (Tocher, 1958). This was described most recently by Ellsworth et al (1981), reprints of which are available. See also figures 1-3 of this handout.

The microseismicity of the last 15 years, during which the U.S.G.S. has operated a dense seismic network in the region, is dominated by microseismicity along the three main branches of the plate boundary in this area; the San Andreas, the Calaveras, and the Hayward faults (Figure 4). This activity is described in detail in recent publications by Olson (198?), Ellsworth et al (198?), and Bakun et al (198?), reprints of which are available.

A number of studies have recently treated the recurrence of large earthquakes as a quasi-periodic, rather than Poisson process (Lindh, 1983; Sykes and Nishenko, 1984; Nishenko and ??, 1985). In the special case of Parkfield, where the historic record clearly identifies the characteristic segment and documents the recurrence, the same formalism has been applied to a magnitude 6 event. The difficulty with applying the technique more generally to M6 events is that it is not clear whether the concept of characteristic events is applicable; segmentation of the faults may not be stationary in time, for instance. In addition it is unclear whether the assumption of independent segments makes sense for earthquakes with dimensions as small as 20-30 km. Parkfield is exceptional in this regard; with a completely locked zone on one side and a continuously creeping one on the other, it may well enjoy relatively steady-state boundary conditions during the long intervals between great 1857 style earthquakes.

Since 1979, three earthquakes of approximate M6 have occurred in the Bay area, the first of this size since 1911. One occurred on the Grenville Fault east of Livermore, the other two on the Calaveras Fault between Hollister and San Jose. These latter two occurred on what we have called the Coyote and San Felipe segments of the Calaveras on adjacent 20-25km segments (Figure 5b). These two events suggest that in the San Francisco Bay area some events of M6 may also be amenable to treatment as quasi-periodic phenomenon. These events appear to represent recurrence of similar events that occurred in 1897 and 1911, respectively, suggesting that an approximately 80 year reccurrence interval applies to the southern Calaveras fault (Figures 3). In light of these events, and the fact that we may now be entering Stage II of the seismic cycle accompanying great 1906 type events on the San Andreas, (and thus might reasonably expect more M6-7 events in the coming decades,) we have attempted to identify those segments most likely to fail in the next 30 years.

Because one must have slip rates to assign reccurrence intervals, I have assigned slip rates to the major fault branches. They are listed below, uncertainies follow each value in parentheses.

San Andreas north of San Juan:	17 (5) mm/yr.
Calaveras south of Calaveras Resivoir:	17 (5) mm/yr.
Calaveras north " " "	7 (5) mm/yr.
Hayward/Mission	10 (5) mm/yr.

The constraints I have applied in reaching these figures are:

1. The geodetic data of Prescott et al (see Prescott's summary, this workshop), and conversations with Will Prescott on the subject.

2. The fact that the San Andreas has been the site in historic time of a M8 earthquake, and has a geomorphic and physiographic expression consistent with a major active strike-slip fault. Based on conversations with some of the geologists familiar with the Bay area, it is my judgement that it is unreasonable to assign less than 50% of the plate-boundary motion to the San Andreas, and that data which suggest otherwise may have larger error bars than is apparent at this time.

3. That slip-rates on major parallel strands will add up to 34 mm/yr. I follow Prescott et al (1985) in effectively assigning negligible slip to the San Gregorio fault.

I realize that this assignment of rates will offend almost everyone, but it seems to me (AGL) -- on balence -- the best guess possible at this time. The uncertainity of 5 mm/yr for all rates has been assigned arbitrarily; it is large enough to bring the assigned rate to within one standard error of most estimates, and to within two S.E. of all estimates. Note that while the Hayward fault is assigned a rate of 10 mm/yr, the recurrence calculations use a value of 5 (5) mm/yr, based on the evidence of Prescott et al (1982??) that aseismic slip accounts for some major portion of the slip budget on the Hayward/Mission fault, at least on the southern half, which I have called here the Mission segment.

In Table 1 we have listed conditional probabilities for all the Bay area segments discussed above for periods of 1, 10, 20, and 30 years from the present. A word of explanation seems in order concerning the Coefficient of Variation used in the calculations for each reccurrence interval.

A time dependent probability estimate for the recurrence of a given earthquake on a given segment depends on three numbers:

1. The time of the last event T0,
2. The mean recurrence interval T, and
3. The variance Var(T).

For some segments estimates of T and T0 are possible, but estimates of Var(T) are usually little more than guesses. As a practical matter in dealing with recurrence on a given segment the total variance can be broken into two parts:

$$Var(T) = Var(E) + Var(I),$$

where Var(E) corresponds to the part of the variance due to the Earth, that is the real intrinsic variation in interevent times due to the vartiations in strain rate, interactions with other faults, etc, and Var(I) represents the portion of the total variance due to our "lack of information" about the Earth.

As a practical matter we know almost nothing about Var(E) except that the coefficient of variation (CV, defined as SQRT(Var(T))/T) must be less than about 0.5 for great plate boundary earthquakes, or there would more cases of earthquakes repeating on the same segment with very short inter-event times; we know of no such cases amoung what must be of the order of 100 relatively well-documented large earthquakes world-wide in modern times.

Moreover we cannot learn very much about Var(E) while Var(I) is as large as it is, although the Parkfield Prediction Experiment can be considered at its most fundemental level as a test of the hypothesis that CV for the Parkfield segment is of the order 0.1 or less. In most cases it is Var(I) that dominates the problem, and will for the foreseeable future. We can take practical steps to reduce Var(I) however.

Var(I) can be broken down into component pieces,

$$Var(I) = Var(u) + Var(s) + Var(Seg)$$

where Var(u) corresponds to the uncertainity in the slip in the last event, Var(s) corresponds to the uncertainity in the strain accumulation rate, and Var(Seg) corresponds to the uncertainity that results from the ambiguity in picking the segments. (Of course if one is estimating the recurrence time directly as Sieh has at Pallett Creek, then Var(r) is substituted for Var(u) + Var(s).)

If one examines the probabilities in Table 1 he finds that the numbers are much smaller than those previously published in Lindh (1983) or Sykes and Nishenko (1984). This is because the CV are larger in every case than the uniform value of 0.3 used in those earlier studies. In the case of three segments (Loma Prieta, Black Mtn, and Crystal Springs) I calculated the CV's from estimates of the uncertainity in the slip in 1906, and in the subsequent strain accumulation rate, using the formalism outlined above. In other cases CV was simply set at 0.5 in recognition of the gross uncertainities in all the relevant parameters.

[From my perspective the value of approaching the conditional probability problem in this way is that it eliminates the need to quote ranges of conditional probability values.]

Another feature of Table 1 worthy of note is that most of the conditional probability estimates do not differ significantly from the unconditional Poisson estimate of 1/T. This is primarily the consequence of the high CV values (see Figure 7). Stated more bluntly this implies that with all of our work and effort and analysis we have not improved on the Poisson estimate, nor will we untill Var(I) is made smaller.

Which brings me to my final point, which is that in my humble opinion, the question of how to sight a detailed prediction experiment in the Bay area is inappropriate, putting as it does the Cart well in advance of the Horse. The very small probabilities listed in Table 1 do not, in my opinion, provide rational grounds for focusing our efforts on any one segment; the uncertainities due to the large Var(I) terms swamp whatever signal we might imagine we can see. [The only possible exceptions to this bleak conclusion are the Loma Prieta and Alum Rock segments, which I hope will be discussed at length in this meeting, and which I will not discuss here.]

This is something we can change, however. The uncertainities on the slip rates and recurrence intervals, and maybe even the segments and the characteristic earthquakes, can be reduced by hard work. The diligent application of geologic mapping and very dense geodetic networks can reduce the Var(I) term on a given segment. However, I believe the lesson in Table 1 is that unless these efforts are focused on one or two segments, they will not help; our current very diffuse efforts do not seem to provide enough detail to get us beyond the Poisson condition.

Which brings me to my very final point, which is that in my opinion one segment should be the focus of an intensive effort, not in an effort to predict and earthquake, but rather in an effort to reduce the Var(I) term. In light of the enormous societal impact a large earthquake on the Hayward Fault would have, I believe our efforts should be focused there over the coming decade. What I have called the Berkeley segment, the northern half of the Hayward Fault, may well consitutue one of, if not THE greatest, seismic hazard in California, running as it does directly through a heavily built up urban area and presumably capable of an event up to M7. I do not believe the portion of the Hayward Fault north of San Leandro failed in 1868, but may have been the site of a very poorly documented M6.5-7 event in 1836. The 200 year recurrence interval in Table 1 reflects the presumed 1 m slip in 1836 divided by 5 mm/yr, the assumed strain accumulation rate. Prescott (198?) found about 1 cm/yr of displacement across the Hayward Fault, but concluded that most of this displacement was occurring as aseismic creep, and that little strain accumulation was taking place.

In Table 2 I have constructed two hypothetical sets of probabilities corresponding to improved information on the Hayward Fault. The point I wish to make is that relatively modest improvements in what we understand could radically change our assesment of the prospects of a large Hayward earthquake in the coming decades.

TABLE 1
Conditional Probabilties
(Uncertainities follow in parentheses)

Segment	Last Earthquake Date	Mag	Slip	Slip Rate	Reccur Int(yr)	CV	Probabilities (yrs) 1	10	20	30
Loma Prieta	1906	8	1.75(.75)	17(5)	103(52)	.5	.0104	.108	.220	.335
Black Mtn	1906	8	2.5 (.50)	17(5)	147(51)	.35	.0037	.041	.093	.154
Crystal Spr	1906	8	4.0 (1.0)	17(5)	235(89)	.38	.0010	.011	.025	.041
Coyote	1979	5.9	0.5??	17(5)	80 (24)	.3	0	0	0	0
San Felipe	1984	6.1	1.0	17(5)	80 (24)	.3	0	0	0	0
Alum Rock	1903	5.8		17(5)	80 (40)	.5	.0211	.207	.399	.565
Sunol	1864	5.8		7(5)	190(95)	.5	.0043	.044	.091	.141
San Ramon	1861	5.6		7(5)	190(95)	.5	.0044	.046	.094	.145
Mission	1868	6.7	1.0	5(5)	200(100)	.5	.0036	.037	.078	.120
Berkeley	1836	6.7	1.0	5(5)	200(100)	.5	.0051	.052	.106	.162

TABLE 2
WHAT IF Experiment on Berkeley Segment
With various T and CV

				Recurr Int	CV	Probabilities (intervals in years) 1	10	20	30
Berkeley	1836	6.7	1.0	200(100)	.5	.0051	.052	.106	.162
				150(50)	.33	.016	.159	.311	.451
				100(33)	.33	.058	.468	.739	.882

Figure 1. Historic seismicity, 1850-present (from Ellsworth et al, 1981)

Figure 2. Space-time plot of the seismicity of California, 1850-present (from Moths and Ellsworth, unpub. man.).

Figure 3. Cartoon showing the three main faults of the S.F. Bay area showing one possible division into characteristic segments. Also listed beside each segment are earthquakes which either do, or may, represent characteristic earthquakes for those segments. The division into segments ranges from obvious to highly speculative; the association of specific earthquakes with segments is highly interpretive in some cases, and should be treated only as one possible interpretation.

Figure 4. Microseismicity of the Bay area.
 a. 1969-80
 b. 1981-85
 c. Detail of East Bay (From Ellsworth et al, 198?)

Figure 5. Longitudinal cross-sections in the plane of the major faults.

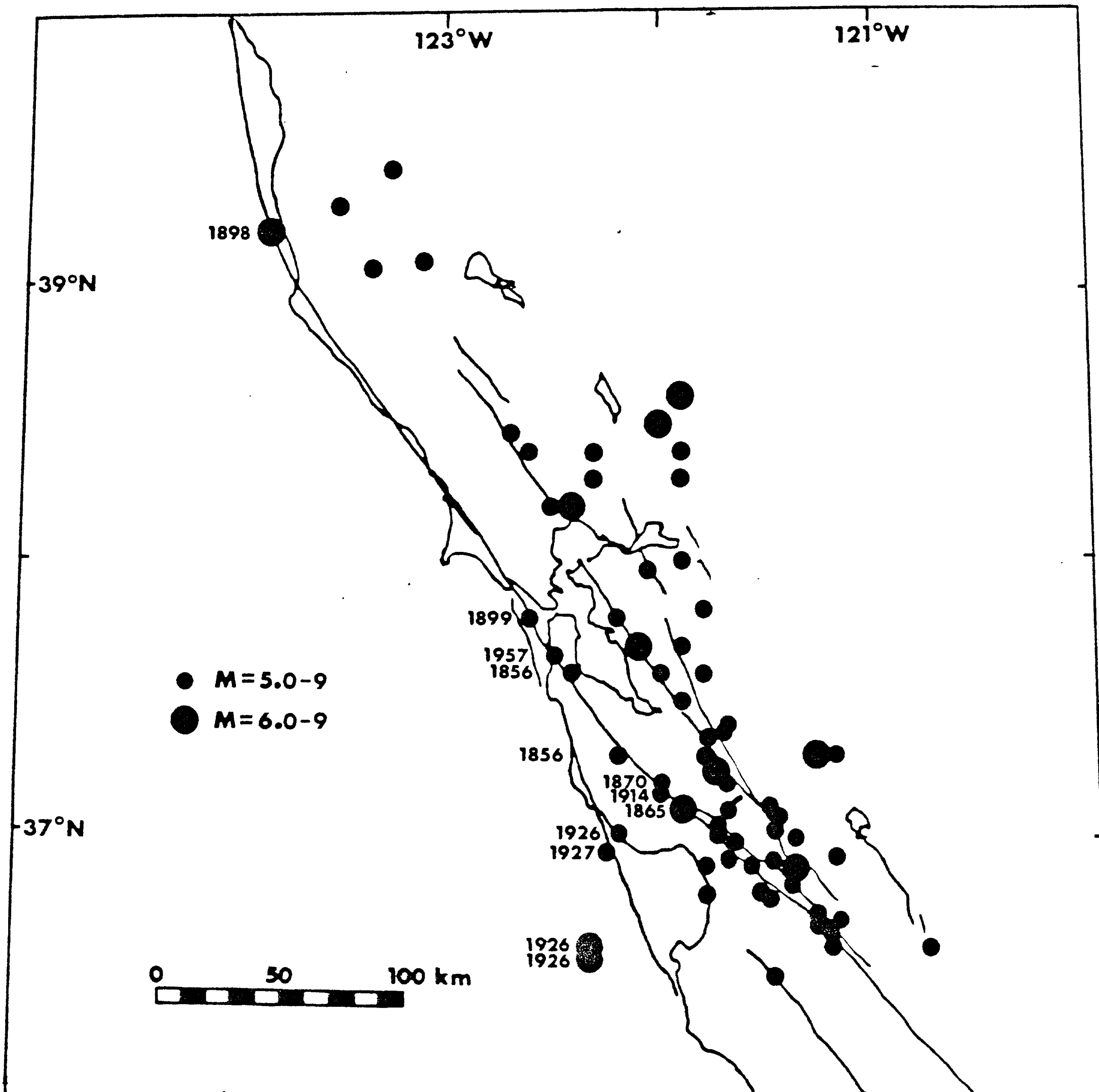
a. San Andreas Fault from San Juan Bautista to Daly City. The shallow events (around 5 km) near Black Mtn are predominantly east of the San Andreas on the SW dipping thrust faults of the Black Mtn Fault system. The dense shallow activity at the far right corresponds to the NW terminus of the creeping segment near San Juan Bautista. (from Olson, 198?).

b. Calaveras Fault from Concord to Hollister. The Hayward/Mission Fault system intersets the Calaveras at the point marked Mission fault (from Bakun et al, in press).

c. Hayward, Mission and northern Calaveras faults in East Bay (from Ellsworth, et al, 198?).

Figure 6. Focal mechanisms.
 a. San Francisco Pennisula (from Olson, 198?).
 b. East Bay (from Ellsworth et al, 198?).
 c. Detail of Hayward/Mission Fault system.

Figure 7. Conditional Probabilities illustrating earthquake reccurence for a reccurrence interval of 100 years. Curves are shown for four different values of the Coefficient of Variation (the ratio of the standard deviation to the mean), 0.1, 0.3, 0.5, and 1.0, which corresponds to the time-independent Poisson distribution.



123°W
121°W
39°N
37°N
1898
1899
1957
1856
1856
1870
1914
1865
1926
1927
1926
1926
M=5.0-9
M=6.0-9
0
50
100 km

Figure 1

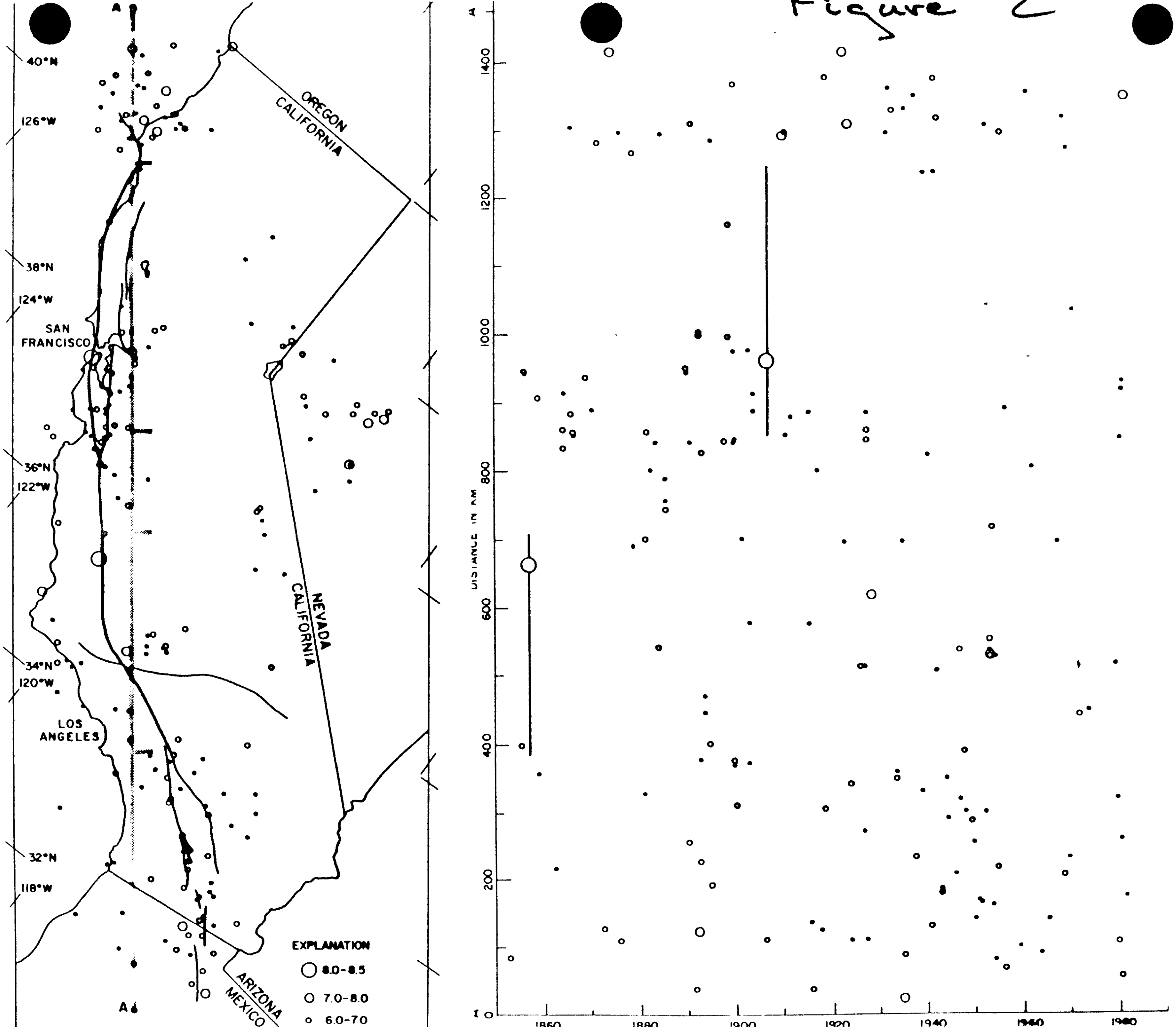
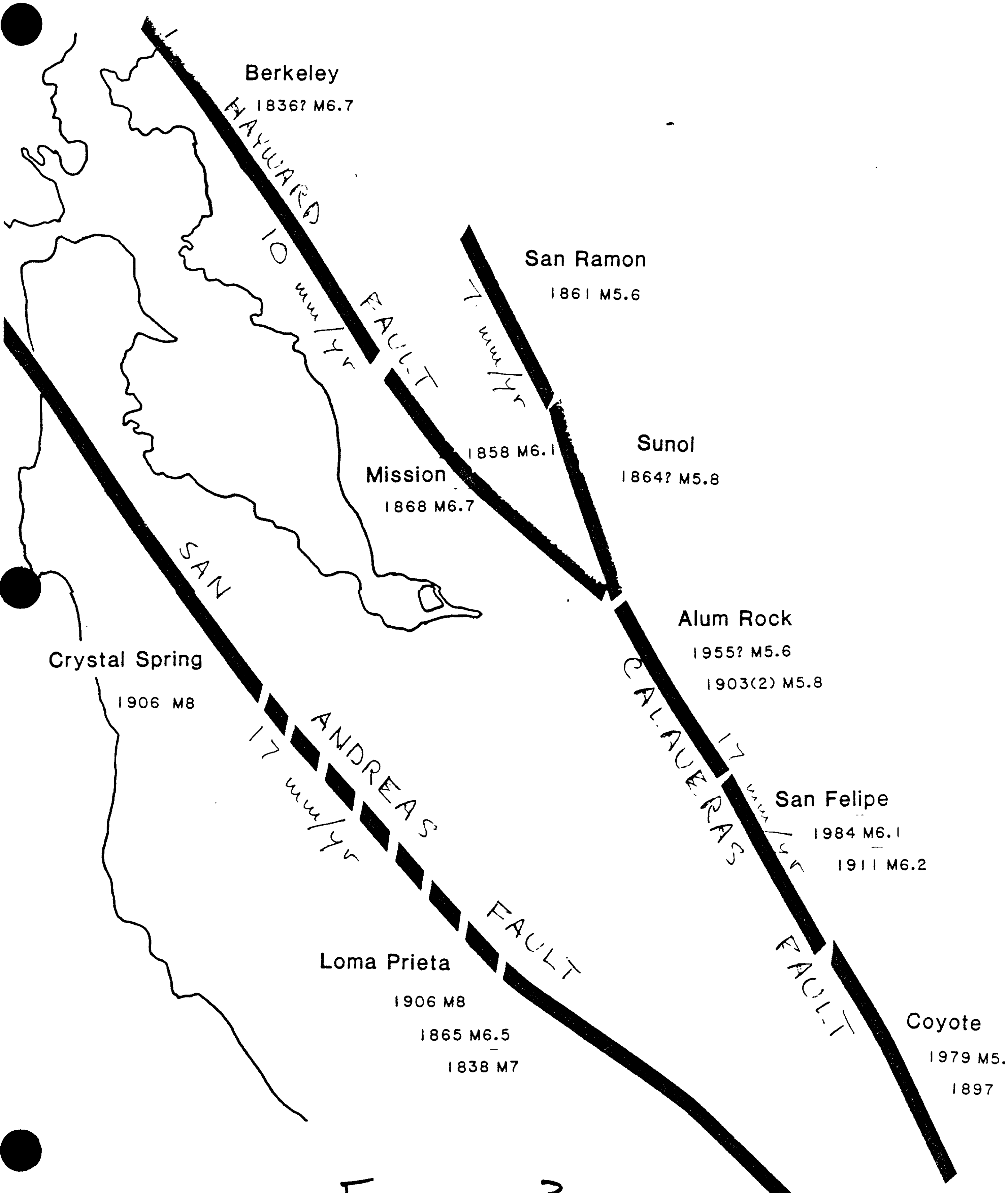

A
A'
40°N
126°W
38°N
124°W
SAN FRANCISCO
36°N
122°W
34°N
120°W
LOS ANGELES
32°N
118°W
OREGON
CALIFORNIA
NEVADA
CALIFORNIA
ARIZONA
MEXICO
EXPLANATION
8.0-8.5
7.0-8.0
6.0-7.0
DISTANCE IN KM
0
200
400
600
800
1000
1200
1400
1860
1880
1900
1920
1940
1960
1980

Figure 2



Berkeley
1836? M6.7
HAYWARD
10 mm/yr
FAULT
San Ramon
1861 M5.6
7 mm/yr
Sunol
1864? M5.8
1858 M6.1
Mission
1868 M6.7
SAN
Alum Rock
1955? M5.6
1903(2) M5.8
Crystal Spring
1906 M8
ANDREAS
17 mm/yr
CALAVERAS
17 mm/yr
San Felipe
1984 M6.1
1911 M6.2
FAULT
Loma Prieta
1906 M8
1865 M6.5
1838 M7
FAULT
Coyote
1979 M5.9
1897

Figure 3

SAN FRANCISCO BAY AREA SEISMICITY
1969 - 1980

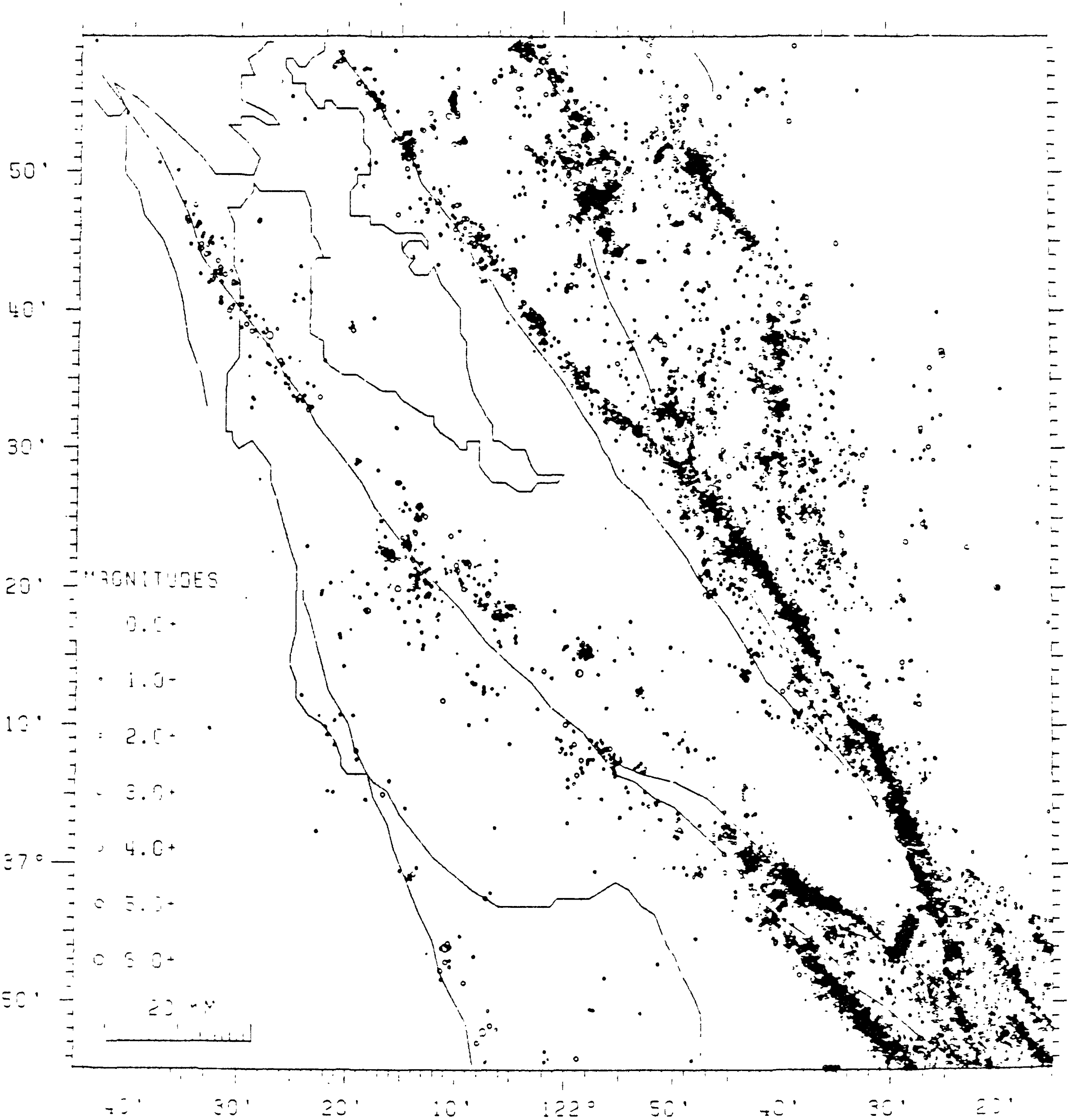


Figure 4a

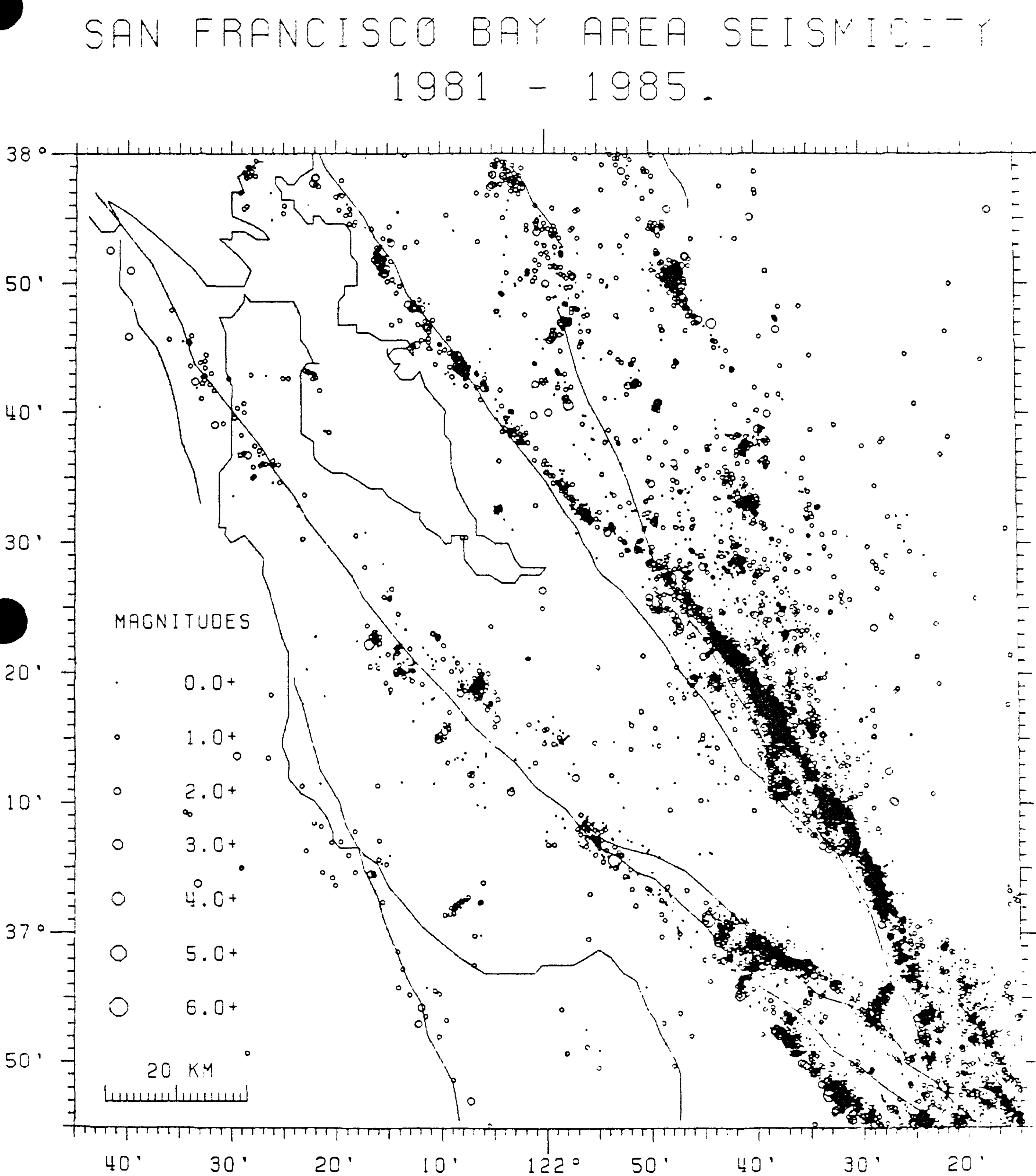
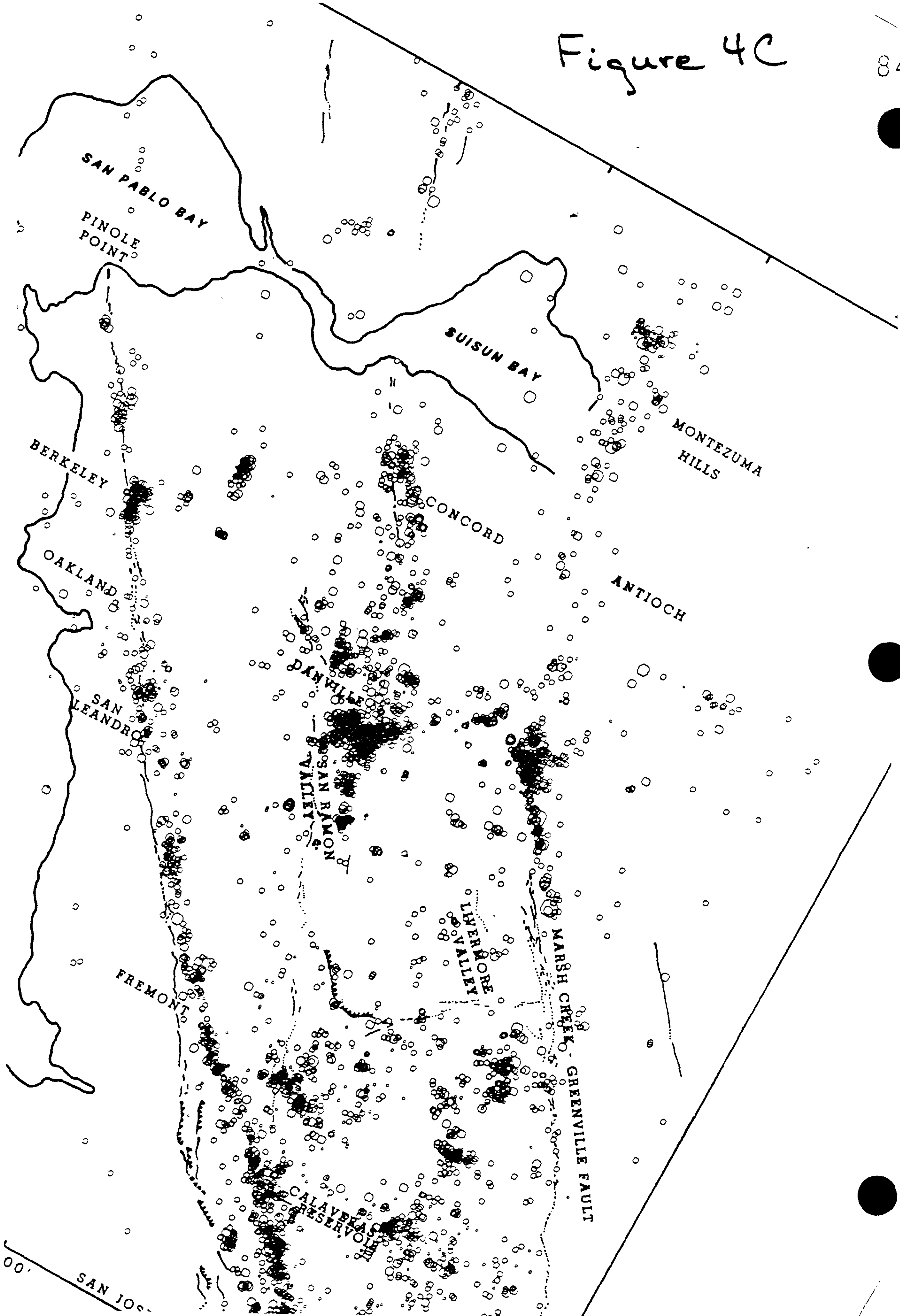

SAN FRANCISCO BAY AREA SEISMICITY
1981 - 1985
38°
50'
40'
30'
20'
10'
37°
50'
MAGNITUDES
0.0+
1.0+
2.0+
3.0+
4.0+
5.0+
6.0+
20 KM
40'
30'
20'
10'
122°
50'
40'
30'
20'

Figure 4b

Figure 4C


SAN PABLO BAY
PINOLE POINT
SUISUN BAY
MONTEZUMA HILLS
BERKELEY
CONCORD
OAKLAND
ANTIOCH
DANVILLE
SAN LEANDRO
SAN RAMON VALLEY
LIVERMORE VALLEY
MARSH CREEK - GREENVILLE FAULT
FREMONT
CALAVERAS RESERVOIR

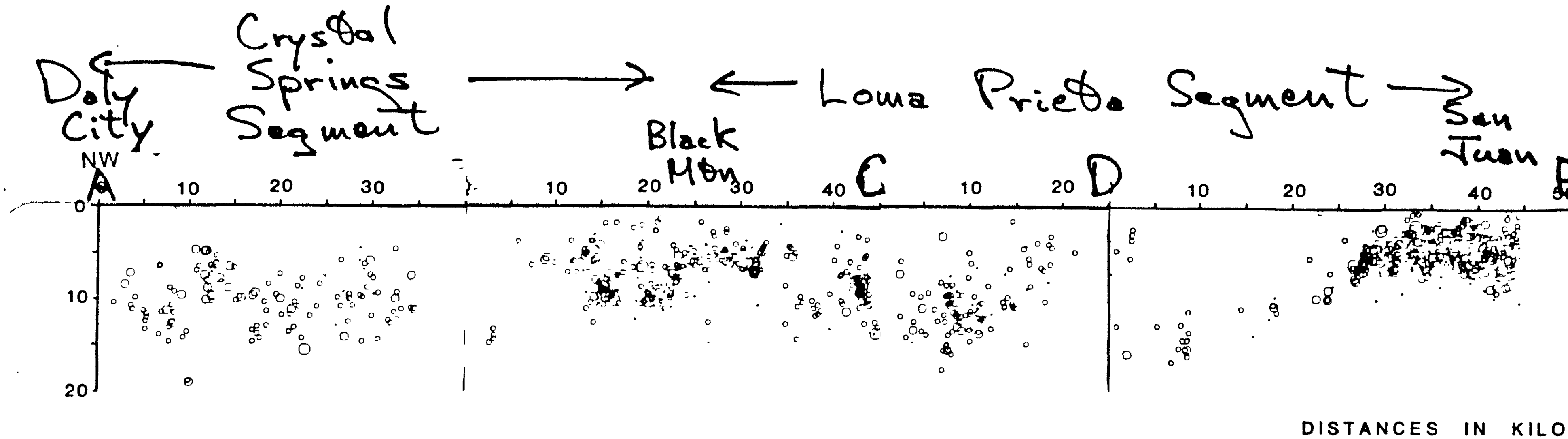
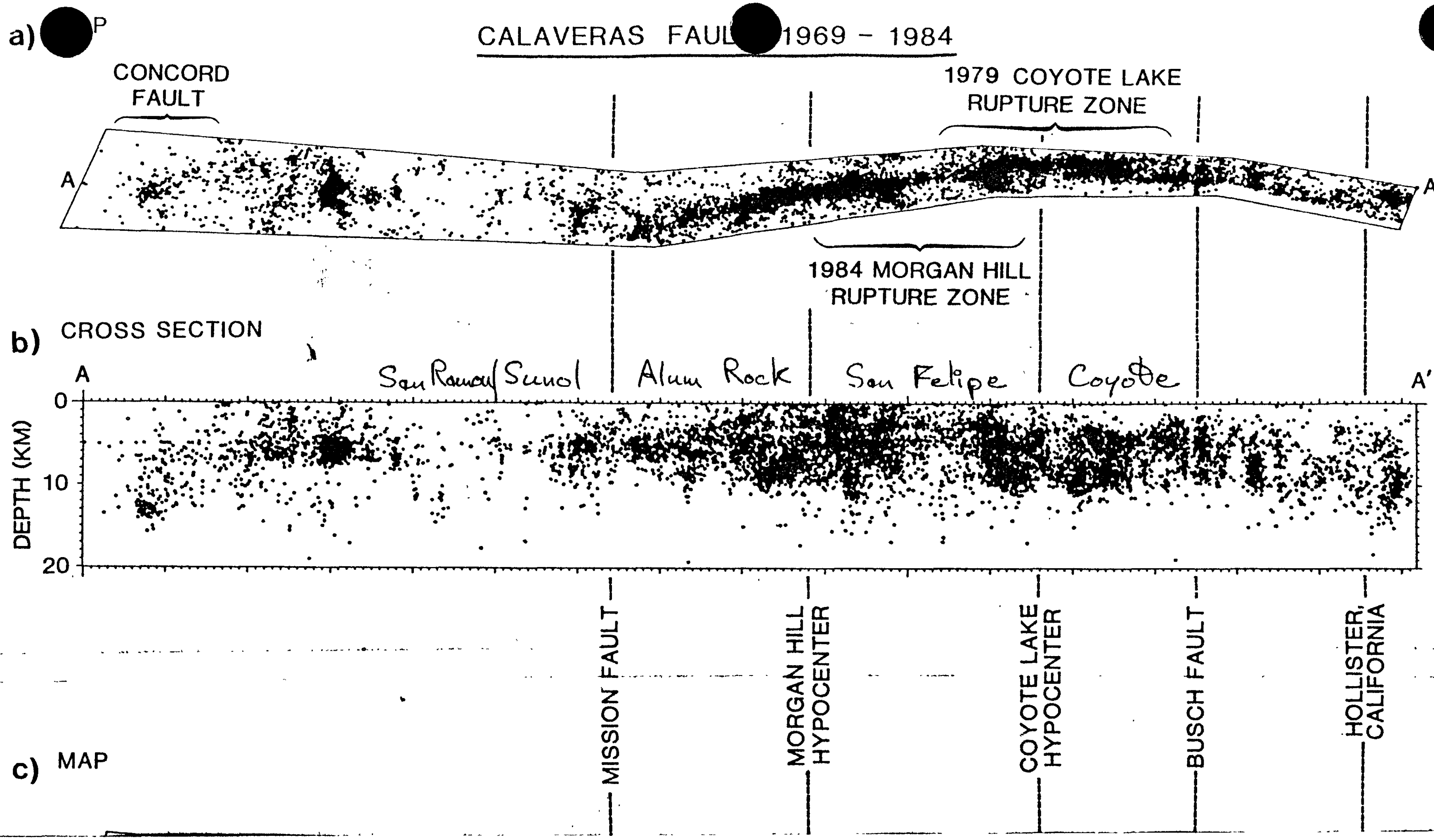

Daly City
Crystal Springs Segment
Black Mtn
Loma Prieta Segment
San Juan
NW
A
C
D
E
DISTANCES IN KILO

Figure 5A


a)
CALAVERAS FAULT 1969 - 1984
CONCORD FAULT
1979 COYOTE LAKE RUPTURE ZONE
1984 MORGAN HILL RUPTURE ZONE
A
A'
b) CROSS SECTION
San Ramon
Sunol
Alum Rock
San Felipe
Coyote
DEPTH (KM)
0
10
20
MISSION FAULT
MORGAN HILL HYPOCENTER
COYOTE LAKE HYPOCENTER
BUSCH FAULT
HOLLISTER, CALIFORNIA
c) MAP

87

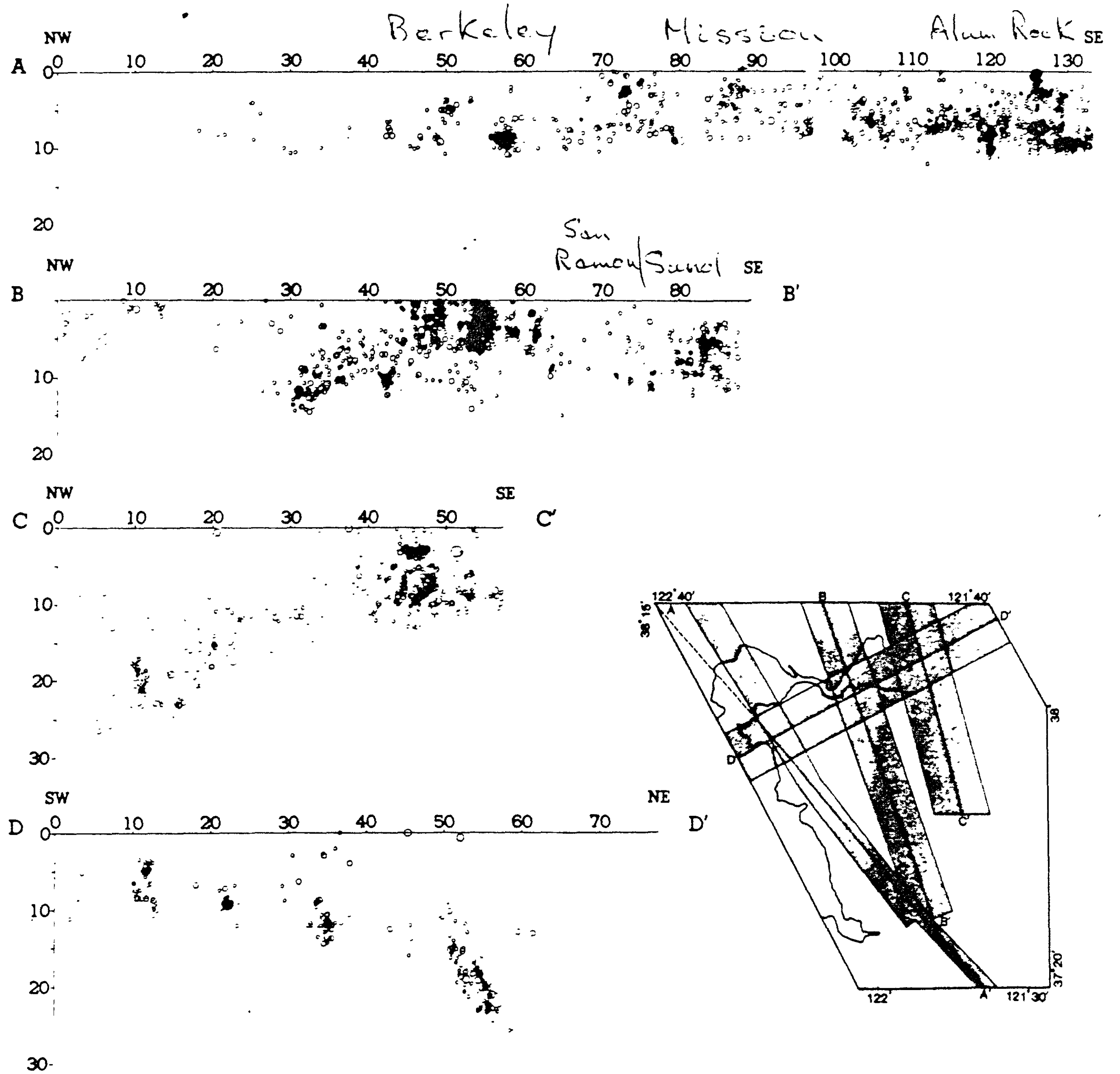


Figure 5C

~~Figure 5.~~ Cross-sectional view of seismicity along Calaveras-Hayward-Rodgers Creek faults (A-A'), along Calaveras-Concord-Green Valley faults (B-B'), within zone between Marsh Creek-Greenville fault and area east of Suisun Bay (C-C'), and within a transverse zone (D-D'). Exact location of each section shown in insert. All sections are shown without vertical exaggeration. Note that apparent northeastward dip of deep activity east of Suisun Bay (D-D') is an artifact of projection line.

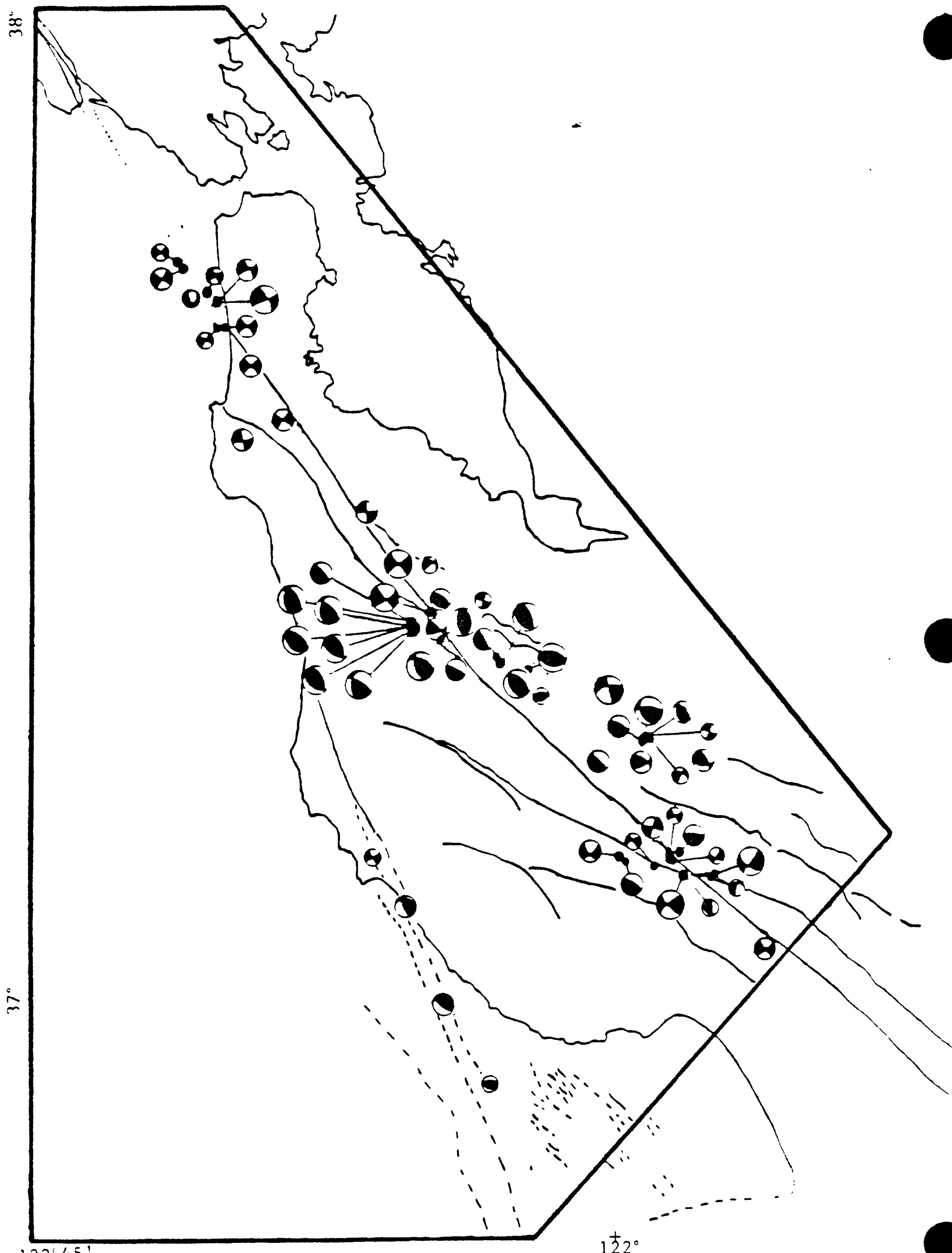


Figure 6A

Figure 4.

alignments occur: (1) northwest of Calaveras Reservoir between the Calaveras and Hayward faults, (2) beneath San Pablo Bay between the Hayward and Rodgers Creek faults, and (3) east of Suisun Bay beneath the west edge of the Montezuma Hills (Figure 2). Other epicentral alignments shown in Figure 2 suggest subsurface shear zones with continuities measured in kilometers. One example is the north-northwest epicentral alignment along the eastern side of the San Ramon Valley and parallel to the Calaveras fault; another alignment extends out of the dense cluster of epicenters near Danville (center, Figure 2) and trends northeast. In addition, low-level seismic activity that is not associated with the Hayward fault occurs near the east

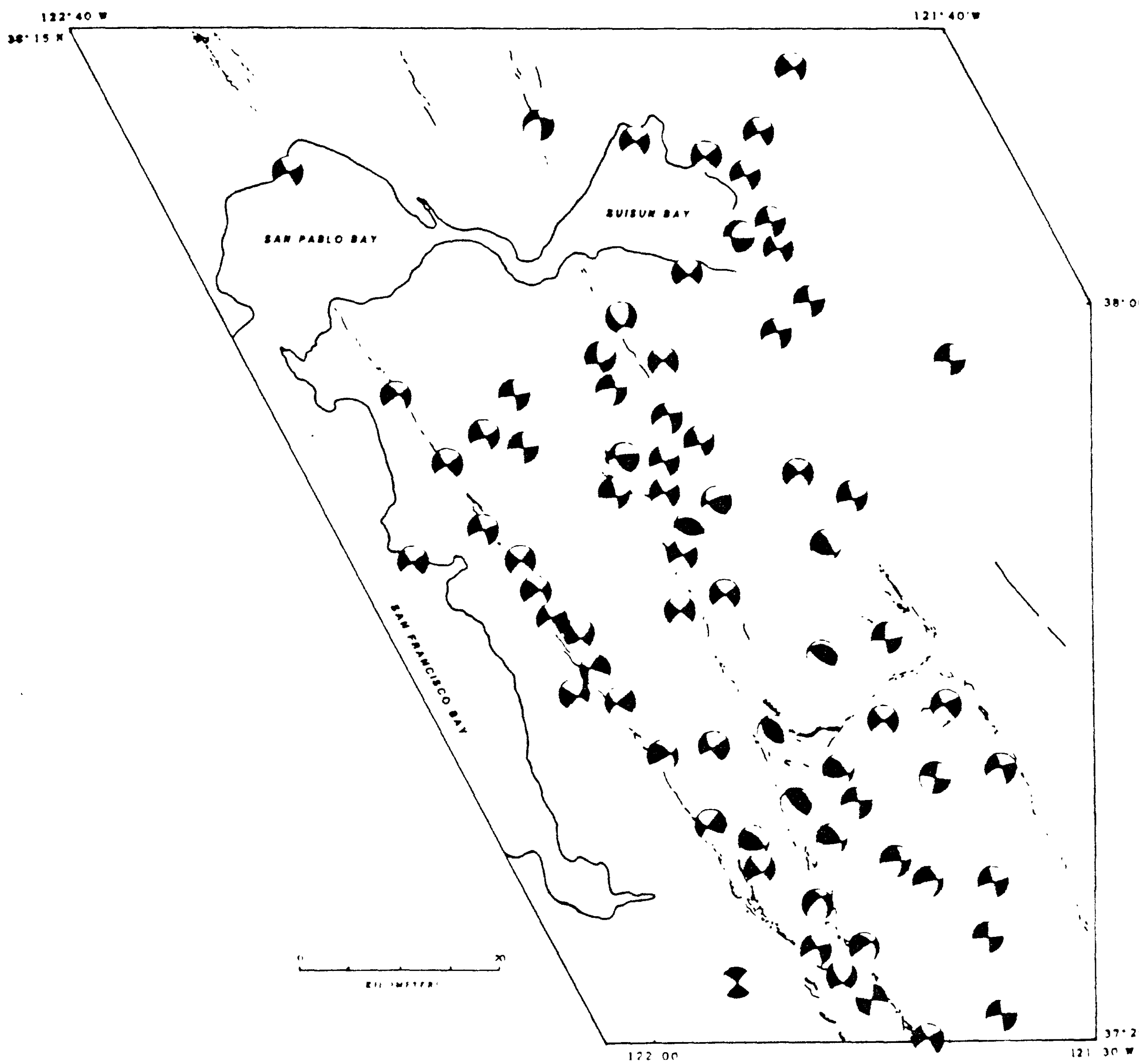
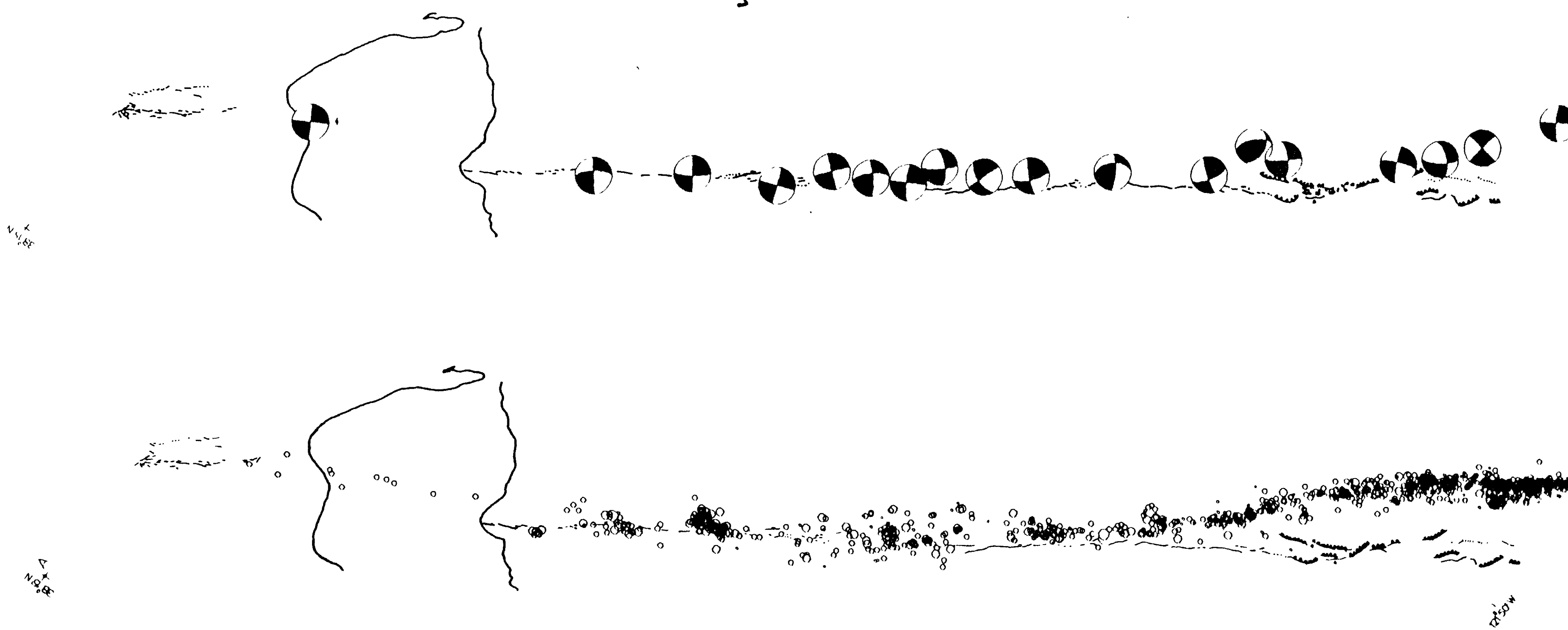


Figure 3. Representative focal mechanism solutions for the eastern San Francisco Bay region, 1969-79. Nodal lines are shown on lower hemisphere with compressional quadrants shaded.

Figure 6B

Figure 6C



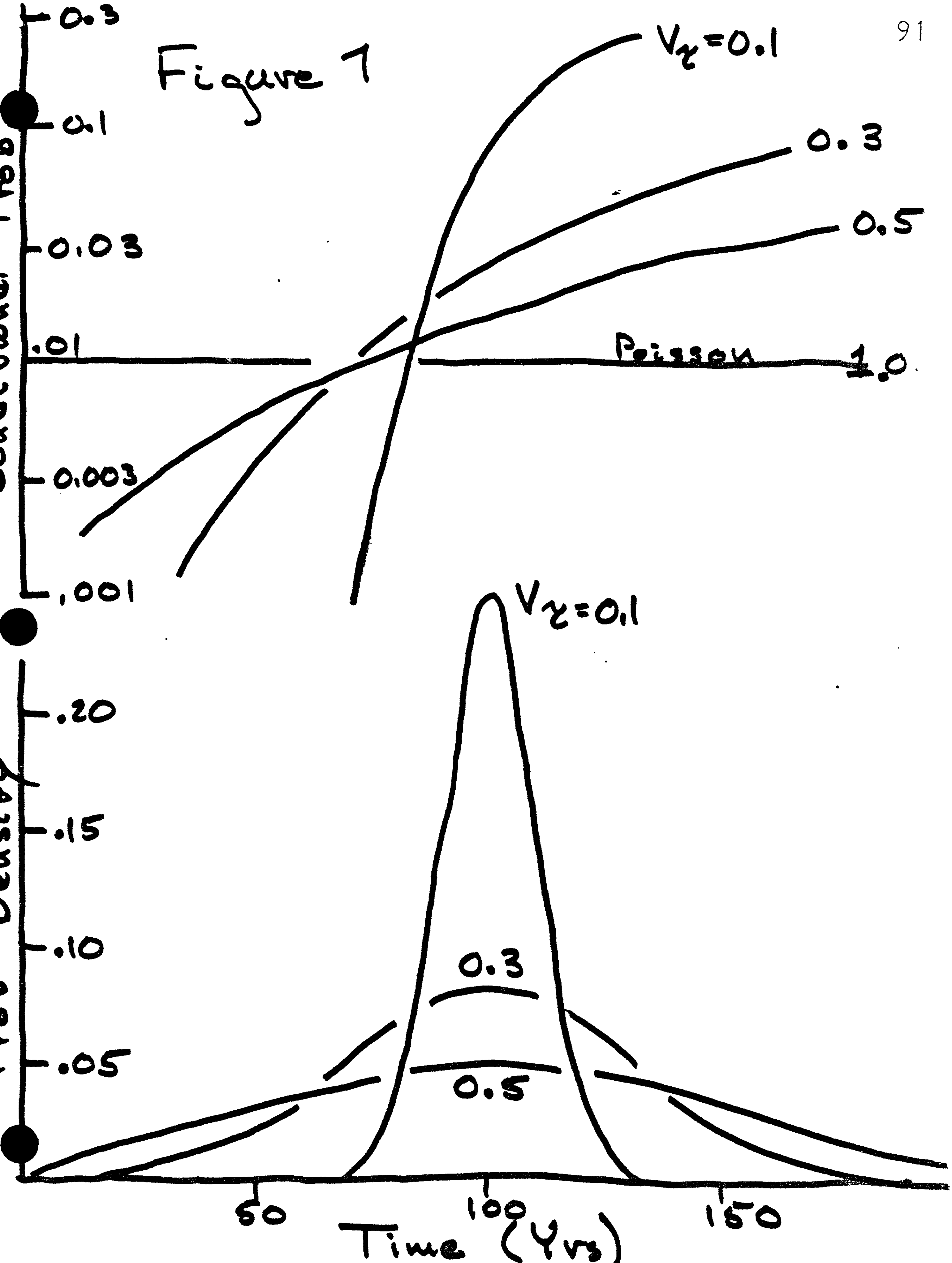


Figure 7
0.3
0.1
0.03
.01
0.003
.001
Vγ=0.1
0.3
0.5
Poisson
1.0
.20
.15
.10
.05
Vγ=0.1
0.3
0.5
50
100
150
Time (Yrs)

SLIP DEFICIT ON THE PARKFIELD, CALIFORNIA, SECTION OF THE SAN ANDREAS FAULT AS REVEALED BY INVERSION OF GEODETIC DATA

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March 7, 1986

ABSTRACT

A network of geodetic lines spanning the San Andreas fault near the rupture zone of the 1966 $M = 6$ Parkfield, Calif., earthquake has been repeatedly surveyed since 1959. We have inverted the average rates of line-length change since 1966 to determine the distribution of interseismic-slip rate on the fault. Our results indicate that the Parkfield rupture surface has not slipped significantly since 1966. Comparison of the geodetically determined seismic moment of the 1966 event with the interseismic-slip-deficit rate suggests that the strain released by the latest shock will be restored by the end of this decade. These results lend support to the earlier forecast of a $M = 6$ earthquake near Parkfield within 5 years of 1988.

Since 1857 five $M = 6$ earthquakes have occurred on the 30-km-long Parkfield, Calif., segment of the San Andreas fault, the latest on June 27, 1966. The next Parkfield earthquake has been forecast to occur in 1988 ± 5 years, on the basis of an extrapolation of the historical earthquake sequence, which has a mean repeat time of 21.9 ± 3.1 years (1). Recently, however, possible errors in the locations of earthquakes prior to 1922 have led some workers to question the regular recurrence of past Parkfield earthquakes (2).

A network of geodetic lines spanning the fault near Parkfield has been repeatedly surveyed with precise distance-measuring instruments since 1959. We use observed changes in line lengths to constrain the distribution of fault slip at depth. A unique aspect of the Parkfield data set is that measurements span virtually a complete earthquake cycle, from before the 1966 earthquake to the present. This allows a comparison of the strain accumulation since 1966 with the strain released in the 1966 sequence. The results of this comparison provide a test of the potential for an $M = 6$ earthquake in the Parkfield area by 1988, a test that is independent of the earthquake sequence before 1966.

A total of 13 lines in the Parkfield area were surveyed before the 1966 earthquake (3); subsequently the network has been expanded to more than 80 lines (4, 5). In this study, we use only the 45 frequently surveyed lines illustrated in Figure 1 (6). Repeated measurements of these lines were analyzed to determine average rates of extension for the interseismic period (1966–84), as well as coseismic offsets of the 13 lines measured prior to the 1966 earthquake (7).

The Parkfield segment of the San Andreas fault is bounded on the northwest by an aseismically slipping, or creeping, zone and on the southeast by a nonslipping, or locked, zone that last ruptured during the 1857 Fort Tejon $M = 8$ earthquake. The average rate of shallow fault slip measured by an array of instruments, including creepmeters, alignment arrays, and short-aperture trilateration networks (7–10), decreases monotonically from nearly 30 mm/yr in the creeping zone northwest of the 1966 epicenter to zero in the locked zone south of the 1966 rupture (Fig. 2).

We postulate that the observed line-length changes are caused by some distribution of slip on the San Andreas fault, plus a small component of random survey error. The fault is modeled as a planar surface of displacement discontinuity in a homogeneous, isotropic, elastic half-space. The fault-slip distribution is approximated by a discrete distribution of small rectangular elements, each with uniform slip. This parametrization allows the geodetic observations, the data, to be written as linear functions of the unknown slip rates, the model parameters (12). The slip rates are estimated by a variation of the "natural" or "generalized" inverse (13–14). That part of the slip-rate distribution that is overdetermined by the data is found by least squares, while the underdetermined part is specified by the assumption that the slip rate is to some degree smooth. The inverse operator is thus chosen to minimize the weighted sum of the squares of residuals in the "data space," while at the same time minimizing the roughness of the slip distribution in the "model space." (14, 15). Separation of the overdetermined and underdetermined parts of the slip-rate distribution is accomplished through a singular-value decomposition (13–14). Increasing the number of singular values used in constructing the inverse operator improves the data fit while increasing the roughness and variance of the slip-rate distribution.

At shallow and intermediate depths, the 36-km-long model Parkfield fault segment is divided into 3-km-long by 2-km-deep rectangular elements. Below a specified depth, referred to as the transition depth, the slip rate is modeled as spatially uniform and steady in time. Coseismic- and postseismic-slip transients of Parkfield earthquakes do not extend below the transition depth, as so defined. The slip rate in the near-surface layer is constrained to fit the surface-creep-rate profile shown in Figure 2. The slip rate northwest of the Parkfield segment is fixed at 25 mm/yr from the surface to the transition depth, a value consistent with the limited fault-creep data in this region (8, 9). Southeast of the Parkfield segment, the fault is locked from the surface to the transition depth.

An initial series of inversions was performed varying the transition depth in order to determine the dependence of the deep-slip rate on the transition depth. The results (Table 1) demonstrate that the best-fitting deep-slip rate increases with increasing transition depth and that transition depths from 14 to 22 km fit the data equally well. Clearly, the geodetic data alone cannot uniquely resolve the transition depth. The simplest model may be one with a slip rate of 33 mm/yr below the transition depth, since this is the rate of rigid-block motion in the creeping zone northwest of the Parkfield segment (12) as well as the late Holocene slip rate southeast of Parkfield (16).

Figure 3 illustrates the slip-rate patterns for a series of inversions, with a transition depth of 22 km and a deep-slip rate fixed at 33 mm/yr. In Figure 3a, the number of singular values in the inversion equals the number of constrained elements (17). This solution minimizes the roughness, or curvature, of the slip-rate distribution and is thus the smoothest model of the transition between the creeping zone and the 1857 locked zone. Despite its simplicity and the fact that the smooth model (Fig. 3a) reproduces the gross features of the deformation pattern (18), we find that significant improvements in the data fit can be achieved by increasing the number of independent parameters in the inversion.

The model with one additional degree of freedom is shown in Figure 3b. We note that improvement in the fit to the trilateration data (Table 2) is achieved by reducing the slip rate relative to the smooth model. In this case, the southern 20 km of the Parkfield segment is locked, or nearly locked, at intermediate depths. Adding another degree of freedom to the model results in the slip-rate profile shown in Figure 3c. The improvement in data fit is achieved primarily by extending the locked zone northwestward to Middle Mountain. Analysis of the model misfits demonstrates that addition of the first two degrees of freedom significantly improves the fit to the geodetic data, whereas the effect of adding the third is insignificant (Table 2). Thus, the geodetic data are capable of determining only three linearly independent characteristics of the slip distribution, of which the third is the deep-slip rate (19). Excluding the remaining singular values and their associated eigenvectors from the inverse operator tends to suppress short-wavelength variations in the slip, and results in estimated models that are presumed to be smoothed versions of the true slip distribution (20).

Inversions with transition depths ranging from 14 to 20 km exhibit locked or slowly slipping zones similar to those shown in Figure 3c. We conclude that the data are capable of detecting the presence of a locked zone but cannot resolve details of the locked-zone geometry, including the depth of its lower boundary. Considering the resolution of the data, the locked zone predicted by the inversions coincides quite closely with the rupture surface of the 1966 earthquake as delineated by its aftershocks (21). The hypocenter of the main shock is located at the northwest end of the locked zone, and the aftershocks extend some 30 km southeastward into the zone of negligible interseismic slip (Fig. 3c).

For transition depths greater than 14 km the locked zone extends to greater depth than the aftershocks (Fig. 3c), implying substantial aseismic afterslip below the aftershock zone.

Coseismic changes in the length of the 13 lines measured before and after the 1966 earthquake were inverted to determine the event's seismic moment, using the same inversion procedure described previously. Because the geodetic measurements do not adequately constrain the depth of seismic slip, we assume in the following analysis that no coseismic or postseismic slip occurred below the transition depth, and allow this parameter to vary from 14 to 22 km. The fact that aftershocks in 1966 occurred to depths of 14 km (21) demonstrates slip to at least that depth. The 1966 seismic moment estimated from the geodetic data ranges from 5.5×10^{25} dyne-cm, assuming no slip below 14 km, to 9.1×10^{25} dyne-cm, assuming no slip below 22 km. These estimates exceed the seismic moment of 0.9×10^{25} to 2.1×10^{25} dyne-cm calculated from surface waves (22). Because the postearthquake surveys were conducted several weeks or more after the earthquake, the geodetically determined seismic moment includes an unknown amount of aseismic afterslip. In comparison, postseismic slip more than doubled the geodetically determined moment of the 1985 Morgan Hill earthquake (23). The remaining discrepancy between the seismic and geodetic moments may be an artifact of the inversion procedure, which, by minimizing the curvature in the slip distribution, tends to introduce substantial slip at depth where the 1966 network has poor resolution. An alternative inversion procedure that tends to minimize the slip (24) yields a moment of 3.2×10^{25} dyne-cm, which we take to be a lower bound on the 1966 moment.

The previous results are used to calculate the time required for a moment deficit equal to the 1966 seismic moment to accumulate. This should be equivalent to the recurrence time according to the time-predictable earthquake-recurrence model (25). We consider two limiting models, the first with a transition depth at 14 km the second at 22 km (26). In each case, the slip deficit relative to the corresponding deep-slip rate is used to calculate a moment-deficit rate. This value is then compared to the moment of the 1966 event, which for consistency is calculated from the geodetic data, assuming no coseismic or postseismic slip below the same transition depth. In model 1, slip deficit in the upper 14 km accumulates at a rate of 2.4×10^{24} dyne-cm/yr relative to the deep-slip rate of

25 mm/yr (Table 3). In model 2, slip deficit in the upper 22 km accumulates at a rate of 5.2×10^{24} dyne-cm/yr relative to the deep-slip rate of 33 mm/yr (Table 3). Comparison with the corresponding seismic moments yields strain-accumulation times of 23 and 18 years for models 1 and 2, respectively.

Both estimates in Table 3 are reasonably close to the 22 ± 3 yr recurrence time for past Parkfield earthquakes. Given the inherent nonuniqueness in inverse problems, it is difficult to place meaningful error bounds on the estimated strain-accumulation time. We note, however, that although various alternative assumptions lead to strain-accumulation times less than 22 years, it is difficult to posit conditions that lead to intervals greater than 22 years. For example, in the first model, the deep slip rate over the latest Parkfield earthquake cycle does not accommodate the entire late Holocene slip rate; the remaining slip is presumed to occur as coseismic and postseismic slip to great earthquakes on the 1857 segment of the fault. Arbitrarily increasing the slip rate below 14 km to 33 mm/yr increases the moment-deficit rate in the seismogenic zone and thus decreases the strain-accumulation time from 23 to 16 years. We note, also, that the estimated seismic moment of the 1966 earthquake in both models exceeds the surface-wave estimate. Reducing the moment to the lower bound of 3.2×10^{25} dyne-cm decreases the strain-accumulation times to 13 and 6 years for models 1 and 2, respectively; intervals that are significantly less than the 19+ years since the latest Parkfield earthquake. None of our estimates, however, significantly exceeds the past recurrence interval of 22 years.

SUMMARY

Interseismic extension rates of geodetic lines in the Parkfield, Calif., area are consistent with the interpretation that the 1966 rupture surface has been locked since the latest earthquake. Comparison of the slip-deficit rate since 1966 with the moment of the 1966 earthquake suggests that the strain released in 1966 either has already accumulated or will accumulate by the later part of this decade. These results imply that sufficient elastic strain will be stored for an $M = 6$ earthquake to occur at Parkfield by the end of the decade.

TABLE 1. TRANSITION DEPTH AND DEEP-SLIP RATE

Transition Depth (km)	Deep-Slip Rate (mm/yr)	χ^2 (1)
14	25.5	101.2
16	27.3	101.3
18	29.1	101.3
20	30.9	101.3
22	32.7	101.3

(1) $\chi^2 = \sum_{i=1}^{N} \frac{(o_i - c_i)^2}{\sigma_i^2}$, where o_i is the observed rate of line length change, c_i is the calculated rate, σ_i is the *a priori* standard deviation in the rate, and $N = 45$ is the number of geodetic lines.

TABLE 2. MODEL FIT TO GEODETIC DATA

Number of Singular Values[1]	χ^2 [2]	P [3]	Moment Rate (dyne-cm/yr)	
0	185.2	—	4.5×10^{24}	Figure 3a
1	117.2	$< .001$	2.7×10^{24}	Figure 3b
2	101.7	$< .005$	2.5×10^{24}	Figure 3c
3	99.7	$< .25$	2.5×10^{24}	(not shown)

(1) Exclusive of singular values used to satisfy constraints.

(2) See Table 1.

(3) Probability that improvement in fit due to the additional singular value would occur randomly.

TABLE 3. TIME-PREDICTABLE RECURRENCE ESTIMATES

	Model 1	Model 2
Transition Depth (km)	14	22
Deep-Slip Rate (mm/yr)	25.5	32.7
Interseismic-Moment Rate (dyne-cm/yr)	1.5×10^{24}	2.6×10^{24}
Interseismic-Moment-Deficit Rate (dyne-cm/yr)	2.4×10^{24}	5.2×10^{24}
Coseismic Moment (dyne-cm)	5.5×10^{25}	9.1×10^{25}
Time-Predictable Recurrence Interval (yr)	23	18

REFERENCES AND NOTES

1. W. H. Bakun and T. V. McEvilly, *J. Geophys. Res.*, **89**, 3051 (1984). W. H. Bakun and A. G. Lindh, *Science*, **229**, 619 (1985).

2. T. R. Toppozada, *Trans. Amer. Geophys. Union*, **66**, 982 (1985). C. R. Real, *ibid.*

3. California Department of Water Resources, Geodimeter fault movement investigations in California, Bull. **116-6**, (1968).

4. The California Department of Mines and Geology took over the network from the California Department of Water Resources in 1970, and expanded the network in the Parkfield region to 32 lines (5). In the late 1970's, the U.S. Geological Survey expanded the network to 80 lines (7).

5. J. H. Bennet, *Calif. Div. Mines Geology Special Rep.* **140**, (1980).

6. Of the 45 lines analyzed 44 were surveyed more than 3 times, and 40 were surveyed 5 or more times.

7. N. E. King, W. H. Prescott, and P. Segall, Geodetic measurements near Parkfield, Calif., 1959-1984, in preparation.

8. S. S. Schulz, G. M. Mavko, R. O. Burford, and W. D. Stuart, *J. Geophys. Res.*, **87**, 6977 (1982).

9. R. O. Burford and P. W. Harsh, *Bull. Seis. Soc. Amer.*, **70**, 1233 (1980).

10. M. Lisowski and W. H. Prescott, *Bull. Seis. Soc. Amer.*, **71**, 1607 (1981).

11. Slip rates from creepmeters are averages of least-squares fits and simple end-point averages for all data before 1980 (8). Slip rates calculated from alignment arrays are averages of block-fit and end-point rates (9). In both

cases, error bars encompass both estimates. Slip rates from short-aperture geodetic nets are weighted means of fault-crossing lines (7, 10).

12. W. Thatcher, *J. Geophys. Res.*, **84**, 2283 (1979).

13. C. Lanczos, *Linear Differential Operators*, Van Nostrand-Reinhold, Princeton, New Jersey (1961). K. Aki and P. G. Richards, *Quantitative Seismology: Theory and Methods*, W. H. Freeman Co., San Francisco (1980).

14. W. Menke, *Geophysical Data Analysis: Discrete Inverse Theory*, Academic Press (1984).

15. The roughness of the slip-rate distribution is quantified by $\sum_{i=1}^{M}(\nabla^2 s_i)^2$, where $\nabla^2 = \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial z^2}$ is the Laplacian operator (in practice, a finite-difference approximation to ∇^2 is utilized), x and z are horizontal and vertical distances along the fault plane, s_i is the slip in the i^{th} element, and M is the total number of elements. It is easily shown (14) that the inverse which minimizes the solution roughness is simply a weighted form of the generalized inverse which minimizes the solution length $\sum_{i=1}^{M}(s_i)^2$.

16. K. E. Sieh and R. H. Jahns, *Geol. Soc. Amer. Bull.*, **95**, 883 (1984).

17. In this series of inversions there are 14 constraints: 12 surface-creep rates, the slip rate in the creeping zone, and the deep-slip rate.

18. W. F. Slawson and J. C. Savage, *Bull. Seis. Soc. Amer.*, **73**, 1407 (1983).

19. With p singular values, the residual sum of squares has 45-p degrees of freedom. The large χ^2 values in Table 2 thus warrant explanation. Independent calculations demonstrate that the *a priori* estimates of the data variance are too small by a factor of 1.6. A least-squares adjustment of the data to determine a consistent set of station-displacement rates

yields an estimate of the data variance that is independent of the source of deformation. For the Parkfield network, the adjustment has a residual sum of squares of 12.62 with eight degrees of freedom and so $s^2 = 1.58$.

20. R. L. Parker, *Ann. Rev. Earth Planet. Sci.*, **5**, 35 (1977).

21. J. P. Eaton, M. E. O'Neill, and J. N. Murdock, *Bull. Seis. Soc. Amer.*, **60**, 1151 (1970).

22. Y.-B. Tsai and K. Aki, *Bull. Seis. Soc. Amer.*, **59**, 275 (1969).

23. W. H. Prescott, N. E. King, and G. Guohua, *Calif. Div. Mines Geol. Spec. Publ.* **68**, 137 (1984) show that the net geodetic moment (coseismic plus postseismic) between April and August 1984 equaled 1.8 times the seismic moment. This ratio increased to 2.3 as of January 1986 (W. H. Prescott, personal communication).

24. For this calculation, we use the natural or minimum-length inverse (12), which minimizes the length of the solution vector. In practice, the natural inverse places nearly all the slip at shallow (2–4 km) depth, where the resolution of the network is greatest. Although this calculation is clearly artificial and at variance with the seismic evidence for rupture to depths of at least 10 km, this model does provide a reasonable lower bound on the seismic moment.

25. K. Shimazaki and T. Nakata, *Geophys. Res. Lett.*, **7**, 279 (1980).

26. A lower bound of 14 km on the transition depth is reasonable because the occurrence of aftershocks to 14 km demonstrates postseismic and, possibly, coseismic slip to this depth. We do not consider transition depths greater than 22 km here because the best-fitting deep-slip rates for these models exceed the long-term slip rate of 33 mm/yr. Time-dependent response to the 1857 earthquake might have caused the deep-slip rate during the 25-year

measurement period to lag the long-term rate. We consider it unlikely that the deep-slip rate during this period exceeded the long-term rate, although this possibility is not inconsistent with the data.

27. We thank W. Ellsworth, J. Langbein, W. Menke, D. Oppenheimer, W. Prescott, J. Savage, R. Simpson, and W. Thatcher for advice and/or thoughtful reviews of the manuscript.

FIGURE CAPTIONS

Figure 1. Parkfield trilateration network. Straight lines represent geodetic survey-lines used in interseismic-slip-rate inversion. 1966 Parkfield mainshock (star) and $M > 2$ aftershocks (circles), (ref. 21). A-A' indicates cross section shown in Figs. 2 and 3.

Figure 2. Shallow-fault-slip rate versus distance along fault, showing average slip-rate as measured by creepmeters (circles), alignment arrays (squares), and short-baseline trilateration networks (triangles), (ref. 11). Heavy line shows shallow-fault-slip-rate profile used in interseismic inversions. MM = Middle Mountain, GH = Gold Hill.

Figure 3. Interseismic-slip-rate pattern for a transition depth of 22 km and a deep-slip rate of 33 mm/yr. Colors indicate slip-rate in mm/yr. (a) Smooth model. (b) Same as (a) but with one additional degree of freedom in the inversion. (c) Same as (a) but with two additional degrees of freedom. Longitudinal cross section of 1966 aftershocks (circles) and mainshock (star) projected onto model fault plane (ref. 21) outlines rupture surface of 1966 Parkfield earthquake. MM = Middle Mountain, GH = Gold Hill.

Fig 1

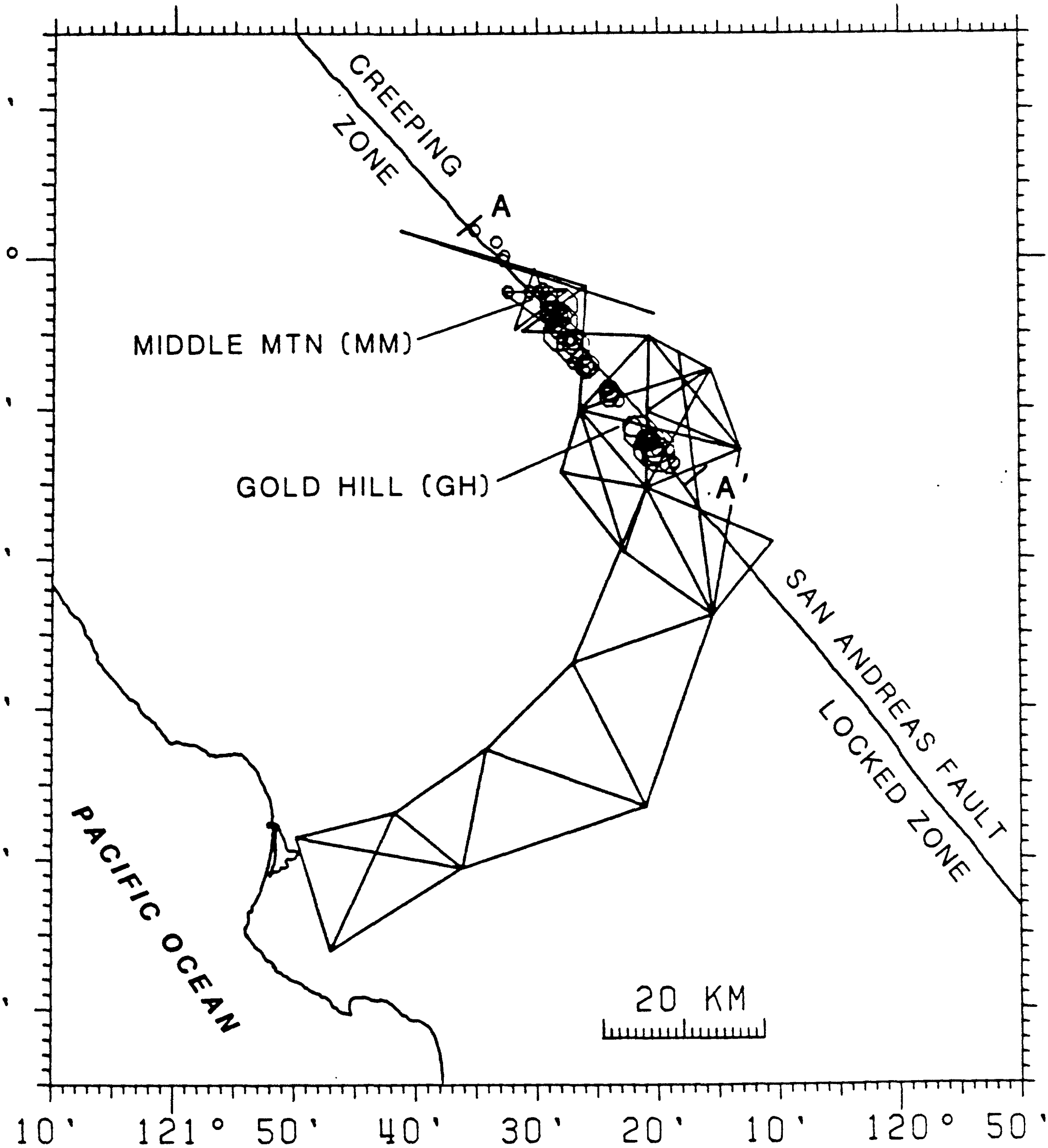
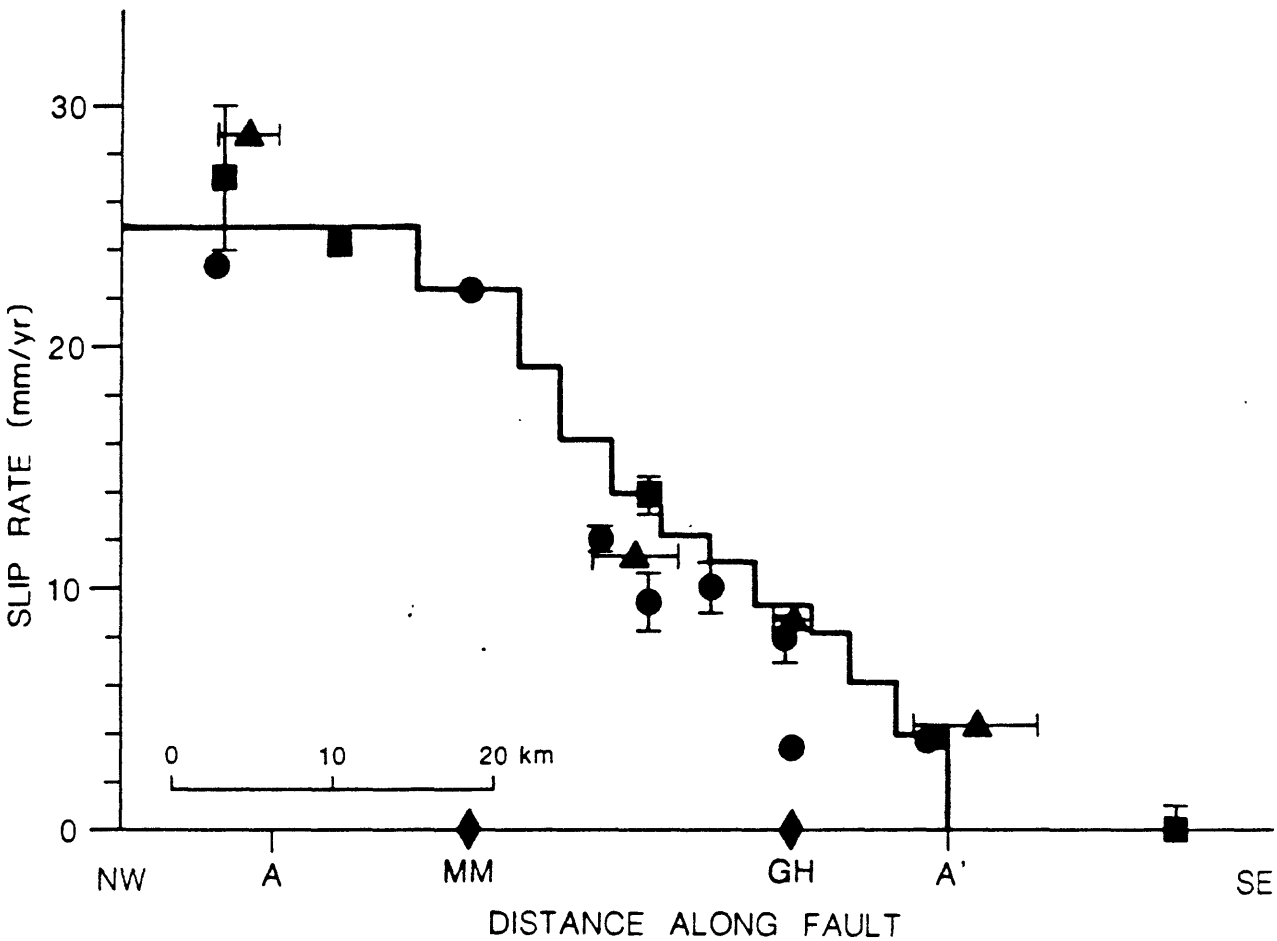
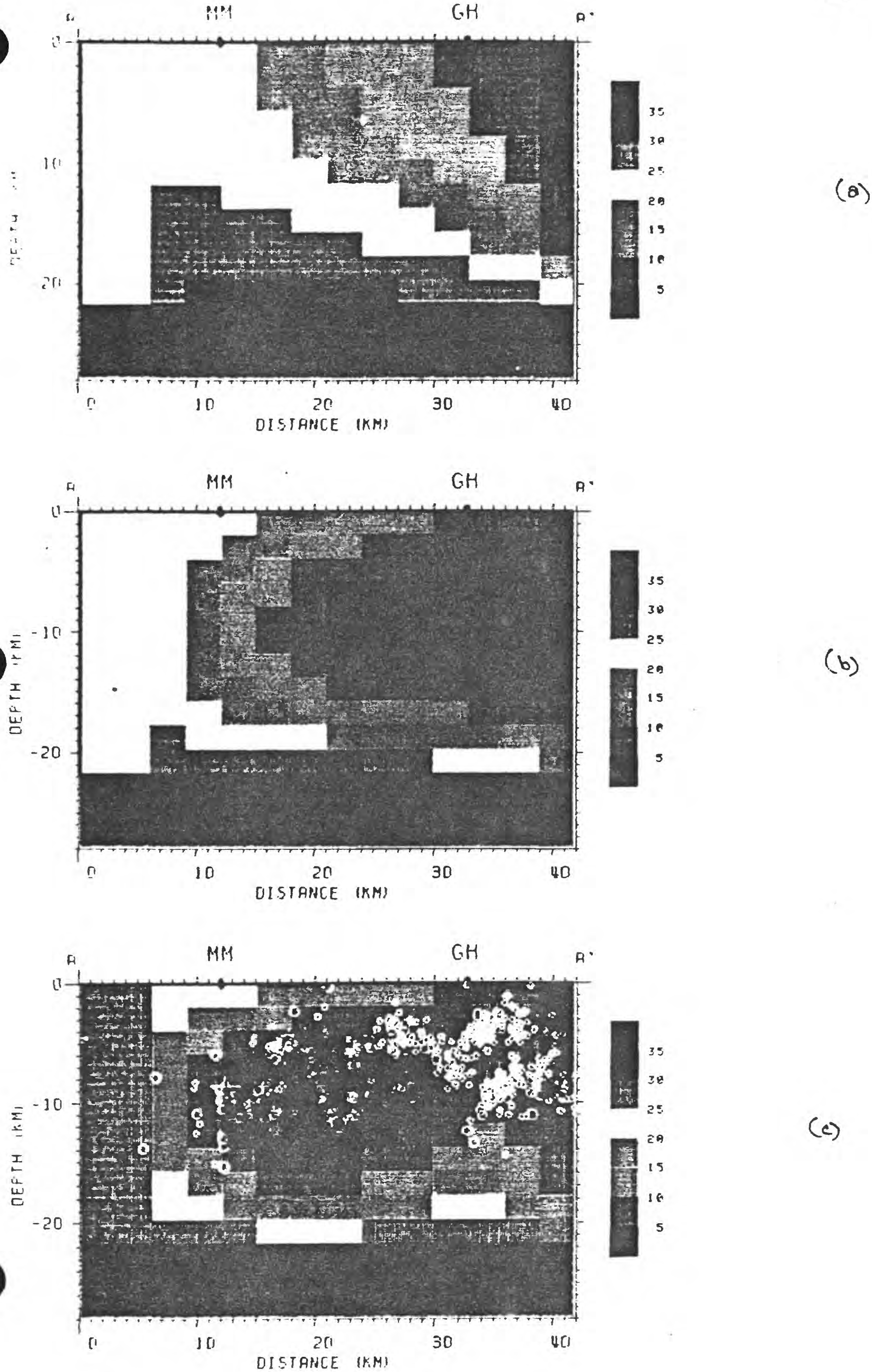

CREEPING
ZONE
A
MIDDLE MTN (MM)
GOLD HILL (GH)
A'
SAN ANDREAS FAULT
LOCKED ZONE
PACIFIC OCEAN
20 KM
10'
36°
50'
40'
30'
20'
10'
10'
121° 50'
40'
30'
20'
10'
120° 50'

Figure 2



30
20
10
0
SLIP RATE (mm/yr)
0
10
20 km
NW
A
MM
GH
A'
SE
DISTANCE ALONG FAULT

Figure 3


A
MM
GH
A'
DEPTH (KM)
0
-10
-20
0
10
20
30
40
DISTANCE (KM)
35
30
25
20
15
10
5
(a)
(b)
(c)

SEISMIC SLIP ON THE CALAVERAS FAULT, CALIFORNIA

William H. Bakun, Geoffrey C. P. King, Robert S. Cockerham

ABSTRACT

The 1969-1984 history of seismic slip on the Calaveras fault in central California illustrates different modes of fault failure. The recent rate of seismic slip along the creeping section near Hollister has lagged the geodetic slip rate and the seismic slip rate to the northwest where moderate earthquakes apparently occur every 75-80 years. The rupture zones of the $M_L = 5.8$ Coyote Lake earthquake of 6 August 1979 and the $M_L = 6.2$ Morgan Hill earthquake of 23 April 1984, both located northwest of the Hollister section, were relatively deficient in seismic slip in the decade before the earthquakes, suggesting that slip histories can be used to help identify fault sections where significant future seismic slip is most likely. The recent rate of seismic slip over the 20-km-long section of fault northwest of the Morgan Hill rupture zone is much less than that to the southeast and lags the geodetic slip rate; although undetected aseismic slip or off-fault deformation may be responsible, an interpretation of the discrepancy as potential for a future damaging shock cannot be rejected.

INTRODUCTION

A detailed description of earthquake processes is contained in the earthquake history obtained using the dense networks of seismographs along the Calaveras fault in central California (Figure 1). Along its southernmost section near Hollister, the Calaveras fault fails by aseismic slip (fault creep) and in small earthquakes (Rogers and Nason, 1971). While fault creep is observed at Shore Road (Schulz, 1984) near the southeast end of the rupture zone of the Coyote Lake earthquake, the trace of the Calaveras fault is obscure farther northwest, precluding unambiguous recordings of aseismic slip. The M_L=5.8 Coyote Lake earthquake of August 6, 1979 and the M_L=6.2 Morgan Hill earthquake of April 24, 1984 apparently were repeats of earlier shocks in 1897 and 1911 respectively, suggesting a recurrence time of 75-80 years for these sections of the Calaveras fault (Reasenberg and Ellsworth, 1982; Bakun et al., 1984). Northwest of the rupture zone of the Morgan Hill earthquake, the mode of failure for the Calaveras fault is not yet understood.

In this paper, we consider earthquakes along the approximately linear and continuous trend of the Calaveras, the Calaveras-Sunol, and the Concord faults. It is clear that slip on the Calaveras fault is connected with slip on other faults of the San Andreas fault system. For example, the M_L=5.1 Thanksgiving Day shock on November 28, 1974 on the Busch fault apparently affected the creep rate along the Hollister section of the Calaveras fault (Mavko, 1982). Furthermore, some slip transfers to the Hayward fault farther northwest. Where this occurs is not clear, and while geologic maps indicate that the Hayward and Calaveras faults converge near the center of the rupture zone of the 1984 Morgan Hill earthquake, epicenters of recent earthquakes diverge from the Calaveras trace onto the Hayward fault trace near the Mission fault of Hall (1958) (see Figure 2). Thus our interpretations of spatial segmentation of the seismicity (Figure 1) recognize that all active structures onto which slip can transfer have not been included. Nonetheless, important features of earthquake processes are apparent in the Calaveras fault seismic activity of the past 15 years.

Analyses of seismic activity usually depend on maps, cross sections, and space-time plots of earthquake hypocenters where the number of shocks is often so large that important aspects of the combined seismicity are not apparent. Basically, the predominant effects of the few larger shocks are not appreciated when viewed together with many smaller shocks. In this paper we describe analysis tools to display the combined effects of seismicity occurring over extended time periods. We weight the effects of individual shocks by seismic moment and hence, according to their contribution to deformation. Reid's (1910) elastic rebound theory coupled with the theory of plate tectonics implies that permanent deformation along plate boundaries occurring as seismic slip, aseismic slip, and folding, must match the relative plate motion if sufficiently long time periods are considered (Brune, 1968).

The seismic moment of the earthquakes are summed to obtain the distribution of seismic slip over the fault surface. This seismic slip distribution is the map over the fault surface of brittle failure, summarizing the part of the deformation monitored by the seismic networks. As such, the slip distributions can be easily compared with other deformation measurements. Such comparisons, the estimates of the slip budgets along the active faults, identify differences between the potential for seismic slip and the observed seismic slip. We show that the interpretation of these differences can be useful in anticipating future earthquakes.

DATA

We use the U.S. Geological Survey's (USGS) catalogs of central California earthquakes for the years 1969-1978 (Lee et al., 1972 a, b, c; Wesson et al., 1972a and b, 1973, 1974a and b; Bufe et al., 1975, Lester et al., 1976a and b;

Lester and Meagher, 1978; McHugh and Lester, 1978, 1979; Marks and Lester, 1980a and b; Marks and Fluty, 1981; Fluty and Marks, 1981) and unpublished USGS catalogs for 1978-1984. The catalogs generally are complete down to a magnitude of 1.5 (Reasenberg, 1985). A comparison of the catalogs for 1978-1984 with earthquake catalogs of the University of California, Berkeley, Seismographic Station (UCB) indicates that the unpublished USGS catalogs are essentially complete for shocks with magnitudes greater than 2.5, the reporting threshold of the UCB catalogs. Because of the weighting by seismic moment, the omission of individual magnitude $M \lesssim 3$ shocks has little effect on the distributions of seismic slip considered in this analysis (see Table 1).

Magnitude estimates for $M \lesssim 3.5$ shocks are based on coda durations (Lee et at., 1972d). Magnitude $\gtrsim 3.5$ estimates are M_L determined using amplitudes recorded on Wood-Anderson seismographs operated in central California by UCB. It is likely that the coda-duration magnitudes M_D (or the seismic moment -versus- M_D relations described below) are not adequately adjusted for develocorder magnification changes early in 1977. Preliminary comparisons of M_D with UCB's M_L (W.H. Bakun, unpublished USGS internal report, 1979) show that M_D after the develocorder change in 1977 are on average 0.26 ± 0.06 and 0.23 ± 0.06 less than comparable pre-1977 M_D for the north and south halves respectively of the polygon in Figure 1. (Additive M_D corrections of 0.2 and 0.1 were used by Bakun (1980) and Reasenberg and Ellsworth (1982) respectively in their analyses of seismicity within the south half of the polygon). Because the log M_o -versus- M_D relation (Bakun, 1984) is based on post-1977 seismicity, systematic errors in the seismic slip distributions caused by uncompensated develocorder changes probably are limited to the 1969-1976 data. We believe these errors do not seriously affect our results because the errors are spatially nearly uniform, the weighting by seismic moment substantially decreases the contribution of small shocks for which M_D are used, and most of the seismic slip occurred in the seismically more active post-1977 time period for which the log M_o-versus-M_D relations were developed.

While hypocenters in the 1969-1971 catalogs have a relative precision no better than a few kilometers, those in the 1977-1984 catalogs have a relative precision of about 1 kilometer. In this study we adopt a fault area cell size of 1 km x 1 km. The lateral cell boundaries are set by the arbitrary northwest end of the cross section AA' in Figure 1 and the depth cell boundaries by using zero focal depth as the upper edge of the first layer of cells. Each hypocenter is relocated at the center of the cell within which the hypocenter lies. Thus, we convert the hypocenters from a continuous distribution along the fault to a discrete distribution of 1 km grid spacing located on the vertical plane beneath the section AA'.

All of the earthquakes in the USGS catalogs located in the polygon (Figure 1) are used in this study. Tests using a subset of better hypocenter solutions (see caption of Figure 2) yield seismic slip distributions nearly indistinguishable from those obtained using the entire earthquake catalog, with the exception of the Morgan Hill aftershocks, where deleting larger Morgan Hill aftershocks with less precise locations results in minor, yet noticeable changes in the slip distribution during the aftershock sequence.

Earthquakes located within the polygon of Figure 1 generally have focal mechanisms consistent with right-lateral strike-slip displacement on planes striking along the long axis of the polygon (Lee et al., 1971; Bakun, 1980; Reasenberg and Ellsworth, 1982; Ellsworth et al., 1982; Cockerham and Eaton, 1984; Cockerham et al., 1985). Our analysis assumes consistent right-lateral strike-slip focal mechanisms so that the arithmetic sum of the seismic moments can be interpreted as a distribution of right-lateral slip on the Calaveras fault. Known exceptions are limited to a few smaller shocks located within fault jogs.

METHOD OF ANALYSIS

Seismic Moment M_o. Seismic moment M_o is defined in terms of a double-couple shear-dislocation earthquake source model (Aki, 1966) as

$$M_o = \mu \bar{u} A,$$

where

μ = modulus of rigidity,

A = fault surface area, and

$\bar{u}$ = average dislocation amplitude over A.

The logarithm of seismic moment, log M_o, can be estimated to a precision of 0.2 from USGS coda duration magnitudes M_D for $1 \leq M_D \leq 3.5$ earthquakes in central California using log $M_o = 1.2\ M_D + 17$ (Bakun, 1984). For shocks with $3 \lesssim M_L \lesssim 6$, the log $M_o = 1.5\ M_L + 16$ relation obtained by Thatcher and Hanks (1973) for $3 \lesssim M_L \lesssim 7$ southern California earthquakes is consistent with central California log M_o and M_L data (Bakun, 1984). Seismic moment has been estimated independently for the infrequent $M_L \gtrsim 5.5$ shocks in central California in recent years. $M_o = 1.9 - 2.1 \times 10^{25}$ dyne-cm for the 1984 Morgan Hill earthquake (Prescott *et al.*, 1984; Ekstrom, 1984) and $M_o = 5.5 - 5.6 \times 10^{24}$ dyne-cm for the 1979 Coyote Lake earthquake (Bouchon, 1982; Uhrhammer, 1980; Nabelek and Toksoz, 1981).

Distribution of Seismic Slip. We assume a rectangular rupture area A with length L and width (depth) W. We use $L = 2W = 4\left(\frac{u_1}{K}\right)^{1/3}$, where u_1 is the slip for $A = 1\ km^2$ and $K = 10^{-5}$. Values of u_1 and W are listed in

Table 1. These source parameters corresponded to stress drops of a few bars (Scholz, 1982), clearly less than the 10-100 bars usually obtained for shocks on the San Andreas fault system; we use these smaller stress drops (larger W and L) to spatially smooth the slip distribution to account for uncertainties in the location and the extent of rupture.

u_1 is distributed over adjacent cells in an area L km long by W km wide centered on the hypocenter cell. We use a "cosine-squared" weighting so that the slip is larger near the center of slip area (Madariaga, 1976). A suitable distribution such that M_o summed over the cells equals the earthquake M_o is provided by:

$$u_{n,m} = u_1 \left\{ \frac{1}{L} \cos^2 \left[\frac{\pi}{2} \left(\frac{1}{L}\right) n \right] \right\} \left\{ \frac{w}{W} \cos^2 \left[\frac{\pi}{2} \left(\frac{w}{W}\right) m \right] \right\}$$

where $u_{n,m}$ = slip in cell n,m. n,m = cell numbers (length, width) relative to the hypocenter cell (n=m=0),

1 = length increment of each cell (1 km),

w = width increment of each cell (1 km),

and $-\frac{L}{1} \leq n \leq \frac{L}{1}$ and $-\frac{W}{w} \leq m \leq \frac{W}{w}$. The slip distribution for magnitude 4 and 5 shocks are shown in Figure 3.

It is necessary to satisfy different constraints on the slip distribution for large shocks where L and W are comparable to the thickness of the seismogenic zone and the hypocenter is not the center of the slip distribution. For example, the M_L = 6.2 Morgan Hill earthquake was characterized by unilateral rupture propagation toward the south-southeast, and an energetic second source of seismic radiation was located near the southeast end of its rupture zone (Bakun et al., 1984). These two sources can be represented by an M_L=5.8 shock near the hypocenter and M_L=6.1 source near the hypocenter of the second source. A two-source slip distribution with appropriate values for L and W has been chosen such that the 10-cm slip contour approximates the slip boundary of the Morgan Hill main shock that was inferred by Cockerham and Eaton (1984) from the spatial distribution of aftershock hypocenters (see Figure 4). Similarly, a slip distribution can be devised for the 1979 Coyote Lake main shock (Figure 5) using the spatial pattern of aftershock hypocenters on Zone I of Reasenberg and Ellsworth (1982). The slip distributions shown in Figures 4 and 5 are generally consistent with results obtained from near-source strong-ground motions (Hartzell and Heaton, 1986; Liu and Helmberger, 1983).

SEISMIC SLIP ON THE CALAVERAS FAULT

The 1979 Coyote Lake Earthquake. The major seismic events on the Calaveras fault in the past 15 years are the $M_L = 5.8$ Coyote Lake earthquake and the $M_L = 6.2$ Morgan Hill earthquake. The recent history of seismic slip on the Calaveras fault near the rupture zones of these earthquakes is illustrated in Figures 6 and 7. Seismic slip in the decade before the Coyote Lake sequence occurred primarily at shallow depths to the northwest of the rupture zone of the Coyote Lake main shock (Figure 6a). Note that the seismic activity on the Busch fault, including aftershocks of the 1974 Thanksgiving Day earthquake that occurred near the southeast end of the Coyote Lake rupture zone, is not included in this analysis (see Figure 1). Seismic slip during the Coyote Lake aftershock sequence is concentrated near the southeast end of rupture zone of the Coyote Lake main shock (see Figure 6b).

The 1984 Morgan Hill Earthquake. Following the Coyote Lake aftershock sequence, seismic slip occurred primarily at the northwest end of the rupture zone of the 1984 Morgan Hill earthquake (Figure 6c). Summing the seismic slip in Figures 6a, 6b, and 6c, it is clear that the seismic slip in the 15 1/3 years preceding the 1984 Morgan Hill earthquake defines a 20-km-long section where seismic slip in the bottom half of the seismogenic zone (depths $\gtrsim$ 6 km) lagged that in adjacent sections (Figure 6d). The hypocenter of the 1984 Morgan Hill earthquake is located at the northwest end of the lagging slip zone, and rupture during the main shock was unilateral to the south-southeast so that slip in the main shock occurred primarily over the lagging section (Figure 7a). Aftershock activity following the Morgan Hill earthquake was concentrated near San Felipe Valley (Cockerham and Eaton, 1984) so that post earthquake seismic slip occurred near the inflections in the main shock slip boundary as shown in Figure 7b.

The spatial variation in seismic slip along the fault (Figure 7c) confirms the suggestion (Bakun, 1980) that deficits in cumulative seismic slip often mark the sections of fault where subsequent larger shocks will occur. In the case of the 1984 Morgan Hill earthquake, the deficit is somewhat masked by the shallow seismic activity that occurred above the rupture zone of the Morgan Hill earthquake (Figure 6a). With the 75-80 year recurrence time of moderate-size earthquakes suggested by the historic shocks on the Coyote Lake and Morgan Hill rupture zones, it is likely that the 15 years of seismic activity considered in this paper is not sufficient to characterize the complete behavior over an earthquake cycle. The identification of a slip deficit filled by the Morgan Hill earthquake is therefore not certain. However we can extend the 15-year range in an approximate way by using the significant earthquakes that occurred on the Calaveras fault before 1969. The magnitude 5.5 shock on 5 Sept 1955 (Bolt and Miller, 1975) near the north end of Halls Valley significantly increases the relative seismic slip to the north the Morgan Hill rupture zone if 30 rather than 15 years of detailed seismicity are used in the analysis (Figure 7c). Also, a $M_L = 5.2$ shock occurred on 9 March 1949 at the southeast end of Coyote Lake rupture zone (Bolt and Miller, 1975), so that the details of the seismic slip there would also change if a longer time period were used. The distribution of seismic slip with these 2 shocks included is shown as a dashed line in Figure 7c. These additions accentuate the regions of slip deficit noted above. We assume that the pattern would be accentuated further if a longer detailed history of smaller events were available.

Geodetic measurements of crustal deformation along the Calaveras fault suggest a potential slip rate of 1.5 cm/year for the Hollister section (Savage et al., 1979) and 0.7 cm/year for the section northwest of the Calaveras-Hayward fault intersection (Prescott et al., 1981). The slip rate difference reflects a change from deformation concentrated near the Calaveras fault zone near Hollister to deformation spanning a broader region to the northwest, including slip on the Hayward fault. Potential slip for 15 and 80 year intervals inferred from these rates are shown as wavy lines in Figure 8b. Note that the shape and location of the geodetic slip rate transition, drawn in Figure 8b northwest of the 1984 Morgan Hill rupture zone, is poorly constrained. Slip in the 1984 Morgan Hill earthquake in the brittle 5-9 km depth range clearly exceeds the potential slip inferred for the past 15 years. Moreover, seismic slip in the 5-9 and 6-8 km depth ranges near the epicenter of the energetic late Morgan Hill earthquake source is comparable to the geodetic slip potential inferred for the 75-80 year recurrence time for Morgan Hill earthquakes (Bakun et al., 1984). That is, slip in the brittle zone near the southeast end of Anderson Reservoir during the 1984 Morgan Hill earthquake can account for all of the deformation expected along that section of the Calaveras fault over a complete 80-year earthquake recurrence interval. Conversely, the differences between the 80 year potential slip curve and the seismic slip rates shown in Figure 8b for other areas of the fault surface represent either unrealized slip potential (future earthquakes?) or unrecognized deformation. Unrecognized deformation would include fault creep, folding and faulting of near-fault crustal rocks, and earthquakes missing from the seismicity catalogs.

Hollister section. Seismic slip on the Hollister section of the Calaveras fault (kilometers 140-162 along AA') in the past 15 years is less than that obtained for the Coyote Lake and Morgan Hill rupture zones, particularly in the upper half of the seismogenic zone (Figure 8) and much less than the potential slip inferred for the Hollister section from the geodolite measurements of Savage et al. (1979). Furthermore there are no known historic earthquakes located on the Hollister section that are large enough ($M_L \geq 5$) to alter the seismic slip rate pattern shown in Figure 8. Given the geodetic evidence (Savage et al., 1979) for the rigid block motion near Hollister, we conclude that fault creep at depth or near-fault deformation must account for much of the difference between the seismic slip and the potential slip on the Hollister section.

Calaveras fault northwest of Halls Valley. The seismic slip along the northern section of the Calaveras fault, Calaveras-Sunol fault, and Concord fault (kilometers 0-80 along AA') is significantly less than the seismic slip along the sections to the southeast and significantly less than the potential slip inferred from geodetic observations (Figure 8). It is not clear, however, that these data can be used to infer that a larger earthquake should be expected here. As noted above, the difference between the seismic slip and the slip potential might represent fault creep, off-fault deformation, or earthquakes missing from the seismicity catalogs. Inclusion of slip in figures 8c and 8d for the M_L=5.2 March 9, 1949, the M_L=5.5 September 5, 1955 and the M_L=5.4 October 24, 1955 shocks, the only identified $M_L \geq 5$ shocks in the study area in the past 75 years (Bolt and Miller, 1975), accounts for all shocks large enough to significantly alter the slip distribution pattern. Deformation in the region north of the 1984 Morgan Hill rupture zone is apparently distributed over a broad region, including slip on the Hayward fault (Prescott et al., 1981). Furthermore, there is no reason to assume that aseismic slip or fault creep is not occurring on the northern Calaveras fault.

Seismic slip on the northern 20-km-long segment of section AA' is associated with shocks on the Concord fault (Ellsworth et al., 1982), which is offset in a right step from the north end of Calaveras-Sunol fault (see Figures 1 and 2). This geologic segmentation suggests that the northwest end of AA' is comprised of a fault segment, not unlike the Coyote Lake and Morgan Hill segments, that may fail in characteristic earthquakes with features controlled by fault geometry. We speculate that the M_L=5.4 Concord earthquake of 24 October 1955 (shown in Figures 8c and 8d) may be a characteristic Concord fault earthquake.

DISCUSSION

Reid (1910) postulated in his elastic rebound theory that strain energy near faults is released by fault slip during earthquakes. Seismic hazard evaluations (e.g., Lindh, 1983; Sykes and Nishenko, 1984) and long-term earthquake prediction models (e.g., Shimazaki and Nakata, 1980; Bakun and Lindh, 1985) implicitly assume an earthquake process incorporating a nearly constant rate of strain accumulation driven by relative plate motion that is released in large part by seismic slip in infrequent larger shocks on the plate boundary. Comparisons of potential slip inferred from geodetic observations with seismic slip on different parts of the seismogenic zone (e.g., Figures 8b and 8d) provide a detailed display of an important part of this earthquake generation process. Clearly, there are limited areas of the brittle zone, such as near the energetic late Morgan Hill earthquake source, where the potential slip inferred from geodetic observations is comparable to the slip in the recurring larger shocks. Other areas of the presumably brittle 5-9 km depth range probably have not experienced sufficient seismic slip over the past 80 year recurrence time to match the potential slip inferred from the geodetic observations. Unfortunately there is not sufficient data to discriminate accurately between the different processes- fault creep, incomplete catalogs, and off-fault deformation- that might account for the seismic-versus-geodetic slip differences.

The clear evidence for fault creep near Hollister (Schulz, 1984), the lack of magnitude 5 and larger shocks on the Calaveras fault south of the Coyote Lake rupture zone, and the geodetic evidence that deformation in the Hollister area is associated with slip on the major faults (Savage _et al._, 1979) suggest that the brittle section of the fault, normally 5-9 km deep, is largely missing on the Hollister section. Although the deeper seismic slip near Hollister (see Figure 8) might signify a lowering of the brittle zone from 5-9 kilometers to 9-14 kilometers depth, most of the deeper slip shown occurred in 1969-1970 when focal depths are more uncertain.

While the slip distributions used for the 1984 Morgan Hill and 1979 Coyote Lake main shocks (Figures 4 and 5) suggest that only a small area of the Calaveras fault surface has experienced seismic slip comparable to the potential slip inferred for 80 years from the geodetic observation, equally acceptable slip distributions for these shocks change the extent and location of the fault areas with seismic slip comparable to the 1.2 m potential shown in Figures 8b and 8d. Specifically, Reasenberg and Ellsworth (1982) noted that Liu and Helmberger's (1981) maximum dislocation of 1.2 m for the 1979 Coyote Lake main shock matches the slip deficit that would have been accumulated in the 82 years since the 1897 shock ruptured the Coyote Lake section. Nevertheless, it is clear that not all of the 5-9 km brittle zone along the Morgan Hill and Coyote Lake sections (Figure 8) has kept pace with the slip rate expected from the geodetic observations. Although they represent little deformation, off-fault aftershocks of the 1984 Morgan Hill event with thrust-fault mechanisms (Cockerham and Eaton, 1985; Cockerham et al., 1985), suggest that off-fault folding and faulting accounts for at least part of the difference between the seismic and potential slip. Rupture initiation and termination during the 1979 Coyote Lake and 1984 Morgan Hill earthquakes were controlled by offsets and/or bends in the fault (Bakun, 1980; Bakun et al., 1984). Such off-fault deformation near fault bends and offsets where rupture starts or stops follows naturally from geometric considerations of the faulting process (King, 1983; King and Nabalek, 1985; King, 1985).

The explanation of the considerable difference in the seismic slip and slip potential (Figure 8) northwest of the Morgan Hill rupture zone is not clear. Prescott et al. (1981) note that deformation east of San Francisco Bay occurs over a broad region such that a considerable part of the unaccounted for geodetic slip potential might occur in non-elastic deformation, either as fault creep or off-fault deformation. Nevertheless, there is no evidence that precludes the interpretation of the slip difference in terms of continuing elastic deformation with increasing potential for seismic slip in a moderate size magnitude 6 earthquake.

CONCLUSIONS

The distribution of seismic slip on the Calaveras fault for 1969-1984 suggest that:

1. Larger shocks tend to occur within regions of slip deficit left by earlier earthquakes. This is most clearly seen for the 1984 Morgan Hill earthquake, even though only 15 years of detailed seismic history exist and the apparent recurrence interval of larger shocks on the south half of the Calaveras fault is 75-80 years. Consideration of earlier significant shocks on the Calaveras fault enhances the pre-Morgan Hill slip deficit, indicating that it would be more apparent if a larger period of detailed seismicity were available. It is also apparent that in the Morgan Hill case shallow seismicity above the 5-9 km deep brittle zone partly obscures the slip deficit, emphasizing the importance of looking at slip as a function of depth.

2. Comparison of the seismic slip distribution with the potential slip inferred from geodetic observations illustrates details of the earthquake generation process. There are limited areas of the 5-9 kilometer deep brittle zone, such as near the energetic late Morgan Hill earthquake source, where the seismic slip is comparable to the potential slip. Seismic slip on adjoining areas of the brittle zone over the past 80 year recurrence time has not matched the potential slip. The seismic-versus-geodetic slip differences may be explained by a combination of processes-fault creep, incomplete seismicity catalogs, and off-fault deformation - as well as a not yet realized potential for seismic slip in future shocks.

3. Seismic slip on the Hollister section since 1969 is significantly less than the seismic slip elsewhere on the Calaveras fault and is much less than the slip potential inferred from geodolite measurements. There are no known earlier shocks on the Hollister section large enough to alter the potential slip-versus-seismic slip difference. Given the geodetic

evidence for rigid block motion near Hollister, irreversible fault creep or near-fault deformation must account for much of the discrepancy.

4. There exists a considerable potential slip-versus-seismic slip difference on the section of the Calaveras fault northwest of the rupture zone of the 1984 Morgan Hill earthquake. A significant part of this difference might be explained by fault creep or off-fault deformation. An interpretation of the difference in terms of continuing elastic deformation with increasing potential for a damaging shock should not be rejected.

FIGURE CAPTIONS

Figure 1. Seismicity (magnitude > 1.3) in the San Francisco Bay area for 1976-1984. The polygon encloses epicenters of shocks associated with the Calaveras fault, Calaveras-Sunol fault, and Concord Fault (see Figure 2). Hypocenters of shocks with epicenters located by the USGS CALNET inside the polygon are projected onto the vertical plane beneath profile A-A'.

Figure 2. Seismicity within the polygon (Figure 1) for 1969-1984. a) map and b) vertical cross sections of hypocenters of all earthquakes in the USGS CALNET earthquake catalogs. c) and d) are map and cross sections of hypocenters subject to accuracy criteria. We use shocks with DMIN, the epicentral distance to the closest seismograph that recorded the shock, less than 5 km, the std. error of the epicenter less than 2.5 km, and the std. error of the hypocenter < 2.5 km. Also included are shocks with DMIN < 2 focal depths.

Figure 3. Distribution of slip(cm) contoured on the fault plane for a) M_L=4 and b) M_L=5 earthquakes. The rectangular source dimensions shown by dashed lines are calculated from the moment-magnitude relations as described in the text. The dimensions are then converted to integral dimensions which enclose the centers (crosses) of the cells over which slip is distributed. The central cross and outermost crosses have a weight of lw/LW and 0 respectively.

Figure 4. a) Cross section showing aftershocks of the 1984 Morgan Hill earthquake with epicenters located within a 2.1-km-wide band along the Calaveras fault (taken from Cockerham and Eaton, 1984). Dashed line outlines a central quiet area interpreted by Cockerham and Eaton to be the section that slipped during the main shock. The hypocenter of the main shock is shown as a star.

b) Contours of constant seismic slip(cm) obtained using two sources: an M_L5.8 source to the northwest and an M_L6.1 source to the southeast. The location, length, and width of the sources were adjusted so that the boundary of significant slip mimics the dashed line in a).

c) The 10-, 50-, and 100-cm-slip contours from b) superimposed on a).

Figure 5. a) Cross section along the Calaveras fault showing the 1979 Coyote Lake main shock and magnitude 0.5 and larger aftershocks located on Zone 1, the easternmost section (taken from Reasenberg and Ellsworth, 1982). Symbol size is proportional to magnitude. The hypocenter of the main shock is shown as a star. Dashed line outlines a central area around which larger aftershocks are located. Although aftershocks on Zones II and III are located farther southeast, there is no evidence that rupture during the main shock extended to these segments.

b) Contours of constant seismic slip(cm) obtained using two sources: an M_L=5.75 event near the main shock hypocenter and an M_L= 5.2 source located 9 kilometers to the southeast. The two sources were arbitrarily adjusted so that the slip contours mimic the spatial pattern of larger aftershocks on Zone 1.

The 12-cm-slip contour from b) superimposed on a).

Figure 6. Contours of constant seismic slip before the Morgan Hill earthquake on the section of AA' (Figure 1) from 70 to 135 kilometers for (a) 1 Jan 1969 to 5 Aug 1979, (b) 6 Aug 1979 to 6 Nov 1979, (c) 7 Nov 1979 to 23 Apr 1984, and (d) 1 Jan 1969 to 23 Apr 1984. Contour interval = 0.25 cm. Hypocenter (star) and 1-cm seismic slip contour (dashed line) of the Coyote Lake mainshock (Figure 5) are superposed on a), b), and c).

Figure 7. Seismic slip on the section of AA' (Figure 1) from 70 to 135 kilometers. (a) Boundary contour (0.25 cm) of seismic slip from figure 6d. (b) Slip contours for 24 Apr 1984 to 23 July 1984. Contour interval = 0.25 cm. Hypocenter (star) and 1-cm contour of seismic slip (dashed line) of the Morgan Hill main shock (Figure 4b) are superposed on (a) and (b). (c) Seismic slip (per km^2 of fault area) for the Morgan Hill main shock (Figure 4b) averaged over the depth interval of 0 to 15 km is shown as a bold dashed line. The time from 1 Jan 1969 to 23 April 1984 (Figure 6d) averaged over depths of 0 to 15 km and 6 to 15 km are shown as dotted and thin solid lines

respectively. The time from 1 Jan 1969 to 23 April 1984 plus the 9 March 1949 (M_L=5.2) and the 5 Sept 1955 (M_L=5.5) shocks averaged over depths of 0 to 15 km is shown as a thin dashed line.

Figure 8. Seismic slip on the section AA'. (a) Cross section for 1 Jan 1969 to 1 Jan 1985 with contour interval = 0.50 cm. (b). Slip on (a) averaged over depth intervals of 0-4, 4-9, 9-14, and 5-8 km compared with slip potential (wavy lines) for 15 and 80 years inferred from geodetic observations. The geodetic slip potential uses 1.5 cm/yr (Savage et al., 1979) for 80-162 km and 0.7 cm/yr (Prescott et al., 1981) for 30-75 km. The transition at 75-80 km is arbitrarily drawn midway between the intersections of the Mission and Hayward faults with the Calaveras fault.

(c) and (d). (a) and (b) with seismic slip for the 9 March 1949 (M_L= 5.2), the 5 Sept 1955 (M_L= 5.5), and the 24 October 1955 (M_L= 5.4) shocks added. The length, and especially the width, of spatial slip shown for these pre-1969 shocks is arbitrary.

(e) Figure 2b repeated for comparison.

TABLE 1. SOURCE PARAMETERS

M_L	M_o(dyne-cm)†	u_1(cm)‡	$W=\frac{L}{2}$(km)	N_L§	N_W§
1	1.70×10^{18}	5.66×10^{-4}	0.17	1	1
2	2.75×10^{19}	9.18×10^{-3}	0.42	1	1
3	4.47×10^{20}	0.149	1.06	1	1
4	1.00×10^{22}	3.33	2.99	7	3
5	3.16×10^{23}	105.41	9.45	19	9
6	1.00×10^{25}	3333.	29.9	*	*

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† $\log M_o = \begin{cases} 1.2\,M_L + 17 & \text{for } M_L \text{ or coda duration magnitude } M_D < 3.5 \\ 1.5\,M_L + 16 & \text{for } M_L \geq 3.5 \end{cases}$

‡ u_1 for faulting area $A = 1 \times 1\ km^2$ and $\mu = 3 \times 10^{11}$ dynes/cm^2

§ N_L and N_W are the number of 1-km-long cells in length and width necessary to distribute u_1 over source length L and source width W respectively. N_L, N_W, and the position of the center cell are easily changed when additional source parameter constraints are available (See *.)

* Fault width exceeds the 20-km depth of the seismogenic zone assumed in these calculations so that N_L and N_W must be adjusted.

REFERENCES

Aki, K. (1966). Generation and propagation of G waves from the Niigata earthquake of June 16, 1964. Part 2. Estimation of earthquake movement, released energy, and stress-strain drop from G wave spectrum, Bull. Earthquake Res. Inst. Tokyo Univ. 44, 73-88.

Bakun, W. H. (1980). Seismic activity on the southern Calavaera fault in central California, Bull. Seism. Soc. Am. 70, 1181-1197.

Bakun, W. H., M. M. Clark, R. S. Cockerham, W. L. Ellsworth, A. G. Lindh, W. H. Prescott, A. F. Shakal, and P. Spudich (1984). The 1984 Morgan Hill, California earthquake, Science 225, 288-291.

Bakun, W. H. (1984). Seismic moments, local magnitudes, and coda-duration magnitudes for earthquakes in central California, Bull. Seism. Soc. Am. 74, 439-458.

Bakun, W. H., and A. G. Lindh (1985). The Parkfield, California, earthquake prediction experiment, Science, 229, 619-624.

Bolt, B. A., and R. D. Miller (1975). Catalogue of earthquakes in northern California and adjoining areas 1 January 1910-31 December 1972, Seismographic Stations, Univ. of California, Berkeley, 567 pp.

Bouchon, M. (1982). The rupture mechanism of the Coyote Lake earthquake of August 6, 1979, inferred from near-field data, Bull Seism. Soc. Am. 72, 745-758.

Brune, J. N. (1968). Seismic moment, seismicity, and rate of slip along major fault zones, J. Geophy. Res. 73, 777-784.

Bufe, C. G., F. W. Lester, K. L. Meagher, and R. L. Wesson (1975). Catalog of earthquakes along the San Andreas fault system in central California, April-June 1973, U.S. Geol. Surv., Open-File Rept. 75-125, 44 pp.

Cockerham, R. S. and J. P. Eaton (1984). The April 24, 1984 Morgan Hill earthquake and its aftershocks: April 24 through September 30 1984, in The 1984 Morgan Hill, California Earthquake, Calif. Div. Mines and Geol. Spec. Publ. 68, J. H. Bennett and R. W. Sherburne, eds., p. 215-236.

Cockerham, R., J. P. Eaton, P. Reasenberg, and D. Oppenheimer (1985). The April 24, 1984 Morgan Hill earthquake and its aftershocks, EOS, Trans. Am. Geophys. Un. 66 (abstract), p. 307-308.

Ekstrom, G. (1984). Central-moment tensor solution for the April 24, 1984 Morgan Hill, California, earthquake, in the 1984 Morgan Hill, California earthquake, Calif. Div. Mines and Geol. Spec. Publ. 68, J. H. Bennett and R. W. Sherburne, eds., p. 209-213.

Ellsworth, W. L., J. A. Olson, L. M. Shijo, and S. M. Marks (1982). Seismicity and active faults in the eastern San Francisco Bay area, in Hart, E. M., S. E. Hirschfeld, and S. S. Schulz, eds., Proceedings, Conference on Earthquake Hazards in the Eastern San Francisco Bay Area, Calif. Div. Mines and Geol. Spec. Publ. 62, p. 83-92.

Fluty, L. and S. M. Marks (1981). Catalog of earthquakes along the San Andreas fault system in central California October-December 1977, U.S. Geol. Surv. Open-File Rept. 81-1325, 48 pp.

Hall, C. A., Jr. (1958). Geology and paleontology of the Pleasanton area, Alameda and Contra Costa counties, California, Univ. of Calif. Publ. in Geol. Sci. 34, 89 pp.

Hartzell, S. H. and T. H. Heaton (1986). Rupture history of the 1984 Morgan Hill, California, earthquake from the inversion of strong motion records, Bull. Seism. Soc. Am., in press.

King, G. C. P. (1983). The accommodation of large strains in the upper lithosphere of the Earth and other solids by self-similar fault systems: the geometrical origin of b-value, Pageoph 121, 761-815.

King, G. C. P., and J. Nabalek (1985). The role of bends in faults in the initiation of termination of earthquake rupture, Science 228, 984-987.

King, G. C. P. (1986). The geometry of the initiation and termination of earthquake rupture and the evolution of morphology and geological structures, this volume.

Lee, W. H. K., J. C. Roller, P. G. Bauer, and J. D. Johnson (1972a). Catalog of earthquakes along the San Andreas fault system in central California for the year 1969, U.S. Geol. Surv., Open-File Rept., 48 pp.

Lee, W. H. K., J. C. Roller, K. L. Meagher, and R. E. Bennett (1972b). Catalog of earthquakes along the San Andreas fault system in central California for the year 1970, U.S. Geol. Surv., Open-File Rept., 73 pp.

Lee, W. H. K., K. L. Meagher, R. E. Bennett, and E. E. Matamoros (1972c). Catalog of earthquakes along the San Andreas fault system in central California for the year 1971, U.S. Geol. Surv., Open-File Rept., 73 pp.

Lee, W. H. K., R. E. Bennett, and K. L. Meagher (1972d). A method of estimating magnitude of local earthquakes from signal duration, U.S. Geol. Surv., Open-File Rept., 28 pp.

Lee, W. H. K., M. S. Eaton, and E. E. Brabb (1971). The earthquake sequence near Danville, California, 1970, Bull. Seism. Soc. Am. 61, 1771-1794.

Lester, F. W., K. L. Meagher, and R. L. Wesson (1976a). Catalog of earthquakes along the San Andreas fault system in central California, July-September 1973, U.S. Geol. Surv., Open-File Rept. 76-169, 38 pp.

Lester, F. W., S. L. Kirkman, and K. L. Meagher (1976b). Catalog of earthquakes along the San Andreas fault system in central California, October-December 1973, U.S. Geol. Surv. Open-File Rept. 76-732, 37 pp.

Lester, F. W. and K. L. Meagher (1978). Catalog of earthquakes along the San Andreas fault system in central California for the year 1974, U.S. Geol.

Surv., Open-File Rept. 78-1010, 89 pp.

Lindh, A. G., (1983). Preliminary assessment of long-term probabilities for large earthquakes along selected fault segments of the San Andreas fault system in California, U.S. Geol. Surv, Open-File Report 83-86, 15 pp.

Liu, H.-L., and D. V. Helmberger (1983). The near-source ground motion of the 6 August 1979 Coyote Lake, California, earthquake, Bull. Seism. Soc. Am. 73, 201-218.

Madariaga, R. (1976). Dynamics of an expanding circular fault, Bull. Seism. Soc. Am. 66, 639-666.

Marks, S. M. and F. W. Lester (1980a). Catalog of earthquakes along the San Andreas fault system in central California, January-March 1977, U.S. Geol. Surv. Open-File Rept. 80-1233, 47 pp.

Marks, S. M. and F. W. Lester (1980b). Catalog of earthquakes along the San Andreas fault system in central California, April-June 1977, U.S. Geol. Surv. Open-File Rept. 80-1264, 47 pp.

Marks, S. M. and L. Fluty (1981). Catalog of earthquakes along the San Andreas fault system in central California, July-September 1977, U.S. Geol. Surv. Open-File Rept. 81-462, 43 pp.

Mavko, G. M. (1982). Fault interaction near Hollister, California, J.

Geophys. Res. 87, 7807-7816.

McHugh, C. A. and F. W. Lester (1978). Catalog of earthquakes along the San Andreas fault system in central California, for the year 1976, U.S. Geol. Surv., Open-File Rept. 78-1051, 91 pp.

McHugh, C. A. and F. W. Lester (1979). Catalog of earthquakes along the San Andreas fault system in central California for the year 1975, U.S. Geol. Surv., Open-File Rept. 79-1138, 91 pp.

Nabalek, J., and M. N. Toksoz, (1981). Applications of WWSSN seismograms to the study of the Coyote Lake earthquake of August 6, 1979 (abstract), IASPEI Meeting, Int. Assoc. Seismol. and Phys. of the Earth's Inter., London, Ont.

Prescott, W. H., N. E. King, and G. H. Gu, (1984). Preseismic, coseismic, and postseismic deformation associated with the 1984 Morgan Hill, California, earthquake, in the 1984 Morgan Hill, California earthquake, Calif. Div. Mines and Geol. Spec. Publ. 68, J. H. Bennett and R. W. Sherburne, eds., p. 137-148.

Prescott, W. H., M. Lisowski, and J. C. Savage (1981). Geodetic measurements of crustal deformation on the San Andreas, Hayward, and Calaveras faults near San Francisco, California, J. Geophys. Res. 86, 10853-10869.

Reid, H. F., (1910). The California earthquake of April 18, 1906, 2, Mechanics of the Earthquake, Carnegie Institute of Washington, Washington,

D. C.

Reasenberg, P. and W. L. Ellsworth (1982). Aftershocks of the Coyote Lake, California, earthquake of August 6, 1979; A detailed study, J. Geophys. Res. 87, 10637-10655.

Reasenberg, P. (1985). Second-order moment of central California seismicity, 1969-1982, J. Geophys. Res. 90, 5479-5495.

Rogers, T. H., and R. D. Nason (1971). Active displacement on the Calaveras fault zone at Hollister, California, Bull. Seism. Soc. Am. 61, 399-416.

Savage, J. C., W. H. Prescott, M. Lisowski, and N. King (1979). Geodolite measurements of deformation near Hollister, California, 1971-1978, J. Geophys. Res. 84, 7599-7615.

Scholz, C. H. (1982). Scaling laws for large earthquakes: Consequences for physical models, Bull. Seism. Soc. Am. 72, 1-14.

Schulz, S. S. (1984). Triggered creep near Hollister after the April 24, 1984, Morgan Hill, California, earthquake, in the 1984 Morgan Hill, California earthquake, Calif. Div. Mines and Geol. Spec. Pub. 68, J. H. Bennett and R. W. Sherburne, eds., p. 175-182.

Shimazaki, K., and T. Nakata (1980). Time-predictable recurrence model for large earthquakes, Geophys. Res. Lett. 7, 279-282.

Sykes, L. R., and S. P. Nishenko (1984). Probabilities of occurrence of large plate rupturing earthquakes for the San Andreas, San Jaciento, and Imperial faults, California, J. Geophys. Res. 89, 5905-5927.

Thatcher, W. and T. C. Hanks (1973). Source parameters of southern California earthquakes, J. Geophys. Res. 78, 8547-8576.

Uhrhammer, R. A. (1980). Observations of the Coyote Lake, California earthquake sequence of August 6, 1979, Bull. Seism. Soc. Am. 70, 559-570.

Wesson, R. L., R. E. Bennett, and K. L. Meagher (1972a). Catalog of earthquakes along the San Andreas fault system in central California January-March 1972, U.S. Geol. Surv., Open-File Rept., 60 pp.

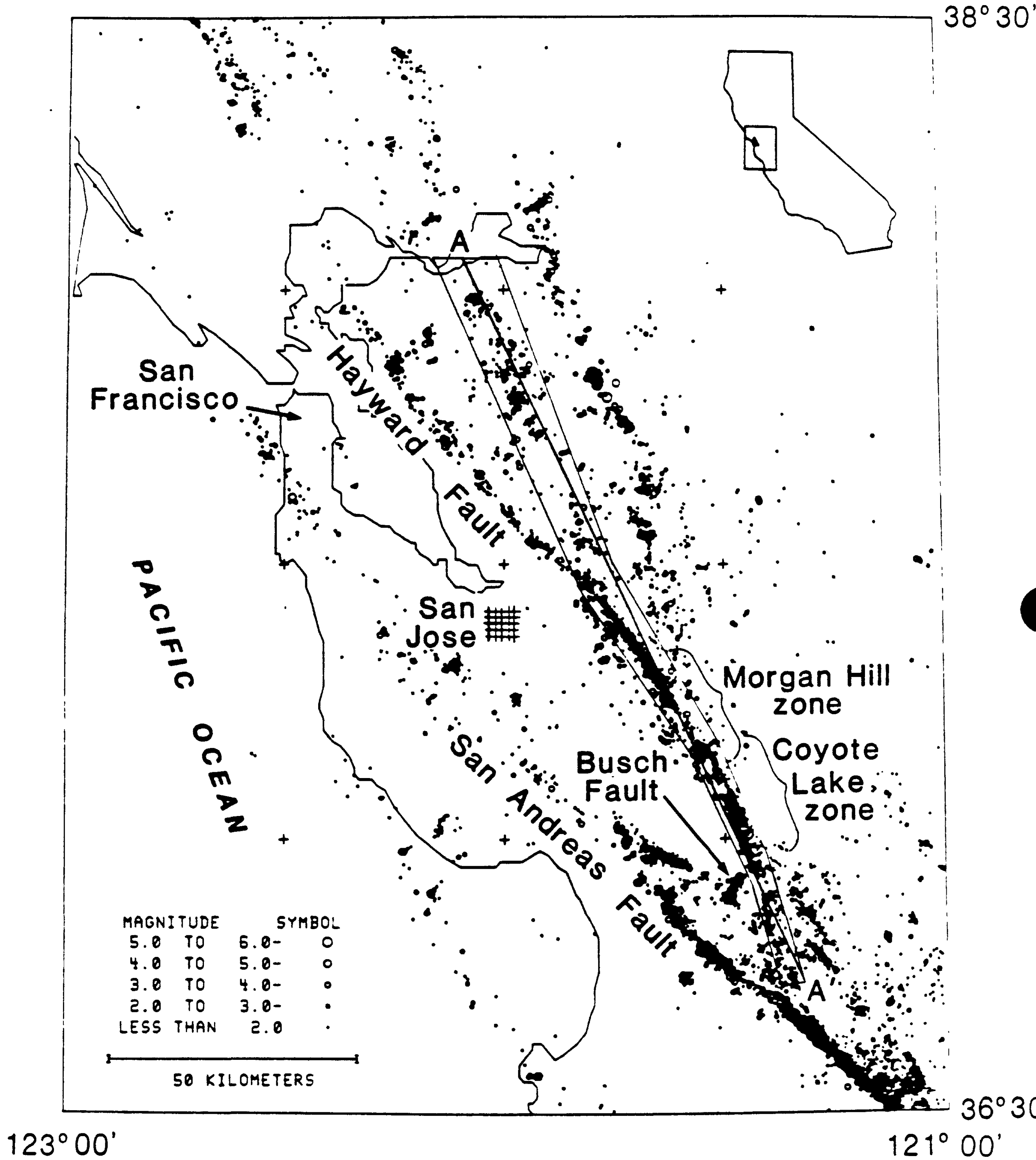
Wesson, R. L., R. E. Bennett, and F. W. Lester (1972b). Catalog of earthquakes along the San Andreas fault system in central California April-June 1972, U.S. Geol. Surv., Open-File Rept., 42 pp.

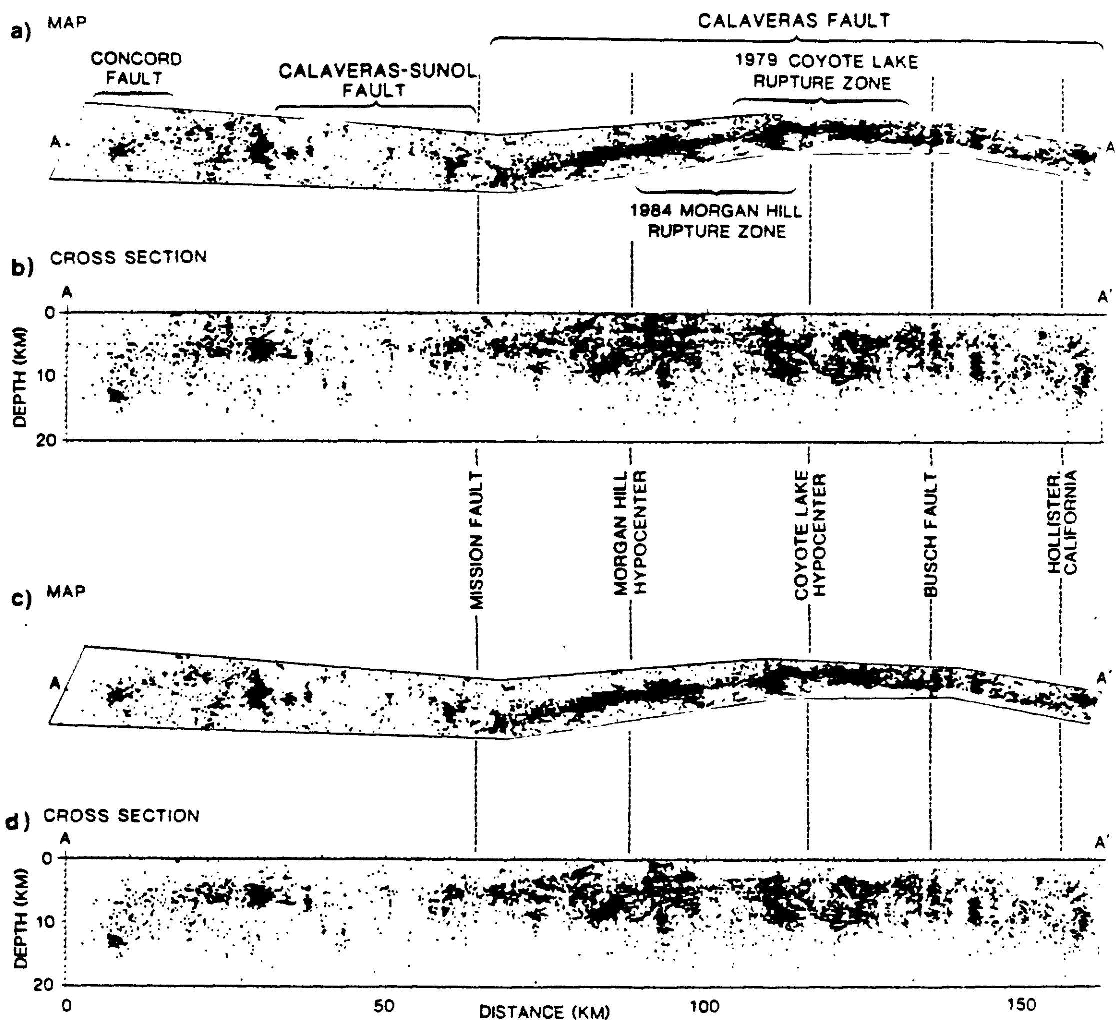
Wesson, R. L., K. L. Meagher, and F. W. Lester (1973). Catalog of earthquakes along the San Andreas fault system in central California July-September 1972, U.S. Geol. Surv., Open-File Rept., 49 pp.

Wesson, R. L., F. W. Lester, and K. L. Meagher (1974a). Catalog of earthquakes along the San Andreas fault system in central California October-December 1972, U.S. Geol. Surv. Open-File Rept., 46 pp.

Wesson, R. L., F. W. Lester, and K. L. Meagher (1974b). Catalog of earthquakes along the San Andreas fault system in central California

January-March 1973, U.S. Geol. Surv., Open-File Rept. 74-186, 47 pp.


38°30'
A
San Francisco
Hayward Fault
PACIFIC OCEAN
San Jose
San Andreas Fault
Morgan Hill zone
Coyote Lake zone
Busch Fault
A'
MAGNITUDE SYMBOL
5.0 TO 6.0-
4.0 TO 5.0-
3.0 TO 4.0-
2.0 TO 3.0-
LESS THAN 2.0
50 KILOMETERS
36°30
123°00'
121°00'


a) MAP
CALAVERAS FAULT
CONCORD FAULT
CALAVERAS-SUNOL FAULT
1979 COYOTE LAKE RUPTURE ZONE
A
A'
1984 MORGAN HILL RUPTURE ZONE
b) CROSS SECTION
A
A'
0
10
20
DEPTH (KM)
MISSION FAULT
MORGAN HILL HYPOCENTER
COYOTE LAKE HYPOCENTER
BUSCH FAULT
HOLLISTER, CALIFORNIA
c) MAP
A
A'
d) CROSS SECTION
A
A'
0
10
20
DEPTH (KM)
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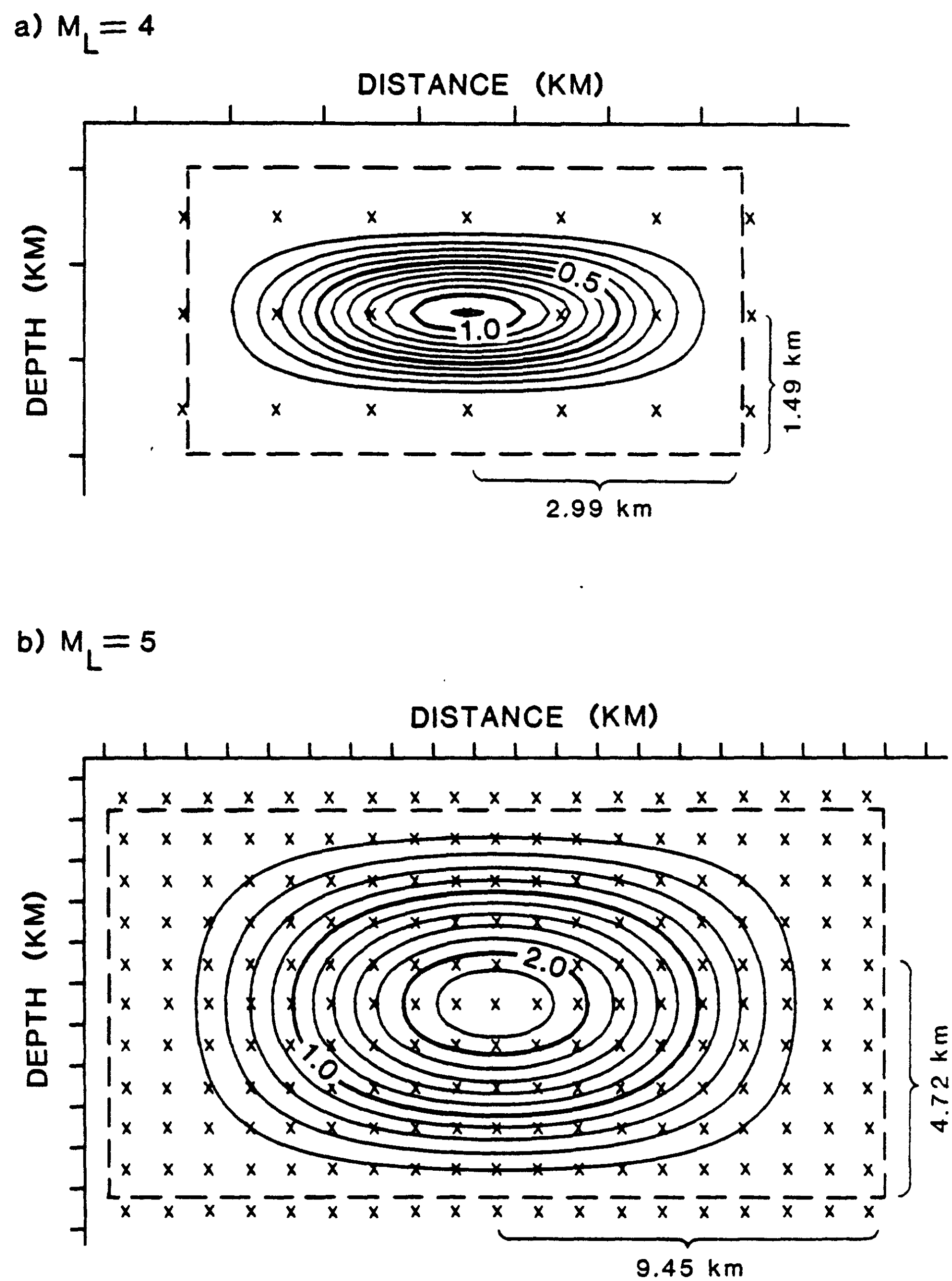
50
100
150
DISTANCE (KM)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DEPTH (KM)
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1.0
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2.99 km
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DEPTH (KM)
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1.0
4.72 km
9.45 km

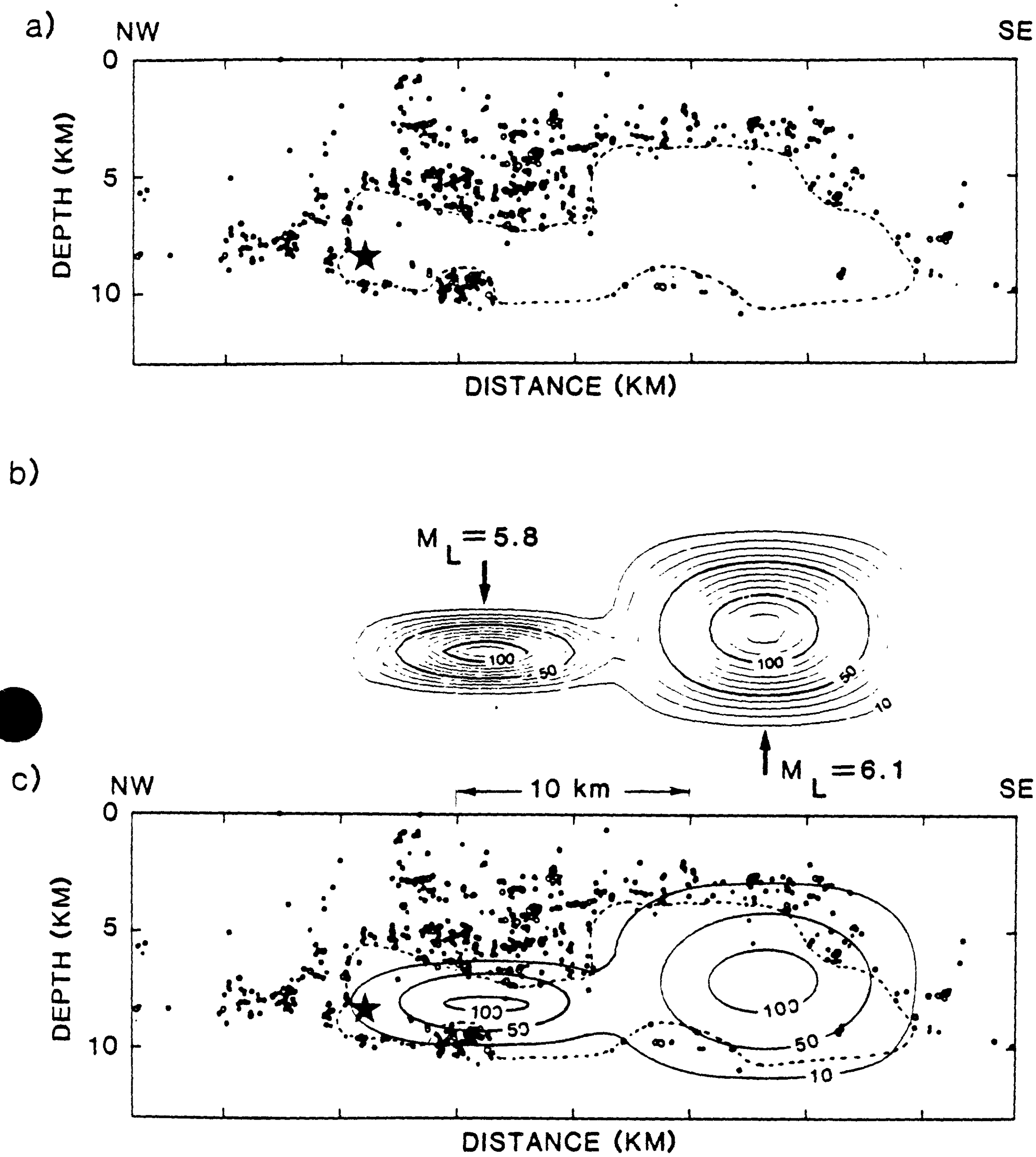
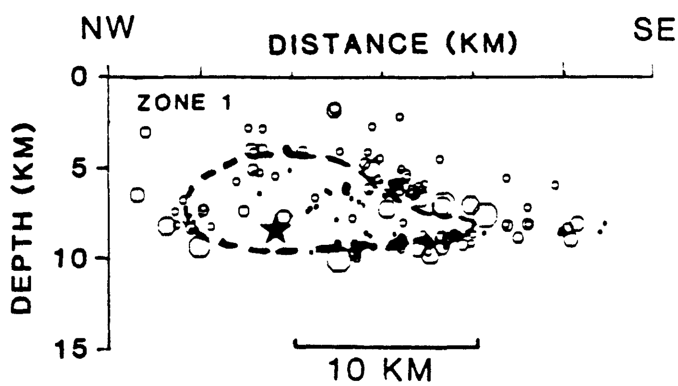

a)
NW
SE
0
5
10
DEPTH (KM)
DISTANCE (KM)
b)
M L =5.8
100
50
100
50
10
M L =6.1
c)
NW
10 km
SE
0
5
10
DEPTH (KM)
100
50
100
50
10
DISTANCE (KM)

Fig. 4

a)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SE
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DEPTH (KM)
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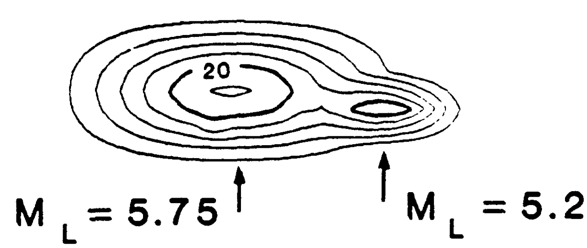
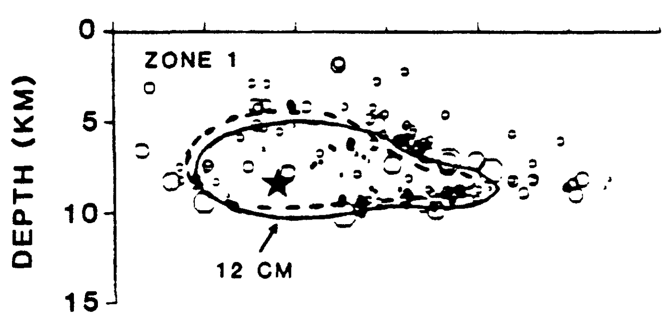
5
10
15
10 KM

b)


20
$M_L = 5.75$
$M_L = 5.2$

c)


ZONE 1
DEPTH (KM)
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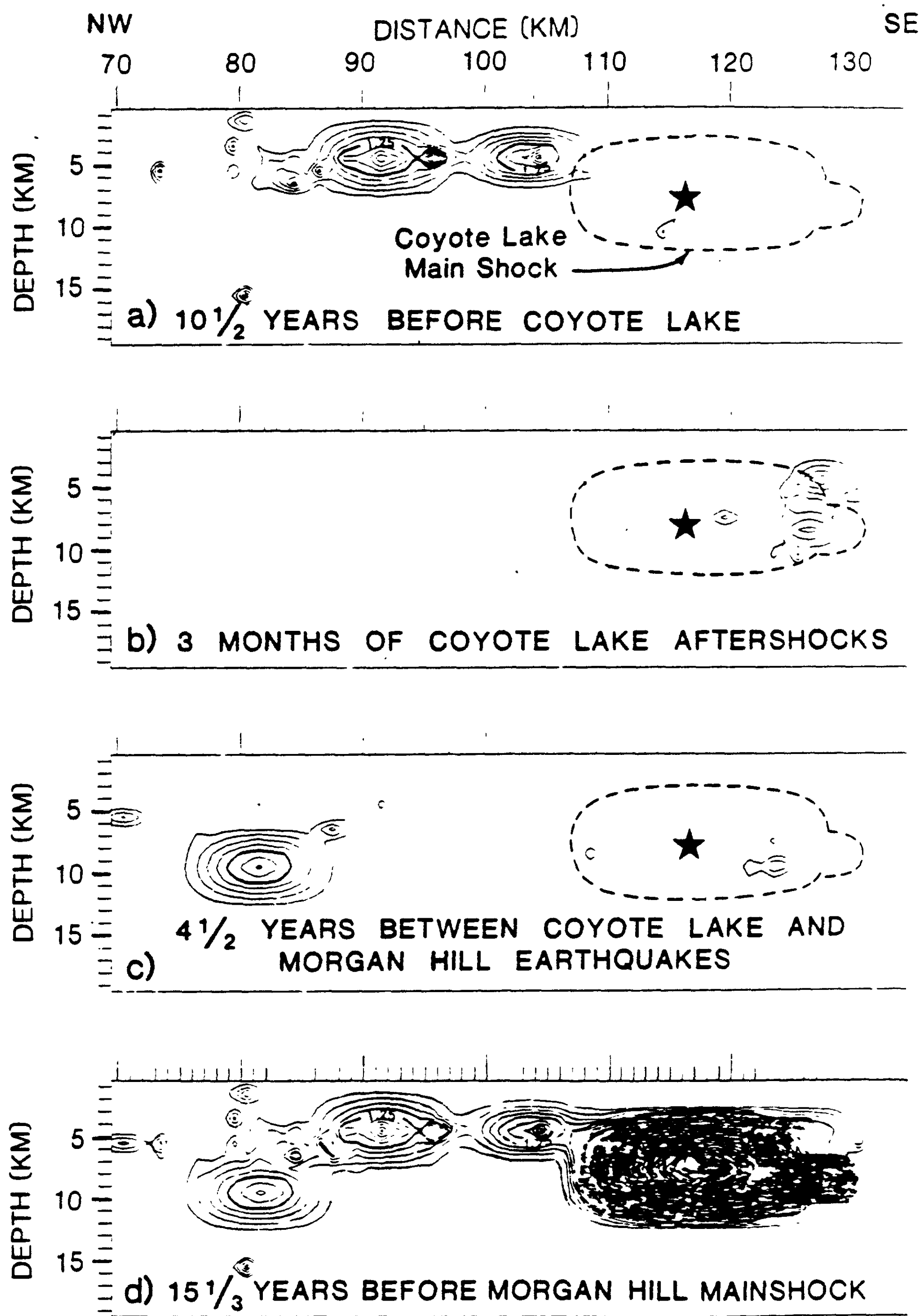
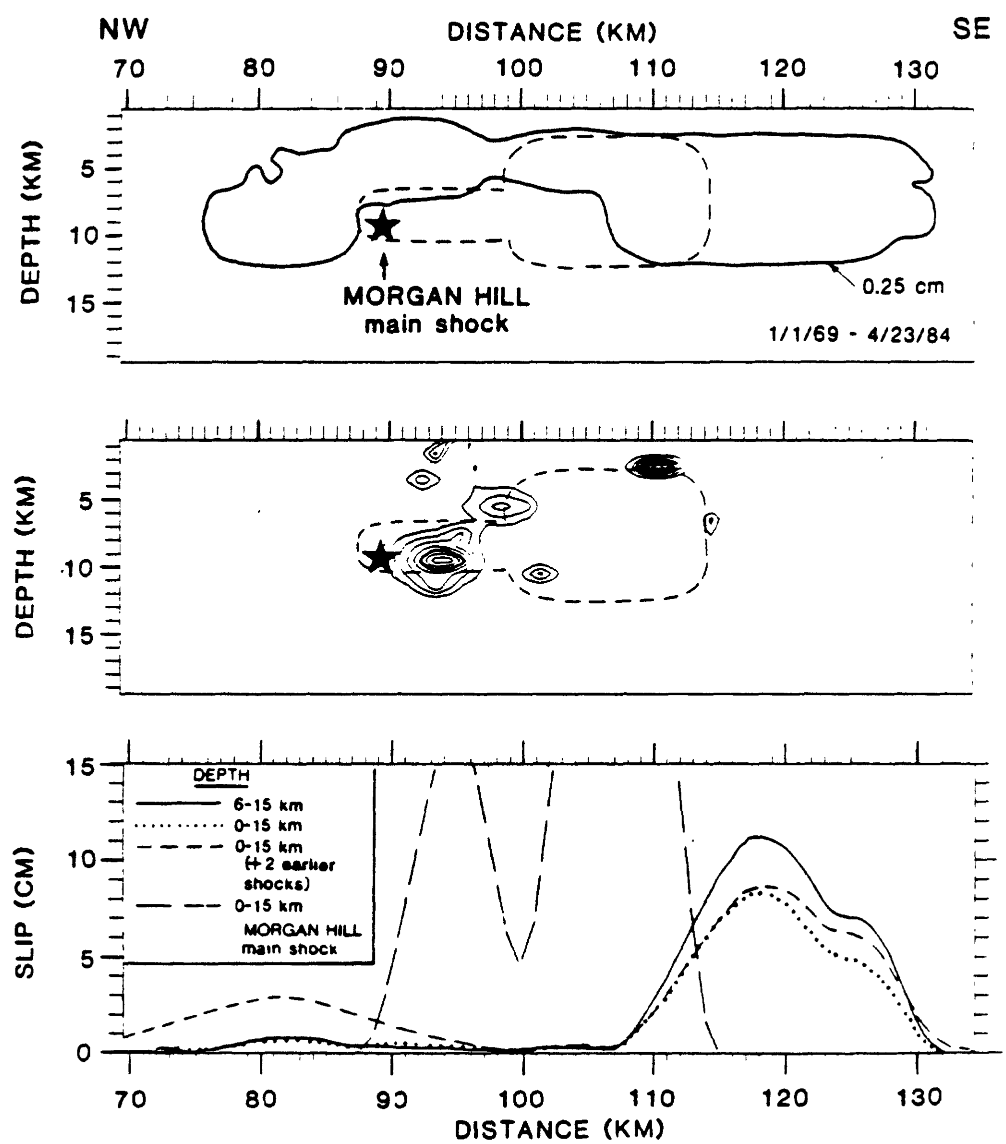
5
10
15
12 CM


NW
DISTANCE (KM)
SE
70
80
90
100
110
120
130
DEPTH (KM)
5
10
15
Coyote Lake
Main Shock
a) 10 1/2 YEARS BEFORE COYOTE LAKE
b) 3 MONTHS OF COYOTE LAKE AFTERSHOCKS
4 1/2 YEARS BETWEEN COYOTE LAKE AND
c) MORGAN HILL EARTHQUAKES
d) 15 1/3 YEARS BEFORE MORGAN HILL MAINSHOCK


NW
DISTANCE (KM)
SE
70 80 90 100 110 120 130
DEPTH (KM)
5
10
15
MORGAN HILL
main shock
0.25 cm
1/1/69 - 4/23/84
DEPTH (KM)
5
10
15
SLIP (CM)
15
10
5
0
DEPTH
6-15 km
0-15 km
0-15 km
(+2 earlier
shocks)
0-15 km
MORGAN HILL
main shock
70 80 90 100 110 120 130
DISTANCE (KM)

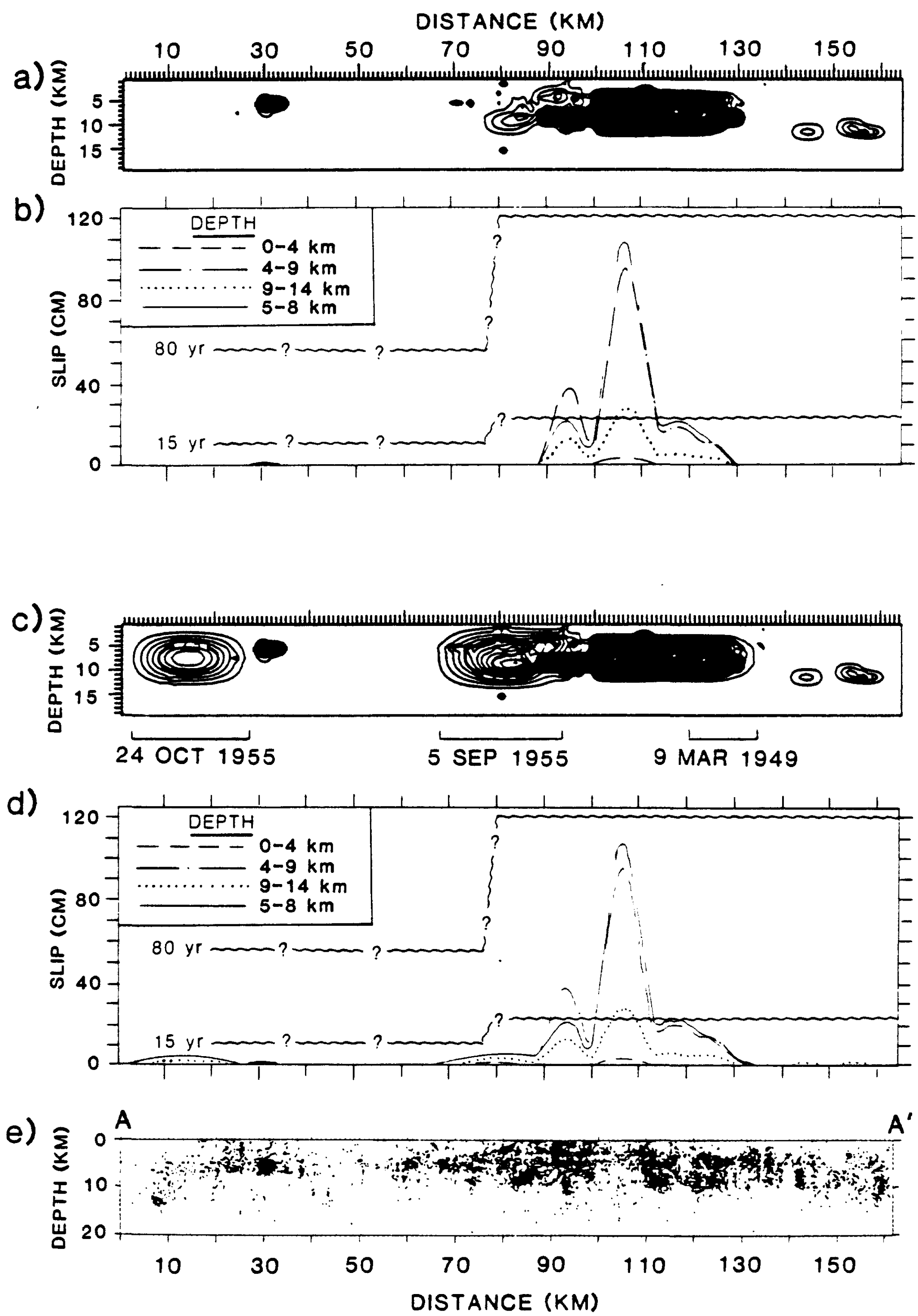

DISTANCE (KM)
10 30 50 70 90 110 130 150
a)
DEPTH (KM)
5 10 15
b)
SLIP (CM)
120 80 40 0
DEPTH
0-4 km
4-9 km
9-14 km
5-8 km
80 yr
15 yr
c)
DEPTH (KM)
24 OCT 1955
5 SEP 1955
9 MAR 1949
d)
SLIP (CM)
80 yr
15 yr
e)
A
A'
DEPTH (KM)
0 10 20
10 30 50 70 90 110 130 150
DISTANCE (KM)

Fig.

UNITED STATES DEPARTMENT OF THE INTERIOR

GEOLOGICAL SURVEY

Parkfield Earthquake Prediction Scenarios and Response Plans

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Contents

Role of National Earthquake Prediction Evaluation Council in Development of Earthquake Prediction Scenarios and Response Plans for Parkfield Earthquake

For the past 2 years the National Earthquake Prediction Evaluation Council (NEPEC) has been involved in a major review of the earthquake monitoring and the earthquake prediction experiment at Parkfield, in reviewing a long-term prediction that was brought to it by personnel from the U.S. Geological Survey (USGS), in recommending that a long-term prediction be officially declared by the Director of USGS, and in urging that a decision matrix and response plan be developed to increase the chances of a successful short-term prediction for Parkfield.

In November 1984 the Council reviewed both the earthquake experiment at Parkfield and a draft prepared by USGS personnel in which a long-term prediction was made for a future Parkfield shock. (NEPEC uses the term "long-term earthquake prediction" to refer to a time interval of a few years to about 1 decade.) NEPEC concurred with the general aspects of the USGS prediction and recommended to the Director of USGS that a long-term prediction be issued for Parkfield and that the State of California be notified of its findings. (It should be noted that NEPEC reports to the Director of USGS and that the Director is formally charged with the issuance of earthquake predictions in the United States). NEPEC notes that while the next Parkfield earthquake is most likely to be similar in size to the shocks of 1934 and 1966, the possibility exists that a 25 mile (40 km) segment of the San Andreas fault to the southeast of Parkfield may also be sufficiently advanced in its cycle of strain buildup that it could rupture along with the Parkfield segment in an earthquake near magnitude 7. NEPEC recommended that the highest priority be given to the monitoring and prediction experiment at Parkfield. This was the first instance in which NEPEC has recommended that a prediction of any type be made for a future earthquake in the United States.

In early 1985 the State of California asked USGS to give high priority to making a short-term prediction (i.e., one of hours to days) for the next major Parkfield earthquake. In July 1985 NEPEC conducted a review of methods that could be used for short-term and intermediate-term prediction at Parkfield and the reliability of various prediction criteria. NEPEC concluded that any realistic attempts at short-term prediction in the near future in the United States are likely to be of a probabilistic nature and would not be warnings in which there was certainty or near certainty that a physical observation would be followed shortly by a major earthquake. NEPEC also concluded that under some scenarios there could be an abrupt increase in the probability of the earthquake within a few hours, or less, and response to such situations would need to be planned well ahead of time and delegation of authority worked out. It is not a reasonable expectation to involve members of the Council, many of whom do not live in California, in making such an immediate response. At NEPEC's recommendation, a senior USGS scientist (Dr. W.H. Bakun) was appointed USGS project leader for Parkfield in July 1985.

NEPEC also recommended that USGS develop a decision tree or decision matrix document that would describe possible anomalous conditions, estimate probabilities that various anomalies are either followed by earthquakes or associated with false alarms, and designate actions to be taken for various alarm levels. A draft of this document was prepared by USGS personnel and presented to NEPEC in September 1985.

NEPEC strongly endorsed the general concept of the document and recommended it be presented to the Director of USGS. NEPEC further advised that procedures and criteria be developed for ending a prediction, either by specifying a time frame in the initial announcement or by formally retracting the prediction of an event that had not occurred by a certain date. On March 1, 1986, the Council recommended adoption of a revised document and that this document be reviewed at subsequent NEPEC meetings.

It should be remembered that this is the first time that an attempt has been made in the United States to devise a plan for short-term response to measured physical parameters that may be indicative of a future earthquake. The parameters and criteria will undoubtedly need to be changed as experience accumulates at Parkfield and elsewhere. The Council is of the opinion that the science of earthquake prediction, especially short-term prediction, is very much in its infancy. Nevertheless, it believes that a rational case can be made for realistic short-term prediction at Parkfield. The scenarios and response plans might well serve as a model for other areas in the future.

It needs to be recognized that predictions that may result from this effort will be probabilistic in nature. A great effort must be made to educate the public and its officials about the nature of probabilistic estimates, to get them to realize that major uncertainties in knowledge exist in earthquake forecasting, and that no technique that presently exists is capable of being used to predict earthquakes with complete certainty or near certainty.

Parkfield represents an area that is relatively well known and well instrumented. It provides an opportunity to test a number of techniques that might be used in the future for earthquake prediction and to provide data for testing hypotheses about fault mechanics, the earthquake-generating process, and changes that may be precursory to earthquakes.

Lynn R. Sykes
Higgins Professor of
Geological Sciences,
Columbia University,
Chairman, National Earthquake
Prediction Council

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA. 22092

In Reply Refer To:
Mail Stop 905

June 12, 1986

Memorandum

To: Director

Through: Chief Geologist

From: Chief, Office of Earthquakes, Volcanoes, and Engineering

Subject: Parkfield Earthquake Prediction Scenarios and Response Plans

The attached subject report has been reviewed and endorsed by the National Earthquake Prediction Evaluation Council. The Council has agreed to review the report periodically and recommend revisions as the science of earthquake prediction and the conditions at Parkfield evolve.

The most significant aspect of this report is that, under the highest alert level, its adoption will delegate responsibility and authority for notification of State officials to the Chief Scientist of the Parkfield Earthquake Prediction Experiment.

Please indicate below your approval of this report and adoption of the procedures it describes.

John R. Filson

Attachment

Approved:

Dallas L. Peck
Director

June 18, 1986
Date

SUMMARY

A magnitude 6 earthquake is expected to occur along the San Andreas fault near Parkfield, California before 1993. The Parkfield section of the fault is closely monitored by a variety of geophysical techniques as a prototype earthquake prediction network. It is the intention of the U.S. Geological Survey (USGS) to attempt to issue a short-term warning (minutes-to-days) of the anticipated shock based on observations of precursory phenomena recorded by elements of the prototype earthquake prediction network. The purpose of this report is to define the anomalous conditions that would change the assessment of the imminence of the expected earthquake and the action that would be taken by the USGS. Thus, this report is intended as a USGS planning document that describes the conditions culminating in a communication (a geologic hazards warning) from the USGS to the California Office of Emergency Services (OES). Responsibility for communicating these warnings to the public, to local governments and to the press resides with OES.

Because viable deterministic models (if A occurs, then B _must_ follow) of the earthquake process are not available, we adopt a probabilistic approach to earthquake prediction. That is, we attempt to assess the increased likelihood in the near future of the anticipated shock given the observation of anomalous conditions (e.g., increased seismicity). Couching warnings in a probabilistic framework explicitly allows for the possibility of warnings not followed in the near future by the anticipated shocks. For example, warnings will take the form "There is a 1 in 5 chance (0.22 probability) that the anticipated magnitude 6 shock will occur in the next 24 hours; the probability of the shock in the next 72 hours is at least 0.37."

Four types of observational networks are being operated around Parkfield: seismic, creep, continuous strain, and geodetic survey. The data for each type of network are analyzed continually to determine the state of the region. If the state is anomalous with respect to the normal background condition for any network, then an alert is indicated. If anomalous conditions are observed from more than one network, the level of the alert is increased according to a set of formal rules. Preliminary alert level criteria have been established for each network type. Seismic alert criteria are based on estimates of the probability that an earthquake is a foreshock to the anticipated magnitude 6 event. The criteria for the other 3 network types are based on how frequently anomalous conditions are expected to occur and subjective estimates of the probablity that an anomalous condition will precede a magnitude 6 shock at Parkfield.

We define the following set of alert levels in order of increasing concern and the corresponding USGS response:

Alert Level	Response	Probability of M6 Parkfield earthquake in next 24 (72) hours	Anticipated Time Interval between Alerts
n (normal)	Continue normal operation	0.0001 to 0.0035 (0.0003 to 0.01)	---
e	Alert project personnel; possible maintenance.	--------------	---
d	Alert Parkfield Working Group and Data Collection Operations.	0.0035 to 0.014 (0.0068 to 0.028)	2 mo. - 6 mo.
c	Alert Office Chief, and respond to Alert Level d.	0.014 to 0.059 (0.028 to 0.11)	6 mo. - 18 mo.
b	Alert Director, USGS, and Calif. State Geologist, Calif. Division of Mines and Geology (CDMG) and respond to Alert Level c.	0.059 to 0.22 (0.11 to 0.37)	18 mo. - 54 mo.
a	Issue Geologic Hazards Warning and respond to Alert Level b.	> 0.22 (> 0.37)	> 54 mo.

The earthquake probability is greatest immediately after the occurrence of an alert and generally is expected to decrease with time to the long-term probability of 10^{-4} - 10^{-3}/day appropriate to the normal background. Alerts defined in this report have a finite lifetime of 72 hours after the end of the last signal triggering the alert.

Associated with each alert level is an estimated time interval for normal background conditions between alerts (e.g., 2 to 6 months for alert level d and longer than 54 months for alert level a), These time intervals can be used to estimate the false alarm rate for individual observational networks (i.e., alerts not followed within 72 hours by the expected magnitude 6 shock). However alerts arise from anomolous conditions on any of the several observational networks described in this report. Furthermore, nearly simultaneous lower-level alerts can combine to result in a higher-level alert. Thus, more frequent-than-indicated false alarms are likely, particularly for the lower alert levels. Establishment of more accurate false alarm rates will be based on future analyses of the ongoing Parkfield experiment.

INTRODUCTION

The 25-km-long Parkfield section of the San Andreas fault, midway between San Francisco and Los Angeles (see Figure 1), has experienced moderate-size magnitude 6 earthquakes in 1857, 1881, 1901, 1922, 1934, and 1966 (Bakun and McEvilly, 1984). The mean interevent time of 21.8 $\pm$ 5.2 years, together with the 19+ years that have passed since 1966, suggest that the next shock is now due; estimates of the probability of its occurrence before 1993 range up to 95 percent (Bakun and Lindh, 1985).

The evidence supporting the long-term (few years - several years) prediction of a magnitude 6 shock at Parkfield was independently reviewed and approved by the National Earthquake Prediction Evaluation Council (Shearer, 1985) and the California Earthquake Prediction Evaluation Council. In a letter (dated April 4, 1985) to William Medegovich, the Director of the Governor of California's Office of Emergency Services (OES), the Director of the U.S. Geological Survey reviewed the earthquake hazard situation at Parkfield and promised to notify OES immediately of any changes in the USGS assessment of the situation at Parkfield.

It is the intention of the USGS to attempt to issue a short-term (minutes-to-days) warning (a geologic hazards warning) of the anticipated Parkfield shock. The USGS warning will be directed to OES which has the responsibility to disseminate hazard warnings to the public, to county and local officials, and to the press. Development of explicit USGS plans for issuing a geologic hazards warning to OES are necessary if effective emergency response plans are to be developed by OES. Coordination of the USGS and OES plans to respond to an enhanced earthquake hazard near Parkfield are essential for maximizing public safety.

The purpose of this report is to define those conditions that would so change our assessment of the earthquake hazard at Parkfield that a communication (a geologic hazards warning) from the USGS to OES would be warranted. Emphasis is placed on extreme situations that require decisions within a few hours or less; more gradually developing circumstances will allow time for additional data collection, interpretation, and possibly review by the National Earthquake Prediction Evaluation Council. Our intent here is to provide a means for rapid response to certain anticipated alarming conditions, but we do not intend to limit our responses to just those unusual conditions listed here. If other anomalous alarming conditions arise that were not anticipated in this report, then those conditions would be relayed as rapidly as possible to the Director of the USGS so that a timely geologic hazards warning might still be possible.

In the 1970s, earth scientists optimistically assumed that earthquake research would permit the definition of deterministic earthquake processes. That is, if certain earthquake percursors were observed, then scientists would be able to predict with near certainty the subsequent occurrence of damaging earthquakes. However no viable, reliable deterministic earthquake model capable of reliable short-term predictions is now available. While deterministic earthquake prediction is not now feasible, it is possible to provide specific information that is useful in reducing earthquake hazards. A statistical treatment of anomalous precursory phenomena allows the development of a probability model for earthquake warnings. Rather than warning that an earthquake will occur in the near future, we revise our estimates of the likelihood that a specific shock will occur in the next few days. Such probabilistic warnings can be the basis of meaningful emergency response measures by state and local officials; development of emergency response plans to earthquake prediction in California assumes that the predictions will be couched in probabilistic rather than deterministic terms. The probabilistic models allow for, and permit estimates of, the frequency of warnings without earthquakes (false alarms).

The USGS, in cooperation with the California Division of Mines and Geology of the California Department of Conservation, operates a prototype earthquake prediction network along the Parkfield section of the San Andreas fault. The prototype network has two purposes: (1) to attempt a short-term warning of the anticipated Parkfield earthquake; (2) to identify geologic and geophysical techniques that would be generally useful in earthquake prediction networks elsewhere. Whereas foreshocks and precursory fault creep appear to be significant features of the earthquake process at Parkfield (see the following section), they clearly are not a universal feature of the earthquake process. Thus, while foreshocks and precursory fault creep figure prominently in the Parkfield prediction scenarios described in this report, other techniques must be developed and evaluated to satisfy the second purpose of the prototype network at Parkfield. Thus, we include here descriptions of newer "continuous strain" and "geodetic survey" networks that have significant potential for earthquake prediction efforts elsewhere. There is not yet sufficient understanding of these newer networks so that they figure prominently in the specific Parkfield prediction scenarios considered in this report. However, in future versions of this document our increased understanding of the character and limitations of the "continuous strain" and "geodetic survey" networks likely will be reflected in more reliance on them in specific Parkfield prediction scenarios.

Implicit in this discussion is the admission that we do not yet know how to reliably predict earthquakes. The Parkfield prototype earthquake prediction network then should be viewed as a concentrated attempt to learn how to predict earthquakes both at Parkfield and in general. As we learn, we anticipate changes and refinements in the prediction scenarios described herein. These charges and refinements will be described in subsequent updated versions of this report.

II. HISTORICAL PRECURSORS AT PARKFIELD

Available evidence (Bakun and McEvilly, 1984) is consistent with the hypothesis that the five historic Parkfield main shocks were similar, suggesting that the Parkfield section is characterized by recurring earthquakes with predictable features. The hypothesis of a characteristic earthquake means that the design of a prediction experiment can be tailored to the specific features of the recurring characteristic earthquake. We rely primarily on evidence of changes in seismicity before the 1934 and 1966 Parkfield earthquakes and possible creep (aseismic slip) anomalies before the 1966 shock as a guide to potential precursors to the upcoming quake.

A. Seismicity The 1934 and 1966 main shocks were each preceded by prominent foreshock activity (Bakun and McEvilly, 1979) located in the "preparation zone", a 2-km-long section of the fault immediately northwest of the common epicenter of the main shocks (Figure 2). In both 1934 and 1966 the foreshock activity included a magnitude 5.1 shock 17 minutes before the main shock. (There were no foreshocks larger than magnitude 4-1/2 in 1922 and no foreshocks were reported as felt in 1881, 1901, or 1922). In 1934 fifteen magnitude 3 and larger foreshocks, including two of magnitude= 5.0-5.1, occurred in the 67 hours before the mainshock (Wilson, 1936). In 1966 three magnitude 3 and larger foreshocks occurred, including the one with magnitude 5.1, all in the 3 hours before the 1966 mainshock (McEvilly et al., 1967).

B. Fault Creep Although there were no instruments operating near Parkfield capable of resolving short-term precursory deformation before the historic Parkfield shocks, there were anecdotal accounts of changes in 1966 consistent with significant aseismic slip on the Parkfield section of the San Andreas fault (Brown et al., 1967). First, an irrigation pipeline that crosses the fault trace 5 km south of Parkfield broke about 9 hours before the 1966 main shock. The magnitude of the slip immediately preceding the main shock is unknown. Second, fresh-appearing en echelon cracks were observed along the fault trace near Parkfield twelve days before the 1966 shock. If tectonic in origin, these cracks imply 1-to-2 cm of aseismic slip within the three months preceding the mainshock. It has been suggested, however, that the cracks were related to desiccation and were not tectonic in origin.

III. POTENTIAL FOR PRECURSORY DEFORMATION

Some theoretical and laboratory models of faulting predict accelerating deformation before the slip instability that constitutes an earthquake. The magnitude and character of the precursory deformation, the time scale of the process, and the dimensions of the fault zone involved in the deformation are major unknowns. While there are an infinite variety of possible precursory scenarios, it is possible to delineate end member cases consistent with what is known about previous Parkfield earthquakes.

A favorable scenario for prediction might involve significant amounts of accelerating fault slip extending over the entire eventual rupture surface for weeks to days before the earthquake. This would be revealed by foreshocks in the hypocentral region, accelerating surface fault creep, and changes in the local strain field. The large magnitude, extent, and time scale of such a precursory process would permit detection with current instrumentation.

A much less favorable scenario for prediction might involve a limited amount of preseismic deformation localized to a small section of the fault at depth near the expected main shock hypocenter. Such a process might be manifest solely by small foreshocks and low level strain changes that would be difficult to measure and interpret with existing instrumentation. These examples emphasize the uncertainties involved in formulating precursory scenarios without a widely accepted physical model of the failure process.

IV. SUMMARY OF CURRENT INSTRUMENTATION

The current instrumentation at Parkfield (Figure 3) is divided into four networks: (1) seismic, (2) creep, (3) continuous strain, and (4) geodetic survey. Data from these networks will provide valuable information about the earthquake process even if a short-term warning of the anticipated Parkfield shock is not possible. Note that we restrict our attention in this report to established instrumentation for which there is a history of reliable observations; we do not consider here suggested precursors (e.g., radon concentrations and animal behavior) that are presently too poorly understood to be of use in predicting the next Parkfield earthquake.

A. Seismic The seismic instrumentation (Figure 4) consists of seismographs of the USGS central California seismic network (CALNET), the borehole seismographs operated by P. Malin of the Univ. of California at Santa Barbara (UCSB), and the strong-motion accelerograph array operated by the Calif. Div. of Mines and Geology (CDMG).

CALNET. There are currently 18 high-gain, short period, vertical-component (Z) seismometers located within 25km of the town of Parkfield; seven of these sites have 2 or 3 additional components.

	Component(s)	Location relative to Parkfield
Antelope Grade (PAG)	Z	25km SE
Castle Mountain (PCA)	Z	10km E
Curry Mountain (PCR)	Z	22km N
Gold Hill (PGH)	Z	12km SE
Harlan Ranch (PHA)	Z	9km SE
Hog Canyon (PHO)	Z + low-gain 3 comps	5km SW
Hope Ranch (PHP)	Z + 2 horiz. comp.	17km NW
McMillan Canyon (PMC)	Z + low-gain 3 comp.	20km SW
Middle Mountain (PMM)	Z + 2 horiz. comps.	8km NW
Maxie Ranch (PMR)	Z	23km SE
Portuguese Canyon (PPC)	Z + 2 horiz. comps.	15km NW
Parkfield (PPF)	Z	4km SE
Smith Mountain (PSM)	Z	23km NW
Scobie Ranch (PSR)	Z	15km SE
Stockdale Mountain (PST)	Z	8km NW
Turkey Flat (PTF)	Z + 2 horiz. comps.	3km SE
Vineyard Canyon (PVC)	Z + 2 horiz. comps.	9km NW
Work Ranch (PWK)	Z	11km SW

This array permits routine location of $M > 0.8$ events along the Parkfield section of the San Andreas fault from data continuously telemetered to the USGS offices in Menlo Park. The Menlo Park real-time processor (RTP) provides estimates of earthquake locations and magnitudes within 3-5 minutes of their occurrence (Allen, 1978). The seismic network is well suited to the detection of potential $M \geq 1$ foreshocks at Parkfield.

Borehole Seismograph Network. Three 3-component borehole seismometers (Malin, 1985) have been installed with support provided by the USGS external grants program. The borehole seismographs are currently in the test/evaluation phase; they should provide high-gain high frequency seismic information on $M > 0$ shocks in the Parkfield area not obtainable from the CALNET systems.

Strong-motion Accelerograph Network. Nearly 50 SMA-1 strong-motion accelerographs are operated by CDMG in the Parkfield area (McJunkin and Shakal, 1983). This network is designed to record the details of ground motion during the Parkfield main shock and during any M3.5 or larger foreshocks or aftershocks. The accelerographs are recorded onsite so that data from the strong-motion network will probably not be useful for prediction of the anticipated M=6 shock.

B. Creep

There are 8 creepmeters (Schulz et al., 1982) that are located in the Parkfield area (Figure 5). Locations on the fault from the northwest to the southeast: Slack Canyon (XSC1), Middle Mountain (XMM1), Parkfield (XPK1), Taylor Ranch (XTA1), Durham Ranch (XDR2), Work Ranch (WKR1), Carr Ranch (CRR1), and Gold Hill (XGH1). The Middle Mt. creepmeter is located in the epicentral region of past Parkfield main shocks and foreshocks. Six creepmeters (XSC1, XMM1, XPK1, XTA1, XDR2, XGH1) are invar-wire instruments with 0.02 mm resolution, and two (CRR1, WKR1) are invar-rod instruments with 0.05 mm resolution. Creep data is telemetered to Menlo Park every 10 minutes via GOES satellite and telephone telemetry.

C. Continuous Strain

Strainmeters - Two types of strain-measuring devices are currently in use near Parkfield (Figure 6). Sacks-Evertson borehole volumetric dilational strainmeters (dilatometer) (Sacks et al., 1971) are located at two sites along the southern end of the expected rupture zone (Gold Hill One (GHS1) and Gold Hill Two (GHS2). The dilatometers are operated by the USGS in a cooperative effort with the Carnegie Institution of Washington. A single-component, linear strainmeter (extensometer) (Johnston et al., 1977) is sited on the Claussen Ranch (CLS1) near Middle Mt. at the northern end of the rupture zone. The resolution of the dilatometers range from 10^{-2} parts per million (PPM) for signals with periods of several weeks to 10^{-3} PPM for much shorter periods. Resolution of the extensometer is 0.5 PPM at short periods, unless severe meteorological conditions cause an increase in the noise level. The data are recorded on site and also transmitted once every 10 minutes with digital telemetry via the GOES satellite or telephone circuits to the low frequency data computer in Menlo Park.

Tiltmeters - A network of 4 closely-spaced shallow borehole tiltmeters (Mortensen et al., 1977) is operated at Gold Hill (Figure 6). These data are also recorded on site and transmitted every 10 minutes with digital telemetry to the low-frequency data computer in Menlo Park. Although the tilts due to earth tides are coherent between sites, the long-term tilts are not and reflect long-term instability in the near surface materials. The tilt resolution is of the order of 0.1-1 microradians at periods of days and 0.01-0.1 microradians at periods of hours.

Water Wells - Water level fluctuations in a network of 5 wells (figure 7) near Parkfield are monitored by the USGS Water Resources Division (WRD). At periods of 2 weeks or shorter, water levels respond to the local volume strain, so that water level changes can be directly compared to dilatometer data (Roeloffs and Bredehoeft, 1985). These wells record clear earth tides, and have sensitivities at intermediate periods (days) comparable to the dilatometers. Water levels in wells at Gold Hill, Turkey Flat, Joaquin Canyon and Flinge Flat are sampled every 15 minutes, transmitted every 3 hours by GOES satellite to the low frequency data computer in Menlo Park, and also to WRD in Phoenix and then by the WRD data network to a WRD computer in Menlo Park; water level in the well at Vineyard Canyon currently is recorded only at the well head.

Differential Magnetometers - Local magnetic fields are monitored with absolute total field magnetometers (Mueller et al., 1981) at 7 sites [Varian Ranch (VRRM), Lang Canyon (LGCM), Turkey Flat (TFLM), Hog Canyon (HGCM), Gold Hill (GDHM), Antelope Grade (AGDM), and Grant Ranch (GRAM)] in the Parkfield region (Figure 8). The data are synchronized to within 1.0 sec and are transmitted with 16-bit digital telemetry to Menlo Park. The measurement precision in the period range 10 min to tens of days is about 0.2 nT. Changes of 1.0 nT corresponding to stress changes of several bars, according to current models, can be detected with the present instrumentation at periods greater than a day.

D. Geodetic Survey

There are several dense geodetic networks, both trilateration and leveling, in the Parkfield region.

Two-color Laser Geodimeter Network - A distance-ranging network employing an observatory-based two-color geodimeter (Figure 9) was deployed in 1984 by the Cooperative Institution for Research in the Environmental Sciences (CIRES) of the University of Colorado and is operated through a joint USGS/CIRES program (Slater and Burford, 1985). The network currently consists of 17 baselines distributed radially around the central instrument site, which is located just south of Parkfield. Under optimal conditions the network can be measured nightly. Typical standard errors of individual line length measurements are 0.5-0.7 mm for 4-6 km long lines.

Geodolite Network - A network of 80 geodolite lines (Segall et al., 1985) spans the Parkfield region. Standard errors of individual line-length measurements range from 3 mm to 7 mm for lines 4 km to 33 km in length. It is anticipated that at least part of the network will be measured annually. Four "monitor" lines near the southern end of the rupture zone will be surveyed quarterly.

Small Aperture Networks - Three small aperture trilateration networks (Segall et al., 1985) span the Parkfield section of the San Andreas fault. Standard errors for individual measurements are 4 mm. Thirty-one near-fault lines are scheduled to be surveyed quarterly.

Leveling Network - A network of leveling lines (Segall et al., 1985) in the Parkfield region has been periodically resurveyed since 1979. The network consists of four lines; a 10-km-long line perpendicular to the fault at Parkfield, a 32-km-long line in the vicinity of Middle Mt., a 17-km-long line perpendicular to the fault at the southern end of the rupture zone, and a 24-km-long line parallel to the fault line. Short (~1 km) sections of these long lines are surveyed 3-4 times/yr in a joint effort with the University of California at Santa Barbara (UCSB).

V. ALERT THRESHOLDS.

Based on analyses of the historic seismicity at Parkfield, the probability of a characteristic Parkfield earthquake is about 10^{-4}/day. Anomalous signals result in short-term increases in our estimate of the probability and are used to initiate a series of alerts: e.g., notification of the Parkfield Working Group and other personnel responsible for the operation and maintenance of the data collection systems. In addition to real-time, or near real-time, processors that respond to predetermined threshold signals by activating radio beeper-paging alert systems, data from all of the monitoring networks described in this report are reviewed frequently so that anomalous signals that are not specified in the design of the beeper alert algorithms might be detected and evaluated.

From reported anomalies before historic Parkfield shocks, it is possible to define conditions that would cause a reassessment of the short-term earthquake potential in the Parkfield region. Observations of foreshocks before the 1934 and 1966 shocks permit approximate (i.e. order of magnitude) estimates of the probability that a given earthquake is a foreshock to a characteristic Parkfield earthquake. Data from the other (non-seismic) networks which have been recently established can only be analyzed in terms of the expected occurrence interval of a range of anomalous signals. Consequently these probabilities are assigned subjectively. There is no sound statistical basis for determining the probabilities that these anomalous conditions would be followed by a characteristic Parkfield earthquake. We attempt to define alert levels that correspond in our best judgement to the following probabilities and/or anticipated time interval between alerts:

Alert Level	Probability of shock in next 24 hours	Anticipated Time Interval Between Alerts
d	0.0035 to 0.014	2mo. to 6mo.
c	0.014 to 0.06	6mo. to 18mo.
b	0.059 to 0.22	18mo. to 54mo.
a	> 0.22	>54mo.

The occurrence of anomalous conditions intuitively increases our estimate of the earthquake probability for some short time period. Unless the anomaly continues or unless other anomalous conditions occur, our estimate of earthquake probability decreases with time back to the pre-anomaly level. That is, the level of concern implicit in the alert has a natural lifetime. Although there is not sufficient data to define these lifetimes empirically, the 67-hour duration of foreshock activity before the 1934 shock (Wilson, 1936) suggests that a 3 day (72-hour) lifetime is appropriate.

The anticipated time interval between alerts in the above table emphasizes that use of any set of probabilistic alert criteria implies the occurrence of some false alarms. Whereas the rate of alerts for level d implies 2 to 6 "inhouse" alerts per year for each observation network, the more stringent criteria for level a imply an anticipated alert to OES less frequent than once every 4 to 5 years. Given the Parkfield seismic window of 1988 $\pm$ 5.2 years, we expect that the use of the criteria in this report could result in 1 to 2 warnings to OES without a magnitude 6 shock if the anticipated shock occurs at the end of the prediction window (1993).

Care should be taken in use of the anticipated time interval between alerts. Data are not sufficient to reliably estimate the time interval between alerts for several of the observational networks. Furthermore, the stated anticipated time intervals refer to an individual observation network so that the total alert frequency is likely to be significantly greater than indicated, particularly for the lower alert levels.

A. Seismic

Seismic signals from the CALNET stations are telemetered to Menlo Park and processed by computer in real time to provide estimates of earthquake locations and magnitudes within 3-5 minutes of their occurrence (Allen, 1978). Alert thresholds that signal unusual Parkfield seismicity activate paging systems that alert the seismologists responsible for surveillance at Parkfield. Two criteria are used to define an anomalous seismic condition: (1) a magnitude 2.5 or larger shock in the Parkfield area alert zone, and (2) either a magnitude 1.5 shock, or two magnitude 1.0 shocks within a 72-hour period, in a restricted Middle Mt. zone that includes the Parkfield preparation zone (Figure 10). Occurrence of a magnitude 3.5 or larger shock anywhere in central California also activates the beeper-paging system. Based on recent seismicity rates, we expect the automated seismicity alert system to be triggered 3-5 times per year by earthquakes at Parkfield, for a total of 25 alerts by 1993.

The probability that an earthquake near Middle Mt. will be a foreshock to the characteristic Parkfield earthquake has been calculated based on the following assumptions:

1) The next characteristic Parkfield earthquake is assumed to have a 0.5 chance of having some foreshocks, magnitude unspecified, within the Middle Mt. alert zone.
2) The probability of any one earthquake within the Middle Mt. alert zone being <u>the</u> foreshock, is inversely proportional to the number of such earthquakes that occur per 21.7 year recurrence cycle.

The resulting conditional probability that the next characteristic Parkfield earthquake will follow an earthquake of magnitude M within the Middle Mt. alert zone is estimated to be

$$P_F = P\left(\begin{array}{l}\text{Next Characteristic} \\ \text{Parkfield Earthquake}\end{array} \middle| \begin{array}{l}\text{Potential Foreshock} \\ \text{of magnitude M}\end{array}\right) \cong 3.1\times10^{-4} \times 10^{0.62M}$$

P_F is an estimate of the probability of a Parkfield earthquake occurring within the first few days following a potential foreshock of magnitude M.

If we wish to apply this estimate to a specific time interval following a potential foreshock, we must have an estimate of how this probability decays with time. Lindh and Jones (1985) showed that probability density functions of the form e^{-at} provided a reasonable fit to the foreshock data of Jones (1985) for southern California. Based on this, we have used $f(t) = e^{-0.021t}$, where t is in hours after the potential foreshock. Thus the probability of a Parkfield main shock occurring between time t_1 and t_2 after a potential foreshock (given that it has not already occurred by time t_1) is

$$P_{F,T} = P_F \times \int_{t_1}^{t_2} e^{-0.021t}dt$$

For $t_1 = 0$ and $t_2 = 24$, 48, or 72 hours following a potential foreshock, the integral equals 0.41, 0.65, and 0.79 respectively. Thus the probability of a characteristic Parkfield earthquake in the 24 hours following a potential foreshock of magnitude M is

$$P_{F,24} = P_F \times 0.41 \equiv 1.27 \times 10^{-4} \times 10^{0.62M}$$

In addition, for a current estimate of the total probability at any particular time, some estimate of an increase in background probability as times passes is necessary, as it seems intuitively compelling that the probability increases with time as one approaches or passes the mean recurrence time. Combining the estimate of Bakun and Lindh (1985) of 1988.0 + 5.2 for the next Parkfield event with the long-term conditional probability formulation of Lindh (1983), we obtain an estimate of the daily probability attributable to the long-term recurrence model:

$$P_R = 4.1 \times 10^{-4} \times 10^{0.12T}$$

where T is years after 1 Jan 1986.

These numbers can be combined to give a single probability estimate P using the formulation of Utsu, (1979)

$$P = 1/(1+r_o r_1 r_2), \text{ where}$$
$$r_o = (1/P_o) - 1$$
$$r_1 = (1/P_R) - 1$$
$$r_2 = (1/P_F) - 1, \text{ and}$$

P_o (the Poisson probability) = 1/21.7 x 1/365

= 1.26×10^{-4} per day

The resulting total probability estimates for a potential foreshock on 1 Jan 1986 being followed within 24, 48, and 72 hrs by a characteristic Parkfield earthquake are listed below. The total probability for T = 24 hours is plotted in figure 10b as a function of M, the magnitude of the potential foreshock. While these probabilities are quoted to 2 significant figures, they are approximate and somewhat subjective, and are best treated as order of magnitude estimates.

Seismic Alert Level	Seismicity (See Figure 10a for alert zone boundaries)	Estimated Prob. of Parkfield Main Shock in first 24	48	72 hrs.	Anticipated Time Interval Between Alerts
d	(1) one M 1.5 shock in the Middle Mt. alert zone (2) two or more M 1.0 shocks in a 72-hour period in the Middle Mt. alert zone (3) one M 2.5 shock in the Parkfield alert zone (4) one M 3.5 shock in the Parkfield area (San Ardo, Coalinga, etc.)	0.0035	0.0056	0.0068	2 - 6 mo.
c	(1) one M 2.5 shock in the Middle Mt. alert zone (2) two or more M 1.5 shocks in a 72-hour period in the Middle Mt. alert zone (3) one M 3.5 shock in the Parkfield alert zone	0.014	0.023	0.028	6 - 18 mo.
b	(1) One M 3.5 shock in the Middle Mt. alert zone (2) two or more M 2.5 shocks in a 72-hour period in the Middle Mt. alert zone	0.059	0.090	0.11	18 - 54 mo.
a	(1) One M 4.5 in the Middle Mt. alert zone (2) two or more M 3.5 shocks in a 72-hour period in the Middle Mt. alert zone	0.22	0.32	0.37	> 54 mo.

B. Creep

Parkfield-area creepmeters exhibit long-term average creep rates ranging from 23 mm/yr at Slack Canyon to 4 mm/yr at Gold Hill (Schulz et al., 1982). Data from the eight Parkfield creepmeters are sampled every 10 minutes. The automated anomaly detector compares the average creep at each of the 8 sites in the past hour with the average level in the preceding 23 hours. A change of 0.25 mm or greater activates the paging device. In 1985, 16 beeper-paging alarms were triggered by creep events.

Creep Alert Level	Creep Observations (in the absence of M 3.5 or larger shocks)	Anticipated time interval between alerts
e	(1) At one site, a right or left-lateral creep step of >0.25 mm within one 10-minute telemetry sample period. (See Figure 11a.) (In the past 2 years, there have been at least 6 of these alerts, all due to battery, telemetry, and/or telephone transmission failures.)	< 4 mo.
	(2) At one site, a small right- or left-lateral creep event; i.e. creep exceeding 0.25 mm within 1 hour with slip velocity decreasing exponentially within 1-2 hours after onset. (See Figure 11b)	< 2 mo.
d	(1) At any one site other than XSC1, a nearly continuous increase in creep (see Figure 11c) that exceeds 0.25 mm within 7 days and continues at a comparable or greater rate over a period greater than 10 days. (This alert has been reached 4 times in the period 1982-1985; XSC1 normally moves 0.25 - 0.5 mm/week).	6 mo.
	(2) At any two sites other than XSC1, nearly simultaneous onset of an almost continuous increase in creep that exceeds 0.2 mm in 24 hours and continues at a comparable or greater rate for more than 2 days. (This alert occurred for the first time in December 1985; XSC1 normally moves 0.25-0.5 mm/week.)	
	(3) At one site, an unusually large creep event (see Figure 11b). For creepmeters northwest of XDR2 (XSC1, XMM1, XPK1, XTA1, and XDR2) events with creep >0.5 mm in the first 30 min. would be unusually large. For creepmeters southeast of XDR2 (WKR1, CRR1, and XGH1), events with creep >0.33 mm in the first 30 minutes would be unusually large.	
c	(1) Nearly simultaneous onset of creep at 2 or more creepmeters that exceeds 0.5 mm in one hour.	6 mo.- 12mo.
	(2) More than 1 mm of creep on the Middle Mt. creepmeter in one hour.	

b	(1)	More than 5 mm of creep in 72 hours on the Middle Mt. creepmeter	>24 mo.
	(2)	More than 5 mm of creep in 72 hours on 2 or more Parkfield area creepmeters.	
a	(1)	Creep rates on multiple instruments (or at Middle Mt. alone) in excess of 0.5 mm /hour sustained for 6-10 hours or cumulative creep in excess of 5 mm in a shorter period.	>24 mo.

C. Continuous Strain

The sizes of strain anomalies that might precede a Parkfield earthquake can be estimated on the assumption that these anomalies would be produced by aseismic slip on a vertical fault. Calculations of the net volumetric strain that would be produced by such aseismic slip show that the moment required to produce observable strains anywhere at the surface is comparable to that of a M 2.5 earthquake located near the surface, and considerably larger for slip at depth. Figures 12a and 12b are contour maps of volumetric strain for slip events having moments of 10^{25} dyne-cm centered at 5 and 10 km depth, respectively. The deeper event is comparable in moment and depth to the 1966 characteristic Parkfield earthquake. Assuming a detection threshold of 0.03 PPM, such an event would have been observable over almost all the area shown in the contour maps. The area within which an event with ten times smaller moment would have been observed is somewhat reduced; such an event might be comparable to a magnitude 5 foreshock.

1. Strainmeters. Data from the Parkfield strainmeters are sampled automatically every 10 minutes and the data are transmitted to Menlo Park. For the dilational strain data, average strain for the last 60 minutes is computed. Earth tides and atmospheric pressure loading, determined from a theoretical earth tide model and an onsite pressure transducer, respectively, are removed from the data. Provided the instruments and telemetry are operating correctly, changes in strain of 0.2 PPM over several days (longer term) or 0.1 PPM at periods less than a day, (short term), can be clearly detected. Short-term strain changes are detected by an algorithm that identifies strain changes of more than 0.05 PPM in a 24 hour period. Longer-term strain changes are detected by an algorithm that identifies changes in strain rate normalized by estimates of noise in the data.

Although only two borehole strainmeters now operate in the Parkfield region, during the past two years (Nov. 83-Nov. 85) four longer-term alerts have been triggered for strain rate increases of about 0.03 PPM/day for periods of about a week. One of these strain perturbations occurred on a dilatometer at the same time as minor seismicity and a creep event at Middle Mt. All four longer-term strain perturbations were independently recorded and identified in water level data in a well at Gold Hill.

Strainmeter Alert Level		Changes in strain
e		Changes of 0.05 PPM or greater within a 24 hr period on one dilatometer. These may occur because of phone line, telemetry, or instrument malfunctions, and generally triggers maintenence response.
d	(1)	Changes of 0.1 PPM per week on two dilatometers
	(2)	changes of 0.1 PPM within a 24 hour period on one dilatometer with indications of a simultaneous signal on a second dilatometer.
c	(1)	Changes of 0.2 PPM per week on two or more independent dilatometers
	(2)	changes of 0.2 PPM within a 24 hour period on one dilatometer with indications of a simultaneous signal on a second dilatometer.

b Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant warnings to the Directors of the USGS and CDMG.

a Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant a warning to OES.

2. Water Wells In order to define the network alert levels, the sensitivity of each well is determined based on observed water level change per unit strain associated with the M2 semidiurnal tide. Although sensitivities and noise levels vary among the wells, a value of 0.03 PPM is the smallest dilatation that could be observed if it took place over a few hours. Water level changes can be observed in response to dilatational strains imposed with time scales ranging from a few seconds to a few weeks, but the observability of strain events generally decreased with lengthening event time scale. For example, seasonal water level changes will mask strain events of amplitude less than about 0.20 PPM that take place over a period of a week. In addition, slow strain events will require more time to detect.

Water level data are examined daily, and filtered and plotted two times per week. In addition, as water level data are received (every three hours), each water level observation is corrected for barometric pressure variation and compared with a projected water level, which is equal to the previous day's mean water level plus variation due to earth tides. If, at any time, observed and projected water levels differ by an amount representing strain of more than 0.05 PPM, a message is sent alerting personnel to examine the data in order to determine whether an alert should be issued. No alert is issued if visual inspection indicates that the event generating the message is attributable to barometric or rainfall disturbances, or to instrument, telemetry, or software malfunction.

An anomaly could escape detection by the real-time scanner either because it is smaller than the threshold level at which the scanner is set, or because it does not rise to the threshold amplitude within one day, which is the time period after which the reference level for the projected water level is reset. Numerical experiments have delineated a curve of event amplitudes versus rise-time constant within which water level events having exponential forms (similar to creep events) could be perceived by visual inspection of filtered data. This curve, which is labeled "detectable" in Figure 13, shows that for events with rise times long than 2 days, the minimum amplitude that can be detected increases with increasing rise time. Although any event with an amplitude of 0.05 PPM or greater can represent significant slip at depth, only those events in the region indicated in Figure 13 have a high probability of being identified. These events are the ones that will generate alarms, provided they are not ascribable to rainfall, barometer, or equipment problems.

Water Well Alert Level		Changes in Strain
e		Event of amplitude greater than 0.05 PPM at one well. (See above description of the water well "real-time" detection algorithm).
d	(1)	Unexplained event of amplitude greater than 0.05 PPM at one well with rise time less than 24 hours (corresponds to an e level alert that cannot be attributed to rainfall, barometric disturbances, etc.)
	(2)	Unexplained event at one well with rise time greater than 24 hours and clearly detectable amplitudes (i.e., amplitudes to the right of the "detectable" curve in figure 13.)
c	(1)	Unexplained events of amplitude greater at 0.05 PPM at two wells, each with rise time less than 24 hours.
	(2)	Unexplained events at two wells with rise time greater than 24 hours and clearly detectable amplitudes (i.e., amplitudes to the right of the "detectable" curve in figure 13).
b		Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant warnings to the Directors of USGS and CDMG.
a		Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant a warning to OES.

3. Differential Magnetic Field. Differential magnetic field data are sampled automatically every 10 minutes and transmitted to Menlo Park where they are monitored frequently and plotted weekly. Changes of ~1 nT within a day, or at longer periods, in the averaged data are considered anomalous. This has happened only once during 10 years of monitoring and occurred during the few months following the May 1983 Coalinga earthquake.

Continuous Magnetic Field Alert Level	Changes in Magnetic Field
e	Changes of 1 nT or greater between station pairs over time periods less than 24 hours. This may occur because of instrument malfunction and/or clock syncronization failure and generally triggers maintenance.
d	Changes of 1 nT or more in a day or longer between two instruments. This has occurred only once during the past five years in the Parkfield region.
c	Changes of 1 nT or greater in a day or longer on two independent instrument pairs. This has not occurred during the past five years in the Parkfield region.

b	Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant warnings to the Directors of USGS and CDMG.
a	Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant a warning to OES.

D. Geodetic Survey

Distance measurements using the two-color geodimeter are collected 2-3 times/week, weather conditions permitting, so that the two-color observations are more appropriate for a more slowly developing scenario than has been considered in this report. Nevertheless, it is possible to identify some circumstances under which these relatively infrequent discrete measurements would contribute to a rapid reassessment of the Parkfield earthquake hazard. Sufficient data now exist to define specific criteria for alert level d; specific criteria for alert levels a, b, and c must be developed as a history of line length changes is obtained.

Anomalous Line length Alert Level	Line-Length Changes Between Successive Measurements
d	(1) Short-term changes. Three or more lines with distance changes (absolute value) of $\geq$ 3.0 mm each within a single event window of 25 days or less, with at least one line changing by $\geq$ 4.0 mm. Changes on each line must exceed the 2 σ level of significance where $\sigma = \sqrt{\sigma_1^2 + \sigma_2^2}$, and σ_1 and σ_2 are the std. error of the lengths measured before and after the changes. (In the case of oscillatory changes, at least two independent, consecutive measurements, made 15 or more hours apart within the same event window identified for other lines, must deviate by more than 2 σ from the mean of the final 3 independent values obtained just before the beginning of the event window.) Two such periods of change have been documented in the Parkfield 2-color geodimeter network since October, 1984, the first from April 22 to May 8, and the second from July 28 to August 20, 1985. These examples are presented in Figures 14 and 15. (2) Trend changes. Three or more lines showing changes in rate of extension (or contraction) of $\geq$ 0.04 mm/day (15 mm/yr), as determined by least-squares analysis. The times of the three line changes must fall within one event window of 30 days or less. The change on each line must exceed the 2 σ level of significance.
c	Not yet defined.

b	Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant warnings to the Directors of USGS and CDMG.
a	Given the lack of experience at Parkfield, at this time there are no clear criteria for anomalies that in the absence of other anomalies would warrant a warning to OES.

E. Alert Thresholds on Multiple Instrument Networks

Clearly anomalous conditions detected on several networks would increase our concern that a Parkfield earthquake is imminent. Simultaneous alarms can combine to establish a level of concern appropriate to a higher alert threshold. We propose that a set of simple alert level combination rules be applied to the alert levels for the individual network groups:

Status of Network Alert Levels*

Rule	Network 1		Network 2		Network 3		Network 4		Combined Alert Level
1)	d	+	n	+	n	+	n	>	d
2)	d	+	d	+	(d or n)	+	(d or n)	>	c
3)	c	+	(d or n)	+	(d or n)	+	(d or n)	>	c
4)	c	+	c	+	(c,d, or n)	+	(c,d, or n)	>	b
5)	b	+	(c,d, or n)	+	(c,d, or n)	+	(c,d, or n)	>	b
6)	b	+	b	+	(b,c,d, or n)	+	(b,c,d, or n)	>	a
7)	a	+	(a,b,c,d or n)	+	(a,b,c,d, or n)	+	(a,b,c,d or n)	>	a

* n = normal condition

To apply these rules, rank the four network groups in decreasing order of current alert level status. For example, if the seismic, creep, continuous strain, and geodetic survey alert levels were c, b, c, and d respectively, then creep, seismic, continuous strain, and geodetic survey would be labelled networks 1, 2, 3, and 4. That is, the networks alert level status would be b, c, c, d, corresponding to combination rule 5. Rule 5 states that one level b, two level c, and one level d alert are not sufficient to warrant an alert level a response - i.e., a warning to OES.

VI. RESPONSE

Project Chief. The responsibility for recognizing the anomalous condition described in this report resides with the project chiefs of the individual Parkfield earthquake prediction networks. Each project chief has the following specific responsibilities:

1) Maintain a monitor system for the data collected by the project.
2) Maintain an effective detector system capable of detecting the anomalous conditions defined in the preceding section.
3) Immediately alert the Chief Scientist and the Chief of the Seismology Branch or Tectonophysics Branch of all a, b, c, or d level alerts.
4) Train and maintain an alternate capable of assuming the above r ponsibilities.
5) Delegate these responsiblities to the alternate whenever the project chief cannot adequately perform these responsibilities. The Chief Scientist and the appropriate branch chief (Seismology or Tectonophysics) must be notified of this delegation of responsibility.

Chief Scientist. The responsibility for coordinating earthquake prediction efforts at Parkfield resides with the Chief Scientist. The Chief Scientist has the following specific responsibilities:

1) Once alerted by a project chief that a d, c, b, or a alert level has been recognized, the Chief Scientist has the responsibility of notifying the Chiefs of the Seismology Branch and Tectonophysics Branch of the status of the alert levels.

2) After consultation with these branch chiefs and determining the alert level, the Chief Scientist is responsible for notifying the Chief of the Office of Earthquakes, Volcanoes and Engineering whenever a c, b, or a alert level is reached.

3) For an a alert level, the Chief Scientist is responsible for notifying the Office of the Director of OES (See Appendix B).

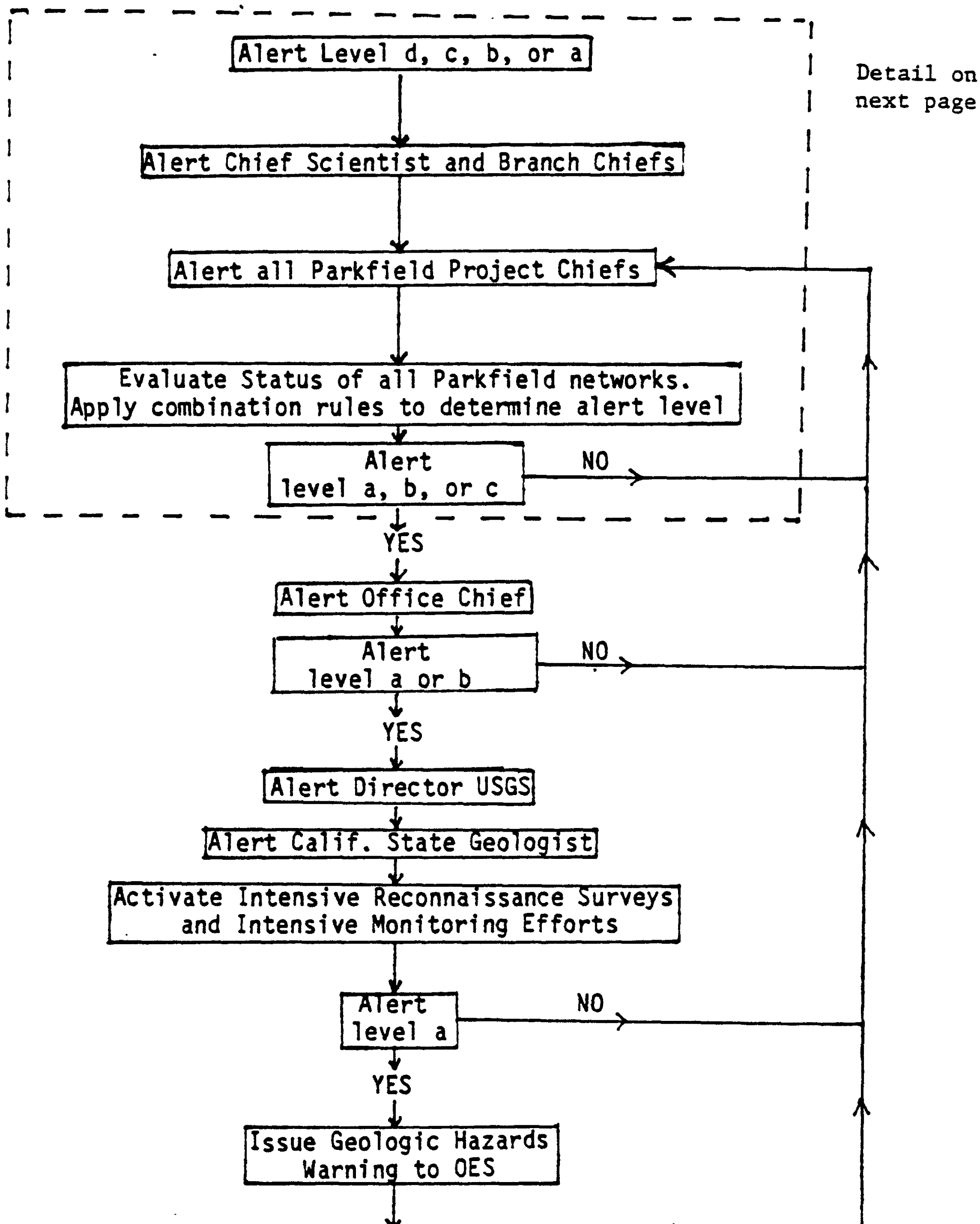
Chiefs, Seismology and Tectonophysics Branches. The branch chiefs have the responsibility for maintaining the personnel and resources within their branches that are necessary to maintain and operate the real-time surveillance and prediction capabilities described in this report. The branch chiefs have the following specific responsibilities:

1) Advise the Chief Scientist regarding the status of alert levels for the 4 network groups whenever a d, c, b, or a alert level is recognized by a project chief.
2) For a d,c,b, or a level alert, notify the appropriate project chiefs of the alert status. The project chiefs to be notified by each branch chief are indicated on the detailed decision flow diagram that follows.
3) For a b or a level alert, coordinate the intensive reconnaissance and monitoring efforts described in Appendix A.
4) Serve as a replacement for the Chief Scientist in fulfilling the Chief Scientist's responsibilities that are described above.
5) Serve as a replacement for the Office Chief in fulfilling the Office Chief's responsibilities that are described below.

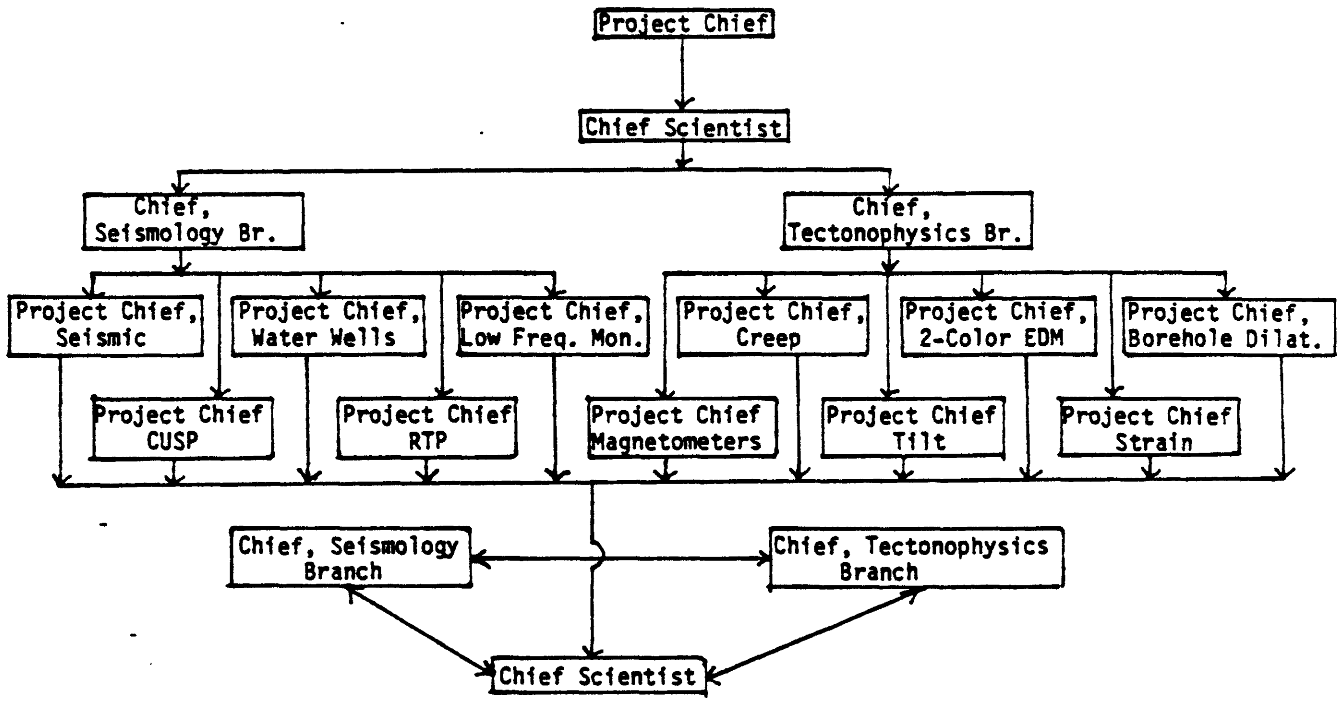
<u>Chief, Office of Earthquakes, Volcanoes, and Engineering (OEVE)</u>. The Office Chief is responsible for communicating the alert level status to non-USGS OEVE personnel. The Office Chief has the following specific responsibility:

1) Once alerted by the Chief Scientist that a b or a level alert has been reached, the Office Chief has the responsibility to notify the Director of USGS and the Calif. state geologist, CDMG.

DECISION FLOW DIAGRAM



DETAIL OF DECISION FLOW DIAGRAM



APPENDIX A. INTENSIVE MONITORING-RECONNAISSANCE EFFORTS

In the event that a high-level (a or b) alert is initiated, additional efforts at Parkfield are necessary if the maximum information regarding the generation process of Parkfield earthquakes is to be obtained and information relevant to the imminent occurrence of a large shock on the San Andreas fault southeast of the Parkfield section is to be available. Although these efforts have not yet been fully planned, it is clear that the following steps should be undertaken.

1) Alert Chief, Branch of Strong Ground Motion and Faulting
2) Alert CDMG manager of strong-motion network at Parkfield.
3) Remeasure geodetic baselines estabished along the San Andreas fault in the Parkfield area, and to southeast of the Parkfield section.
4) Alert cooperating agencies (University of California at Berkeley, University of California at Santa Barbara, University of Colorado, Carnegie Institute)
5) Verify that telemetry (phone, radio, microwave, and satellite) are functional.
6) Institute nightly measurements on the two-color geodolite network.
7) Measure alignment networks in the Parkfield region.
8) Reconnaissance of highways that cross the active traces of the San Andreas fault within and southeast of the rupture zone of the characteristic Parkfield earthquake.
9) Establish temporary seismic networks in Parkfield area.

APPENDIX B. SAMPLE WARNING MESSAGE

Experience in other fields where public safety is at issue has consistently shown the necessity of clear, complete, unambiguous communication of information to agencies responsible for disseminating warnings to the public and to news media. Prior agreement by the USGS and OES on the content and format of warnings to OES from the USGS are essential if the USGS estimates of immediate geologic hazards due to Parkfield earthquakes is to be quickly understood and acted upon by OES. Thus, we propose to communicate the geologic hazards warning to OES in the following message:

"Recent observations by the U.S. Geological Survey (USGS) along the 25-km-long Parkfield section of the San Andreas fault, midway between San Francisco and Los Angeles, suggest that there is about a 1 in 2 chance that a moderate-size magnitude 6 earthquake will occur near Parkfield in the next 72 hours. This warning is based on anomalous signals recorded on geophysical instrument networks operated by the USGS near Parkfield.

An earthquake of magnitude 6 is of moderate size, at the threshold of being able to cause modest damage to some structures that have not been designed for earthquake resistance. The last magnitude 6 Parkfield earthquake occurred on June 28, 1966 and caused only minor damage to wood frame houses in the region. The potential exists for a shock larger than the 1966 shock and for the fault to rupture southeast into the adjacent 25-mile section of the San Andreas fault."

REFERENCES

Allen, R. V. (1978). Automatic earthquake recognition and timing from single traces, Bull. Seismol. Soc. Am., 68, 1521-1532.

Bakun, W. H., and T. V. McEvilly (1979). Earthquakes near Parkfield, California: comparing the 1934 and 1966 sequences, Science, 205, 1375-1377.

Bakun, W. H., and T. V. McEvilly (1984). Recurrence models and Parkfield, California, earthquakes, J. Geophys. Res., 89, 3051-3058.

Bakun, W. H., and A. G. Lindh (1985). The Parkfield, California, earthquake prediction experiment, Science, 229, 619-624.

Brown, R. D., Jr., J. G. Vedder, R. E. Wallace, E. F. Roth, R. F. Yerkes, R. O. Castle, A. O. Waananen, R. W. Page, and J. P. Eaton (1967). The Parkfield-Cholame California, earthquakes of June-August 1966, U.S. Geol. Surv. Prof. Paper 579, 66 pp., Washington, D.C.

Johnston, M. J. S., A. C. Jones, and W. Daul (1977). Continuous strain measurements during and preceding episodic creep on the San Andreas fault, J. Geophys. Res. 82, 5683-5691.

Jones, L. M. (1985). Foreshocks and time-dependent earthquake hazard assessment in southern California, Bull. Seismol. Soc. Am., 75, 1669-1680.

Lindh, A. G. (1983). Preliminary assessment of long-term probabilities for large earthquakes along selected fault segments of the San Andreas fault system in California, U.S. Geol. Surv. Open-File Rept. 83-63, 14 pp.

Lindh, A. G., and L. M. Jones (1985). Parkfield foreshock probabilities (abstract), EOS, Trans. Am. Geophys. Un., 66, p. 982.

Malin, P. E. (1985). Initial results from downhole/surface seismometer pairs near Parkfield, CA (abstract), EOS, Trans. Am. Geophys. Un., 66, p. 982.

McEvilly, T. V., W. H. Bakun, and K. B. Casaday (1967). The Parkfield, California earthquakes of 1966, Bull. Seismol. Soc. Am., 57, 1221-1244.

McJunkin, R. D., and A. F. Shakal (1983). The Parkfield strong-motion array, California Geology, 36, Calif. Div. Mines and Geol., Sacramento, CA, 27-47.

Mortensen, C. E., E. Y. Iwatsubo, M. J. S. Johnston, G. D. Myren, V. G. Keller, and T. L. Murray (1977). U.S.G.S. tiltmeter networks, operation and maintenance, U.S. Geol. Surv. Open-File Rept. 77-655, 17 pp.

Mueller, R. J., M. J. S. Johnston, B. E. Smith, and V. Keller (1981). U.S. Geological Survey magnetometer network and measurement techniques in western U.S.A., U.S. Geol. Surv. Open-File Rept. 81-1346, 18 pp.

Roeloffs, E. A., and J. D. Bredehoeft (1985). Coseismic response of water wells near Parkfield, California to the August 4, 1985 North Kettleman Hills earthquake (abstract), EOS, Trans. Am. Geophys. Un. 66, p. 986.

Sacks, I. S., S. Suyehiro, and D. W. Evertson (1971). Sacks-Evertson strainmeter, its installation in Japan and some preliminary results concerning strain steps, Proc. Japan. Acad., 47, 707-712.

Schulz, S. S., G. M. Mavco, R. O. Burford, and W. D. Stuart (1982). Long-term fault creep observations in central California, J. Geophys. Res., 87, 6977-6982.

Segall, P., W. Prescott, R. Stein, N. King, R. Harris, and A. Lindh (1985). Summary of geodetic survey measurements near Parkfield, Appendix A in Minutes of the National Earthquakes Prediction Evaluation Council July 26-27, 1985 Menlo Park, California, ed., C. F. Shearer, U.S. Geol. Surv. Open-File Rept. 85-754, 103-116.

Shearer, C. F. (1985). Minutes of the National Earthquake Prediction Evaluation Council November 16-17, 1984 Menlo Park, California, U.S. Geol. Surv. Open-File Rept. 85-201, 81 pp.

Slater, L., D. Hamann, and R. Burford (1985). Two-color geodimeter strain monitoring at Parkfield, California (abstract), EOS, Trans. Amer. Geophys. Un., 66, p. 986.

Utsu, T. (1979). Calculation of the probability of success of an earthquake prediction (in the case of Izu-Oshimar Kinkai earthquake of 1978), Rep. Coord. Comm. for Earthquake Prediction 21, 164-166.

Wilson, J. T. (1936). Foreshocks and aftershocks of the Nevada earthquake of December 20, 1932 and the Parkfield earthquake of June 7, 1934, Bull. Seismol. Soc. Am., 26, 189-194.

FIGURE CAPTIONS

Figure 1. Map of earthquake epicenters (1975-June 1985) relative to the trace of the San Andreas fault (bold line) and the epicenters of the M=5.1 foreshock and the main shock in 1966, shown as small and large stars respectively. Epicenter clusters near the western edge (faint line) of the San Joaquin Valley are aftershocks of the 1975 Cantua Creek, 1976 Avenal, 1982 New Idria, and 1983 Coalinga earthquakes. Epicenters for all $M \geq 2.3$ earthquakes are shown, except for the very many $M \geq 3$ aftershocks of the 1983 Coalinga earthquake, which cover the Coalinga area when plotted.

Figure 2. Map of the Parkfield area showing epicenters of earthquakes associated with the 1934 (left) and the 1966 (right) characteristic Parkfield earthquakes. In 1934, only $M > 4$ shocks can be accurately located; in 1966, $M \geq 2$ shocks for 28 Jan 1966 - 30 June 1966 are shown.

Figure 3. Location of geophysical instrumentation relative to the preparation zone and the rupture zone of the characteristic Parkfield earthquake.

Figure 4. Seismic instrumentation near Parkfield. See caption for Figure 3. The letter code designation corresponds to the list given in the text. Borehole seismographs exist at PGD, PJQ, and PVC (located within about 50 m of the CALNET sensor at PVC.) The location of the strong-motion sensors operated by CDMG are shown in McJunkin and Shakal (1983).

Figure 5. Creepmeters located near Parkfield. See caption for Figure 3. The creepmeter at Slack Canyon (SLC1) is located on the trace of the San Andreas fault just off the top of the map.

Figure 6. Strainmeters (borehole dilatometers, tiltmeters, and linear strainmeter) located near Parkfield. See caption for Figure 3.

Figure 7. Water wells located near Parkfield. See caption for Figure 3.

Figure 8. Magnetometers located near Parkfield. See caption for Figure 3. Magnetometer sites at Antelope Grade (AGDM) and at Grant Ranch (GRAM) are near the trace of the San Andreas fault off the map to the south.

Figure 9. Two-color geodolite reflector sites located near Parkfield. See caption for Figure 3.

Figure 10. (a) Seismic alert zones near Parkfield. The Middle Mt. alert zone includes shocks with epicenters within the small figure centered on Middle Mt. and with focal depths $\geq$ 6.5 km. The Parkfield area alert zone extends along the San Andreas fault trace from the creeping section northwest of Middle Mt. to the Simmler section southeast of Cholame.
(b) Probability of a characteristic Parkfield earthquake in the 24 hours following the occurrence of a potential foreshock of magnitude M.

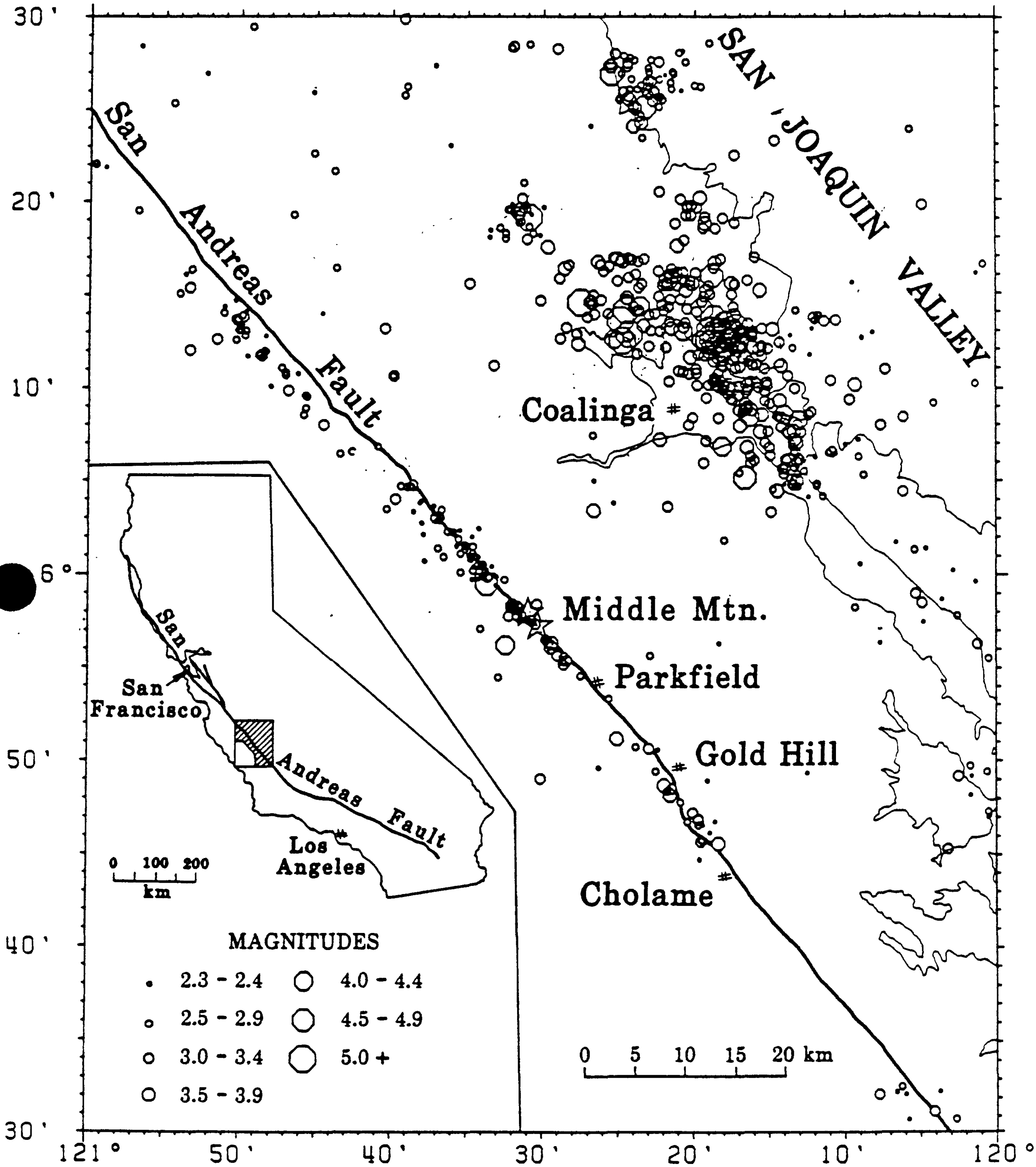
Figure 11. (a) A creep step recorded at XMM1, caused by telemetry problems. This signal triggered the beeper-paging system (an e alert level). (b) A creep event recorded at XMM1. Although not large enough for a d level alarm, it did trigger the beeper-paging system (an e alert level). (c) Sustained rapid creep at XPK1. This kind of signal does not trigger the beeper-paging system, but would constitute a d level alert if sustained for a few more days.

Figure 12. Contour maps of volumetric strain produced at the surface by strike-slip over a 5 km x 5 km section of vertical fault. a) Hypocenter at 5 km depth. b) Hypocenter at 10 km depth. The key assumes a detectability threshold of 0.03 ppm, which is appropriate for water level detection of events having rise times shorter than 1 day.

Figure 13. Minimum amplitude strain event that can be detected as a water level change, as a function of event rise time. Events below and to the right of the curve can be distinguished from noise and environmental effects. Effects within the dashed box should be detected by the real-time processing system. The diagonal line at the top and left is the threshold above which events would be masked in a well with a sensitivity of 0.025 PPM/cm and with seasonal water level trends of 20 cm/month.

Figure 14. Distance readings to 11 reflector sites in the Parkfield 2-color geodimeter network recorded between April 18 and June 12, 1985 (error bars represent +/-1 standard deviation). Measurements to stations TODD and HUNT were not begun until late July. Distances to TABLE, MIDE, BARE, and CAN changed by 3 mm or more during the 17 days between April 22 and May 8 (pairs of vertical dashed lines). The change to station MIDE reached the 4-mm minimum required for one of the lines, according to the criteria for alert level d (1) (-4.9 +/-0.8 mm if the event window is extended to May 16, a full 25 days).

Figure 15. Distance readings to 12 reflector sites in the Parkfield 2-color geodimeter network recorded between July 3 and September 29, 1985 (error bars represent +/-1 standard deviation). As for distance changes to MASON, records for station TODD show no length changes meeting the alert level d criteria and are omitted. Distances to MELV, TABLE, MIDE, and HUNT changed 3 mm or more during the 24 days between July 28 and August 20 (pairs of dashed vertical lines). Distance changes to stations TABLE and MIDE exceeded the 4-mm minimum required for at least one line, according to the criteria for alert level d (1).



SAN JOAQUIN VALLEY
San Andreas Fault
Coalinga
Middle Mtn.
Parkfield
Gold Hill
Cholame
San Francisco
San Andreas Fault
Los Angeles
0 100 200
km
MAGNITUDES
2.3 - 2.4
2.5 - 2.9
3.0 - 3.4
3.5 - 3.9
4.0 - 4.4
4.5 - 4.9
5.0 +
0 5 10 15 20 km
30'
20'
10'
36°
50'
40'
30'
121°
50'
40'
30'
20'
10'
120°

Figure 1

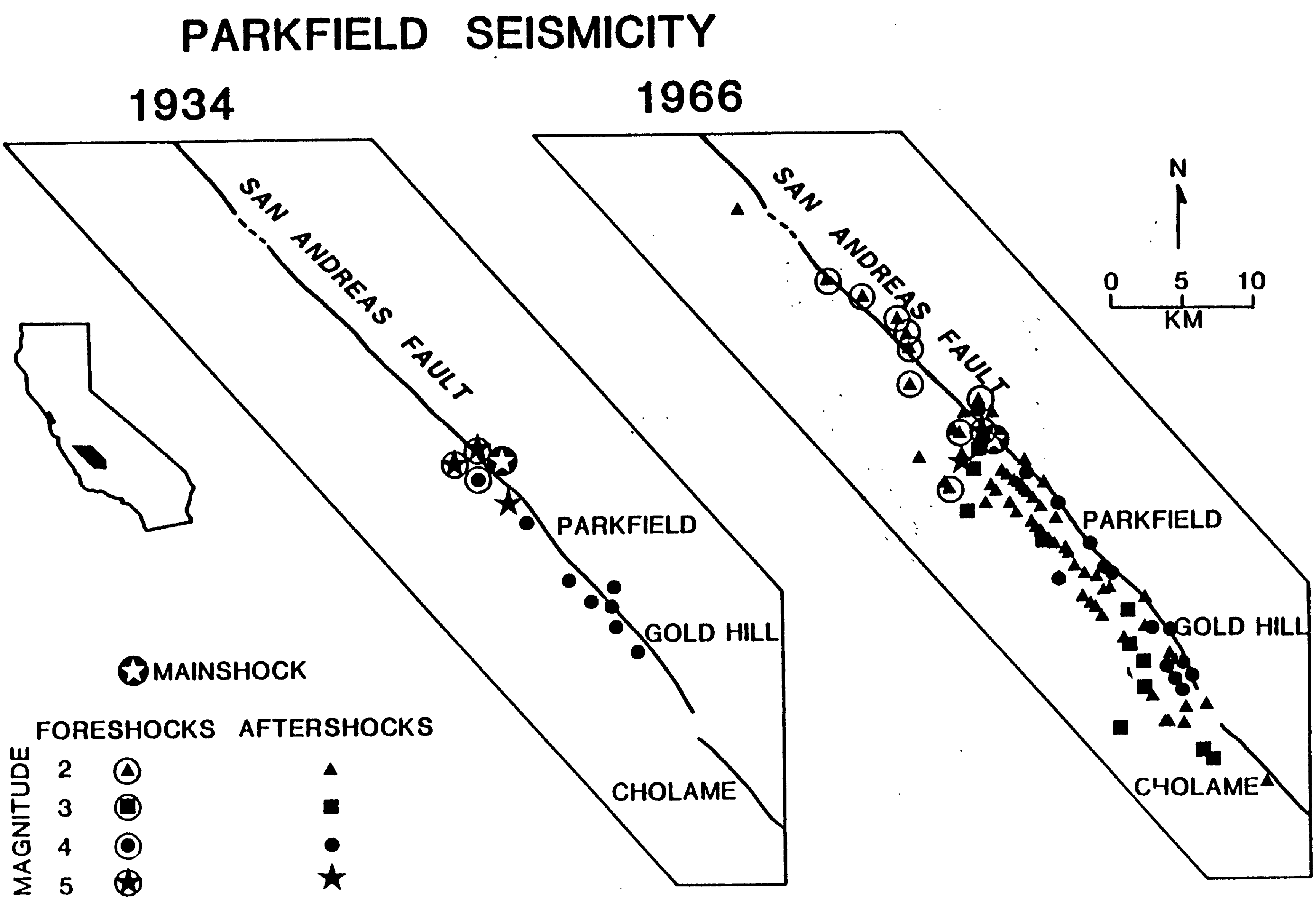

PARKFIELD SEISMICITY
1934
1966
SAN ANDREAS FAULT
PARKFIELD
GOLD HILL
CHOLAME
SAN ANDREAS FAULT
PARKFIELD
GOLD HILL
CHOLAME
N
0 5 10
KM
MAINSHOCK
FORESHOCKS AFTERSHOCKS
MAGNITUDE
2
3
4
5

Figure 2

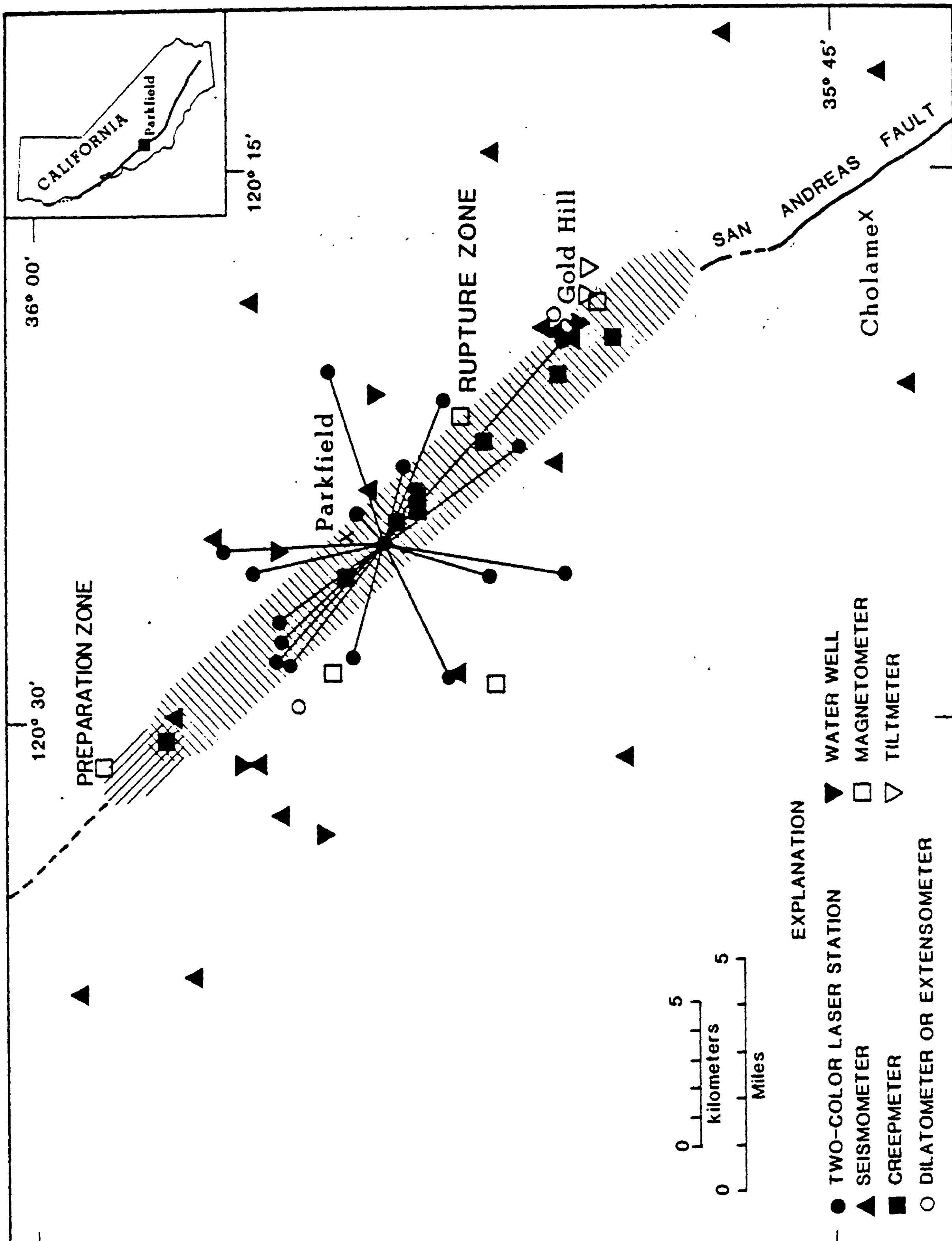

CALIFORNIA
Parkfield
120° 15'
36° 00'
35° 45'
120° 30'
PREPARATION ZONE
RUPTURE ZONE
Parkfield
Gold Hill
SAN ANDREAS FAULT
Cholame
0 5
kilometers
0 5
Miles
EXPLANATION
TWO-COLOR LASER STATION
SEISMOMETER
CREEPMETER
DILATOMETER OR EXTENSOMETER
WATER WELL
MAGNETOMETER
TILTMETER

Figure 3

SEISMOMETERS

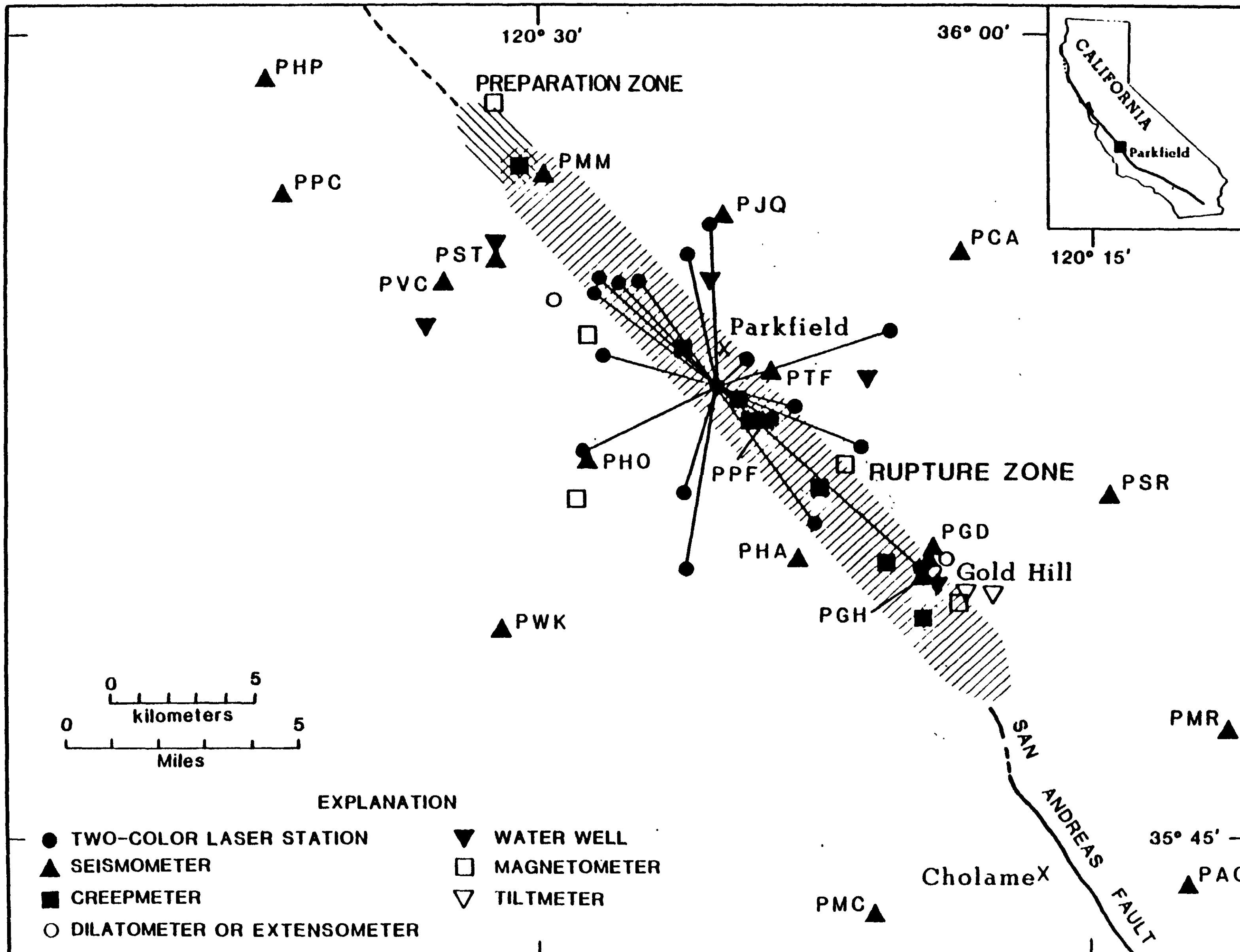


Figure 4

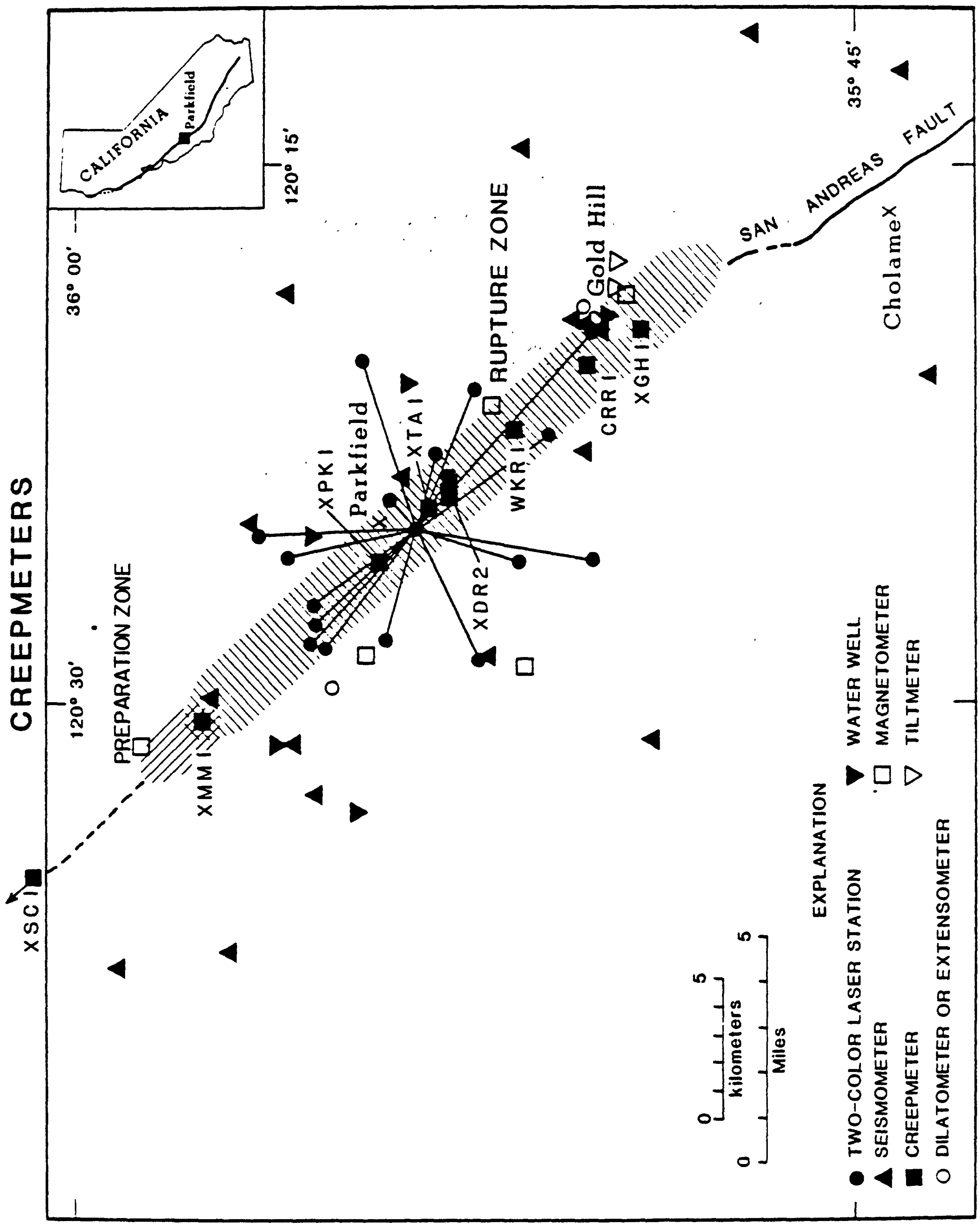

CREEPMETERS
CALIFORNIA
Parkfield
36° 00'
120° 15'
120° 30'
35° 45'
PREPARATION ZONE
RUPTURE ZONE
SAN ANDREAS FAULT
Cholame
Gold Hill
Parkfield
XSC1
XMM1
XPK1
XTA1
XDR2
WKR1
CRR1
XGH1
0 5 kilometers
0 5 Miles
EXPLANATION
TWO-COLOR LASER STATION
SEISMOMETER
CREEPMETER
DILATOMETER OR EXTENSOMETER
WATER WELL
MAGNETOMETER
TILTMETER

Figure 5

DILATOMETERS, EXTENSOMETERS, AND TILTMETERS

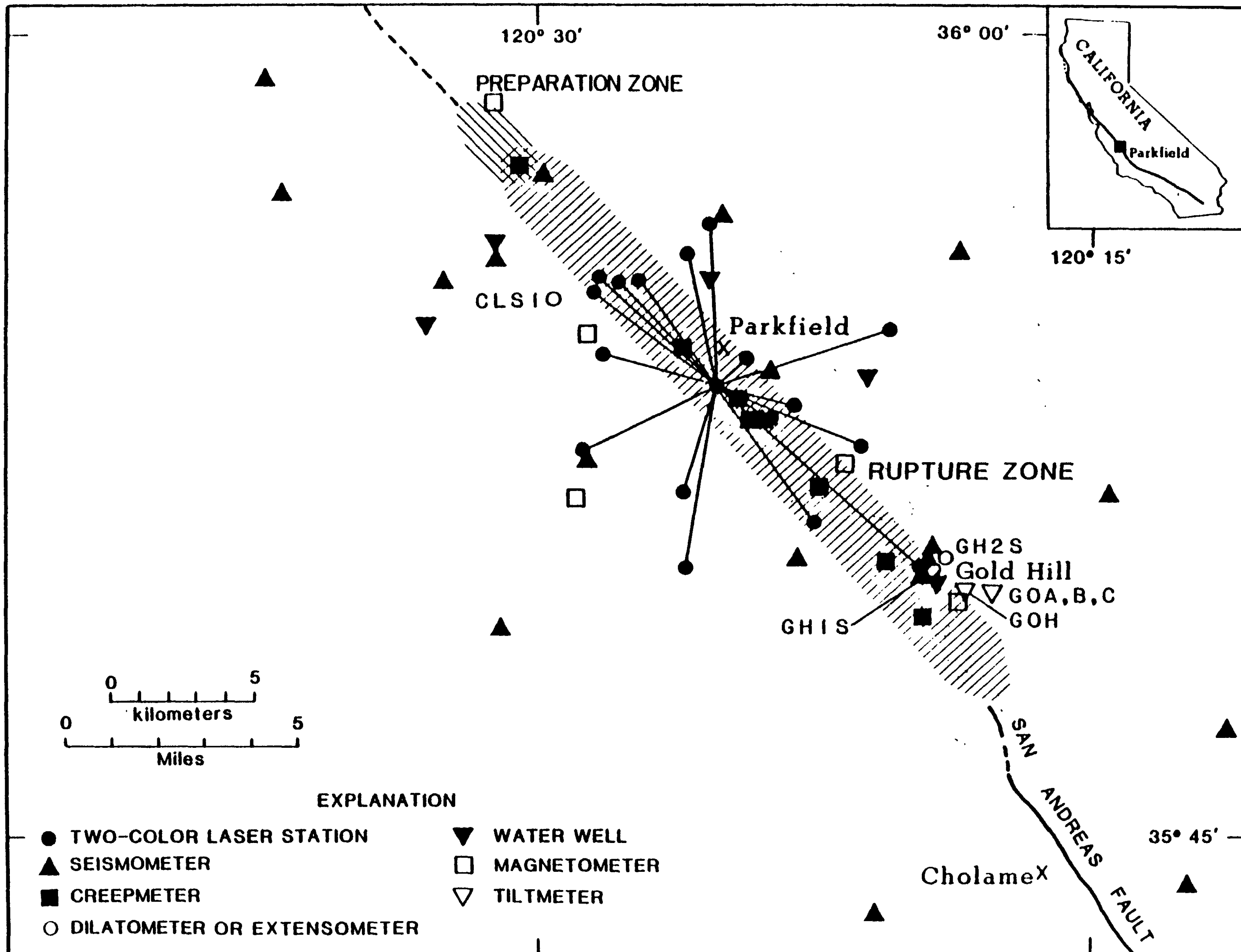


Figure 6

WATER WELLS

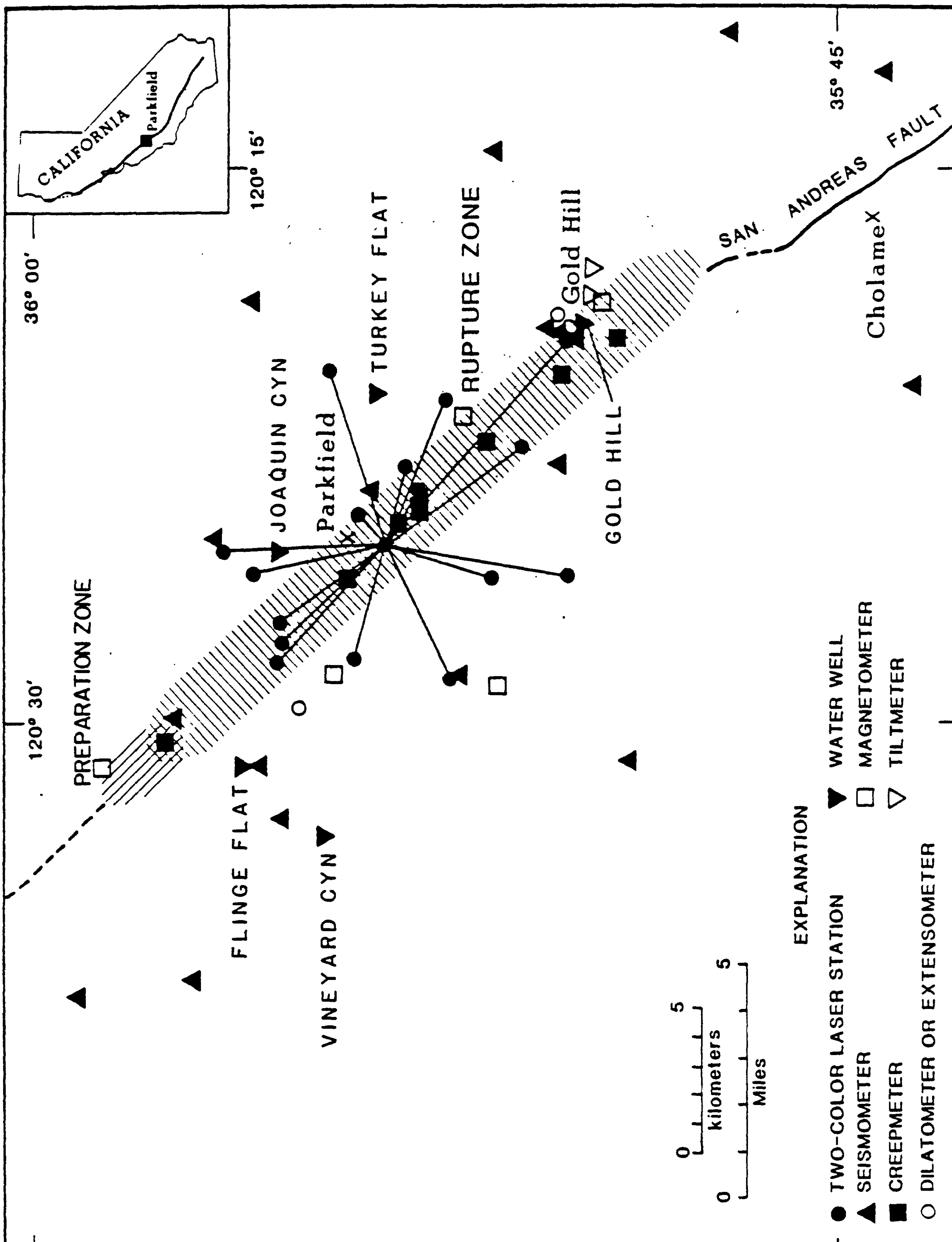


Figure 7

MAGNETOMETERS

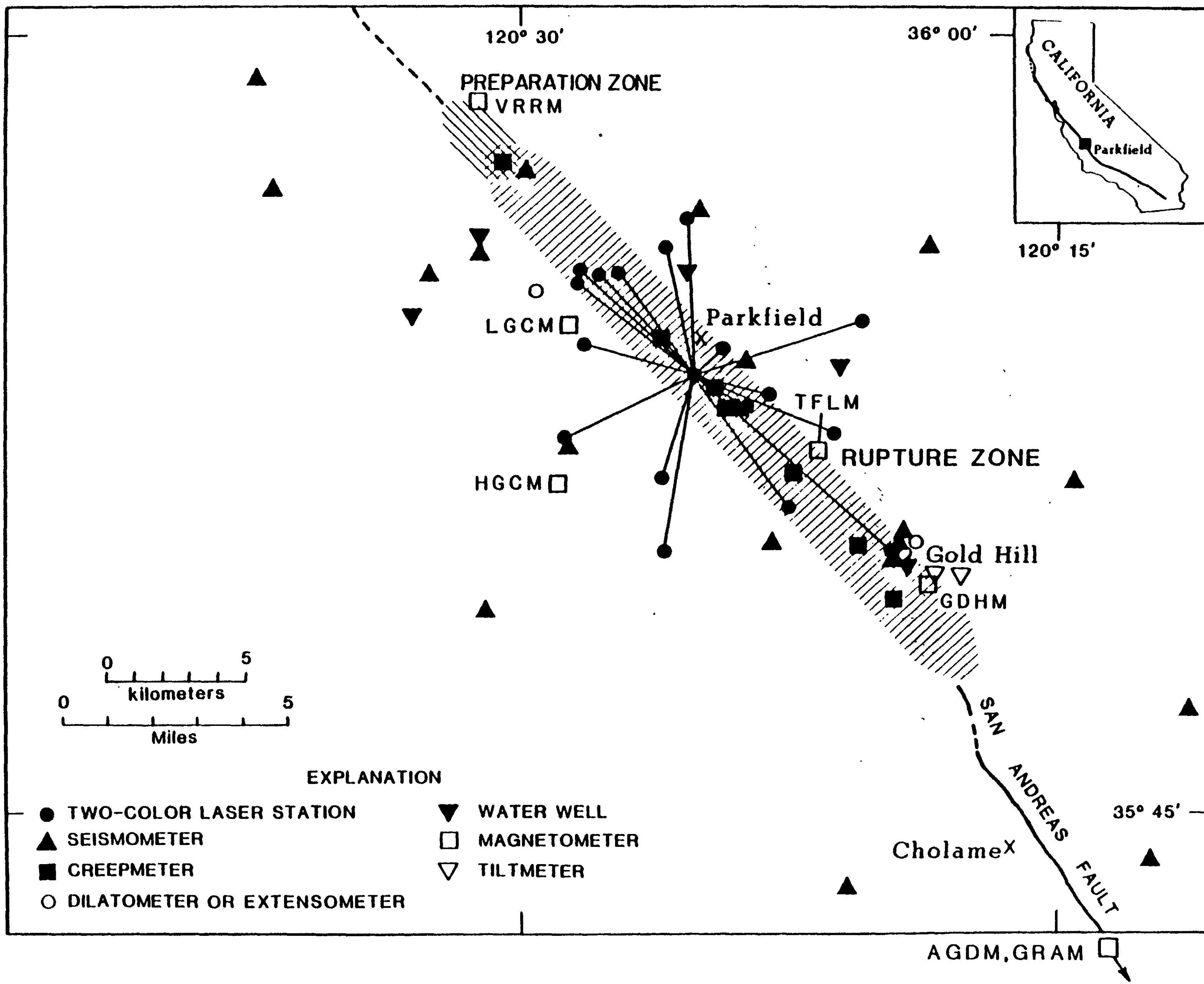


Figure 8

TWO-COLOR LASER STATIONS

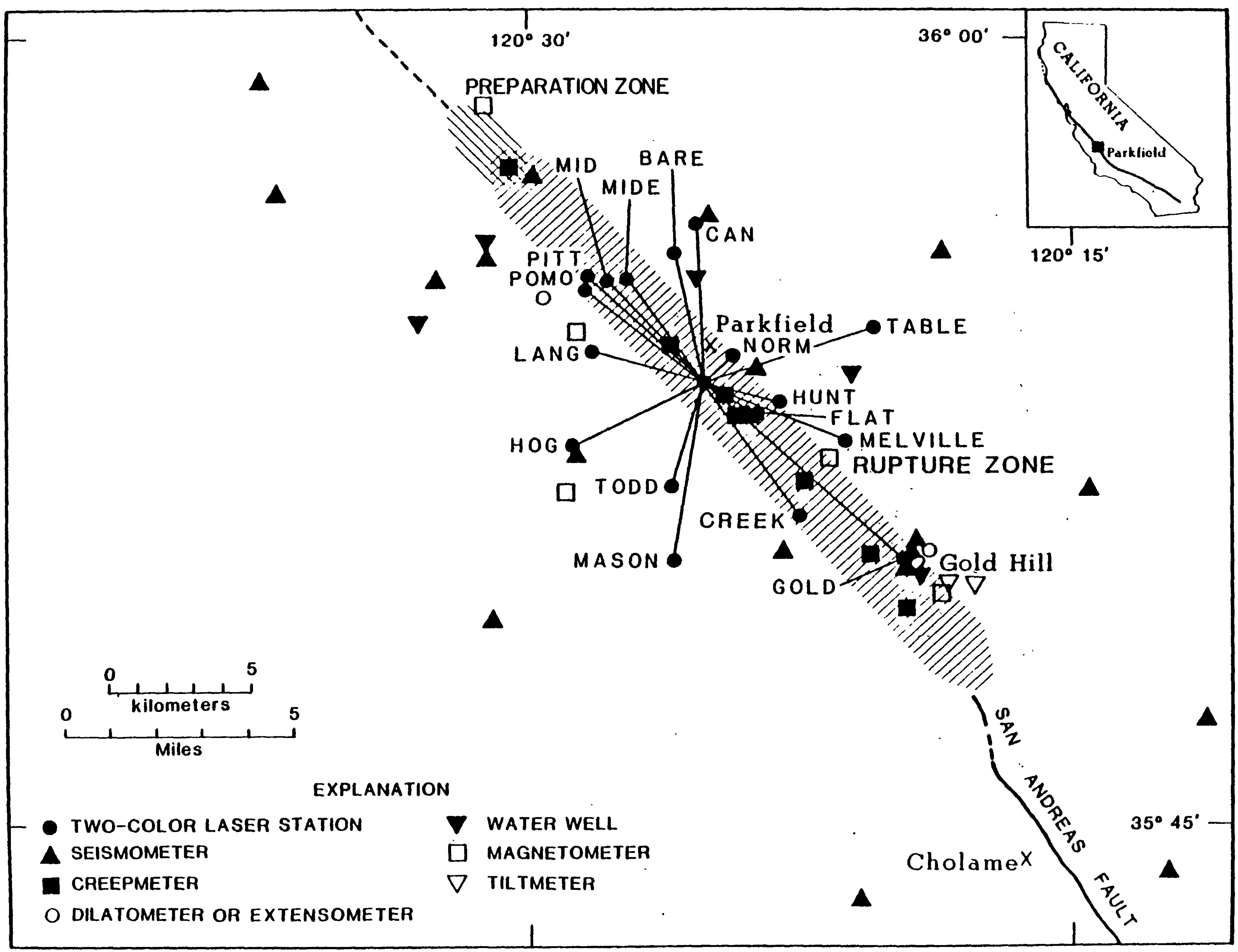


Figure 9

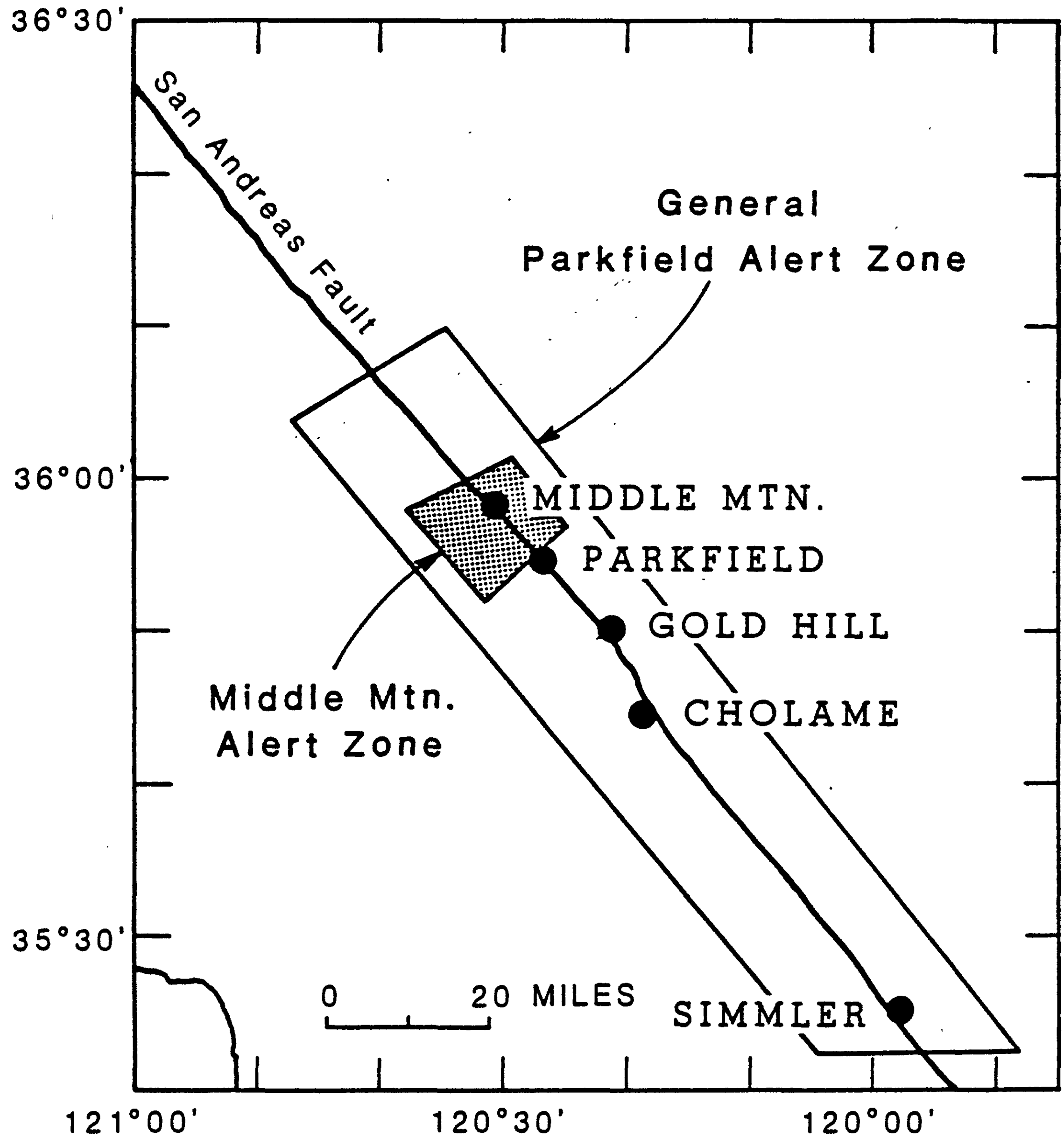

36°30'
36°00'
35°30'
121°00'
120°30'
120°00'
San Andreas Fault
General
Parkfield Alert Zone
MIDDLE MTN.
PARKFIELD
GOLD HILL
CHOLAME
Middle Mtn.
Alert Zone
0
20 MILES
SIMMLER

Figure 10a

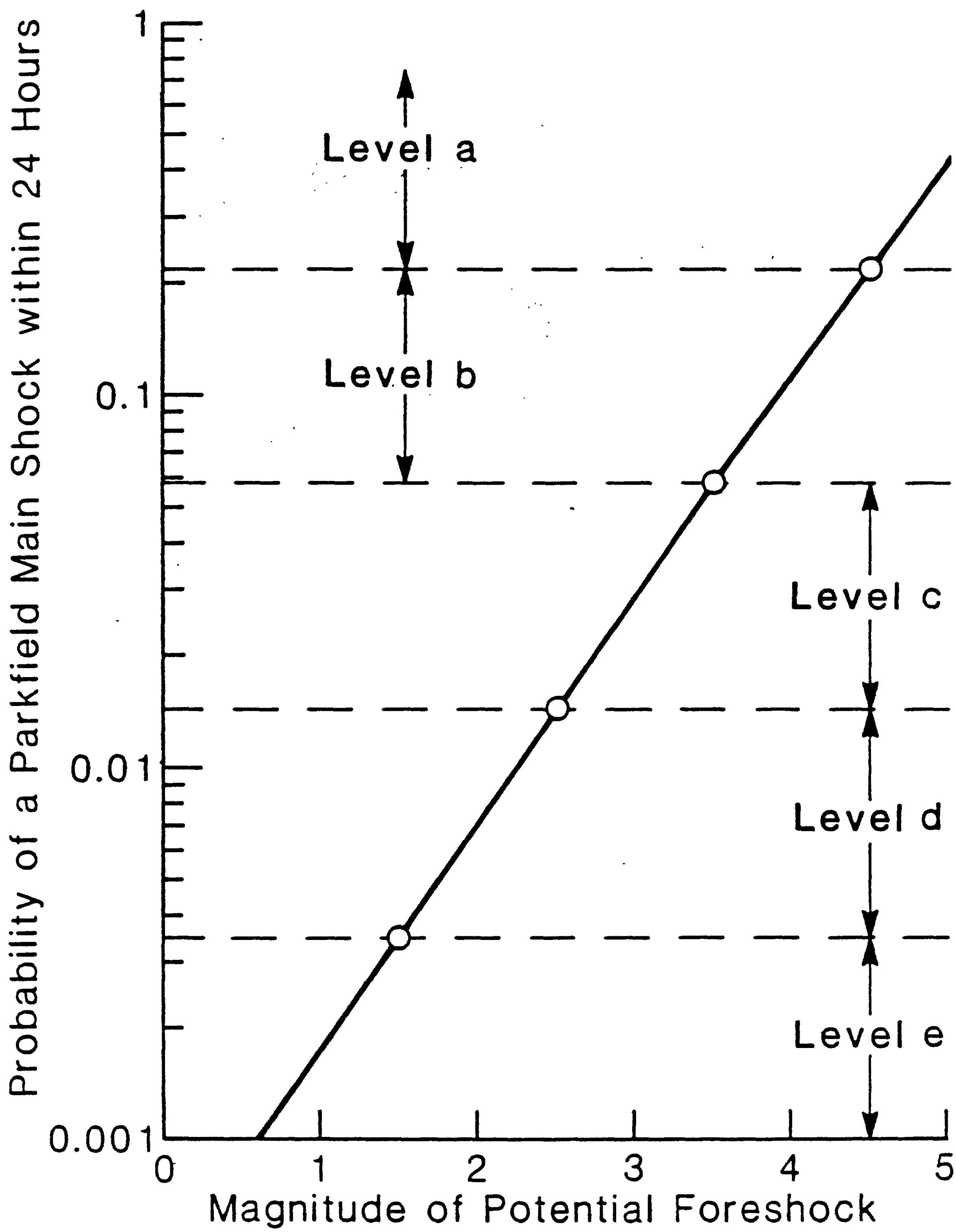

Probability of a Parkfield Main Shock within 24 Hours
1
0.1
0.01
0.001
Level a
Level b
Level c
Level d
Level e
0
1
2
3
4
5
Magnitude of Potential Foreshock

Figure 10b

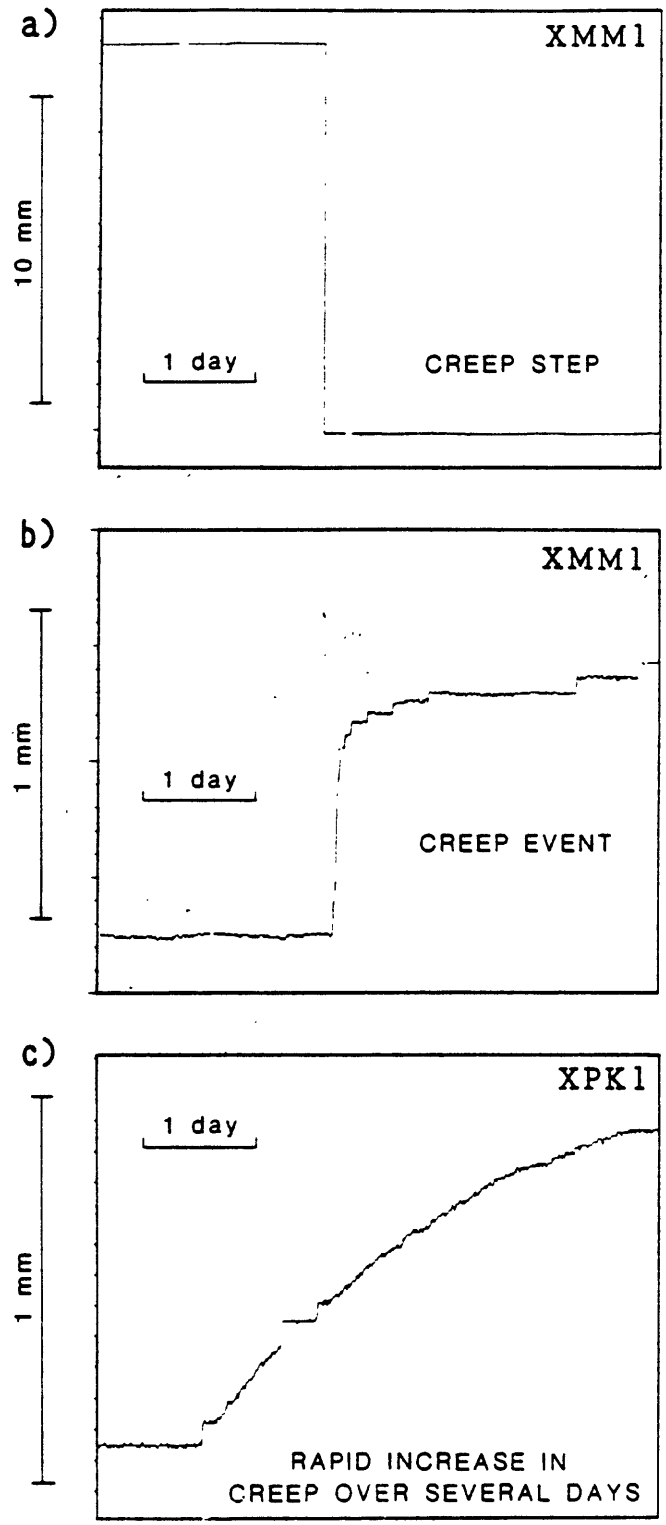
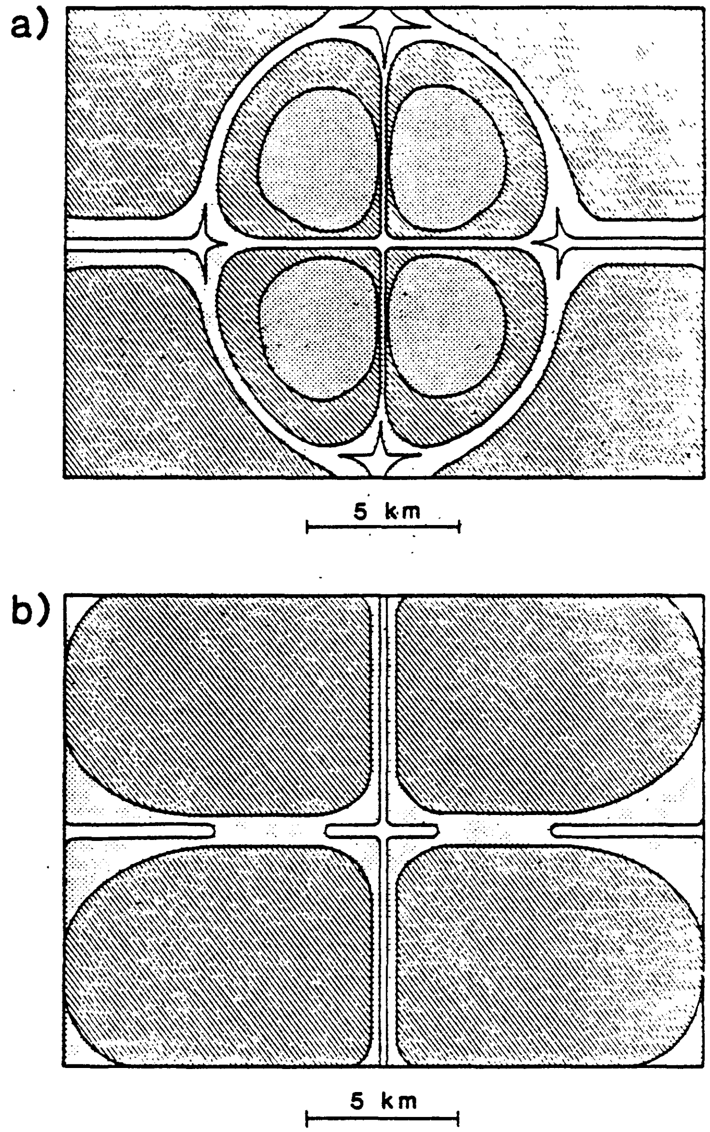

a)
XMM1
10 mm
1 day
CREEP STEP
b)
XMM1
1 mm
1 day
CREEP EVENT
c)
XPK1
1 day
1 mm
RAPID INCREASE IN
CREEP OVER SEVERAL DAYS

Figure 11



Moment, dyne-cm

	10^{25}	10^{24}	10^{23}
(dense stipple)	100 to 1000	10 to 100	1 to 10
(medium stipple)	10 to 100	1 to 10	not detectable
(light stipple)	1 to 10	not detectable	not detectable
(blank)	not detectable	not detectable	not detectable

Units in table are in multiples of detectability threshold.

Figure 12

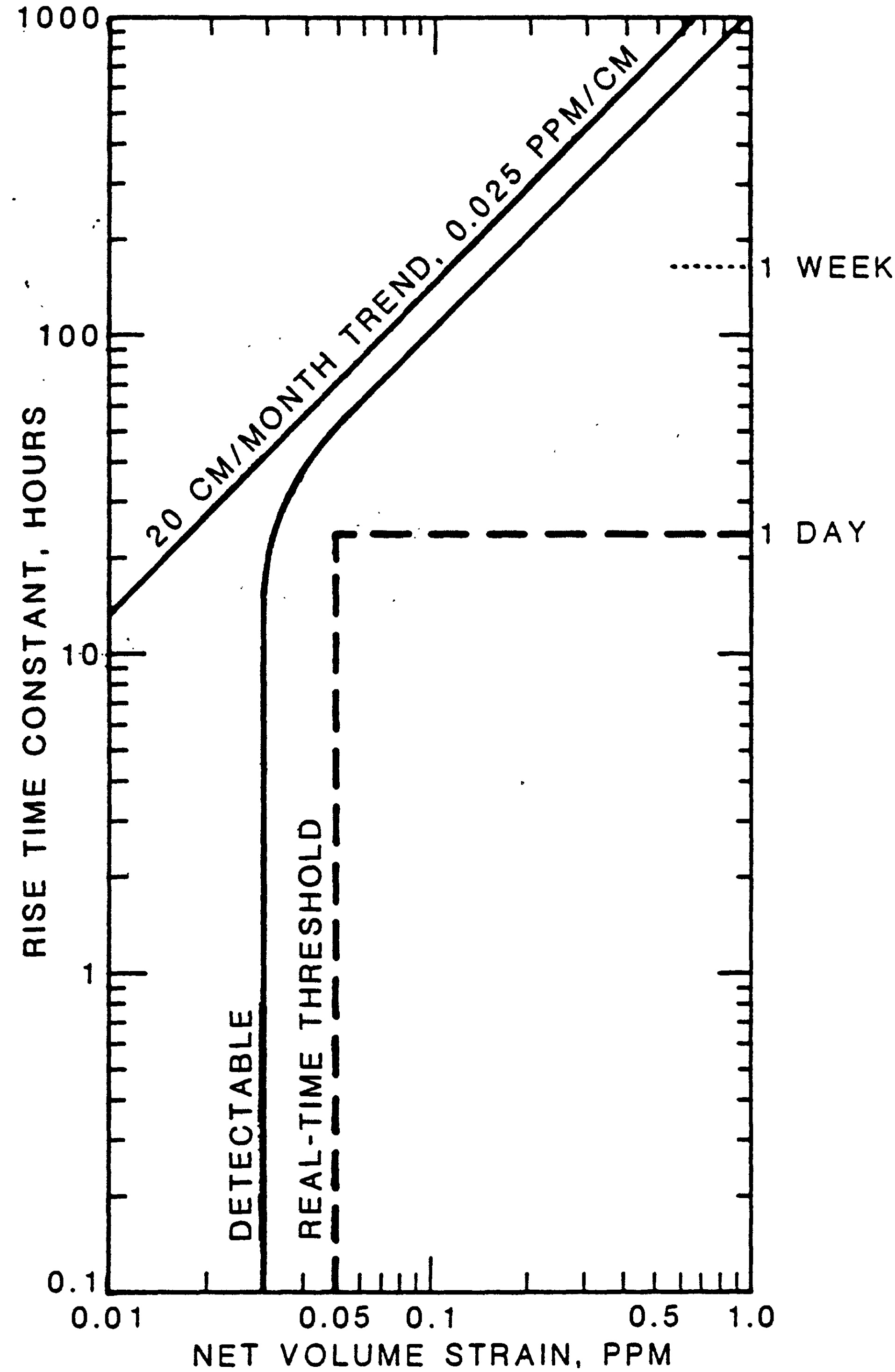

1000
100
10
1
0.1
RISE TIME CONSTANT, HOURS
20 CM/MONTH TREND, 0.025 PPM/CM
1 WEEK
1 DAY
DETECTABLE
REAL-TIME THRESHOLD
0.01
0.05
0.1
0.5
1.0
NET VOLUME STRAIN, PPM

Figure 13

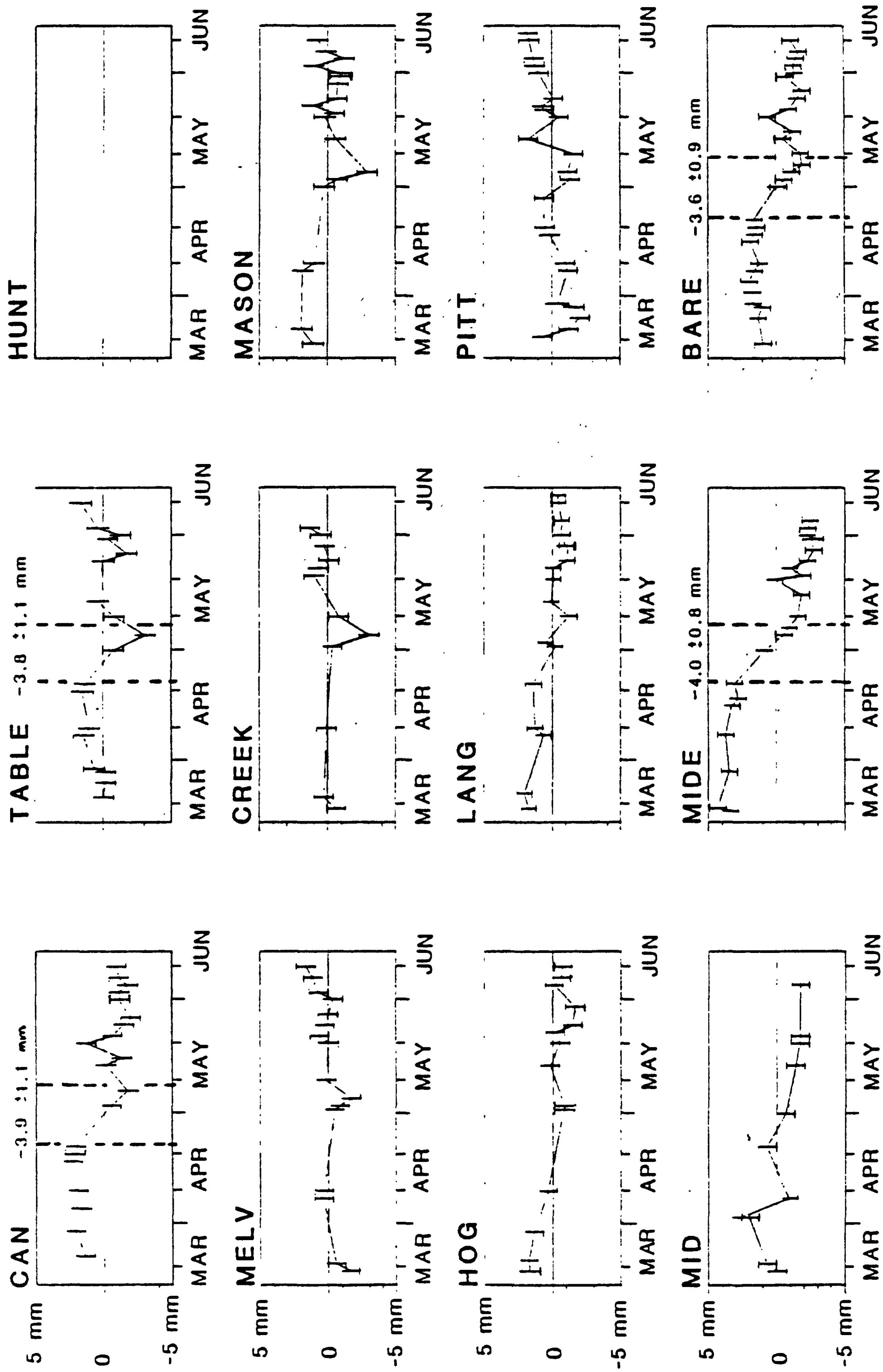

CAN
TABLE
HUNT
MELV
CREEK
MASON
HOG
LANG
PITT
MID
MIDE
BARE
5 mm
0
-5 mm
MAR
APR
MAY
JUN
-3.9 ±1.1 mm
-3.8 ±1.1 mm
-4.0 ±0.8 mm
-3.6 ±0.9 mm

Figure 14

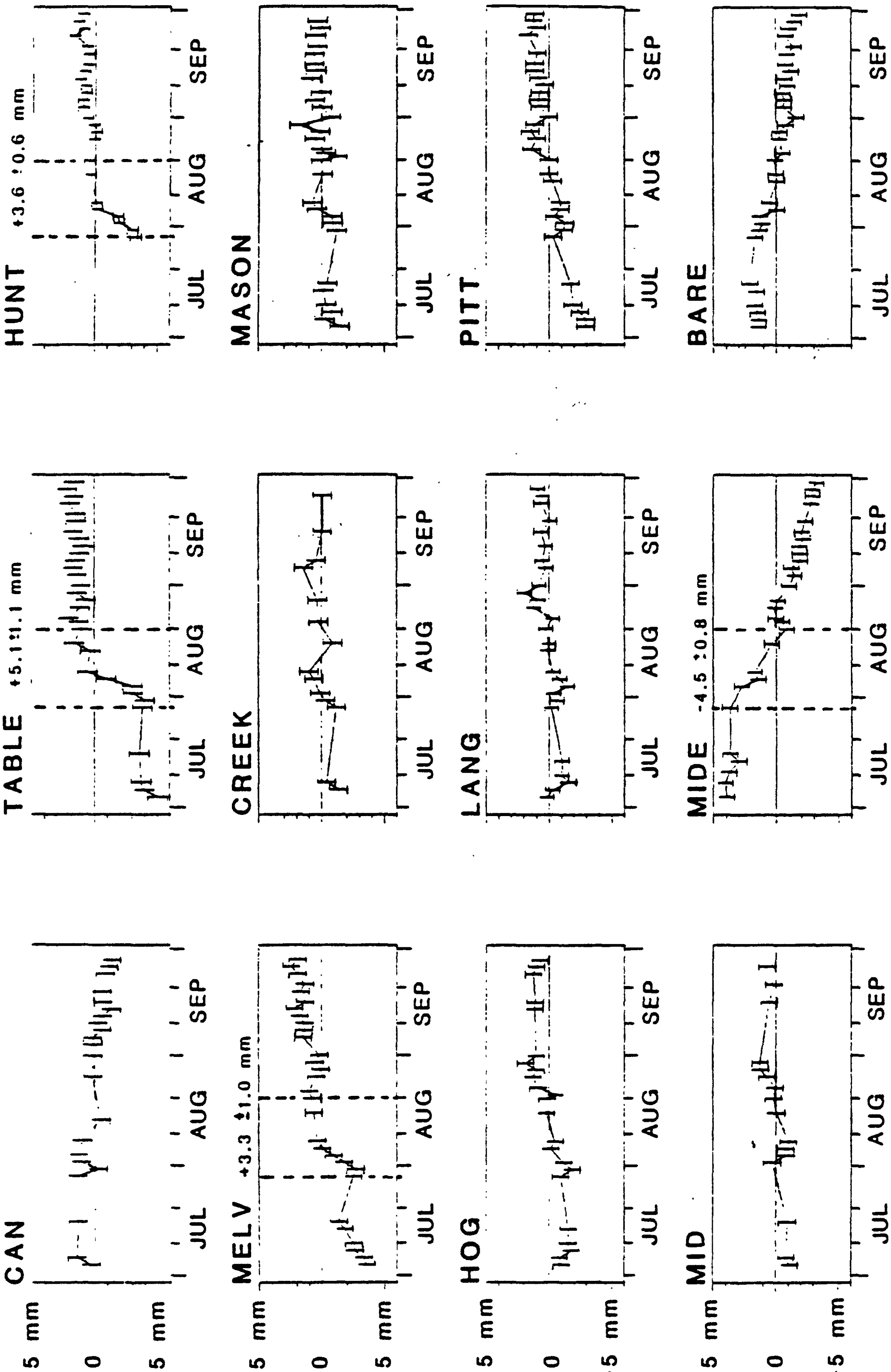

CAN
TABLE
HUNT
MELV
+3.3 ±1.0 mm
CREEK
MASON
HOG
LANG
PITT
MID
MIDE
-4.5 ±0.8 mm
BARE
+5.1 ±1.1 mm
+3.6 ±0.6 mm
JUL
AUG
SEP
5 mm
0
-5 mm

Figure 15

REPORT ON U.S. GEOLOGICAL SURVEY

NATIONAL EARTHQUAKE HAZARDS REDUCTION PROGRAM WORKSHOP

ON

SAN ANDREAS FAULT--MOJAVE SEGMENT

26-28 September 1985

DRAFT

WORKSHOP OVERVIEW

Objectives and Scope

On September 26-28, 1985, a workshop was held in San Bernardino to develop a plan for new earthquake prediction research on the Mojave segment of the San Andreas fault, with the specific goal of recommending new work needed to select sites and implement detailed geophysical, seismological, and geological monitoring of areas of potential rupture initiation on this segment.

The Mojave segment, as defined prior to the meeting, is the part of the fault between Tejon Pass and Cajon Pass. The recurrence data of Sieh (1978, 1984) from Pallet Creek indicate large displacement earthquakes occur along this segment on the average of 140-200 years. These data support an interpretation that recurrence at Pallet Creek results from an overlap of two rupture segments. One is the 1857 rupture segment. The lateral extent of the second rupture segment is not known. Whether this rupture initiates between Tejon Pass and Pallet Creek and propagates south, begins south of Cajon Pass and propagates through Pallet Creek to the north, or is bilateral, is uncertain. Using available recurrence data Lindh and Ellsworth (1984) calculated a conditional probability of >40 percent for a M 7.5-8 on the Mojave segment during the next 30 years. Sykes and Nishenko, using the different fault behavior models--one based on paleoseismicity data and the other based on displacement in 1857 and fault slip rate--calculated probabilities of 10 to 25 percent and 20 to 50 percent, respectively, for the next 30 years. Based on our present understanding of the Mojave sesgment, the next large event is likely to be the second rupture segment.

Twenty-two scientists attended the workshop, which was chaired by K. Aki (USC) and D. Schwartz (USGS). They represented University of Southern California, U.C. Santa Barbara, CalTech, Lamont-Doherty, Stanford, U.C. Los Angeles, and the U.S. Geological Survey (see attached list of participants). The workshop agenda included both invited overview presentations and a short presentation by each participant on what, where, and why things should be done. The general topics covered included structure and paleoseismology, instrumental seismicity, geodetic monitoring, and the potential uses of the Cajon Pass deep well. Following the presentations, the workshop divided into three subgroups for the purpose of developng specific sets of recommendations. The subgroups were: Geology (R. Wallace, Chairman), Short Baseline Borehole

Measurements and Crustal Structure (T. Henyey, Chairman); and the Network Seismology and Geodetics (L. Teng, Chairman). On the final day of the workshop J. Matti (USGS) and R. Weldon (USGS) led a field trip that looked at structural relationships in the vicinity of the confluence of the San Andreas and San Jacinto faults, visited the Cajon Pass drill site, and observed offset terraces and geomorphic evidence of Holocene faulting at Cajon Creek.

Based on the presentations and discussions at the meeting, several general points were made. These are noted below and are followed by the recommendations of the individual workshop subgroups.

- There was general agreement that to best understand the behavior of this part of the San Andreas fault, the section between Cajon Pass and San Gorgonio Pass should also be included as part of the Mojave segment.
- There was a consensus to strongly endorse and support investigations associated with the Cajon Pass drill hole, both at its present depth of 19 km and its proposed deepening to 5 km. The drill hole clearly has the potential to provide important geophysical and seismic data in a region that may represent a nucleation or termination site for future large events.
- An important concept discussed at the meeting that has the potential for developing into a framework for selecting sites for future monitoring was segmentation of the Mojave segment and identificiation of potential nucleation sites for future ruptures. Based on a combination of the geologic and seismicity data presented at the workshop, four potentially important areas were identified:

  Tejon Pass Area: This is a structurally complex zone that includes the big bend of the San Andreas and the intersection with the Garloc, White Wolf-Pleito, and Big Pine fault zones. Seismicity extends to a greater depth (20 km) and is more frequent than areas to the north and south. Focal mechanisms are variable.

  Lake Hughes: This is the location of a proposed change in the amount of slip (7 m to the north, 3 to 4 m to the south) during the 1857 earthquake. From Lake Hughes to Cajon Pass, seismicity extends to approximately 10 km and is characterized by focal mechanisms with a distinct oblique component.

Cajon Pass Area: This is a structurally complex zone representing the intersection of the San Andreas, San Jacinto, and Cucamonga faults. The southern end of the 1857 rupture appears to be located just northwest of Cajon Pass. Focal mechanisms are variable.

San Gorgonio Pass: A structurally complex zone containing stepovers and splay faults. Seismically is complex, extending to about 20 km with variable focal mechanisms.

SUBGROUP RECOMMENDATIONS

GEOLOGY

(R. Wallace, R. Weldon, D. Schwartz, G. Jacoby, J. Matti, E. Padovani, and R. Sibson)

1. Paleoseismology: The ability to understand the future behavior of the Mojave segment, especially with regard to where rupture may initiate so that appropriate monitoring can be developed, depends upon understanding its recent past behavior. Of primary importance are the lateral extent, timing, and slip distribution of past events, as well as information on slip rates along the segment.

   A. Trenching: There are a number of potential trench sites along the Mojave segment that appear to have stratigraphic and structural relationships suitable for developing better constraints on the timing and lateral extent of past events, and of fault segmentation. These are: Wrightwood, Three Points, Little rock, Palmdale Lake, Pitman Canyon, Hunts Lane-Barton, and San Bernardino (Fig. 1). The sites span the Mojave segment and the northern San Jacinto fault zone.

   B. Slip Rates: At present, slip rates for the segment are sparse and somewhat contradictory. Rates have been estimated at Cajon Creek of 25 mm/yr for the Holocene (Weldon and Sieh, 1985), at Pallet Creek of 9 mm/yr for the past 1600 yrs (probably a minimum, Sieh, 1984), and at three points of 45-60 m/yr for the past 1000 years (Rust, 1986). Additional late Pleistocene-Holocene slip rates, especially those that need to represent a similar interval, need to be obtained between well-constructed San Gorgonio Pass and Wallace Creek. These are important for evaluating models of fault behavior and earthquake recurrence (for example, uniform slip *vs.* characteristic slip). The localities noted in A. above as Yucaipa Valley are potential sites for developing slip rate data.

C. Dendrochronology Studies: Dendrochronology studies are strongly recommended. Alternative rupture models are derived, in large part, from the uncertainty in the timing of past events, especially the two or three most recent San Andreas earthquakes. This uncertainty is partially a function of the variability in the completeness of the stratigraphic record. However, to a very large degree it reflects problems with the precision of radiocarbon dates (the age range of each date), especially the multiple calendric dates yielded by radiocarbon dating of very young materials. Dendrochronology studies have the potential to significantly imporve our knowledge of the timing, and very possibly the lateral extent, of the two San Andreas events prior to 1857. The actual timing of the postulated 1720±50 earthquake (event X of Sieh) affects the recurrent interval at Pallet Creek as well as estimates of the elapsed time along the part of the fault south of Cajon Pass that did not rupture in 1857. In addition, because trees in the region are at least as old as 400 years, it may be possible to determine the timing of the pre-1857 event at Wallace Creek, which would provide data on recurrence of an 1857-type earthquakes. This event has been correlated with Sieh's event V at Pallet Creek (1550±70) but the basis for this is very weak and the timing of this event has not been constrained by dating at Wallace Creek or elsewhere along the 1857 rupture segment.

Tree rings have been looked at to study the effects of the 1857 rupture (Meisling and Sieh, 1980). New dendrochronology studies represent the potential to provide precise timing of past San Andreas events, and therefore greater insight into the future behavior of the fault.

2. <u>Tectonic Framework Studies</u>:

Sieh (1977) mapped the 1857 surface rupture and showed discrete steps in the distibution of slip. Tentative correlations suggest a potential relationship between changes in slip in 1857 and recognizable structural features (right steps, fault intersections) in the fault zone. Some of these structural complexities appear to correlate with seismic domains (Fig. 2). These may represent potential nucleation or termination sites of future events.

Continued regional geologic analysis aimed at understanding the tectonic framework, with special reference to the relationship between structure and seismicity, is required. This should include more detailed structural mapping of: a) relationships between the San Andreas, San Jacinto, and Cucamonga faults; b) San Andreas, Garlock, and Pleito-White Wolf faults, c) San Gorgonio Pass; and d) variability in the styles of secondary deformation along the length of the San Andreas fault.

SHORT BASELINE BOREHOLE MEASUREMENTS AND CRUSTAL STRUCTURE

(P. Davis, J. Healy, T. Henyey, A. Lachenbruch, P. Malin, A. McGarr, and W. Thatcher)

1. An instrument working group should be assembled to begin the design and construction of a monitoring package for the Cajon deep well.
2. Short baseline instrument deployments for earthquake prediction in the future may require one- to two-kilometer deep boreholes (6-1/2" diameter or greater) to achieve increased sensitivity and stability. At least some core in the fault zone should be obtained for material property measurements. Twelve holes in two to three clusters would look at potential nucleation sites defined by geologic analysis. One cluster should support the Cajon deep drill hole. Hole cluster would permit:
    - o Cross hole work (seismic, electrical)
    - o Spatial distribution of stress, temperature, *etc.*
    - o Redundancy and coherence of measured signals. Holes should be located on both sides of the fault and one in the fault zone. Perhaps two-thirds of the holes in a given cluster should be close to the fault (within 1-3 km) and one-third at distances of 1/2 and 1 seismogenic scale depths.
3. Important Measurements:
    - o Dilatometer and/or three component strainmeter
    - o Pore pressure
    - o Temperature (0.1 m degree)

- o Seismic
- o Stress and permeability
- o Heat flow

4. The boreholes, together with active surface reflection and refraction seismology (VSP comprehensive; seismic monitoring -3 comp 1-2Hz; travel time anomalies, particularly cross-fault), would be used to determine fault structure, fault orientation, and proximal crustal structure in an approximately 10 km x 10 km region around the cluster. Surface reflection and refraction seismology would continue to be used with surface geology to help delineate potential nucleation points for siting future deep holes.

NETWORK SEISMOLOGY AND GEODETICS

(K. Aki, T. Heatokn, L. Jorres, H. Kanamori, J. Langbein, L. Teng)

The probability of a large earthquake occurring on the Mojave section of the San Andreas fault is estimated to be as great as 50 percent within the next 30 years. This is the highest probability of a M > 7.0 occurring anywhere in California. A better understanding of the physical processes controlling the behavior of the Mojave section is needed. This includes understanding the mechanics controlling the ends of potential rupture zones, the spatial variations in amount of slip in 1857, the effect of the Garlock fault on the San Andreas, and the significance of ongoing microearthquake activity. The followingmonitoring goals and research and to attain them identified to help in alleviating this major threat:

Monitoring Goals:

- o Monitor physical processes occurring on the fault and give notification of any obvious changes. A major way to understand the physical processes that lead to failure in major earthquakes is to collect data at the site of such an event and the Mojave segment is a likely location. Without such data the question of possible fundamental differences between moderate and great earthquakes cannot be resolved. Given that recordings of physical parameters are being made on the Mojave section, we will then be responsible for notifying civil authorities of any changes in those parameters.

- o Provide real-time assessment of seismic crises. Seismic activity is likely to be the most obvious short-term precursor to a large earthquake. In the event of moderate earthquake activity on or near the San Andreas fault, the local civil authorities will be asking the seismological community for advice and we should prepare ourselves for those questions.

Specific Research Recommendations:

In the long run, the only means of attaining these goals is through a thorough understanding of the physics of the earthquake process. At a more practical level, intermediate steps must be taken to conduct the research to develop that understanding. We propose the following plans to make the necessary research feasible and to provide continuously updated estimates of seismic hazard during periods of seismic crises:

1. Formation of a Data Center. To conduct research or evaluate the seismic hazard, data is needed in a timely and organized manner. When large amounts of data are being collected it can be even more difficult to organize it and maintain access to it. Given the large scale data collection proposed for the Mojave section, it is crucial that there be a central data acquisition center for the whole experiment that will allow use of the data by all interested scientists. In addition, the data center would process information in real time to provide timely updates of the earthquake hazard from the Mojave segment.
2. Upgrading the Seismic Network: The present USGS-CalTech regional network of telemetered short-period vertical seismometers provides sufficient data to locate earthquakes of $M > 1.0$ anywhere on the Mojave segment. However, the dynamic range of this network is generally less than 40 dB. This means that most events larger than M 2.5 overdrive the system and little useful information is available for the most significant events. Furthermore, because horizontal components of ground motion are unavailable, interpretations of later phases (such as S) are very unreliable. At quiet sites, the total range of ground motions from background noise to strong shaking from large earthquakes is about 160 dB. Digital telementry could increase dynamic range to about 100 dB/channel, but it would be necessary to go to two sensors to maintain a system that would yield on-scale recordings during the most significant seismic sequences. Digital tele-

metry and three-component stations are very important goals for the present array. In addition, it is desirable to locate some stations in shallow boreholes to improve signal-to-noise ratios. Finally, some new stations should be located within the San Andreas fault valley in order to study the nature of any special wave propagation effects within the fault zone and to monitor any possible changes in elastic parameters.

3. Strain Measurements. Measuring deformation of the crust is essential to both research and monitoring efforts. Because localization of the strain at any one site in the Mojave section is not an obvious certainty, it is important that spatial variations in the strain be recorded. Towards this end, geodetic surveys of the fault must be conducted at regular intervals. If sufficient stability is developed in the GPS systems, these could replace some of the geodetic measurements. Three or four new two-color laser geodetic nets would also provide important information about the strain and its regional variation. Possible sites for the new nets would be Pearblossom, Palmdale, Cajon Pass, Lake Hughes, and Gorman.

A. Two-Color Geodimeter Networks

- Antelope Valley Networks: These networks, which already exist, are located at Pearblossom and the Buttes, approximately 30 km NNE of Pearblossom and Palmdale. The purpose would be to investigate and determine both the spatial and temporal coherence of strain accumulation on the San Andreas fault in this centrally located segment. To accomplish this study, an observatory based two-color instrument should be reinstalled at Pearblossom to continue the bi-weekly distance measurements that were initiated in late 1980 but ceased in early 1984. Less frequent surveys at the Buttes and Palmdale can be accomplished with a portable, two-color geodimeter.
- Use of two geodimeters to measure long-term strain accumulation where geologists have identified structural complexities would involve establishling extensive networks near Gorman, Lake Hughes, and Cajon Pass. For example, a comprehensive network within Cajon Pass could delineate the strain accumulation between the San Andreas and San Jacinto faults. These networks need only be resurveyed infrequently to determine the long-term deformation. If necessary, subsets of these networks could be re-measured more frequently to monitor the temporal pattern of strain accumulation.

B. Use GPS on long baselines (> 20 km) to determine pattern of strain accumulation in the Mojave segment provided that the stability of this technique is demonstrated to be better than one part per million.

C. Continued precise gravity monitoring ($\sigma \approx 5$ μGals) throughout the Mojave segment. These measurements should be taken several times each year and at sites such as Pearblossom for comparison with geodetic measurements.

D. Resurveys of existing geodolite networks (Los Padres, Tehachapi, and Cajon) and releveling in Southern California should be carried out roughly once every other year. The Palmdale network should be resurveyed using the two-color geodimeter because the baselines are too short for the geodolite to have adequate precision.

4. Recognition of Creep. Surface creep on a fault is an obvious precursor that is well-monitored in other parts of California. No creepmeters are presently operating on the Mojave section and this situation should be remedied. Creepmeters should be installed at regular intervals on the Mojave section (15 km) and telemetered to the central monitoring site.

5. Develop Warning Algorithms. Data from the Mojave segment could be used to test automated earthquake hazard recognition algorithms. Routine calculations of various seismic parameters (such as code decay, b-value, focal mechanisms, and spectra analyses) could be performed at the data center. The Mojave segment should provide data to test the significance of proposed prediction techniques.

6. Develop Models of the Physical System. Deterministic computerized models that approixmate the physical properties of the San Andreas system are important to gain better insight into the significance of observed parameters. These models help to integrate what is known about the behavior of this fault and they also help to identify key important unknowns in the problem.

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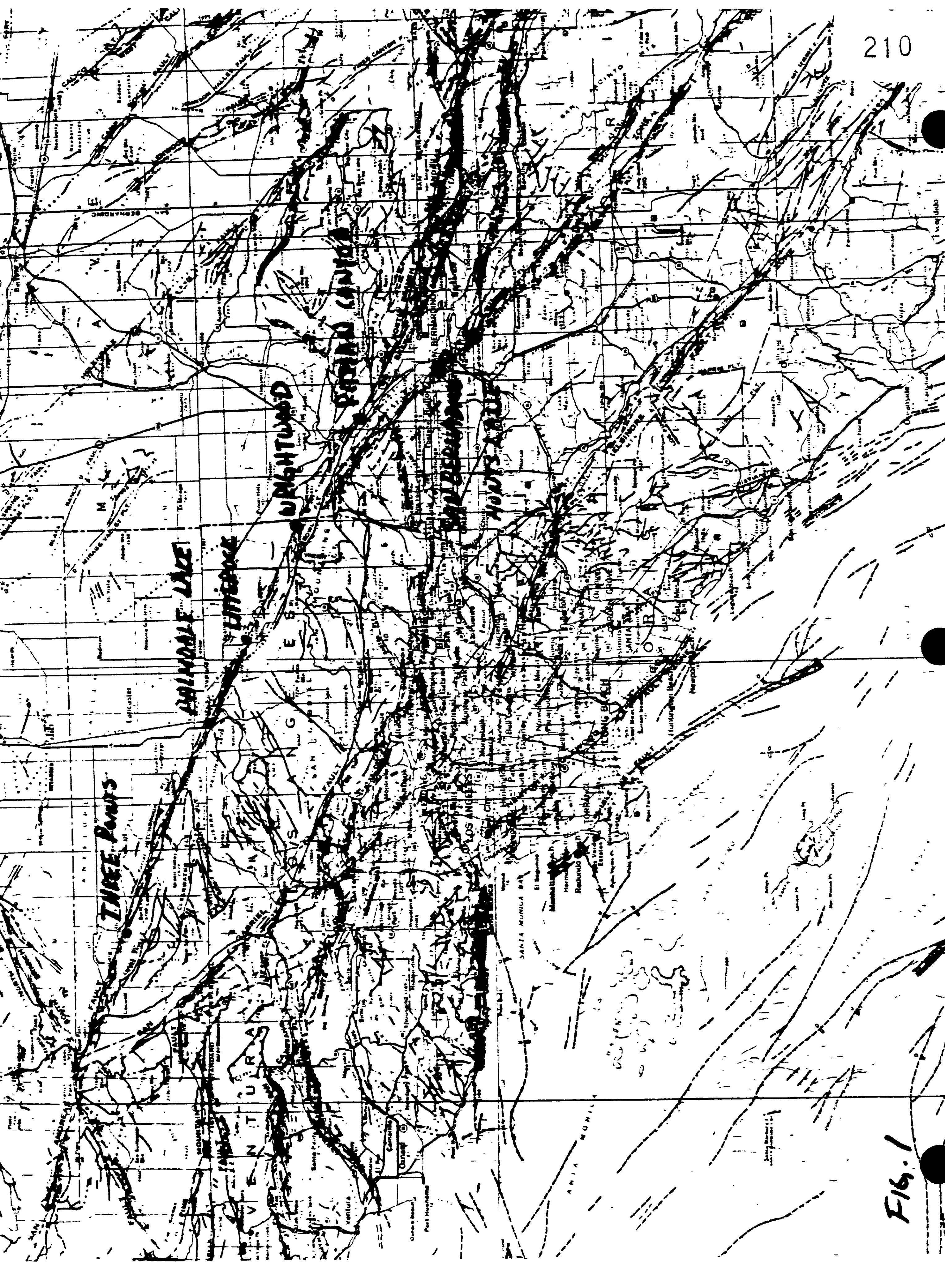
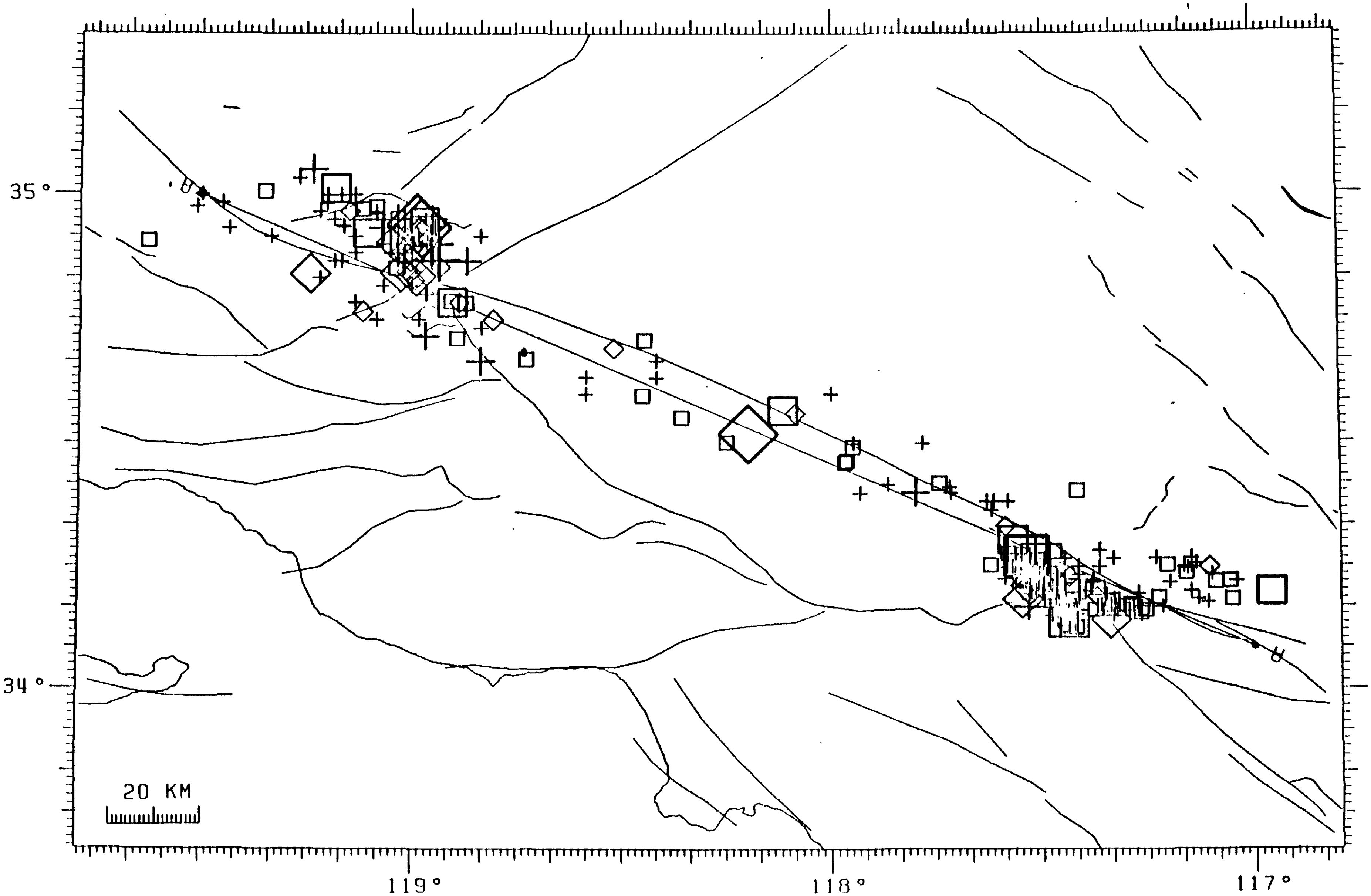
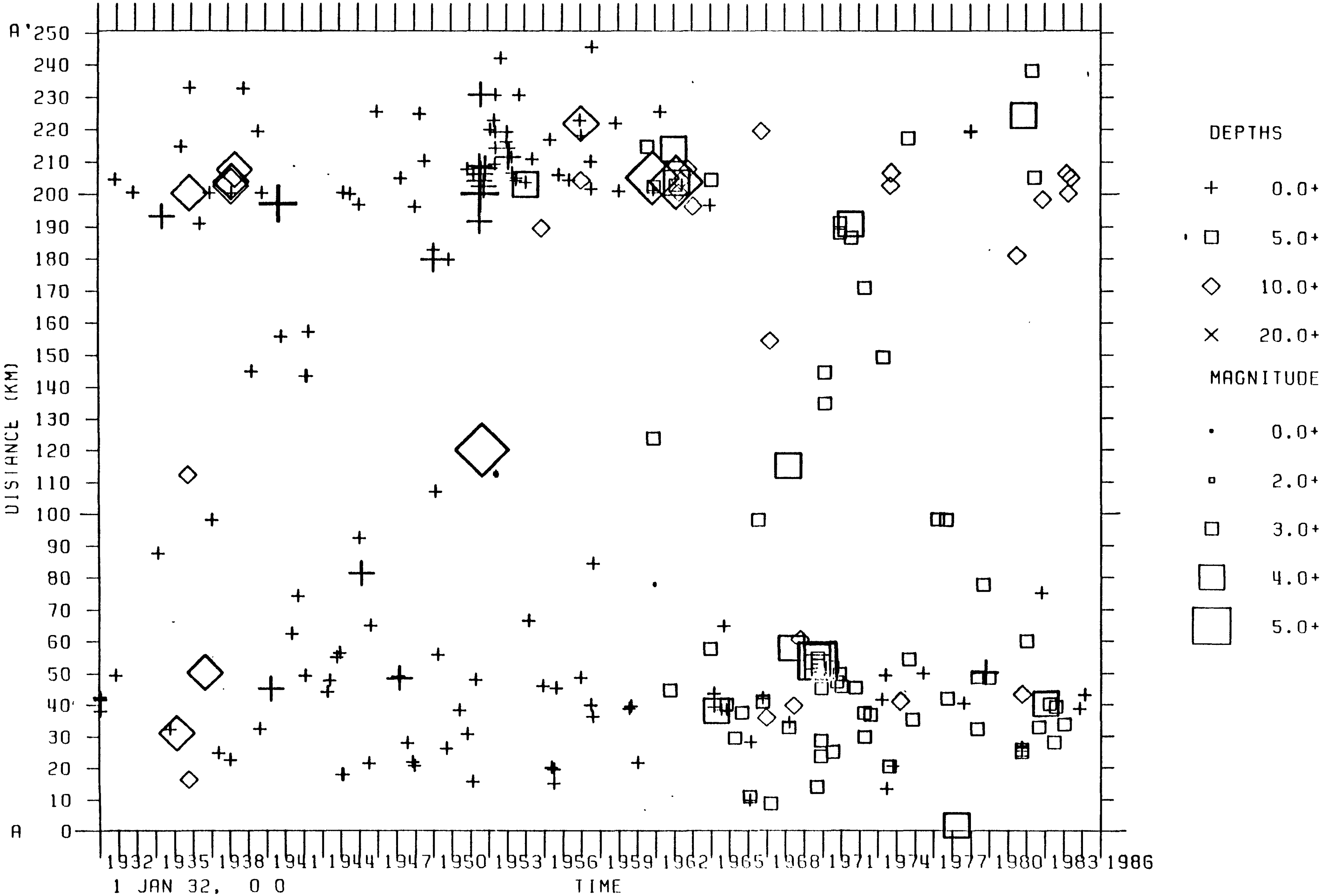

THREE POINTS
LITTLEROCK
WRIGHTWOOD
LOS ANGELES
LONG BEACH
SANTA MONICA BAY
Oxnard

Fig. 1

MOJAVE SECTION
1932 - 1985 M > 3.0



1932 - 1985 M > 3.0



REPORT ON UNITED STATES GEOLOGICAL SURVEY
NATIONAL EARTHQUAKE HAZARD REDUCTION PROGRAM
WORKSHOP ON

INTENSIFIED STUDIES ON THE SAN JACINTO FAULT ZONE

10-12 October 1985

INTRODUCTION AND SUMMARY

During 10-12 October 1985 a workshop sponsored by the United States Geological Survey's (USGS) National Earthquake Hazard Reduction Program (NEHRP) and co-chaired by Hiroo Kanamori and Jim Brune was held in Palm Springs, California. Its purpose was to discuss the seismic potential of the San Jacinto fault zone and to frame recommendations aimed at selecting sites for intensified earthquake prediction monitoring experiments. The meeting brought together 25 researchers from the USGS, the California Institute of Technology, the Universities of California at San Diego and at Santa Barbara, Columbia University, Lamar-Merifield Consultants and San Diego State University actively involved in studies of the San Jacinto fault and its environs. A list of attendees is attached. The workshop included both open discussion by all participants and deliberations by four topical subgroups. It was followed by a fieldtrip to examine recent geomorphic features of the fault near Anza (led by Robert Sharp and Tom Rockwell) and a site visit to the crustal movement observatory at Piñon Flat (led by Duncan Agnew).

Most if not all of the southern San Jacinto fault zone ruptured in a series of M=6 to 7 earthquakes between 1942 and 1968. For this reason there was general agreement to concentrate attention on the northern half of the San Jacinto fault zone. In addition, because a 40-km-long segment near Anza has not ruptured since at least 1890 there was considerable discussion of detailed work that should be performed in this region, especially studies that would utilize the 10 station broad-band, high dynamic range seismic network that has been sited within the slip gap.

The recommendations of the subgroups form the main body of this report and are given below. In each case there is a separation between immediate objectives, ones that could be achieved within the present scope of the earthquake program, and long-term plans that would require an expanded program. Among the most critical short-term goals are:

- o the refinement of slip-rate and most-recent-event characterization by paleoseismic studies, especially in the Anza slip gap region;
- o development of methodologies for using digital seismic data in real-time prediction monitoring.

Longer term objectives include:

- o expansion and densification of geodetic networks measuring horizontal crustal deformation;
- o upgrading of selected regional seismic network stations along the northern San Jacinto fault to the standard of the digital state-of-the-art local network in the Anza region;
- o intensified seismic and crustal deformation monitoring of specific fault segments selected on the basis of the paleoseismic studies mentioned above.

ATTENDEES

California Institute of Technology (CIT)	Diane Dozer Art Frankel Hiroo Kanamori Harold Magistrale Chris Sanders
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United States Geological Survey (USGS)	Bill Ellsworth Joe Fletcher Gary Fuis Doug Given Tom Hanks Steve Hartzell Doug Morton Elaine Padovani Dave Schwartz Bob Sharp Wayne Thatcher

Subgroups (chairman underlined)

1: Paleoseismicity/fault slip	Paul Bodin, UCSD Paul Merifield, L-M Doug Morton, USGS Tom Rockwell, SDSU Dave Schwartz, USGS Bob Sharp, USGS
2: Improved regional seismic/geodetic coverage	Diane Dozer, CIT Bill Ellsworth, USGS Doug Given, USGS Hiroo Kanamori, CIT Mike Reichle, CDMG, UCSD Chris Sanders, CIT Leonardo Seeber, LDGO
3: Intensified studies at Anza/seismic	Jim Brune, UCSD Joe Fletcher, USGS Art Frankel, CIT Gary Fuis, USGS Tom Hanks, USGS Steve Hartzell, USGS Harold Magistrale, CIT Frank Vernon, UCSD
4: Intensified studies at Anza/Non-seismic	Duncan Agnew, UCSD Elaine Padovani, USGS Art Sylvester, UCSB Wayne Thatcher, USGS

Subgroup 1 Recommendations: Paleoseismicity/Fault Slip

Summarized by R.V. Sharp

These recommendations for geologic investigation along the San Jacinto fault zone are separated according to decreasing priority into categories bearing on (1) characterization and age estimation of the last surface faulting events, together with past recurrence times and slip estimates per event, (2) the slip rate budget among the various strands of the zone and possible longitudinal and/or temporal variations, and (3) more precise identification of the location of concealed fault strands by seismic reflection.

Category 1 recommendations can begin to be implemented under the existing NEHRP, but completing this work and fully implementing Category 2 and 3 recommendations will require an expanded program.

Category 1 [Immediate Objectives]

In order to directly answer whether a slip gap on a time scale longer than the historic record of seismicity exists in the Anza region, the study rated with highest priority is to identify and date the most recent movement on the main strand of the fault at Anza. For complete characterization of this event, its slip should be established at as many places as possible, and its surface rupture length determined. However, the discontinuous distribution of Holocene sedimentary materials necessary for this kind of study probably will allow only a rudimentary picture. The most recent slip event should be identified on the continuous main strand (Clark fault) in southeastern San Jacinto Valley near Hemet and in Clark Valley in order to constrain the fraction of the strand length that slipped at the time of last movement at Anza.

Other problems that are important to the understanding of the recent past behavior of the San Jacinto fault zone near Anza include the time between previous slip events (recurrence time), the amount of slip per event (lower-bound measures of the earthquake magnitudes), and the rate of movement in relatively recent versus more ancient periods of time. Because the fault is complicated by branching and en echelon splaying near Anza, the Holocene history should be worked out for each of the principal strands to the extent allowed by the present distribution of Holocene deposits across the fault traces. Answers to these questions will shed light on whether activity jumps from one strand to another from event to event, or whether several strands go at once.

These kinds of questions may be answered to a certain extent by a careful study of sedimentary materials in their naturally exposed condition, which at some locations around Anza will allow sufficiently good three-dimensional understanding of stratal relations. More generally, extensive trenching or the creation of other types of artificial exposure will be required to develop an adequate picture at critical locations. As these kinds of studies tend to go, the first sets of data developed by any method may tend to raise more questions than they answer. To develop an unambiguous late Holocene history of movement for each of the important fault strands probably will require a succession of studies over a considerable length of time.

Contractors of USGS currently are funded to do initial studies of Holocene fault slip at Anza, so that some answers and/or important additional questions may be posed within the coming year.

Special notice should be taken of the restricted abundance of material that offers potential for Holocene fault slip study in the Anza region. Some unique features that will have critical interpretive importance will undoubtedly be discovered, and to the maximum extent possible these materials should be preserved. Because the only effective long-term way to preserve relations exposed by trenching is by backfilling and this itself is somewhat deleterious, the original exposures should be studied in detail by as many experienced fault geologists as possible. The informally traditional "visitor day" at trench sites generally does not offer adequate time for close scrutiny of important features by large groups of people.

Category 2 [Longer Term Objectives]

The slip and slip-rate budgets for the numerous strands of the San Jacinto fault zone throughout its regional extent are important subjects to investigate. In principle, this involves the same kind of investigations outlined in Category 1 but at locations distributed throughout the length of the zone from its junction with the San Andreas fault, as well a extending backward in time to the most ancient offsets that can be detected. Data now available suggest a possible longitudinal peak in total displacement (and perhaps slip rate) of the fault zone in its central part near Anza. Although the reality of longitudinal and/or temporal variation of slip and slip rate on a regional scale is not firmly established, the possibility of such variations has important implications to both the tectonics of plate motions and earthquake occurrence.

Category 3 [Longer Term Objectives]

The location of some segments of the principal strands within the San Jacinto fault zone are only approximately known primarily because of accumulation of very young alluvium. These concealed fault strands offer some potential for further study, as outlined in Categories 1 and 2, but require more precise location by geophysical techniques, such as shallow seismic reflection profiling (perhaps with a miniSOSI source). Such sites apparently have not ruptured in very recent time. The age of the unfaulted alluvial skin at these sites gives a minimum estimate of the elapsed time since the last movement. Uncertainty of fault continuity or geometric complication could also be clarified with shallow reflection profiles.

Fault strands where reflection profiles would be useful include the Clark fault in Clark Valley, the Coyote Creek fault along the southwest-facing scarp at Coyote Mountain, the unnamed concealed fault along the east side of Coyote Mountain, the northward projection of the Clark fault into San Jacinto Valley where is possibly joins the Casa Loma fault, and the southernmost part of the Claremont fault along the eastern edge of San Jacinto Valley.

Subgroup 2 Recommendations: Improved Regional Seismic and Geodetic Studies

Hiroo Kanamori, Chairman

In view of the simplicity of the fault geometry, higher seismic potential, and higher social impact, we recommend that the northern part of the San Jacinto fault (north of the 1968 Borrego Mt. earthquake zone) be given higher priority than the southern part.

Recent seismicity studies have demonstrated that the distribution of micro-earthquakes in the San Jacinto fault zone seems to delineate the bottom of the seismogenic zone and the rupture zone of the larger earthquakes. Since the depth variation of the seismogenic zone has an important bearing on nucleation of seismic rupture and the distribution of coseismic slip of large events, it is important to accurately map the spatial and temporal distribution of micro-earthquakes in the fault zone. Precise determinations of the focal depths are especially important. In order to improve the accuracy of earthquake locations, use of S waves recorded on horizontal-component seismograms is crucial. Also better crustal models are needed to improve the location accuracy.

In the past, location and focal mechanism have been the only routinely determined seismic source parameters. However, recent developments in the methodology of waveform analysis have made it possible to determine the source dimension, stress drop, and the rupture direction from broadband seismograms. The stress drop is a key parameter to monitor temporal variation of stresses in the fault zone. The direction of rupture provides direct information on the kinematics of faulting. In order to routinely utilize this technique, the regional seismographic network should be upgraded to a broadband system and methodology for routine determination of these source parameters should be developed.

In order to understand better the constitutive relation and the frictional characteristics of fault zones, more heat-flow measurements in the San Jacinto fault zone are needed.

Determination of the spatial and temporal pattern of strain accumulation is the key to earthquake prediction. However, the present coverage of strain network in the San Jacinto fault zone is inadequate for this purpose. Since we are concerned with variations of strain field over a length scale of a few km, it is necessary to establish a network which has a spatial resolution of about 5 km. To supplement geodetic data, installation of alignment or quadrilateral arrays at selected sites is necessary.

In addition to the main trace of the San Jacinto fault system, cross faults (e.g., Inspiration Point fault) need to be investigated. These faults are important not only for understanding the kinematics of the San Jacinto fault system but also for evaluation of seismic hazard.

To utilize existing data more effectively for earthquake prediction studies, standardization of data format and software is desirable.

Immediate Objectives

For more general research purposes, we recommend that:

(1) The data-format of existing data be standardized for easy access by investigators.

(2) Methodology for routine determinations of detailed source parameters (e.g., rupture direction, stress drop, etc.) be developed.

(3) Efforts be made to utilize existing data such as seismograms of old events (e.g., 1923 earthquake in the northern San Jacinto fault).

Long-Term Objectives

For higher resolution studies of the northern San Jacinto fault zone and its environs we recommend expanded programs along the following lines:

(1) The regional seismographic network be upgraded to a 3-component-broad-band-high dynamic range system. A minimum of 20 to 40 stations should be upgraded.

(2) Detailed crustal structure studies and heat-flow measurements be conducted.

(3) A dense (resolution or approximately 5 km) and broad geodetic network be established spanning the San Jacinto fault zone. At a few areas of special interest (e.g., near cross faults, near locked segments, etc.), denser networks be established.

(4) Detailed mapping of spatial pattern of earthquakes be undertaken.

Subgroup 3 Recommendations: Intensified Studies at ANZA/Seismic

Jim Brune, Chairman

I. Immediate Implementation

1. Support continued research on data now being collected.

We should make every effort to take full advantage of data currently being collected by the Anza digital array. This will require a vigorous, adequately funded research effort. The research is important in its own right, but is also crucial in helping make decisions about expanding the array and implementing other arrays. The best way to discover present inadequacies and develop new techniques, is to keep up to date on analysis of the data presently being collected. Suggested studies: Focal mechanisms, micro-mechanics, coherence, directivity studies, spectra and source physics, wave polarization, V_p/V_s, temporal variations.

2. Experiment with variations in threshold gain.

Experiments should be carried out with the gain high enough to record background noise at all frequencies, to record many small events, and also to record some N.T.S. explosions and quarry blasts. Except for a few weeks operating at these high gains, the gain should be lowered enough to eliminate the gap between our saturation or non-linear threshold, at larger magnitudes, and the trigger level of the strong motion array. No important larger events should be missed.

II. Long-Term Objectives

1. Results obtained to date justify increasing the number of seismic stations in several ways:

- Add a few stations in areas verified to have minimal waveform distortion.
- Add a few stations to improve the overall geometry and coverage of the array, even if this requires that several stations have repeater links.
- One station should be added to the northwest and a couple to the southeast.

2. For strong motion instrumentation we recommend:

- All existing telemetry sites should also be equipped with strong motion instruments, so that any large earthquakes which occur will be recorded on scale at the telemetry sites, allowing extrapolation from small earthquakes to larger ones.
- Eventually we hope that strong motion data can be telemetered along with the weaker motion, as envisaged at Parkfield.
- Outside of the local strong motion array we should encourage installation of equipment which will take full advantage of any large events. In particular we encourage engineering instrumentation of buildings and sedimentary basins.

3. To further understand attenuation of seismic waves we recommend:

- Several boreholes be drilled and equipped with down-hole instrumentation to determine near surface weathering and sedimention effects on waveforms. These will be important in understanding attenuation and scattering.
- A coherence array should be operated at one of the down-hole sites to help separate near surface and deeper contributions to incoherence.

4. To determine velocity structure at Anza and adjoining regions, we recommend detailed refraction, wide angle refraction, and reflection surveys to determine crustal structure; this could be carried out in

cooperation with PACE (Pacific-Arizona Crustal Experiment), PASSCAL and other crustal structure programs with interest in the area.

5. To implement special studies on the Cahuilla shallow earthquakes, we recommend that if those earthquakes are verified to have depths as shallow as 1-2 km, special studies should be implemented to take advantage of this to directly study accessible events intermediate between normal earthquakes and laboratory simulations of rock fracture.

Subgroup 4 Recommendations: Intensified Studies at Anza/Non-Seismic

Duncan Agnew, Chairman

The main area covered is as described in the title; it should be noted that geologic studies were not included. The subgroup did not completely restrict itself to the Anza area, but one result that became very clear during the conference is that that region is the only one in which the fault is clearly defined enough for some experiments to take place (such as creepmeters). Both northeast and southwest of the 'Anza gap' there are many fault strands in the fault zone, and instrumenting all of them is probably not practicable.

The following list is arranged into groups, roughly in order of increasing cost, and hence (probably) in order of how soon different projects could be started. The times given are of course only rough guesses. Within each group the order is arbitrary.

Immediate Projects
(1-2 years)

1. **'Archival' low-precision fault-crossing measurements.** When the fault ruptures it would be nice to have as detailed a picture as possible of the size of the offset; at the moment so few structures cross the fault that we would know very little about slip distribution along it. What is needed is lots of relatively imprecise (1 cm accuracy) measurements. These are called 'archival' because they would be made once and not repeated (unless there is evidence for substantial creep). These would

also give prepared locations at which postseismic slip could be monitored. The only maintenance required would be occasional checks to ensure that whatever markers are put in have not been destroyed. Since the expected rupture would be primarily strike-slip, most such measurements would need to record horizontal deformation. They can be done at low cost; the initial need is for someone to organize and archive, whatever measurements are made by different groups. A big improvement over the current situation could be achieved just with taping between monuments; somewhat better results (especially in rough terrain) would demand EDM equipment. (Higher-cost options are discussed below.)

2. **Repeat high-precision fault-crossing measurements.** To look for fault creep, measurements must be made to better than 1 mm precision and repeated at least annually. The Anza segment does not seem to be creeping, but this needs to be checked, and it would be desirable to have measurements on other branches of the San Jacinto as well. UCSB currently has 3 high-precision level lines crossing the fault; the most urgent need now is for more horizontal control. It has been suggested that, in the rugged terrain of the San Jacinto fault, quadrilaterals may be better than alignment arrays. This needs to be checked, but again, the main need may be for coordination between different groups. The number of sites appropriate for the Anza area would not seem to be more than three.

3. **Gravity measurements.** This is the cheapest sort of large-scale geodetic measurement available. A network was established by John Fett in the area of the northern San Jacinto in the late 1970's; it would be desirable to recover his stations and make whatever measurements are needed to tie that network into the existing USGS net and the absolute gravity site at Piñon Flat Observatory (PFO) (though this may have already been done). It would

be valuable to establish a higher density of stations in the Anza area; at present these are very few.

Short-Term Projects
(2-4 years)

1. **Geodetic surveys.** The result of King and Savage [1983], that shear strain rates fall off from 0.4 με per year in the fault zone to 0.1 με per year (or less) 20-30 km away (a differential motion of 10 mm per year) is the best evidence now available on slip at depth on the San Jacinto fault. It implies that the fault slips 'freely', and apparently aseismically to very shallow depths. We need to confirm and extend this result with more detailed profiles of strain rates on both sides of, and crossing, the fault, and also obtain information on how strains vary along the fault. The existing Geodimeter net, while very valuable, has too great a spacing and insufficient coverage. The sooner additional measurements are made, the sooner repeating them will give useful results. (Note that the goal here is **not** to monitor strain changes with time, as now done at Parkfield, but to get a spatially more detailed picture; these surveys would be repeated annually at most.) The types of surveys needed are:

   A. Extension of the existing network southeast along the fault. This gets into geologically complex regions, and geodesy might provide some evidence to help sort out what strands of the fault are now active.

   B. A detailed trilateration network (line lengths of order 5 km) running at least from Piñon Flat to Cahuilla, possibly to Aguanga. This might require use of the 2-color EDM system, operating in a roving rather than fixed mode.

C. Leveling over the same path (which is relatively flat). This should be done at least once to check against older NGS leveling.

2. **Shallow boreholes.** The main purpose of these would be to try to get a better heat-flow near Anza, to test the suggestion of Lee [1983] that there is a heat-flow anomaly across the San Jacinto fault. In planning and drilling these holes, consideration should be given to using them for other studies, especially water-level monitoring and shallow stress measurement.

3. **Creepmeters.** While there is no evidence for creep, installation of a creepmeter is relatively inexpensive and would provide data relevant to precursory motion before an earthquake. In view of the complexity of the fault zone in most places, the Anza area seems to be the only one in which such an instrument could be sited.

Long-Term Projects
(3+ years)

1. **Repeat geodetic monitoring.** These would be measurements in the style used at Mammoth and Parkfield: a fixed 2-color EDM station, continuously (or at least very frequently) occupied, with radial lines. Table Mountain, Thomas Mountain and Santa Rosa (the latter for comparison with PFO) are possible sites.

2. **Precise deformation monitoring.** This encompasses installations of strainmeters and tiltmeters. The instruments at PFO currently give excellent coverage at one point, but in the spirit of looking at strain rate falloff away from the fault, there would seem to be a case for more installations. A set of instruments running southwest from Anza could be useful. It

remains unclear what the capabilities of the techniques are, and they are relatively expensive to set up and to keep operating, but these are one of the few types of non-seismic measurements that could be useful for short-term monitoring.

3. **Deep stress measurements.** In terms of understanding the physics of the fault, it seems very important to relate short-term strain rate to absolute stress levels. The best measurements would be made in deep boreholes. This is probably the most important non-seismic study that could be made of the Cahuilla swarm, especially if the seismogenic zone is shallow enough to be drilled into.
4. **Water-level monitoring.** While not inherently expensive, this is included here because the best results are likely to come from deep holes, such as those drilled for stress measurement. (Note that fractured, and open, holes are undesirable for borehole strainmeters, so that water level measurements probably would not be made in holes drilled for them.)
5. **Detailed near-field positioning.** If more thorough coverage of post-seismic displacements near (< 1 km) to the fault is wanted, more expensive procedures than those described above will be needed. Two possibilities (both of which need investigation) are aircraft laser-ranging and photogrammetry.

REFERENCES

King, N. and J.C. Savage (1983). Strain-rate profile across the Elsinore, San Jacinto, and San Andreas faults near Palm Springs, California, 1973-81. Geophys. Res. Lett., 10, 55-57.

Lee, T-C. (1983). Heat flow through the San Jacinto fault zone, southern California. Geophys. J. Roy. Astron. Soc., 72, 721-731.

DRAFT

Workshop on the Southern San Andreas Fault
November 21-23, 1985

The workshop on the southern San Andreas fault was organized to (1) assess the state of understanding of the potential for a large earthquake along this section of the fault, and (2) discuss investigations and instrumentation that could lead to a testable hypothesis about the time of the next large earthquake. The boundaries of the fault segment were the subject of some discussion, and indeed, merit further investigation. The southern end of the segment is commonly taken to be near Bombay Beach, where the San Andreas fault is intersected by a more northerly striking zone of high seismicity extending northnorthwest from Brawley and the Imperial Valley. The north end of the segment is taken variously as Cajon Pass or the San Gorgonio "knot." Between Bombay Beach and San Gorgonio Pass, the faulting is relatively simple, virtually pure strike-slip and displays copious geomorphic evidence of late Quaternary activity. In contrast, faulting between Cajon Pass and San Gorgonio Pass is quite complex, involving several strands of various ages of faulting and considerable dip-slip, as well as strike-slip movement. Whether a single earthquake could rupture through the San Gorgonio "knot" is a subject of continuing discussion.

Agreement on the potential of the segment for a large earthquake (magnitude 7 or larger) was substantial, based on the geologic evidence for continued movement, current evidence for about 2 mm/yr of creep and geodetic evidence for strain accumulation. The alternative hypothesis is that faulting along the segment occurs primarily as creep. There is enough uncertainty currently in the long term geologic slip rate so as not to exclude this possibility. Additionally, to date the trenching studies have not demonstrated convincingly that the observed offsets occured suddenly as fault rupture accompanying earthquakes, although that is the favored interpretation of those engaged in the work. The density of geodetic observations is also such that the possibility of distributed permanent deformation, as opposed to elastic strain accumulation, cannot be ruled out. Virtually all participants, however, were inclined to the view that the segment has the potential for a large earthquake.

The time at which the next large earthquake might be expected is constrained primarily by the preliminary work of Sieh, suggesting a recurrence interval of about 150 years. No earthquake with significant surface rupture, or large earthquake without surface rupture, has occurred along this segment at least since the 1850's, and possibly several decades earlier. These two pieces of evidence argue that a large earthquake along the fault is likely within the next several decades.

Additional investigations are needed in the following areas:

- Neotectonic framework studies—Geologic and geophysical investigations aimed at delineating the individual active faults, particularly at the ends of the segment, and deciphering the age, rate and style of movement on each.

- Paleoseismicity studies—Work is currently underway at Indio and planned near Bombay Beach. Additional work is needed, particularly in and near the San Bernardino mountains at the north end of the segment, if suitable sites can be found.

- Seismicity--Detailed investigations of the relationship of seismicity to structure in this part of southern California have really only just begun. Density of seismograph stations could be improved in key areas.

- Seismic source studies--Seismic instrumentation adequate for source studies of earthquakes in the region is needed.

- Geodetic studies--The region is reasonably well covered by a regional scale geodetic network, however there is considerable feeling that more dense coverage at least in certain areas would be useful in deciphering the pattern of deformation. In the San Gorgonio pass area, where vertical movements are anticipated, these studies should place appropriate emphasis on leveling.

- Creep--Additional telemetered creepmeters are planned. The potential for using creep as a tool for understanding the secular variations in regional stress seem to be almost as promising here as in central California. Consequently, additional opportunities for expanding these observations need to be pursued.

- Modelling--Additional work modelling geologic slip rate,geodetic, creep, and seismicity data can test hypotheses for relating these observations and suggest new approaches.

CALIFORNIA INSTITUTE OF TECHNOLOGY

SEISMOLOGICAL LABORATORY 252-21

26 November 1985

Dr. Robert L. Wesson
U. S. Geological Survey, MS 922
Reston, Virginia 22092

Dear Rob:

Let me get some of my own personal thoughts re the Bombay Beach segment down in black and white before they become influenced by the letters from all the other working group members. Although Kerry Sieh and John Louie have given me copies of their letters, I have purposely not yet read them.

In the first place, I see no reason on the basis of our recent workshop to modify my earlier opinion that this is a very important segment of the San Andreas to monitor. All in all, I think it is probably the most likely fault in California to generate a magnitude ±7 event in the reasonably near future, and there is certainly a distinct possibility that, by breaking together with the Mojave segment, a magnitude 8 earthquake could result. (On the other hand, I suspect that the next M = ±7 event in California will probably occur somewhere else--as a surprise; and I would still rate Parkfield higher for a M = ±6 event.) I would not be as "astounded" as would Carl Johnson if the epicenter of this southern San Andreas event were not at Bombay Beach, but this is nevertheless a very promising area to monitor with extra care. The asperity at San Gorgonio Pass is much harder to define and instrument, but I see no reason why the rupture could not just as well originate there.

Regarding what we should do, let me suggest the following:

(1) I am not competent to argue with Jim Savage re the geodetic coverage, which he seems to think is currently adequate. But I would like to get a "second opinion" on this issue; clearly the geodetic results and interpretation are crucial to our understanding of the seismic potential of this fault segment, and I can't help but think that there are other things we should be doing by way of geodesy, such as a denser network between the San Jacinto and San Andreas faults.

(2) The paleoseismicity studies clearly must move ahead with vigor. On the other hand, this seems to be happening, at least in the Coachella Valley area, and I'm not sure that a great deal more can be done here than is currently underway and planned. I do think that some suitable trenching sites might be

found in San Gorgonio Pass, and a more careful look should be given to the segment north of San Bernardino. Understanding of the seismic potential for the San Andreas fault southeast of San Gorgonio Pass depends critically on knowing what has happened northwest of the Pass as well, and I hope that the San Bernardino segment is not "falling between the cracks" of the interests of the Mojave and Bombay Beach working groups. I feel particularly strongly about the paleoseismicity work because, as you could sense from my comments at the workshop, earthquake prediction is not our only goal in the hazard-reduction program, and trenching studies seem to be our best bet for quantitative (probabilistic) hazard evaluation.

(3) Seismographic coverage in this region is not bad at present, but I see two ways in which it could be improved:

(a) A few more stations very close to the fault, even if only temporary and portable, would help to resolve the question of whether small earthquakes are occurring on the fault surface itself. And a better understanding of the velocity structure close to the fault would help in other ways as well--such as understanding the rheological behavior with depth.

(b) Although no one at the moment seems to be doing source studies on earthquakes in this area, I can't help but believe that they will become increasingly important in understanding the earthquake process here. In this light, some of the nearby stations should be converted to three-component, wide-band, high-dynamic-range instruments. We have been repeatedly told by Menlo Park that such initiatives are underway for selected stations of the entire USGS-Caltech southern California array, and we should make sure that these stations are selected with the "Bombay Beach problem" in mind.

(4) Creep studies need to be expanded and integrated. This is a bit of a ticklish problem, because various groups (particularly Caltech) have been doing their own things up until now. But it seems clear to me that, not only is a more extensive creep-measuring program essential, but that some sort of master planning and integration of real-time data analysis is called for. This should be a major initiative.

Thanks, incidentally, for all that you and your USGS colleagues (particularly Bill Stewart and Maria Castain) did to make the workshop successful. Although Jim Savage is by no means alone in some of his feelings of frustration, I think that the workshop participants--and particularly those who completed the field trip--came away with a constructive and positive feeling.

Sincerely,

Clarence R. Allen
Professor of Geology
and Geophysics

CALIFORNIA INSTITUTE OF TECHNOLOGY

SEISMOLOGICAL LABORATORY 252-21

25 November 1985

Rob Wesson
U. S. Geological Survey
National Center, Mail Stop 922
Reston, VA 22092

Dear Rob,

I am very grateful to you for the invitation to participate in the southern San Andreas workshop. It was essential, I believe, for the workers in this area to all get together and review our state of knowledge about this enigmatic fault. In addition, it was very good for me to be able to meet all of the participants, since I have only been working in this field for a few years.

Unfortunately, it is apparent that we do not know enough about the southern San Andreas to be able to propose an experiment that would definitively add to our ability to evaluate the fault's earthquake hazard potential or predictability. The fault segment lacks both an historical damaging earthquake and patterns of strong seismic activity, unlike the Mojave and San Jacinto areas. Many interesting experiments can be suggested, but it is likely that only 10% of the money spent on such would later be justifiable in terms of the above goals. For this reason I would suggest that efforts over the next five years continue to emphasize the collection of basic information on the behavior of the fault. I don't believe that we could justify a radical departure from this strategy.

In terms of geological studies, I feel that Sieh and Williams are on the verge of discovering the critical information we need about the slip rate over the last 100, 1000, and 10,000 years. Matti's study of the structural knot at the north end of our segment is also of the utmost importance. Perhaps additional geologists might be persuaded to work in the San Gorgonio area.

I feel that the geodetic studies could be best supplemented by looking at more locations along the strike of the fault-- we should have both long and short range measurements on each of Bilham's 12 km segments. Perhaps Savage's trilateration data could be re-analyzed with the separation of the segments in mind, even at the expense of some precision. At Caltech we will continue to add sites and will soon embark on a major upgrade of our present sites. In the next five years we should be able to give more accurate values for the slip rates on each of the 12 km segments. The definition of any differences in slip or strain rates along fault strike could help to guide your and Stuart's modeling efforts, insofar as such differences should give an indication of changes in physical properties.

Seismological efforts should, I think, be concentrated in those areas having the greatest activity, namely the Brawley seismic zone and San Gorgonio Pass. The remainder of the southern San Andreas just does not have enough seismicity to show us any meaningful trends, even though Hawkson has finally shown us that the few nearby events can be located on the fault. The imminent departure of Johnson from this area is worrisome.

There is a very good chance that seismic reflection profiling will be a reality in this area within the next five years. Fortunately, it is one of the stated objectives of the CALCRUST consortium to cross the Salton Trough within the next 3 or 4 years in the course of assembling a transect from the Colorado River to the Borderland. Eric Frost is currently trying to obtain some of the abundant existing industry data which almost certainly cross the southeastern extension of the San Andreas, the Imperial fault, and the Brawley zone. It may be useful for scientists from our group to make proposals to CALCRUST, especially since it will be this group who CALCRUST will look to in planning the route and methods used crossing the San Andreas. CALCRUST has also discussed a line in the area of Cajon and Banning Passes and has given it high priority for next year. It would, in fact, be most helpful to CALCRUST for the geologists in our group to put together generalized cross-sections showing alternative models of structure at depth where there are questions about the fault geometry, such as between the Banning and Mission Creek faults. With such cross-sections, it is relatively easy to decide whether the differing hypotheses can be tested with seismic reflection.

Again, Rob, allow me to thank you for the opportunity to participate in this interesting and most necessary meeting. I look forward to participating in any future symposia at which my work could be appropriately presented.

Sincerely,

John N. Louie

--

United States Department of the Interior

GEOLOGICAL SURVEY
Geological Division
Branch of Western Regional Geology
345 Middlefield Road MS 975
Menlo Park, California 94025

December 9, 1985

MEMORANDUM

TO: Robert L. Wesson, Clarence R. Allen

FROM: Jonathan C. Matti JCM

THROUGH: R. O. Castle, Chief, Branch of WRG

SUBJECT: Southern San Andreas fault: where do we go next?

Thank you for a stimulating workshop on the state of our understanding of the San Andreas fault in the Coachella Valley. I learned a great deal, and enjoyed interacting with all the folks.

What I learned most was how much we do not know about the late Pleistocene and Holocene history of the fault. Categories of ignorance include: (1) ground-rupture lengths for past and future eauthquakes; (2) late Quarternary slip rates and paleoseismicity for various segments of the fault; (3) its geometric and kinematic relations with other late Quarternary right-lateral and left-lateral faults and with associated anticlinal uplifts and normal-fault complexes; (4) its relationship with the Mojave Desert segment of the San Andreas via the convergent zone in San Gorgonio Pass; and (5) models for where ground ruptures might start and end.

I think these catagories of ignorance prevent us from recommending a specific local segment of the fault for a Parkfield-type prediction experiment (at the present time, anyway). Instead, I recommend that we devote the next 5 years to a well coordinated seismologic-geodetic-geologic program designed to address the questions we raised at the workshop (some of which I summarized above). I propose that the working group remain intact (with the possible addition of a few more players who could not attend due to schedule conflicts), and meet 6 to 9 months down the line to define more clearly the problems and their possible solutions. I suspect that a reasonable and practical approach to earthquake forecasting and hazard mitigation will naturally fall out of a five-year buildup period.

As I understand it the USGS is mandated to achieve a specific goal: an operational prediction network. I believe a guided program is necessary to achieve this goal--a coordinated team effort where investigations dovetail in terms of their impetus, direction, and short-term purpose. For example, the give-and-take of the "Indio Committee" showed me ways to focus my field-oriented geologic investigations in a way that can more effectively address questions that bear on earthquake prediction. Without that give-and-take atmosphere, I might focus my work in directions that might not serve the mandated goal as effectively. I needed the consultation with others on the team. I am not suggesting science by central-committee vote, but I suspect that the Survey will take the heat if we invest dollars in an unmanaged portfolio.

If I had to make recommendations for what to do over the next 5 years, I would establish 3 priorities (in no particular order): (1) Continue paleoseismicity studies like those of Kerry Sieh; (2) define more clearly the late Quarternary history of the San Andreas and Banning faults in the northern Coachella Valley; (3) have the geodesy people agree among themselves (no exaggerations for clarity, please!) as to the best near-field strain-monitoring strategy to augment the far-field work of Savage and his group.

I will close by thanking you again, by wishing you good luck, and by asking for megabucks to fund the best game in town--GEOLOGIC FRAMEWORK STUDIES IN SUPPORT OF NEOTECTONICS!!

Cheers,

Jonathan

Jonathan C. Matti

December 2, 1985

Dr. Rob Wesson
U.S. Geological Survey
MS 922, National Center
Reston, VA 22092
Re: USGS Workshop on the Southern San Andreas fault

Dear Rob,

I would first like to commend the convenors of the workshop for a well-run, informative session. The field trip was particularly helpful as I had not previously been to any of the sites visited, and now, because of the trip, that section of the fault is a little less mysterious. Jim Savage's comments, although refreshingly candid, were not very relevant to what I thought was the main point of the meeting, except in one regard: more time did need to be devoted to in-depth discussion without the restriction of formal presentations. My only present misgiving of the proposed special session at AGU '86 is the lack of provision for such informal meetings of the working group.

As for where we stand on the southern San Andreas fault itself, I think certain facts are no longer in dispute. The southern San Andreas is currently accommodating strain at the rate of 2-2.5 cm/yr and therefore should be considered the primary locus of relative plate motion in southern California. There is no evidence to suppose that much of this strain accumulation is non-elastic, as creep (either continuous or episodic) can only be documented at a long term rate of ≈ 2 mm/yr. The fault has experienced large (at least 3 m) slip events in the Holocene, but no major earthquake has occurred along the fault in historical time. Two out of the 5 best documented cases for slip occurred during high stands of Lake Cahillia. Few microearthquakes are currently located along the fault, and although proper modeling of lateral inhomogeneities in velocity can move some earthquake hypocenters closer to the fault, few of these events exhibit right-lateral motion along nodal planes that parallel the local strike of the main surface fault trace. Certain sections of the fault are susceptible to triggered slip induced by large regional earthquakes. These sections, as well as the locations of local transpressive features, appear to be controlled by fault geometry that may also influence the seismic behavior of the main fault during a large earthquake rupture.

Areas that need further investigation include: Can rupture propagate through San Gorgonio Pass and is there any evidence that events identified at Cajon Pass or Indio are seen in the sediments of San Bernardino Valley? To what extent is relative plate motion simply accommodated by vertical deformation in San Gorgonio Pass and not strike-slip? What is the partition between elastic and non-elastic strain accumulation during the inter-seismic period? If most of the current earthquake activity is off the fault, how does slip on these secondary faults affect the distribution of normal and shear stress along the main fault trace? Do transpressive features such as the Durmid anticline form as the result of repeated deformation in large earthquakes (*i.e.*, Coalinga) or during the interseismic period? How wide is the present zone of strain accumulation? Is there a characteristic size for slip events on the southern San Andreas fault and is it 3, 5, or 8 meters? Is there any correlation between events at both the northern end (near Indio) and the southern end (near Salt Creek)? Is the presently observed rate of creep a persistent feature of the fault at time scales longer than 40 years? What is the variation in depth of the seismogenic zone along the fault, and do any of the events that can be associated with slip on the southern San Andreas have any special spatial coherence? Since the southern San Andreas fault is the only major fault with a high probability for experiencing a large earthquake in the next few decades that currently exhibits creep, what is the spatial and temporal behavior of the creep?

Specific recommendations for further research, besides those areas already under investigation include: the determination of the uplift rate in San Gorgonio Pass; determination of fault geometry (*e.g.*, angle of dip) and the installment of geodetic lines in the area where the fault changes strike near the head of Coachella Valley; and the assessment of the spatial distribution of shear and normal stress along strike, as shown by the pattern of microearthquake activity. I think it also should be emphasized that because of the large amount of 'off-fault' activity and because of the large uncertainty in how this fault may behave seismically, a concentrated seismograph network would not be appropriate (*e.g.*, Anza), but that a major effort should be given to expanding and improving the regional networks. This improvement should be of the form of 3-component broad-band stations, some of which may need to located in boreholes with Coachella Valley to provide sufficient signal-to-noise and lateral resolution.

Although a number of people, especially Carl, are in favor of rupture (if it occurs) starting at Bombay Beach, I think some considerable attention should be paid to the northern end of Coachella Valley. Like the transition from the Brawley seismic zone to the southern San Andreas, the active trace undergoes a major change in strike. The stress regime changes suddenly from nearly pure strike-slip south of the Morongo Valley fault to thrust faulting in San Gorgonio Pass. The area has generated a large ($M_L = 6.5$) earthquake in the past, and could be again near failure. A repeat of the Desert Hot Springs earthquake could easily evolve into a much larger slip event, given our knowledge on the present state strain accumulation on the southern San Andreas. There is some evidence to suppose that a large event in this area would not occur without some prior warning, whereas this may not be the case at Bombay Beach. The region is close to San Gorgonio Pass where the seismicity suddenly deepens, normal stress is likely to be high and strengths of rock large, providing sufficient strain energy to drive rupture in a large earthquake. This is one of the few areas of the fault that has a large number of thermal wells and hot springs associated with it; and no ready explanation as to why there should be high heat flow in this corner of the fault. These wells and springs could be monitored for temporal variations that might empirically signal an approaching slip event. And if nothing else, rupture will either have to stop here (if it doesn't propagate through San Gorgonio Pass), or if rupture mirrors the 1857 event, slip will be largest along this segment.

Sincerely,

Craig Nicholson

Craig Nicholson

P.S. Welcome to my committee.

United States Department of the Interior

GEOLOGICAL SURVEY
OFFICE OF EARTHQUAKES, VOLCANOES, AND ENGINEERING
Branch of Tectonophysics
345 Middlefield Road, MS/977
Menlo Park, California 94025

December 16, 1985

Memorandum

To: Rob Wesson

From: Jim Savage

Subject: Southern San Andreas Workshop

1) Assessment of Earthquake Potential on the southern San Andreas.

The evidence that the southern San Andreas fault is subject to periodic great earthquakes seemed to me to be quite convincing. Although the only direct evidence for great earthquakes there in the past depends upon the observations of Sieh, all of the other evidence (strain accumulation, seismicity, and creep) seemed consistent with a substantial earthquake hazard along the southern San Andreas fault. Clearly, Bombay Beach lies at the southern end of the potential rupture zone. The location of the northern end of the rupture is quite uncertain, but it might reasonably be placed in the vicinity of Cajon Pass.

The time at which the next great earthquake might be expected is defined only by the work of Sieh. Sieh concludes that we are already in the interval in which an earthquake might be expected. The usual theory of the seismic cycle predicts an increase in the frequency of moderate shocks in the decades before a great earthquake. I am not aware of such an increase along the southern San Andreas. In fact, the southern San Andreas is notably aseismic.

2) Investigations which could lead to a testable hypothesis regarding the time of the next large earthquake along the southern San Andreas.

This topic was not discussed at the workshop. Sieh's work on recurrence times is clearly pertinent, but it does not appear that the recurrence times are sufficiently regular that one can make a useful earthquake prediction. There appeared to be some interest in monitoring Bombay Beach closely as it appeared to be a logical nucleation point for a great earthquake on the southern San Andreas.

Conclusions

1) The southern San Andreas fault is certainly a possible site for a great earthquake in the next 40 years.

2) We do not know enough about earthquake prediction at present to justify installing a Parkfield-type monitor site at one point (presumably Bombay Beach) along the southern San Andreas.

3) The seismicity studies of the southern San Andreas fault seem to be the one program that could be most profitably intensified. Such studies could cover both the San Jacinto and San Andreas faults. The objective should be to approach a real-time analysis of the seismicity. Carl Johnson has identified several interesting anomalies in seismicity and, in a general sense, predicted the 1979 Imperial Valley earthquake. Thus, seismicity studies seem to have some predictive potential. Moreover, seismicity studies are within the capabilities of the USGS group.

CALIFORNIA INSTITUTE OF TECHNOLOGY

DIVISION OF GEOLOGICAL AND PLANETARY SCIENCES 170-25

November 26, 1985

Dr. Robert Wesson
Office of Earthquakes, Volcanos and Engineering
U.S. Geological Survey
Reston, VA 22090

Dear Rob:

This letter is in response to your and Clarence's request for a one or two-page summary of my impressions of the Indio segment working group meeting last week.

I found the day of presentations very useful in providing an overview of current research, published and unpublished, along this segment of the San Andreas fault. I support the suggestion that we organize a symposium at the December 1986 AGU meeting in anticipation of significant new results from research along this potentially active fault segment. I would also be willing to contribute to a JGR special symposium volume at about that time.

On the basis of the geologic, geodetic, and seismologic data, I think we are on the right track in selecting this and the Mojave segment of the San Andreas fault as the most likely candidates to produce the next great earthquake in California. Because of this, I think that a focussing of research along these segments is warranted. However, I caution that the government should not proscribe the geographic locality of research to such a degree that we become blindered by confidence in our models. What I'm saying is that a healthy government-sponsored research program must include a substantial amount of research not along the Mojave or Indio segments of the San Andreas fault. That said, let me now detail my thoughts about research that would be appropriate along the Mojave and Indio segments.

Although results from the Indio site are at this point still preliminary, I think it is fair to surmise that that site will, when completed, show that large earthquakes occur along the Indio segment about every 150 or so years. Geologists such as myself are now considering the possible segmentation of the southern 300 km of the San Andreas fault. Can we expect the Mojave segment to break in concert with the San Gorgonio Knot and the Indio segment? Or should we expect the Indio segment to generate a 7.5 by itself? Perhaps the most promising avenue of research for resolving this question will be that proposed by John Matti. If the San Gorgonio Knot can be shown to break less frequently than the Mojave and Indio segments to either side of it, we may well be able to conclude that the Mojave and Indio segments sometimes break independently in more than one event, but together with the San Gorgonio Knot during other events.

The work proposed by Gordon Jacoby recently may help resolve questions we have about the sequencing of Indio and Mojave segments. Gordon is probably going to be able to resolve the dates of the last two or three great earthquakes on the Mojave segment. The Survey should encourage research by individuals who have a good chance of precisely dating the last events along the Indio segment as well. Perhaps dendrochronologists will

PASADENA, CALIFORNIA 91125 TELEPHONE (818) 356-6811

be able to date events on the San Gorgonio Knot using trees in the San Bernardino Mountains. Unfortunately, I don't believe other dating techniques are going to allow us to resolve the detailed time history of large events along the fault completely.

I'm intrigued by the modeling work done by individuals such as Bill Stuart, yourself, Paul Segall, and others regarding the mechanics of faulting, both seismic and aseismic. The continued and expanded collection of geodetic and geologic data will be critical in allowing us to resolve from observational data which models are most appropriate for fault behavior. I think this kind of work needs to be encouraged.

One last suggestion, which was not made during the meeting: I think the Survey needs to do more to encourage long-term growth in the area of earthquake hazard assessment, forecasting, and prediction. One specific suggestion I have is that the Survey expand its postgraduate fellowship program.

Respectfully yours,

Kerry

Kerry Sieh

kes/ph

xc: Clarence Allen

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INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS, A-025
SCRIPPS INSTITUTION OF OCEANOGRAPHY

LA JOLLA, CALIFORNIA 92093

January 14, 1986

Robert L. Wesson
U.S. Geological Survey
Office of Earthquakes, Volcanoes
& Engineering, MS 905
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Rob:

I have been attempting to write my summary and suggestions from the meeting on the southern section of the San Andreas Fault. I found it quite informative to see the primary directions which the research has taken in that region. I was not familiar with all the geodetic and geologic studies which are in progress and it was beneficial to learn about them. It is apparent that if we want to increase our knowledge of this section of the San Andreas Fault more research is necessary to augment the current geological, geodetic and geophysical studies.

Probably the most imperative studies which should be approached are those which determine the seismic potential of the southern section of the San Andreas Fault. To gain a better understanding of this will require more information about the recent slip rates and slip history, the strain accumulation along the fault, and the expected length of rupture during the next large event. As was discussed in detail at the meeting, the study of these topics will need more paleoseismic, geologic, and geodetic data.

I found the above discussions to be fascinating but a little out of my primary area of expertise. I was surprised that there was not more discussion or interest in the seismological possibilities which exist for this section of the San Andreas. We find in this area that there is a minimal amount of strong and weak motion instrumentation, especially when we consider the likelihood of a large earthquake occurring here. There are many levels of experimentation which could and should be considered for this area.

The barest minimum for seismic monitoring is to maintain the current level of effort. This would mean maintaining the current Caltech/USGS array as well as the existing strong motion array. Carl Johnson mentioned that their network will lose some stations in the Westmoreland area when Union Oil Company moves the equipment it operates. It is important to replace these stations with permanent equipment to continue monitoring the area seismicity.

The next level of support should be to augment the coverage of the Caltech/-USGS array, and the existing strong motion array. This is easily justified considering the sparse coverage of the current arrays.

Another type of seismic monitoring which would be useful is to have portable sensors and digital recorders available for short term detailed studies. These studies could include the determination of source parameters or detailed monitoring of local swarms and large increases in seismicity. From a practical point of view these temporary studies should not last for more than six to eight months. Any studies which are appreciably longer than this should probably be handled with the installation of more permanent equipment. However any permanent digital array needs an appreciable number of stations to be worthwhile and has a high cost associated with it. The objectives, potential benefits and logistics of such an installation should be considered carefully.

Probably the most interesting seismic experiment which can be implemented is not a premonitory study. This part of the San Andreas offers a unique opportunity to study the dynamic properties of earthquake rupture. Carl Johnson mentioned that he thinks there is a good possibility that the Salton Sea termination of the San Andreas will be the nucleation point of a large earthquake. This location allows us to set up an experiment to possibly observe the initiation of faulting. This experiment would require a high dynamic range recording system, with the permanent three component stations located with a station spacing of the order of one or two kilometers. The sensors would need to include accelerometers to stay on scale during a larger event. An experimental set-up like this would be very useful in understanding rupture mechanics. If the rupture initiated somewhere else the data collected would still be very valuable in understanding the dynamics of faulting as long as the rupture propagated through the array. Unfortunately experiments like this are not cheap and are not necessarily easy to install. But the scientific benefits of the success of such an experiment could be invaluable towards understanding the physics of faulting.

The future research along the southern section of the San Andreas Fault will obviously depend on the amount of available funding. There are several important areas of study which should be considered including the evaluation of the seismic potential of the fault, looking for premonitory phenomena, and studying faulting, fault dynamics and ground motion. Each of these areas have their own merits but I would tend to concentrate first on research which will contribute towards the understanding of the structure of the fault and the extent of faulting we might expect during a major event. There are many interesting research possibilities and hopefully we, as a scientific community, can make the most of the opportunities which present themselves along this section of the San Andreas Fault.

Sincerely,

Frank Vernon

Frank Vernon

FV:kb

United States Department of the Interior

GEOLOGICAL SURVEY
RESTON, VA. 22092

OFFICE OF THE DIRECTOR

In Reply Refer To:
WGS-247203
Mail Stop 106

JAN 16 1986

Dr. Lynn R. Sykes
Chairman, National Earthquake
Prediction Evaluation Council
Lamont-Doherty Geological Observatory
of Columbia University
Palisades, New York 10964

Dear Lynn:

We have read and considered the National Earthquake Prediction Evaluation Council's recommendations of November 28, 1985. In general, we see merit in those recommendations and would like to respond to you with our suggested actions.

We agree that in special circumstances our scientists have to react quickly to imminent hazards and, without specific approval, notify appropriate public officials of their scientific findings and conclusions. Actually, our scientists have had this liberty since we formally adopted a geologic hazards warning procedure in 1977. Still, we agree with the Council that public safety would be enhanced, as would our ability to issue effective earthquake predictions, if we had a specific plan of action for the several Parkfield scenarios. Therefore, we will schedule a meeting with our Office of Earthquakes, Volcanoes, and Engineering to discuss the draft decision matrix and the delegation of authority to issue earthquake predictions for Parkfield.

The remaining three Council recommendations all concern the communication of concern and long-term probabilities for earthquakes in either California or Alaska. Although our latest procedures for issuing hazard warnings no longer include formal transmittal concerning situations not posing a significant and imminent threat to public safety, they do allow for the routine communication of information which may be of a general or long-term interest to a restricted number of agencies. These Council concerns seem to fit this last category. Thus, we propose to forward not a formal hazard warning but a summary statement of the Council's deliberations about the areas noted in your letter along with copies of the U.S. Geological Survey

Dr. Lynn R. Sykes

Open-File Report of the Council's September 1985 meeting to the Director, California Office of Emergency Services, the State Geologists for California and Alaska, and selected Federal officials.

Thank you for your extensive efforts concerning this most important issue.

Sincerely yours,

Dallas L. Peck
Director

David D. Jackson
Geophysical Institute
Faculty of Science
University of Tokyo
Tokyo 113 Japan
1986 Feb 17

Mr. James F. Davis, State Geologist
California Division of Mines and Geology
1416 Ninth St, Room 1341
Sacramento, CA 95814

Dear Jim,

Thanks for sending the draft of the USGS Parkfield prediction scenarios and response plan. The plan seems basically sound to me, and I agree entirely that criterea for response and warning must be worked out in advance. However, the plan has some deficiencies that should be corrected before it is implemented. Most important, the plan attempts to be too detailed . It reads like an algorithm, with specific thresholds of signal on each instrument automatically activating a certain level of alert, which in turn automatically triggers a certain level of response. The plan should allow more flexibility; if we get too boxed in by our ideas about characteristic earthquakes and periodic behavior we could look like fools. The role of human judgment should be made explicit. For example, the plan apparently calls for the Chief Scientist to make the final judgment on the overall alarm level, but it is not clear who decides the level of alarm for the four networks (seismic, creep, geodetic, and strain). Furthermore, the plan includes no provision for incorporating other forms of data (for example surface cracking, geysers, broken water pipes), nor for disregarding some network data when there is an obvious nontectonic explanation for anomalies (such as extreme weather, electrical storms, human activity, etc.) Clearly such considerations would be weighed before implementing any significant response, so they should be included in the plan, and the official responsible for such judgments should be specified.

In addition to being too rigid, I feel the plan is a bit too complicated. Why should the rules for combining network alarm states be nonlinear? Alarm level "d" seems to be a matter of internal concern only, generally implying only one unverified anomaly. It is wise to have a policy to deal with such things, but perhaps that should be separate from this document which could more productively focus on interagency considerations.

The summary shows that the overall alarm level is a function of alarm levels for four networks, but in the text and appendices, the geophysical data and responsible officials seem to be categorized differently. For example, on pages 25-29 there are separate alarm criteria for strainmeter (borehole dilatometer), water well, and continuous magnetic data. There is no specification for how these separate alarm levels should be combined to form the "strain network" alarm level, or who should make the judgment. For each of the subnetworks, the alarm specifications apparently preclude alarm levels "b" or "a". Does this mean that the strain network as a whole would not reach those alarm states, or that other forms of data should be included in deciding on the alarm state of the strain network? In the discussion of the geodetic network there is a similar ambiguity. How will two-color, geodolite, and small aperture data be combined, who will make the judgment on alarm level, and are alarm levels "b" and "a" forbidden? On page 37, the detailed flow chart, there are references to a "Project Chief" with no qualifiers, and Project Chiefs for Seismic, Water Well, Low Freq. Mon., Creep, 2-color EDM, Borehole Dilat, CUSP, RTP, Magnetometers, Tilt, and Strain. Some of these terms are not defined in the report, and it is not clear how these project chiefs relate to the four major networks.

Some of the probabilistic arguments in the draft report are misleading. What we would all like to have, and what the draft implicitely presents, are conditional probablities of a characteristic event, given an observed geophysical anomaly. To estimate these conditional probabilities we need observations of the occurrence of such anomalies both jointly with and independent of characteristic earthquakes. But we have clearly inadequate data linking earthquakes with foreshocks and creep events, and we have no data whatsoever on the joint occurence of earthquakes and strain or geodetic anomalies in California. All we can really say is that the occurrence of an anomaly increases the conditional probability over the unconditional probability and heightens our fear.

The reported conditional probabilities of an event, given a specified alarm level, are inconsistent. Because the alarm levels are distinct, we have that

$$p(C) = p(C|a)\ p(a) + p(C|b)\ p(b) + p(C|c)\ p(c) + p(C|d)\ p(d) + p(C|e)\ p(e)$$

where p(C) is the unconditional probability of a characteristic event in the next 24 hours, p(C|x) is the conditional probablility of a characteristic event given alarm state x, and p(x) is the unconditional probability of having alarm state x. Using p(x) = 3 days/t(x), where t(x) is the maximum recurrence time for alarm state x, the data given on pages 4 and 5 of the draft imply the following:

Alarm level, x	t(x), days	p(x)	p(C|x)	p(C|x) p(x)
e		.971	.0001	.000097
d	180	.017	.0010	.000017
c	365	.008	.0100	.000082
b	730	.004	.1000	.000411
a	?	?	.4000	?
all		1.000		.000607

Thus the minimum unconditional event probability is .000607 per day, assuming that alarm level a is so rare that it can be neglected. But the maximum unconditional probability justified by seismicity is

.000150, assuming earthquakes in 1966, 1922, 1901, 1885, 1881, and 1857. Thus the conditional probability for each alarm level has been overestimated.

The discussion of conditional probability given a possible foreshock, on pages 20-22, doesn't make sense because assumption #1 is completely arbitrary.

More effort could go into the contingency plans in appendix A and B. Regarding Appendix A, item 3, not only should baselines southeeat of the Parkfield area be remeasured, but northwest as well, and provision should be made to check results of 2-color observations as well. Provision should also be made for detailed aerial photography that could be used to document coseismic changes in the event of an earthquake. The probability estimate in Appendix B should be considerably lower, about 10% instead of "1 in 2." I think it is useful to be as quantitative as possible, but I think 50% is too high for the criterea given.

If I understand the plan correctly, an alarm of level "a" or "b" would trigger notification of CDMG, and CEPEC would almost certainly be called into official action. I think we should be notified unofficially if the alarm level reaches c, so that we can be on alert and become familiar with the data.

I hope these comments are useful. Please note that I am in Japan until 15 Jun 86, and it would speed things up if you wold send any relevant materials to me directly.

Yours truly,

David D. Jackson

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Prof. L. R. Sykes
Chairman of NEPEC
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U S A

28 Feb. 1985

Dear Lynn,

with separate mail I sent you a draft for an article entitled "Regular Intervals Between Five Hawaiian Mainshocks Suggest That The Next $5.5 \leq M_s \leq 6.6$ Kaoiki Earthquake Will Occur in 1994±1.5 Years". I feel that you should be informed of this as chairman of NEPEC. However, I believe that this may not be very sensitive, and I would like to send it to Science or Nature in a few weeks, unless you advise against it.

I have also sent copies to the following people who are knowledgable about the Kaoiki area: R. Koyanagi (HVO), F. Klein, and E. Endo. I would be grateful if you or somebody from your lab would send critical comments.

May I propose that I send it to a journal in three weeks (21 March) unless I hear from you. If you need more time please let me know by telex.

With best wishes

Max

Max Wyss.

Regular Intervals Between Five Hawaiian Mainshocks Suggest That Earthquakes ($5.5 \leq M_s \leq 6.6$) In The Kaoiki Area May Perhaps Be Predicted.

by

Max Wyss

CIRES

University of Colorado

Boulder, Colorado, 80309

April 1986

Large earthquakes along a given fault segment do not occur at random times, because it takes time to accumulate the strain energy for the rupture in the source volume (1). The rates at which tectonic plates move and accumulate strain at their boundaries are approximately uniform. Therefore, in first approximation, one may expect that large ruptures of the same fault segment will occur at approximately constant time intervals. If subsequent mainshocks (2) have different amounts of slip across the fault, then the recurrence time (3) may vary, and the basic idea of periodic mainshocks must be modified to include variation in recurrence time (4). For great plate boundary ruptures the length and slip often vary by a factor of two. Along the southern segment of the San Andreas fault the recurrence interval is 145 years with variations of several decades (22). The smaller the standard deviation of the average recurrence interval , the more specific could be the long term prediction (5) of a future mainshock.

In the Kaoiki, Hawaii, area it appears that mainshocks happen at unusually constant intervals (Figure 1). In a volume of the earth's crust which has a radius of approximately 6 km (Figure 2), mainshocks have occurred in 1941, 1951, 1962, 1974, and 1983 (Table 1). The recurrence interval Tr(K)=10.5±1.5 years (Tr(K)= time of recurrence at Kaoiki). Adding Tr(K) to the date of the last mainshock, one would expect the next one to occur in May, 1994±1.5 years. Although the epicenters of 1941 and 1951 are not very accurately known, one may estimate the approximate location of the next mainshock as the average of the locations given in Table 1: latitude 19°23.4'+ 2.7', longitude 155°26.4'+ .9'. The magnitude is expected to lie within the bounds of the past events: 5.5 ≤ Ms ≤ 6.6 (20).

This observation is similar to the Parkfield, California case where Tr(P)=21.9±3.1 years (6,7). The Kaoiki recurrence time and standard deviation are about half of the Parkfield ones. The magnitudes of the mainshocks in the two areas are similar: the last events measuring Ms=6.6 (Kaoiki) and Ms=6.4 (Parkfield). A dissimilarity may be found in the constancy of the mainshock size. While the Parkfield earthquakes are nearly carbon-copy events (6) the magnitude and style of faulting varies at Kaoiki (8, Table 1).

The Kaoiki area is not located at the boundary of a tectonic plate. Instead the strain is accumulated by crustal expansion which takes place in the volcanoes Mauna Loa and Kilauea (9,8). Magma rises in narrow conduits under the volcano summits, and then often intrudes along shallow paths into the volcanic rift zones, thus compressing the adjacent crust (9). Because the Kaoiki volume is located between the summits of Mauna Loa and Kilauea, the compressive stresses from the two volcanoes combine to create a stress tensor with the greatest principal stress in the direction connecting the two summits (Figure 2). The resulting mainshock's focal mechanisms are therefore right-lateral strike-slip on a near vertical plane (10,8) like the San Andreas style faulting at Parkfield (11). However, the Kaoiki faulting is more complicated. In addition to the strike-slip motion a detachment along the near horizontal oceanic sediment layer (12) occurs in some, but not all, of the mainshocks, such that a mixture of strike-slip and thrusting mechanisms is found among the aftershocks (8). Thrusting in the sediment layer occurred in the 1983 mainshock, while there was no evidence for this process in the 1974 event. This could be the cause of the difference in magnitude between mainshocks in the Kaoiki sequence (Table 1).

A refinement of the prediction of occurrence time might be possible if precursory anomalies can be observed. Before the 1983 Kaoiki mainshock a most pronounced period of seismic quiescence (13) existed. During 2.4 years the seismicity-rate was reduced by 60% to 90% in the source volume, except for the immediate vicinity of the main-rupture initiation point, where the rate remained constant (Figure 2, (14)). This same pattern was previously observed for the 1975 Kalapana, Hawaii, (Ms=7.2) earthquake (15). However, quiescence could not be found for the 1974 Kaoiki earthquake. We will assume that quiescence before the next Kaoiki mainshock may be expected with a probability of 50% to 67%, because one out of two Kaoiki mainshocks, or alternatively two out of three Hawaiian mainshocks were preceded by quiescence.

Based on the 1983 precursor time Tq=2.4 years, one would expect the next quiescence to start at the beginning of 1992. If quiescence appears again, then we can refine the estimate of the predicted time, provided that the variance of Tq is less than that of Tr. From the little we know about Tq and its variance (16) we assume that for a mainshock in the same location and of similar magnitude the standard deviation of Tq is less than 0.5 years. Thus it is proposed that, if quiescence appears in 1992 (or thereabouts), the mainshock should occur 2.4±0.5 years later. So the time estimate of the event may be refined to about half a year uncertainty by the medium range precursor of seismic quiescence.

The model for the Kaoiki mainshock sequence is not yet as well developed as that for the Parkfield case (6,7). The following working hypothesis to explain the Kaoiki sequence is as yet supported by few facts only and may therefore need to be revised substantially as more data will be analysed. Magma rises at a steady rate under the volcanoes and causes a strain accumulation which is constant if averaged over several years (23). The 10 km of the crust above the oceanic sediment layer is the main obstacle to failure, while the thrust-plane within the sediments is relatively easy to rupture (12,17). The crustal volume caught between the two volcanoes' stress systems is constant in size, therefore, the extent of the source volume is approximately constant, hence the time between Kaoiki mainshocks does not vary much. When failure occurs, it starts as a strike-slip rupture (10,8) because the top 10km of the crust controls the rupture, and then the slip on the thrust-plane (which allows the southeast flank of Mauna Loa to move away from the volcano (8)) may take place. As a speculation, one could add the idea that every second mainshock includes the thrusting part (resulting in larger magnitudes and intensities, see Table 1) because the more easily moving thrust-plane slips a comparatively large amount each time it ruptures. If this suggestion is true, then the next Kaoiki earthquake will more likely be in the magnitude range of $5.5 \leq Ms \leq 6.0$, and if the smaller types of mainshocks do not show quiescence (another possible speculation) then we may not be able to observe a quiescence precursor before the 1994 mainshock.

Tests can be done for parts of this hypothesis. For the years 1834 to 1939 there were 39 Hawaiian earthquakes reported as felt(18). Many of these were clearly not located near Kaoiki, but others are attributed to the nearby Mauna Loa or Kilauea areas. It may be possible to ascertain by searches in old documents and by interviews with senior residents, which of these may have been Kaoiki mainshocks. Based on the above hypothesis one would expect that Kaoiki mainshocks took place in early 1931±1.5 and mid 1920±1.5, if the strain accumulation rate was the same in the early part of the century as during the last few decades. Another test of the hypothesis will be provided by events between 1992 and 1996. But it is hoped that before that time much work can be done to refine the present crude hypothesis.

The Kaoiki earthquake sequence, and the model to explain it, have some avantages which recommend it for earthquake prediction research: (1) The area is sparsely populated. Therefore, announcements about future earthquakes may not be as sensitive an issue as in more populated areas, and the permission to place measuring devices into the area may be more readily available. (2) A test of the hypothesis occurs every 10.5 years, apparently twice as frequently as at Parkfield, which means that progress in learning how to predict earthquakes can be made relatively rapidly. (3) The type of faulting of a brittle crust under local stress concentrations may be similar in some aspects to intra-plate ruptures as they occur in Alaska and the mid-to eastern United States. Thus, a model derived from the Kaoiki events may be more pertinent to these areas than a Parkfield (San Andreas fault) prediction model.

The results and speculations presented above give hope that long and medium term prediction of some $Ms=6\pm0.5$ mainshocks may be possible with accuracies of 1 ± 0.5 years. On the one hand, one would hope that this capability might be improved by the detection of short term precursors. For example, if foreshocks occur and can be identified correctly, the time of the mainshock might be estimated to within a week (21), but the problem of how to recognize foreshocks without many false alarms is not yet solved. On the other hand randomly occuring triggering events might upset the schedule and cause possibly random deviations from the expected occurrence time (7). Also, the assumption of a constant strain accumulation rate is central to the hypothesis proposed above. One would expect that the regularity of the mainshocks would be broken if the supply rate of magma or the storage rate in the rifts change. Given the complexity of the volcanic system it is surprising that mainshocks occur at regular intervals as they do. As long as we understand little about the processes leading to earthquakes, it is better to treat predictions as tests of scientific hypotheses, rather than as information useful for warning the public. While there is no doubt that we are slowly making progress in learning how to predict earthquakes, there is also no doubt that there will always be failures.

References and Notes

1. H.G. Reid, Carnegie Institute (1910).
2. The expression "mainshock" is often restricted to the largest event in a sequence. We will extend its use here to mean a relatively large earthquake with rupture length comparable to the fault segment or crustal volume considered. Such an earthquake will cause a major release of strain energy in the source volume. Energy will have to be built up anew by tectonic processes before another mainshock can occur.
3. The "recurrence time" is the time which elapses between repeated mainshock ruptures of the same fault segment or source volume. The recurrence time would be exactly constant if the source volume, the strain release in each mainshock, the strain accumulation rate by tectonic forces and the failure strengths would all be constant, but these parameters often vary by large amounts.
4. K. Shimazaki and T. Nakata, Geophys. Res. Lett. 7, 279 (1980).
5. In a long term prediction the ocurrence time is specified many years ahead of the event with uncertainties measured in years. Medium term predictions are likely to be based on the observation of specific anomalies interpreted as precursors, and the event is predicted with one to few years of lead time, and an uncertainty of less than a year. In short term predictions, the event should occur within weeks and the uncertainty is measured in days. R.E. Wallace, J.F. Davis and K.C. McNally, Bull. Seismol. Soc. Am., 74 (1984).
6. W.H. Bakun and T.V. McEvilly, J. Geophys. Res. 89, 3051 (1984).
7. W.H. Bakun and A.G. Lindh, Science, 229, 619 (1985).
8. E.T. Endo, Ph.D. thesis, University of Washington (1985).
9. D.A. Swanson, W.A. Duffield, and R.S. Fiske, U.S. Geol. Surv. Prof. Paper, 963 (1976); J. Dvorak A. Okamura and J.H. Dietrich, J. Geophys. Res., 88, 9295 (1983).
10. R.Y. Koyanagi, H.L. Krivoy and A.T. Okamura, Bull. Seismol. Soc. Am., 56, 1317 (1966); R.Y.Koyanagi, E.T. Endo, W.R. Tanigawa, J.S. Nakata, A.H. Tomori, and P.N. Tamura, U.S. Geol. Surv. Open-File Rpt. 84 (1984).
11. J.P.Eaton, M.E. O'Neal, and J.N. Murdock, Bull. Seismol. Soc. Am., 60, 1151 (1970).
12. The volcanic edifice of the Hawaiian Islands is deposited on top of the oceanic sea floor which is created at the east Pacific rise and travels by plate motion towards Japan. On its way to Hawaii, about half a kilometer of sediments were accumulated on the sea floor. Due to the weight of Hawaii,

the sea floor is depressed under the islands, and thus the oceanic sediment layer is at a depth of 9 to 10 km from the Earth`s surface at Kaoiki. Geodetic and seismologic evidence shows that near horizontal slip occurs at the depth of these sediments allowing the southeast flanks of the volcanoes, Kilauea and Mauna Loa, to move away from the volcanoes and the rifts toward the southeast (9,8,17).

13. "Seismic quiescence" is defined as a decrease of the rate of earthquake occurrence within the volume in question. The detection of quiescence presumes that a nearly constant background rate can be defined in the same volume. In two mainshocks in Hawaii, where detailed data were available, only parts of the source volume showed quiescence, while major asperities produced micro-earthquakes at constant rates up to the mainshock (14,15).

14. M. Wyss, Bull. Seismol. Soc. Am., in press (1986).

15. M. Wyss, A.C. Johnston and F.W. Klein, Nature, 289, 231 (1981a); M. Wyss, F.W. Klein and A.C. Johnston, J. Geophys. Res., 86, 3881 (1981b).

16. M. Wyss, Earthquake Pred. Res., 3, 519 (1985).

17. A.S. Furumoto and R.L. Kovach, Phys. Earth Planet. Interiors, 18, 197 (1979); M. Ando, J. Geophys. Res., 84, 7616 (1979).

18. J.L. Coffman and C.A. von Hake, U.S. Dept. of Comm. Publ. 41.1, 208pp. (1973); U.S. Geol. Surv. Hawaiian Volcano Observatory, Volcano Letters, Sept. (1941) and Sept. (1951).

19. J.N. Brune, J. Geophys. Res., 84, 2195 (1979). A specific triggering mechanism in Hawaii might be the intrusion of a large amount of magma into one of the volcanic rifts. Through the resulting stress pulse the expected event might occur early.

20. A least squares regression of the data as a function of event number finds a correlation coefficient of 0.999 and estimates the arrival of the next event in March, 1995. The prediction proposed here is as follows: An earthquake of magnitude 5.5=Ms=6.6 will occur at latitude 19 23.4`+3` and longitude 155 26.4`+3` in late-1994+1.5 years.

21. Anonymous, Sciencia Geol. Sinica, 2, 120 (1976).

22. K.E. Sieh, J. Geophys. Res., 89, 7641 (1984).

23. D. A. Swanson, Science, 175, 169 (1972); D. Dzurisin, R. Y. Koyanagi and T.T. English, J. Volcanol. Geotherm. Res., 21, 177 (1984).

24. This work was supported by NSF grant EAR-8417014, the Alexander von Humboldt Foundation and the Seismologisches Zentralobservatorium Gräfenberg, Germany. I thank T. L. Wright, R. Y. Koyanagi, P. Basham and R. Kind for comments on the manuscript.

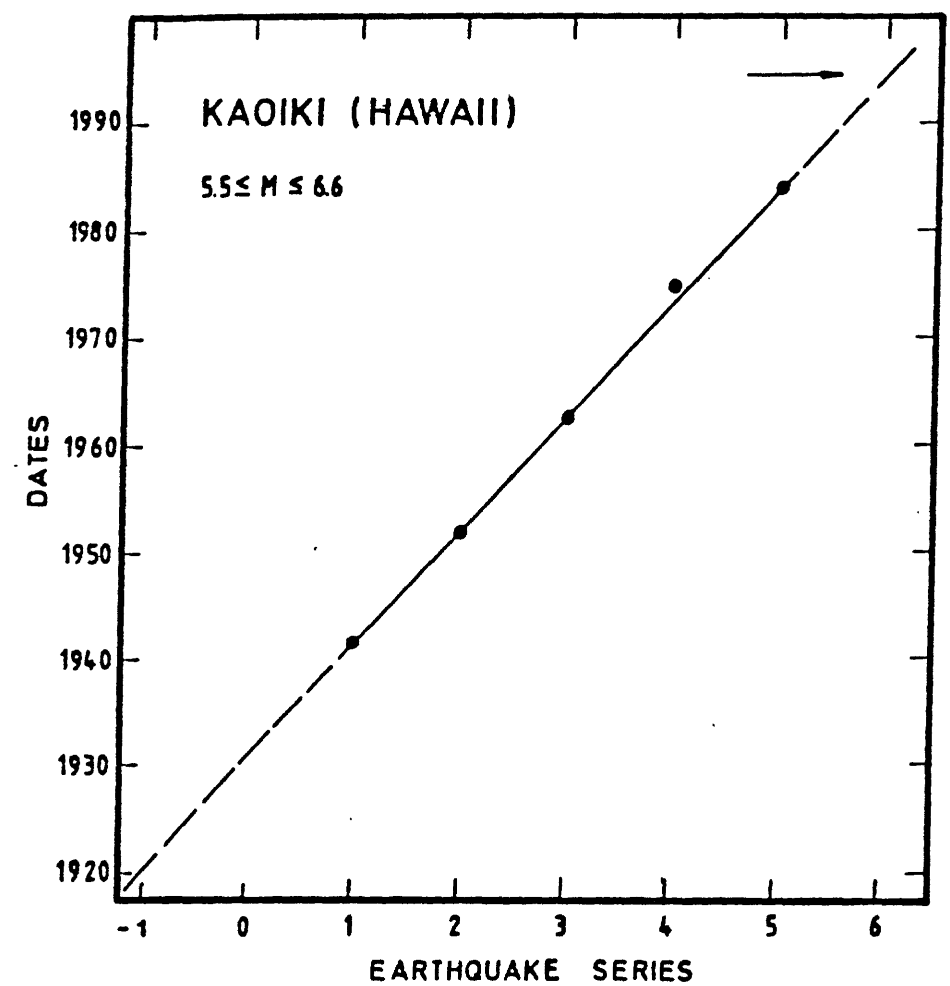

Figure 1: Dates of Kaoiki earthquakes as a function of event numbers show that a straight line fitted through the last five Kaoiki shocks suggests that a sixth shock should occur in 1994, approximately. Based on the hypothesis that the Kaoiki sequence is regular, one may expect to find historic evidence for mainshocks in or near the years of 1931 and 1920.

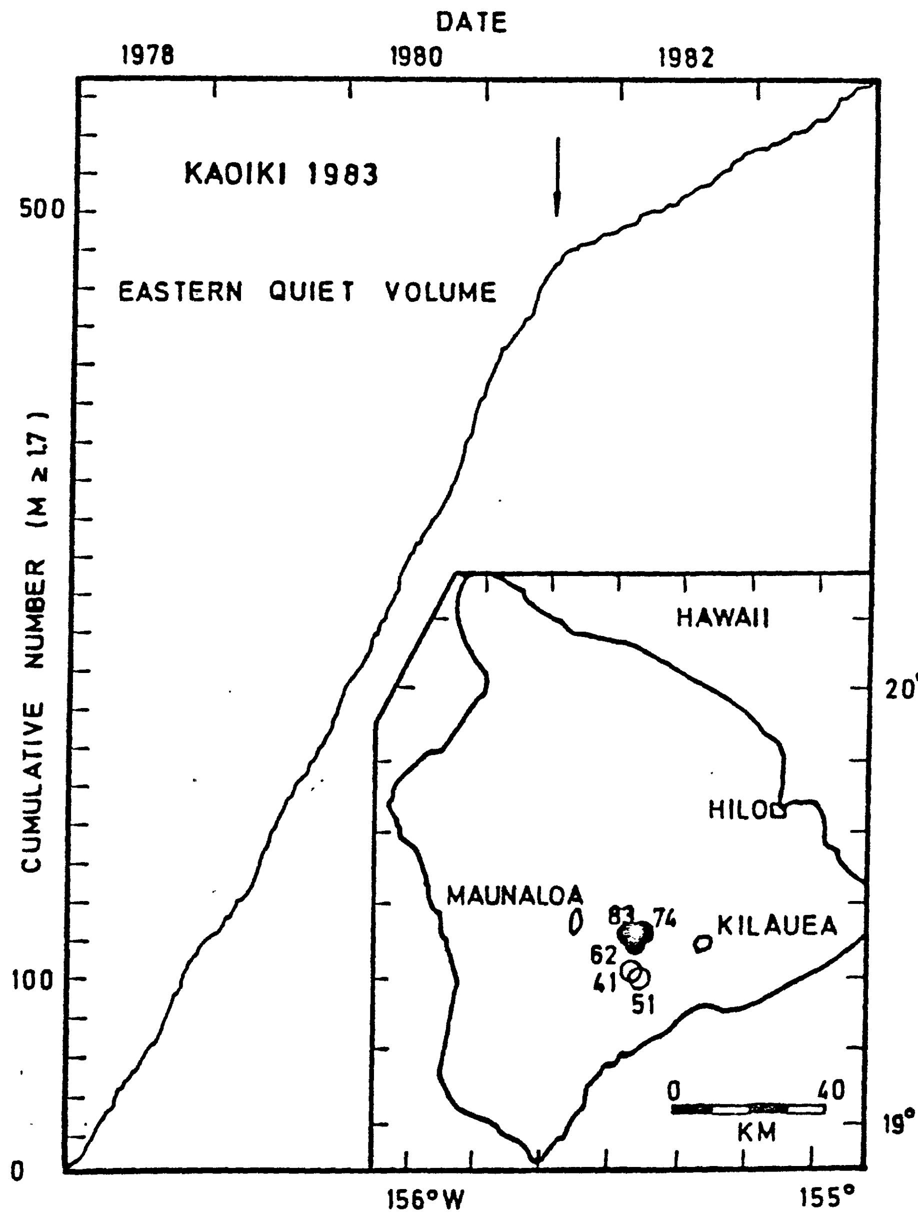

Figure 2: Cumulative number of micro-earthquakes (ML≥1.7) as a function of time for six years before the last Kaoiki mainshock (Ms=6.6, November, 1983). The occurrence time of the mainshock is at the right edge of the figure, the volume in which the earthquakes were counted was located within the eastern part of the mainshock source volume, and had dimensions of approximately 10x5 km (from 14). The arrow in mid-1981 points to the onset of quiescence when the mean rate of micro-earthquakes decreased by 75% as shown by the clear change of slope of the cummulative number curve. The inset shows a map of Hawaii with the accurately determined epicenters (8) of the 1962, 1974 and 1983 Kaoiki mainshocks marked by dots. The open circles mark the 1941 and 1951 epicenters which are less accurately known (18). All epicenters are located within a circle of radius=6 km.

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February 7, 1986

Prof. Lynn R. Sykes
Chairman, National Earthquake Prediction
Evaluation Council
Lamont-Doherty Geological Laboratory
Palisades, NY 10964.

Dear Lynn:

Thank you for putting me on the mailing list for the Council reports.

My own conclusions for what they are worth:

1. Most of the precursor work seems irrelevant. The only really relevant precursors are seismic events but no one seems able to interpret the state of the fault on the basis of past seismicity.

2. If I can somehow get hold of the Parkfield slip history I'd like to give it a try with my new model. Any interest?

3. Lindh thinks that if he could get hold of more different kinds of predictors his probability would get higher. I feel this is wrong. The probability should not depend on measurements. Supplementary measurements should improve his estimate of the probability by narrowing his error range but that is all. In fact, if one doesn't understand why the signals fluctuate on the creep meter or the magnetometer and so on, these supplementary inputs might well increase his error!

4. Einstein once said that science is an extension of ordinary thinking. There is a lot of ordinary thinking in your report but not enough science. I am convinced at least one key premise is wrong but who will evaluate me? I am rather tempted to submit my own prediction paper on Parkfield (if I can get the slip data, that is), but I am wondering as an outsider if the Council can subject it to its scrutiny.

With best regards,

Cinna.

C. Lomnitz
Professor of Seismology.

United States Department of the Interior

GEOLOGICAL SURVEY

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February 27, 1986

Professor L.R. Sykes
Chairman
National Earthquake Prediction Evaluation Council
Lamont-Doherty Geological Observatory
Columbia University
Palisades, New York 10964

Dear Lynn:

At your request I am responding in writing to Chris Scholz's letter of November 15, 1985 regarding the true distribution of coseismic slip in 1906 on the San Andreas fault southeast of San Francisco.

Chris's main point seems to be that the triangulation data used in Thatcher [1975] was contaminated by effects due to the 1868 earthquake on the Hayward fault. He refers to displacement vectors of triangulation stations obtained by Hayford and Baldwin [1908] to buttress his argument, especially referring to the successive displacements of station Loma-Prieta. The relevant part of Hayford and Baldwin's analysis is reproduced in the attached figure. Although I believe that several features of this analysis are suspect, careful inspection of the included figure suggests that the 1868 earthquake is not likely to have caused the inferred station displacements at Loma Prieta. First, this station is nearly 50 km from the nearest reported ground breakage in 1868 and 20 km from the Calaveras fault, the southeastern continuation of the Hayward system. However, even if it were conceded that 1868 earthquake deformation at Loma Prieta was possible, the sense of relative displacement shown there by Hayford and Baldwin would imply <u>left-lateral</u> slip across the Hayward-Calaveras fault system in 1868 (refer to the figure and note that displacement arrow shows SSE movement of Loma Prieta relative to the assumed fixed stations to the east of the Hayward-Calaveras). I conclude that Chris's argument cannot rationalize the difference between the surface slip values observed southeast of Black Mountain and those determined in my 1975 paper using geodetic measurements.

However, could other movements during ~1860-1880 possibly explain the discrepancy? Mike Lisowski and I have been looking into this and think not. After rechecking the angle change observations used in Thatcher [1975] we did a series of new model calculations that investigated possible ways of reconciling the discrepancy between surface offset and geodetic slip estimates. In particular, we did a model calculation in which all pre-1880 data were excluded, and the derived slip values did not differ significantly from those obtained earlier. We also examined the effects of varying the precise location of model fault segments, changing fault depth from the assumed value of 10 km, and using adjusted rather than observed angle changes in the computations. Results were all embarrassingly close to the earlier estimates of 2.7 ± 0.3 meters for the slip southeast of about Page Mill Road, roughly twice the maximum surface slip values reported for this segment. We believe the geodetically-determined values are the more reliable slip estimates and recommend that anyone doing earthquake recurrence calculations for the southeast end of the 1906 rupture use these values.

The cause of the discrepancy in slip estimates is of some interest. I suspect it is caused by the very same trend changes quoted by Scholz and by Nishenko and Williams in their reports from the July 1985 NEPEC meeting. These trend changes cause the fault zone to be significantly broader and more complex southeast of Black Mountain as compared with northwest of it, making the total fault offset much more difficult to observe at the surface. Extensive landsliding and difficulty of access may have contributed as well.

We're preparing a paper for publication and will pass it along once it's completed.

Sincerely,

Wayne

Wayne Thatcher

Enclosure

cc: C.H. Scholz

Figure 1: Displacement vectors of triangulation stations in San Francisco Bay region determined by Hayford and Baldwin [1908]. Extent of surface faulting in 1868 (Hayward fault) and 1906 (San Andreas fault) earthquakes is indicated by heavy lines. The three eastern-most triangulation stations, held fixed in Hayford and Baldwin's analysis, are shown circled.

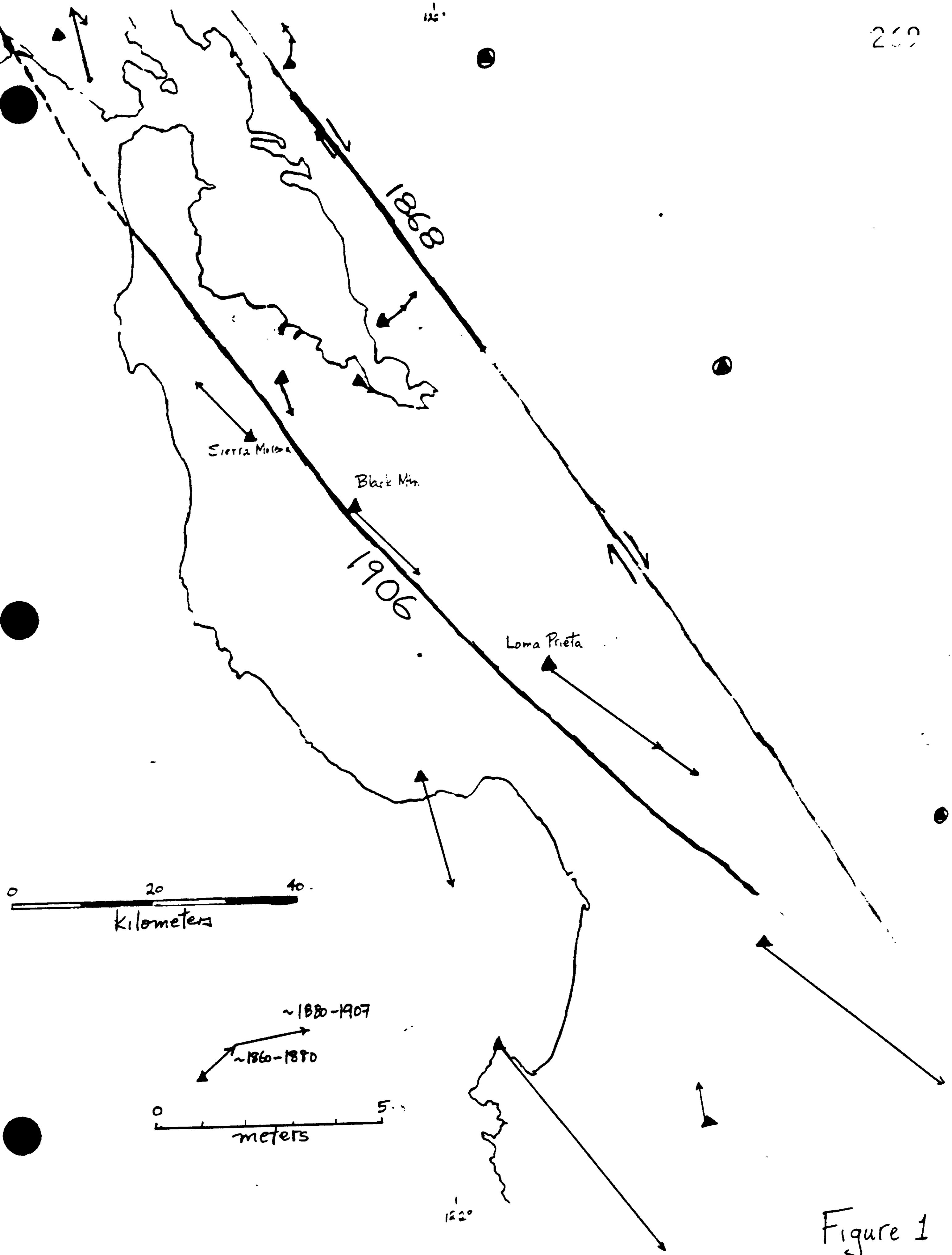
122°
1868
Sierra Morena
Black Mtn.
1906
Loma Prieta
0
20
40
kilometers
~1880-1907
~1860-1880
0
5
meters
122°

Figure 1

STATE OF CALIFORNIA—THE RESOURCES AGENCY GEORGE DEUKMEJIAN, Governor

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February 28, 1986

William H. Bakun
U.S. Geological Survey

✓ Lynn Sykes
Lamont Doherty Observatory

SUBJECT: Commentary on "Parkfield Earthquake Prediction Scenarios and Response Plans"

I am presenting initial comments on the latest draft of "Parkfield Earthquake Prediction Scenarios and Response Plans."

I have circulated your material to CEPEC members as well as undertaken my own review. In general, CEPEC members find the document a useful analysis of prospective precursory observations and a satisfactory interpretation of the probabilistic estimates associated with their occurrence. Time has not provided us the opportunity to discuss this material as a group, but I doubt that we will have radical changes to propose after we consider it at our next meeting. I plan to convey any suggestions that we do have at that time to you.

The manner in which the material is presented is thought to be overly complicated by some of our group. I personnaly find it as straightforward as the subject material permits.

In the discussion of Response, I plan to coordinate my remarks regarding a and b alarm level responses with OES, prior to mailing specific recommendations. These will be transmitted to you as soon as possible.

I understand from pevious conversations that the U.S. Geological Survey wishes to implement this arrangement of thresholds and responses on or about March 1. I encourage you and the Director of the U.S. Geological Survey to do this. This arrangement will provide a means of dealing with the eventualities that I expressed concern about at the NEPEC meeting in July 1985. I believe that subsequent fine tuning can then proceed on the basis of experience and additional contemplation by parties such as NEPEC and CEPEC.

William H. Bakun
Lynn Sykes
Page 2
February 28, 1986

I am enclosing a letter which I received from Dave Jackson of CEPEC who comments on the flexibility of the plan and the probabilistic estimates. CEPEC will review this letter at its next meeting.

I regret that the circumstances in Sacramento will make it impossible to be present for the meeting on March 1, 1986.

James F. Davis
State Geologist

cc: John Filson
Jim Watkins
Jim Goltz
Dennis Miloti
Brian Tucker

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7 March 1986

Dr. John Filson
Chief, OEVE
U.S. Geological Survey
National Center, MS 905
12201 Sunrise Valley Drive
Reston, VA 22092

Dear John,

I am enclosing a letter of March 3 that I received from Dr. Christopher Scholz about the geodetic data from the peninsular section of the San Andreas fault that was presented to us at the last NEPEC meeting on March 1. Scholz asks NEPEC to obtain an independent opinion about the triangulation data.

In view of the importance of the geodetic data to understanding the potential for a large earthquake on the peninsular section of the San Andreas fault, I am writing to ask you to solicit an opinion from the National Geodetic Survey or another university or governmental group. I would suggest that they be given all of the materials that have been presented by either Thatcher or Scholz on this matter. I believe that we should ask them for their evaluation of the inferred movements of the survey point Loma Prieta and an assessment of its accuracy for the two time intervals in question. If they are able to make an assessment, we should also ask them to comment upon the inferred displacements along the adjacent parts of the San Andreas fault in 1906 and of their accuracy.

I would hope that we get some type of opinion on this matter for our next NEPEC meeting in about six months. One person's name who comes to mind in NGS is Dr. William Strange.

Sincerely yours,

Lynn R. Sykes
Chairman, National Earthquake Prediction
Evaluation Council

LRS/llm

cc: All NEPEC Members

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March 3, 1986

Prof. Lynn R. Sykes
Chairman
National Earthquake Prediction Evaluation Council
Lamont-Doherty Geological Observatory
Palisades, NY 10964

Dear Lynn:

I would like to once again respond to the assertions made by Wayne Thatcher, in his testimony at the workshop on Feb. 28, and in his letter to you on Feb. 27, 1986, concerning geodetic data on fault movements on the part of the 1906 fault break southeast of Black Mountain.

Thatcher reiterated his 1975 position by running a few new models but without re-examination of the data and, of course, got the same result. I pointed out that his results were entirely dependent on the acuracy of the movements inferred for the Point Loma Prieta. Hayford and Baldwin (1908) reported 1M SSE motion of Loma Prieta in 1906, which is essentially the same as the Thatcher result. In questioning the accuracy of this determination, however, I pointed out that for the earlier 1868 era, Hayford and Baldwin obtained 3M SSE displacement of Loma Prieta. I agree with Thatcher that this movement could not be explained by the 1868 earthquake (or by any other conceivable earth movements), the point being that this earlier observation, which Thatcher and I would both reject as being meaningless, casts into considerable doubt the degree of accuracy that Thatcher ascribes to the 1906 movement determination to Loma Prieta.

Thatcher's response was that "he inferred from Hayford and Baldwin that Loma Prieta was not measured in the intermediate period, and the large movement for it determined for the 1868 era was caused by large errors in carrying measurements made far to the north, so that it is not comparable with the 1906 determination." When questioned about the accuracy he quoted for the triangulation measurements, he stated that he used standard errors for first order surveying, which were checked with closure errors within triangles. Since, as I pointed out, horizontal refraction could be a source of serious error in triangulation in a climatic area like the San Francisco Bay Area, his statement is then crucially dependent on triangle closure checks.

I have checked these statements with Hayford and Baldwin, and found them incorrect in the two crucial points:

a) Loma Prieta was measured three times, 1854-55, 1876-87, 1906-07. Thus the intermediate determination of the movement there was based on actual measurements.

b) In the 1906-07 measurements, there were no closed triangles to Loma Prieta, hence no closure checks could be made.

I support this with the following quotes from Hayford and Baldwin (1908), and I attach a sketch map for your information.

i. They did not carry displacement determination as stated by Thatcher, except over short distances and where carefully stipulated in their table: pg. 122, para. 6.

"For some cases, as, for example, Point Reyes Hill, the separate displacements were not directly determined by the triangulation but only the combined displacements. In such cases, if probable values could be derived for the separate displacements, indirectly, by inference from surrounding points, they were so derived and placed in the table. In each case, such inferred displacements are clearly distinguished in the table from others which were determined directly by measurement, by leaving the third and fifth columns blank and by having columns six through ten enclosed in parentheses."

ii. Loma Prieta was measured three times, pp. 130, para 1.

"Southern part of primary triangulation
In this group, extending Southward from the line Mocho-Sierra Morena, there are nine points of which the positions were predetermined after the earthquake of 1906. Of these, one, Loma Prieta, had been formerly determined both before and after the earthquake of 1868; five others had been determined before 1868 but not after, and three had been determined after but not before 1868. In this group, therefore, but one point is available to show the displacement of 1868."

"The triangulation of 1854-55 starting from the line Ridge to Rocky Mound near the Pulgas Base consisted of a single chain of triangles with all angles measured, down to the line Loma Prieta-Gavilan."...

"The main triangulation of 1876-1887, from the line Mount Diablo-Mocho to the line Mt. Toro-Santa Ana, consisted of a strong chain of figures with many checks, being substantially the same as map 24, if Gavilan be omitted and all stations occupied. In this triangulation, however, no complete independent determinations with checks were made of Black Mountain, Santa Cruz azimuth station, Gavilan, Point Pinos Lighthouse and Point Pinos station.

iii. Regarding accuracy to Loma Prieta in the 1906-07 measurements, pg. 130, para. 5

"...The new primary triangulation is much weaker in the figure defined by the five points, Mocho, Loma Prieta, Mt. Toro, Gavilan, and Santa Ana, than elsewhere for two reasons. First, the length must be carried without a check through the triangle Loma Prieta, Mocho, Mount Toro, of which only two angles were measured and this triangle is very unfavorable in shape for an accurate determination of length. Second, it so happened that the least accurate observations made in the primary triangulation were in this triangle or its immediate vicinity."

iv. Map 24, which gives all lines measured in 1906-07, expands on the last quoted statements. There were no triangles to Loma Prieta that were closed in 1906-07, and in particular, of the two triangles Mt. Diablo, Loma Prieta, Mocho; and Mocho, Loma Prieta, Santa Ana; which are the most crucial for movement determinations of Loma Prieta, only one angle was measured in each (from Loma Prieta).

v. Finally, regarding Thatcher's argument that data for Black Mountain is supportive of his argument, I point out that, aside from the fact that Black Mountain is at the end of the fault segment in question and in a documented disturbed zone, it was not measured in 1876-87 (quote ii above and Hayford and Baldwin, map 24), so should not be included in the discussion. Any criticism of the early Loma Prieta measurement would apply equally to Black Mountain.

I think that the above documentation should serve to show that the two main statements made by Thatcher to support his claims for the accuracy of the movements inferred at Loma Prieta are plainly wrong. The accuracy of this determination is clearly in doubt. In the interests of clearing up this dispute, I think that it would be advisable if your committee asks someone who is an expert in triangulation data and who can take an independent position to re-examine the original data to advise you of the accuracy of the Loma Prieta measurement. You can probably request this of someone in the NGS.

Sincerely,

C. H. Scholz

CHS/ajd

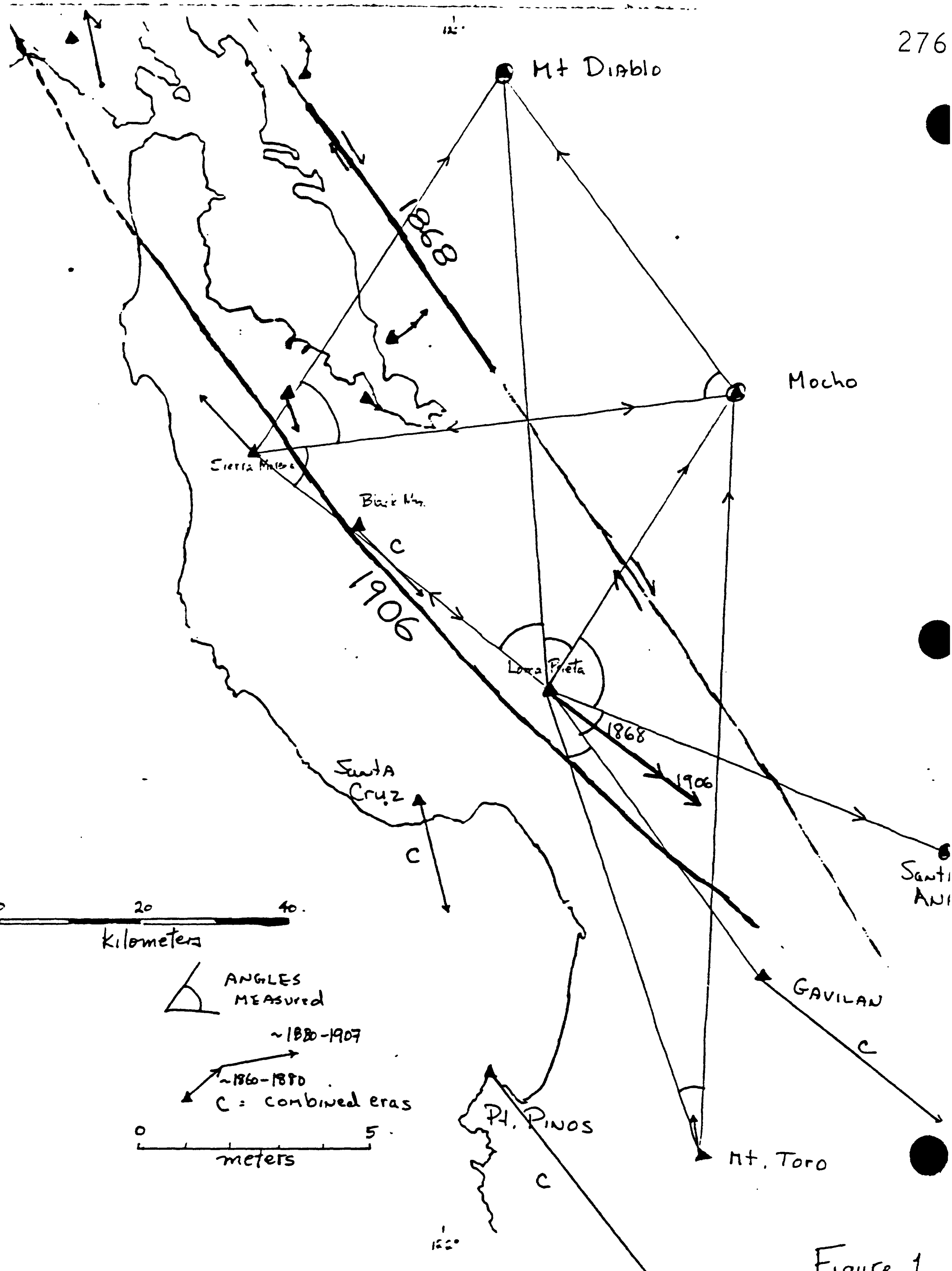
Mt Diablo
Mocho
1868
1906
Sierra Morena
Black Mtn.
Loma Prieta
1868
1906
Santa Cruz
C
Santa Ana
0 20 40
kilometers
ANGLES MEASURED
~1880-1907
~1860-1880
C = combined eras
0 5
meters
GAVILAN
Pt. Pinos
Mt. Toro

Figure 1

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May 9, 1986

Dr. Dallas Peck
Director
U.S. Geological Survey
National Center, MS
12201 Sunrise Valley Drive
Reston, VA 22092

Dear Dallas,

I am writing to report to you on the conclusions of the recent meeting of the National Earthquake Prediction Evaluation Council (NEPEC) held on Saturday, March 1, 1986 in Menlo Park. The formal NEPEC meeting was preceded by a two-day workshop on earthquake hazards and earthquake prediction for the greater San Francisco Bay area. The minutes of the meeting will be sent to you separately and will be published in an open file report along with copies of papers presented at the two meetings.

There are three major findings that we wish to convey to you: 1) NEPEC made a final review and approved the USGS document "Parkfield Earthquake Prediction Scenarios and Response Plans"; 2) reached a conclusion about faults and long-term forecasts for the greater San Francisco Bay area; and 3) synthesized results from the four major areas that we have reviewed at our meetings of the last two years and indicated priorities for further work in 12 sub-areas or fault segments. In addition to those topics NEPEC heard presentations on three of the study areas in southern California for which we had recommended working groups be organized. For each one of those areas a working group was organized with co-chairman from within and external to USGS.

NEPEC plans to hold its next meeting this summer in conjunction with a symposium on intermediate-term earthquake prediction and a later meeting devoted to earthquake hazards and earthquake forecasting for the Pacific Northwest.

SUMMARY REGARDING SAN FRANCISCO BAY REGION

The National Earthquake Prediction Evaluation Council (NEPEC) concludes that the possibility of an earthquake of magnitude near 7 within the next 20 to 30 years in a highly populated part of the San Francisco Bay region warrants very serious concerns by emergency planning groups and agencies. The highest concern is for an earthquake of M≈7 on the Hayward fault comparable to those of 1836 and 1868. Somewhat less concern was expressed for an earthquake of similar magnitude on the segment of the San Andreas fault between Black Mountain (on Monte Bello Ridge near Palo Alto) and San Juan Bautista.

The earthquake history of the San Francisco Bay region shows that M 5 1/2 to 7 earthquakes can occur throughout the 50 mile-wide zone of faults between the Pacific Coast and the central valley. During the 19th century, events within this magnitude range occurred at an average rate of about one a decade. Following the great earthquake of 1906, earthquakes of moderate size ceased for about 50 years. However, reappearance of numerous earthquakes of moderate size in the past 20 to 30 years suggests that the area may have entered into a period of activity comparable to that experienced in the few decades before the 1906 earthquake. Planning for future earthquakes, therefore, should include the possibility of locally damaging earthquakes on any of the active faults in the region.

Concern about an M≈7 earthquake on the Hayward fault is based primarily on the historical record of moderate- to large-size earthquakes and other evidence of fault activity, including abundant microseismicity, and the occurrence of creep. Details of slip-rate distribution appear to be equivocal, although a lower creep rate in the Oakland-Berkeley portion of the fault is suggested. No clear, longer-term slip rate from paleoseismicity data is available. Data on regional strain measured by geodetic nets are extremely sparse.

Additional review of the 75 km long Black Mountain-San Juan Bautista segment of the San Andreas fault reconfirmed the concern for an M≈7 earthquake on that segment, but also emphasized the near lack of data and, thus, the uncertainties reported in the September 1985 meeting. The conclusions from the September meeting are restated below in slightly modified form.

Several independent investigations of the Black Mountain-San Juan Bautista zone of the San Andreas fault have concluded that there exists a high potential for rupture of at least part of this segment of the fault sometime in the coming 20 years. Because of great uncertainties intrinsic to the historical data and uncertainties as to the details of the fault slip budget in this area, there is a divergence of scientific opinion about the probability of a large earthquake that would rupture the entire 75 km section of the fault. One interpretation of the limited data is that 1906 slip on the entire 75 km segment has now been recovered as strain buildup and consequently that this segment may now be capable of generating a large earthquake. Should this entire segment rupture, the resulting earthquake would have a magnitude of about M 7. Because of the proximity of the area to the large population centers of the south San Francisco Bay region, such an earthquake would have significant public impact.

In further review of the Calaveras fault, it was noted that the sections that broke in the 1979 Gilroy (Coyote Lake) and 1984 Morgan Hill earthquakes had slip deficits in the decades before those events. Because of the displacements in 1979 and 1984, the hazard probably is now very low on those two segments. A slip deficit does exist on the section of the Calaveras fault directly northwest of the Morgan Hill segment. Historical seismicity suggests that the earthquake recurrence rate for that segment of the Calaveras fault and others to the south are approximately 80 years. Some uncertainty concerning the association of certain nineteenth century and early twentieth century earthquakes with the Calaveras fault preclude statistical treatment and quantitative statements regarding the immience of future events northwest of Morgan Hill along the Calaveras fault. However, available evidence does indicate that consideration should be given to the occurrence of an intermediate-size event of approximately magnitude 6 on the Calaveras fault northwest of Morgan Hill during the next decade.

In summary, although the NEPEC concludes that a damaging earthquake is very possible, it emphasizes the inadequacy of existing scientific data as a basis for assessing the likelihood of earthquakes on specific faults and segments of faults in the San Francisco Bay area. Considering the potential for damage(one estimate gives $44 billion for a large shock along the Hayward fault), the NEPEC concludes that gathering additional critical data is of very high priority.

PARKFIELD EARTHQUAKE PREDICTION SENARIOS AND RESPONSE PLANS

NEPEC recommends the immediate adoption by the Geological Survey of the "Parkfield Earthquake Prediction Scenarios and Response Plans" as presented at our March 1, 1986 meeting. We appreciate that this document represents an attempt to accomplish something which has not been done before in the United States and that understanding of the earthquake phenomena is developing very rapidly. We therefore recommend that the current document be used as the basis of operations for some period of time, perhaps six months.

At that time, revision of the decision rules should be considered based upon the experience gained in working with the current document, and any new understanding of the observations or physical mechanisms of the earthquake process. NEPEC further recommends that future revisions of these decision rules also take into account possible higher level alarm thresholds for continuously recorded strain measurements (such as dilatometer and water level measurements), as well as results from computer modeling of the phenomena leading to earthquakes.

CRITICAL AREAS IN THE UNITED STATES FOR INTENSIFIED STUDIES OF EARTHQUAKE PREDICTION

In late 1984 NEPEC agreed to meet several times per year to begin a systematic review and synthesis of data from areas in the United States considered to be most critical from an earthquake prediction standpoint. NEPEC decided to examine the following areas and fault segments over its next several meetings: the Parkfield segment of the

San Andreas fault, the San Andreas and San Jacinto faults in southern California, the greater San Francisco Bay area, and several areas in Alaska. With our latest meeting we have now completed our initial goals with regard to those areas. Part of our meeting on March 1, 1986 was devoted to a discussion of those areas and fault segments that individual NEPEC members thought deserved the highest priority for instrumentation and other study. We believe that what follows represents our conclusion about priorities, advances in earthquake prediction, need for making much more intensified efforts, and opportunities for making major advances in earthquake prediction and hazards reduction.

There is a broad consensus that major advances have been made in the past five to ten years in long-range earthquake forecasts and in long-term earthquake prediction. Critical to this increased understanding have been results from paleoseismicity studies that have extended back the record of known large earthquakes in California for periods of hundreds to as much as two-thousand years. These investigations are still very much in their infancy and dates of past large earthquakes and precise rates of long-term fault movement are only available as yet for a few places. At the workshop on the greater San Francisco Bay area in February 1986 it was glaringly evident that long-term rates of fault motion simply do not exist for several major faults such as the Hayward fault that traverses the densely populated area along the east side of San Francisco Bay.

In November 1984 NEPEC endorsed the general aspects of the long-term prediction for a future earthquake at Parkfield that was brought to it by members of the USGS. NEPEC recommended that high priority be given to the Parkfield earthquake prediction experiment. We are already learning important information from Parkfield that appears to be transferable to other areas of the United States. For example, creep (aseismic slip) at Parkfield appears to be concentrated in the uppermost few kilometers whereas strain is building up on a segment or patch that extends from depths of about 4 to 13 km. That depth range was the main locus of rupture in previous Parkfield earthquakes and can be expected to be the locus of future slip in shocks of similar size. One lesson here is that creep at the surface should not be taken as a definitive indication that deeper segments of faults cannot be building up strain which is then released in large earthquakes. Large numbers of geodetic lines of various lengths re-measured many times and numerous creepmeters were needed to resolve the distribution of creep and strain buildup at Parkfield. Those studies suggest obvious experiments and work that need to be done along portions of the Hayward and Calaveras faults in the East Bay area, both of which exhibit creep at the surface.

NEPEC has identified 11 subareas along the San Andreas, San Jacinto, Hayward and Calaveras faults of California and two areas in Alaska for which opportunities exist to make significant progress in earthquake prediction during the next decade. Each of those regions is known to have broken in one or more large earthquakes either from the historic or the pre-historic record. While our understanding of many of those areas in terms of repeat times of large earthquakes, rates of long-term fault motion and time to the next large earthquake is poor, a case can be made that any one of them could rupture in a large earthquake during the next one to two decades. Not all of those zones can be expected to rupture in a large shock during that interval; it is also very unlikely that a large earthquake will not occur in at least one of those 13 areas in the next one or two decades. An intensified program of monitoring and study for the next decade could help to zero in on the few most likely places to rupture in the subsequent few years. It would also permit us to establish that several of those zones were unlikely to rupture in large shocks during the next several decades, something we cannot now do.

Our knowledge of certain of these fault segments is so poor that we are unable to assess more than approximately where they now stand in the cycle of buildup of strain to the occurrence of future large earthquakes. For example, the Hayward fault ruptured in two large earthquakes of magnitude near 7 in 1836 and 1868. Given our poor knowledge of the distribution of creep and strain buildup along segments of that fault and the absence of measurements of the long-term rate of strain buildup, we are simply unable to come to a conclusion about whether segments of that fault have either a high or a low probability of rupturing in large earthquakes in say the next decade. The point here is we can now see what needs to be done to remedy that situation.

We see the need for greatly intensifying efforts in geodetic monitoring, making paleoseismicity studies, analyzing various seismic data, and beginning the monitoring of several of the other parameters that are now being done at Parkfield if we are to make a significant impact on our understanding and to make long-term or intermediate-term predictions for the above 12 areas that are distinct from Parkfield. It would be a mistake to exactly duplicate the Parkfield experiment in each of those areas; each requires a tailoring of experiments to it alone, something that we think can be done given our present level of understanding. NEPEC recommended that working groups be established for three areas in southern California, the greater San Francisco Bay region and Alaska. All of the critical areas we identified are now covered by these working groups. The working groups for the three areas in southern California have now met and have drawn up proposed experiments and monitoring that they believe are needed to advance prediction work and have assigned priorities to various facets of their proposed work.

There is also general agreement that several segments of major faults in California, Alaska and other states have a low probability of rupturing in large earthquakes during the next one to two decades. For example, much of the southern San Jacinto fault, several portions of the Calaveras fault and the San Andreas fault north of San Francisco do not appear to be in an advanced stage in the cycle of buildup of strain, and hence it appears unlikely that they will be the sites of large to great earthquakes during the next few decades. Thus, we need not spread our resources uniformly over all active faults. In its identification of 13 critical areas, NEPEC chose to concentrate on faults or fault zones with fairly high rates of long-term movement, sites of known historic or pre-historic earthquakes and areas in which earthquakes and fault motion are better understood. The list of 13 areas should not be taken as inclusive of all sites of future large shocks; large earthquakes are known to have occurred in other areas of California and in other parts of the United States. Even if our resources were expanded by many times, we could not cover all of the fault segments that could generate significant or damaging earthquakes over the next one or two decades. For example, the rupture lengths of shocks of magnitude 6 to 6.5 are such that events of that type could occur along any one of hundreds of fault segments. Nevertheless, we think that significant progress can be made by concentrating on several subareas or fault segments that have a more obvious potential to be the sites of large earthquake over the next one or two decades.

It should be recognized that the subject of earthquake prediction, particularly predictions on a time scale shorter than a few years, is very much in its infancy. The U.S. prediction program needs to continue to emphasize the development of the scientific basis for predictions on various time scales, for understanding of the physics of fault behavior, rupture propagation and strain buildup and to continue active programs of monitoring and study in several states.

We conclude that the types of monitoring, data analysis and other scientific studies that are now being done at Parkfield will be required in the other areas we have designated for intensified efforts if we are likely to have a reasonable chance of predicting the next large earthquakes in each of those subareas or fault segments. The level of funding that either exists or is currently be considered for the Earthquake Hazards Reduction Program would not permit us to mount even one other effort similar to that at Parkfield. It needs to be recognized that significant progress in those critical areas will require intensive monitoring and analysis of a level that is much greater than those that could be obtained with present levels of funding through either federal, state or local governments.

It should be recognized that Japanese efforts in earthquake prediction, hazards analysis and engineering seismology in the Tokai region are much more extensive than say the U.S. efforts in southern California or the greater San Francisco Bay area. Scientists from the United States were instrumental in developing and perfecting the paleoseismic methods that have been so valuable for long-term forecasting and prediction. Japan has, however, taken the lead in providing the financing of large trenching efforts along active and critical faults in Japan; large numbers of scientists are involved in a given major excavation.

The chance of a large or great earthquake happening in one or a few of the areas we have designated are sufficiently great that we believe a much greater effort is required if the United States wants to make a serious and determined effort to predict those events and to gain the data we will need to predict the following generation of earthquakes. We believe that it is very important that the risks, financial needs and opportunities for the next decade be recognized by policy makers and the public. While the routine prediction of earthquakes is not just around the corner, our knowledge is of a high enough level and our progress in prediction has been sufficient that we can identify a number of key areas in which we can decidely improve our prospects for earthquake prediction over the next decade. As well as the federal government making choices about the funding of work on earthquake prediction, the level of effort we foresee, particularly in California, would seem to demand major funding by state and local governments as well as increased federal funding.

I have asked each member of NEPEC to provide a prioritized list of areas for concentrated earthquake prediction efforts. Those comments are attached. Most of us conclude that some balance is needed between making use of scientific opportunities for work on earthquake prediction (i.e., in some areas of low-population density) and societal needs in areas of high-population density. Many conclude that we need to concentrate efforts in several key areas not just a single one as is now largely the case at Parkfield.

I am enclosing a table indicating priorities for the 13 subareas or fault segments that we have reviewed thus far. While there are some differences about priorities among the various members of NEPEC, there is remarkable agreement on several important aspects. Most of the members give highest priority to those segments of the San Andreas fault between the Salton Sea and Palm Springs and between Cajon and Tejon passes, to the northern part of the San Jacinto fault near Riverside and San Bernadino, to the Hayward fault and to the Alum Rock gap along the Calaveras fault. The members split in their views about that segment of the San Andreas fault between Black Mountain on the San Francisco Peninsula and San Juan Bautista and about placing additional resources at Parkfield or elsewhere. Faults in the Los Angeles basin are given the next to highest priority by several members. The Anza gap and gaps in Alaska are generally assigned second or third priority. While San Gorgonio pass could be the site of a great earthquake along the San Andreas fault, most members conclude that that complicated area is so large and so difficult to study that increased monitoring and study of that region should be deferred pending intensive investigations of those segments of the San Andreas fault on either side of it (Salton Sea to Palm Spring and Cajon to Tejon passes). The section of the San Andreas fault between Cholame and Simmler was also thought to be of third priority, probably because of its low population density.

Sincerely yours,

Lynn R. Sykes
Chairman, National Earthquake
Prediction Evaluation Council

LS/llm
Encs.

U.S. Priorities for Areas of Intensified Studies in Earthquake Prediction
(1 = top priority; 2 = second; 3 = third)

Member of Nepec	San Andreas Fault: Salton Sea to Palm Springs	San Andreas Fault: San Gorgonio Pass	San Andreas Fault: Cajon to Tejon Passes	San Andreas Fault: Cholome to Simmler	San Andreas Fault: Parkfield: additional work	San Andreas Fault: Black Mt. to San Juan Bautista	San Jacinto Fault: Northern part	San Jacinto Fault: Anza Gap	Hayward Fault	Calaveras fault: Alum Rock Gap	Faults in Los Angeles Basin such as Newport-Inglewood	Alaskan Gaps (Alaska Peninsula and Yakataga
Aki							1					
Ellsworth	1		1		1	1	1		1		1	
Filson	1b		1c				1d		1a			
Kanamori	1	3	2	3	3	3	1	2	2	2	2	2
Sykes	1	3	1	3	2	1	1	3	1	1	2	2
Thatcher	1	3	1	3	3	3	2	3	1	1	2	3
Wallace	1b		1b		1a				1c			
Wesson	1	3	1	3	2	3	2	2	1	1	2	2
Zoback			1			1			1	1	1	

United States Department of the Interior

GEOLOGICAL SURVEY

Office of Earthquakes, Volcanoes, and Engineering
345 Middlefield Road, MS 977
Menlo Park, California 94025

March 5, 1986

Dr. Lynn R. Sykes, Chairman
National Earthquake Prediction
Evaluation Council
Lamont-Doherty Geological Observatory
of Columbia University
Palisades, New York 10964

Dear Lynn:

To voice my priorities for focussing what meager efforts are likely within the next few years:

Priority No. 1: We must remember that our highest priority is to develop a scientific basis for prediction, not necessarily to study an area. To the extent that the scientific goal can be pursued effectively in a socially-critical area, fine. In any event, we must be sure to develop base-line data in socially critical areas. We now know that, at the very least, we will need seismic, strain and paleoseismic data.

Area Priority No. 1: Continue, and amplify, the Parkfield experiment.

Area Priority No. 2: Begin to try to establish a basis for predicting the great Southern California earthquake. We should not deviate from this previously stated high priority. Do this by developing Parkfield-like experiments on the Mojave and Indio segments of the San Andreas fault. In my estimation the Cajon Pass region is the likely site for the next big earthquake, but the Pass area will be at first too complicated for instrumentation and analysis. We should use the strategy of moving into the Pass from the somewhat simpler, and more easily interpretable, Mojave and Indio segments. These experiments can be excellent science as well as leading to useful predictions.

Area Priority No. 3: The Hayward fault area. As logistically difficult as it is, a Parkfield-like experiment should be established. Because of the strain linkage across the entire fault system, the Bay Area should be treated as a whole. This could be an excellent scientific experiment as well as socially useful.

Sincerely,

Robert E. Wallace

United States Department of the Interior

GEOLOGICAL SURVEY
OFFICE OF EARTHQUAKES, VOLCANOES & ENGINEERING
Branch of Tectonophysics
345 Middlefield Road, MS/977
Menlo Park, California 94025

March 11, 1986

Professor L. R. Sykes
Chairman, NEPEC
Lamont-Doherty Geological Observatory
of Columbia University
Palisades, New York 10964

Dear Lynn:

As requested I include here a seat-of-the-pants prioritized list for special study areas in California.

1. San Andreas fault Lake Hughes → Cajon Pass ("Mojave segment")

2. Hayward fault in San Francisco East Bay region

3. San Andreas fault, San Gorgorio Pass to Salton Sea ("Indio segment")

4. Alum Rock Gap segment of Calaveras fault (over the next ~5 years).

Currently, other areas have significantly lower priority in my own mind, but this might change radically with new data, especially paleoseismic or geodetic observations in areas at present imperfectly understood. These include the Anza gap, the Peninsular San Andreas, and the Cajon Pass to San Gorgorio Pass - segment of the southern San Andreas.

I hope this list of hunches is of some use to you.

Regards,

Wayne

Wayne Thatcher

CALIFORNIA INSTITUTE OF TECHNOLOGY

SEISMOLOGICAL LABORATORY 252-21

22 April 1986

Professor L. R. Sykes
Lamont-Doherty Geological Observatory
Columbia University
Palisades, New York 10964

Dear Lynn:

Enclosed is a copy of your table of priority ranking which I filled according to the scheme provided.

Obviously, I cannot defend my vote very strongly, and do not mind being voted out, if someone else makes persuasive arguments.

In general, I gave a high priority for the segments which are simple and well defined, yet relatively little geophysical work has been done. I gave somewhat low priority to segments which involve complex loading mechanisms (e.g. edge effect). I still think that the Anza gap is a mature seismic gap, but we can monitor it using seismicity data and the data from Pinon Flats for the time being.

My general philosophy is, perhaps in contrast to that of many others, not to concentrate too intensively on one location. The relatively low rating for Parkfield reflects this .

Yours sincerely,

Hiroo Kanamori
Professor of Geophysics

HK:dp

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United States Department of the Interior

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RESTON, VA. 22092

In Reply Refer To:
Mail Stop 905

May 1, 1986

Dr. Lynn R. Sykes
Lamont-Doherty Geological Observatory
of Columbia University
Palisades, New York 10964

Dear Lynn:

This is in response to your letter of April 14, 1986. I apologize for being tardy, but I have been travelling for much of the last three weeks.

I think your draft letter to Dallas Peck is excellent, and I have no recommended changes.

Relevant to priorities for intensified studies, my order of the various fault segments is:

1. Hayward Fault

2. Salton Sea to Palm Springs section of the San Andreas

3. Cajon to Tejon Pass section of the San Andreas

4. Northern section of the San Jacinto fault

Sincerely yours,

John R. Filson
Chief, Office of Earthquakes,
Volcanoes, and Engineering

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EPARTMENT OF GEOPHYSICS
School of Earth Sciences

March 6, 1986

Professor Lynn Sykes
Chairman, National Earthquake Prediction
Evaluation Panel
Lamont-Doherty Geological Observatory
Columbia University
Palisades, New York 22092

Dear Lynn:

In response to your request at the last NEPEC meeting, I am responding on the issue of priorities for intense study within the earthquake prediction program. I am also commenting again on the current nature of the Parkfield earthquake prediction experiment because I have very serious reservations about the implications of the response matrix presented at the meeting.

First, it is clear in an era of significantly reduced funding and already severe demands on key personell, that serious choices have to be made about "what and where" things get done. In the following, I've tried to weigh factors such as earthquake probability, population density, and my personal view of the responsibilities of the Earthquake Program to the public. The following list of priorities is related specifically to where intense studies should be undertaken with the intent of eventually leading to a medium-to-short term earthquake prediction capability. Lengthening the priority list would be counter-productive.

1) San Andreas Fault from Tejon Pass to San Gorgonio Pass- This is where we know there is a high probability of large events and many people live. The section of the fault to the south of San Gorgonio Pass is a lower priority.

2) Faults in the S.F. Bay area- In order, these would be the Hayward fault, the San Andreas fault from San Juan Bautista to the northern end of 1-1.5 m slip zone, and the Calaveras fault immediately to the north of Morgan Hill.

3) Faults in the Los Angeles metropolitan area- Although this is much more difficult to define and justify, based on the hazard they pose, these faults deserve more attention than they are getting.

This brings me to Parkfield. I don't want to seem overly critical of my former colleagues. They are attempting an extremely difficult task with inadequate funds and personell. Bill Bakun's words about demands on key people were not lost on me. However, if the short-term data from the dilatometers, water wells, and two-color lasers are only going to be used to tell what happened after-the-fact, what is going on at Parkfield is not really a prediction experiment, but an earthquake monitoring experiment (and should be called that). The Parkfield experiment now seems to be a no-win situation. Suppose the next magnitude 6 earthquake at Parkfield is successfully "predicted" by using seismicity data (which does not seem to be likely from my limited understanding of the statistical data). Simply using historical seismicity data to determine the probability that an earthquake is a possible foreshock has essentially no transfer-value to other places. I fully understand that reasonable scenarios for precursory slip indicate that strain signals are likely to be quite small compared to instrumental noise levels. But if the earthquake is not predicted (or predicted only on the basis of seismicity), it seems to me that there is going to be damn little to show for so much time, expense, and hoopla.

I hope these comments are of some use.

Sincerely,

Mark D. Zoback

cc: John Filson
Wayne Thatcher
William Ellsworth

922 National Center
U.S. Geological Survey
Reston, Virginia 22092
April 25, 1986

Professor Lynn R. Sykes
Lamont-Doherty Geological Observatory
Palisades, NY 10964

Dear Lynn:

Balancing social and scientific objectives and taking into account current, existing levels of instrumentation and effort, my areal priorities for increased effort in earthquake prediction research are as follows:

<u>Top priority</u>
San Andreas fault--Salton Sea to San Gregonio Pass
San Andreas fault--Cajon Pass to Tejon Pass
Hayward fault
Calaveras fault--Alum Rock gap

<u>Second priority</u>
Increased work at Parkfield
San Jacinto fault--both northern and southern parts
Faults in Los Angeles Basin
Alaskan Gaps

<u>Third priority</u>
San Andreas fault--San Gorgonio Pass
San Andreas fault--Cholame to Simler
San Andreas fault--Black Mountain to San Juan Bautista

Additional areas where the level of work is low, but which deserve considerably more attention are the Garlock fault and the inland faults north of San Francisco Bay (Rogers Creek, Healdsburg, Alexander Valley, Maacama, Green Valley, etc.)

Sincerely yours,

Rob

Robert L. Wesson

United States Department of the Interior

GEOLOGICAL SURVEY
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March 6, 1986

Professor Lynn Sykes
Lamont-Doherty Geological Observatory
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Dear Lynn:

I have spent the past few days wrestling with your request to prioritize areas for concentrated earthquake prediction studies. The dilema, as I see it, is the trade-off between the areas of highest risk and the areas with the highest scientific potential. They are not entirely coincident in my estimation, and thus require an apparent choice to be made between advancement of the science and its application to societies needs.

From a purely scientific standpoint, the Parkfield experiment must receive top priority, as it is the only place that has thusfar been identified where the forecasted event should occur in less than a decade. All other areas considered in NEPEC's reviews or elsewhere have vastly larger uncertainties associated with the timing. In fact, given our present knowledge, the next place after Parkfield is probably also Parkfield (2010±). If we were able to adequately instrument many of the other areas, probability should favor a success or two in the short-term. However, the present program clearly cannot undertake such an ambitious undertaking without significant expansion. Consequently, I wish to propose an alternative strategy.

It is clear to me from the eight workshops that have been held since last February that we are making tremendous progress in the area of long-term prediction. It is equally clear that much more needs to be done. For many of the faults that we have considered, or would like to, more and better paleoseismic data would go a long way toward resolving current ambiguities about seismic potential. I strongly favor both more and more carefully focussed research in this area. Indeed, this has been one of my long-term objectives as the program coordinator for this element of the program.

The need for more and more carefully focussed geodetic surveys is equally evident. Because these data will require years to acquire, some prioritization of the work using existing technologies should be made. Hopefully, GPS technology will expand our capabilities eventually, but this is less than certain at present.

Basic research on short- and intermediate-term prediction must also continue in parallel to these more site specific objectives. In this regard, I strongly support the continuation of instrumentation (sensor) research at the Pinon Flat Crustal Deformation Observatory as a priority for the program. The instrumentation program at Parkfield is equally important, and should be considered as the primary site for experimental work on other prediction methodologies (electrical, seismic velocity and attenuation, magnetic, radio frequency, geochemical, etc.) that lack a clearly defined physical link to the earthquake preparation process.

Returning, then, to your original request, my priorities are:

- o Parkfield Experiment, including enhancement planned with State of California/U.S.G.S. matching funds and proposed extensions described in the NRC briefing of OSTP.

- o Paleoseismic investigations and enhanced geodetic coverage (using geodimeter, alinement array and portable 2-color laser techniques) in the following areas:
  - Mojave segment of San Andreas fault
  - Hayward Fault
  - Northern half of the San Jacinto fault and its conflyuence with the San Andreas fault
  - Indio segment of San Andreas fault
  - San Francisco Peninsula segment of the San Andreas fault
  - Newport-Inglewood fault

These are also my ranked priorities for concentrated research efforts. However, I do not recommend an expanded instrumental strain program at this time because the community appears to be saturated with the work at hand at Parkfield, Pinon Flat and Long Valley.

In conclusion, let me add that our most basic tool providing continuous observation of all of the identified seismic gaps and high potential faults--regional seismographic networks--are seriously imperiled by budgetary decisions being made for the coming fiscal year. The loss of these networks in the areas of high seismic potential, and indeed elsewhere throughout the country, would constitute a scientific disaster of the first order.

Sincerely yours,

Bill

William L. Ellsworth
Chief, Branch of Seismology

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

10 March 1986

Prof. Lynn Sykes
Lamont-Doherty Geological Laboratory
Palisades, NY 10964

Dear Lynn:

This is in response to your request for the list of likely places for rupture. Ever since I attended the USGS workshop at San Diego, where Dr. Matti told us that the northern part of San Jacinto fault may be the current plate boundary, I have been most concerned with the segment. Major historic earthquakes seem to have occurred along the segment and it shows a rather impressive seismicity gap in Tom Heaton's plot of microearthquake hypocenters. The recent San Bernardino earthquake ($M \gtrsim 5$) may be an indication of stress build-up. Other places discussed in the meeting are also likely to break, but I felt more strongly with the northern part of San Jacinto (north of Anza; I don't feel that Anza is a gap where strain is building up).

Sincerely yours,

Kei

Keiiti Aki

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May 14, 1986

Prof. Lynn R. Sykes
NEPEC

Dear Lynn:

This is to follow up my letter and note of December 26, 1985, on the possible detection of reverse tilt on the Shumagin Island level lines. The two enclosed figures show (1) low-pass filtered sea level from the inner (SQH), central (PRS) and outer (SIM) islands, (2) the difference between SQH and SIM.

Figure 1 shows on all stations the well-known 25-30 cm increase in sea level during the winter. Figure 2 shows a rise in the SQH-SIM difference during the fall (which prompted my earlier letter), followed by a fall during the spring. The shape and timing of the difference signal correlates well with the sea-level curve, suggesting that a decrease in annual cycle amplitude away from the coast is almost certainly responsible. I do not now believe that the sea level gauges recorded any unusual tilt signal in connection with the October-November 1985 earthquakes in the Shumagins.

Sincerely yours,

John.

John Beavan

JB/ajd
Encs.

cc: K. Jacob
J. Taber

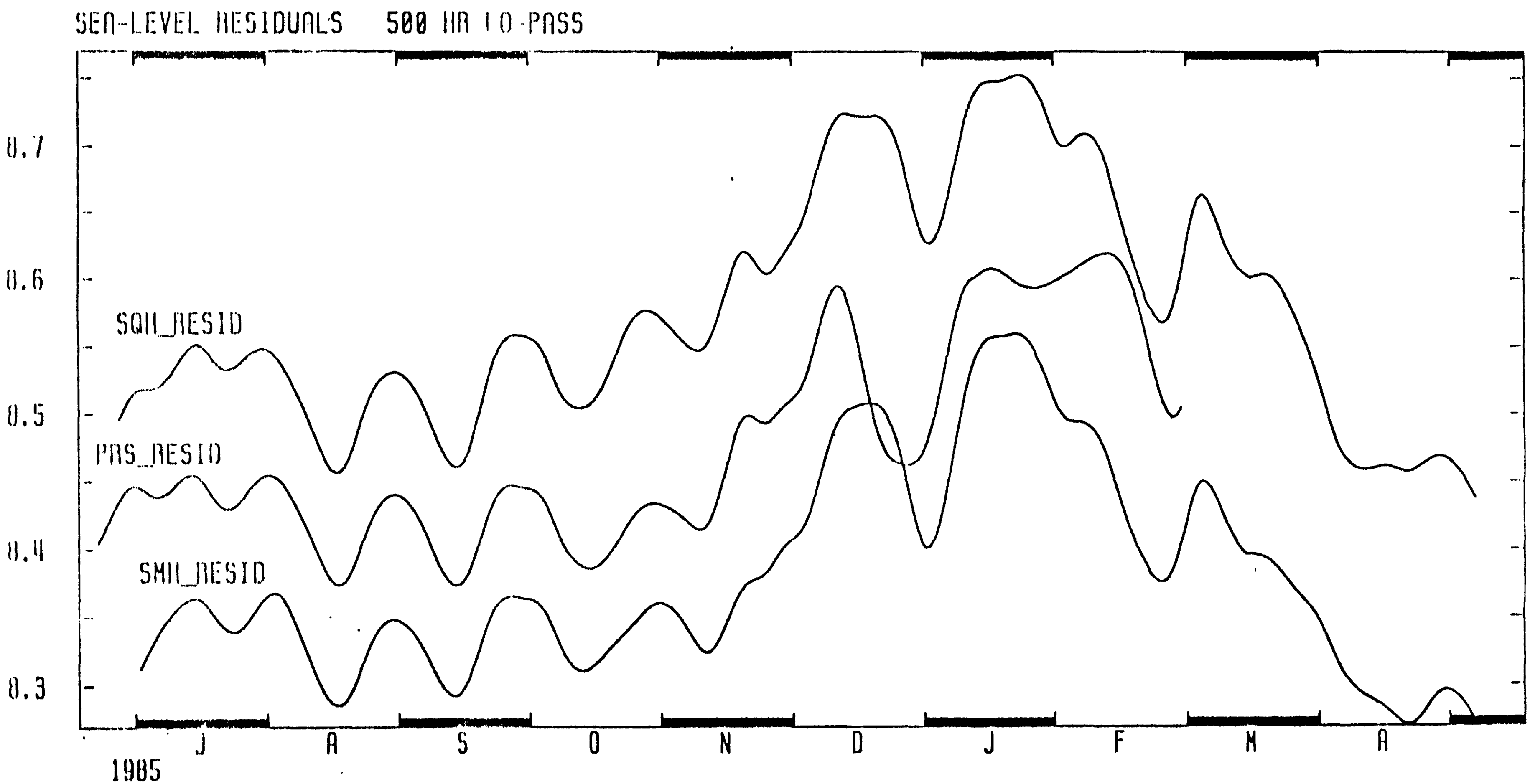

Fig. 1. Sea level signals from the Shumagin Islands, low-passed at 500 hr (≈ 3 weeks). SQH is in the inner islands, PRS in the central islands and SIM in the outer islands. Gauge separation is ≈ 50 km. All three signals show the well-known 25-30 cm annual cycle which is caused by seasonal variations in the North Pacific gyre. Sea level is detected using Paroscientific pressure sensors and the data are processed as described by Beavan et al. (JGR, in press, 1986).

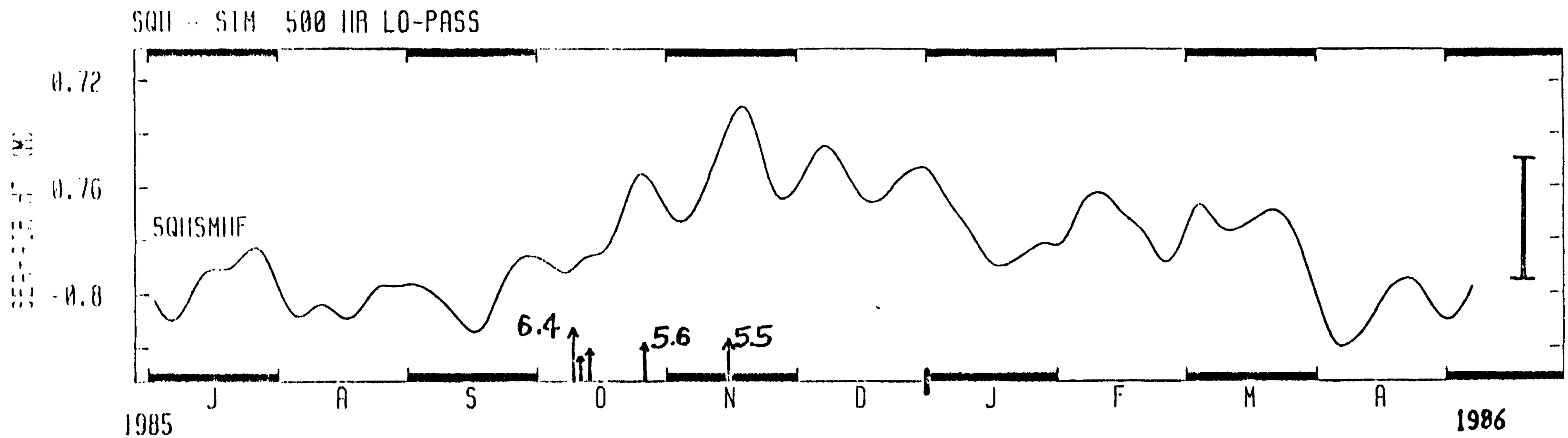

Fig. 2. Sea level difference between SQH and SIM. The increase between September and November 1985, together with the occurrence of the October-November 1985 earthquakes, led to speculation that the sea level gauges might be detecting a "reverse tilt" similar to that observed on level lines between 1978 and 1980 (Beavan et al., JGR, 89, 4478-4492, 1984). The newer data show that the difference has returned to its summer 1985 value. The timing of the rise and fall in the difference correlates closely with the seasonal rise and fall in sea level. Hence, we now believe that the September-November signal was of oceanographic origin and is due to a decrease in the annual cycle amplitude with distance away from the coast.
For detecting future tectonic signals, it is encouraging that the Paroscientific pressure sensors appear to have the long-term stability and low noise level to detect these small (few cm) changes. The rms noise level, excluding the annual cycle, is less than 30 mm. The bar at the right of the figure represents 0.5 μrad equivalent ground tilt.

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May 14, 1986

Prof. Lynn R. Sykes
Lamont-Doherty Geological Observatory
of Columbia University
Palisades, NY 10964

Dear Lynn:

With apologies for the delay and the hope that you can still use it, I offer this response to your request of 14 April.

First, on the report to Dallas of the 01 March 86 meeting, I have only general comments. The summary of the Bay Area review is fine, and the recommended adoption of the prediction scenarios and response plan for Parkfield is given appropriate emphasis. The 'Critical Areas' discussion (leading to the priorities list), however, gives me some pause. Not that the statements of concern over these possible earthquakes are in any way erroneous, but rather that NEPEC is going on record citing the inadequacy of the U.S. effort, and suggesting that a program is needed much expanded to other at-risk areas. Our reasoning goes that the same monitoring intensity (albeit customized) underway at Parkfield would, in the other areas, be inherently good (for the EHRP program, particularly Earthquake Prediction). I am not sure this is a given. While more information on the hazardous fault zones may well be of value, in absolute terms, we are dealing with a program now on the line publicly to produce, despite its infancy. It may well transpire that the successful earthquake prediction techniques will be found only by a more intensive and costly study in one place (e.g., deep borehole observation of electrical properties or some other such presently ignored technology or measurement). My point is that it is not clear to me that spreading more funds around to do 'conventional' Parkfield-like studies in other areas of high seismic potential is better than putting all new funds into more intensive and unconventional but promising measurements at Parkfield (for a bizarre example, a cross-hole shear-wave anisotropy tomography study at 6 km depth across the Middle Mtn. zone - at $20 M or so). In other words, we cannot say whether more of the same at other sites is better than the next level of sophistication at one site.

So, in filling out your table, I can only rate Parkfield as top priority. Next priority would include Cajon, Black Mtn., S. San Jacinto and Hayward. The rest are third, to my way of thinking.

I hope you can fathom my line of reasoning. I think it is the only logical approach given the enormity of the task set in the Parkfield exercise, its significance to the program and the finite human and material resources available for the effort.

Again, my apologies for the delay - no excuse.

Sincerely,

Tom

Thomas V. McEvilly
Division Head, Earth Sciences