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Microearthquake Seismology in USGS Volcano and Earthquake Hazards Studies: 1953-1995

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Development of Microearthquake Seismology
in the USGS Volcano and Earthquake Hazards Reduction Programs

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Introduction

This account chronicles the development of microearthquake seismology in the volcano and earthquake programs of the USGS over the last four decades. It is based primarily on my own experiences in these programs and my perception of circumstances inside as well as outside the Survey that influenced their development. I believe that I am in a unique position to write this history because I have participated in the earthquake program from its inception, both in the development of equipment and analytical methods and as Branch Chief during the critical years, 1970 - 1982, that the California networks were constructed. My vantage point changed with time along with my role in the earthquake program. In Hawaii (Sept 1953 - Oct 1961) and Denver (Oct 1961 - Sept 1965) I worked in relatively small, compact groups that were housed away from the larger USGS centers, and I took part in program planning; so I was well acquainted with the events and issues reported below. In Menlo Park (Sept 1965 - October 1995) my involvement in program planning, management, and execution evolved as the earthquake program grew. From Sept 1965 to Nov 1970 my situation was much as it had been in Denver: the Branch was still of moderate size and was housed away from the regional center; and as the senior earthquake seismologist in the Branch I participated in the councils that planned Branch programs and strategies. During this period our group was reorganized from Crustal Studies, with Jack Healy as Branch Chief, to the Office of Crustal Studies and Earthquake Research, with Lou Pakiser as Office Chief.

In November 1970 Lou Pakiser stepped down and I became the Chief of OERCS. I continued in that position from November 1970 until August 1973, when the USGS acquired most of the earthquake elements of the NOAA program and the OERCS headquarters was moved from Menlo Park to Reston, with Bob Hamilton as its new Chief. I continued to serve as "Chief of the Earthquake Program, OERCS, Menlo Park" until October 1974, when the earthquake program was formally reorganized as the "Office of Earthquake Studies" with four Branches: Branch of Seismology and Branch of Earthquake Tectonics in Menlo Park, Branch of Seismic Engineering in San Francisco, and Branch of Seismicity and Risk Analysis in Boulder, CO. I then served as Chief of the Branch of Seismology until January 1975. Most of the interactions among USGS, NOAA, ARPA, OST, OMB, GAO, and Congress that resulted in the transfer of most of the NOAA earthquake program to the USGS and the first substantial increase in direct funding to the USGS for the earthquake hazard program occurred during my term as Chief of OERCS. (See the glossary for an explanation of acronyms).

From January 1975 until June 1978 I worked as a research seismologist in the Branch of Seismology, and my principal effort was devoted to improving and documenting the performance of the seismic and data recording systems employed by the Northern California Seismic Network (NCSN). In June 1978 OES was reorganized again in response to the passage of the Earthquake Hazards Reduction Act of 1977 and the large increase in funding and responsibility that it brought to the USGS. From June 1978 until June 1982 I served as Acting Chief of the new Branch of Network Operations, with John VanSchaack as Associate Branch Chief.

Since June 1982 I have worked as a research seismologist in the Branch of Seismology on topics involving NCSN instrumentation, data analysis, and network planning as well as on detailed studies of large northern California earthquakes. During this period my attention has been rather narrowly focussed on NCSN and its problems, and my contact with earthquake program planners and strategists has been minimal.

I was separated involuntarily from the Survey by a Reduction In Force action in October 1995, presumably because my special skills were no longer required by the earthquake hazard reduction program.

My motivation for writing this account arises from several concerns. First, the story of the development of the earthquake program within the Geological Survey illuminates the interactions among individuals and institutions inside and outside of government needed to organize and secure public support and funding for such a program. Second, discussions with colleagues in the earthquake program reveal that the history of the program's development and the specific concerns for public safety, as well as the unifying program goals, that drew us into the struggle to establish a national earthquake program are fading from memory. Third, I wish to highlight the critical role that systematic long-term instrumental observations of earthquakes play in the search for a comprehensive understanding of structures and processes within the earth's crust and mantle that generate earthquakes. The development, updating, and operation of the seismic networks and data recording and analysis systems needed to obtain these observations are never-ending tasks that are at the very core of the earthquake program. Finally, I wish to illustrate steps in the development of the USGS microearthquake program with examples drawn from my own work, which has been entwined with that program for the last 42 years.

The history presented below is divided into several sections. The first section covers broader elements of program and institutional development from 1953 through 1978, the second section emphasizes the instruments and techniques devised for use in the earthquake program as well as the installation of the networks, and the third and fourth sections deal with the analysis of network seismograms and research applications of network data. The fifth section summarizes results on the movement of lava between the Kilauea summit reservoir and the Kilauea Iki lava lake during the 1959 Kilauea Iki eruption, based on an analysis of tilt deformation of the Kilauea summit, that I prepared for the Diamond Jubilee Symposium of the Hawaiian Volcano Observatory in 1987. The next two section are devoted to work that I have carried out in support of NCSN during the last five years: development and evaluation of local earthquake amplitude and duration magnitude "scales", analysis of the L4C seismometer and calibration procedures for use with it, and systematic treatment of the overall response of the USGS short-period seismic system. The last section chronicles my efforts to reverse the decline of the northern and southern California microearthquake networks that has resulted from changes in OES organization and funding procedures and the apparent abandonment of earthquake prediction as an earthquake program goal.

Because this history is based on documents saved from my days as a Branch Chief and letters and publications involving my own work, it obviously tells the story from my own perspective. It does not deal with many important parts of the broader earthquake hazards reduction program nor do justice to important contributors to the program because I do not have an adequate record of their work. One might hope that the deficiencies of this report will stimulate others to write complementary versions of the story from their perspectives and help preserve a balanced history of the earthquake program.

I. Historical overview of program development

A. Hawaii

When offered my first professional job as a seismologist at the Hawaiian Volcano Observatory while completing my Ph.D. thesis with Professor Perry Byerly at U.C. Berkeley, I was concerned by the apparent isolation of Hawaii, the primitive state of seismology at HVO, and the general understanding that the Geological Survey did not do instrumental earthquake seismology. In spite of such misgivings, the lure of a two-year adventure in Hawaii was irresistible; so I accepted the offer.

When I joined the staff of HVO as seismologist in September 1953, I became the only "practicing" earthquake seismologist in the Survey at the Ph.D. level. Subsequent participation of the Survey in the Uranium program and at NTS brought exploration geophysicists, including seismologists, into the organization, but their work did not extend to earthquakes; and as a member of the General Geology Branch in far away Hawaii, I had no contact with them.

Offsetting its isolation, HVO offered unending stimulation, freedom to identify opportunities or problems and to pursue them in my own way, a modest but versatile machine shop and capable staff, and supportive professional coworkers who were experts on the geology and petrology of the volcanoes and the work and history of the Observatory. The location of the Observatory at the summit of Kilauea inside Hawaii National Park (now Hawaiian Volcanoes National Park), which encompassed many of the principal active features of the volcanoes, placed our work and living areas at the center of our "field" area. Moreover, during the 8 years that I was in Hawaii, Kilauea went through two similar large cycles of inflation, summit and flank eruption, and deflation separated by several years of quiescence. The first cycle demonstrated the inadequacy of available seismic and strain measuring instruments but revealed the nature of the phenomena that accompanied the eruption. The quiet interval permitted the design and installation of more appropriate seismographs and tiltmeters in preparation for future eruptions. The new instruments reaped a rich harvest of earthquakes and deformation measurements during the second eruptive cycle and provided the principal basis for a model to explain the mechanics of Hawaiian eruptions (Eaton and Murata, 1960; Eaton, 1962).

My experience in Hawaii left me in awe not only of the irresistible destructive power of its volcanoes but also that of even distant earthquakes. A small group of us from HVO witnessed the destruction of a large part of Hilo, Hawaii, by the tsunami generated by the great 1960 Chilean earthquake (Eaton, Richter, and Ault, 1961). We were particularly distressed by the deaths of more than 60 people in Hilo. Timely interpretation of widely available seismic and wave height data and implementation of simple but effective protective measures could have saved them, but such actions were not carried out because of lack of adequate planning and organization for such an emergency.

B. Denver

As my stay in Hawaii drew to an end in 1961, prospects for returning to an earthquake seismology project with the Survey on the mainland brightened. I was invited to join the recently organized Crustal Studies Branch in Denver to help with its long-range seismic crustal refraction program in the western US and to develop an earthquake program in the Rocky Mountain region. In its work and organization, Crustal Studies was very different from HVO. In essence, it was a large seismic exploration project that employed ten commercial truck-mounted low-frequency seismic arrays (6 seismometers at 0.5 km intervals) to record 2,000 to 10,000 pound chemical explosions along profiles up to 300 km in length. The Branch showed how effective a skilled team could be in carrying out a far-flung seismic experiment in an orderly and timely manner. Improvised seismic record sections of these profiles revealed details of crustal wave propagation not recognized in earthquake seismograms from isolated stations.

Working with Crustal Studies introduced me to compact low-frequency commercial seismometers and modern transistorized electronic systems employed by the seismic exploration industry. It also permitted me to develop a self-contained photographically recorded seismic station, designed to run for one month without attendance, for use in a microearthquake network in Yellowstone National Park in the northern Rockies.

Work with Crustal Studies also introduced me to the Russian literature on Deep Seismic Sounding and on their microearthquake program in Tadjikistan. Intensive Russian work on these topics predated our own by about a decade.

As the most experienced earthquake seismologist in the Survey, I was also asked to carry out tasks outside Crustal Studies. The most important of these was a review of seismic hazards at the proposed P G and E nuclear power plant beside the San Andreas fault at Bodega Bay, California.

C. Menlo Park (1965 to August 1973)

The March 27, 1964 Alaskan earthquake accelerated a change in direction of the Survey's earthquake program, particularly Crustal Studies' role in it. In a memo to the ACG for Experimental Geology and Geophysics, written only six weeks after the earthquake, I reported the consensus position of participants in a Menlo Park conference on the future of earthquake studies in the USGS held two weeks earlier: a USGS Earthquake Research Center should be set up in Menlo Park, near the San Andreas fault in central California, and Crustal Studies should move there to join a group of Menlo Park geologists to begin the work.

As these plans were viewed favorably by Survey leaders at Division and Bureau level, Crustal Studies hurried to finish its refraction work and to prepare for the move to Menlo Park. Simultaneously, the question of earthquake research on a national scale was being reviewed at a higher level. After the Alaska earthquake an Ad Hoc Panel on Earthquake Prediction, under the chairmanship of Frank Press, had been convened to prepare a report on that topic for the

Office of Science and Technology. Membership on the committee included prominent people in seismology, general geophysics, rock mechanics, and geology from universities and industry as well as from ARPA, NOAA, and USGS. L. C. Pakiser, founding Chief of Crustal Studies, was a very active member. In May 1965 the Press Panel Report was submitted to OST (Press, F., Chairman, 1965). It recommended an extensive national 10-year program to study methods for predicting earthquakes and outlined a plan of attack. The move of Crustal Studies was soon underway, with Sam Stewart and me moving to Menlo in August and September, 1965, respectively.

The release of the Press Panel Report to the public by OST on October 6, 1965, was followed on October 8 by an announcement by Secretary of Interior Udall that established the Survey's National Center for Earthquake Research (NCER) in Menlo Park, with a statement that NCER was established in direct response to the Press Panel Report. A further action of the Secretary of Interior on February 18, 1966, established an "outside" Earthquake Advisory Panel for NCER to advise the Survey on its earthquake program. The Advisory Panel was composed of 8 members (3 geologists, 2 seismologists, and 3 geophysicists). Three of its members, including its chairman, Frank Press, had served on the Press Panel for OST.

The headquarters of Crustal Studies as well as most of its personnel and facilities, completed the move to Menlo Park in the spring of 1966.

After the move of Crustal Studies to Menlo Park and the release of the Press Panel Report on earthquake prediction, the pattern of events that finally led to the National Earthquake Hazards Reduction Program became quite complicated. The Press Panel Report lifted the stigma of "pseudo-science" from the topic of prediction and stimulated widespread discussion among earth and engineering scientists on the need for an expanded program of earthquake and engineering research and on what it should encompass. It also raised the prospect of substantial funding for a national earthquake program, which swiftly engaged the interest of earthquake research and engineering groups in academia, industry, and government as well as government officials at all levels who were facing the earthquake hazards issue. Important constituencies among these groups formulated plans for such a program that addressed their own particular interests, and they published them under the aegis of a variety of standing or ad hoc committees convened by organizations at the national, state, and university level. On the state level, such activity was greatest in California, which has been the principal seat of research on damaging earthquakes since the 1906 San Francisco earthquake.

Although many of these committees and their reports were important for defining issues and arousing widespread interest in the earthquake program, the succession of committees and reports associated with the Office of Science and Technology in the Executive Office of the President appears to have provided continuity for, and exercised the primary guiding influence on, the development of the national program. OST also played a central role in stimulating federal agencies that conducted or supported earthquake research to define their individual programs more clearly and to coordinate them with those of other agencies. Although its interaction with the agencies was carried out with a "light touch", OST's authority was very great because of its high level in the Executive Department and its close association with the Office of Management and Budget, which exercises great control over the organization and funding of agency programs.

The second report prepared for OST on the earthquake program addressed the issue of how it might be organized at the federal level: "Proposal for a Ten-Year National Earthquake Hazards Program", (Ad Hoc Interagency Working Group on Earthquake Research, Federal Council for Science and Technology, chairman W. T. Pecora, June 1967). This report recommended a program similar to that of the Press Panel, but with more emphasis on engineering research and the addition of two research areas: earthquakes and fluid injection, and psychological aspects and practical implementation of earthquake prediction. It recommended the establishment of a permanent Interagency Coordinating Panel for Earthquake Hazards Research (AEC, NSF, and Departments of Commerce, Defense, HUD, and Interior). It also noted that the National Academy of Engineers had formed a committee on Earthquake Engineering, in 1966, and would recommend a program substantially greater than that of the Pecora Report.

In the mid-1960's both NOAA (Commerce) and the USGS (Interior) had initiated earthquake research programs along the San Andreas fault in central California. As plans for an expanded earthquake program were formulated, the possible duplication of effort between these groups became a major concern both of Congress and the Executive Branch. The situation was complicated further by NSF and ARPA, which both supported seismological research by means of grants or contracts to universities and other research organizations. How to apportion funding and responsibility and to insure coordination among these groups was a problem that threatened to delay development of a national earthquake program.

These issues were resolved over an eventful three-year period, spring 1970 to spring 1973, by a complex set of interactions that are sketched below. Significant participants in these interactions included OST, OMB, GAO, Office of Senator Cranston, USGS, NOAA, ARPA, OEP, and the Earth (San Fernando earthquake), as well as a group in the Nixon administration that sought to develop programs for several New Technology Opportunity initiatives.

List of Interactions (Spring 1970 - Spring 1973)

(1) In the course of the work of the OST Task Force on Earthquake Hazard Reduction chaired by engineer Karl Steinbrugge, OST first requested that the agencies (USGS, NOAA, etc.) submit reports on their capabilities relative to an earthquake hazard reduction program, in May 1970.

(2) In August 1970 the final report of the Task Force on Earthquake Hazard Reduction was released by OST (Steinbrugge, K., chairman, 1970). This report marked a turning point in the early development of the program. It provided a simple outline of "tasks" to be addressed under

an Earthquake Hazard Reduction Program that permitted various disciplines and organizations to identify suitable roles in the program for themselves, which encouraged them to cooperate in planning for joint action within a federal earthquake program.

(3) In the fall of 1970, two investigators from GAO arrived in the Bay Area and began a year-long study of the USGS and NOAA earthquake programs, their real or potential overlap, and the general question of whether a national earthquake hazards program was needed.

(4) In December 1970, OST followed up on the Steinbrugge Report with a letter from the Science Advisor, Dr. E. E. David, to the research agencies requesting information on "action underway or proposed to carry out the recommendations of the Task Force on Earthquake Hazard Reduction" within the agency. Interior responded to the David letter in early February 1971 with a document that began to spell out elements of the earthquake research program that the USGS was prepared to undertake.

(5) On February 9, 1971, a magnitude 6.7 earthquake occurred beneath the San Fernando Valley just north of Los Angeles. It killed more than 60 people and caused extensive property damage, including the collapse of two hospitals, one of modern design and construction.

(6) In April 1971 OST wrote to USGS, NOAA, and ARPA requesting that they prepare a joint budget proposal, following the Steinbrugge Report categories, for an expanded earthquake hazard reduction program.

(7) In May 1971 the USGS and NOAA jointly published "The San Fernando, California, Earthquake of February 9, 1971", USGS Professional Paper #733.

(8) In July 1971 Senator Cranston's Office initiated a months-long discussion with the USGS on the development of an extensive earthquake research program in California that would include an earthquake prediction element. The USGS response to these inquiries further spelled out its plan for an earthquake hazards program. The "final" draft of an "earthquake" bill for introduction by Senators Cranston and Hollings was completed in January 1973.

(9) From November 1971 through the end of 1972, the USGS and NOAA sought to coordinate their programs by means of a Joint USGS/NOAA Coordinating Committee for the Earthquake Hazard Reduction Program. Several topical subcommittees that were set up successfully worked out agreements for cooperation on critical issues.

(10) In the fall of 1971 the Nixon administration set out to develop several new programs that would address national problems by the application of new technologies. One of the areas addressed was natural hazards, including earthquakes. NOAA was the lead agency for the natural hazards initiative, and USGS and ARPA cooperated with NOAA in its preparation. It offered an opportunity to accelerate the development of some elements of the program suggested by the Steinbrugge Report. In December 1971 a joint budget for an accelerated earthquake program prepared by NOAA, USGS, and ARPA proposed a five-year $70 M program ($70 M was the tentative figure suggested by OMB).

(11) In the spring of 1972 GAO completed a draft of its report on the need for a federal earthquake hazards program and submitted it to the USGS and NOAA for comment. The report concluded that an expanded program was needed but pointed out several areas of overlap between the USGS and NOAA programs. The completed report was sent to OMB.

(12) In April 1972 OMB responded to GAO with discussions of a number of the recommendations made in the GAO report.

(13) In June 1972 the request for increases in the earthquake program in response to NTO prepared jointly by USGS, NOAA, and ARPA was revised. It called for a $7 M annual increase for the USGS and an equal amount to be divided between NOAA and ARPA.

(14) In June 1972 the Office of Emergency Preparedness (OEP) requested detailed information on the current and planned USGS earthquake program for its study of pre-disaster programs for earthquakes.

(15) In December 1972 OMB wrote to USGS and NOAA requesting clarification of their roles in the earthquake program and asking their responses to several possible plans for reorganization.

(16) In early 1973 OMB concluded that most of the earthquake elements of the NOAA program should be transferred to the USGS and indicated the level of program increases that would be allowed.

(17) In the spring of 1973 the Chief Geologist appointed a "Committee on Organization of the Earthquake Program", Dallas Peck chairman, to recommend how the USGS earthquake program should be reorganized to accommodate the groups that were to be acquired from NOAA. The report was submitted, with three options, on March 30.

(18) In August 1973 most earthquake-related groups in NOAA were transferred to the USGS, the headquarters of OERCS was transferred from Menlo Park to Reston, and R. M. Hamilton became Office Chief for OERCS. The earthquake program was to be carried out under informal "Branches" of Seismology and of Earthquake Tectonics in Menlo Park, of Seismic Engineering in San Francisco, and of Seismicity and Risk Analysis in Boulder.

(19) In October 1974 the reorganization plan was approved by the Department of Interior. A new Office of Earthquake Studies was established with R. M. Hamilton as its Chief, and the Branches that had operated informally since August 1973 became official. The name and concept of NCER were retired and its planning and coordinating functions were moved to OES in Reston.

The rapid development of the geophysical component of the NCER earthquake program between 1966 and 1973 carried out within OERCS transformed the USGS from a minor to a major participant in the national earthquake research effort and set the stage for its selection as the lead federal agency for earthquake research from 1973 onward. When Crustal Studies (later OERCS) moved to Menlo Park in 1966 its interests and capabilities were still focussed on long-range seismic profiling, and most of its funding was from an ARPA program that was being phased out. Its principal assets were not impressive:

(1) 20 portable "10-day-recorder" seismic stations,
(2) 10 truck-mounted low-frequency seismic refraction systems,
(3) 1 LRSM seismic recording "van", set up at Gold Hill on the San Andreas fault between Parkfield and Cholame in October, 1965, to record the "Longshot" explosion in the Aleutians,
(4) a seasoned field crew trained to carry out long-range seismic refraction profiling: permitting, contracting for drilling, loading and firing 2,000 to 10,000 pound chemical shots in boreholes or bodies of water, as well as operating the recording trucks and associated electronics,
(5) a professional staff of about 15 people that held 8 graduate or engineering degrees among them, plus another 15 people, including the field crew, made up of electronic technicians, physical science technicians, secretarial/clerical workers, and field hands,
(6) experience in recording and analyzing microearthquakes induced by fluids injected into basement rocks beneath the Rocky Mountain Arsenal in Denver.

The rapid progress made by OERCS in developing the field of microearthquake seismology over the next few years was made possible by the effective use by the field crew of the network of portable 10-day-recorder stations, supplemented by the refraction trucks, coupled with the serendipitous occurrence of the Parkfield-Cholame earthquake. That magnitude 5.5 earthquake, which occurred on June 27, 1966, during the final stage of Crustal Studies' move to Menlo Park, produced a 40-km-long surface fracture along the San Andreas fault that was nearly centered on the USGS Gold Hill station (the only USGS station in California) and passed within a few hundred yards of its recording van.

An eight station 20-km-diameter network of 10-day-recorder portable stations, centered on the southwest end of the surface break, was set up by the USGS at my suggestion to record the aftershocks. It operated from July 1 to September 15. Data from 5 similar NOAA stations along the northern half of the fault break were available for the first three weeks of July, and five additional USGS stations were operated along that section of the break from September 1 to September 15.

More than 600 aftershocks recorded on 4 to 13 stations were analyzed by hand from paper playbacks from magnetic tape. Preliminary efforts to locate these aftershocks, which employed "time zone" charts like those used by the Russian team at Garm, demonstrated that focal depths were successfully resolved by the network (Figure 1)(Eaton, in Brown, et. al., 1967). That method proved to be impractical because of the time that it required; so we turned to the computer. A computer location program (HYPOLAYR) employing Geiger's least squares method, based loosely on an earlier program by Bolt and Turcotte used at U.C. Berkeley, was developed on the Stanford IBM 7090 (Eaton, 1969). The program utilized a flat-lying constant-velocity multi-layer crustal model (with an option for separate models on the two sides of the fault) and computed earthquake magnitudes from peak wavelet amplitudes and associated periods. Separate crustal models for the NE and SW sides of the fault, based on explosion refraction profiles recorded by the Crustal Studies refraction trucks, were used to locate the Parkfield aftershocks.

The plotted epicenters lay in a narrow 2-km-wide band just southwest of the surface fracture zone (Figure 2) (Eaton, O'Neill, and Murdock, 1970). A plane fitted by least squares to the 484 best-located hypocenters (5 or more stations, standard deviation of arrival times <0.1 second, computed errors in horizontal and vertical coordinates <2.5 km) outcrops along the surface fracture zone and dips 84^0 to the southwest (Fig. 2, inset). The standard deviation of individual hypocenters from the plane was only 0.71 km (0.45 km if an additional 10 poorly fitting events are eliminated). Hypocenters were scattered, with pronounced clustering, over the fault surface and were virtually confined to the depth range 0 to 12 km (Fig. 3).

The success of this experiment depended on particularly careful installation and monitoring (daily, at first) of the 10-day tape recorder stations. John Coakley, the first new technician hired by Crustal Studies in Menlo Park, stood out for his careful work and dedication to these stations; and he remained the central figure in the installation and operation of this system for more than 20 years.

The 1966 Parkfield aftershock recording experiment and the computer programs that I developed for its analysis provided the USGS with a versatile new tool that was applied to several pressing seismological and engineering problems in addition to aftershock studies. These included induced seismicity from dams and their reservoirs, from fluid injection through deep wells, and from underground nuclear explosions.

An effective system for long-term microearthquake monitoring became available in early 1968 with the completion of an experimental telemetered 30-station short period network between Palo Alto and Hollister. In addition to telemetry, this system featured the use of Develocorders for recording the data: traces from 16 stations were recorded side-by-side on 16 mm film, which was developed automatically by a continuous process and projected onto a viewing screen only a few minutes after recording. The computer programs developed for analysis of the Parkfield data were adapted for use with the telemetered network. Within a few months it was apparent that results from the telemetered network were comparable to those from the portable network and that data analysis on the Develocorders was far simpler than from magnetic tape (Eaton, Lee, and Pakiser, 1970). Earthquakes on the San Andreas, Sargent, and Calaveras faults from March 1968 through April 1969 were clearly resolved by the experimental telemetered network (Fig. 4). Cross sections of events on the Sargent fault (Fig. 5) show the distribution of these events with depth and position on the fault.

From 1967 through 1973 the portable net was put to frequent use in additional studies of aftershocks (Bear Valley, 1967; Borrego Springs, 1968; Santa Rosa, 1969; Danville, 1970; San Fernando, 1971), induced earthquakes from underground nuclear explosions (Benham, 1968; Rulison, 1969), and fluid injection (Rangely, 1967; Geysers, 1971), as well as half-a-dozen reconnaissance studies of background seismicity in Alaska, the Cascades, Imperial Valley, etc. Much of this work was supported by funding from AEC, ARPA, and specialized USGS sources such as the geothermal program.

Beginning in 1969 the short-term portable network experiments were augmented by long-term telemetered seismic environmental networks that were deployed for similar purposes and were supported by the same outside sources: NTS(1969) and Hanford(1969), AEC; Santa Barbara (1969), USGS Conservation Division; Rangely (1970) and Los Angeles Basin (1971), ARPA; New Melones Dam (1972), COE. During this same period the new equipment began to flow to Hawaii to upgrade the HVO network: Develocorders, vhf radio telemetry, and compact seismometers (L4-C and EV-17). The central California telemetered network was also extended northwestward to Santa Rosa and southeastward toward Parkfield. Significant support for the expansion of the central California network and further development of its equipment and analytical facilities was provided by AEC (DRDT) and HUD.

During these years, a strong effort was made to broaden and deepen the OERCS earthquake program through aggressive but selective recruiting of new staff. At the time of the move to Menlo Park, the OERCS staff of about 30 people included only two members with PhD's (one earthquake seismologist and one refraction seismologist), 6 with MS's or equivalent engineering degrees, and 3 with BS's. By 1972 the OERCS staff had grown to nearly 120 people and included 25 members with PhD's, 21 with MS's, and 19 with BS's. Fields of specialization covered by the PhD's included microearthquake seismology (6), engineering seismology (3), refraction seismology (3), theoretical seismology (3), crustal deformation (2), computer modelling (1), rock mechanics (4), geothermal studies (2), and volcano geophysics (1). About one fourth of this build-up was accomplished by transfer of personnel (with most of their funding) within the USGS. The rest were new hires and were funded mostly by the external sources noted above.

In 1969 the USGS submitted an unsolicited proposal to ARPA on the topic of earthquake control (Eaton, Raleigh, Wallace, and Pakiser, 1969). After some deliberation ARPA responded with support for a more limited program focussed on earthquakes induced by fluid injection. A joint USGS/ARPA Fluid Injection Program managed by the USGS and funded by ARPA at $2M per year began in 1970. Less than half of the work was done by the USGS, the rest was accomplished through research contracts administered by the Survey under Barry Raleigh's leadership.

A summary of the OERCS budget, by source, for FY 1971 shows the importance of outside funding for the earthquake program during the early 1970's.

SIR "base"	957K	
SIR (OST, Seis Haz, TAPS, WRD)	272K	
		1229K total
AEC (NTS, Hanford, DRDT)	305K	
		305K total
HUD (NCSN augmentation)	20K	
ONR (Geothermal)	28K	
COE (Childress Co. FI)	24K	
NASA (Satellite Volcano Net)	54K	
AFCRL (Fault Zone Tectonics)	1K	
		177K total
ARPA LASA (carryover)	11K	
" Rangely "	82K	
" FI (Rangely pumping)	85K	
" FI (Libby Seis Net)	56K	
" FI (Reservoir strain)	25K	
" FI (Lab Eq Generation)	62K	
" FI (Downhole tilt)	54K	
" FI (Portable seismic arrays)	155K	
" FI (Automatic Eq Processing)	192K	
" FI (Computer modelling)	51K	
		773K total
		2484K total OERCS program
" FI (Contracts)	1300K	(Administered by USGS)
		1300K total

Thus, in FY 1971 half of OERCS's internal program was supported by outside funding; and much of that support was for the application or further development of the microearthquake expertise that the USGS acquired in its first three years in Menlo Park. The ARPA sponsored Fluid Injection Program was particularly important for sustaining the continued development of the OERCS microearthquake program in FY's 1970 - 1972, and it initiated the external grants and contracts program that has been so important for engaging the participation of university and industry scientists in the broader earthquake hazards program of later years.

From the summer of 1966 to the summer of 1973, NCER provided an umbrella organization under which the geological and geophysical elements of the USGS earthquake program were developed independently through the efforts of their participants. Although there was little programmatic interplay between the two groups, each found synergistic advantage in their association within NCER in its search for support from other agencies. Such clearly was the case for the geophysics group (OERCS) in preparing the Earthquake Control proposal to ARPA that secured support for the Fluid Injection program. The same was true for the geology group in securing support from HUD for the cooperative USGS/HUD "San Francisco Bay Region Environment and Resources Planning Study". That 3-year program (1970 - 1972) focussed ongoing USGS earthquake hazards work in the nine Bay Area counties, provided some additional USGS effort by reprogramming, and secured funding from HUD for planning and "product development". The 3-year in-kind contribution of the USGS was about 4.5 M and the 3-year HUD contribution was about 1.5 M. The pathfinding Bay Area Study set new standards for interpretive products designed to support urban planning and geologic hazard mitigation at the level of local government and the technical consultant sector.

Such success did not extend to joint planning and budgeting for an expanded USGS earthquake hazards program. In 1968 the NCER Executive Committee was unable to write a unified "resource" paper to aid in the preparation of a USGS position paper on Earthquakes and Related Geological Hazards. Instead, the participants from geology and geophysics each prepared separate reports of similar overall content but different balance among program elements that cast the author's group in the leadership role. This impasse resulted from underlying tensions regarding the role of geophysics in the Geological Survey and the manner in which a large mission-oriented project in the Survey could best be organized and managed. The NCER Executive Committee was then reconstituted, but it met infrequently and had little influence; and no further meetings of the NCER Advisory Panel were held for several years.

In December 1970 Bob Wallace, in his role as Regional Geologist for the Western Region, took the lead in preparing a response to OST's request for information on "action underway or proposed to carry out the recommendations of the Task Force on Earthquake Hazard Reduction" in the USGS. He asked me, in my role as Chief of OERCS, to provide material covering OERCS's work and plans. As requested by OST, elements of the USGS program were described in terms of the Steinbrugge Report "tasks". Several months later, after the San Fernando earthquake, Bob and I met informally at the Burbank airport while returning to Menlo Park after attending a Senate Committee hearing on governmental response to the earthquake. Sensing gathering momentum for a possible federal earthquake research program, we agreed that it was imperative for the USGS to resolve its internal problems and press forward with plans for an aggressive earthquake program. We then wrote a joint letter to the Chief Geologist, Vince McKelvey, urging that both the NCER Executive Committee and the NCER Advisory Panel be revived; and he took action to do so.

Over the next two years several versions of an expanded earthquake hazards program were prepared by the USGS in combination with NOAA and ARPA in response to requests from OST and OMB. These programs and their budgets were developed around the Steinbrugge Report "tasks", which proved to be helpful in describing the content and balance of program elements in the USGS part of the overall program as well as between the USGS program and those of NOAA and ARPA. The struggle for dominance in the USGS program continued, but the arguments shifted to the relative importance of tasks with short-term versus intermediate- or long-term benefits. Resolution of the leadership role was ultimately forced by the transfer of NOAA's earthquake research groups to the USGS and by the Survey's designation as the lead federal agency for earthquake research, which led to the creation of the Office of Earthquake Studies, with geophysicist Bob Hamilton as its Chief, as a subdivision of Geologic Division in October 1974. Unfortunately, most of the geologists working on earthquake problems chose to remain with their old Branches.

Menlo Park (post August 1973)

With the move of OERCS headquarters from Menlo Park to Reston in August 1973, the atmosphere within the earthquake program began to change. After the addition of NOAA personnel and the formal reorganization of OERCS into OES and its four Branches, the close ties between individual project personnel and the Office Chief and his deputies began to weaken. New responsibilities for the expanded earthquake program and new interests and allegiances aroused by working in the USGS Headquarters environment drew "management's" attention away from the work of the program toward the mechanics of its management and funding. Moreover, the division of the program into Branches, some staffed predominantly from the recently "captured" NOAA program and headquartered at a distance from the original USGS core group in Menlo Park, led to separate Branch loyalties and inter-Branch competition for status and funding that impeded a further balanced development of the overall earthquake program. Although these changes, perhaps, were an unavoidable consequence of growth, they began the drift from "leadership" to "management" that has steadily increased the level of bureaucracy in the planning and implementation of the program and stifled spontaneity and innovation. On the other hand, development of strong earthquake program leadership at the USGS Headquarters in Reston enhanced the Survey's ability to further program interests at the national level and to secure support for the program in Congress, academia, and industry.

The effective size of the funding increase that accompanied the 1973 reorganization is somewhat obscured by the complexity of the reorganization. Most of the NOAA earthquake program was transferred to the USGS along with part of the funding that supported it in NOAA; and "outside" funding associated with the transferred units generally followed them to the USGS. Program increases worked out for the USGS and NOAA before the merger were the basis for the increases to the USGS after the merger. Part of that increase ($4 M) was made available in FY 1973, which was two thirds over at the time of the merger, and the full amount ($10 M) was expected for FY 1974. At least half of the NOAA personnel transferred were located in Boulder, Albuquerque, and Las Vegas and worked with the global network, NEIS, or Special Projects at NTS - not with the earthquake hazards program. Moreover, in the process of reorganization, a number of "orphan" projects in the Survey whose work was judged to be supportive of the earthquake program were transferred into OES without a commensurate transfer of funds. These projects did broaden the scope of the earthquake program, however. Some of the increase was also required to support critical parts of the ongoing program that had been developed on temporary outside funding. The level of new funding available for program augmentation was probably about 5 million dollars.

In the early years of the USGS earthquake program the emphasis had been on the development and evaluation of instruments and techniques to address a broad range of earthquake and related geologic hazard issues. Application of such techniques had been severely limited by the lack of funding and the need to be responsive to the interests of the agencies funding the work. The 1973 funding increase permitted the projects to expand their work and to focus more directly on the broad earthquake hazard problem along the San Andreas fault as well as in other seismogenic tectonic environments across the country.

To secure participation in the program of a broad range of professionals outside the Survey, a Grants and Contracts Program was funded at $2 M per year. It included $700 K for sole-source geological and geophysical studies and $1300 K for grants and contracts to universities, state organizations, and private industry awarded on an open proposal basis. A non-Survey Review Panel was set up to advise the Survey on the merit of the proposals.

After the 1973 changes were fully implemented it was clear that funding levels were adequate only for research on methods of earthquake hazard reduction, not for implementation of an earthquake hazard reduction program. Moreover, the USGS program did not include earthquake engineering, which was carried out primarily by universities and the private sector. These problems were addressed by OST, in collaboration with NSF and the USGS, in 1976. A new document, "Earthquake Prediction and Hazard Mitigation Options for USGS and NSF Programs (Sept. 1976)", was prepared by USGS and NSF in conjunction with the Science Advisor's Advisory Group on Earthquake Prediction and Hazard Mitigation. This document has become known as the Newmark Report (engineer Nathan Newmark was the chairman of the Advisory Group). The agency staff members that prepared the working papers behind this report were Charles Thiel and Roy Hanson for NSF and Robert Hamilton and Robert Wesson for the USGS.

The Advisory Panel drew its members from a wide range of professions: Seismology (5), Engineering (5), Building Officials (4), Geology (2), Geophysics (1), Architecture (1), Economics

(1), Sociology (1), and Emergency Services (1). Except for its Executive Secretary (from NSF), none of its members was from a federal agency. Panel members represented universities (12), state and local government (5), and private industry (4). Input from the federal agencies was through the working papers that the NSF and UGSG prepared for the Panel's consideration.

It divided responsibility for most aspects of the overall program between NSF and the USGS but shared responsibility for Fundamental Earthquake Studies between them. Exclusively NSF elements were Engineering and Research Utilization. Exclusively USGS elements were Prediction, Induced Seismicity, and Hazards Assessment. The Report proposed a sharply increased Earthquake Hazards Reduction Program to be carried out jointly by NSF and USGS. The suggested "intermediate" funding option for the combined program increased from $20.7 M in FY 1977 to $53.8 M in FY 1978, to $70.2 M in FY 1979, and to $85.0 M in FY 1980; and the separate NSF and USGS suggested funding levels were nearly equal.

The Newmark Report combined the interests of the earth sciences and engineering in a single balanced earthquake program for the first time and provided a consensus program that could be recommended to the Congress for funding.

Growing Congressional interest in earthquake-related research dating back to 1971 culminated in the 95th Congress with the introduction of bills in both Houses of Congress. After holding hearings and receiving Executive Department suggestions for modifications, Congress enacted the "Earthquake Hazards Reduction Act of 1977" (P.L.95-124). On October 7, 1977 the act was approved by President Carter, who assigned the responsibility for preparing an implementation plan to the Office of Science and Technology Policy, then directed by Frank Press. OSTP formed a 9-member Working Group on Earthquake Hazards Reduction with engineer Karl Steinbrugge as chairman. Bob Wallace and Don Nichols of the USGS served as members of the working group. The Working Group was supported by a 3-member Steering Group (from NSF, USGS, and OSTP) and a 12-member Advisory Group with a wide range of specialties drawn from universities (8), private industry (2), county government (1), and the Academy of Sciences (1).

The report of the Working Group, "Earthquake Hazards Reduction: Issues for an Implementation Plan", which was published in 1978, does not offer a blueprint for specific actions to be taken to implement the program. Rather, it identifies problem areas, or "Issues", and explores the nature and interrelations among these issues, which must be addressed by the operating agencies in carrying out their parts of the Earthquake Hazards Reduction Program.

The issues identified and discussed in the report include: (1) Earthquake Prediction and Warning; (2) Earthquake Hazards Reduction Through Construction Programs; (3) Private and Public Financial Institutions; (4) Land-Use Planning and its Implementation; (5) Communication and Education; (6) Roles of Groups and Organizations - Government, Volunteer Organizations, Research Community, and Scientific and Professional Organizations.

One specific recommendation under "Earthquake Prediction and Warning" that was of great significance to the USGS was:

"The membership of the USGS Earthquake Prediction Council should be formally expanded to include non-governmental scientists so that the panel can be free of conflicts of interest, imagined or real, and can provide broad-based objective scientific evaluations. The Council should become the National Earthquake Prediction Evaluation Council".

The USGS Earthquake Prediction Council, which originally included only USGS scientists, was established after the 1973 "NTO" funding increases to provide the Director with technical information and evaluation of results that might warrant issuance of a prediction. In time, NEPEC also came to fill a role similar to that of the NCER Earthquake Advisory Panel, which had lapsed after the formation of the Office of Earthquake Studies in 1974.

Enactment of the "1977 Act" and subsequent appropriation bills put the USGS earthquake program on a firm financial footing, but it further altered the relationship between the Survey groups carrying out the USGS part of the program and their OES leadership in "Washington". Before the 1973 reorganization, the status and survival of the USGS program hinged on the research accomplishments of the core NCER group in Menlo Park; and our Office-level managers essentially "worked for us" and were familiar with details of the work in progress. After the 1977 Act, OES Office-level managers were responsible for a broad research effort that was divided among several specialized Branches in the USGS and the External Grants Program that supported work in universities, State government, etc. The core (ex-NCER) group in Menlo Park became only one of many USGS and non-USGS groups "working for" the OES managers. After 1977, Program Managers as well as Branch Chiefs stood between individual research scientists and Office-level managers. The interplay between individual researchers and top-level program leaders became steadily more bureaucratic, with the introduction of "panels" to evaluate project accomplishments and proposals for new work (and funding). This procedure has impeded fulfillment of long-term program goals and has favored quick approval of projects to pursue a succession of short-term enthusiasms (fads) that have swept the research community (such as the dilatancy-diffusion hypothesis and the search for changes in the ratio of P- and S-wave velocities). The long-term group effort to build program infrastructure and to develop improved equipment and techniques to capture the ephemeral signals that are the foundation for our studies has been subordinated to the cacophony of demands for support of private research interests. Moreover, the status and progress of individual scientists has been increasingly evaluated on the basis of the number of their published papers in refereed journals rather than on the relevance of those papers to the earthquake program and their contribution to the success of the program as a whole.

Other factors have diminished the impact of the voices of USGS scientists on the formulation of program goals and strategies at the Office level. By law, working-level members of the federal agencies must be very circumspect in any action to secure support for their agency's programs; i.e. they cannot "lobby" Congress. On the other hand, "outside" participants (or potential participants) in the earthquake program from universities, other levels of government, and private organizations are not so restrained. The potential direct political support that non-USGS friends of the earthquake program can bring to the program in Congress raises their influence on USGS program managers far above that of Survey scientists whose voices are muted and who can support the program only through the quality of their work. In addition, the time-honored practice of "rotating" working scientists into "Washington" to fill upper level administrative posts for several years and then returning them to project work broke down when Bay Area housing costs skyrocketed in the 1970's. This problem also led to filling "deputy" posts with people from outside the earthquake program, or even outside the Survey. In combination, these problems led both to the formation of a semi-permanent management group with steadily diminishing knowledge of and interest in the actual USGS work under the earthquake program and to further alienation of workers from managers (and vice versa).

II. Development of the instrumental and analytical foundations of the earthquake program

A. Hawaii September 1953 - October 1961

The USGS's introduction to microearthquake seismology was the outgrowth of an effort to modernize the seismic stations operated by its Hawaiian Volcano Observatory to study the volcanoes Kilauea and Maunla Loa in Hawaii. Seismic observations at Kilauea were initiated with a pair of state-of-the-art Bosch-Omori seismographs soon after the Observatory was established in 1912; and seismometers of similar design built at HVO were installed at several additional locations in Hawaii during the next several decades. Isolation, the Great Depression, and World War II blocked improvement of the Hawaiian stations until the early 1950's. Efforts by geologist Gordon Macdonald to upgrade seismology at HVO that began in 1950 resulted in my being hired as the HVO seismologist in the fall of 1953. Fresh from completing a thesis on The Theory of the Electromagnetic Seismograph (Eaton, 1957; Eaton and Byerly, 1957) at U.C. Berkeley, I arrived at HVO to find that a seismic network improvement program was already underway; but it was based on a low-sensitivity 1920's-era mechanical seismograph, called the Loucks-Omori, that was being built in the HVO shop by Burt Loucks and John Forbes. The projected network would be composed of five stations around the perimeter of Hawaii Island (which is about 70 km across) plus three more on and around the summit of Kilauea. Only the three summit stations were tied to a common time base, by means of ground wires between them.

I purchased pendulum clocks and short-wave radios (for WWVH) for the stations and designed an electronic device that permitted the WWVH time signal to be recorded on the seismograph drum when the records were changed (Eaton, 1954, Volcano Letter #525). With these improvements, the relative timing between stations was nominally good to about 0.2 second, but maintaining good clock corrections was always a problem. Even with improved time control, the mechanical seismographs of the 1954/1955 Hawaiian seismic net clearly were inadequate for monitoring earthquakes beneath the volcanoes. Because of their low sensitivity (magnification about 200) these seismographs generally did not record earthquakes smaller than M3 to M3.5 with sufficient clarity at enough stations for hypocentral determinations.

The network also included one optically recorded short-period vertical Sprengnether seismograph, with seismometer and galvanometer periods of 0.5 and 1.5 seconds, respectively, and a peak magnification of about 1750 at a period of about 0.5 second. This instrument could not be operated at higher magnification because of the high oceanic microseismic background in the 1 to 8 second range in Hawaii.

To study the earthquake-signal to background-noise-level problem further, three experimental seismographs were set up in a basement darkroom in my quarters at the summit of Kilauea. The first was a standard Wood-Anderson horizontal component torsion seismometer (period 0.8 sec., damping 0.8 critical, and static magnification 2800). Although it recorded optically, it was a mechanical seismograph like the Loucks-Omori, but with a much shorter free period (0.8 vs 3.0 seconds). The other two seismometers had been manufactured to be short-period, horizontal component, electromagnetic instruments; but I modified one to have a high peak magnification at a very short period (about 10,000 at 0.25 second) and the other to have only moderate peak magnification at a much longer period (about 500 at 10 seconds). These instruments were operated during the 1955 east rift zone Kilauea eruption and the ensuing "collapse" of the Kilauea summit.

This experiment showed that the signal-to-noise ratios for small earthquakes were much higher for periods shorter than 0.5 seconds than for the longer periods favored by the Loucks-Omori. It also showed that the teleseismic signal-to-noise ratios were much higher for periods greater than 5 to 8 seconds than for the shorter periods favored by the Bosch-Omori seismograph (free period about 7.5 seconds). The foregoing observations provided the basis for drawing a magnification curve for an instrument suitable for recording small earthquakes in Hawaii; and they also suggested that the long-period Press-Ewing seismograph could be used for teleseismic recording in Hawaii.

The first instrument designed to have the desired magnification curve was an optically recorded vertical component electromagnetic seismograph with strong coupling ($s^2=0.25$) in which the galvanometer and seismometer had equal free periods and damping (0.5 second and 2.0 critical, respectively). It had a peak magnification of about 25,000 at a period of about 0.2 seconds. When operated in the same vault as the original Sprengnether vertical, it recorded between 5 and 10 times as many small earthquakes as the Sprengnether. This seismograph was called the HVO-1.

The second, and more significant, seismograph that was built for the new HVO net arose almost accidentally. HVO badly needed a sensitive seismograph with "visible" (i.e., non-photographic) recording to monitor seismicity associated with eruptions. Such recorders then in use were designed to record the long-period waves from teleseisms; and they employed unstable photo-tube amplifiers to provide sufficient amplification and power to drive a pen recorder from the very weak signal produced by an electromagnetic seismometer. HVO had experimented with a more modern "Brush" strip-chart recorder, driven by a piezo-electric accelerometer amplified by a sturdy "penmotor" amplifier, as a portable seismograph. That system was far too insensitive in the 1 to 10 hz frequency range to be useful, however. When the accelerometer was replaced by an HVO-built high-output 1-hz moving-coil vertical-component seismometer and a battery-powered x100-magnification vacuum-tube preamplifier, the overall system sensitivity was quite adequate. The strip-chart recorder was not suitable for continuous recording, however; so the penmotor was fitted with a very light 6-inch-long pen tipped with a fine-wire recording stylus and mounted upside down above a standard HVO smoked paper drum with a paper speed of 1 mm per second. A 6 db/octave 10 hz high-cut filter was placed between the seismometer and the preamp and a frequency-adjustable 6 db/octave high-cut filter was placed between the preamp and the penmotor amplifier. A small transformer in the signal path between the filter and the penmotor amplifier served to couple time marks into the record.

The HVO Library, where the recorder was located, turned out to be far too noisy for a sensitive seismograph; so the seismometer and preamp were moved about 3 km south of HVO (at Uwekahuna on the northwest rim of Kilauea Caldera) to the newly built Outlet tiltmeter vault (at the southwest corner of Kilauea Caldera), and the signal was brought back to HVO via a ground-laid "combat wire" phone line. The results were encouraging. When the adjustable filter was set at 3 to 5 hz, the response of the system was very similar to that of the short-period electromagnetic seismograph designed for use in Hawaii. At first this seismograph was not taken very seriously because it included vacuum-tube amplifiers, which are prone to vary as the tubes age. It was soon found to be indispensable for timely surveillance of small earthquakes within Kilauea, however.

With some further work, the preamplifier was improved and repackaged for use in a damp environment (inescapable in Hawaii), and the seismometer was equipped with a "weight-lift" calibrator that was activated remotely from HVO so its sensitivity could be monitored and adjusted. The free period of the seismometer was also reduced to 0.8 sec. to permit higher peak magnification (about 40,000 at 0.2 sec.) without an excessive oceanic microseism background (Fig. 6). The developmental telemetered seismometer, called the HVO-2, was operated in the Outlet vault through the latter part of 1956 and early 1957.

In the spring of 1957 the time-signal line from Uwekahuna to the Loucks-Omori station on Mauna Loa failed, and the mechanical seismograph was replaced by a telemetered HVO-2 seismograph in April 1957. In the summer of 1957 the developmental HVO-2 in Outlet was replaced by a new "standard" model; and in the fall of 1957 a third HVO-2 station, Desert, was installed about 10 km southwest of Uwekahuna. A new 3-drum rim-drive smoked paper recorder designed by experimental geologist Chester Wentworth was built in the HVO shop for the three station telemetered network to improve the relative timing between stations. It was installed beneath the windows in the HVO Library that overlooked Kilauea Caldera so that Park visitors to the volcano could watch the seismograph drum from outside the Observatory. It soon became a popular exhibit. In the summer of 1958 a fourth HVO-2 station was installed at the northeast rim of Halemaumau near the center of Kilauea Caldera (North Pit).

Because the HVO-2 seismograph employed a very primitive telemetry system - i.e. direct transmittal of the amplified seismometer signal via a "hard" telephone line to the Observatory - its use was restricted to a small network inside the Park. The HVO-2 net described above formed a triangle, about 15 km on a side, with its southeast vertex (Outlet) just south of Kilauea Caldera above the zone that supplies magma to the volcano.

To improve the network outside the Park, five photographically recorded HVO-1 seismographs were built in the HVO shop by Burt Loucks and John Forbes. They were installed at HVO (Uwekahuna, April 1957), Haleakala, Maui (May 1957), Barbers Point, Oahu (summer, 1957), Hilo (October 1958), and Pahoa (January, 1960). The stations in Hilo, about 40 km from the Kilauea summit, and Haleakala, about 180 km from the Kilauea summit, were also equipped with two standard horizontal-component Wood-Anderson seismographs to facilitate calculating earthquake magnitudes.

From the earliest days of the Observatory, HVO workers had attempted to build tiltmeters for use around Kilauea Caldera. The Bosch-Omori horizontal component seismographs in the Whitney vault were very sensitive to ground tilting, and they showed an annual tilt cycle with an amplitude of about 40 micro-radians along with occasional large non-periodic excursions that correlated with major volcanic events at Kilauea. Simple tiltmeters built at HVO during the 1920's for use around Halemaumau had not been very successful, and the two that were still in operation in the early 1950's produced ambiguous records. The large "collapse" of the Kilauea summit that accompanied the 1955 Kilauea east rift eruption rekindled interest in tiltmeters. To avoid the many problems associated with horizontal pendulum tiltmeters we turned to a water-tube tiltmeter based on a Japanese design. Short-base water-tube tiltmeters built at HVO were installed in the vaults at Whitney (Bosch-Omori vault), Uwekahuna, and Outlet. The Whitney water-tube tiltmeter closely tracked the record of the Bosch-Omori beside it (Fig. 7A).

Because of the inconvenience and expense of constructing underground vaults for tiltmeters, we decided to apply the water-tube tiltmeter in a new configuration. Permanent piers topped by brass "hubs" to which the micrometer "pots" were clamped were emplaced at the ground surface at the vertices of an equilateral triangle about 50 meters on a side. Two micrometer pots connected by a water tube and a vapor shunt were clamped to the piers on one side of the triangle. Following a carefully worked out procedure, the system was filled with water through a "tee" at the center of the water tube, allowed to drain down to ports that established the water level in the pots, and closed and left undisturbed for 90 seconds; and then the heights of the water surfaces in the two micrometer pots were determined by adjusting the micrometers so that their upper pointed ends just touched the water surfaces from below. The micrometer point was illuminated by a light behind a frosted window in one side of the pot and viewed through a magnifying lens in the other side of the pot. The micrometer pots were then reversed (on the same side of the triangle) and the process repeated. From the four height measurements obtained, the differences in height of the two piers as well as the differences in the zero setting of the two micrometers are determined by simple algebra. The same procedure was then used to measure the differences in pier heights on the other two sides of the triangle. Having "leveled" around the triangle, the height of the first pier could then be compared with itself, i.e. a "closure" error could be determined to check the accuracy of the work. After a suitable interval (one week to several months), the entire process was repeated. Changes in relative heights of the piers then revealed any tilting of the lines between the piers that occurred during the interval between measurements. The tilt measurements were carried out at night, frequently in the rain, to avoid the disturbing effect of temperature variations along the water tube connecting the pots.

The performance of this system exceeded all expectations. By early 1959 half-a-dozen "tilt bases" around the summit of Kilauea were tracking the slow swelling of the volcano that preceded the 1959/1960 eruption. The average tilting "noise" signal at these tilt bases was less than one micro-radian per month (Fig. 7B)(J.P. Eaton, 1959).

The development and successful application of the water-tube tiltmeter was made possible by the enthusiastic cooperation of the whole HVO staff. Chester Wentworth experimentally validated the basic concept; Burt Loucks fabricated the micrometer pots and supervised the installation of the instrument piers; and Harold Krivoy, Burt Loucks, John Forbes, Bill Francis, Don Richter, and Akira Yamamoto joined with me to carry out measurements on many long nights after a full day's work at HVO.

During the critical period of the build-up to the 1959-1960 eruption of Kilauea, the HVO seismic network consisted of the four telemetered HVO-2 summit stations plus the HVO-1 at Uwekahuna, all on the same clock, and the HVO-1 stations in Hilo, Haleakala, and Barber's Point. A somewhat more compact HVO-2 system employing an ink strip-chart recorder served as a portable instrument for "roving" field investigations. Loucks-Omori seismographs continued to operate at Pahoa (until January, 1960), Naalehu, and Kamuela; and an older "Hawaiian Type" seismograph operated sporadically in Kona.

The high-gain vertical component network virtually revolutionized our understanding of the seismicity of Kilauea volcano, particularly of its summit region. The number of earthquakes recorded increased nearly 100 times, and it became possible to resolve them into "families" on the basis of epicenter, depth, temporal pattern of occurrence, etc., and to associate them with the primary structures of the volcano: summit, rift zones, south flank, Kaoiki fault zone, etc. In conjunction with the history of swelling and collapse of the Kilauea summit provided by the tilt bases around the summit, the improved seismic record began to clarify the relationship between seismicity and the accumulation of magma in the summit reservoir and its subsequent discharge at the summit or through the rift zones (Figs. 8 and 9)(Eaton and Murata, 1960; Eaton, 1962).

The HVO-1 and HVO-2 seismographs in Hawaii also record the water-borne sound (T phase) associated with large earthquakes beneath or near the Pacific Ocean with great regularity. The most interesting of these was produced by the great May 22, 1960 (UT) Chilean earthquake. Comparison of the T-phase envelopes of four large foreshocks with that of the main shock provided evidence on the duration and variation of intensity with time of the faulting that produced the main earthquake (Fig. 10A). The transit times of the T phase and tsunami from Chile to Hawaii were about 2 hours and 15 hours, respectively. The action of the tsunami at the mouth of the Wailuku River in Hilo Bay was documented by direct measurement by the HVO team (Fig. 10B).

In spite of the simplification of a common time base for the telemetered summit network, the problem of processing the large number of earthquakes recorded at Kilauea was becoming intractable. Moreover, the hard-wire telemetry that was feasible in the Park simply could not be extended beyond the Park into private lands. Clearly, new technologies for more versatile telemetry and more efficient data processing would be needed before the telemetered HVO net could be extended significantly. Such innovations would have to await the earthquake prediction program in California about a decade later.

B. Denver October 1961 - September 1965

The Crustal Studies Branch was established by the USGS in 1960, with Lou Pakiser as its Chief, and headquarters in Denver, Colorado, to carry out long-range seismic refraction profiling of the earth's crust with support from ARPA under the VELA Uniform program. When my tour of duty in Hawaii drew to a close, I joined Crustal Studies in October 1961 to participate in its crustal refraction work and to broaden its expertise to include earthquake seismology. My first effort, with Wayne Jackson, was to analyze the operating characteristics of the newly-acquired exploration-type seismic system that it employed: low-frequency "prospecting" geophones and transistorized electronic signal processing and recording system (with multichannel oscillograph and magnetic tape recording). To learn the refraction craft, I also undertook the analysis of the 1961 profile between San Francisco and Eureka, Nevada, and the 1962 profile that ran longitudinally through the Sierra Nevada from Shasta Lake to China Lake (Eaton, 1963;

Eaton, 1966; Bateman and Eaton, 1967).

From its inception through 1962, virtually all of Crustal Studies effort was devoted to refraction profiling, and its funding was almost all from ARPA. By 1963 it was apparent that the refraction program would be phased out and the ARPA support withdrawn over the next few years; so my attention shifted to the problem of earthquakes in the Rocky Mountain region. The most active area was in the northern Rockies, particularly around Yellowstone National Park which has extensive geothermal activity and had been shaken strongly by the 1959 Hebgen Lake earthquake at the western edge of the Park. To establish a low-cost, low-maintenance network in the Park, I designed a high-gain electromagnetic seismograph with somewhat greater bandwidth than the HVO-1 that employed commercial 1-hz EV-17 seismometers and 5-hz Geotech galvanometers. The system was recorded on the newly developed Sprengnether "Autocorder", which was designed to run for as long as one month without attendance. It automatically changed its own paper record once per day, by means of photo-paper supply and take-up reels inside the drum, and reset the drum to the starting point for the next day's recording when the drum tripped a micro-switch at the end of its longitudinal range of travel. A WWV radio for receiving time signals and a crystal-controlled clock completed the station.

Three such stations with vertical component seismometers were set up in remote regions of the Park and a three-component station with a conventional photographic drum recorder was set up at Park Headquarters (Mammoth) by Bob Munson in 1963 and 1964. Park Naturalists and Rangers provided necessary servicing and mailed the records to Crustal Studies in Denver for developing and analysis by Mitch Pitt. A fifth station that employed a self-contained short-period seismograph in an "LRSM Van" was set up subsequently at the western edge of the Park.

This equipment was more troublesome to maintain than anticipated, but it did initiate the modern Yellowstone network and served until the next generation of seismographs developed in Menlo Park for the California networks became available to replace it.

Although Denver seemed an inauspicious home base from which to launch an earthquake program, several tasks undertaken there helped to shape the nature and focus of the program that would develop later. One of these was the seismic review of the P G and E Bodega Bay nuclear power plant site. In 1963 I was asked to write a seismic hazard supplement to the geological report on the site prepared by Survey geologists Schlocker, Bonilla, and Clebsch (Eaton,1963). The issue of a nuclear power plant at Bodega Head was very controversial and had deeply divided not only the public but also the engineering, geological, and seismological communities. After reading the "file" containing previous reports on the issue as well as news articles covering the developing controversy, I was appalled by the lack of any substantial body of fact to support any firm conclusion. In spite of the lack of an adequate basis for such conclusions, seismology and geology were expected to produce realistic appraisals of the hazard the nearby San Andreas fault posed to the proposed reactor at the site. Expert consultants were not shy about making impassioned arguments for or against the project (the paid consultants, of course, reflected the views and perceived interests of their employers). As a seismologist I was deeply troubled and embarrassed by the inadequacy of my science and was convinced of the need for extensive, detailed fundamental studies of the structures and processes that generate earthquakes as well as fuller documentation and analysis of their effects.

On another occasion I was asked to help establish a seismic network around Irazu volcano in Costa Rica after its 1963 eruption. Based on my experience in Hawaii and with the Yellowstone network, I suggested that several stations employing the seismic equipment designed for Yellowstone, but with conventional photographic recorders, be used. A Costa Rican seismologist was brought for training to HVO for several months and to Denver for one month. Eventually the stations were built and I travelled to Costa Rica to help install the equipment. In spite of high initial hopes, the system was totally ineffective. At that time the infrastructure to support such equipment did not exist in Costa Rica; and funding for essential supplies (including photo paper for the recorders) was unavailable. This experience showed that a seismic network must be designed with allowance for the capabilities of its users as well as the characteristics of the earthquakes it is to record.

Because of my participation in the USGS crustal refraction program I was asked by AGI to serve as technical editor for a translation of the Russian book "Deep Seismic Sounding", 1962, a collection of papers on the instruments, methods, and results of a broad, energetic Russian crustal refraction program that preceded our own work. The commercial translation was very poor: the translator wished to avoid a "literal" translation of these very technical papers and frequently missed the whole point of the Russian text. Many months later, after substantial retranslation from the original Russian text, the work was finished [Deep Seismic Sounding (Translation), International Geological Review - Book Section, vol.10, nos. 1-6, pp 1-369, Jan.-June 1968]. In the process I had become quite comfortable reading Russian (with a dictionary in constant use) and moved on to publications of the Institute of Physics of the Earth in Moscow on the Russian earthquake studies in central Asia. The most exciting volume, Bulletin 9, described the work of the Tadjikistan Complex Seismological Expedition headquartered at Garm. It described the instruments, methods, and results of the early phases of the Russian earthquake prediction program and anticipated work that would be undertaken in California about ten years later.

In 1964/1965 Crustal Studies developed a compact, low-power, self-contained portable seismic station under the leadership of Don Hoover that employed a very low speed fm tape recorder (PI 5100 "10-day" recorder) for joint use in crustal refraction and earthquake studies. It was essentially a back-pack-portable, low-power version of the system used successfully in the refraction program. The 7-track recorder accommodated 5 seismic channels, a chronometer-generated IRIG-H channel, and a radio time code channel (WWVB). Amplitude modulation of the precise 60 kilohertz WWVB carrier by a digital 1 pulse per second time code that repeats every minute was implemented through Don Hoover's interaction with the NBS staff when WWVB was being prepared for operation. This widely recordable precise time signal has been of tremendous importance to seismology. The 10-day-recorder seismic station employed a 1-hz moving-coil seismometer and had an electronic pass-band of about 1 hz to 17 hz. In the initial purchase (underwritten by ARPA) Crustal Studies acquired 20 units, and another 20 units were distributed among several universities and other federal agencies. These recorders supplemented the refraction trucks on profiles executed in the summer of 1965. The system's evaluation as a portable earthquake network was carried out, first, in Yellowstone in late 1965, and, then, in western Nevada in early 1966. The Nevada experiment was invaluable for learning how to use the 10-day stations in a prolonged experiment, but the seismic data set was of little value because the stations were too widely dispersed and earthquakes were missed when recorders failed to operate the expected 10 days. Changes in network configuration and operating procedures based on this experience laid the groundwork for successful use of the network to record the Parkfield earthquake aftershocks in the summer of 1966. This system was the mainstay of the early earthquake program in California in the late 1960's, and after electronic updating and conversion to a 5-day running time (for better frequency response) in 1975 it was used for rapid augmentation of portions of the telemetered net hit by strong earthquakes for another 15 years.

When a series of earthquakes unexpectedly began to shake Denver in 1963, one "autocorder" station was established at my home in Lakewood (west Denver) and a second, portable, seismograph was operated at several sites, in sequence, in order to locate the source of the earthquakes. It was near a deep waste-water injection well on the Rocky Mountain Arsenal northeast of Denver. In 1966 a seismic array formed by several 2.5-km-long 6-station linear arrays composed of refraction truck seismic systems recorded on low-speed magnetic tape recorders was set up around the well (Healy, Ruby, Griggs, and Raleigh, 1968). This array established a firm link between the earthquakes and water injected into basement rocks beneath the Denver basin through the well. Recording and analyzing this unique data set put Crustal Studies at the forefront of research on the triggering of earthquakes by fluid injection in the US and set the stage for important additional research during the next 10 years.

The great Alaskan earthquake of March 27, 1964 had a strong influence on the development of the earthquake research program in the USGS. It precipitated the decision to move Crustal Studies to Menlo Park to join with Menlo geologists in expanded studies of the San Andreas fault. It also led to the convening of an Ad Hoc Panel on Earthquake Prediction, chaired by Frank Press, to prepare a report on that subject for the Office of Science and Technology. The presence of Lou Pakiser, Chief of the Branch of Crustal Studies, on the Panel gave the USGS entree to the discussions and voice in critical recommendations for a greatly expanded earthquake research program on the national level.

As described in the Historical Overview, the rapid launching of the Survey's earthquake program in Menlo Park and the early involvement of USGS planners in the councils in Washington that guided the development of a national earthquake program were critical to establishing the Survey's central role in the ultimate program.

C. Menlo Park September 1965 - October 1995

The principal developments in the earthquake program and USGS organizational changes that occurred between the move of Crustal Studies to Menlo Park in 1966 and the enactment of the Earthquake Hazards Reduction Act of 1977 have been described in the previous section. Also, the early development of the USGS expertise in microearthquake instrumentation and analysis that helped to qualify the USGS for selection as lead agency for federal earthquake programs from 1973 onward was outlined there in some detail. Although my primary purpose in this section of the report is to follow the development of the microearthquake network, its equipment, processing procedures, and selected results, I shall first digress to report some of my early work in Menlo Park that influenced other aspects of the Survey program.

When the ERTS-A (later LANDSAT) satellite was being planned, we had an opportunity to propose a project that would utilize its long-distance telemetry capability. Several times a day this meridional-orbit satellite passes sufficiently near sites from southern Alaska to middle Central America to relay small packages of information from the field site (DCS platform) to Goddard Space Flight Center in Maryland. I wrote a proposal to establish surveillance of volcanoes from Costa Rica to Alaska by means of seismic event counters and tiltmeters located on about 15 volcanoes, including Adams, Rainier, St Helens, and Lassen in the Cascades and Iliamna and St Augustine in Alaska. What most attracted me was the provision to establish "ground truth" seismographs on many of the volcanoes. The radio-telemetered seismic systems developed for use in California seemed ideal for such use. Where possible, data from the ground truth seismographs was telemetered to nearby Survey or University microearthquake networks. Peter Ward, Elliot Endo, Dave Harlow, Rex Allen and Dan Marquez implemented the project on the basis of the funding my proposal received (Ward, et. al., 1974). The ground truth station installed on St Helens and recorded at the University of Washington, where Elliot was doing graduate work, gave advance notice of increased activity at St Helens and led Elliot to build up the net around the volcano, which prepared the Survey for the 1980 eruption and helped diminish the loss of life resulting from it. The Survey's performance at St Helens helped secure funding for a substantial Volcano Hazards Program after the eruption.

When Bill Joyner moved to Menlo Park to begin a strong ground motion program, we wanted to acquaint ourselves with strong motion recording and decided to install a small strong-motion array across the Santa Clara Valley north of Gilroy. As previously employed, the SMA-1 accelerographs we proposed to buy had no provision for accurate time control. Based on our experience with the 10-day-recorders and their use of continuously recorded WWVB time code signals, we specified that the SMA-1s should be equipped to record WWVB continuously so that the time code could be recorded during the triggered run of the recorder during an earthquake. From then on, WWVB-equipped SMA-1s were the standard model. Accurate timing on strong motion records enhances their usefulness tremendously and makes them important resources for studying the seismic source as well as for earthquake engineering.

Before leaving Denver I had translated a Russian article (Popov, 1959) that related local variations in seismic intensity to the geologic materials underlying the site. His characteristic geologic sections were numerous and seemed to be applicable to the Bay Area. We wished to estimate such intensity correction factors for typical geologic materials in the Bay Area; so we met with several Survey geologists who had worked with the soft formations around the Bay to make provisional correlations between the formations mapped on the 1/250K CDMG maps and Popov's type sections. Jim Gibbs then supervised the digitization of the maps on a 1' x 1' grid to record the character of the predominant formation in each cell (Gibbs and Eaton, 1971). When the digitized formations were translated into the appropriate intensity corrections and plotted on a map, we had taken the first step toward seismic microregionalization of the Bay Area. Jack Evernden incorporated this material into his prediction of intensities resulting from stipulated earthquakes along the principal faults in the Bay Area.

Development of the California microearthquake networks

The development of the northern and southern California networks from 1966 to about 1990 involves several interwoven themes that will be recounted in a step-wise chronological manner: we shall follow one theme for several years and then back-track to pick up another. The themes include: (1) general background on the development of network seismic and telemetry systems and the expansion of network objectives as its power increased, (2) building the network - number and distribution of stations as a function of time, (3) analysis and documentation of seismic system response and fm tape compensation systems, and (4) development of data processing techniques and systems.

Wayne Jackson began work on the telemetered network and its equipment in late 1966 in search of an alternative to the portable network. When he asked for advice on station locations, seismometers etc., I provided it; but the poor appearance and low dynamic range of the

Develocorder film records dampened my enthusiasm for the system. After the analysis of data collected through mid-1969, however, it was clear that, with all its limitations, the telemetered system would support continuous operation of a much more extensive network than the portable tape system could. From that time onward, throughout the installation of both NCSN and SCSN, whatever my formal relationship to the networks, I provided the guiding philosophy under which the networks developed as well as the hands-on work of selecting tentative station sites in support of the field crew. The driving energy behind the installation was supplied by Wayne Jackson (later, by John VanSchaack); and I fell into the role of principal network advisor. Because of my long involvement in this role, I feel that it is appropriate for me to attempt to document important considerations encountered in the development of the network and the equipment and methods on which it depends.

1. *Background on the development of the telemetered networks*

In the formative stage of the Survey's earthquake program (1965-1971), plans were developed to combine a response to the Press Panel Report on Earthquake Prediction with the Survey's long-standing interest in geologic hazard analysis. A tentative outline of a program proposed in 1968 (Earthquake and Related Geologic Hazards: preliminary draft of material for Geologic Hazards Issue Paper- by J. P. Eaton, including material prepared by Art Grantz) called for limited use of portable networks and, by omission, showed that telemetered seismic networks on the scale of NCSN and SCSN were not yet anticipated. Only three years later, in the fall of 1971, however, a primary element of the program to investigate means of predicting earthquakes in California, prepared in response to inquiries from Senator Cranston (letter to Jan Foley, 1971), was a massive telemetered network of short-period seismographs along the San Andreas and related faults. That dramatic shift in emphasis resulted from progress in the development and utilization of the unique telemetered seismic network in central California by the Branch of Crustal Studies.

The value of a dense seismic network for recording aftershocks or the ongoing seismicity along creeping sections of a fault had been demonstrated with portable networks at Parkfield in 1966 and at Bear Valley in 1967 (Eaton, Lee, and Pakiser, 1970). Unfortunately, operation of even a small portable network (10 to 20 stations) and playback and analysis of data recorded during the short lifetime of the network (several weeks or months) was so labor intensive and time consuming that continuous operation and analysis of a large portable network (100 stations or more) seemed entirely impractical.

The feasibility of long-distance telemetry of short-period seismic data had been demonstrated by Don Tocher at U.C. Berkeley, who had used commercial phone lines and a seismic system developed to support the nuclear test detection effort to telemeter about a dozen stations of a sparse regional network in central California to Berkeley for recording. Most of the stations were of conventional design, were supplied with commercial power, and employed 1.0

hz Benioff seismometers and phototube amplifiers (PTA's) that incorporated 5.0 hz galvanometers. Electronic signals from the PTA were sent via a proportional bandwidth fm telemetry system to Berkeley for recording on 16mm strip-film recorders (Develocorder). The 1.0-5.0hz seismometer-galvanometer combination produced a rather sharply peaked magnification curve that fell off rapidly above 5 hz; and the data channel frequency response in the proportional bandwidth telemetry system varied from channel to channel.

The initiative for developing a prototype telemetered USGS network was taken by the Field Operations and Instrumentation group, under Wayne Jackson's leadership, with occasional advice on system response, seismometers, and station locations from me and others. The successful 10-day tape recorder network was adopted as the prototype for "station" installation, seismometers, and overall system response. A compact 1.0 hz "geophone" was planted directly in the ground and connected to a transistorized amplifier/VCO unit, with a battery power supply, in a small covered "tub" set into the ground nearby. A signal cable connected the VCO to a phone drop. The frequency response of the system, which depended on the seismometer and the filters in the amplifier/VCO, was flat to constant peak ground velocity from about 1 hz to 25 hz.

Because of the paramount importance of telemetry to the success of the network, the question of which telemetry system to use was revisited. An audio frequency, constant bandwidth, fm system previously used with teletype, that accommodated eight data channels on a voice grade phone circuit, was adopted. It suffered from the drawback that system noise increased from channel to channel with increasing carrier frequency but had the advantage that the frequency response of all channels was the same. Favorable phone rates for multiple drops in the same telephone exchange and favorable inter-exchange rates available to federal agencies made the phone system a practical choice for telemetry in the region that it covered.

The early amplifier/VCO's were built to specifications developed jointly by the USGS and the manufacturer, a small company (Develco) in nearby Silicon Valley. Though transistorized, these units were built of "discrete" components and had relatively high power consumption; and the batteries had to be changed every few months. A variety of 1- and 2-hz seismometers were tried in the early network, but the final choice was the compact 1-hz Mark Products L4-C seismometer because of its low cost, small size, and high output. The experimental USGS telemetered network was also recorded on Develocorders, which reduced the overall system bandwidth somewhat (1 to 15 hz). This response was still much broader than that of the older system employed in the U.C.Berkeley network, and the overall seismic system was much simpler and less expensive than a conventional station.

The initial network experiment had several goals: (1) to develop the equipment and procedures required to build, install, and operate a reliable, economical telemetered network, (2) to develop a data processing facility for the timely analysis of the records it produced, and (3) to explore how the network could be used to study seismogenic structures and processes in the shallow earth's crust. Sufficient progress toward all three goals had been made by 1970 that it was clear that the telemetered network would play a central role in the earthquake program. This realization was forced upon us by the analysis of 14 month's data (March 1968 - April 1969) from the early 30+station telemetered network between Palo Alto and Hollister (Figs. 4 and 5) (Eaton, Lee, and Pakiser, 1970). It showed, in three dimensions, that microearthquakes "illuminate" creeping sections of the San Andreas, Sargent, and Calaveras faults, and it defined the southern end of the "stuck" portion of the San Andreas fault that broke in 1906.

A defining characteristic of the USGS microearthquake networks is that they are designed to help solve problems of geological process and structure involving features with dimensions of only a few kilometers by providing hypocental locations with errors of 1 kilometer or less. Identifying and mapping seismogenic structures, particularly thrust faults (often hidden), requires precise hypocentral determinations and sets a lower limit on acceptable station density in a serious microearthquake network. The large areal extent of the active boundary between the Pacific and North American plates in California and the many major faults that define it requires that the California networks be extensive. Limitations of funding and manpower available for building, operating, and analyzing the network have made it necessary to set network priorities and to strike a balance among network size, station density, and station capability. The latter involves sensitivity, frequency response, number of components, dynamic range, and reliability.

Experience with the 10-day-tape portable network, which included 3-component stations, had shown that the incremental value of two horizontal components at a 3-component station was far less than that of two additional stations with vertical seismometers only. This judgement was particularly true for a network recorded only on Develocorders. Because recurring network costs for telemetry, recording, and processing depend much more strongly on the number of seismic channels than seismic stations, it was clear that the most effective network (of a given number of seismic channels) would be one composed predominantly of single vertical component stations. In the early network recorded only on Develocorders, the dynamic range was limited to 40 db or less by the Develocorder itself. With careful management, the constant bandwidth fm telemetry system adopted for the network provided a dynamic range of 40 db or more in each data channel, and its ability to transmit 8 data channels over a single telephone circuit minimized telemetry costs. Limited funding and the unyielding requirement for hundreds of stations in any network covering a significant portion of the San Andreas fault system simply demanded that stations be cheap to acquire, install, and operate (including the cost of telemetry). These considerations suggested that the experimental telemetered network layout and equipment were appropriate for a larger network along the San Andreas fault system, and work continued to extend it northwestward to San Francisco and southeastward to Cholame for a detailed study of microearthquakes along that section of the fault (southern end of the 1906 rupture zone and the creeping section between the 1906 rupture zone and the 1857 rupture zone).

Use of the USGS short-period telemetered seismic system was not limited to the experiment in central California. Early success of that experiment on the San Andreas fault led to the system's use in many environmental networks in the western US, supported by funding from other agencies. Such networks were installed around deep well injection sites, large reservoirs, the Nevada test site, the Santa Barbara Channel, and sites of existing or proposed nuclear power stations. Most were relatively small, 6 to 20 stations, and were telemetered to Menlo Park via "telpac" for recording and analysis. Most of the environmental networks in California were later incorporated into either NCSN or SCSN. Application of these networks to a variety of problems in diverse environments benefitted the central California network experiment in several ways. It accelerated the development of equipment, particularly the use of vhf radios where phone service was not available; it raised the visibility of the networks as a tool for studying critical tectonic problems; and it provided funding to develop the skills and facilities required to install and operate the telemetered networks efficiently.

As the California networks expanded and their capabilities became apparent, the tasks to which they were applied increased in both detail and scope. The primary purposes of NCSN and SCSN were seen to be (Eaton, 1992C):

(1) long-term monitoring of local earthquakes throughout the broad zone of seismicity associated with the San Andreas and related fault systems

(a) to construct a uniform, long-term earthquake catalog (with supporting phase data and seismograms) to document seismicity of the region,

(b) to map seismogenic zones and identify the geologic structures and styles of deformation with which these zones are associated, and

(c) to provide a basis for monitoring spatial and temporal variations in seismicity that might presage major earthquakes;

(2) detailed monitoring and determination of precise hypocentral, magnitude, and focal mechanism parameters of earthquakes along sections of major faults that are expected to produce damaging earthquakes within a decade or so;

(3) real-time monitoring and analysis of earthquakes to provide timely, reliable information on their locations and magnitudes for crisis management after large earthquakes and to fill the need for general public information on "felt" earthquakes at any time.

These purposes do not include recording and rapid analysis of strong ground motion from large earthquakes. Because of the limited dynamic range of seismic recording systems (until the last few years), "microearthquake" and "strong motion" seismology have developed as separate fields that employed different instruments and pursued different objectives. Strong motion seismology saw large earthquakes as the only important ones and did not concern itself with the broad regional tectonic processes that generate a large earthquake as the culminating event of widespread seismicity over a long period of time. As suggested by the network purposes listed above, microearthquake seismology has attempted to document the seismicity of entire tectonic regions in a uniform manner for decades as a means of identifying seismogenic structures and monitoring their activity through the entire major earthquake cycle. Though very important, the major earthquake is only one of thousands that contribute to an understanding of the system.

From the early 1970s there has been an ongoing effort to upgrade the performance and lower the cost of acquisition and operation of key components of network equipment. Most of the improvements have been incremental ones that resulted from adapting new electronic devices for use in instruments designed for use in the network. A modest instrument development effort, associated with the ongoing electronics maintenance and repair activity required for network operation, has enabled the Survey to identify and exploit new components that were developed commercially in Silicon Valley.

One important area of development has focussed on the amplifier/VCO unit in the field and the discriminator in the recording center. Improvements in the amplifier/VCO involved cost, power consumption, carrier frequency stability, and input noise level. Improvements in the discriminator involved cost, base noise level, data channel frequency response, and rejection of adjacent telemetry carriers. Design, testing, and fabrication of these improved systems was carried out or supervised by John VanSchaack. The principal types of these devices that have been used in the network were described in "Review of Procedures for Calculating Short-Period USGS Seismograph System Response" (Eaton, 1993).

A second, even more critical area of adaptation, development, and skills acquisition was in data telemetry. On most telemetry circuits there are at least two steps: 1) station to telephone exchange on a "local" line, and 2) telephone exchange (combined with data from other stations) to recording center via a "long" line. Initially, phone lines were used on both paths. Subsequently, when the network expanded into regions without phone service, low-power VHF radio links were substituted for "local" phone lines, and data from a number of stations was telemetered via radio to a receiving site at a phone drop, whence it was sent by "local" line to the nearest phone exchange and then via "long" line to the recording center. Still later, the net expanded into the northern Coast Ranges where phone service was very sparse, but where we had access to a COE microwave system. Here, a typical telemetry circuit was VHF radio from the station to the nearest microwave tower, then microwave to the COE office in San Francisco, and finally "long" line to the recording center in Menlo Park. Later still, the USGS took over maintenance of the northern Coast Range microwave system and added compatible microwave links, including one to the recording center, that extended the microwave "trunk" system southeast to Parkfield. The USGS' development of its own telemetry system was spurred by the virtual collapse of the phone service we enjoyed initially: when the Bell system was broken up by Court order, phone rates skyrocketed, particularly for long lines, and we were forced off of the system by excessive cost.

Further extension of the northern California microwave system was achieved recently when the USGS gained access to the COE Sierra foothill microwave system, which is linked to the Coast Range system, and to an FAA system that links Reno to Sacramento.

In southern California, the USGS installed critical microwave links that tie Caltech to a COE system that runs into the Mojave Desert, to an Air Force system that runs to Edwards Air Force Base, and to the COE Sierra Foothill system that connects with Menlo Park.

The equipment and practices for microwave telemetry are well developed, and the patchwork system that provides telemetry for NCSN and SCSN works well. However,the radios used to tie individual stations to the microwave system present more difficult problems. We have access to a limited number of narrow "splinter" channels (about 30) that are used over and over in different parts of the network. The radiated power is low, generally only 100 milliwatts; and many paths are quite long - up to more than 50 km. Many receiving sites are at fairly well built-up mountain-top communication facilities that house much higher-powered transmitters than we are allowed to use. Hence, the receivers must have high sensitivity and very high selectivity.

The most satisfactory radio that we have found was improvised from a 1960s-vintage Motorola "walkie-talkie" that had separate transmitter and receiver cards. Replacing the crystals, reducing the radiated power of the transmitter, and mounting the transmitter and receiver in small separate water-tight boxes cost only a few hundred dollars a pair. The transmitter power required is low and can be supplied by an inexpensive solar-power/battery unit. The selectivity of the receiver, which employs a tuned LRC input filter, is remarkably good. The hundreds of transmitter-receiver pairs that were fabricated from surplus walkie-talkies are the mainstay of the seismic network telemetry system.

The adaptation of the Motorola radio for use in the network and its integration with the microwave network to form an inexpensive, flexible, and generally reliable telemetry system for the seismic network has been one of the crowning instrumental achievements of the earthquake program. That system permitted stations to operate in large regions not effectively served by the phone system and literally saved the network from shut-down as a consequence of escalating phone rates when the Bell System was broken up.

These radios are now nearly 30 years old, however, and are no longer manufactured or supported by Motorola. They have become difficult and expensive to repair and maintain. Moreover, they do not comply with the performance standards set by IRAC for enforcement in the near future, and they will have to be replaced over the next few years.

To provide for backup recording of the network as well as to permit more flexible analysis of network records than was possible with the Develocorders, the incoming multiplexed phone-line signals were recorded on magnetic tape. Playback of the multiplexed signals through appropriate banks of discriminators recovered signals from the stations in analog electronic form suitable for playout at a variety of speeds and gain levels, with or without filtering, as well as automatic digitization by an analog to digital converter. The greater flexibility and dynamic range provided by tape playbacks stimulated the development and installation of a subset of low-gain 3-component seismic stations in NCSN to supplement the high-gain vertical net. Setting up the tape recording and playback systems with appropriate tape compensation to attain an acceptable dynamic range posed many problems. Analyses and solutions of those problems were described in a series of Open-File Reports (see below).

Although the analog tape recording and playback system was a vital part of the network for nearly 20 years, it has now been replaced by digital tape recording of the multiplexed network data stream at the output of the analog to digital converter.

2. *Building the networks - number and distribution of stations as a function of time*

For maintenance, operation, and data analysis the California microearthquake network is divided into two parts, about 390 stations recorded in Menlo Park (NCSN) and about 210 stations recorded in Pasadena (SCSN); but virtually the entire network was planned and installed by the team in Menlo Park. This situation arose because the NCER Field Operations group that was housed in Menlo Park developed the instrumentation and the procedures for installing and operating the experimental telemetered microearthquake network between Palo Alto and Hollister at the onset of the USGS earthquake program between 1966 and 1968 - before the USGS had a network agreement with Caltech and a field office in Pasadena. Through the early 1970s the central California microearthquake network held center stage as it was extended as a 30- to 50-km-wide band along the faults of the San Andreas system, northwestward to Santa Rosa and southeastward to Parkfield. Several small isolated environmental microearthquake networks set up by the Menlo Park team during these years, which were later incorporated into the two networks, were initially operated and analyzed as separate networks (Santa Barbara, 1968; Los Angeles Basin, 1971; New Melones Dam, 1972, Oxnard, 1973; Imperial Valley, 1973, and Eastern Mojave Desert, 1974). The Los Angeles Basin network was installed by the USGS as part of the Caltech network; and several other southern California environmental networks were telemetered to Caltech and recorded there on an informal basis, but their analysis was carried out by staff from Menlo Park.

After the "NTO" program increases became available in late 1973, a more formal arrangement was worked out by the USGS and Caltech to establish a joint USGS/Caltech southern California microearthquake network. The USGS was to install and operate the network and Caltech was to assume primary responsibility for data analysis, with support from a USGS contract. A small USGS group was to be housed at Caltech to operate the network and to cooperate with the Caltech staff in carrying out research on network data. The SCSN that came into being at this time was composed of about 15 telemetered stations of the sparse Caltech southern California network and about 80 telemetered USGS stations. Caltech continued to maintain its stations, and the USGS provided a minimal technical staff to maintain and operate the USGS stations.

Following the agreement, the Menlo Park team continued to augment the SCSN by installing clusters of stations in several gaps in the network: Coso, 1975; San Bernardino Mountains, 1975; Carrizo Plains, 1976; southern coastal region, 1976; and the southern Sierra Nevada, 1979. The last big effort by Menlo Park to complete SCSN was mounted in 1981, with about 10 additional stations, each, in the Transverse Ranges, western Mojave Desert, San Bernardino Mountains, Imperial Valley, and the southern coastal region.

In Northern California the important detached environmental networks were in the Sierra Nevada-Mt. Lassen region: New Melones Dam (near Stockton), 1972; Oroville, 1975; Auburn, 1976; and Lassen, 1976. Like the southern environmental networks, they were operated as separate projects and analyzed independently. After 1974 the central California "strip net" between Santa Rosa and Parkfild was expanded in length and breadth to cover the entire Coast Ranges from the Transverse Ranges on the south to the Oregon border on the north and from the Pacific Ocean on the west to the Great Valley on the east. The most striking additions were in the Geysers/Clear Lake region (1975) and the Cape Mendocino/Klamath region (1979).

In 1980 the procedures for analyzing the network data recorded in Menlo Park were revised so that the detached Sierra Nevada-Lassen networks were combined with the Coast Ranges network to form the present NCSN. In 1982 the number of stations in NCSN reached 322, and in SCSN the number reached 209.

By the end of 1982 the USGS earthquake program and its management were very different from what they had been before 1982, and these changes radically altered the status of the networks. Prior to 1982, building the networks was carried out by the Field Operations group in direct consultation with the Chief of the Branch of Seismology (or the OERCS Chief before August 1973). Along with the replacement of the Chiefs of the Seismology and Network Operations Branches in 1982, the Network Operations Branch was eliminated and NCSN was reduced to a project in the Seismology Branch. This change dramatically diminished the status of networks in the earthquake program and put them in direct competition for funds with a multitude of small "research" projects whose chiefs had no responsibility for the health and development of the overall earthquake program. It also effectively severed the connection between NCSN and SCSN.

The networks were required to compete for funds on the basis of the "research" content of their work, and little allowance was made for their unique non-research contributions and responsibilities to the program. Moreover, the merits of their proposals were judged by panels composed of members or chiefs of other projects that were competing for the same funds as the networks and who did not have the same responsibilities for quality, uniformity, and longevity in their work as did the networks. Other project chiefs were in a position to dismember the networks by "outvoting" them in the panels.

The results of these changes can be seen in the growth of NCSN and SCSN as a function of time. Averaged over periods of relatively steady growth, the growth rates and corresponding time intervals for the two networks are:

NCSN 1966-1974, 15 stns/yr; 1974-1982, 22 stns/yr; 1982-1995, 5 stns/yr.

SCSN 1968-1972, 5 stns/yr; 1972-1976, 24 stns/yr; 1976-1980, 9 stns/yr,
1980-1982, 31 stns/yr; 1982-1995, 0 stns/yr.

The continued slow growth of NCSN after 1982 does not reflect a systematic effort to upgrade the network as much as several isolated efforts, sponsored by non-NCSN projects, to improve the network to fulfill their own special needs. Some of these additions, however, did lead to significant improvement of the network in the affected regions.

The most significant post-1982 development in southern California has been the installation of a parallel network of about a dozen broad-band high-dynamic-range stations.

Although there were other factors at work, as analyzed by Eaton (1992C), these figures suggest that the reduction of the network from Branch to project status in 1982 effectively killed the further development of telemetered short-period microearthquake networks in the earthquake program, in spite of demonstrated critical gaps in the coverage provided by those networks.

The networks were installed one station at a time, and the work required for each station was time-consuming as well as extensive. The required skills were acquired by the Field Operations group in the course of setting up the experimental telemetered network between 1966 and 1968 as well as in earlier crustal refraction work. It had worked out procedures for identifying and permitting station sites and arranging for phone circuits to telemeter their signals to Menlo Park. It had also selected and evaluated the principal components of the seismic systems (seismometers, amplifier/VCO's, discriminators, and film-strip recorders) and was learning to work within the government procurement system to acquire them in a timely manner. The principal enemy of progress was delay: delay in procurement and deliveries, delay in access to phone circuits, and delay in obtaining permission to use selected sites. Careful planning and scheduling were required to keep such delays at a tolerable level.

Permitting a site can be an exhausting process that requires visiting the County Recorder's office to identify landowners, tracking down and arranging appointments with landowners, and visiting with landowners to explain the importance of our work and to obtain permission to install the equipment on their land. Success rates have been remarkably high, approaching 100%.

Arranging for telephone service was far from straightforward. It required identifying the telephone pole, by number, where a service "phone drop" was needed. The phone company could then determine whether existing lines could provide the service. Because billing depended on details of exchange boundaries, how and where circuits were combined, etc., it was necessary to plan the circuits carefully to insure economical phone service.

When VHF radios came into use there was greater flexibility in the selection of station sites, but they brought new problems of their own. Transmission paths from station to candidate receiving sites had to be evaluated for line-of-sight and tested.

When a new station was installed, provision for it had to be made at the recording center. It required a discriminator, a channel on a Develocorder, a channel on the backup tape recorder (after 1975), and a channel on the CUSP A/D (after 1984). It also required appropriate wiring and notekeeping to carry the signal to the assigned channels and to document the station's place in the recording system.

Given the meager budgets available for equipment, and the rapid growth of the network (average of 37 stns/yr in NCSN and SCSN combined between 1970 and 1982), economy was essential. Extensive use was made of government surplus lists: most of the Develocorders and Bell and Howell tape recorders as well as almost all of the VHF radios came from this source. Cost was also one of the factors that led us to design and build preamp/VCO's and discriminators. At half to one third the cost, the USGS units matched or exceeded the performance of the commercial ones, particularly in power consumption. To avoid delay and to secure favorable prices for seismometers, contracts were negotiated early in the fiscal year that stipulated a conservative lower limit but a large upper limit for the number to be delivered during the year. This practice permitted additional seismometers (beyond the minimum) to be ordered when needed without renegotiation and the attendant delay, and for a price that diminished as the total number increased throughout the year.

Although many people worked to install and operate the networks, a few stand out in one or more aspects of the work. Wayne Jackson, and then John VanSchaack, who led the Field Operations group energized and organized its work with remarkable effectiveness. For many years Gene Taylor carried the principal burden of permitting, selecting the physical site for the station, setting up the telemetry, and installing the field equipment. In the last task, he was assisted by Alex Loquaio for many years. Gene undoubtedly arranged for the installation of more stations in California than any other person, perhaps even more than all others combined.

For more than twenty years Wes Hall has designed and implemented the data receiving and signal distribution systems in Menlo Park and Pasadena as well as supervised the operation of the Menlo Park network recording system: discriminators, tape recorders, Develocorders, etc. His group has also monitored the performance of the telemetry system and field stations and interacted with the phone company and USGS field technicians while they carry out repairs in the field. A major participant in network monitoring has been Charles Daiss. When I wished to implement a more rigorous procedure for keeping track of components and attenuator settings at individual field sites in 1976, Charles collaborated with me in establishing the procedure; and he has carried the sole responsibility for implementing it and assuring its completeness for nearly 20 years. In this work and similar documentation of telemetry paths and equipment as a function of time, he has benefitted NCSN enormously and has become an irreplaceable member of the recording team.

The data telemetry system (microwave and VHF "feeder" radios) is one of the most important elements of NCSN. John Kempe has played a central role in setting up and maintaining the extensive microwave system we now use. Though largely self-taught in this technical area, he has clearly attained "expert" status; and he applies his skills with unusual dedication and energy. Our continued success with the aging Motorola radios depends on the laboratory and field skills of Bob Mclearn and Eric Tofsrud. Finally, one can only wonder how two seismic field technicians, Dave Reneau and Dave Croker, manage to maintain nearly 400 stations spanning California from the Transverse Ranges to the Oregon border. This feat is more remarkable in the light of chronic shortages of critical repair components (principally radios) that they have faced in recent years of dwindling network support.

3. *Seismic system response and tape compensation*

When I stepped down as Seismology Branch Chief in 1975, the number of stations in NCSN was approaching 200, and in SCSN, 100, and I became more deeply involved in network equipment and calibration. This work is documented in a sequence of reports cited in this section. A variety of seismometers, amplifier/VCO's, and discriminators were in use, and standards for adjustment and operation of the equipment that had been established in about 1973 were being implemented. To document the current network situation, I collected data from the field technicians and the electronic support group on what combinations of components were in use, and I carried out tests on the preamp/VCO-discriminator-Develocorder combinations to determine empirical frequency response curves for the electronic component of the seismic system. The total system response was obtained by combining the theoretical seismometer response (from free period, damping, motor constant, and mass) with the empirical electronic response (Fig. 11).

To simplify the process for the future, I stipulated the characteristics of a "standard" seismic system and calculated its response along with those of the system combinations then in use. The response of the standard system was completely determined, except for a sensitivity factor that depended on the amplifier/VCO attenuator setting. Thus, a single response curve, scaled by the appropriate sensitivity factor, described all of the standard systems. This work was written up as an Open-File Report for future reference and to guide other users of the USGS seismic system (Eaton, 1975A).

To offset the poor data quality of the Develocorder and to provide back-up when they failed, in the early 1970s the network electronics support group set up a tape recording system to capture the flow of data from the network. Individual "phone lines" with up to 8 multiplexed fm data channels were recorded in direct record mode on a single track of a 14-track 1" x 14" instrument grade recorder. To recover the seismic signals, the tapes were played back through banks of discriminators (eight per track) and written on an ink-jet oscillograph.

The first tape recorder used for this purpose was unsatisfactory. It was a CEC machine that had been hastily modified (by the manufacturer) to run at the 15/16 inch/second speed required to accommodate 24 hours recording, but the modification introduced a strong low-frequency flutter. The second machine that was tried was the Bell and Howell 3700B. It was much better than the CEC, but it introduced broad-band noise that was only about 40 db below the maximum recordable signal on many traces.

The Develco discriminator then used in the network had been designed to support subtractive tape speed compensation, but the electronics network support group had been unable to implement it successfully. With the help and advice of John VanSchaack, I set out to identify the problem and correct it. The approach I used was quite elementary. From components supplied by John, I assembled a "bank" of 8 VCO's that permitted modulation of the carrier frequencies in two modes: constant (the same on all channels), and proportional to the carrier frequencies. The proportional mode simulated signals produced by tape speed variations, and the constant mode permitted the adjustment of modulator sensitivities. The outputs of all eight modulators were combined (added) to simulate a phone line carrying the full set of 8 modulated subcarriers. Modulation of each of the eight channels could be turned on or off separately. This device provided the type and (amplitude) range of signals needed to examine the signals at various stages in the discriminator to discover what was wrong. It turned out that most of the discriminators were operating outside the compensation range because of an incorrect choice of a "bias" resistor in the discriminator circuit. Once the resistors were replaced with the correct ones and the compensation level adjustments were set correctly on the individual discriminators (by means of the proportional modulation option), the Develco discriminators provided substantial compensation for tape speed variations. The system noise from unmodulated carriers played back from tape was about 50 db below the full range signal. The results of this work were also described in an Open-File Report (Eaton, 1975B). Although these results were very important for the microearthquake network, they did not appear to be "new" and publishable in the electronics literature.

Intrigued by the possibilities of subtractive compensation, I expanded the experiment to cover two additional systems of interest to seismology. For the first, I set up, tested, and described a 3-channel broad-band system (0 to 100 hz) with subtractive compensation (Eaton, 1976A). For the second, I modified a low-power portable PI5100 (10-day) tape recorder to run at 15/16 inch/second and showed that it could be used to record the standard USGS system (8 channels, 0 to 30 hz)(Eaton, 1976B).

In the next investigation this "cycle" (documented in Eaton 1976C) I extended the format of the standard USGS multiplex system by adding two timing channels and one compensation channel "above" the eight data channels. The telephone and VHF radio channels covered the band 300 to 3000 hz, but the Bell and Howell could record up to 5000 hz. This change did not alter the field equipment or procedures. It was accomplished by mixing the multiplexed phone-line signal with the timing and compensation signals in the lab just before recording on the Bell and Howell. This scheme was superior to the earlier one because it freed up a tape track previously used for timing and it "embedded" timing and compensation signals in the data on each recorder track, which promised to improve compensation and diminish track-to-track timing errors (Figs. 12 and 13).

This paper also documented recording the standard augmented USGS multiplex system on a low-cost "consumer" fm tape recorder (Sony TC126). The effect of subtractive compensation was spectacular: with compensation, the TC126 produced results comparable to those of the Bell and Howell (Figs. 14 and 15). This part of the experiment paved the way for the development of the "Cassette" recorder refraction system (100 copies were built) that supported the revival of the USGS seismic refraction program (Eaton, 1976C).

The next paper examined other factors that affected the design of the Cassette recorder system (Eaton and VanSchaack, 1977).

By 1977 the USGS short-period seismic system was the mainstay of most telemetered microearthquake networks in the US: 220 station in NCSN, 130 stations in SCSN, plus many stations in Washington, Nevada, Missouri, Wyoming, Hawaii, and Alaska; and the suitability of the system for use in such networks was a topic of frequent discussion. To describe the system in terms of its component parts and to examine its response in relation to the frequency spectra of magnitude 1 to 7 earthquakes as well as background earth noise, I prepared another Open-File Report, "Frequency response of the USGS short-period telemetered seismic system and its suitability for network studies of local earthquakes" (Eaton, 1977). This paper analyzed the response of the system in terms of its component parts (Figs. 16 and 17) and estimated the expected spectra of the recorded seismograms as a function of magnitude (Fig. 18). The system is well adapted to the study of earthquakes up to about magnitude 4. For larger quakes it should be supplemented by high-dynamic-range broad-band seismographs.

By 1975 experience with the 10-day-recorder seismic system had turned up several problems, including the response of its amplifiers, so we undertook to modify it to consume less power and to provide better frequency response. The motor-drive power was reduced, the tape speed was doubled, and the fm center frequency was quadrupled. One of its seven channels was set up to record, in direct mode, two multiplexed fm timing signals (WWVB and IRIG-H) and a compensation reference signal, leaving 6 fm data channels for high- and low-gain 3-component seismic channels. These modifications were reported along with detailed set-up and operating instructions in an Open-File Report (Criley and Eaton, 1978).

Although components had been purchased for tape playback facilities to take advantage of the revised NCSN and 5-day-tape formats that supported subtractive compensation, no action had been taken to design and assemble the playback system. In desperation I proceeded to do so myself. It was a complex task that required interplay with and assistance from several electronic technicians committed to other projects. Steady work for at least 3 months was required, but the system was completed and a description and operators manual were ready in early 1978 (Eaton, 1978). That system made high quality playouts of the larger (M3.5 to M6) California earthquakes available on a routine basis and led me to the study of focal mechanisms and traveltimes of such earthquakes.

My initial treatment of the seismic system response in 1975 avoided the issue of calculating the frequency response of the electronic system by measuring it empirically. In 1980 I adapted a method of treating those aspects of the system, developed by Healy and O'Neill (1977) and by Stewart and O'Neill (1980), to calculate the whole system response in terms of the poles and zeros of the response functions of its component parts (Fig. 19) (Eaton, 1980B).

After the introduction of the CUSP system, which digitizes network signals prior to degradation of the signals in the tape recording/tape playback process, we found that telemetry system noise that was previously hidden in the Develocorder or tape recorder background noise was higher than expected. From analysis of noise levels preceding the onset of waves from a moderate Cape Mendocino quake played back from tape, and with the help of John VanSchaack in analyzing system noise at critical points between the discriminator output and the A/D output, I identified several sources of low-level system noise and set priorities for reducing noise in specific components by redesign or better adjustment of those components (Eaton, 1984B). One such modification was the addition of a small capacitor shunting each A/D input channel to ground to eliminate radio-frequency noise from the A/D converter that was leaking back into the input.

4. *Development of data processing techniques and systems*

The sophistication and capacity of seismic network data processing systems have increased even more remarkably than the number of stations in the network and the size of the areas they cover. The advances occurred incrementally as improved circuit components and faster, cheaper computers became available. Seismology's contributions to this process were in identifying and adapting new circuit components for collecting and recording the data and in identifying, assembling, and programming the new computer systems to carry out the data recording and analysis. The new systems have increased analysis productivity by at least 100 times over those available for the 1966 Parkfield experiment, and they are far superior in the manner in which the seismic traces are presented for analysis and in the accessibility of archived seismograms and the results of analysis.

The many steps required to process network recordings were most painfully obvious with the 10-day-tape system:

(1) event detection and selection for analysis,

(2) accessing the seismograms for individual stations,

(3) identifying and timing wave arrivals, measuring maximum wavelet amplitude and period, and determining event duration on each seismic trace,

(4) combining readings from all stations into an event phase list for input to the location program,

(5) locating events by computer,

(6) examining the computer location and supporting station data for internal consistency, and flagging inconsistent data,

(7) reexamining original seismograms to verify or correct suspicious readings,

(8) relocating the events,

(9) preserving the seismograms, readings, and results of analysis.

A great deal of time-consuming hand work was required to carry out these tasks, and many opportunities for error arose in the marking and scaling of records, card punching, etc., that were required in the preparation of event phase card "decks" or computer files.

With the portable networks and the early telemetered network, the primary data set consisted of boxes of punched computer cards made up of the sets of phase cards from each event that was processed. Corrections were made by physically removing and re-punching the erroneous cards. Events were located in batch mode as the cards were read into the computer. The corrected phase cards themselves constituted the primary record of network seismicity. Supplemental archive material included computer printouts of the locations and supporting data as well as selected paper playbacks and magnetic tapes. All were bulky to store and difficult to access when archived.

For ease of processing, the Develocorders were an important advance relative to the 10-day-tapes. The first three steps were collapsed into one as the films were run through the film reader for simultaneously identifying events, accessing individual traces, and reading the traces. Card punching by hand, batch location of events, hand correction of errors by re-punching cards, and archiving of the phase cards was done as before, however. Develocorder films were also saved in the archives.

Procedures for processing the telemetered central Coast Ranges network were set up originally by John Roller, who employed the HYPOLAYR location program. This early effort was augmented, then superseded, by a more ambitious data processing system worked out by Willie Lee, who upgraded and expanded the location program, then renamed HYPO71 (Lee and Lahr, 1971), and wrote a number of additional programs to expedite and improve network analysis.

The next big step forward in network data processing was the introduction of the table-top digitizer, by Ward, Lahr, and Ellsworth, as a means of reading the Develocorder films in about 1975. Rapid pre-analysis scanning of selected specially organized Develocorder films provided lists of events for analysis. Events on each film were read in sequence, and the readings from the entire set of films were later collated by computer. With the image of an event on film projected onto the table, the operator recorded two minute marks, each, on two timing traces (one at the top and and the other at the bottom of the film) to establish the time base and the positions of the separate traces on the table. Positioning the crosshairs of the cursor over a spot on the trace to be read and pressing the recording button "read" the x and y coordinates of that point on the table and generated a punched card to record them. Those coordinates were interpreted by computer to identify the trace that was measured and to calculate the time, etc., of the measured feature. The table-top digitizer combined the first four steps into one and eliminated hand punching of phase cards; and the collation program assembled fragments of an event read from different films into a single phase deck for analysis. Event locations were still carried out in batch mode and errors were screened and corrected by hand.

The next advance, in about 1978, was accomplished through better access to and interaction with the computers that located events. Interactive access to computers with at least a modest amount of "user" disk space permitted the phase card files to be stored and manipulated "inside" the computer and facilitated review of the quality of individual solutions, the correction of individual "card" errors in the editor, and immediate relocation of the event. Although corrected punched phase card files were generated for the archives, increasing use was made of digital magnetic tape for archiving phase and location files. The principal computer used for network analysis at this time was the (USGS) Computer Center Division's Honeywell MULTICS computer in Menlo Park.

A separate but parallel approach to network data processing dating back to about 1970 sought to accomplish the task automatically by computer. The first functioning real-time-processor was built for the small Rangely network (about 16 stations) and was implemented by Sam Stewart on a CDC1700 computer (Stewart, 1977). This system was later expanded to about 100 stations and applied on an experimental basis to the central Coast Ranges network.

With the advent of microprocessors, Rex Allen and Jim Ellis set out to implement a larger, faster real-time-processor that utilized distributed processing, in a system of their own design and construction, that employed a number of Texas Instruments microcomputers. Although a separate pre-processor (to accomplish A/D conversion, event detection, and phase picking and timing) was required for each group of eight stations, the system was expanded eventually to cover about 250 stations in central California (Allen and Ellis, 1980). This system was particularly effective for small to medium events in dense parts of the network. This system, like the CDC1700 before it, did not save seismic traces; so the quality of a location was judged solely on its internal consistency.

An effort to develop an interactive program to process seismic traces directly in the computer was begun in Menlo Park in the mid-1970s. The program was written mostly by Pete Stevenson and was designed to run on a Data General Eclipse minicomputer. It depended on an independent system to detect and select network events and store their records on standard NCER analog tapes. The Eclipse analysis system employed an A/D converter to read the analog tapes into the system. Display of the seismic traces for analysis was on a Textronics 4014 storage terminal. Reading events into the system piecemeal from the library tapes and then reconstituting them in the computer for analysis proved to be so slow for the large central California network that the Eclipse system was never put into routine use in Menlo Park.

The smaller HVO seismic network, which employed a single network tape recorder, was a much better match to the Eclipse system. Fred Klein and Alex Bittenbinder completed "debugging" an Eclipse system in Menlo Park and then took it to HVO in December 1978. In Hawaii, the Eclipse system soon accommodated the entire network and pushed HVO ahead of Menlo Park (NCSN) and Pasadena (SCSN) in the quality and efficiency of network analysis.

Until mid-1976, equipment and maintenance records for NCSN were kept in "station folders" which accumulated the irregular notes written by the installation and maintenance crews to record events or facts that they deemed important. This material was supplemented by periodic "tape-lineup" tables prepared by Wes Hall to document the phone line and carrier frequency that brought a "station" to Menlo Park as well as the tape recorder and track on which the phone line was recorded. Information on equipment in the stations, including attenuator settings, was unsystematic and fragmentary and was clearly inadequate to support the tape-based Eclipse analysis system that was being developed. In collaboration with Rick Lester and Charles Daiss (for Wes Hall) I set up two network history files, one to keep track of station locations and dates of installation and termination, and the other to keep track of individual instruments in each station. The first of these had been developed by Rick Lester and appeared to serve its purpose well; so it was adopted, with minor modifications, as the location history file (*lochst*). A second file, the maintenance history file (*mnthst*), combined information on the tape-lineup tables with information on equipment, attenuator settings, etc., in the individual stations. The *mnthst* file was based primarily on "maintenance visit" reports, that were completed to document each visit to a station in the field. The report included station name and component, date and time of visit, VCO type and serial number, calibrator code, carrier frequency, attenuator setting, seismometer type and serial number, tape recorder ID and track, dates of installation of VCO and seismometer, dates of installation of VCO and calibrator batteries, date and time of tape track assignment, and codes specifying reason for visit and actions taken. This information was punched on an 80-column card, and two copies were made. The first replaced the previous card for the same instrument in a network status file, and the second was added to the set of cards for the same instrument in a maintenance history file.

When interactive computers became available, the maintenance history and network history files were set up in a network history directory. I then wrote a program that uses the location and maintenance history files to compose a "station list" file, valid for a specified date, that is required by HYPO71 and later location programs to calculate earthquake locations and magnitudes.

In early 1980, central California seismic network data analysis was shifted from the Computer Center Division's Honeywell MULTICS computer, which had become too expensive to use, to the Office of Earthquake Studies' (DEC) PDP11/70, which employed a UNIX operating system. Because the 11/70 had a smaller memory than the Honeywell and used FORTRAN77 instead of FORTRAN66, the network analysis programs had to be revised significantly to run on the 11/70. The network analysis project was seriously understaffed at that time because of the departure of Travis Houck, who had helped implement analysis on the MULTICS system and had provided programming support for that effort. Moreover, the whole Calnet project was in a state of disarray. It needed clarification of the boundaries and purposes of the network as well as a clearer definition of standards and procedures for carrying out its work.

As Acting Chief of the Branch of Network Operations as well as the Project Chief of Calnet (under the Branch of Seismology) I had a keen interest in updating network procedures and transferring network analysis to the more economical UNIX system as soon as possible. Lacking any qualified person to assign to the task, I undertook to do it myself. This effort occupied most of my time during 1980 as well as several additional months in 1981, when I developed programs and procedures for the routine merging of the hand-timed data and carefully screened RTP data.

My work on the first part of this task was described in a Calnet handbook on a "Daily Event Scan List" (Eaton, 1980A). The procedures that it described were required to:

(1) scan the daily network records to identify and classify events that were recorded,
(2) decide which of these should be saved (appropriate network tapes dubbed), and which should be processed to obtain their locations, magnitudes, etc.,
(3) generate a "dub request list" to govern the computer-controlled automatic dubbing of network tapes and compilation of a tape library,
(4) provide materials to facilitate timing (hand timing from film and interactive timing on the computer) of the events that were to be located.

Fortran listings as well as brief descriptions were provided for the programs required to implement those tasks.

The computer-controled automatic dubbing of network tapes to compile a library tape (in step 3, above) was implemented by Tom Jackson on the Eclipse computer.

The second part of the work, on processing phase data, was reported in Open-File Report 81-110 (Eaton, Lester, and Cockerham, 1981). It provided a detailed outline of the Calnet system for processing network data, including descriptions of the directory structure and individual programs that were employed to do so.

The procedures and programs described in these reports were utilized for processing NCSN records until early 1984, when the CUSP system was implemented in Menlo Park.

A major advance in network processing was achieved at Caltech between mid-1976 and early 1979 by graduate student Carl Johnson (Johnson, 1979). He developed a system that was implemented with two 16-bit 32K minicomputers (Data General Nova 850 and Data General Eclipse S/230) that consisted of an on-line digital data acquisition system and an off-line data analysis system. The on-line system digitized the entire southern California network in real time, analyzed the traces to detect the sudden onset of seismic energy, and saved the digital record of the entire network for the events so detected. The off-line system performed a number of display and analysis functions, including display of seismic traces, timing of wave onsets, and location of earthquakes. The initial system, CEDAR, accommodated about 150 stations at a sample rate of 60 sps.

After joining the USGS, Johnson remained in Pasadena to develop a more advanced processing system (CUSP) that was implemented on two 16-bit 32K minicomputers (PDP 11/44's). Like CEDAR, CUSP passed the events saved by the on-line system to the off-line system by means of 9-track digital tape. The digital seismograms and results of analysis were archived also on 9-track tape. The analog network tape recorder was continued in operation as a backup to the CEDAR and CUSP systems.

By 1983 it was clear that the Eclipse processing system developed in Menlo Park was too slow for processing the NCSN records and that the CUSP system developed in Pasadena showed great promise; so the decision was made to implement a CUSP system in Menlo Park. That task was undertaken by Sam Stewart, who was later joined by Bob Dollar, Peter Johnson, and Tom Jackson. By early 1984 a CUSP system that employed a pair of PDP 11/44's was ready for testing. Like the Pasadena system, it depended on 9-track digital tape to pass the digitized events from the acquisition system to the analysis system and to archive the digital seismograms and results of analysis. A separate network processing group under Shirley Marks' supervision was set up to test and implement the Menlo Park CUSP system.

The initial Menlo Park CUSP system did not effectively cover the northernmost part of the network. The Coast Ranges north of Clear Lake and the Sierra network were processed in a hybrid manner. The old system that depended on the RTP or film scanning to identify events and on dubbed analog tape to save them was continued. Those records were then digitized and presented to the off-line analysis system for processing and integration with the rest of the network. At that time the CUSP analysis system was run on two PDP 11/44's and one VAX 750. I undertook the analysis of most of the recovered northern NCSN events on the VAX750 to familiarize myself with the CUSP system.

Work to refine CUSP, to develop a standard "generic" version of the system, and to implement it on more advanced computers continued both in Menlo Park and Pasadena. In Menlo Park, Bob Dollar re-wrote the on-line data acquisition system to run on a Microvax and Sam Stewart re-wrote the off-line analysis system, now called TIMIT (Stewart, 1993), to run on DEC 3100 workstations. The greater speed, increased memory, and increased disk capacity of these machines, which are networked together, supported an increase in capacity of the system and greatly reduced its dependence on magnetic tape as an interim storage medium. In normal operation, events are stored on disk until they are processed and archived, but magnetic tape is available to accommodate the heavy load of aftershock sequences. The new Microvax/3100 CUSP system went into routine operation in Menlo Park in early 1990. The current system accommodates more than 600 channels at a sample rate of 100 sps.

Fear of losing important network events (teleseisms, for example) that failed to trigger CUSP kept the five analog Bell and Howell tape recorders in use. They became increasingly expensive to maintain, and the time and effort required to digitize the events they preserved for input to the analysis system discouraged their use. This problem was finally resolved by the advent of inexpensive, high capacity digital tape recorders. In 1993 the five Menlo Park analog recorders were replaced by an EXABYTE digital tape recorder and stacker. The multiplexed digitized record of the entire network streams onto a ring buffer in the data acquisition computer. This data stream is written off to the EXABYTE in 10-minute blocks. A single audio-cassette-sized EXABYTE tape holds about 10 hours record of the entire network. Because they are digital and their structure (where a given 10-minute block is located) is known, they can be read into the analysis system rapidly, and the data are ready for use.

In contrast to the prolonged 9-step sequence required for analyzing an earthquake recorded on the 10-day-tape network, the analysis of an NCSN earthquake in the CUSP system is accomplished in a single session lasting only a few minutes for a small or moderate earthquake. For a large event (M>4.5) recorded at hundreds of stations the task can take several hours even on CUSP, but such events are rare and would require several days for comparable analysis from tape playbacks. The CUSP system minimizes the slow, repetitive work of preparing seismograms for analysis; finding and labelling individual stations, and reading and recording data from them; compiling phase files for individual earthquakes and entering them into computer files; locating earthquakes and reviewing their solutions for discrepancies; re-accessing seismograms to verify or correct suspicious readings; re-locating corrected events; etc.

When the analyst "brings up" an event for processing on CUSP, a great deal of preliminary work has already been done automatically by the computer, and traces are displayed in a manner that facilitates the critical seismological judgements that the analyst must make. The on-line data acquisition system has received the multiplexed digital data stream from the 600+ channel A/D converter that records the network, has maintained surveillance of short-term-average/long-term-average signal level ratios at individual stations to detect seismic wave onsets, and has saved the digital record from portions of the network that detected the occurrence of an event. The multiplexed records of the saved events are then demultiplexed, individual station files are associated with their proper station parameters (location, etc.), all traces are "picked" by an off-line version of the RTP to generated an event phase file, and the phase file is processed to determine a preliminary hypocenter and magnitude. The seismic trace data is saved in an *event.grm* file and other data pertaining to the event are saved in an *event.mem* file. When the event is brought into TIMIT for interactive analysis, the preliminary location and calculated station distances are used to display the traces in order of increasing distance and to plot them against a "reduced" time scale so that first arrivals of waves travelling through the "average" crust fall near the same vertical line on the screen. The amplitude of each trace is scaled to offset the very large difference in amplitude between near and far stations and between high and low gain stations. The starting time and duration of the portion of the traces displayed on the screen can be set, as well as the number of traces that are plotted on a single "page". Portions of individual traces chosen for closer scrutiny are displayed in a "picking window" where the portion of the record displayed can be shifted along the time axis, expanded or contracted in time, amplified or diminished in amplitude, and filtered by a widely adjustable band-pass Butterworth digital filter. These operations can greatly enhance the legibility of an earthquake signal, particularly in the presence of high frequency noise. Picking and labelling wave onsets and measuring wavelet amplitudes and associated periods are accomplished rapidly with the use of a "mouse"-driven cursor. The readings so obtained are indicated on the displayed traces and are automatically saved in the *event.mem* file, where they are available to a variety of programs that support TIMIT and can be run in separate "windows" without exiting from TIMIT. One such program is QED/Grope, which determines the hypocenter of the event and tabulates station distances, onset arrival times, travel times, and residuals. Examination of this table permits rapid detection of gross errors while the earthquake traces are still available for display and verification or correction. A second essential program produces a map that shows the current epicenter and the stations that recorded the event. The station symbols encode information on the status of individual stations for the event studied: stations that belong to subnets that were "triggered" by the event in the on-line system, stations that were picked by the off-line RTP, and stations that were timed interactively in TIMIT.

When analysis of an event is complete it is "posted" to a "state" that permits it to move to the next stage of processing/data management in CUSP

Archived *event.grm* and *event.mem* files preserve not only the seismograms and supporting data but all of the analysis carried out on them in the CUSP system. The location program used in CUSP is not very flexible, however, and it is difficult to develop new programs in the CUSP environment; so standard HYPOINVERSE (or HYPO71) phase files are extracted from the *event.mem* files for further analysis. The central task in reanalysis is the relocation of all events by the program HYPOINVERSE (Klein, 1978), which employs different crustal models in different regions and calculates magnitudes according to a variety of systems, including XMAG and FMAG (Eaton, 1992A). The work is carried out in the DEC workstation cluster, where phase files and event summary files are organized in "monthly" directories. These data, as well as the *event.grm* and *event.mem* files are also transmitted to the Northern California Earthquake Data Center at U.C. Berkeley, where they can be accessed via the Internet.

III. Routine and research grade analysis of network seismograms In this section I shall review dramatic advances in the analysis of network data made possible by new technology, express some concerns that I have about current analysis practices, and indicate some corrective work that must be undertaken to correct errors in the data base. In the next section I shall illustrate some important types of analysis made possible by the high-quality data now available.

Routine analysis of the relentless stream of seismic traces collected by the network is an imposing task. As related in the previous section, the sophistication and efficiency of data recording and analysis equipment and processes have increased at least as dramatically as the number of stations in the network. Consequently, the number of people required for hands-on processing of network data, from identifying events to producing archived event summary and phase files, is no greater for the 600-trace 1995 NCSN net than it was for the 100-trace 1972 net; and the quality of analysis is much better for the 1995 net than the 1972 net because of the improvement in the manner in which traces are presented for analysis.

The principal task of the network analysis group is to determine accurate hypocentral locations, origin times, and magnitudes of events recorded by five or more stations (M1 to M1.5) and to preserve those results and their supporting data, in the network archives. For small events it is important that all clear traces be read for an accurate hypocentral determination. For larger events (M > 3), the situation is not so clear cut because first arrivals can be read at stations out to distances of 100 km or more. Pn emerges as a first arrival at about 100 km in central California, but its onset time can vary appreciably because of variations in crustal thickness. Inclusion of improperly modeled Pn arrivals can degrade hypocentral solutions, particularly focal depths; so wave arrivals beyond about 100 km are usually "weighted out" of the solutions. On the other hand, maximum wavelet amplitudes for large events are clipped at short distances and can be read only on more distant stations; so stations beyond the "cutoff distance" must be examined even though their arrival times are not used for determining the hypocenter. Such extended analysis can be very tedious, and the temptation not to read P-wave arrivals beyond 100 km and to settle for a minimum number of amplitude observations is very strong. For these reasons large earthquakes are analyzed incompletely during routine work. Such analysis is adequate for producing a preliminary earthquake catalog, but it falls far short of providing the material required for a competent treatment of large earthquakes.

I am also concerned by the sharp division of labor that has arisen in seismology with the advent of large networks and computerized analysis systems. Virtually all of the record analysis is carried out by technicians with little or no formal training in seismology, while network seismologists devote their efforts to processing the "numbers" provided by routine analysis. The resulting lack of contact with the raw seismograms robs researchers of an important means of discovering new features of wave propagation and earth structure revealed by the network.

For more than 15 years I have attempted to compensate for the abbreviated routine analysis of large earthquakes and the lack of really critical analysis of network seismograms by re-analyzing selected large events. Such reanalysis became feasible after the implementation of subtractive tape-speed compensation and the construction of an effective tape playback system in 1978. From 1978 to 1986 reanalysis was based on fast (25 mm/sec) paper playbacks of the events studied. Playback and analysis of a single M5+ event took two to three days. During this period my efforts were limited primarily to large earthquakes.

After the introduction of the first Menlo Park CUSP system in 1984 (on the 11/44's and the VAX750) the work went faster, but it still took most of a day to process an M5+ earthquake. The CUSP program for measuring maximum wavelet amplitudes and associated periods, which was written by Chris Stephens in 1987, was implemented on this version of CUSP and run on the VAX750. I used this augmented CUSP system to analyze all the events (Sept. 1985 to Sept. 1990) for my 1992 BSSA paper on magnitudes. These events included large numbers of small and medium-sized earthquakes in the northern part of the network and large numbers of early Loma Prieta aftershocks in addition to large earthquakes throughout the network.

The work was accelerated again with the introduction of the new CUSP system (microvax/3100) which reduced the time for analyzing an M5+ earthquake to two or three hours. Although the new system was available for use in 1990, I did not move from the old CUSP (on the VAX750) to the new CUSP (with TIMIT), until early 1992. Unfortunately, a "bug" in the routine for measuring maximum wavelet amplitudes and associated periods that was implemented in TIMIT went undetected for several years. The faulty routine correctly identified the data samples required for the purpose but the sub-routine that calculated the actual amplitudes returned incorrect values. The error has been corrected, but several years of faulty measurements now recorded in the archives must be corrected. The corrections can be made by a non-interactive procedure designed to recalculate amplitudes and periods on the basis of seismogram data points that have already been identified.

Since becoming proficient in the use of the CUSP analysis system in 1986, I have devoted about half of my time to the routine analysis of NCSN seismograms to improve the treatment of large earthquakes and to demonstrate my concern for the lack of general interest in the recorded seismograms themselves. In addition to analyzing most earthquakes in the network with magnitudes of 4 and greater, I have concentrated my effort on difficult-to-analyze early aftershocks of large quakes as well as events around the edges of the network and in sparsely instrumented regions within it. Since switching to TIMIT in 1992, I have analyzed nearly 15,000 earthquakes of all sizes on the CUSP system. The events that I analyzed can be identified in the CUSP system because the identity of the analyst is preserved in the *event.mem* files.

In addition to "research grade" analyses of the relatively rare large earthquakes recorded by the network, I have also analyzed the mainshock and aftershock sequences of several unusually interesting large earthquakes recorded by both the telemetered network and a supplemental 5-day-tape portable network. All of the data resulting from these special studies are included in the general NCSN data set, and they have labels that identify their origin and the analyst responsible for them.

My initial interest in the large earthquakes was aroused by their potential use to map variations in crustal structure across the network. Playbacks of large events from 1975 through 1979 were analyzed to determine the onset times and amplitudes of first P-wave arrivals for a study of Pn in the central Coast Ranges.

After two M6- earthquakes near Livermore in January 1980, my interest in large earthquakes expanded to include determination of first-motion focal mechanisms, magnitudes, and well documented hypocenters as well as the Pn observations. From 1980 to 1988, I played back most earthquakes in NCSN larger than about M4.5 from tape and analyzed them soon after they occurred. Particular care was taken in determining their hypocenters, often with refinement of the local crustal model to match the event travel time data. Magnitudes were computed from maximum wavelet amplitudes and associated periods by the method that I developed for the Parkfield aftershocks and programmed for inclusion in HYPO71. Most of the on-scale maxima for the larger events were from the subset of 3-component low-gain stations that had been installed to increase the dynamic range of selected NCSN stations. First-motion focal mechanisms were determined and reduced-time traveltime plots were constructed to round out the analysis. The purpose of this work was to supplement the routine NCSN data analysis, which did not provide reliable focal mechanisms and magnitudes of large earthquakes, to enhance the reliability and completeness of the NCSN catalog.

IV. Use of research-grade data from NCSN

Beyond the general upgrading of NCSN archive data for large earthquakes, my research-grade analyses have supported studies on several topical themes.

A. Crustal structure studies based on Pn arrivals.

1. Observations from about 30 M4 to M6 quakes from 1977 through 1979 were used in a time-term-difference analysis of Pn velocities beneath an overlapping chain of 50-km-diameter sub-networks that circled the San Andreas fault between Parkfield and Santa Rosa (Eaton and Mooney, 1979).

2 . Observations from 76 M3.3 to M6.5 earthquakes and one NTS blast between 1977 and 1982 were inverted to determine Pn velocities and the depth and orientation (strike and dip) of the Moho beneath sub-nets covering portions of the Coast Ranges, Great Valley, and Sierra Nevada foothills in central California (Oppenheimer and Eaton, 1984).

3. Earthquake refraction profiles of the Sierra Nevada root were constructed from CUSP analyses of eight strategically located earthquakes and one NTS blast (1983-1992) as well as earlier data from the 1966 Truckee earthquake (Savage, Li, Eaton, Jones, and Brune, 1994).

B. Large earthquakes and their aftershocks

1. Playbacks from NCSN stations and eight 5-day-recorder stations deployed along the coast between Cape Mendocino and Crescent City were analyzed to delineate the causative fault of the November 8, 1980 M7.2 Eureka earthquake. The aftershocks defined a 140-km-long NE-trending zone that ran from just north of the Mendocino Fracture Zone 125 km west of Cape Mendocino to a point on the continental shelf about 30 km NW of Trinidad Head (north of Eureka). The main shock occurred in the zone of aftershocks about 20 km SW of its NE end (Fig. 20). The N54°E strike of the left-lateral strike-slip first-motion focal mechanism of the main shock parallels the trend of the aftershock zone (Eaton, 1981).

2. The M6.7 May 2, 1983 Coalinga earthquake and its protracted aftershock sequence occurred beneath the eastern edge of the southern Coast Ranges section of NCSN at the boundary between the Coast Ranges and the Great Valley. The network was augmented rapidly by the addition of four new permanent stations spanning that boundary east of Coalinga, and by 12 portable 5-day-tape recorder stations in and around the aftershock region. The more than 6000 aftershocks located by the RTP between May 2 and September 30 overwhelmed the pre-CUSP analysis system then in use; so a hybrid RTP/tape-playback analysis system was developed to study the sequence. In the first iteration (Eaton, Lester, and Cockerham, 1983), the main shock and nine of its largest aftershocks were studied in detail from tape playbacks to

develop an improved velocity model and associated station delays, but the bulk of the aftershocks were located by a careful screening and reanalysis of the RTP arrival time and duration data (Figs. 21, 22, and 23). In the second iteration (Eaton, 1990), more than 130 additional M3+ aftershocks were played back from both the portable tapes and the NCSN tapes for detailed hand analysis.

Focal mechanisms of the main shock and most of the 143 aftershocks analyzed (by hand) were compressional and indicated contraction across the Coast Ranges/Great Valley boundary beneath Anticline Ridge and adjacent Guijarral Hills (Fig 24). The initiating fault was found to be a low-angle thrust fault that appears to underlie most of the 30-km-long by 20-km-wide aftershock zone, has a strike of about N53°W and a dip of 23°SW (Fig. 23), and passes beneath Anticline Ridge at a depth of about 10 km.

3. The April 24, 1984 M6.2 Morgan Hill earthquake contrasted sharply with the Coalinga quake in many respects: it resulted from right-lateral strike-slip movement on the near-vertical Calaveras fault, in a well developed part of NCSN, and produced an aftershock pattern that was remarkable for its simplicity. The network was augmented rapidly with six 5-day-tape recorder stations and 14 digital GEOS stations to optimize coverage in the aftershock region. The primary analysis of the aftershocks was carried out on the newly implemented Menlo Park CUSP system; but I analyzed playbacks from both NCSN and SCSN to determine hypocenter, focal mechanism, and magnitude of the main shock (Eaton, 1987).

"Aftershocks of the earthquake clearly outline a near-vertical fault that is parallel to, but much simpler in structure than, the mapped strands of the Calaveras fault in the aftershock area (Fig. 25). A central quiet zone in the aftershock pattern on the inferred fault surface marks out adjacent 3- by 10-km and 5- by 13-km patches that are believed to outline the rupture region of the main shock (Fig. 26). The transition between these two patches coincides with a 6° change in the strike of the inferred fault surface. A conspicuous group of aftershocks southwest of the principal band of aftershocks appears to be bounded by the Calaveras fault on the northeast and by the Silver Creek (or related, reverse) fault below and on the southwest. These aftershocks suggest that the Morgan Hill earthquake was accompanied by, or stimulated, movement on one or more nearby reverse faults." (Eaton, 1987, USGS Bull. 1639: Cockerham and Eaton, 1987, USGS Bull. 1639).

4. The M5.7 August 4, 1985 Kettleman Hills earthquake, like the M6.7 May 2, 1983 Coalinga earthquake, occurred along the very eastern edge of the southern section of the of the Coast Ranges network. Eight 5-day-tape recorder portable stations were rushed to the field to reinforce the network to record the aftershocks. The main shock, two foreshocks, and the 39

largest aftershocks were played back from the 5-day and NCSN tapes; and I analyzed them by hand to determine their hypocenters and magnitudes. NCSN records of the smaller aftershocks were timed interactively on the CUSP system. The 5-day-tape records were played back and processed by an off-line version of the Allen/Ellis RTP to determine P-wave onsets and event duration times.

In a preliminary analysis, the Kettleman Hills earthquakes were located by HYPO71 with the crustal model that I developed for the Coalinga earthquake sequence; and focal mechanisms for the main shock and ten of its largest aftershocks were determined graphically from P-wave first motion printerplots (Fig. 27) (Eaton, 1985; Eaton, 1986B). The same data set was analyzed more extensively in a broader study of the seismicity of the combined New Idria-Coalinga-Kettleman Hills seismic zone (Ekstrom, Stein, Eaton, and Eberhardt-Phillips, 1992; Stein and Ekstrom, 1992).

Focal mechanisms for 30 of the hand timed events (two foreshocks, main shock, and 27 aftershocks) indicate contraction across the Coast Ranges/Great Valley boundary in a direction perpendicular to that boundary and the axis of Kettleman Hills (Fig. 28). The initiating fault is a low angle thrust fault with a strike of about N53^{0}W and a dip of about 14^{0}SW that appears to underlie much of the 20-km-diameter aftershock zone and passes beneath Kettleman Hills at a depth of about 12 km. The area of the Kettleman Hills aftershock zone is about half that of the Coalinga aftershock zone, and the Kettleman zone immediately abuts the southeast end of the Coalinga zone without overlapping it (Fig. 27). The number of Kettleman aftershocks, however, is only about one tenth the number of Coalinga aftershocks.

From analyses of teleseismic broadband seismograms of the two earthquakes, the duration and maximum moment rate of the two sources were 5 seconds and 1.25 10^{25} dyne cm/sec for Coalinga and 16 seconds and 0.2 10^{25} dyne cm/sec for Kettleman Hills.

5. The October 18 (UT) M7 Loma Prieta earthquake and its aftershock sequence strained NCSN and the CUSP recording system to the breaking point. An important part of the network southeast of the epicenter failed immediately after the main shock when telephone service at the San Juan Bautista exchange was disrupted by strong ground shaking. Additional phone lines in the same region failed several hours later as emergency back-up power supplies ran down. The initial CUSP trigger for the main shock lasted for 27 minutes. For several hours after that, CUSP triggers (and sections of the digitized records that were saved) were somewhat erratic because of high aftershock activity throughout the epicentral region. Still later, the CUSP disk that saved seismograms was overfilled and its contents were rendered indecipherable.

I developed a scheme to "map" network outages on a phone-line by phone-line basis as a function of time so that we could lay plans to recover data lost by CUSP by playing back appropriate groups of recorded phone lines.

To identify aftershocks that were recoverable from the high background noise level in the aftershock region, I selected a set of critical low gain stations in and near the aftershock zone for plotting on a multi-trace strip chart record from the CUSP triggers. Two of these stations were the low-gain vertical component at Stanford (JSFZ), and the vertical component of an ultra-low-gain 3-component L4C station in Menlo Park that fed directly into the CUSP A/D converter (JMPF; gain equivalent to 72db attenuation, compared to 42db attenuation for JSFZ). The JMPF trace for the first 27-minute-long trigger identified recoverable events for that interval, and the JSFZ trace identified events of about M2 and larger as background noise diminished.

I undertook the analysis of the first 17 hours of the Loma Prieta sequence on the CUSP system that was implemented on the VAX750. For the first half hour, the only readable near-in stations were the NCSN low-gain (42db attenuation) stations, which fortunately were deployed rather evenly around the aftershock region. During this interval, the smallest shocks recovered were about M3. After 17 hours, the smallest shocks recovered were M1.5 to M2. A total of about 200 aftershocks were recovered during the first 17 hours. This number represents only a fraction of the aftershocks that occurred but were lost in the jumble of tiny events occurring throughout the aftershock region. After the first day, I concentrated on Loma Prieta aftershocks of M3 and larger.

As there were several people standing by to analyze the Loma Prieta sequence, I continued to concentrate on research-grade analysis of the seismograms and did not participate in further analysis of the data set that I was helping to generate.

6. Continuing interest in earthquakes near the Mendocino triple junction has focussed my CUSP analysis efforts particularly on that region. I have attempted to analyze most of the smaller earthquakes (M1.5 to M3) from the northern end of NCSN as well as the large ones. These events include the mainshock and aftershock sequences of the August 17, 1991 M6.0 Honeydew earthquake, the M7.1 April 25, 1992 Petrolia earthquake, and the two April 26, 1992 M6.6 earthquakes offshore near Cape Mendocino (Oppenheimer, Beroza, Carver, Dengler, Eaton, and 15 others, 1993). My purpose has been to insure uniformity and completeness in the analysis of earthquakes in this important region which has relatively sparse network coverage.

C. Tectonic analysis

As time passed, the length and quality of the northern California microearthquake catalog and the number and areal coverage of large hand-processed earthquakes increased steadily. The resulting expansion of the material available for study stimulated periodic reanalyses to assess the effectiveness of the network and the level of our understanding of the earthquake generating system.

1. An early study of temporal variations in the pattern of seismicity was reported at a UNESCO symposium on earthquake prediction in Paris in 1979 (Eaton, 1984C). Examination of annual plots of earthquakes located by NCSN from 1970 through 1977 showed several superposed patterns of earthquake distribution (Fig 29):

a) linear concentrations of epicenters, of rather uniform density, along selected portions of the principal faults of the region, which are repeated with little variation from year to year.

b) episodes of seismic activity that spring up suddenly and die out slowly over a period of a year or more: these episodes are moderate isolated earthquakes and their aftershocks, and they occur both on and away from well-recognized faults.

c) scattered epicenters throughout seismically active portions of the Coast Ranges, from the Pacific shore to the western edge of the Great Valley.

2. Focal mechanisms, magnitudes, and hypocenters of six near-shore earthquakes between Santa Barbara and Monterey were determined by hand analysis of tape playbacks from NCSN and SCSN (Eaton, 1984A). The focal mechanisms of these six earthquakes show a progressive change in character as a function of their position along the coast (Fig 30). From southeast to northwest:

Name	Date	Mag	Focal Mechanism
Santa Barbara	780813	5.9	left-lateral reverse oblique
Santa Maria	820929	4.0	left-lateral reverse oblique
Point Sal	800529	5.1	reverse
San Simeon	830829	5.4	right-lateral reverse oblique
Pinon Point	830721	3.9	right-lateral strike slip
Point Sur	840123	5.2	right-lateral strike slip

The last event is on-line with the San Gregorio-Palo Colorado fault that runs across the mouth of Monterey Bay, and the fault plane in the solution is parallel to the fault.

3. The potential for future large earthquakes along the west side of the Great Valley opposite Sacramento was addressed by comparing the tectonic environment of that region with that of the Coalinga/Kettleman Hills region 300 km to the southeast (Fig. 31) (Eaton, 1986A). Both regions are characterized by folds in the Great Valley sediments just east of the Coast Ranges/Great Valley boundary that appear to be caused by eastward thrusting of a tectonic wedge of Franciscan formation between the Valley basement and overlying sediments at a depth

of about 10 km (Wentworth, et. al., 1984). The largest earthquake in the studied region between 1972 and 1985, the M4.3 780908 Winters quake, had a focal depth and focal mechanism similar to those of the Coalinga and Kettleman Hills main shocks. The M6.4 1882 Vacaville/Winters earthquake originated about 10 km south of the epicenter of the 1978 quake. Its aftershock region appears to have encompassed less than half of the region that appears to be capable of producing such an earthquake.

4. Short term (Jan. 1982 through April 1983) (Fig. 32) and long-term (Jan. 1972 through April 1983) (Fig. 33) seismicity patterns in the central and southern Coast Ranges and focal mechanisms of moderate-to-large earthquakes, which occurred on the principal faults of the San Andreas system as well as along both flanks of the Coast Ranges, were analyzed for evidence on the processes that generate earthquakes in the region (Eaton and Rymer, 1990). Creeping sections of the major faults are marked by dense, very narrow bands of earthquakes; and focal mechanisms of M4 to M6 earthquakes along them indicate right-lateral strike-slip displacement on near-vertical surfaces that parallel the faults (Figs. 34 and 35). Earthquakes with reverse and thrust mechanisms occur in regions with a distinctive pattern of seismicity (characterized by clusters of epicenters) that lie along both flanks of the southern Coast Ranges and are separated from the San Andreas by regions of relative seismic quiescence. Focal mechanisms of earthquakes in these regions indicate crustal contraction, nearly perpendicular to the San Andreas fault, along the Pacific shoreline as well as across the Coast Ranges/Great Valley boundary.

In the proposed model (Fig. 36), the misalignment of the San Andreas fault (N41°W) and the Pacific/North American transform boundary beneath it (N35°W, Fig. 32) is taken to be the proximate cause of an apparent widening of the southern Coast Ranges that drives the brittle upper crust out across the margins of the transform that underlies the central axis of the Coast Ranges.

Lines on the northeast and southwest sides of the San Andreas drawn parallel to the transform (and regional relative motion of the plates) move apart at approximately one tenth the rate of accumulating movement along the San Andreas. Reverse and thrust fault earthquakes occur where detachment zones extending outward from the San Andreas, within a ductile lower crust above the transform zone, pass beyond the edges of the transform zone and curve upward through the brittle crust above its margins.

The fault plane solutions described in this paper indicate that earthquakes in the southern Coast Ranges do not result from a broad, uniform regional stress field acting on the crust across the region. Rather, earthquakes occur in response to a spatially varying stress field induced in the brittle crust by the slow deformation imposed on it by the relative motion of the Pacific and North American plates across the transform zone, as modulated by the interaction between crustal units with different elastic properties. To understand the forces at work in the crust and upper mantle we should focus our attention on crustal deformation, not stress. Stress is an inferred condition induced in the crust in response to the elastic deformation to which it subjected.

5. The spatial distribution of earthquakes located by NCSN in northern California from 1980 through 1986 (Fig. 37) was compared with the principal faults and other geologic features of the region by means of maps and cross sections (Eaton, 1989). Analysis was focussed on the two broad zones of seismicity that dominate the map: the Coast Ranges seismic zone between Cholame on the south and Laytonville on the north, and the Mendocino seismic zone within a radius of about 100 km around Eureka. The principal tectonic conclusions from this analysis are summarized below.

a) Mendocino

"The pattern of earthquakes in the Mendocino seismic zone suggests that the southeastern corner of the Gorda plate is being crushed against the northeastern corner of the older, stronger Pacific plate in consequence of the convergent component of their relative motion in the region. The larger right-lateral strike-slip component of relative motion should carry the disrupted remains of the south edge of the Gorda plate eastward beyond the east edge of the Pacific plate, where these remnants appear to be obducted onto (or against) the western edge of the North American plate. Subsequent northwestward motion of the Pacific plate relative to the North American plate should entrain the Gorda-plate debris in the boundary zone between those two plates in the northern Coast Ranges."

"The continuity of the grossly furrowed topography onshore between Cape Mendocino and Trinidad Head with that east of the Maacama fault farther south suggests that these regions share important features of origin and internal structure. The Franciscan Complex, which largely coincides with that topography, contains much material that appears to have been crushed and mixed by a process like that now affecting the southeastern corner of the Gorda plate."

"The torque required to rotate the Gorda/Juan de Fuca plate in a clockwise direction, and to maintain the plate-crushing contact between the Gorda and Pacific plates just west of Cape Mendocino, may be produced by the southeastward drag along the eastern edge of the Gorda plate caused by its oblique subduction beneath the North American plate. Left lateral faulting on northeast striking faults across the southeastern corner of the Gorda plate, such as occurred during the November 8, 1980 Eureka earthquake, is a consequence of the strong normal forces developed across the eastern end of the Mendocino fracture zone by the process outlined above."

b) Coast Ranges

"The driving force behind earthquakes in the Coast Ranges seismic zone is the transform (boundary) between the North American and Pacific plates. Details of the gross physical properties of this boundary and how it works are not well understood."

"The principal observations from contemporary seismicity and the historic seismic record can be summarized by the following statements: (1) The maps and cross-sections (presented above) show that the Coast Ranges seismic zone is complex and that its most prominent features vary from northwest to southeast within it. (2) Most of the displacement between the Pacific and North American plates at the earth's surface occurs on the San Andreas fault. (3) Where the San Andreas crosses the center of the Coast Ranges, between Cholame and Corralitos, most of the displacement occurs as creep accompanied by countless small earthquakes; where it lies along the edges of the Coast Ranges, most of the offset occurs during infrequent, very large earthquakes. (4) Long-term offset rates (over hundreds of years) across the Calaveras/Maacama and Greenville/Bartlett Springs fault zones, which are marked by linear concentrations of small earthquakes like that along the rapidly creeping section of the San Andreas, are small compared to that on the San Andreas (now locked) farther west. (5) The trace of the San Andreas fault between Point Arena and Cape Mendocino lies offshore and is difficult to identify. It is believed to deflect about 40 km to the east and generally to follow the coast north of Point Arena. The long-term offset rate on this section should be the same as that south of Point Arena, but the total offset across it should decrease to zero as it approaches the triple junction, which is its point of origin. (6) The depth to the base of the seismogenic zone along the principal strike-slip faults in the Coast Ranges averages about 10 km north, and 12 to 13 km south, of Clear Lake. The depth to the base of the seismogenic zone appears to increase gradually from west to east across the Coast Ranges. This effect is most pronounced along the eastern margin of the Coast Ranges and the adjacent Great Valley, where focal depths are as great as 15 to 25 km. Earthquakes deeper than 15 km (in the lower crust) are extremely rare elsewhere in the Coast Ranges but common beneath the Great Valley. (7) Both flanks of the Coast Ranges southeast of Hollister are marked by scattered patches and broad zones of earthquakes whose sources indicate crustal compression normal to the San Andreas fault. These earthquakes may be caused by lateral spreading of the southern Coast Ranges resulting from misalignment between the San Andreas fault in the upper crust and a more northerly trending transform in the mantle below. Such a misalignment is suggested by recent studies of global plate-motion directions."

"From the distribution of current seismicity and the position of the San Andreas fault, it appears that the entire Coast Ranges is underlain by a broad zone of right-lateral shear deformation. The restriction of earthquakes to the upper crust beneath the Coast Ranges, but not beneath the contiguous portions of the Great Valley on the east or the Mendocino seismic zone on the north, suggests that the lower crust deforms plastically beneath at least the central part of the Coast Ranges. The branching and spacing of major strike-slip faults in the upper crust north of Hollister suggest some sort of decoupling between the brittle upper crust and the plastic lower crust. The parallel, subequally spaced traces of the three major branches northwest of Livermore suggest that sections of the brittle upper crust resist internal deformation and concentrate distributed displacements beneath them onto their fault boundaries."

"Decoupling of the upper and lower crust in the southern Coast Ranges can also account for the relatively aseismic zones lying between the actively creeping San Andreas fault and the zones of compression and reverse-fault earthquakes along both the east and west flanks of the Ranges. Horizontal decoupling horizons 12 to 15 km deep beneath the center of the Ranges curve upward through the brittle upper crust where it is driven beyond the margins of the plastic zone (over the transform) in the lower crust."

6. For USGS Professional Paper 1515, "The San Andreas Fault System: Chapter 5, Seismicity, 1980-1986" the NCSN and SCSN catalogs were combined for an analysis of the seismicity of the entire San Andreas fault system in California (Hill, Eaton, and Jones, 1990). The overall patten of seismicity and its regional variations were presented and analyzed on a state-wide map (Fig. 38). More detailed analyses based on maps and cross sections were presented for each of five characteristic regions of the fault system: (1) the Mendocino triple junction, (2) the 1906 break and the northern Coast Ranges, (3) the central creeping section, (4) the 1857 break and the Transverse Ranges, and (5) the southern section of the San Andreas fault system. The first three of these regional analyses updated material from northern California presented in Eaton (1989), and the last two extended the same treatment to southern California (SCSN). The section on "Focal mechanisms and transform boundary kinematics" (Fig. 39) analyzes 106 focal mechanisms (mostly M3.5 to M7 events between 1977 and 1987) distributed throughout the state in conjunction with material from the five regional analyses. The results of this analysis led to discussions of (1) strike-slip kinematics of the San Andreas fault system, (2) crustal convergence adjacent to the San Andreas fault system, (3) fragmentation of the southeast corner of the Gorda plate, (4) east-west extension in the Sierra Nevada, and (5) conjugate faulting in the Sierra Nevada-Great Basin boundary zone.

The two largest earthquakes in California during the interval 1980-1986 occurred off the faults of the San Andreas system, and their occurrence emphasizes the importance of deformation within the plate margins along the San Andreas transform boundary. The M7.2 Eureka event (Nov. 8, 1980), for example, involved deformation internal to the Gorda plate; and the M6.7 Coalinga event (May 2, 1983) involved crustal shortening with reverse slip perpendicular to the San Andreas fault. These two earthquakes and the many smaller "off fault" events reflect local deviations from the rigid-plate approximation of plate tectonics.

V. Completing a chapter on Kilauea

Analysis of the seismic and tilt records of the 1959-1960 Kilauea eruption was incomplete when I left Hawaii; so the critical records were sent to Denver where I continued to work on them. Strange offsets of the rest position (or zero-line) of the horizontal-component Press-Ewing seismographs at Uwekahuna, which Harold Krivoy had correlated with the start and finish of individual phases of the Kilauea Iki eruption (Fig. 40) (Richter, et. al, 1970), were particularly intriguing. The information embodied in these offsets was difficult to see in the raw records: even short eruptive phases were drawn out over several revolutions of the seismograph drum, i.e., several meters of the seismogram, and the offset of the zero-line was comparable to (i.e., 1- to 2-times) the double amplitude of 4-second-period oceanic microseisms that pervaded the records. I devised a template to estimate the normal rest position of each of the 24 one-hour-long traces on the seismogram as well as a transparent measuring overlay on which a millimeter scale was marked off relative to a 4-inch-long "baseline". When the transparent overlay was laid on the seismogram at the time a trace offset measurement was to be made and the baseline was adjusted to match the local "average" position of the trace (underlying the microseisms), the offset of the trace could be measured with respect to the appropriate "undisturbed" trace on the template by means of the millimeter scale on the overlay. By this method, zero-line offset measurements were made approximately at 5-minute intervals throughout the Kilauea Iki eruption.

The Press-Ewing seismographs, along with a two-component liquid-level tiltmeter that is read once per day, are in the Uwekahuna Vault, which is about 2 km WNW of the center of Kilauea Caldera and about 4 km W of the principal eruptive vent at Kilauea Iki (Fig. 41). It can be shown that the zero-line offset of the E-W component Press-Ewing seismograph is proportional to the rate of eastward tilting of the earth's surface at Uwekahuna; and the constant of proportionality can be calculated from the Press-Ewing constants. When east-west rate of tilting at Uwekahuna was plotted at a time scale that permitted easy comparison with other observations relating to the eruption, some striking correlations appeared (Fig. 42). For "simple" eruptive phases, like phase 5 (Fig. 43): a) eastward tilting began when the fountain started, continued at a rate proportional to fountain height, and terminated abruptly when the fountain died; b) rapid westward tilting commenced immediately after the fountain died when backflow of lava into the vent began, and westward tilting declined gradually as backflow rate diminished; c) the total area under the "backflow" westward tilting part of the curve was approximately equal to the total area under the "eruption" eastward tilting part of the curve (i.e., the backflowing lava refilled the reservoir and restored the tilt at Uwekahuna to its pre-phase 5 level); and d) strong harmonic tremor accompanied the fountaining and ceased when the fountain died. The relationship among fountain height, harmonic tremor amplitude, and rate of eastward tilting became more complex during later eruptive phases, however; and I set aside the analysis in the mid-1960s as my involvement in the earthquake program increased.

Twenty years later, I returned to the Kilauea Iki records as a means of tracking the movement of magma from depth into the Kilauea summit reservoir, from the reservoir into Kilauea Iki lava lake and then back into the reservoir, and from the reservoir into the east rift zone. That work was carried out in preparation for the HVO Diamond Jubilee Symposium in 1987. The outcome of the new effort is summarized by the abstract of the paper that was prepared for the symposium volume (Eaton, Richter, and Krivoy, 1987).

"The fortunate combination of observations at the summit of Kilauea Volcano during the 1959 Kilauea Iki eruption and an accurate log of the eruption of lava into Kilauea Iki lava lake and its subsequent partial withdrawal provides a record of the movement of lava into and through the summit reservoir during that eruption. The initial eruption of 30 10^6 m^3 of lava into Kilauea Iki, with an accompanying 50 μrad of eastward tilt at Uwekahuna, established a scaling factor relating change in volume of the summit reservoir to tilting at Uwekahuna. A record of the initial deflation and subsequent reinflation of the summit reservoir was obtained by the liquid-level tiltmeter at Uwekahuna (Fig. 42). A good record of episodic eastward tilting during the eruptive stage and westward tilting during the backflow stage of the later phases of the eruption was obtained by analysis of the zero-line deflections of the east-west component Press-Ewing seismograph (Fig. 43). Converted to volumes of deflation and reinflation, the tilting associated with eruption of lava into and withdrawal of lava from the lava lake offers a means of determining the net gain or loss of lava in the lake-reservoir system during each phase. Analysis of the interplay between volcanic tremor and the details of the reinflation tilt records permits estimation of the volume of lava that escaped from the summit reservoir during individual backflow episodes."

"The 30 10^6 m^3 of lava originally withdrawn from the reservoir was eventually more than replaced by about 60 10^6 m^3 of lava rising from depth. Of this 60 10^6 m^3, 40 10^6 m^3

finally was retained in the reservoir, 8 10^6 m^3 was added to the lava lake, and about 12 10^6 m^3 was driven out of the summit reservoir during the late-phase backflow episodes. Lava that erupted into Kilauea Iki from phase 2 onward was material from the summit reservoir, which was being refilled with new lava from below; this lava was repeatedly erupted from the reservoir and then withdrawn back into it. Overfilling of the reservoir, combined with sharp reinflation pulses during later backflow episodes, drove an estimated 12 10^6 m^3 of lava out of the reservoir, possibly into the upper end of the east rift zone."(Fig. 44a).

"The recycling process was made possible by a lava conduit, which remained open after phase 1, that connected a supply of lava in the lake with one in the reservoir. Large-scale vesiculation of lava in the conduit effectively pumped lava from the reservoir up into the lake. Flooding of the vent by devesiculated lake lava quenched the fountain and permitted lake lava to flow back into the reservoir. The process was rekindled repeatedly, after backflow stopped, by the introduction of fresh gas-charged reservoir lava into the conduit, which permitted the vesiculation pump to operate again". (Fig. 44B).

VI. Determination of amplitude and duration magnitudes from NCSN seismograms

One of the most important descriptors of an earthquake is its magnitude. Determination of magnitudes from early NCSN seismograms recorded on Develocorders was difficult because of the high sensitivity and low dynamic range of the seismic system. Calculation of amplitude magnitudes from similar seismographs employed with the 10-day-tape recorder stations was feasible because they were recorded at both high and low gain levels and were played back on easily readable paper strip-chart recorders (Eaton, O'Neill, and Murdock, 1970); but as a rule, the peak amplitudes and associated periods required for such calculations simply could not be read on the NCSN Develocorder films.

A stop-gap solution for determining NCSN magnitudes was devised by Lee, Bennet, and Meagher (1972) on the basis of earthquake duration measurements on the Develocorder films from the standard high-gain verticals. Their "FMAG" scale was calibrated by comparison with Wood-Anderson magnitudes (mostly M2 to M4) and 10-day-tape amplitude magnitudes (mostly M0.5 to M2) for about 350 events for which such measurements were available in 1972. From 1972 to 1992, FMAG was the standard magnitude reported in the NCSN catalog.

Comparison of FMAG with U.C. Berkeley magnitude for larger quakes has shown that FMAG seriously underestimates magnitudes of such earthquakes, beginning at about M3.5 After 1980, magnitudes of many of the larger NCSN earthquakes were computed from peak amplitudes and associated periods, measured on playbacks of NCSN tapes, by a method that was adapted from the 1966 Parkfield aftershock paper (Eaton, O'Neill, and Murdock, 1970) and implemented in HYPO71. The 3-component low-gain NCSN stations were particularly important for this purpose. The "XMAG" values so obtained generally agreed well with those reported by U. C. Berkeley.

Over the last decade, several formulations of amplitude or duration magnitude based on subsets of NCSN instruments have been reported. Bakun and Joyner (1984) employed records from the low-gain horizontal component instruments, transformed into the equivalent Wood-Anderson seismograms, to compute standard M_L magnitudes in the manner of Richter. They derived a zero-magnitude-earthquake amplitude-vs-distance curve from the simulated NCSN Wood-Anderson records and real Wood-Anderson records from the UC Berkeley network. Bakun (1984) recast duration magnitude in terms of $\log\tau^2$ and D, where τ is the event duration measured from the onset of P and D is epicentral distance, rather than $\log\tau$ and D. Michaelson (1990) defined a "duration" magnitude in terms of $\log\tau 1$ and $\tau 1$ (where $\tau 1$ is lapse time, i.e., cut-off time minus origin time), with no term in D. Both Bakun and Michaelson, like Lee, et. al., used only the normal high-gain vertical component records. Hirshorn, Lindh, and Allen (1987) developed a formulation of duration magnitude that employed records from the low-gain vertical component seismographs for use with the RTP for earthquakes in the M3 to M6 range.

Chris Stephens' development (in 1987) of a "trace editor" that permitted interactive measurement of maximum wavelet amplitudes and associated periods on the CUSP system began a new era in NCSN magnitude determinations. The use of maximum wavelet amplitude and associated periods for computing amplitude magnitudes, which previously required laborious tape playback and hand analysis of the seismograms, became feasible for all NCSN earthquakes. This new CUSP feature supplemented an existing CUSP routine that automatically measured the event duration times required for calculating duration magnitudes.

No method for computing either amplitude or duration magnitude from the wide range of instrument types (high and low gain, vertical and horizontal component) used in NCSN had been adequately documented; so I undertook a review and reformulation of both types of magnitude on the basis of earthquakes that I analyzed on CUSP for the interval 1986 to 1990. The results from that effort can be summarized by quotes from the abstract and discussion section of the resulting BSSA paper (Eaton, 1992A).

"Equations for determining amplitude magnitude (MX) and duration magnitude (MF) that employ all calibrated instruments in the USGS short-period telemetered seismic net in northern California (NCSN) were developed and tested against a set of 1276 earthquakes from 1986 to 1990 that were analyzed on the Caltech-USGS processing system (CUSP) (Fig 45A). The expressions for decay of amplitude and record duration in these equations are functions of distance alone. Sensitivity corrections for both MX and MF are simply the logarithms of the ratios of the magnification of the reference instrument to that of the instrument actually used. Component corrections were chosen so as to minimize the dependence of instrument site residuals on instrument component."

"MX was designed to approximate the Wood-Anderson local magnitude, but with modified input. Maximum peak-to-trough amplitude (and associated period) is chosen without regard to where it occurs on the seismogram: P, S, or surface waves. This choice was made because NCSN instruments are predominantly verticals."

"MX and MF are based on complementary aspects of earthquake seismograms, and both constitute valid estimates of the size of the earthquake. MX is more directly related to the Wood-Anderson local magnitude than MF; and MX can be determined from records that do not support MF determinations, e.g., very small earthquakes and earthquakes followed too soon by another earthquake. With present NCSN instrumentation and analysis procedures, however, far more measurements are normally available of duration than amplitude and associated period. This result stems from the fact that even severely clipped seismograms of large earthquakes near their epicenters permit measurement of duration, whereas unclipped records are required for measurements of amplitude."

"Records from short-period NCSN seismographs of all sensitivities and components can be used directly to compute MX and MF. Although MX was designed to approximate M_L as closely as possible and agrees with MBK (M_L from U.C. Berkeley) from M2.5 to M5.5, MX is a primary scale, as is M_L. Because MX can be computed from records of sensitive seismographs at small epicentral distances, it provides an amplitude magnitude for very small earthquakes. MF was designed and scaled to match MX as closely as possible; so it is a secondary scale. MF agrees closely with MX from M0.5 to M5.5." (Fig. 45B).

"MX and MF site residuals are closely linked: MF residuals averaged over 0.05 unit intervals of MX residuals indicate a near one-to-one correspondence between the two residuals (Fig. 46A). Both MX and MF site residuals depend systematically on bedrock lithology: older, more consolidated (metamorphic and igneous) rocks have negative site residuals, and younger, less well-consolidated (sedimentary and volcanic) rocks have positive residuals." (Fig. 46B).

"For the 293 events for which MBK's were available, the average MX-MBK and MF-MBK are small (0.04 for both MX and MF) and show little variation with magnitude. The standard deviations of the magnitude differences are 0.18 for MX and 0.23 for MF."

The values of $\log A_0$ versus distance proposed in this paper (MX) and those given by

Gutenberg and Richter (1942), Bakun and Joyner (1984), and Hutton and Boore (1987) were compared with the empirical data set for northern California developed for this paper (Fig. 47). The good agreement between the MX curve and the empirical data set is expected because they are based on the same observations. The agreement between the BJ curve and the empirical data set is good out to about 400 km, which was the limit of their observations (in northern California), but is poor at greater distances. The discrepancy between the HB curve and the northern California empirical data set is not surprising because the HB curve is based on southern California observations. The Gutenberg and Richter curve was based largely on southern California observation and was very poorly defined at short distances.

An analysis of the periods associated with the maximum wavelets used to compute MX shows a striking correlation of log(period) with magnitude. Data for the case in which the maximum occurred in S or later (MAX) were plotted separately from the case in which it occurred in P (PMAX) (Fig. 48). The two curves are described by:

$$\log(PMAX) = -1.13 + 0.19M$$

$$\log(MAX) = -1.00 + 0.19M$$

At a given magnitude $T_{MAX} = 1.35T_{PMAX}$; and both T_{MAX} and T_{PMAX} increase approximately as the fifth root of MX.

The magnitude subroutines and supporting materials (site corrections, etc.) developed in my work on magnitudes have been incorporated into HYPOINVERSE and have been used to calculate MX and MF for NCSN events since 1992.

VII. Recent work on seismometer calibration and seismic system response

A. Seismometer calibration

The Mark Products L4-C seismometer that was adopted for use as the standard NCSN seismometer in the early 1970s is a sealed unit, and all communication with the seismometer moving system is through the seismometer output cable that is connected to the seismometer coil. Theory shows that a steady electrical current passing through the seismometer coil offsets the seismometer moving system from its rest position and holds it there as long as the current is maintained. If the current is cut off abruptly and the seismometer cable is connected to an external load (recorder), the emf produced in the seismometer coil as the moving system moves back to its rest position drives a current through the coil and the external load. The detailed motion of the seismometer moving system and the resulting emf and current induced in its coil can be expressed in terms of the offsetting current and the constants of the seismometer: mass, natural frequency, damping constant, motor constant, and resistances of the seismometer coil and the external circuit. If the offsetting current, seismometer moving system mass, and coil and circuit resistances are known, the seismometer natural frequency, damping constant, and motor constant can be deduced from the seismometer response to such a "release test".

A test circuit, typical current release test record (diagram), and equations for interpreting the record to obtain the seismometer constants were presented in my first open-file report on the NCSN seismic system response (Eaton, 1975A); but the derivation of the interpretive equations was omitted. The test was recorded on a "storage" oscilloscope that had relatively poor resolution.

The simple theory of a moving coil seismometer neglects the effect of self induction in the seismometer coil because of the low frequency of seismic waves. When the offsetting current in the release test is cut off abruptly, however, the collapsing magnetic field in the coil induces a sharp, narrow spike in the output voltage that begins when the circuit is broken and lasts only a few milliseconds. To avoid the question of how that spurious spike might affect the initial motion of the seismometer in the current release test, I did not use the offsetting current to calculate the seismometer parameters. The ratios of subsequent sequential extrema of the test record provide sufficient information to determine the seismometer constants. The program that I wrote to implement the release test was incorporated into a somewhat more comprehensive calibration program by Elliot Endo. Elliot's program has been used to calibrate OEVE seismometers for the last 20 years.

Several years ago Mark Products introduced an automated computer-based seismometer calibration system that employs an "open circuit" current release test that requires the use of the ratio of the offsetting current to the first extremum of the release test. Because that calibration system is fast and effective, I turned my attention to the quantitative evaluation of the current release "spike". A current-release-test laboratory experiment carried out with John VanSchaacks' help examined the nature of the voltage spike that accompanies the cutoff of the offsetting current and compared it with the expected seismometer response to the test. The current release test was carried out, first, with a vertical component L4-C seismometer in an upright operating position and, then, with the same seismometer inverted so the mass was immobilized (Fig. 49). The effect of self inductance is to delay the cutoff of current through the coil when the offsetting voltage is removed: the current decays exponentially and approaches zero after about 4 milliseconds in the set-up we employed. During that interval an "unmodelled" current pulse equal to 0.0238 10^{-6} amp-sec (= $i_0 L/R'$, where i_0 is the initial current, L is the self inductance of the seismometer coil, and R' is the total resistance in the circuit) is driven through the circuit. The effect of that short pulse of current is an adventitious "tapping test" applied to the seismometer, to delay its return to rest, during the first few milliseconds after the offsetting voltage is removed.

To provide a framework for interpreting the experiment I wrote an open-file report (Eaton, 1991) that derived the equation of motion for a vertical-component moving-coil seismometer and solved it for "generic" release and tapping tests as well as for sustained harmonic motion. The generic release test was further developed for "weight lift" and "current release" tests; and the generic tapping test was further developed for "pellet drop" and "current pulse" tests.

The complete expression for the response of the L4-C to the current release test contains two terms, the expected response to the current release test and the response to an adventitious current pulse (tapping) test. When the factors common to both terms are removed, both remaining terms are simple sine waves and the coefficient of the current release term is very much larger than that of the current pulse term; 667 to 1, for the set-up we used. Moreover, the current pulse test has a zero at the time of the first extremum of the current release test. These results validate the use of the ratio of offsetting current to first current release test extremum in seismometer calibration.

The derivation of the equations for determining the seismometer constants from damping tests was also presented in the open-file report.

B. Seismic system response

When NCSN seismic data, including the raw seismic traces, was made widely available to scientific users by its inclusion in computerized data archives that are accessible via the Internet (IRIS Data Center, Northern California Earthquake Data Center, etc.) we found that the procedure for computing system response developed for use with NCSN, principally for magnitude determinations, was awkward for the general user. Moreover, there was no concise summary and comparison of the characteristics of the variety of amplifier/VCO's, discriminators, and recorders that had been employed to collect the data. Because of my long involvement with these issues, I wrote an open-file report to provide such a summary and translated the calibration material into a format (SEED) used by the IRIS system in order to facilitate general access to NCSN data (Eaton, 1993).

VIII. Ongoing network concerns

In the decade that followed the reduction of NCSN from branch to project status in 1982, there was neither an integrated plan nor effective leadership for the operation and development of the network. Under the program manager structure, which required proposals and peer reviews, funded projects became more isolated and specialized; and developments that were vital to the health of the network were parcelled out to poorly coordinated projects staffed by personnel with little or no expertise in seismic instrumentation and no long-term commitment to the network. Some of these projects, like the implementation of the Menlo Park CUSP system, were eventually successful. Others, like the Parkfield Prediction digital network were costly, time consuming, and embarrassingly ineffective. Still others, like the augmentation of the analog network at Parkfield for the prediction experiment and the development and operation of the network around Mammoth Lakes to monitor earthquake swarms (from volcanic unrest?) at Long Valley Caldera - which were installed in cooperation with the network - have added to the strength of the network.

In spite of reduced funding and manpower that impeded the timely maintenance of the network and replacement of outdated equipment, the network performed reasonably well and brought great credit to the earthquake program because of its effective response to large earthquakes and their aftershock sequences: Coalinga, Morgan Hill, Kettleman Hills, and Loma Prieta. Particularly during the Loma Prieta sequence, the network and its results were the center of scientific and public attention. Network results held center stage in the earthquake program public display area in Menlo Park and were featured in TV news reports on a daily basis during the aftershock sequence. In the longer term, the network produced the most comprehensive record of the sequence and the best evidence for developing a detailed understanding of the structures and processes that produced it. Because of developments detailed below, I doubt that this record of accomplishments can be sustained in the future. In this section, I shall reiterate the concerns I have expressed in the past few years as the Survey program has embarked on a path that seriously undermines the capabilities of the network.

A. Loma Prieta emergency NEHRP funding and response to it

When a special appropriation for network "hardening" and improvement was included in the Loma Prieta emergency earthquake relief bill, it seemed reasonable to expect that the capability, status, and needs of the networks would be reviewed in the context of long-term program objectives. When plans for a network review were announced, I found them very disturbing. To my dismay, I was excluded from the meeting; and both the agenda and the participants were ill-chosen for a competent review of the California networks. I set down my views on these issues in a memorandum, "Thoughts on the 'Review of regional seismic networks to be held in Reston on August 21, 1990'", as background for the Seismology Branch technical spokesman at the meeting (John Van Schaack). Fearful that the network review would miscarry, I wrote a document to provide background and context for a meaningful review: "Reflections on the design, development, status, and current needs of the USGS short period seismic networks in California (December 19, 1990). That document was transmitted to Rob Wesson, Chief of OEVE, on January 4, 1991, along with a letter that summarized my concern with the course events were taking:

"My long-term interest in the health and integrity of both the northern and southern California short-period seismic networks is well known to you. In fact, your support, as Office Chief, for my effort to build up the southern California network in the late 1970s and the early 1980s was critical for bringing that net up to its present level of development. I also appreciate and acknowledge the unfailing support of the OES Chiefs and the Seismology Branch Chiefs throughout the 1980s that has assured the continued operation of the networks and analysis of their records."

"Overall, the networks have performed well and have recorded a wealth of data from both local earthquakes and teleseisms that have contributed to many studies that support the Survey's earthquake program. The impact of the network's data goes well beyond its direct use to document California's seismicity and to unravel the relationship of major earthquakes and their aftershocks to the faults that produce them. Those data have also stimulated the development and refinement of new procedures for more effective analysis of local earthquakes, for structural studies of the crust and mantle, etc. They have also provided a bridge between Survey researchers and their university counterparts."

" In light of the tremendous success enjoyed by the Survey in its response to the Loma Prieta earthquake, as well as the central role that the northern California network played in that response, I had hoped to see a thorough review of the California networks and the development of a network-wide plan to insure that they will be able to meet the needs of the earthquake hazards reduction program in coming decades. In my view, no such network review and planning is occurring; rather, I see a balkanization of the earthquake program that further impedes such network-wide review and planning. The scheme that you have devised for selecting and funding additional work to be undertaken in response to the recent budget increase unfortunately favors "new" work, carried out by untested instruments and methods, over enhancement of highly successful but still inadequate work carried out within the core program. You are in fact fulfilling the whimsical "law" that I suggested to you when you first told us in Menlo Park that a budget increase was likely in the aftermath of Loma Prieta: "visibly successful aspects of the program, like Calnet, will receive little or no additional support because their success shows that they are adequately funded, while techniques or projects that have produced disappointing results will receive significant increases to bring their performance up to that of Calnet". In proposing that "law" I was not trying to be prophetic. I was merely describing a process that is repeated every time significant new money comes into the earthquake program."

"If the rest of the network were developed as fully as the section around Loma Prieta, I would not be writing this letter. Unfortunately, critical parts of both the northern and southern halves of the network, in regions to which we have drawn public attention through our reports on "Probabilities of Large Earthquakes..." (USGS OFR 88-398) and the news coverage surrounding those reports, simply are not adequate for the level of surveillance appropriate to the level of hazard that we have described. With this problem in mind, I have attempted to review the development and current status of both networks as well as to outline a rational basis for identifying critical weaknesses and planning appropriate action to overcome them. The heart of this effort is contained on pages 10-16 of the enclosed "Reflections ..." and on the accompanying station maps that show current stations as well as new ones that would bring the net to a more adequate level of completion. That document is not offered as a concrete proposal to supplement the networks in the manner described. Rather, it is intended to reopen the question of the adequacy of the present networks and to suggest a rational basis for evaluating that question so that network needs can be weighed against other proposals for earthquake program enhancement."

"Other parts of the enclosed document deal with other aspects of the regional network question. Section I, pages 1-4, recapitulates the history of development of regional networks in the Survey and attempts to identify their defining characteristics and special role in seismology. The Appendix (sections III and IV) reviews my own involvement with instrument development and rehabilitation in the Survey in order to illustrate how much time and effort was required to get our relatively simple analog systems to work effectively. I hope that example, coupled with our troubling record to date with digital system selection and management, will suggest the levels of standardization, coordination, and oversight that should be applied to future digital systems developed for use in the Survey."

An expanded version of that document was released as an open-file report: Regional Seismic Networks in California (OFR 92-284, J. P. Eaton, 1992C). Its content is described by its section headings:

I. History of network development

II. Factors underlying the design and implementation of the northern and southern California short-period seismic networks

III. Current status of the northern and southern California regional networks

IV. Principal functions of the regional networks, and dependence of their performance on network configuration

V. Analysis of coverage provided by the current and proposed short-period seismic networks

VI. Further development of NCSN and SCSN

Judging from events that followed, the efforts described above had absolutely no effect on plans for network development. No clear network plan was promulgated, and network development was left to the accidental impact of independent projects that were funded under an increasingly fragmented proposal and administrative system.

B. FY95-99 strategic plan and response to it

The next chapter in the network planning and evaluation saga showed that benign neglect was not the worst approach to network planning. In 1994, a small group of early- and mid-career professionals in OEVE was given the task of developing a "FY95-99 Strategic Plan" to streamline the Earthquake Hazards Reduction Program to prepare it for the anticipated lean budget years ahead. In July, a preliminary version of the plan was circulated for comment. I was shocked by its recommendations, as I explained in my "Comments on the 'FY95-99 Strategic Plan'"(July 14, 1994) which is quoted, in part, below.

"The strategic plan team deserves our thanks for undertaking a difficult, controversial task with little hope of producing a plan that would make many of us happy. Nonetheless, in spite of hard work and good intentions, this report is appallingly misguided and inadequate. I am particularly distressed by its complete lack of understanding of the purposes and accomplishments of the northern and southern California microearthquake networks. This deficiency is not surprising because these networks, which have played such a vital role in the development and success of the Survey's earthquake program, were not represented on the membership of the strategic plan team."

"Although the plan addresses some real problems, its proposed solutions are strongly biased by the personal, branch, and regional interests of its authors. I should have expected a more serious, even-handed document from a group of Survey scientists charged with preserving the integrity and vitality of the Survey's portion of the National Earthquake Hazards Reduction Program during the anticipated lean years ahead. In my judgement the plan is so flawed that it does not provide a framework within which the solutions we seek can be found. It should be tabled forthwith so that we might proceed to develop a more reasonable plan based on scientific as well as political considerations."

"Most of my detailed comments deal with the proposed network seismology portions of the program, because that is where my current work is centered. My silence on other aspects of the plan does not constitute endorsement. Rather, it reflects my belief that those directly involved in those aspects of the program are better qualified than I am to evaluate the proposed plan in those areas."

"The terminology used in the strategic plan to denote the various networks is highly prejudicial and misleading. It reflects an old NOAA view of the hierarchy and importance of networks: global, national, regional, and local. The so-called regional networks in California were designed and developed specifically to support earthquake hazard/prediction studies in the broad, complex San Andreas fault zone that encompasses most of California. Its history, purposes, attributes, and early results were summarized in USGS Open-File Report 92-284, and its contributions, through 1986, to an understanding of the San Andreas fault system form the basis for Chapter 5, Seismicity, in "The San Andreas Fault System, California", ed. R. E. Wallace, USGS Prof. Paper 1515."

"Although the need to upgrade the seismic systems employed in the networks has long been recognized (see O.F.R. 92-284), such upgrading has been blocked since 1982 by policy in Reston, presumably due in part to divergent views (between Menlo, Pasadena, and Golden) on how best to proceed."

"My response to 'the dramatic upsurge in the number of destructive earthquakes in California (1979 Imperial Valley, 1983 Coalinga, 1987 Whittier Narrows, 1989 Loma Prieta, 1992 Petrolia, 1992 Landers, 1994 Northridge) compared to a relatively quiescent period during the 1970s' is different from that of the strategic plan team. They argue that these earthquakes should focus our attention more strongly on urban areas, where maximum damage will occur in future quakes; hence, greater emphasis should be place on evaluating hazards in cities throughout the nation, with a consequent reduction of effort in California. I would argue, instead, that the occurrence of these earthquakes vindicates our early judgement that the hazard in California relative to most of the nation is so high that it requires urgent study of the earthquake generating system and consequent earthquake hazards in California, where information will accumulate rapidly, with later application of resulting new techniques and knowledge to other regions where the likelihood of destructive earthquakes in the next few decades is much lower. I recognize that this purely scientific conclusion must be tempered to some extent by the political necessity of addressing earthquake hazards on a nation-wide basis so as to attract sufficient support in academia, industry, and Congress to sustain the earthquake program. We should not, however, blur the earthquake facts to make necessary political compromises seem more plausible."

"I concur with the conclusion that some aspects of NEHRP must be curtailed to preserve more fundamental and urgent parts of the program. I strongly disagree with most of the adjustments recommended by the strategic plan team. In my judgement the likely contribution of the Global and so-called National networks to the reduction or mitigation of hazards in regions of urgent concern is minimal. I am concerned that the strategic plan team recommends the expansion of these networks in regions of very low relative hazard at the expense of networks in high risk regions that are operated specifically to identify, locate, and evaluate seismogenic structures that traverse heavily populated areas."

"The Global network is of great scientific importance, and the Survey team that collects and interprets its data appears to be doing excellent work. It also brings the Survey prestige and gratitude from the many academic institutions that employ its data. However, the proposed expansion of the network, which will require expenditure of additional Survey funds for maintenance and operation when the new stations are transferred to the USGS, simply should not be funded by reductions in urgent programs in the hazard-prone western US. Instead, we should renegotiate the conditions for maintaining and operating the network with the other government agencies that depend on its results (NSF, AFTAC, ?)."

"Likewise, the sparse National network proposed for eastern and midcontinent US will have little short- or intermediate-term impact on our knowledge of long-term seismicity in those regions. We will continue to rely primarily on the historical record of earthquakes in these regions for constructing seismic risk maps for decades to come. Moreover, the planned national net may already be obsolete. Networks already squeezed out in the eastern US by the growth of the national net appear to be re-establishing themselves as independent economical networks employing Willie Lee's or similar low-cost systems. The future of seismology in these regions, perhaps, lies in networking such independent, low budget networks over the data superhighway."

"The section of the report that deals with the proposed management structure strikes a discordant note. It emphasizes the need for more coordination and control from above in a manner that suggests a punitive management style, rather than one of facilitating, motivating leadership. The proposed structure is designed to support an orderly flow of paper rather than the development of a sound scientific program."

"When I look over our dwindling ranks I do not see a sufficient corps of tested, qualified, experienced candidates to fill the many specified management slots. It is difficult to overstate the demoralization experienced by the troops subject to an unqualified or inept leader. In my view, significant participation in the NEHRP program at scientist and project leader level should be a prerequisite for all managers from Branch Chief up."

In an open meeting to discuss the plan, members of the team who were present suggested that its release was premature (the whole team had not reviewed the final version) and appeared to be taken aback by the vehemence of the plan's many critics. Although it was said that the plan would not be implemented, subsequent events have shown that the positions put forth in the plan did indeed represent the thinking of the OEVE management group.

C. Analysis of the FY1995 BSIS program and the NCSN superproject

In mid-1994 Walter Mooney succeeded Al Lindh as Chief of the Seismology Branch of OEVE. I volunteered to help Walter assess the status of the overall Branch program as well as the NCSN "superproject" on the basis of available documentation: primarily the project proposals submitted to the various funding sources and the project budget allocations, which were broken down according to funding source. The 50-page document that I prepared from these sources included:

I. Summary of NEHRP proposals, with panel scores and awards, requested manpower and funding, and a brief description of the work proposed.

II. List of BSIS projects with funding and funding sources.

III. Grouping of BSIS projects by topic (into 10 categories).

IV. NCSN superproject breakdown by task.

V. NCSN organization, and analysis of problems.

VI. Issues.

The total branch budget of about $12.1M supported 102 people divided among 53 projects plus 15 additional people involved in administration and management that were supported by $1.3M in assessments against the other projects. The 53 projects covered 10 topical ares of research and received funding, by proposal, from 20 different program sources: $9.3M from the eight regional and topical NEHRP subprograms, $0.8M from the Volcano Hazards program, and $2.1M from 11 other USGS or outside sources.

The number of projects devoted to each topical area of research as well as the corresponding funding and manpower were as follows:

SUMMARY OF PROJECTS BY TOPIC

Topic	Funds(K)	%	M.Y.	%	No
Earthquake Seismology	6808.2	55.6	58.24	57.3	18
Strong Motion and Attenuation	559.4	4.6	4.06	4.0	4
Site Response	1287.6	10.5	8.91	8.8	8
Strong Mtn and the Seismic Source	256.7	2.1	2.44	2.4	2
In-situ Velocity and Q	402.5	3.3	3.16	3.1	2
Volcano Hazard	832.4	6.8	7.95	7.8	4
Crust and Mantle Structure	1226.4	10.0	9.41	9.3	6
Seismic Instrumentation	542.7	4.4	4.81	4.7	3
Outreach	204.1	1.7	1.35	1.3	5
Computer Support	124.3	1.0	1.27	1.3	1
Total	12240.3	100.0	101.6	100.0	53
NCSN superproject	3742.8	30.6	32.7	32.2	4

The NCSN described in the NEHRP proposal (included in Earthquake Seismology, but broken out and shown separately below the "Total" line in the table above) actually consisted of four formal BSIS projects plus several other former projects that had been subsumed by the principal "Calnet" project. These projects were:

03	Van Schaack	Field Operations
08	Jensen	Instrument Development
09	Hall	Network Operations
15	Oppenheimer	N. Calif. Seis. Network

Grouping of the cluster of independent projects that participated in NCSN into a single superproject was first done in response to an OES office-level request to rationalize the NEHRP proposal for NCSN support. It was argued that each proposal should be justified by its own research content and that "operational" projects that supported the work of other projects would be hard to evaluate and fund. It also seemed reasonable that the entire Calnet activity should be guided by unified goals and planning that could be embodied in the proposal.

The wide range of tasks, and the corresponding specializations and skills, required to carry out the work of NCSN pose serious problems for planning, coordination, supervision, and evaluation. The principal tasks carried out under the NCSN banner were:

Field/Laboratory operation, management, and operation	12.3MY	$662.7K	(Project)
Analysis system operation and data reduction	6.0MY	$371.5K	"
Data analysis and archiving	4.3MY	$263.4K	"
Software development	4.2MY	$320.0K	"
Network equipment development	2.0MY	$162.6K	"
Computing support	2.9MY	$162.6K	"
Research	1.0MY	$94.4K	"
Total	32.7MY	$2894.6K	"
		$3742.8K	(Bureau)

If we consider only the maintenance and operation of the network, recording system, and analysis system plus data reduction, analysis, and archiving, these figures are reduced to 21MY and $1236K (Project). If these costs are spread over 390 NCSN field stations we find that NCSN operates and analyzes 19 stations per person at a cost of 3.2K (Project) or $4.1K (Bureau) per station per year. The remaining 11.7MY and $785.6K (Project) are required for software development, equipment development, general computing support, research, and operation of the GEOS (portable digital recorder) system.

Operation of NCSN as a superproject appears to have been advantageous for maintaining funding at the minimum level required to operate the network and analyze its data; but it did not lead to the development of an overall network plan nor to effective planning and coordination of the work of its constituent subprojects. Factors contributing to these difficulties include:

1) blurred lines of technical and administrative responsibility and authority within the superproject as well as ineffective monitoring of progress on critical multi-player tasks,

2) weak Branch influence (technical oversight) over project work because project funding largely by-passes the Branch Chief under the current regional/topical proposal system,

3) tight funding, which requires difficult choices and limits our ability to recover from bad technical decisions.

These same factors also tie the hands of the Branch Chief in his traditional role as planner and leader of the scientific program. His role has been reduced to that of a personnel manager serving the needs of a group of independent project-chief "contractors" who compete for funding from program managers who have no clear responsibility for the integrity of the Branch scientific program. The program managers, in turn, are sufficiently separated from the projects they support, by Branch boundaries and the formalism of the proposal and review system, that they provide only weak scientific leadership and ineffective monitoring of project progress and results. The resulting fragmentation of leadership and funding makes it extremely difficult to develop and implement a balanced, coherent program in earthquake hazard reduction research.

D. Post Northridge earthquake network planning

The January 19, 1994 Northridge earthquake occurred beneath a relatively sparse section of SCSN west of San Fernando that had been identified and targeted for upgrading in OFR92-284. Moreover, telemetry (principally, phone lines) in the immediate vicinity of the epicenter was disrupted by the earthquake, further reducing the network available for recording early aftershocks. The Menlo Park network field team responded promptly to this emergency and installed half-a-dozen radio-telemetered stations in and around the epicentral region to repair and enhance the network for aftershock recording.

As after the Loma Prieta earthquake in 1989, a Northridge earthquake emergency relief bill passed by Congress included funds for "hardening" and upgrading SCSN. Again, as after Loma Prieta, the short-period telemetered network that had provided the data for close monitoring of the aftershock sequence and had earned great public respect for the USGS/Caltech earthquake program was passed over when plans were laid for "network improvement". A separate parallel network, designed to record strong ground motion throughout the greater Los Angeles urban region, that was proposed would employ digital telemetry over "donated" digital phone circuits and would require the development of an entirely new data recording and analysis facility. This approach was also held up as a possible model for upgrading NCSN.

In NCSN, two new telemetered digital seismic systems, the 16-bit DST developed by Gray Jensen, and the "24"-bit Nanometrics system purchased for the East Bay network (from Loma Prieta funds), are being recorded in Menlo Park. Records from both systems can be merged with data in the CUSP system to facilitate evaluation and comparison of the three systems. The CUSP analysis program, TIMIT, can handle 12-, 16-, and 24-bit data as well as different sample rates.

To provide the background for a discussion of alternative plans for upgrading NCSN, I prepared Open-File Report 95-269: "Merging of analog and digital data in NCSN, and characteristics of the principal seismic systems it employs" (Eaton, 1995). Topics explored included:

1) Calculation of system magnification and C_{10} factor
2) Characteristics of seismometers, accelerometers, and dilatometer (L4C, L22, HS1, W731 Wilcoxon, FBA23 Kinemetrics, and Sacks-Evertson dilatometer)
3) Seismic Systems [telemetered analog USGS system (12-bit); telemetered DST system (16-bit); telemetered Nanometrics system (24-bit); portable RefTek field recorder (16-bit, 24-bit);GEOS portable recorder (16-bit)]
4) Comparison of sensor response (Fig. 50), and recording range (Fig. 51) of various system configurations
5) telemetry requirements for the analog, DST, and Nanometrics systems.

OFR95-269 was the principal basis for my contribution to the NCSN plan (Upgrading NCSN to a hybrid digital/analog network), submitted to Dave Oppenheimer on May 26, 1995, in response to his call for material on that topic. The approach that I recommended is outlined by the following quotes from that contribution.

"This analysis has been stimulated by plans for an upgraded SCSN outlined by our colleagues at Caltech. Their solution is a fully commercial one that uses top-of-the-line commercial seismic systems, depends on the phone company for 2-way digital telemetry (at no cost), and proposes the development of a new generation of computers and programs to record and analyze the data. It will be very costly and could be rendered inoperable, under present budget restrictions, if the phone company limits or backs off on its promise of cooperation and free telemetry."

"The need to upgrade NCSN is just as urgent as it is for SCSN, but we don't have a cache of money set aside for the purpose. On the other hand, the NCSN infrastructure, particularly telemetry, is quite different from that of SCSN. Our nearly complete utilization of radio and microwave telemetry should facilitate selective introduction of digital systems into the network."

"Work to evaluate both the commercial 6-channel, 24-bit Nanometrics system and the 3-channel, 16-bit DST/IASPEI system, as well as seismometers, accelerometers, etc., used with them is underway. Merging of the digital data with the CUSP data stream on an event by event basis has been tested for the Nanometrics system and appears to be feasible for the DST/IASPEI system. The telemetry requirements of the DST/IASPEI system are compatible with our current telemetry system, but the Nanometrics system requires a broader-band channel than our present system can supply. Plans to upgrade the DST/IASPEI system to 20+ bits show promise, and the current telemetry system should still suffice for the 3-channel 20+ bit system."

"The strategy for network upgrading in SCSN appears to be based on benign neglect of the analog network while it is being replaced, over several years, by an entirely new digital network."

"To upgrade NCSN we should proceed to transform it from within by replacing analog systems with digital ones on a selective basis so as to maintain operational integrity while the work proceeds. Indeed, with limited funding, we really have no choice but to convert the network gradually as the opportunity arises. The relatively low cost of this approach, coupled with the major enhancement of the usefulness of the data obtained from the hybrid network, should make it an attractive choice."

"In choosing which sites to upgrade we should consider how those sites will enhance the overall effectiveness of the network. An attractive place to start is the San Francisco Bay Area, where the Nanometrics system is already devoted to close-in recording along the Hayward fault. It may be wise to use the first DST's to extend that experiment to provide a test-bed for developing a system to provide early notification and evaluation of moderate to large earthquakes in the urbanized Bay Area. The design of the EARTHWORM recording and analysis system specifically addresses the need for incremental analysis and reporting of a large event while it is still in progress; so we are well along on the development of the system needed to exploit the data from a hybrid digital/analog joint microearthquake/strong-ground-motion network. Early concentration on completing a Bay Area hybrid net to provide input to the EARTHWORM system would have strategic as well as scientific merit. It would demonstrate our commitment to providing the tools to ameliorate the urban earthquake hazard problem and it would provide the opportunity to evaluate the new equipment and analytical procedures in a modest-size prototype network very close to Menlo Park. Success on a critical experiment at this scale could validate the approach used and help secure support to extend the digital sub-net to cover the most critical parts of NCSN."

"The call for more effort in urban regions has been interpreted by some as reason for de-emphasizing studies of the same faults that traverse these regions in their broader tectonic context outside the urban regions. This approach is as short-sighted as parking your car on the railroad tracks when no train is in sight. Moreover, it ignores the reality that NCSN is already broadly adjusted to provide better coverage in seismic regions with large populations than elsewhere."

"The average station density in the central Coast Ranges between Clear Lake and Bear Valley, at 3.83 stns/1000km^2, is about 4 times greater than that for the northern Coast Ranges, Sierra Foothills, and Lassen/Shasta regions. In the former region, the largest concentration of population in northern California is strung out along the faults that have produced the most damaging historical earthquakes in northern California. The enhanced station coverage in the southern Coast Ranges, 2.46 stns/1000km^2, is largely in response to the Parkfield earthquake prediction experiment. The high density of stations in the Long Valley region, 6.15 stns/1000km^2, supports the ongoing volcano hazard watch maintained by the USGS at Long Valley caldera."

"Tentative plan to modernize NCSN (outline)

(1) Insure the integrity of our microwave/vhf telemetry system by complying with IRAC equipment requirements. This compliance will require replacement of most of our radios.
(2) Convert 100 to 150 telemetered analog stations distributed throughout the network to 3-component digital L4C stations (20-bit, if possible). These stations will replace and extend the present low-gain 3-component sub-net and open new opportunities to study earthquake wave forms and their detailed interaction with the crust.
(3) Establish about 100 telemetered digital 3-component accelerometer stations in the greater Bay Area

a) along principal fault traces to provide telemetered on-scale data during large earthquakes along those faults,

b) at networks of "hard" and "soft" sites throughout the region to provide telemetered on-scale records of strong ground motion at carefully selected reference sites to permit rapid projection of shaking and shaking damage throughout the region.

(4) Reconfigure the analog high-gain vertical net to optimize hypocentral control throughout the network.
(5) At an early date, define the role that various instrument, recording, and analysis systems should play in the network and develop specific plans and timetables for completing and implementing the separate functions of the EARTHWORM system."

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GLOSSARY

ACG Assistant chief geologist (USGS)

AEC Atomic Energy Commission

AFCRL Air Force Cambridge Research Center

AGI Americal Geological Institute

AFTAC Air Force Technical Applications Center (DOD)

ARPA Advanced Research Projects Agency (DOD)

A/D Analog to digital converter

BSIS Branch of Seismology (USGS)

CDMG California Division of Mines and Geology

COE Corps of Engineers (DOD)

CUSP Caltech/USGS Seismic Processing system

DRDT Division of Reactor Design and Technology (AEC)

DST Digital seismic system developed by USGS/IASPEI

ERTS-A Earth Resources Technology Satellite (NASA)

FAA Federal Aviation Administration

FI Fluid injection

fm frequency modulation

GAO General Accounting Office (Congress)

GEOS USGS portable 16-bit digital recorder

HUD Department of Housing and Urban Development

HVO Hawaiian Volcano Observatory (USGS)

IASPEI Institute of Earth Physics and the Earth's

IRAC Interdepartmental Radio Advisory Committee

LASA Large Aperture Seismic Array (DOD)

LRC inductance-resistance-capacitance

LRSM Long Range Seismic Measurements (Van) (AFTAC)

NASA National Aeronautical and Space Agency
NCER National Center for Earthquake Research (USGS)
NCSN Northern California Seismic Network (USGS)
NEIS National Earthquake Information Service (USGS)
NEPEC National Earthquake Prediction Evaluation Council (USGS)
NOAA National Oceanic and Atmospheric Agency
NSF National Science Foundation
NTO New Technology Opportunities
NTS Nevada Test Site (AEC)
OEP Office of Emergency Preparedness
OERCS Office of Earthquake Research and Crustal Studies (USGS)
OEVE Office of Earthquakes, Volcanoes, and engineering
OFR Open-File Report
OES Office of Earthquake Studies (USGS)
OMB Office of Management and Budget
ONR Office of Naval Research (DOD)
OST Office of Science and Technology
OSTP Office of Science and Technology Policy
PTA Phototube amplifier
RTP Real-time Processor
SCSN Southern California Seismic Network (USGS - Caltech)
SEED Standard for Earthquake Exchange of Data
TAPS Trans-Alaskan pipeline Studies
SIR USGS funds from Congress (Scientific Investigations and Research)
UT Universal time (Greenwich)
USGS United States Geological Survey
VCO voltage controlled oscillator
VELA DOD program to upgrade U.S. seismological capability
VHF very high frequency (radio frequencies near 200 megahertz)
WRD Water Resources Division (USGS)
WWV Radio time station
WWVB Low frequency NBS radio time station
WWVH Short wave NBS radio time station in Hawaii

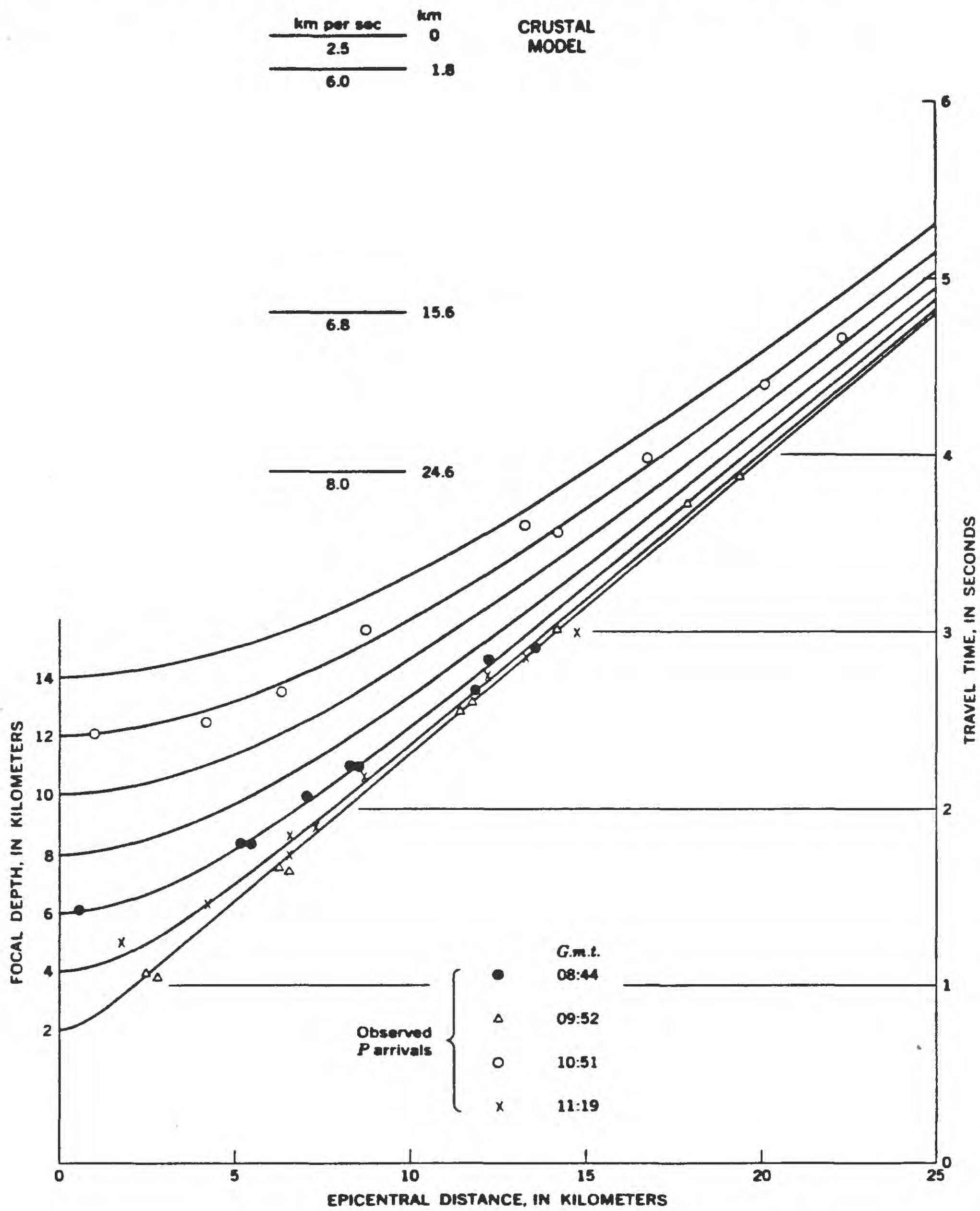

Figure 1. Theoretical traveltime curves for earthquakes with focal depths from 2 to 14 km, and the crustal model from which they were calculated. Observations from four july 17, 1966, Parkfield aftershocks are plotted on the diagram for comparison with the theoretical curves to show the depth resolution provided by the network.

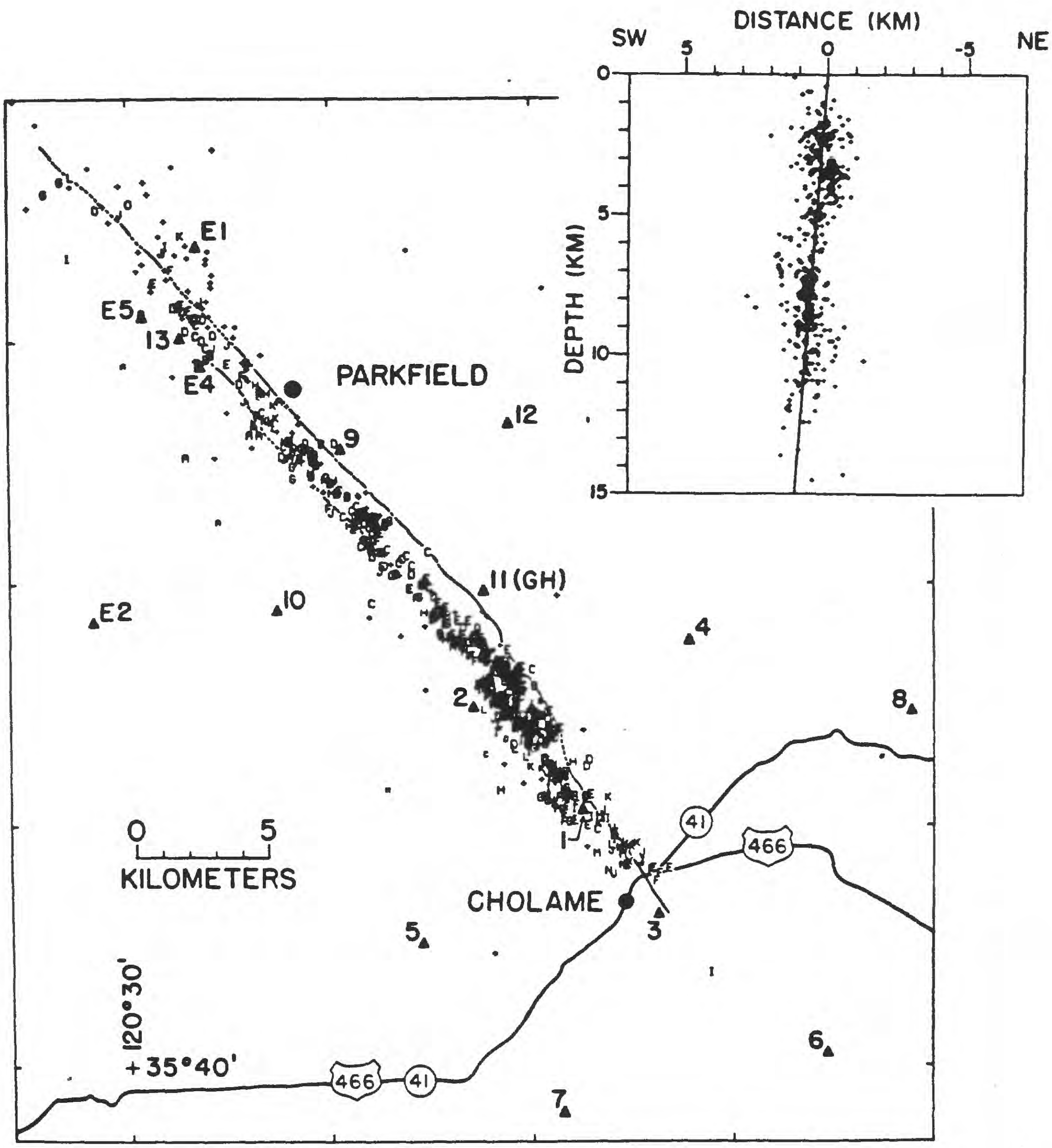

Figure 2. Aftershocks of the 1966 Parkfield-Cholame, California, earthquake. Stations of the portable network are indicated by triangles. Stations E1 through E5 were operated by the Earthquake Mechanism Laboratory of NOAA; the others, by NCER (USGS). Zones of surface fracturing that accompanied the main shock and the aftershock sequence are shown as solid and broken lines. The letter symbol indicates focal depth: 0-1 km = A, 1-2 km = B, etc. Aftershocks for which focal depths could not be determined are plotted as crosses.

(Inset) Hypocenters of well-located Parkfield-Cholame aftershocks projected onto a vertical plane perpendicular to the surface trace of the reference plane fitted by least squares to the well-located hypocenters. The trace of the reference plane is indicated by the solid line.

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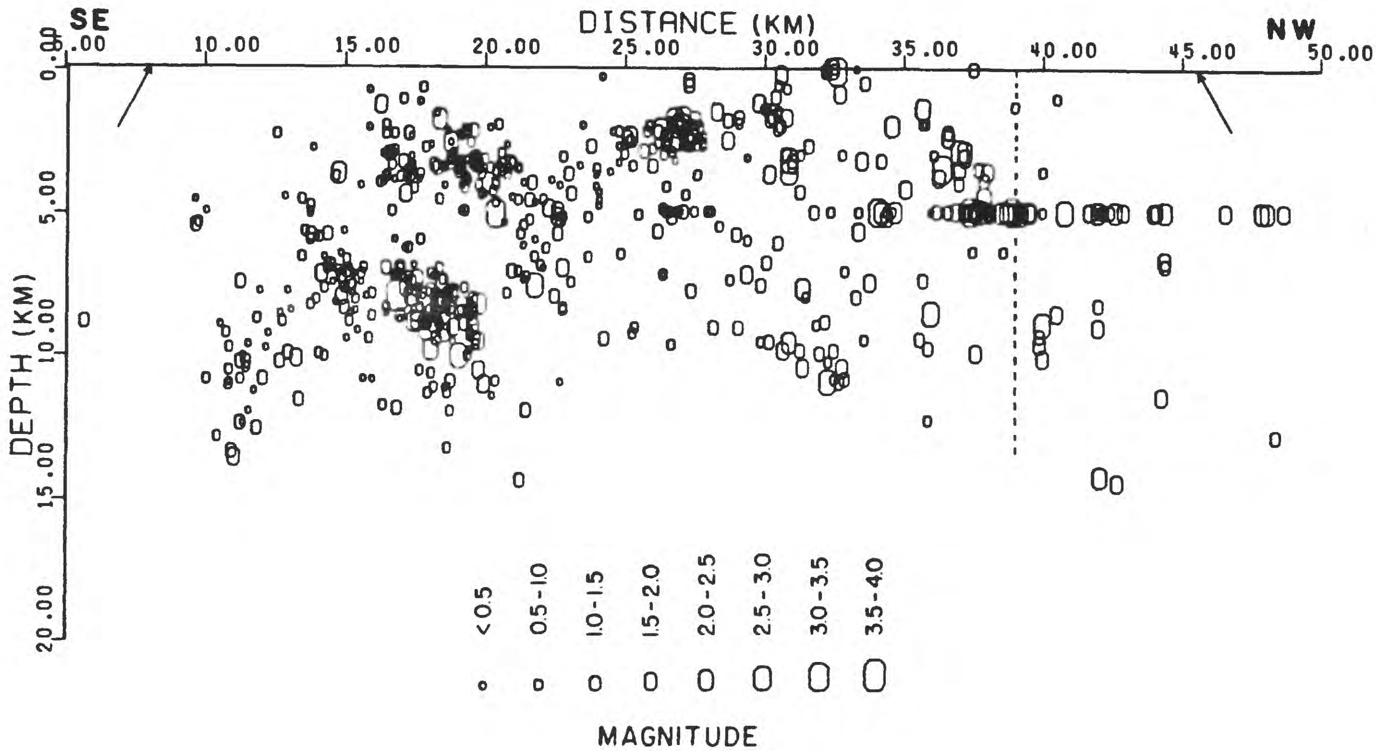

Figure 3. Hypocenters of Parkfield-Cholame aftershocks projected onto a vertical plane parallel to the surface trace of the reference plane fitted by least squares to the well-located hypocenters. The epicenter of the main shock is marked by the dashed vertical line, and the limits of surface fracturing are indicated by arrows. The concentration of hypocenters at a depth of 5 km at ranges of 35-50 km should be disregarded. Inadequately recorded aftershocks were constrained to that depth to permit their epicenters to be calculated. The origin of the distance axis is near station 6 (Figure 2).

PRINCIPAL RESULTS OF MICROEARTHQUAKE STUDIES

Results from the San Francisco Bay area telemetered seismic network

A year-long effort to install and test an effective telemetered short-period seismic net and to develop efficient methods of reducing the data was culminatèd in early 1968 with the completion of an operating 30-station microearthquake network concentrated between the south end of San Francisco Bay and Hollister (Fig.5). In its original conception, this network

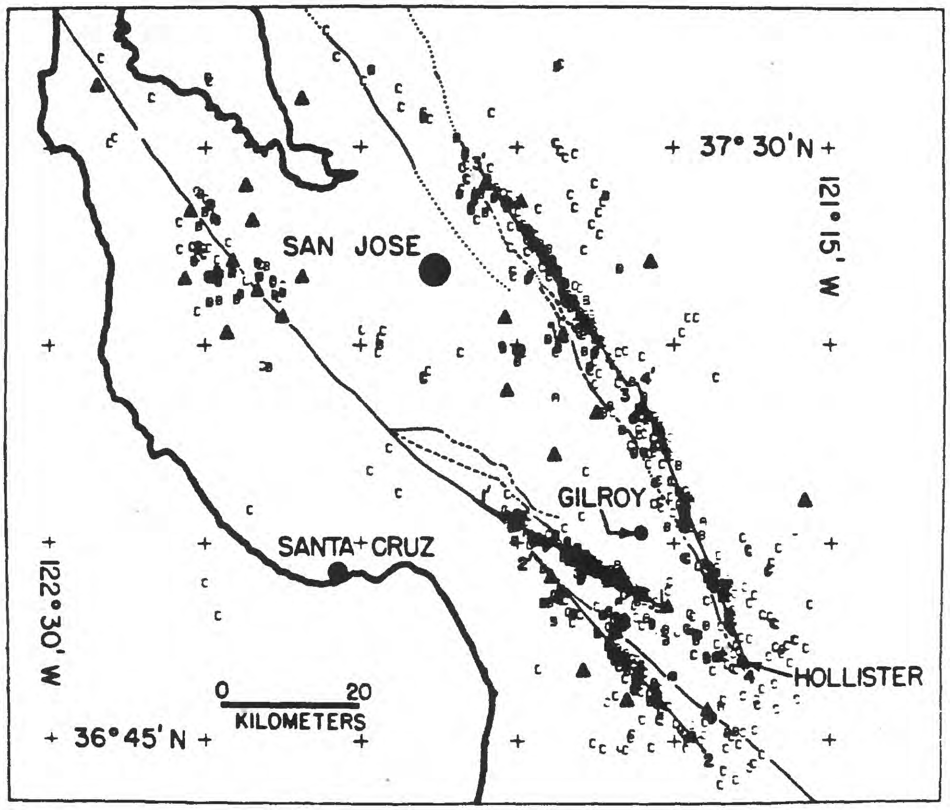

Figure 4. Epicenters of well-recorded events within the telemetered network from March 1968 through April 1969. Plotted symbols indicate the reliability of hypocentral determinations: A, well determined epicenter (± 1 km) and focal depth (± 2 km); B, fairly well determined epicenter (± 2.5 km) and focal depth (± 5 km); and C, moderately well determined epicenter (± 5 km) but undetermined focal depth. Zones of hypocenter concentrations marked off by numbered lines are as follows: 1-1', Sargent fault; 2-2', San Andreas fault west of Hollister; 3-3', Calaveras fault, northern section; 4-4', Calaveras fault, southern section.

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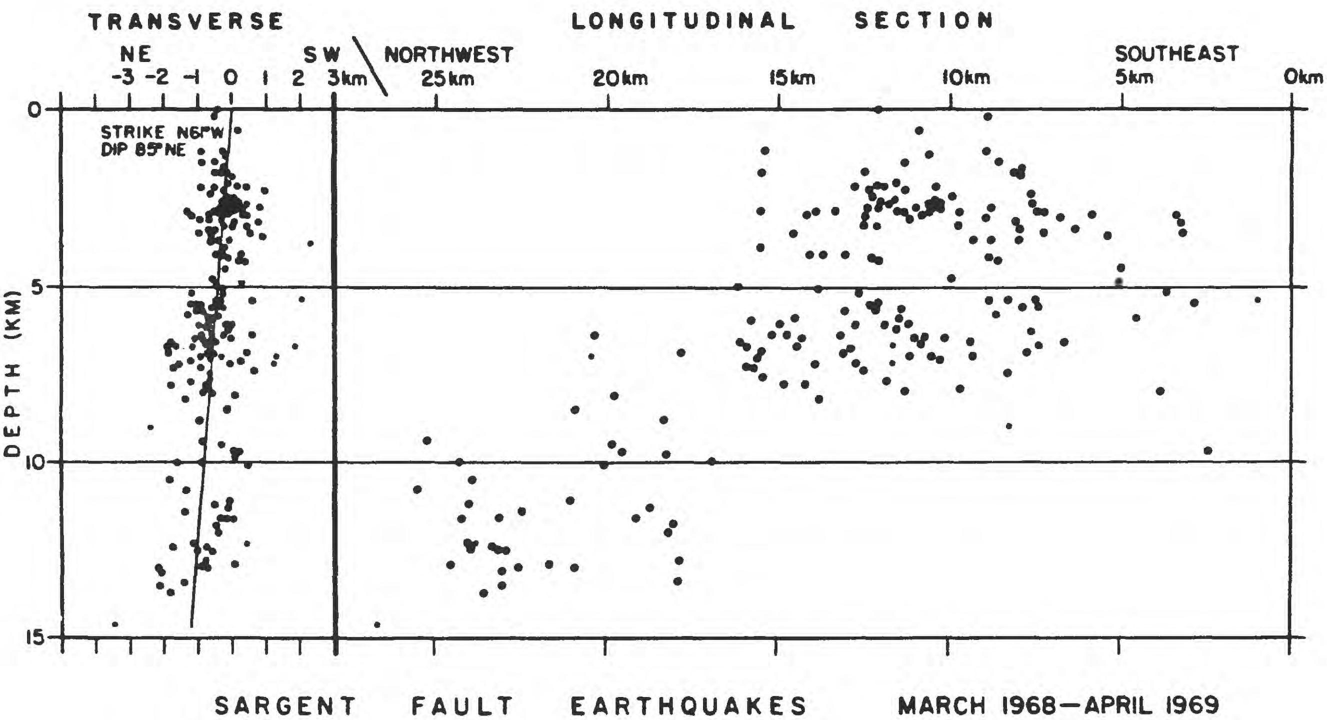


Figure 5. Earthquakes in the Sargent fault zone hypocentral region projected onto planes perpendicular (left) and parallel (right) to the surface trace of the plane fitted by least squares to the hypocenters. Events that lay more than two standard deviations from the fitted plane are plotted as small dots.

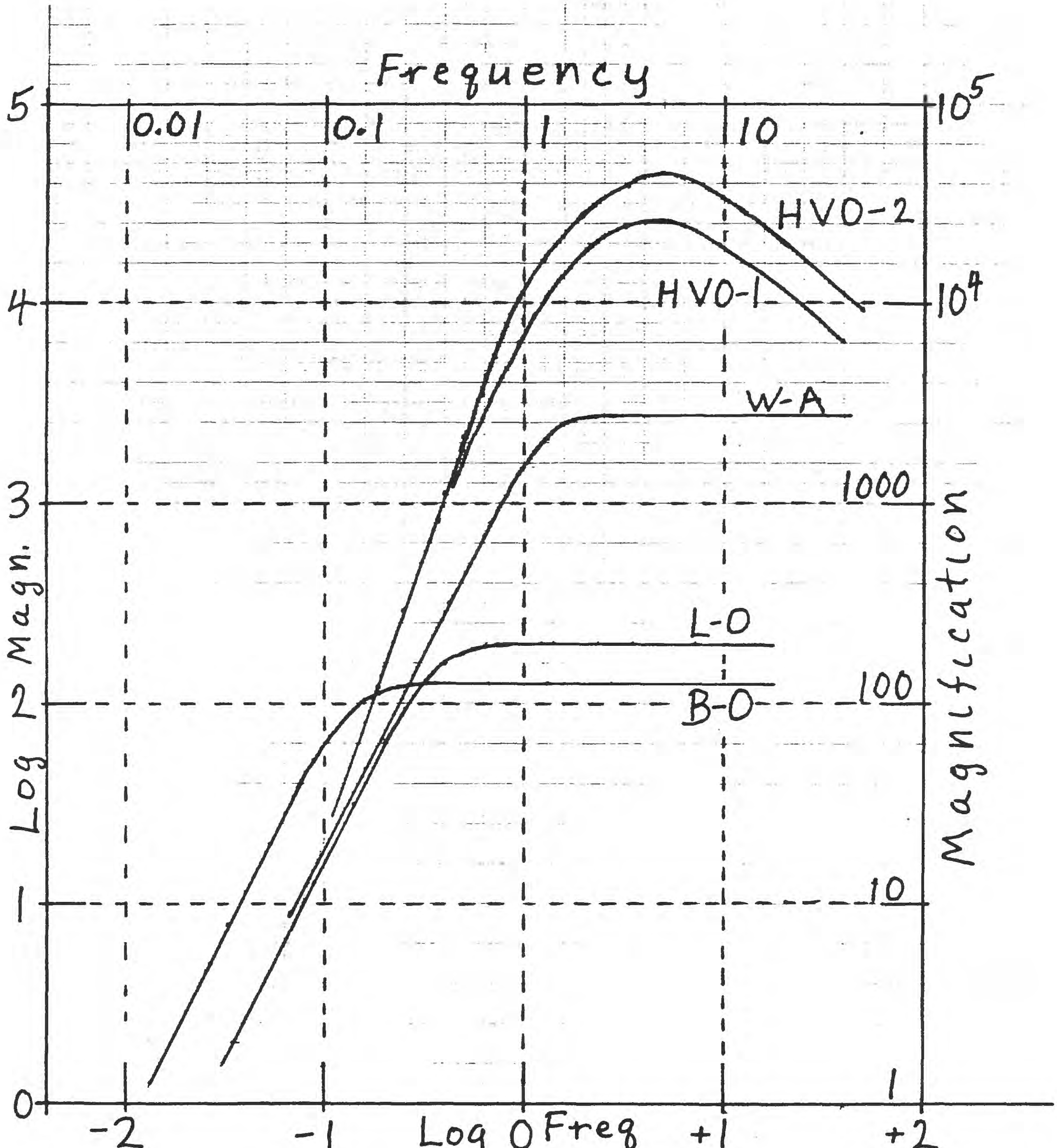


Figure 6. Response curves of the new HVO-1 and HVO-2 seismographs compared to those of the standard Wood-Anderson (W-A), the Loucks-Omori (L-O), and the Bosch-Omori (B-O) seismographs. The relatively high magnifications of the HVO-1 and HVO-2 seismographs in the 0.1 to 0.5 second range (or 2 to 10 hz) enable them to record small earthquakes that are missed by the B-O and the L-O. At these periods the magnifications of the HVO-1 and HVO-2 are approximately 10 times that of the W-A and 100 times that of the L-O and B-O.

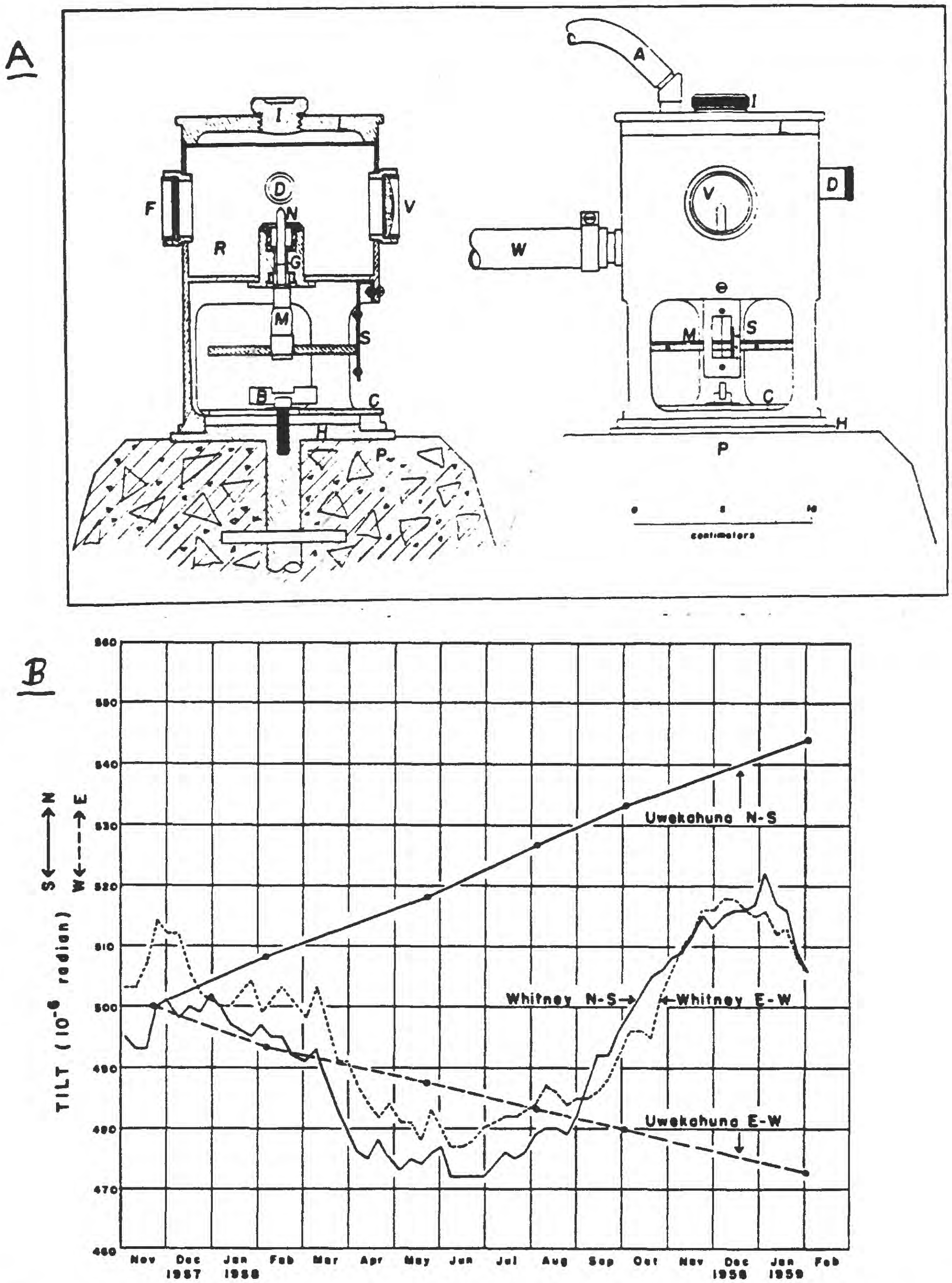


Figure 7. A. Cutaway side view and face view of micrometer pot, hub, and pier. A=air shunt, B=clampdown bolt, C=clampdown plate, D=drain plug, F=frosted glass window, G=micrometer adapter and mercury packing gland, H=hub, I=inspection plug, L=lid, M=micrometer, N=monel micrometer point, P=pier, R=water reservoir, S=scale, V=viewing lens, W=water tube.

B. Tilting during 1958 at the Whitney station and at the Uwekahuna tilt base. Whitney shows a strong seasonal tilting (40μrad p-p) superposed on slow northeastward tilting at a rate of about 20 μrad/year. Uwekahuna shows a steady northwestward tilting at a rate of about 45 μrad/year with virtually no seasonal component.

then the rate of lava outpouring, which had decreased as the erupting fissure shortened, began to increase again, and it continued to increase steadily until the fountain died out suddenly on 21 November. The 40 million cubic yards of lava poured into Kilauea Iki crater filled it to a depth of 335 feet, slightly above the level of the vent.

Seismographs and tiltmeters warned that the eruption was not over. Feeble harmonic tremor that persisted after the fountain died was soon augmented by a growing swarm of tiny, shallow quakes such as preceded the eruption; and tiltmeters, which recorded a rapid deflation of the shallow lava reservoir while the fountain poured out its lava, revealed that the volcano was being inflated rapidly once more (Fig. 7). At 1:00 A.M. on 26 November the main vent of the first phase of the eruption revived. By 4:35 P.M. an additional 4.7 million cubic yards of lava had poured into the pond, increasing its depth to 350 feet and raising its surface high above the level of the original vent. Again the fountain died abruptly, and this time lava began to pour back down the vent. By 12:30 P.M. the next day 6 million cubic yards of lava had disappeared from the lake, leaving a black ring of frozen lava 30 feet above its receding surface.

During the following three weeks 14 more eruptive phases of shorter and shorter duration but with increasingly vigorous fountaining took place at the Kilauea Iki vent (Fig. 9). The highest fountain was measured during the 15th phase, on 19 December, when a column of incandescent, gas-inflated lava jetted to 1900 feet, by far the greatest fountain height yet measured in Hawaii. At its highest stand, at the end of the eighth phase, the lava pond was 414 feet deep and contained 58 million cubic yards of lava. At the end of each phase the fountain died abruptly, and from the 2nd to the 16th phase, a mighty river of lava surged back down the vent as soon as the fountaining stopped (Fig. 10). Of the 133 million cubic yards of lava spewed out into

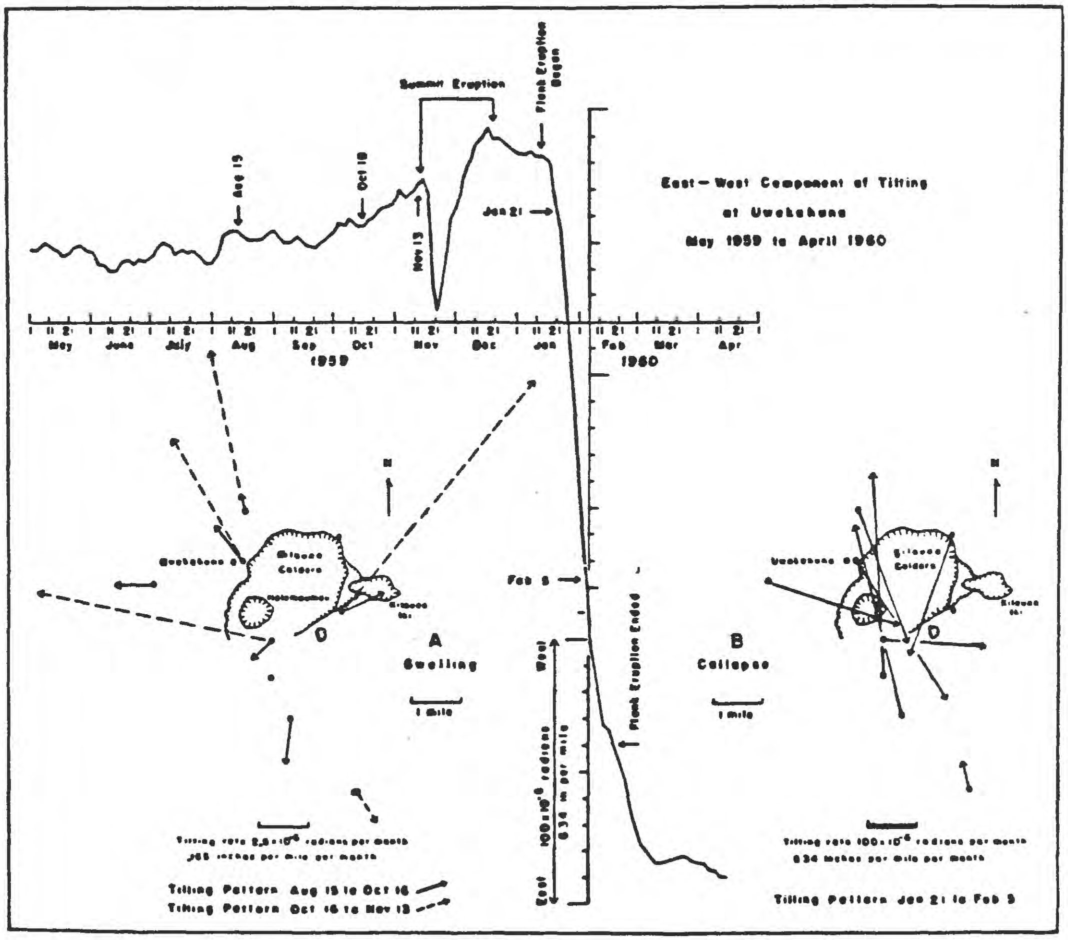
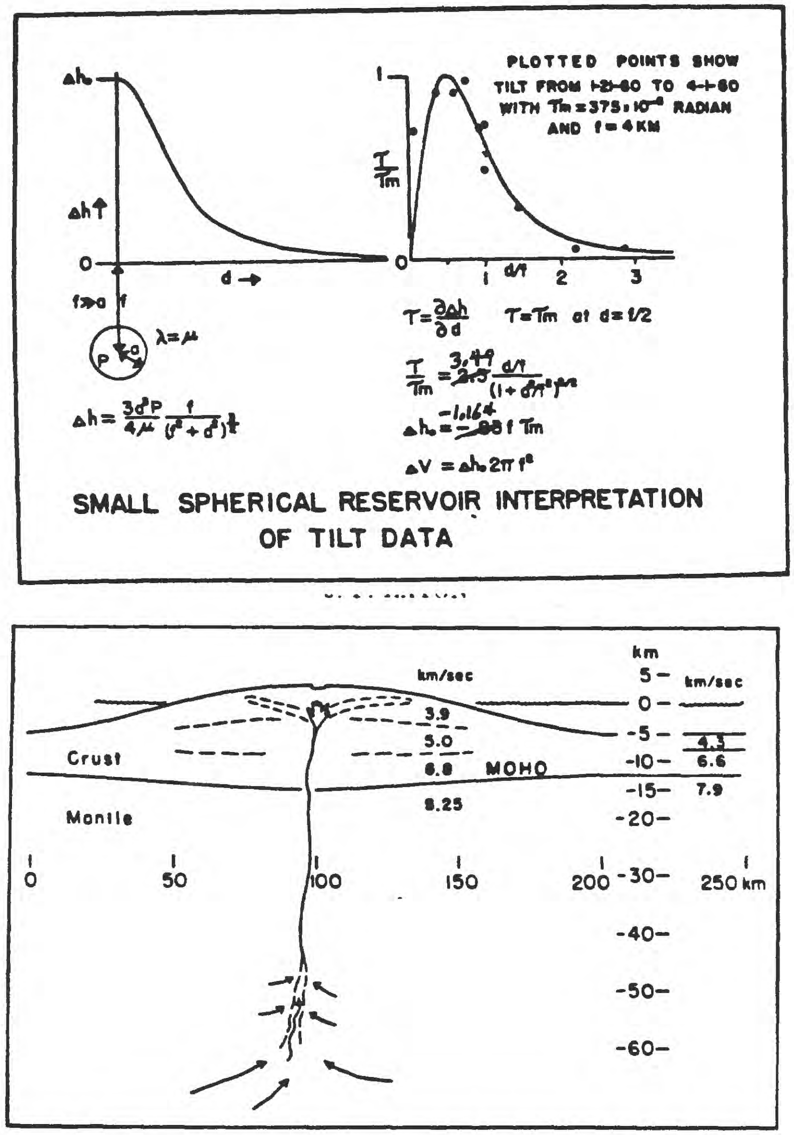


Figure 8. Ground tilting at stations around Kilauea caldera associated with the 1959-1960 eruption of Kilauea. The east-west tilting at Uwekahuna Vault shows the swelling and collapse of the summit of Kilauea as a function of time. Westward tilting (up) corresponds to swelling, and eastward tilting (down), to collapse. Inset A illustrates the pattern of tilting (at the tilt bases) during two periods of swelling. Inset B illustrates the pattern during collapse. Note the 40-fold difference in scale between A and B.

A



B

Figure 9. A. Comparison of tilting during the most rapid subsidence of the summit with that expected from a small spherical reservoir with its center 4 km beneath the south rim of the caldera. Open dots represent tilting at stations north of Kilauea caldera; solid dots, at stations in other azimuths.

B. Schematic longitudinal cross section of an idealized Hawaiian volcano. The dashed rift zone cores are in imperfect communication with the central reservoir. A transverse section of the volcano would have steeper slopes and no rift zone core. Approximate velocities for longitudinal waves in the volcano are from the March 7, 1955, earthquake; those for the ocean section are from Raitt (1956). The arrows suggest magma moving into the conduit beneath the volcano.

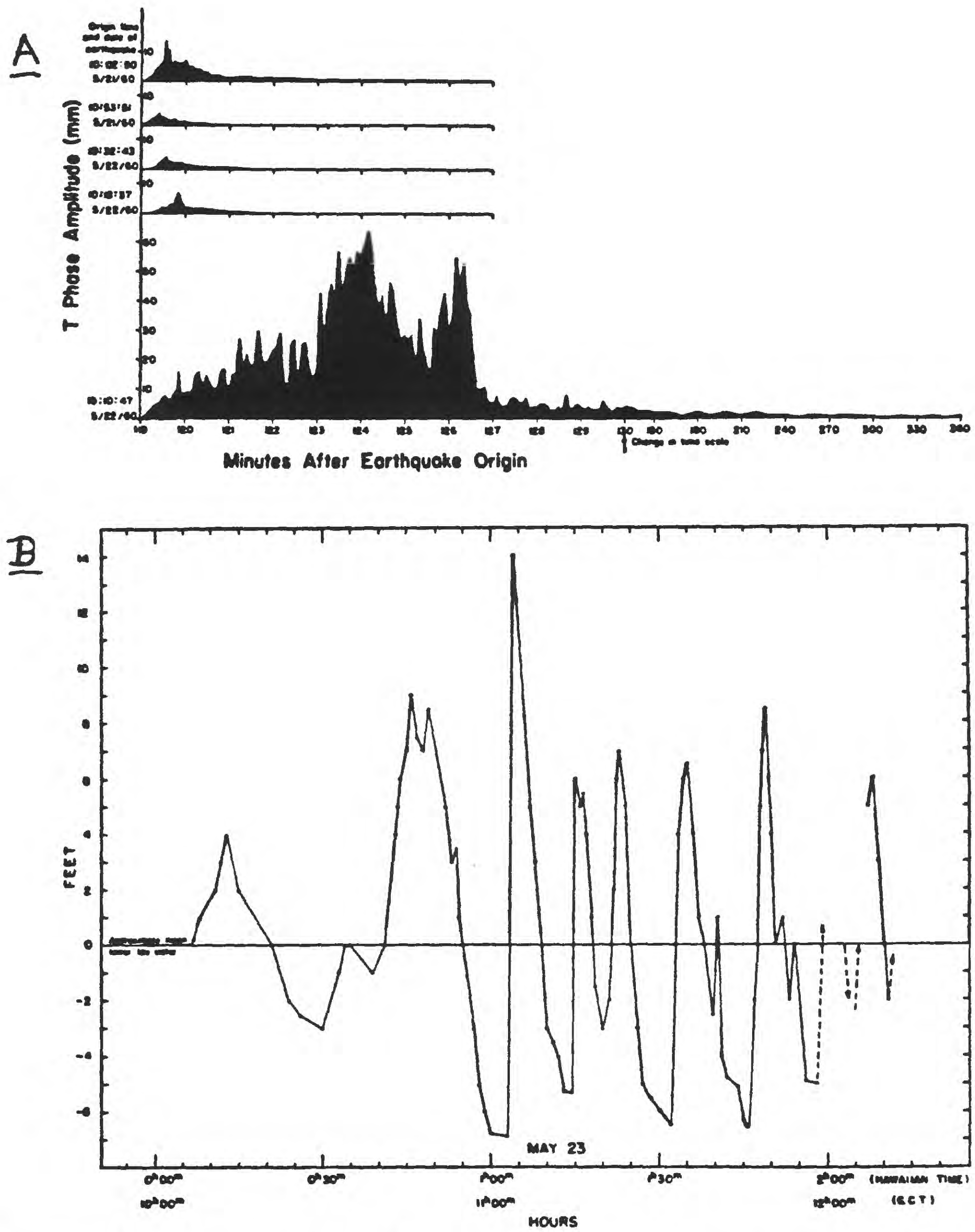
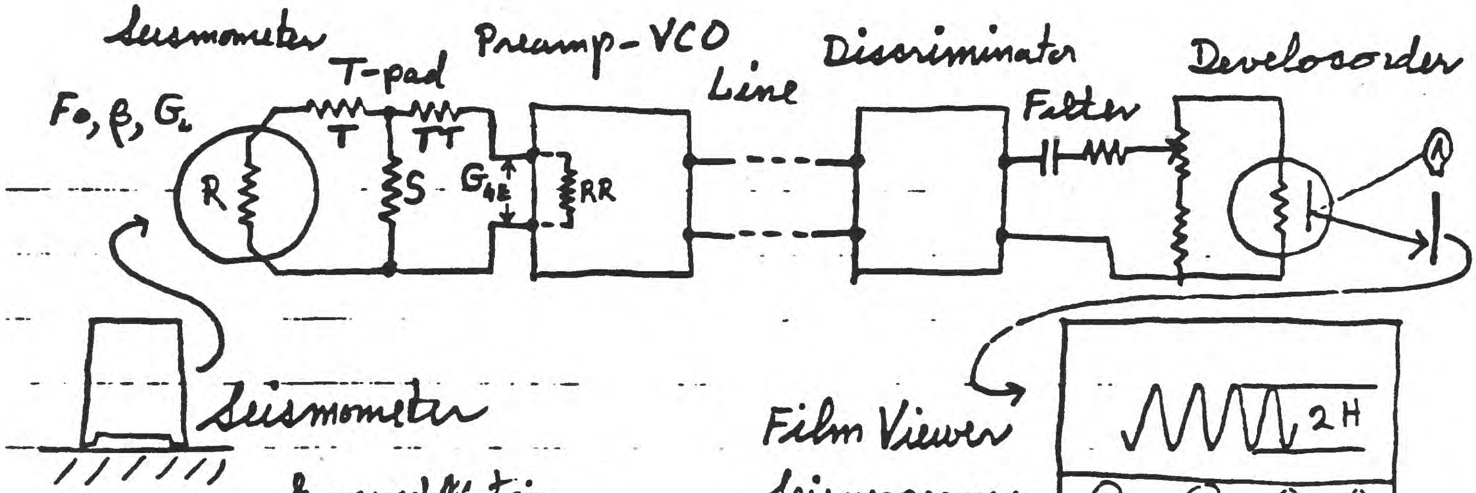


Figure 10. A. Comparison of T phases for the main Chilean earthquake and its principal foreshocks. Recorded amplitudes from the Desert seismograph are plotted as a function of T-phase traveltime. This evidence suggests the duration of faulting associated with the earthquake (5 to 7 minutes) as well as the "intensity" of faulting as a function of time.

B. Record of the tsunami of May 23, 1960, from measurements made at the Wailuku River bridge. Each dot represents a measurement. This report includes an eyewitness report of the impact of the tsunami on Hilo as well as an early description of the destruction that it caused.



Ground Motion

$\xi = h \sin 2\pi f t$

Seismogram

$Z = H \sin(2\pi f t + \varphi(f))$

$MF(f) = \frac{2H}{2h}(f)$ Magnification at frequency f

$\varphi(f)$ Phase angle at frequency f

Harmonic response of seismometer mass:

$$z(t) = -\frac{h f^2 \sin\left[2\pi f t + \tan^{-1}\left(\frac{-2\beta f F_0}{F_0^2 - f^2}\right)\right]}{\left[(F_0^2 - f^2)^2 + 4\beta^2 F_0^2 f^2\right]^{1/2}}$$

emf produced across the preamp input:

$$EP(t) = G_{LE} \cdot \dot{z}(t) = -\frac{2\pi h f^3 G_{LE} \cos\left[\left(2\pi f t + \tan^{-1}\left(\frac{-2\beta f F_0}{F_0^2 - f^2}\right)\right)\right]}{\left[(F_0^2 - f^2)^2 + 4\beta^2 F_0^2 f^2\right]^{1/2}}$$

Peak-to-peak emf across the preamp input:

$$EP(f)_{P-P} = \frac{4\pi h f^3 G_{LE}}{\left[(F_0^2 - f^2)^2 + 4\beta^2 F_0^2 f^2\right]^{1/2}}$$

2 Units: h[mm], G_{LE}[μv/mm/sec], $EP(f)_{P-P}$[μv_{P-P}]

Figure 11. Diagram of USGS seismic system recorded on the Develocorder, and derivation of the peak-to-peak emf developed across the preamp input in terms of the parameters of the seismometer and the amplitude and frequency of the ground motion (from Eaton, 1975A).

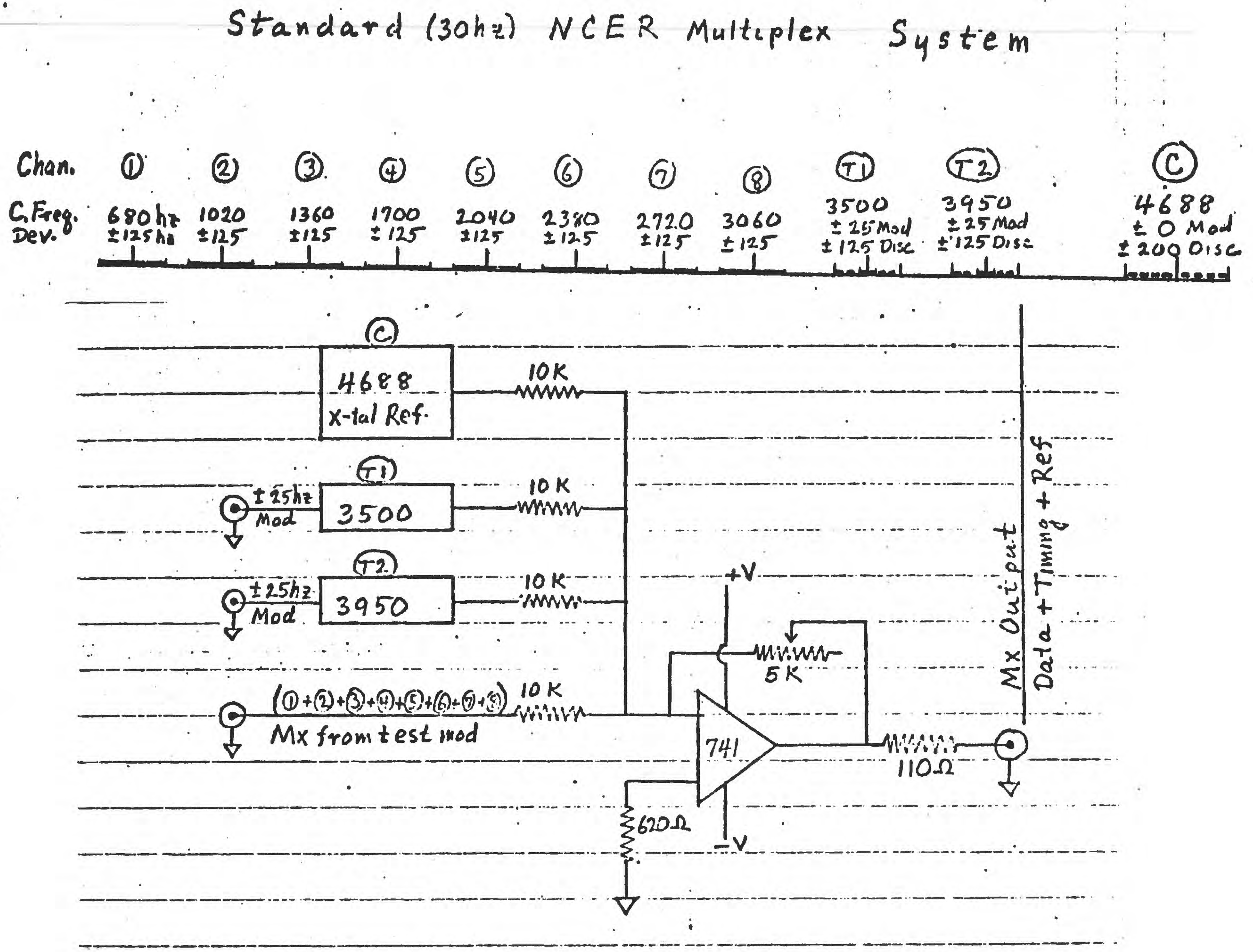


Figure 12. Augmented USGS telemetry standard and diagram of the circuit for combining the incoming multiplexed field signal with timing and compensation signals prior to recording on the Bell and Howell tape recorder.

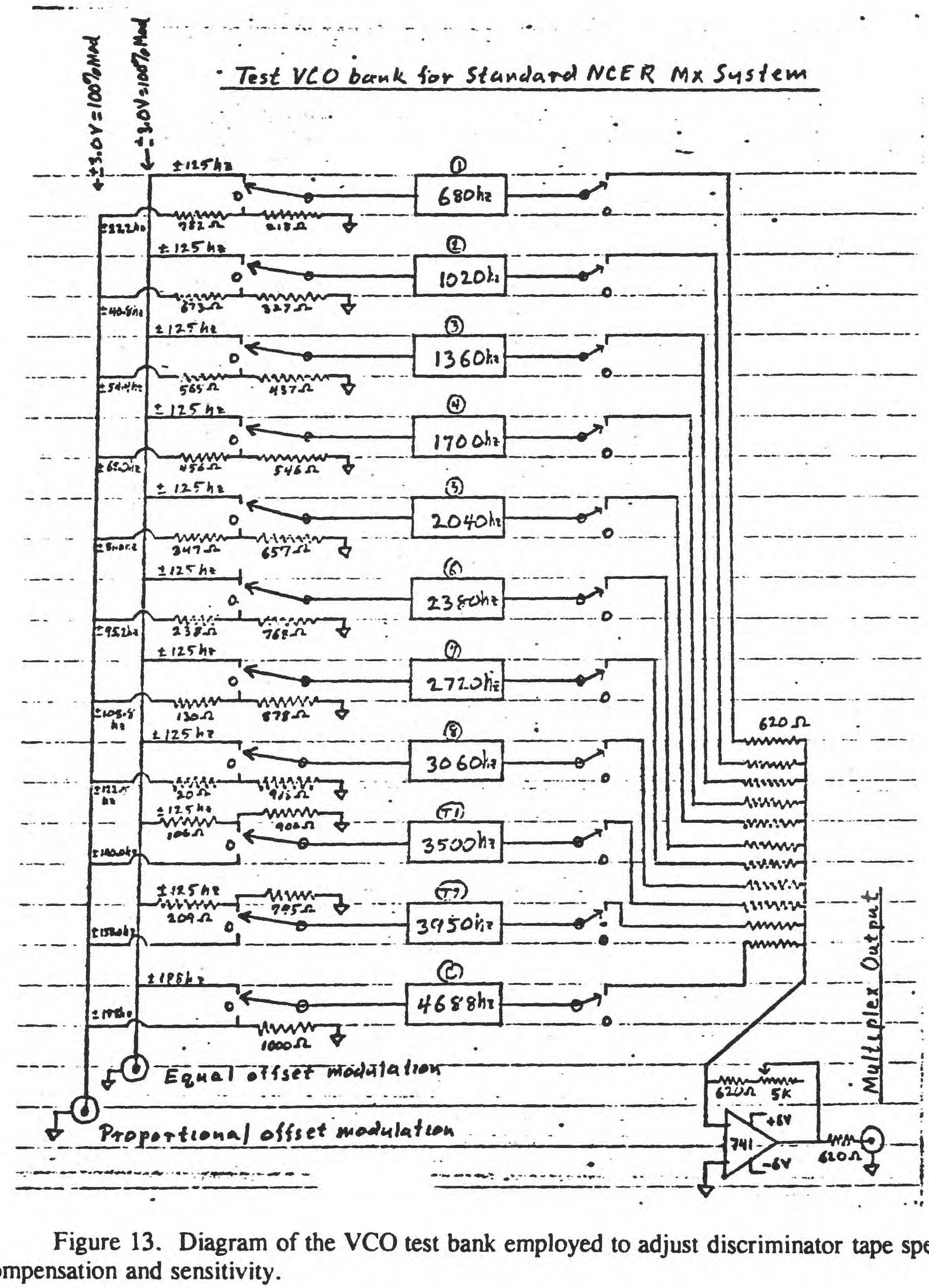


Figure 13. Diagram of the VCO test bank employed to adjust discriminator tape speed compensation and sensitivity.

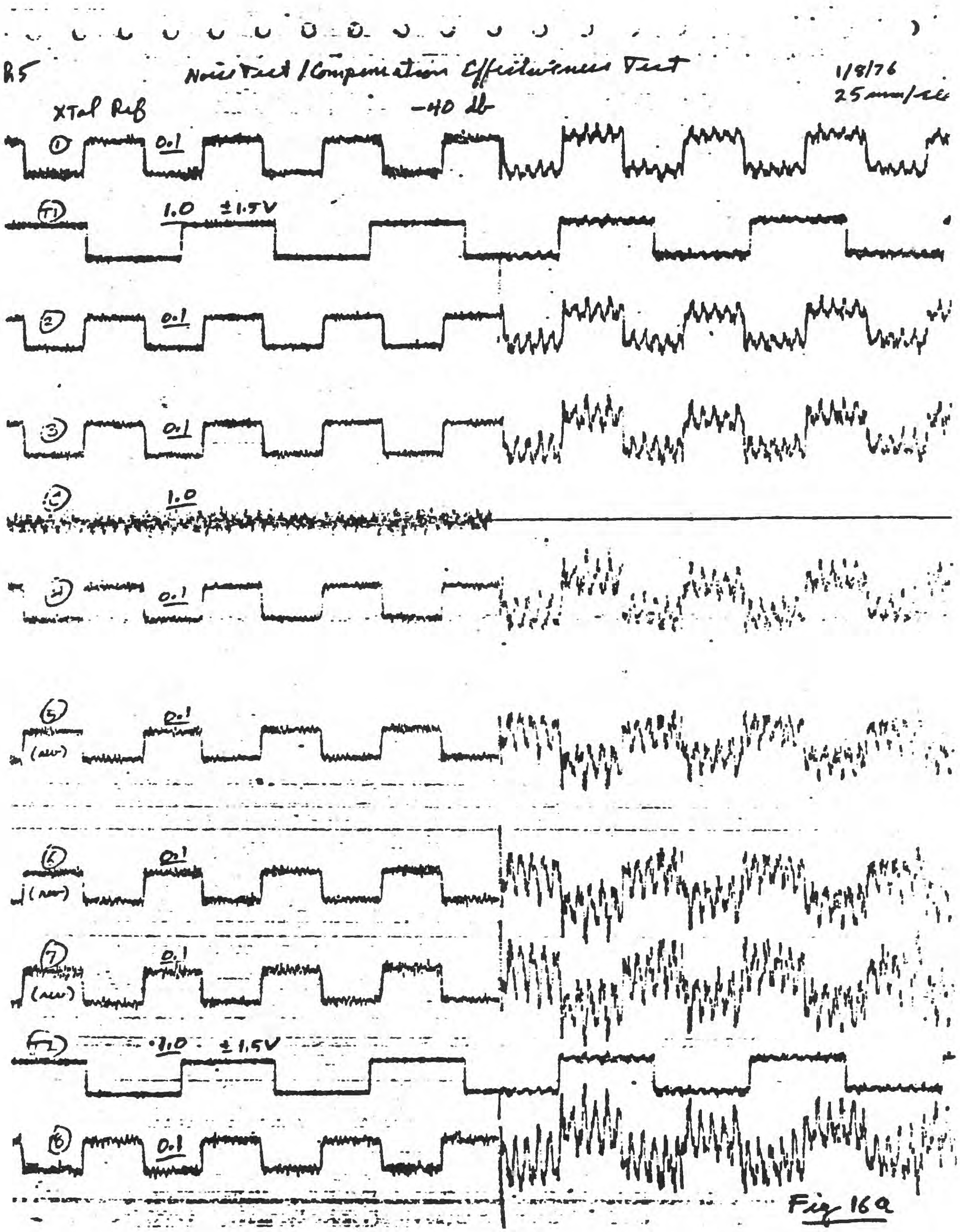


Figure 14. Tape speed compensation test of the standard USGS multiplex system recorded on the Bell and Howell recorder. All data channels (1 through 8) were modulated by a 1 hz square wave at the 1% modulation level (i.e., at the -40 db level). Tape speed compensation was applied on the left half of the figure and was turned off on the right half. With tape speed compensation, tape noise was reduced to a level of about -50 db.

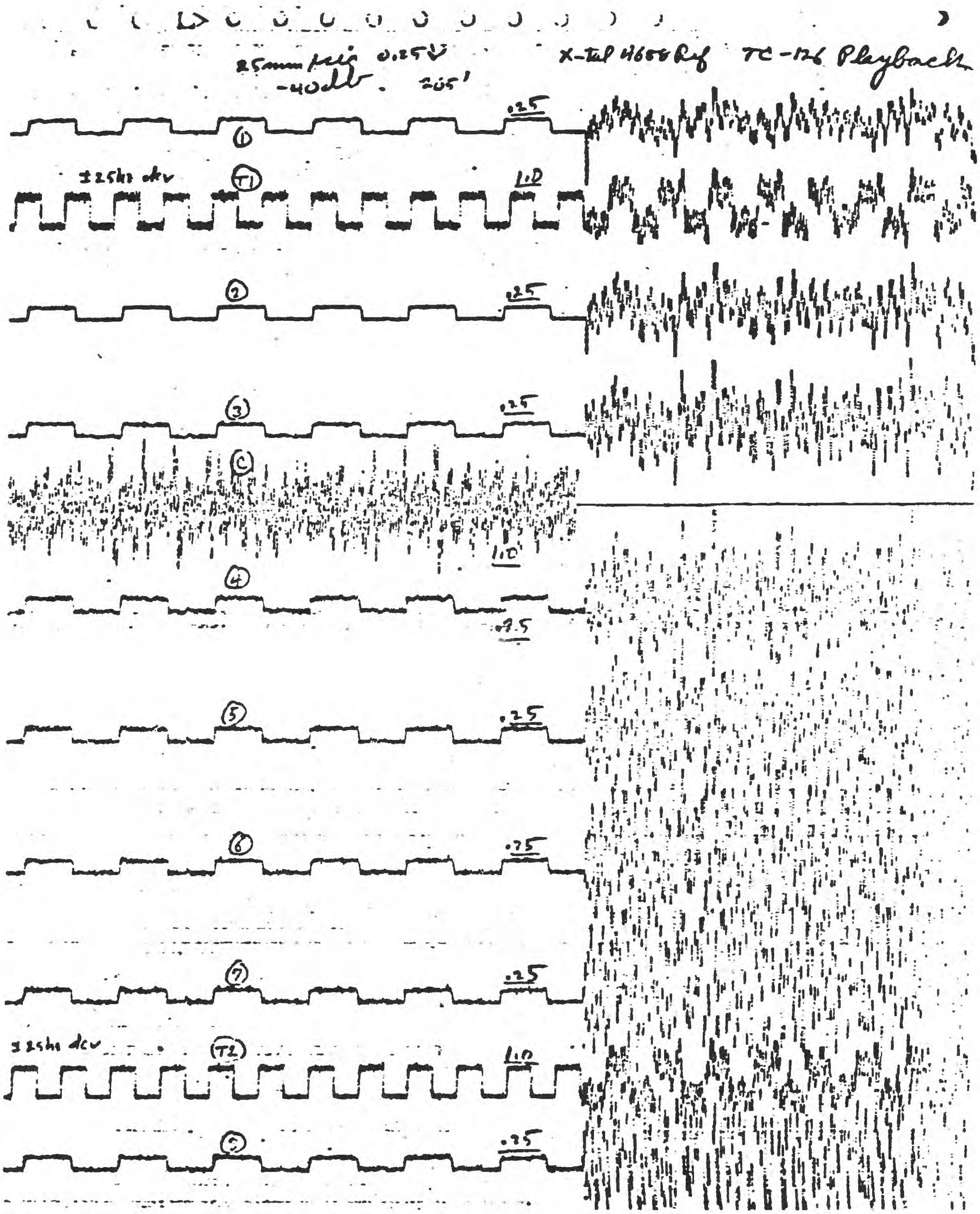


Figure 15. Tape speed compensation test of the standard USGS multiplex system recorded on the Sony TC126 tape recorder. All data channels were modulated by a 1 hz square wave at the 1% level (-40 db). Tape speed compensation was applied on the left half of the figure and turned off on the right half. With compensation, tape noise was reduced to about -46 db.

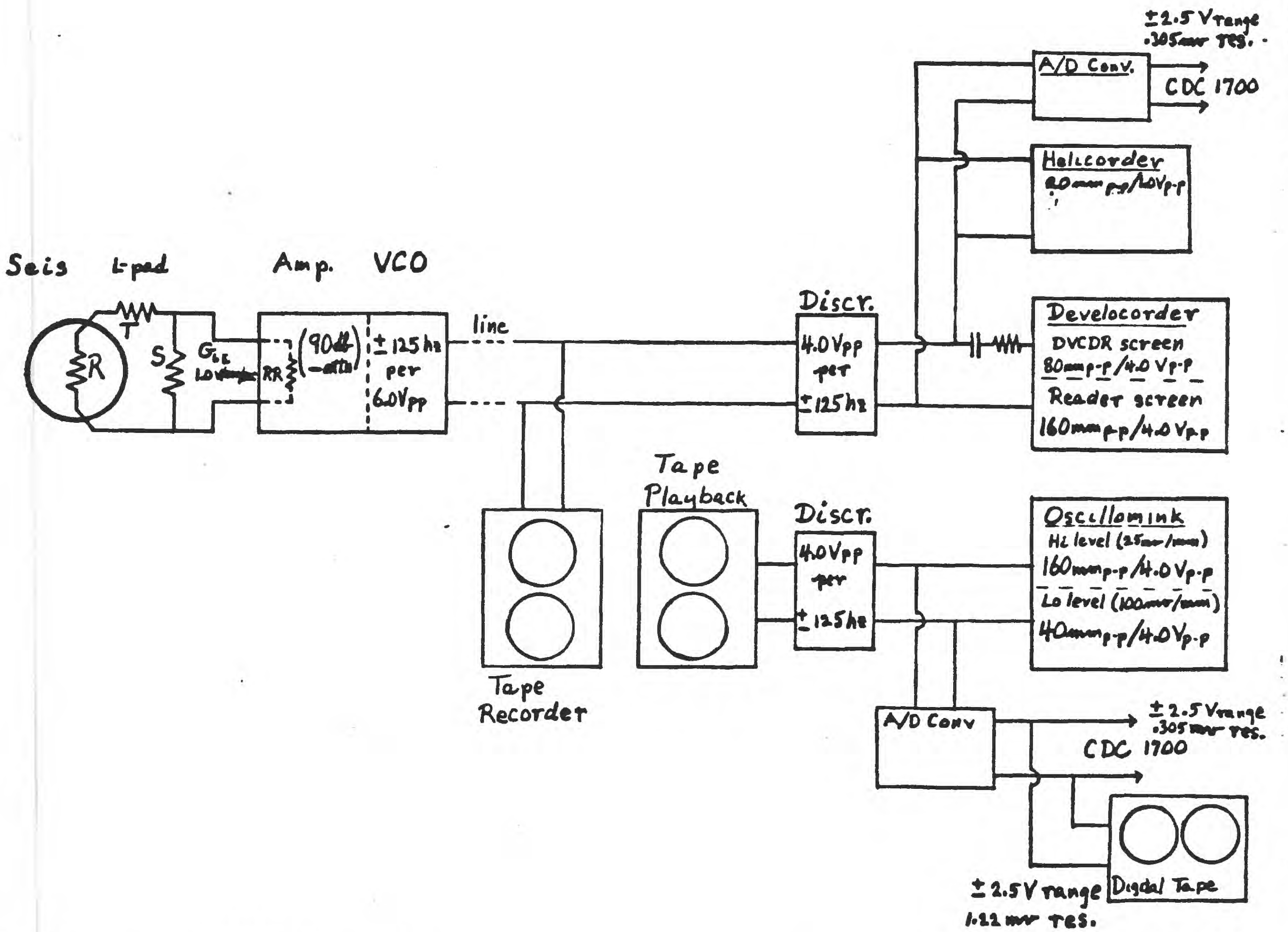


Figure 16. Block diagram of the USGS short-period seismic system (as of 1975).

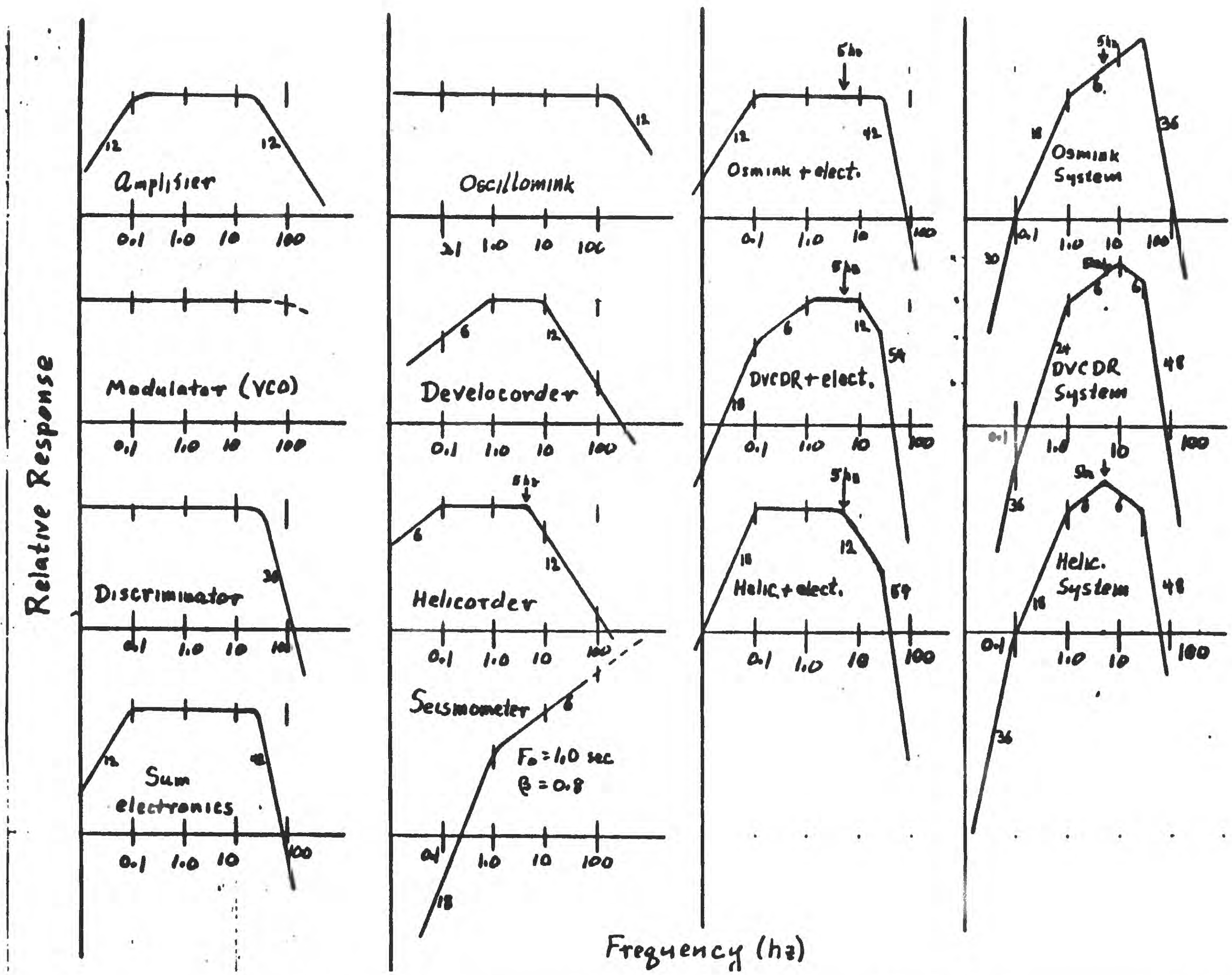


Figure 17. Relative (asymptotic) frequency responses of individual components of the USGS short-period seismic system and of various combinations of components, including the overall responses of the system when recorded on 1) the Siemens Oscillomink, 2) on the Develocorder film strip recorder, and 3) on the Helicorder drum recorder.

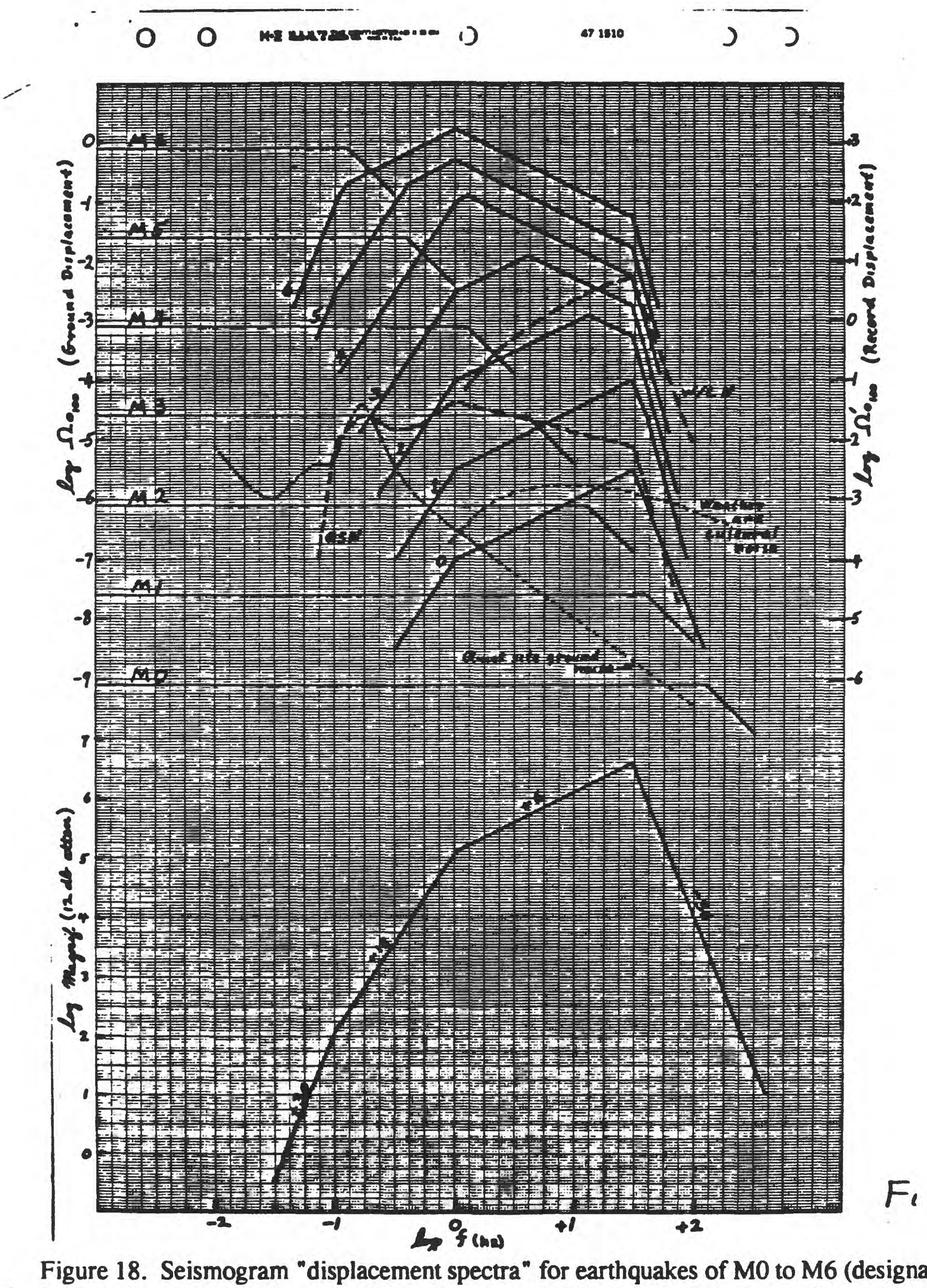


Figure 18. Seismogram "displacement spectra" for earthquakes of M0 to M6 (designated by 1, 2, 3,..6) expected at a recording distance of 100 km. Calculated from "Brune" displacement spectra (designated by M0, M1,...M6) at 100 km for a 5 bar stress drop and from the USGS seismic system response curve (for 12 db attenuation) shown at the bottom of the figure. Also shown are the quiet site ground motion displacement spectrum and the corresponding seismogram noise displacement spectrum (QSN).

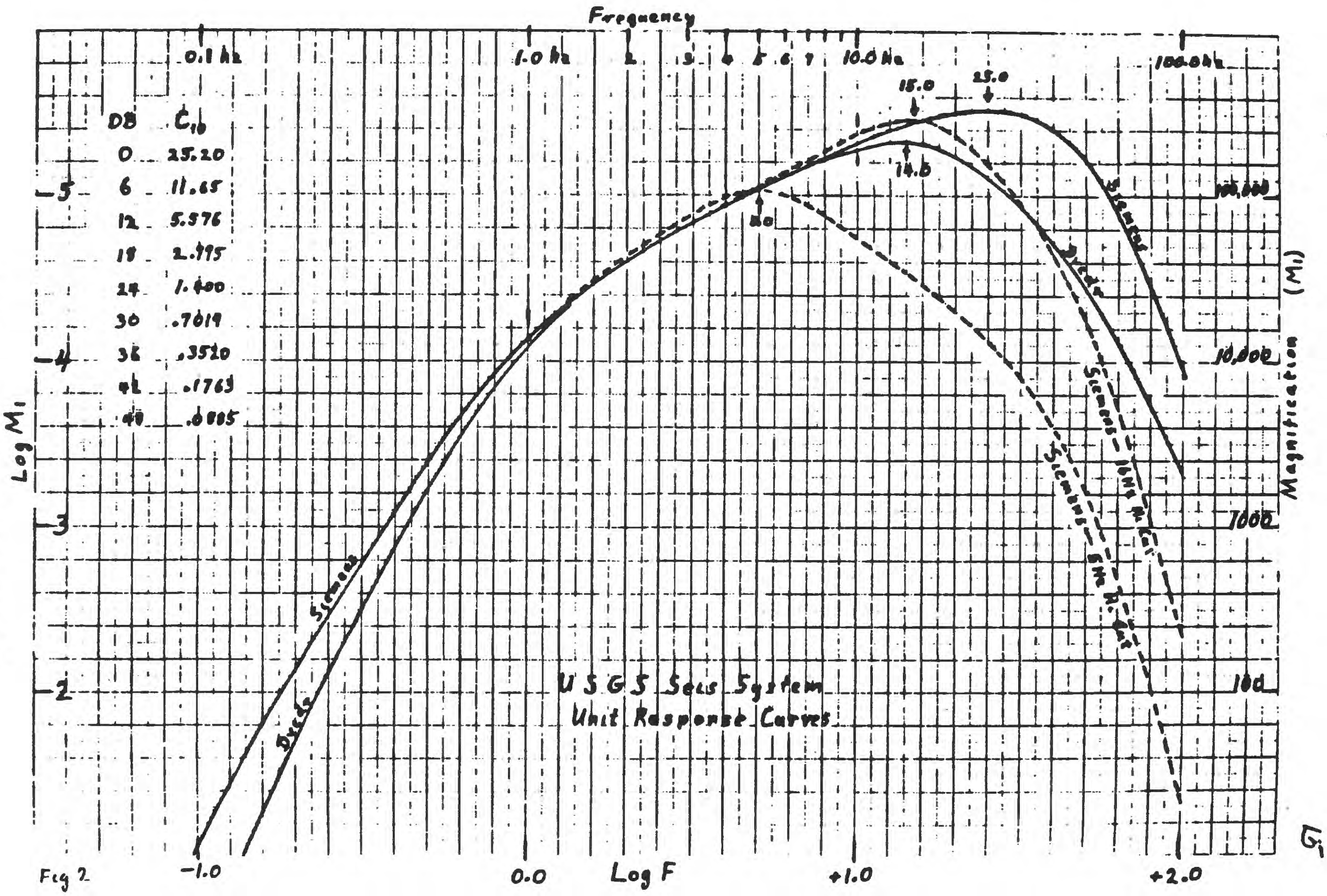


Figure 19. USGS seismic system "unit" response curves for various recorders and filters: Siemens unfiltered, Siemens with 12 db/octave 16 hz high-cut filter, Siemens with 12 db/octave 5 hz high-cut filter, and Develocorder. Multiplication factors (C_{10}) corresponding to attenuator settings (DB) from 0 db to 48 db are shown on the upper left.

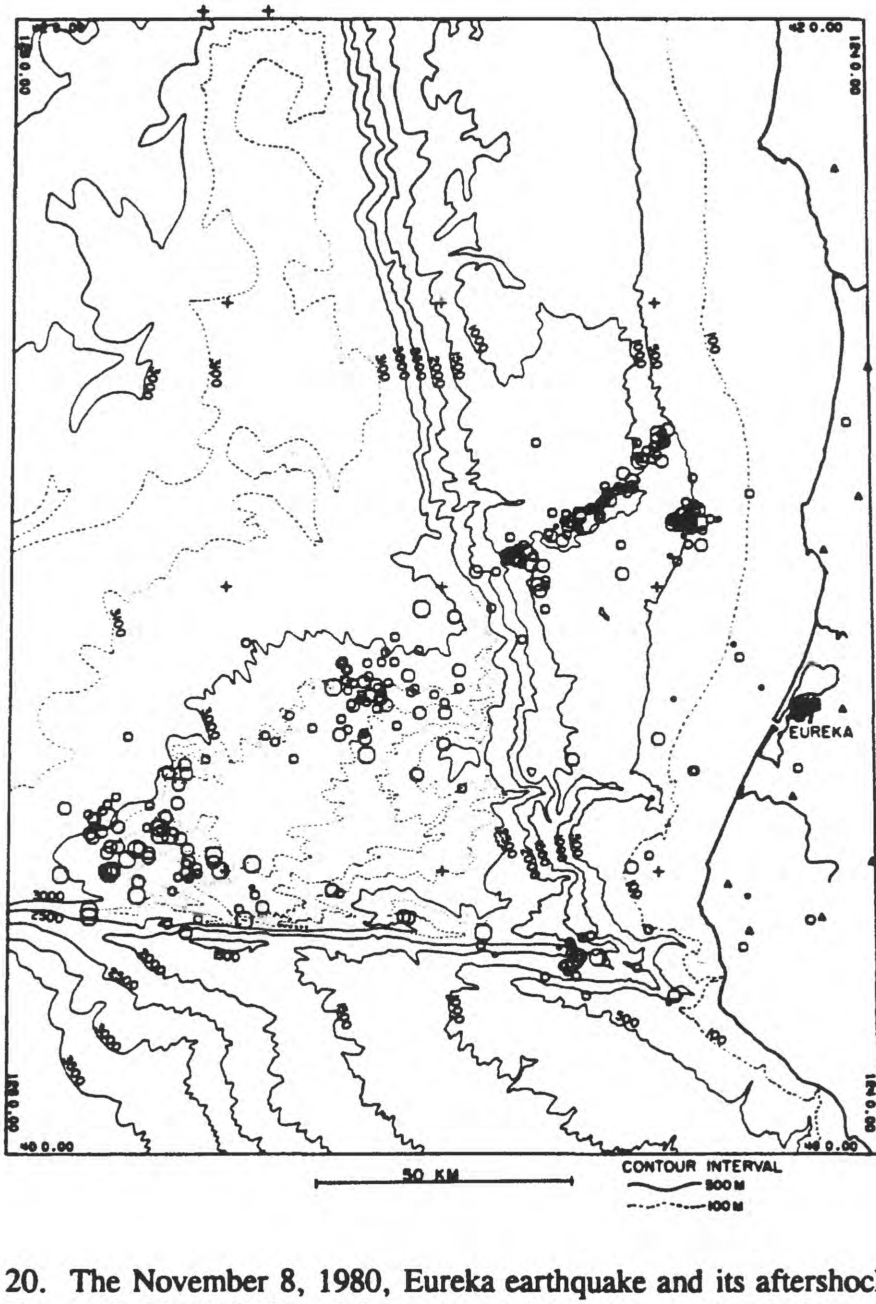


Figure 20. The November 8, 1980, Eureka earthquake and its aftershocks (M $\geq$ 1.6, Nov. 8 - Dec. 3) recorded by NCSN and 8 portable 5-day tape-recorder stations deployed along the coast between Cape Mendocino and Crescent City. The main shock is near the center of the dense band of aftershocks beneath the continental shelf at the northeast end of the principal NE-SW trending aftershock zone.

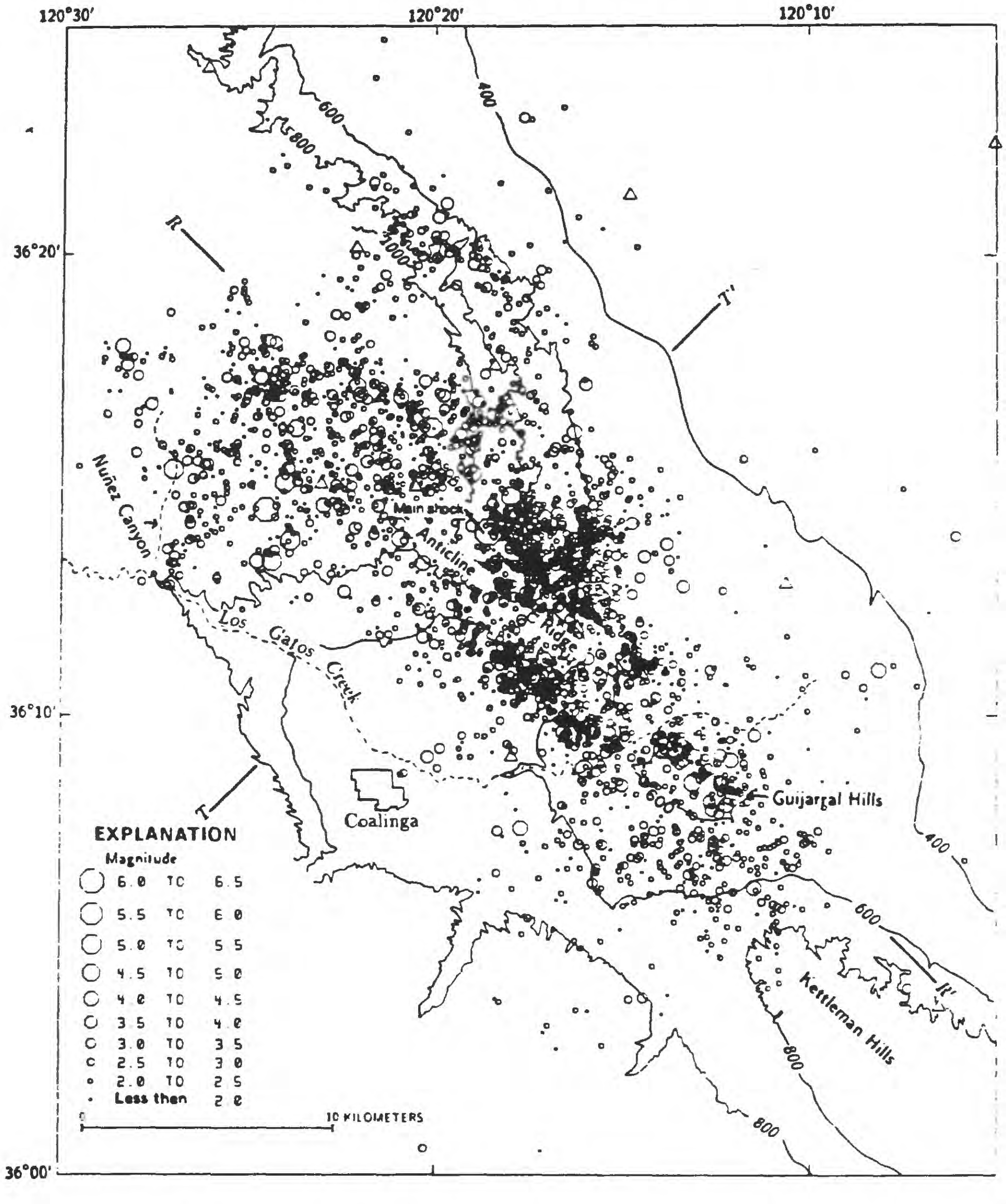


Figure 21. Coalinga, Calif., area, showing locations of May 2 earthquake and aftershocks from May 2 through Sept. 30. Contours below 400 and above 1000 ft. are omitted for simplicity. Aftershock screening parameters: magnitude $\leq$ 1.7; rms traveltime residuals $\leq$ 0.20 sec; number of stations in solution $\geq$ 10; distance to nearest station $\leq$ 30 km; estimated epicentral error $\leq$ 3 km.

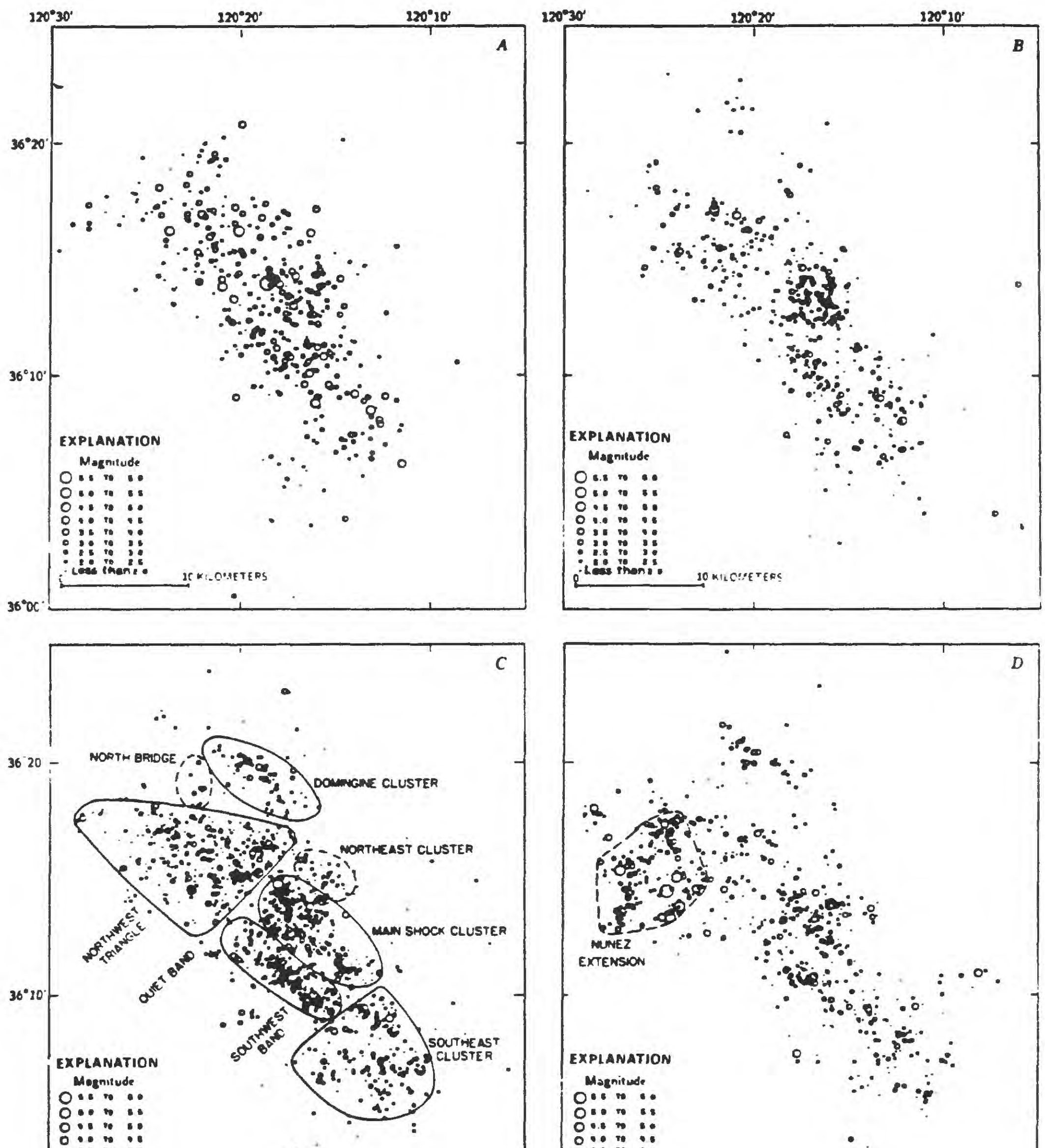


Figure 22. Coalinga, Calif., area, showing locations of May 2 earthquake and aftershocks for four periods: May 2-3 (A), May 4-5 (B), May 6 - June 10 (C), and June 11 - September 30 (D). Aftershock screening parameters same as in Figure 21. Panel B (May 4-5) shows the effect of increased station density (portables) on reducing scatter in epicenter locations. The principal clusters of aftershocks that made up the overall aftershock sequence are shown in panels C (May 6 - June 10) and D (June 11 - September 30).

(*M*=6.0) were excluded. The plotted points represent the log number of events in each ¼-magnitude unit from *M*=1.5 to 7.0. The line fitted by inspection to the May 2–September 30 data has a slope of −0.87. The slope of the curve fitted to the same data (but including the *M*=6.7 main shock and *M*=6.0 aftershock) by the formula

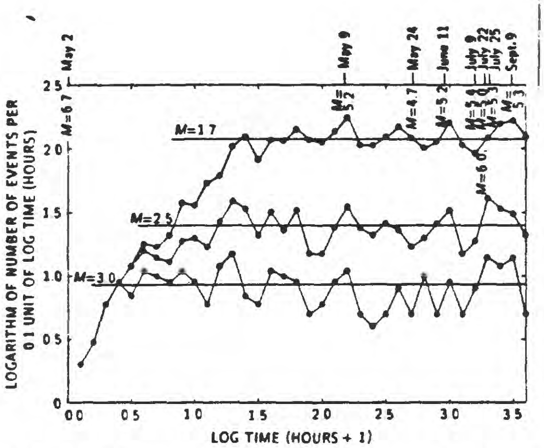


FIGURE 8.5.—Frequency versus time for Coalinga aftershocks of $M \geq 1.7$, $M \geq 2.5$, and $M \geq 3.0$. Dates and magnitudes of events of $M \geq 4.7$ are labeled at top.

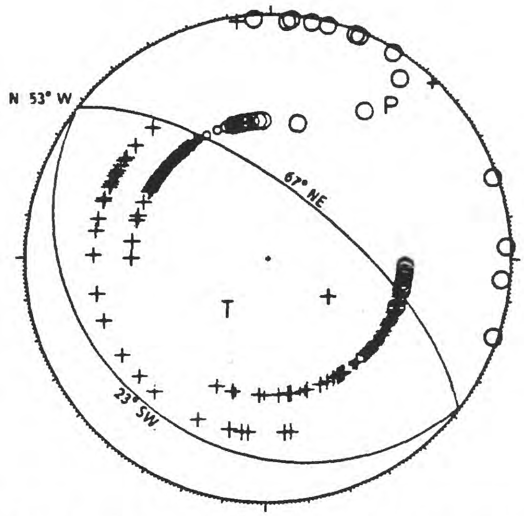


FIGURE 8.9.—First-motion focal-mechanism solution for Coalinga main shock (2342 G.m.t. May 2, 1983). Crosses, compressional first arrivals; circles, dilatational first arrivals; P, direction of maximum compressive stress (*P*-axis); T, direction of minimum compressive stress (*T*-axis). Less certain first arrivals are shown by smaller symbols. Figure drawn by FPPLOT (Reasenberg and Oppenheimer, 1985).

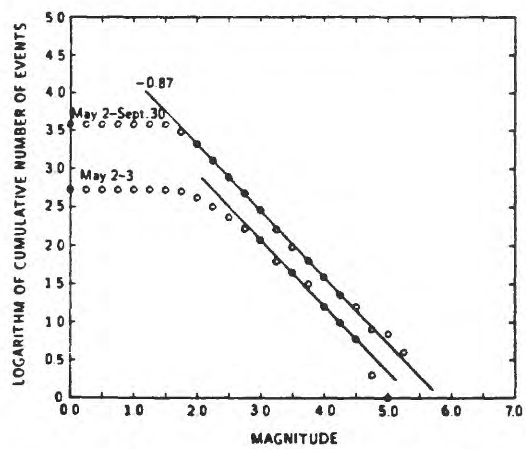


FIGURE 8.6.—Frequency versus magnitude for Coalinga aftershocks during first day (May 2–3) and first 5 months (May 2–Sept. 30) of the earthquake sequence. $M = 6.7$ main shock and $M = 6.0$ July 22 aftershock are omitted. Application of Utsu's (1961) formula gives $b = -0.88$ for longer data interval and for $M \geq 2.0$.

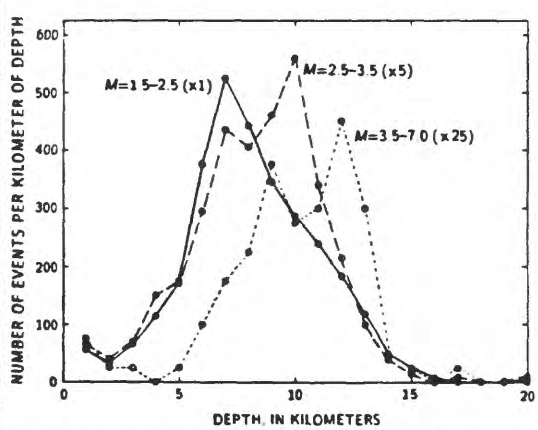


FIGURE 8.7.—Depth versus frequency for three ranges of earthquake magnitude. Ordinates of curves for $M = 1.5$–2.5, $M = 2.5$–3.5, and $M = 3.5$–7.0 have been multiplied by 1.0, 5.0, and 25.0, respectively, to facilitate comparison. Points are plotted at upper end of depth range—for example, at 5 km for depth interval 4–5 km.

Figure 23. First motion plot of the main Coalinga shock (upper right), and analytical plots of earthquake frequency versus time (upper left), earthquake frequency versus magnitude, (lower left), and earthquake frequency versus depth (lower right).

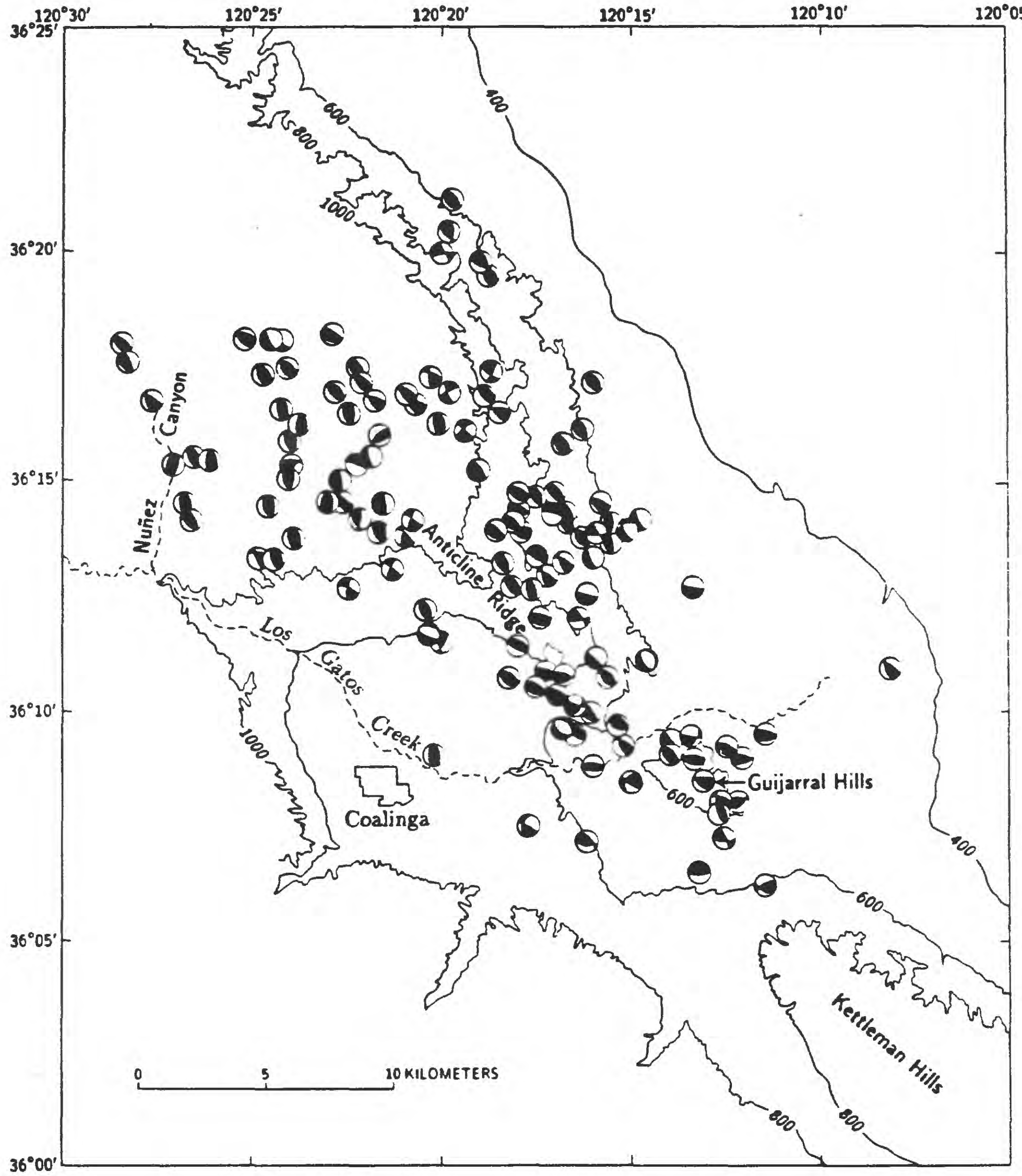


Figure 24. Focal mechanisms of the May 2 Coalinga earthquake and its larger aftershocks. On focal-mechanism symbols, dark areas represent compressional first arrivals, and white areas represent dilatational first arrivals. Symbols are not scaled according to magnitude.

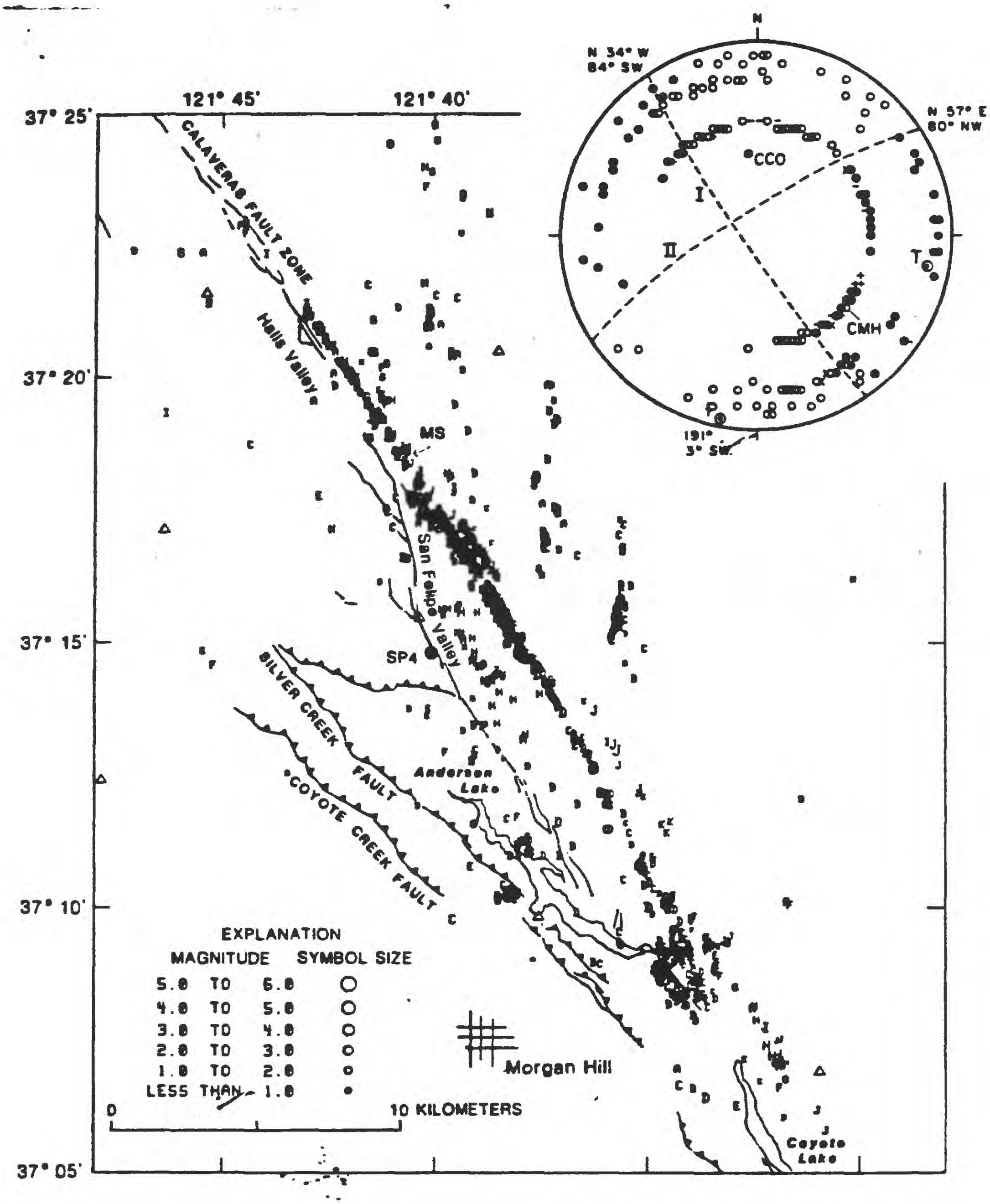
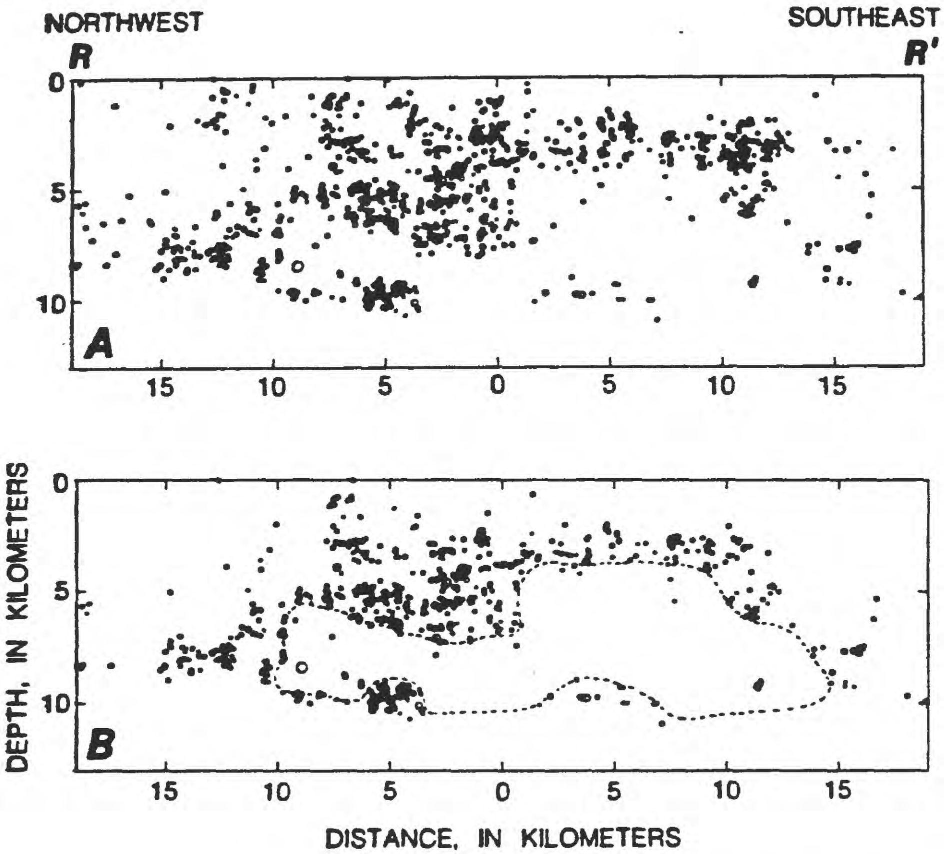


Figure 25. Sketch map of the Morgan Hill, Calif., area, showing locations of epicenters of April 24 main shock (MS) and its aftershocks through September 30, 1984. Magnitude and focal depth are indicated by symbol size and letter; A, $0 < H \leq 1$; B, $1 ,< H \leq 2$ km, etc. Solid fault traces from D. G. Herd (unpubl. data, 1982). Sawteeth, thrust faults, barbs on upper plate. Triangles, seismic stations in permanent network.

(Inset) Equal-area lower-hemisphere first-motion plot, showing focal plane solution for the main shock. Dots and crosses, certain and questionable compressional first arrivals, respectively. Circles and dashes, certain and questionable dilatational first arrivals, respectively. X's, conflicting first arrivals. P and T, inferred axes of maximum and minimum compression, respectively. I and II, nodal planes. CCO and CMH, discordant first motion readings.

pattern are also well defined. The shallow alignments of aftershocks northeast of the principal fault are well separated from it. A diffuse horizontal zone of aftershocks at 5- to 8-km depth extends outward to



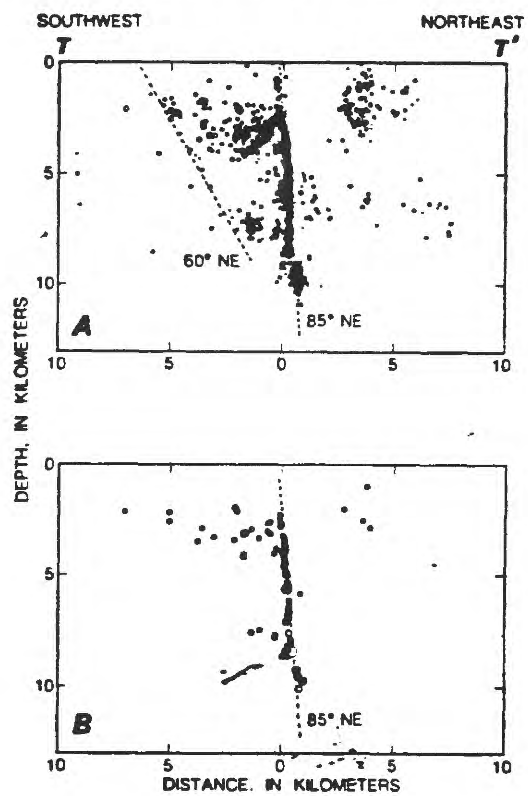
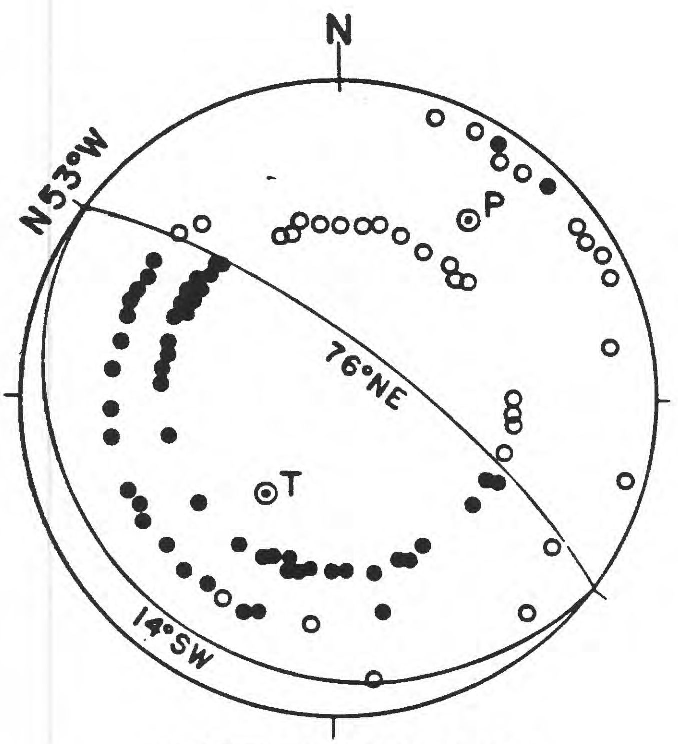


Figure 26. (Left) Longitudinal sections of Morgan Hill aftershock distribution along the principal line of aftershocks (Figure 25). A, All events within 10 km of the line of aftershocks. B, All events within 1.3 km northeast and 0.8 km southwest of the line of aftershocks. The area surrounded by the dashed line (in B), which contains the main shock hypocenter at its northwest end, is believed to be the area that slipped to produce the main shock.

(Right) Transverse sections of aftershock distribution perpendicular to the principal line of aftershocks (within the range R-R' on the longitudinal sections). A, All events. B, Events with M $\geq$ 2.0.



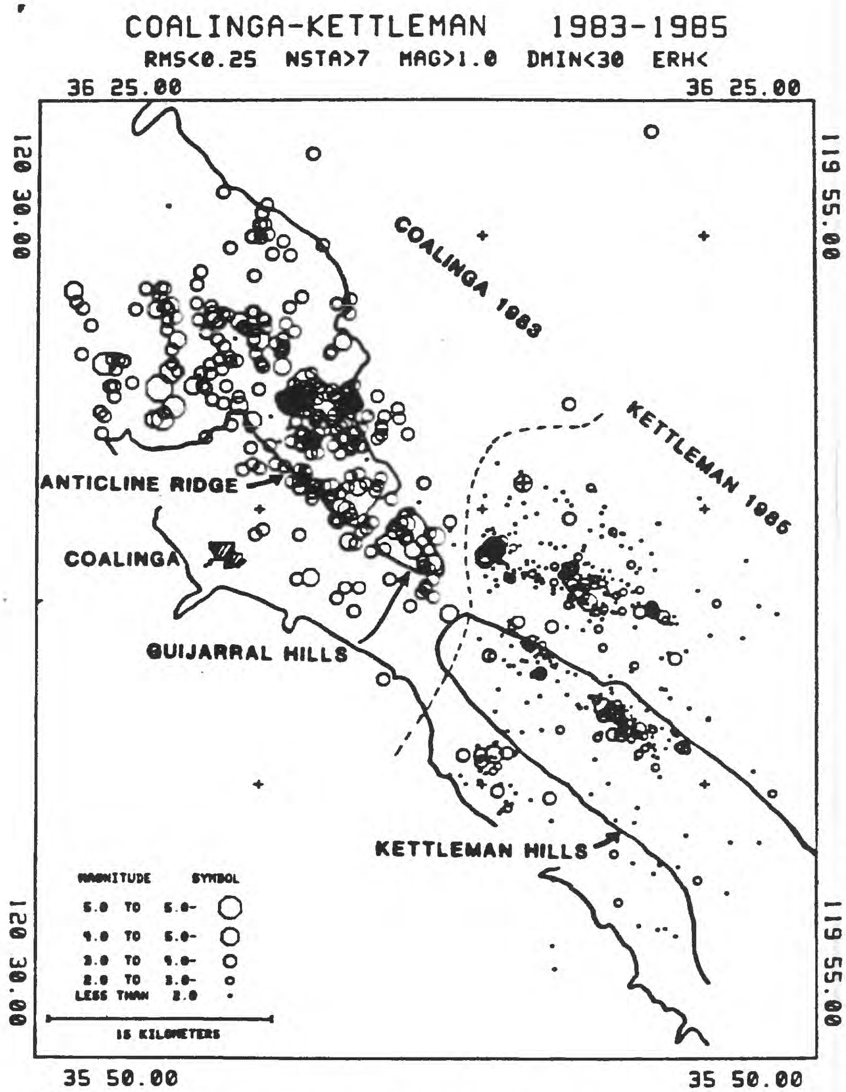


Figure 27. (Left) First motion plot and focal mechanism solution for the main Kettleman Hills earthquake.

(Right) Map of Coalinga - Kettleman Hills, Calif., area, showing the August 2, 1983, Coalinga earthquake and its aftershocks (M ≥ 3) and the August 4, 1985, Kettleman Hills earthquake and its aftershocks (M ≥ 1). The dashed line separates the two sequences, with only two discordances.

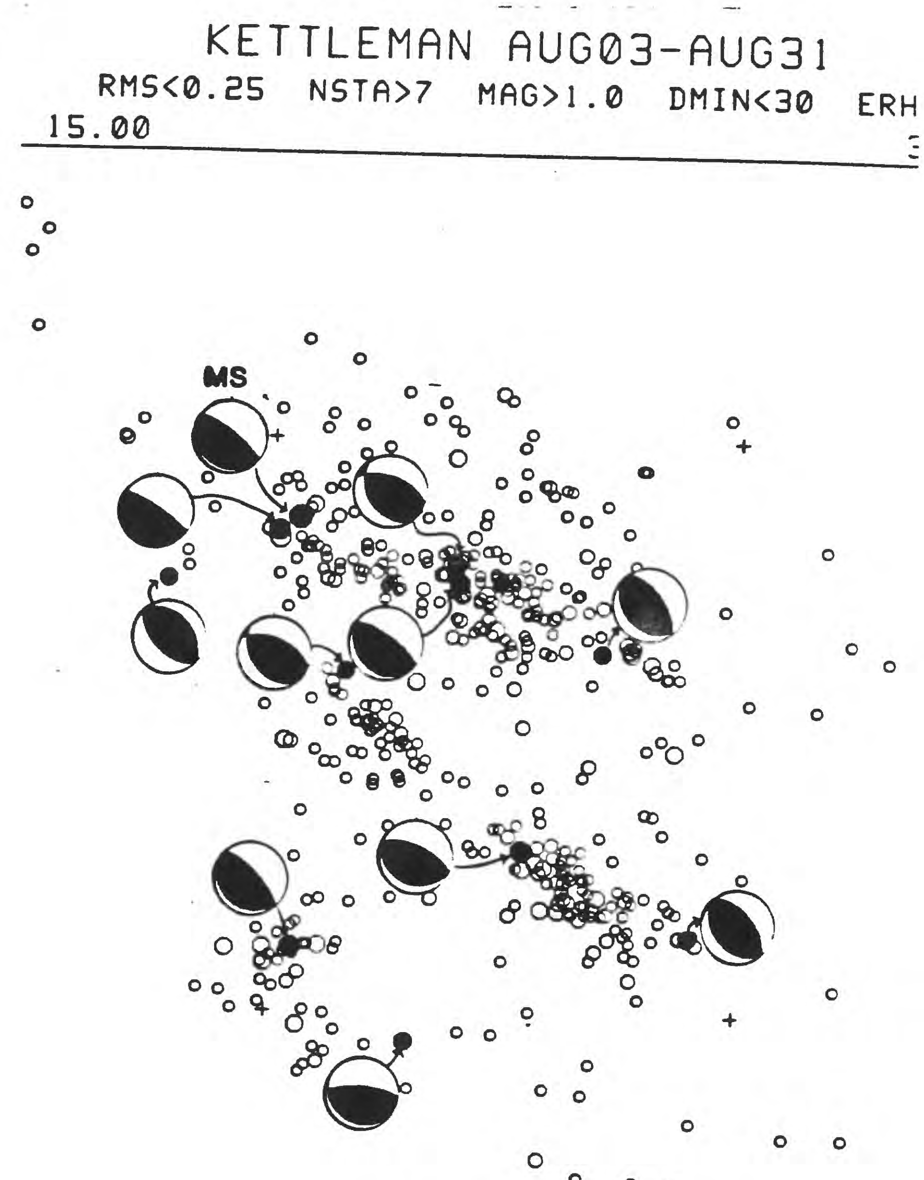


Figure 28. First motion focal mechanisms of the Kettleman Hills main shock (MS) and its 10 largest aftershocks.

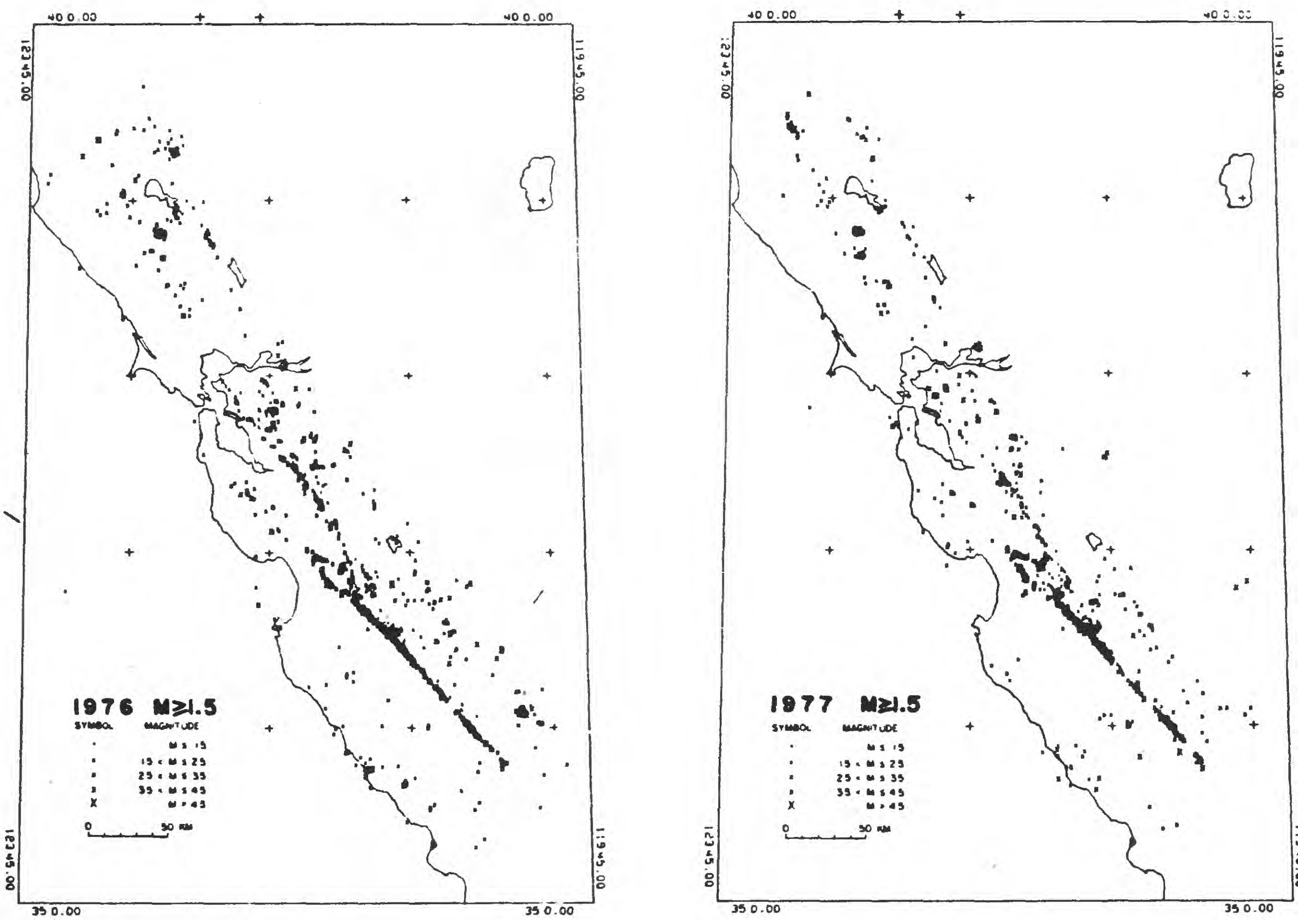


Figure 29. Epicenters of earthquakes in central California with M ≥ 1.5 during 1976 (left) and 1977 (right).

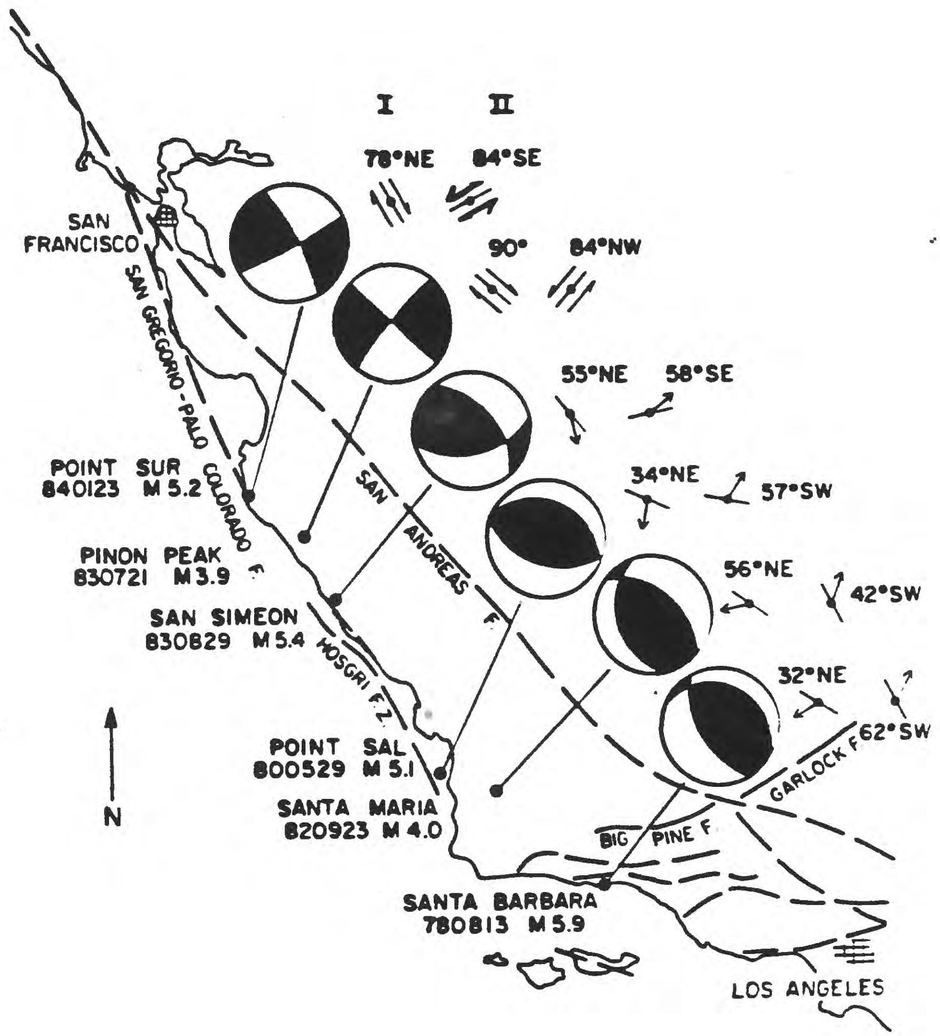


Figure 30. First motion focal mechanism solutions of 6 earthquakes along the central California coast between 1978 and 1984. Note their progressive change in character from southeast to northwest.

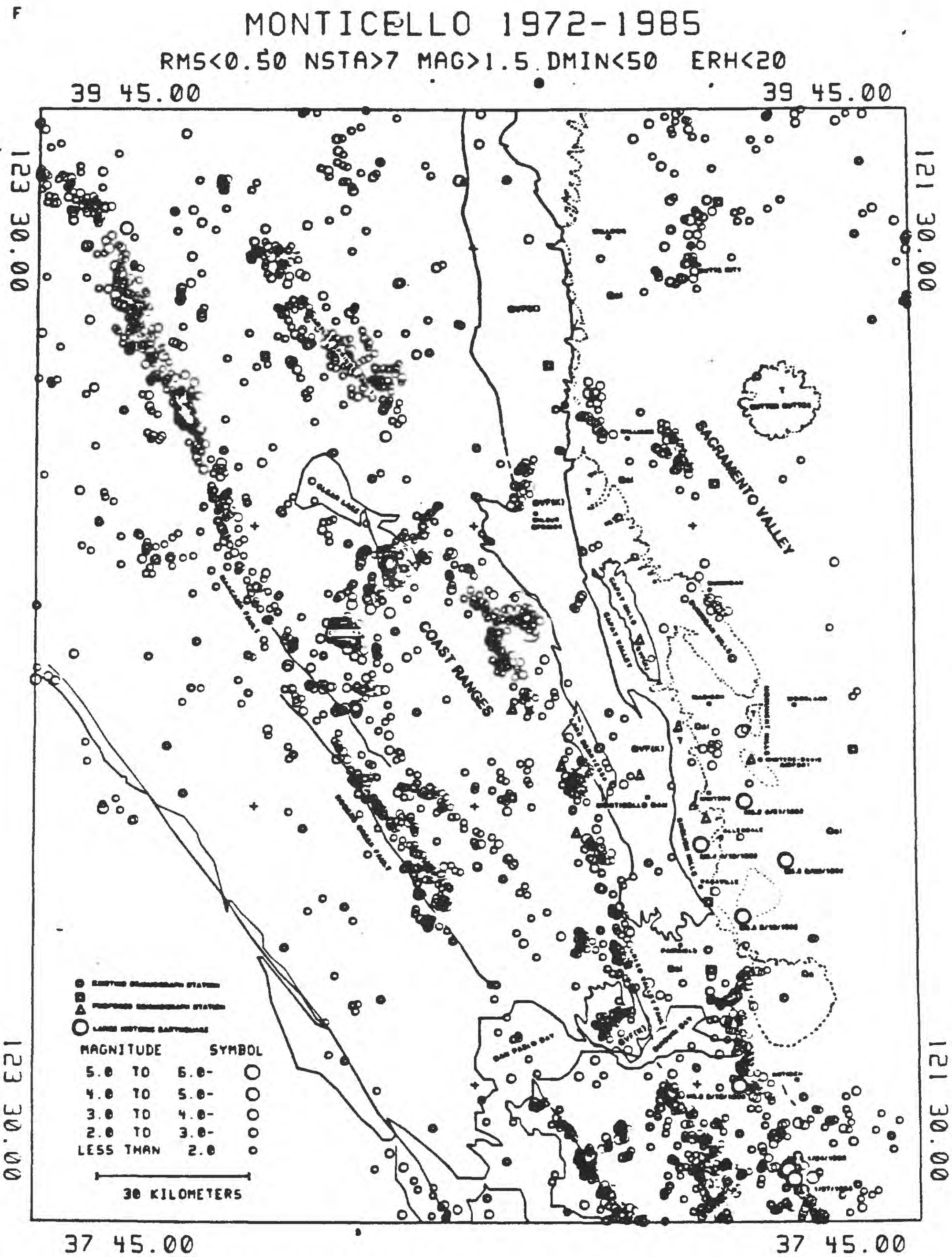


Figure 31. Network-determined epicenters of earthquakes north of San Francisco Bay from 1972 to 1985 as well as large historic earthquakes (heavy circles) along the Great Valley/Coast Ranges boundary. The large historic earthquakes lie along a sparse, interrupted line of modern seismicity that runs northward from the east end of Suisun Bay along the western edge of the Sacramento Valley. This region is also characterized by folds in the Valley sediments just east of the mountain front in a manner similar to the Coalinga/Kettleman Hills region.

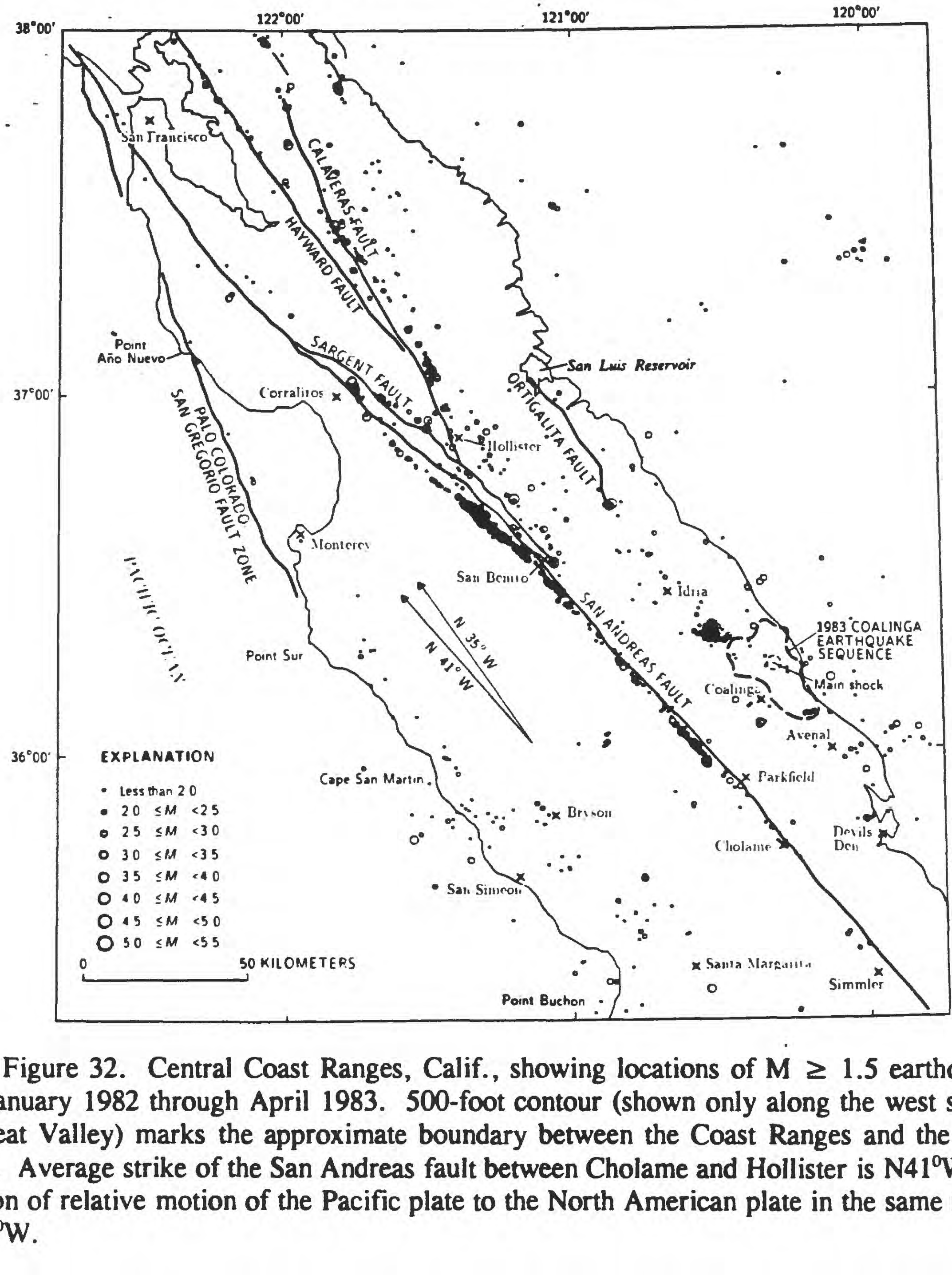


Figure 32. Central Coast Ranges, Calif., showing locations of $M \geq 1.5$ earthquakes from January 1982 through April 1983. 500-foot contour (shown only along the west side of the Great Valley) marks the approximate boundary between the Coast Ranges and the Great Valley. Average strike of the San Andreas fault between Cholame and Hollister is N41°W, and direction of relative motion of the Pacific plate to the North American plate in the same region is N35°W.

evolution of the network. The second and third seismicity maps, which cover the period January 1972–April 1983, show the cumulative distribution of earthquakes in the southern Coast Ranges (fig. 7.2) and in the southern Coast Ranges east of the San Andreas fault (fig. 7.3), respectively. These two maps are biased by the loss of

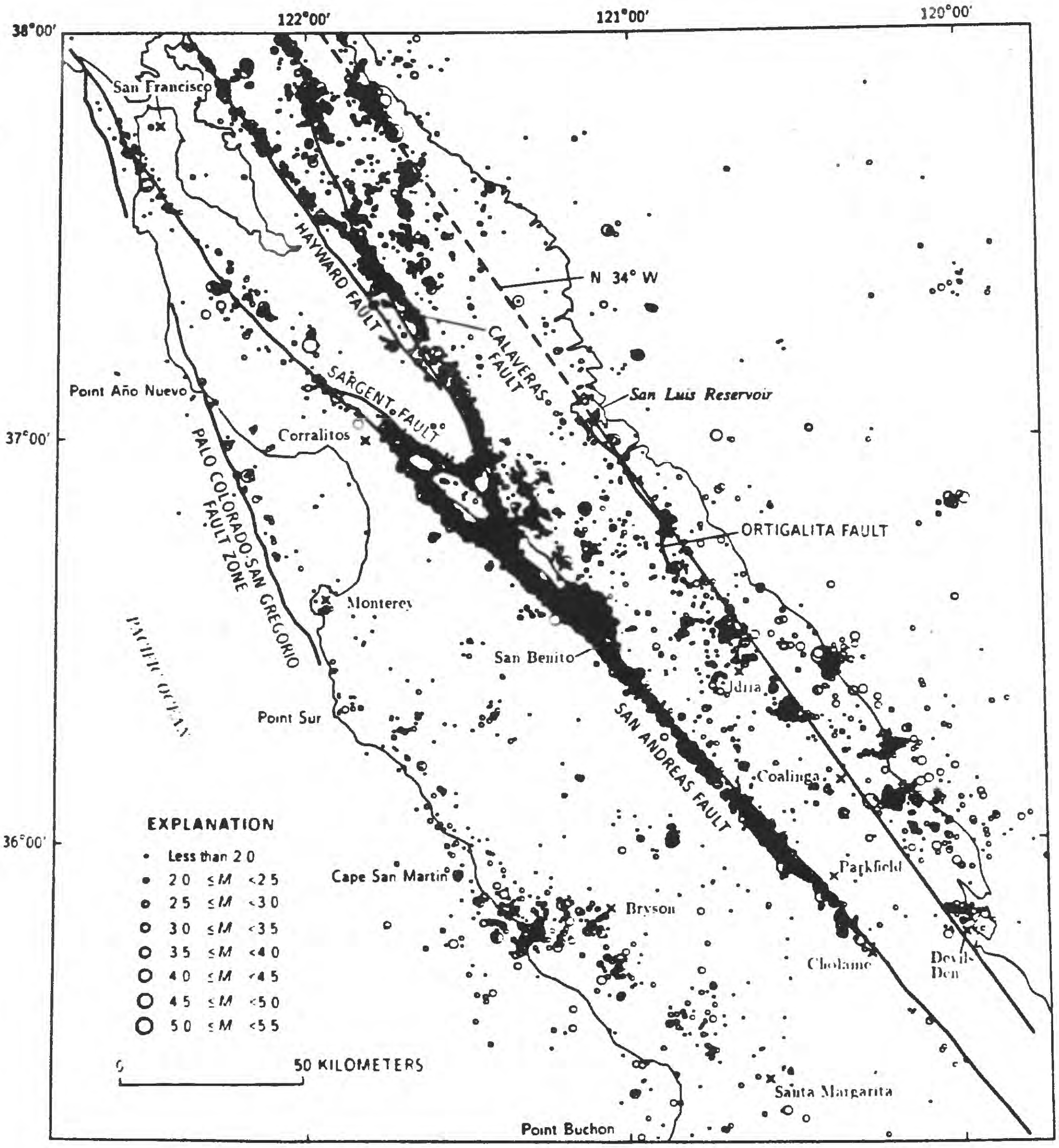


Figure 33. Central Coast Ranges, Calif., showing M ≥ 1.5 earthquakes from January 1972 through April 1983. 500-foot contour marks the approximate boundary between the Coast Ranges and the Great Valley.

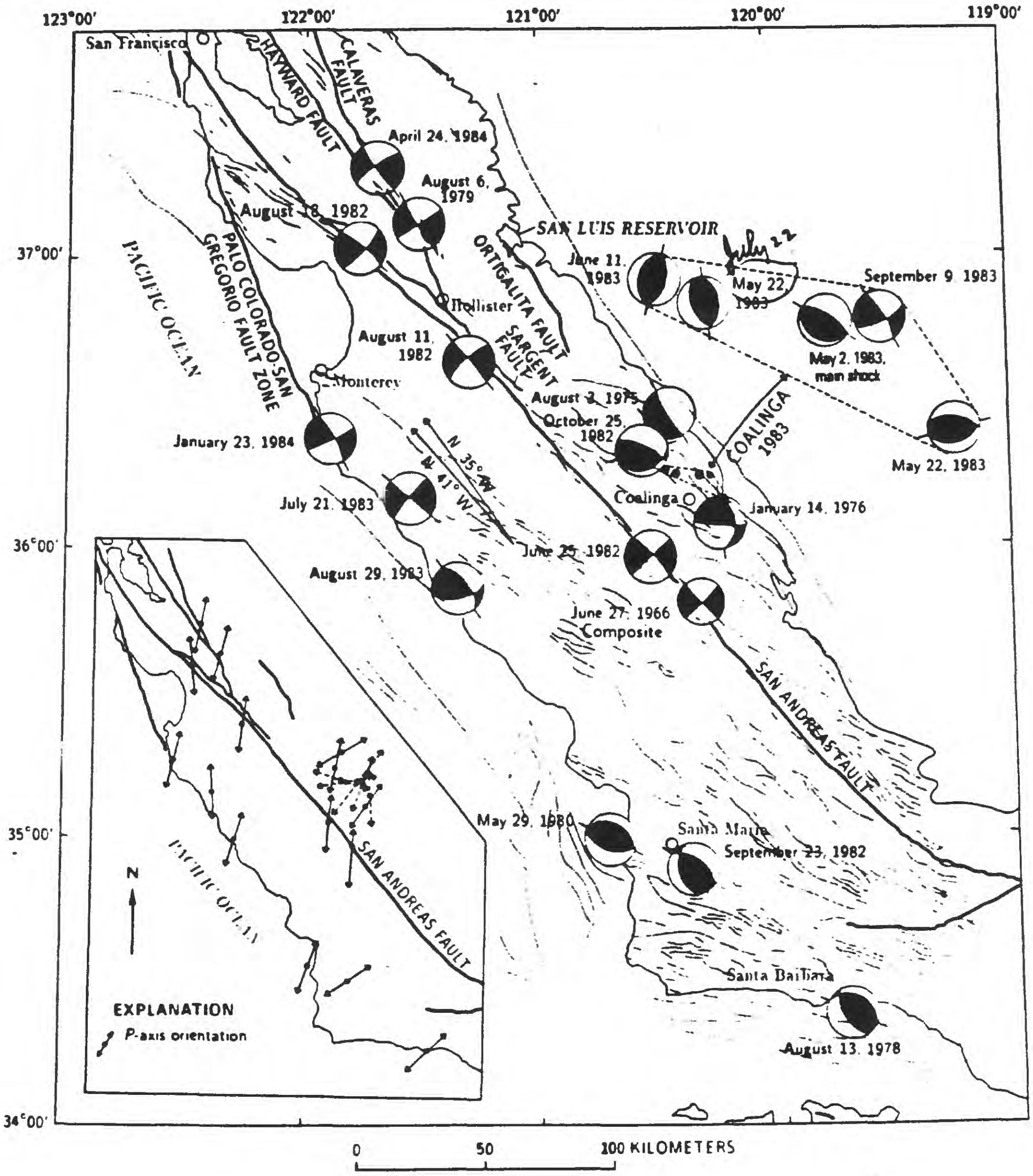


Figure 34. First motion diagrams and P-axis orientations for selected earthquakes in the southern Coast Ranges and western Transverse Ranges. Individual earthquakes are identified by their date of occurrence. First-motion diagrams of events in the 1983 Coalinga earthquake sequence are shown at an expanded scale. Inset shows P-axis orientations, with events of the Coalinga earthquake sequence plotted with dashed lines. Fold axes (light lines) after Hoskins and Griffiths (1971) and Jennings (1977).

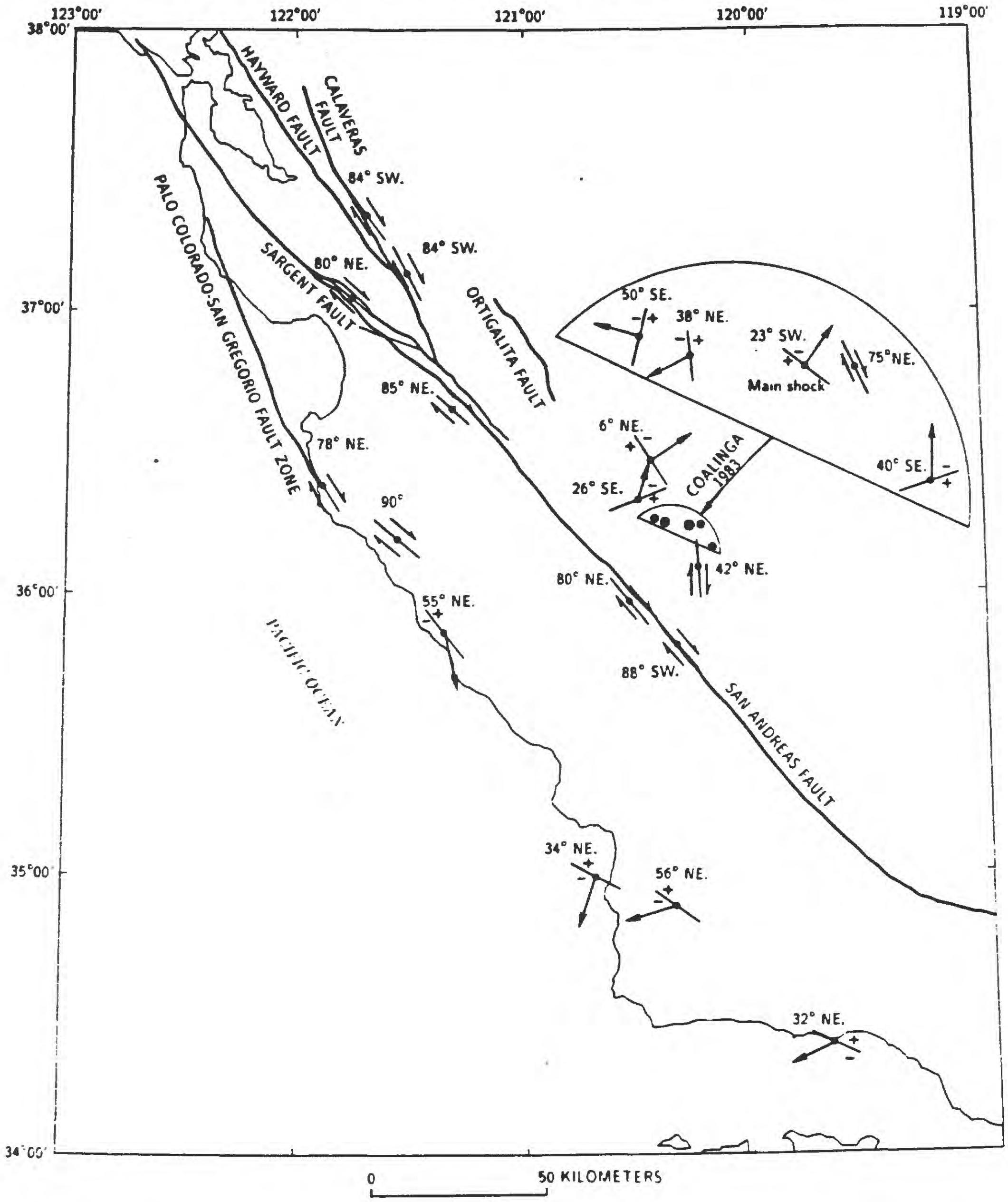


Figure 35. Fault plane orientations and slip directions for selected earthquakes in the Coast Ranges and Transverse Ranges. Strike directions are indicated by the line segment drawn through the epicenter symbol; dip angles and directions are shown by each solution. Slip sense and direction for strike-slip solutions are indicated by half-barbed pairs of arrows. For events with appreciable dip-slip displacement, an arrow indicates the direction of slip of upper plate relative to lower plate, and plus and minus signs indicate relative vertical displacement of the two plates. Events of the 1983 Coalinga earthquake sequence are shown at an expanded scale.

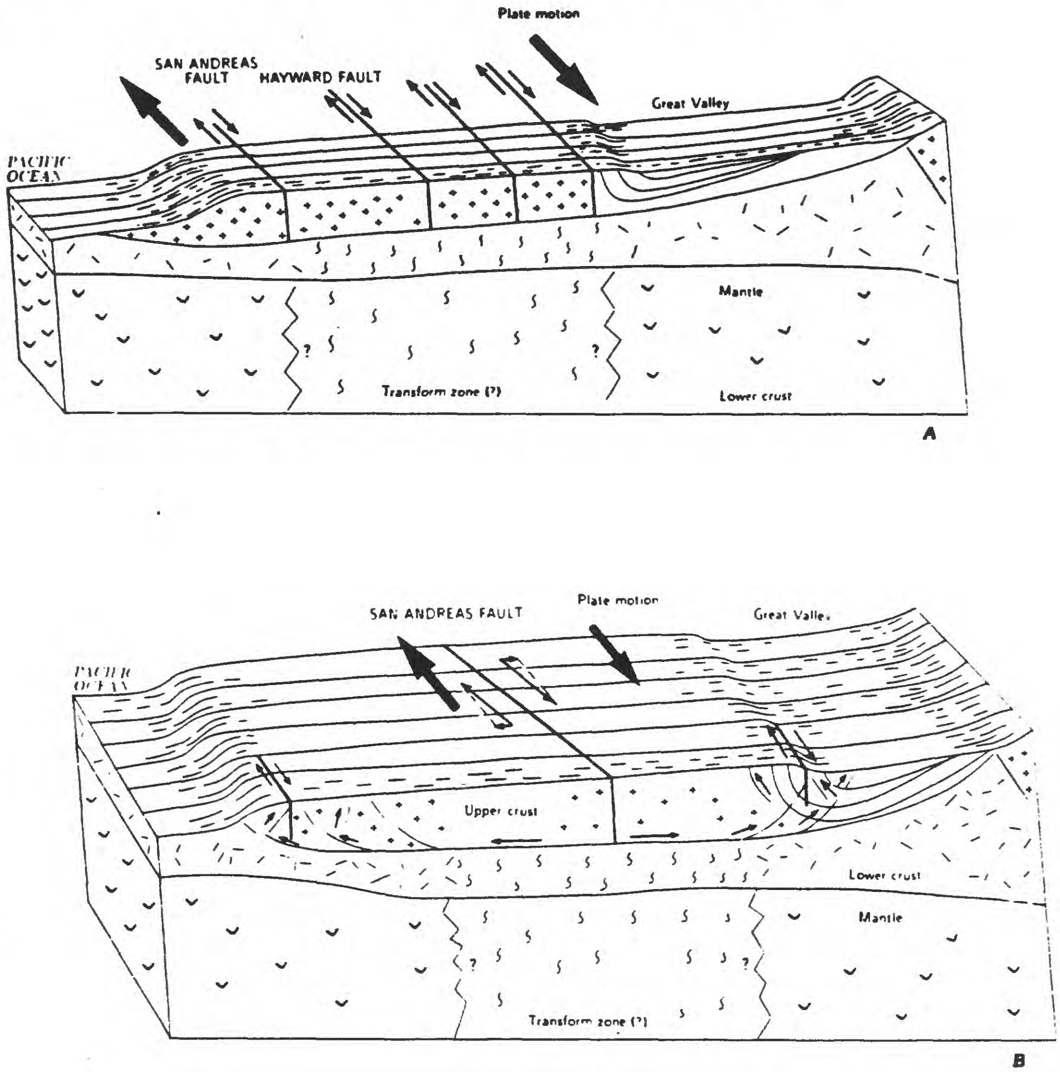


Figure 36. Schematic cross sections at latitudes of San Francisco (A) and Coalinga (B), illustrating the seismotectonic model proposed for the southern Coast Ranges. In both regions, a brittle upper crust (crosses) overlies a lower crust (short slashes) that lies on the mantle (checks). Beneath the central part of the ranges, ductility of the lower crust and upper mantle (vertical "squiggles") accommodates relative motion of the plates and provides for decoupling of the upper crust from the lower crust. In A, major (strike-slip) faults parallel the direction of relative plate motion, and almost all relative motion between the plates is accommodated by those faults. In B, slip along the misaligned San Andreas fault accommodates most motion between the plates but also widens the upper crust over the transform and drives the outer edges of the Coast Ranges crust out beyond the ductile part of the lower crust over the transform. Earthquakes occur on thrust and high-angle reverse faults along the edges of the ranges where the detachment zone beneath the center of the ranges encounters less ductile lower crust and passes up into the brittle upper crust.

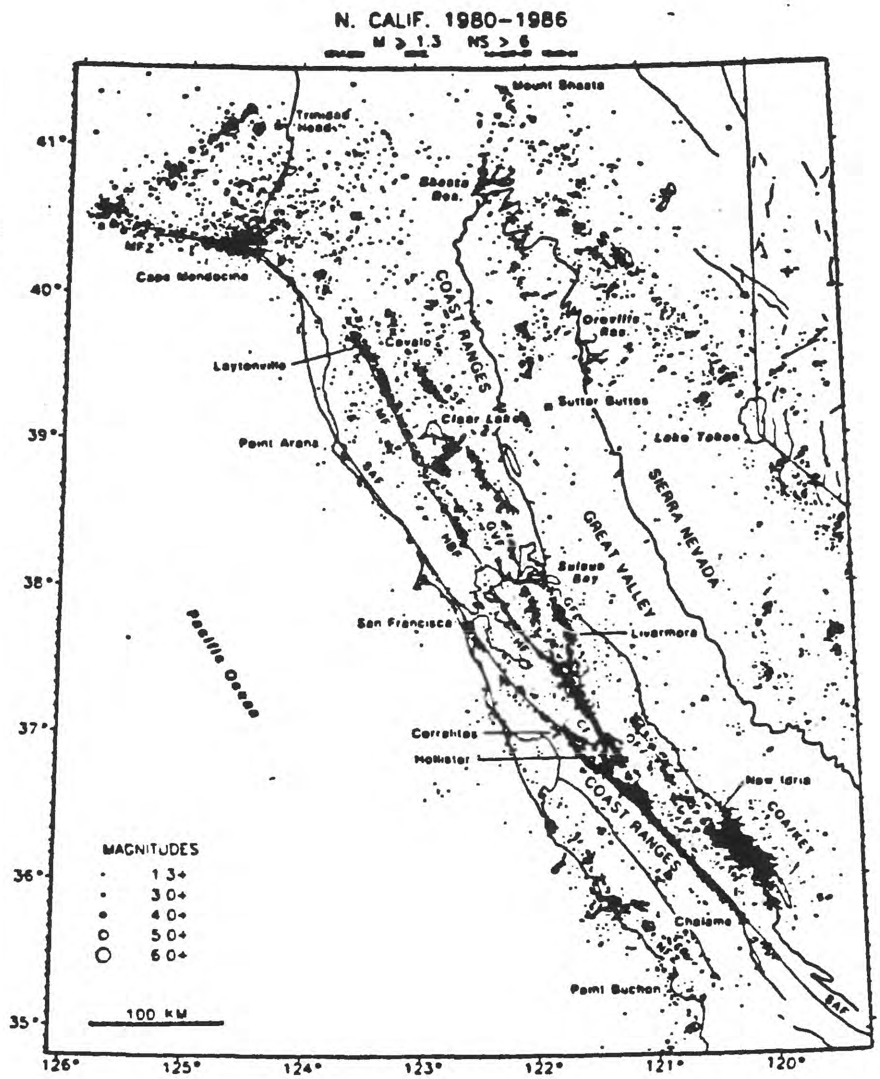


Figure 37. Northern California seismicity: 1980-1986. Symbol sizes are scaled according to magnitudes. Only events with magnitudes ≥ 1.3 and with 7 or more stations in the hypocentral solution were included in the plot. Abbreviations: SAF = San Andreas fault, CF = Calaveras fault, HF = Hayward fault, GF = Greenville fault, GVF = Green Valley fault, BSF = Bartlett Springs fault, HBF = Healdsburg fault, MF = Maacama fault, MFZ = Mendocino fracture zone, COA/KET = Coalinga/Kettleman aftershock region.

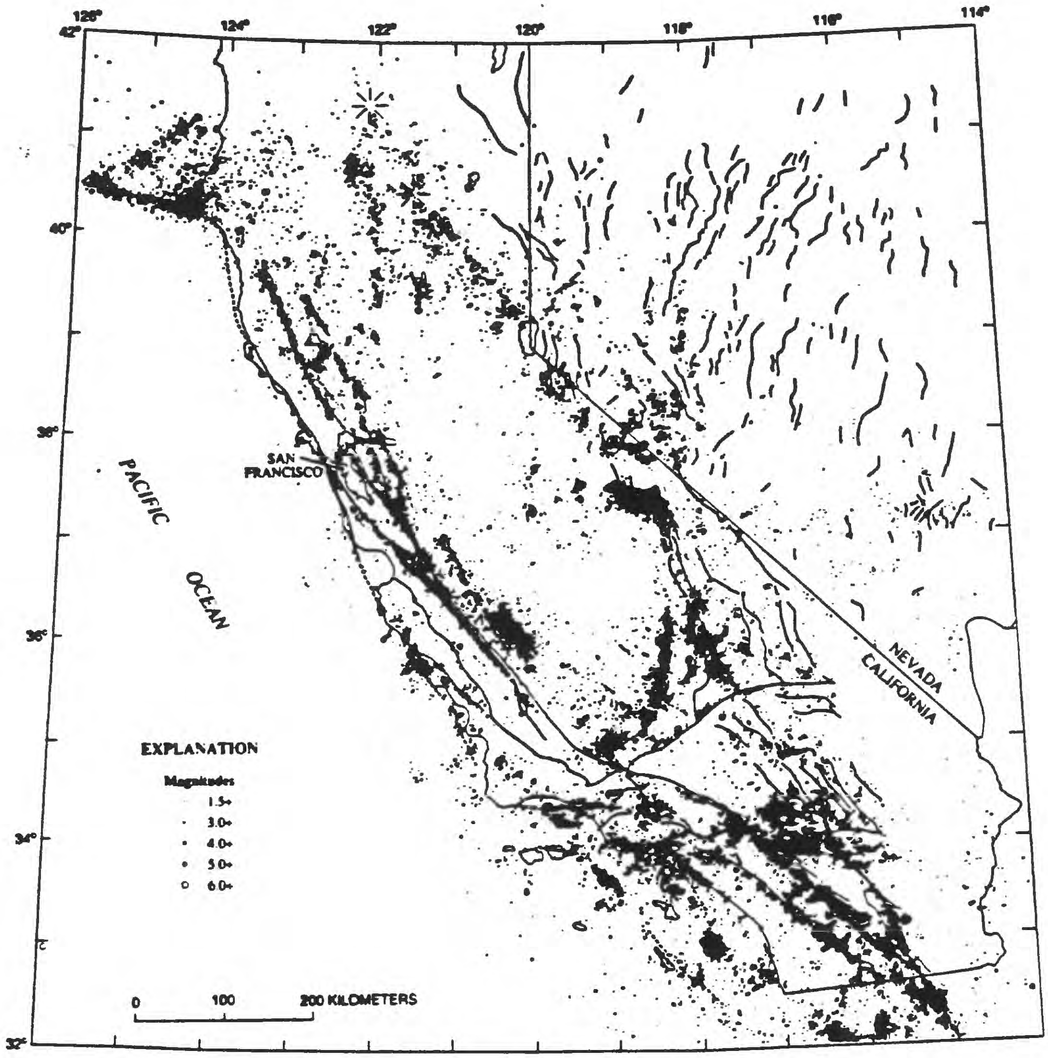


Figure 38. Locations of 64,000 M ≥ 1.5 earthquakes in California and western Nevada during 1980-1986 and mapped Holocene faults (dotted lines where concealed).

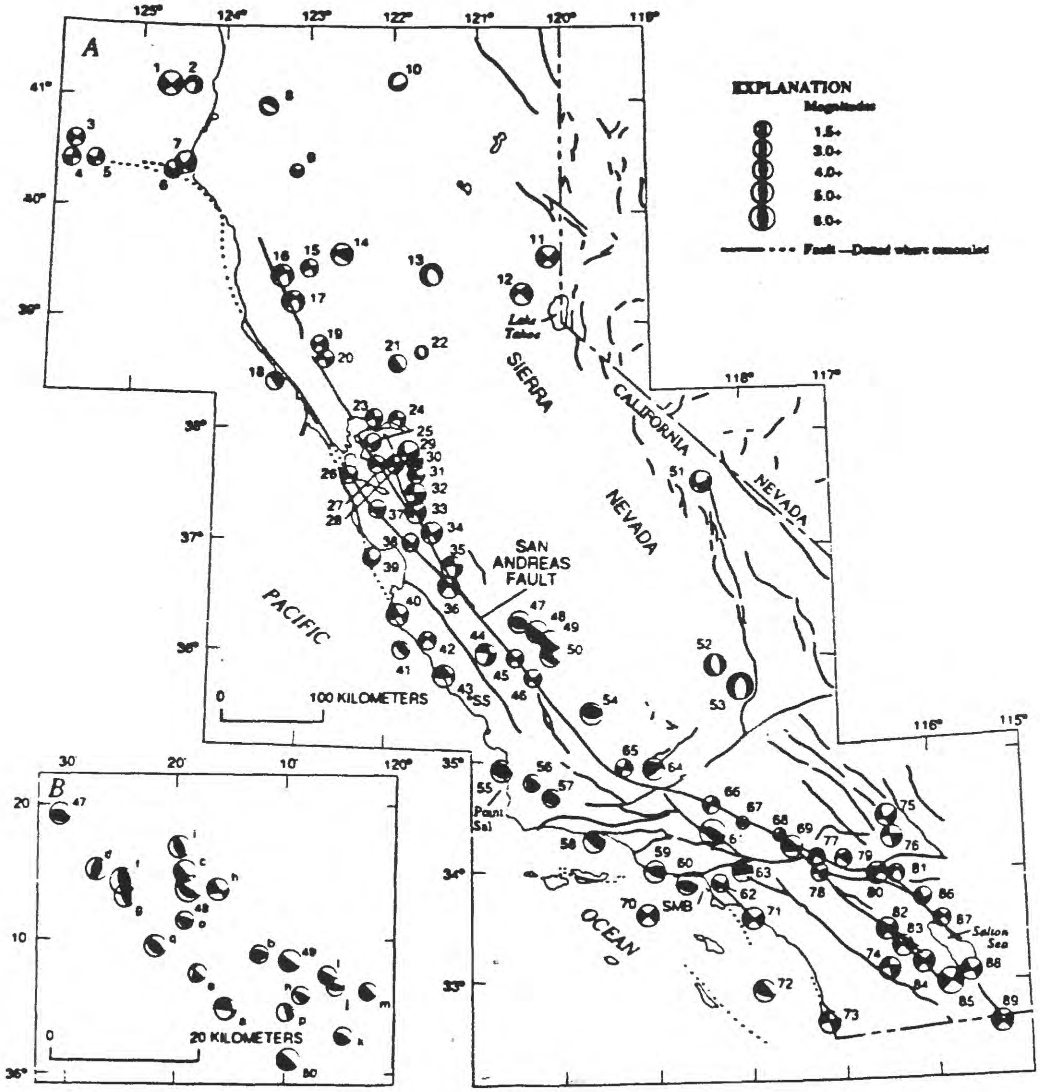


Figure 39. Focal mechanisms for larger earthquakes. A, California; B, Coalinga/Kettleman Hills region. Circle size increases with magnitude from 3.5 to 6.7. Abbreviations: SMB = Santa Monica Bay, SS = San Simeon.

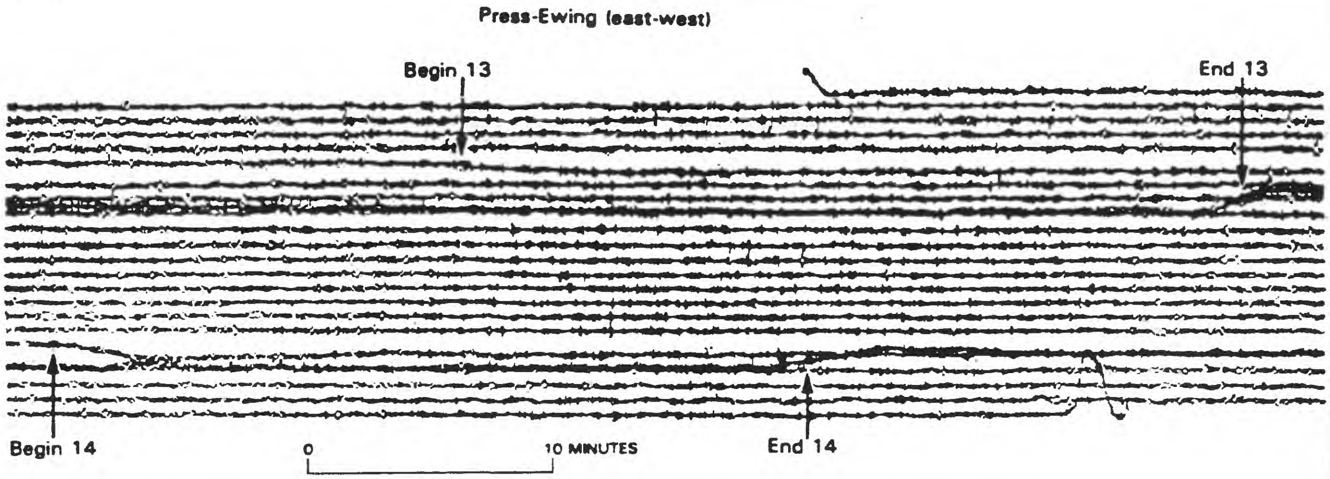


Figure 40. Seismogram from east-west Press-Ewing seismograph for December 15-16, 1959, showing shifts in the zero line associated with eruptive phases 13 and 14 (arrows).

SEISMIC NETWORK

Before the 1959 eruption a network of sensitive, telemetered, short-period seismographs had been installed around the summit of Kilauea and on the adjacent flank of Mauna Loa to facilitate study of the many earthquakes occurring regularly in that region. The telemetered network was augmented by optically recording seismographs with similar response characteristics that were deployed in a ring around the Island of Hawaii. The combined network provided the means for detecting and locating a variety of seismic

1959, to November 15, 1960, show the patterns of regional deformation of the summit area during the 1960 Kapoho eruption, the rapid deflation of the summit reservoir accompanying and following the Kapoho eruption, and the onset of rapid reinflation of the summit reservoir in the fall of 1960. The vectors depicting rate of tilting point outward away from centers of inflation and inward toward centers of deflation (fig. 48.3). The lengths of the vectors are scaled according to the rate of tilting. Note the large variation in scale factor from one interval to the next. These plots are shown together for easy comparison, but we shall return to consider them

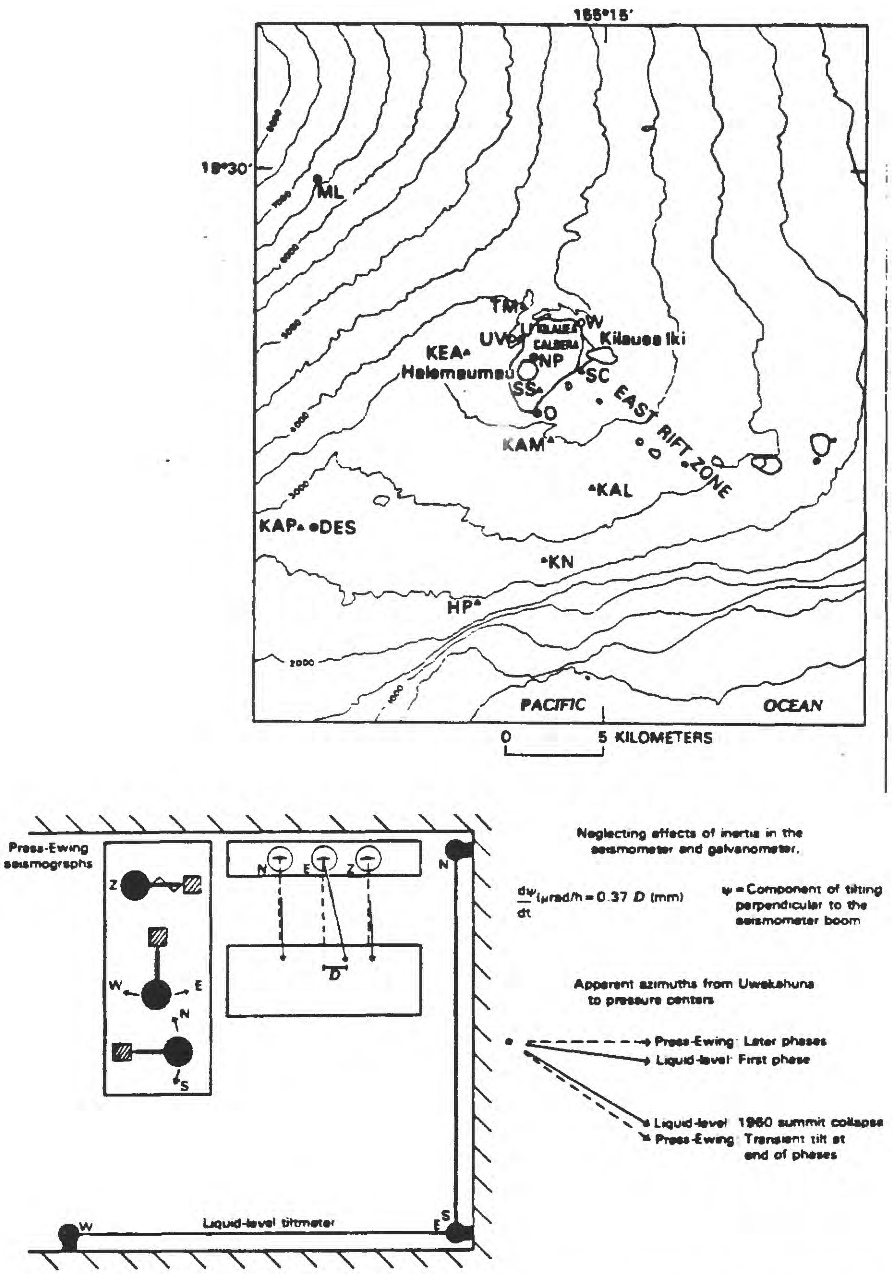


Figure 41. (Top) Summit of Kilauea volcano, showing tilt bases as triangles (TM, SC, SS, KEA, KAP, KAM, KAL, KN, HP), tiltmeter vaults as circles (UV and W), telemetered seismographs as dots (ML, DES, NP, O), and geographic localities mentioned in the text. Pit craters are indicated by hachures. Elevation contours in feet.

(Bottom) Layout of liquid-level tiltmeter and long period Press-Ewing seismographs in Uwekahuna vault. Press-Ewing seismographs are shown at left, and their galvanometers are shown at the top of the figure. Letters Z, N, and E denote parts of the instruments and the component of tilting they record: Z, vertical (no tilting); N, north-south (N-S); and E, east-west (E-W). D = deflection of seismogram trace from normal; $d\phi/dt$, rate of tilting.

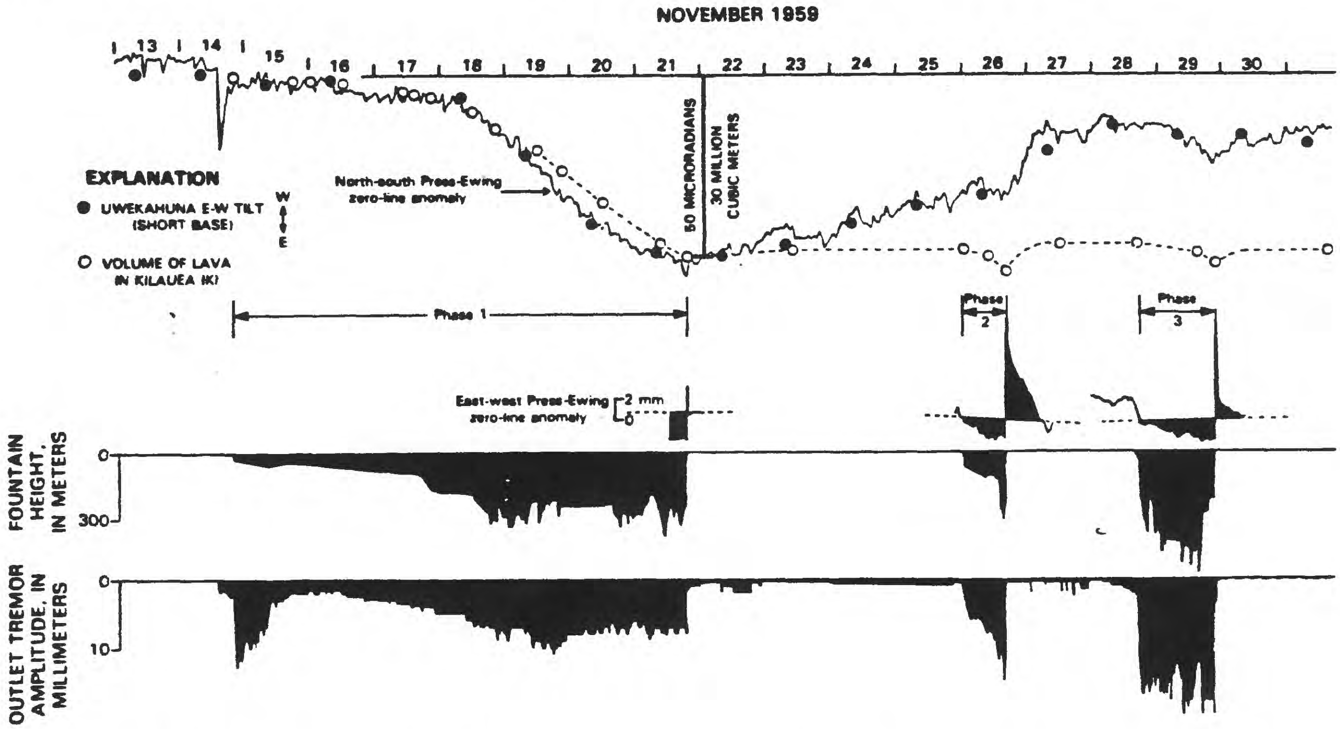


Figure 42. Eruption and deformation parameters during first three eruptive phases of the 1959 eruption. Volume of lava in Kilauea Iki crater, east-west component of Uwekahuna vault liquid-level tiltmeter, and north-south Press-Ewing zero-line anomaly are superposed in upper half of figure; fountain height and tremor amplitude at Outlet station are plotted in the lower half. Duration of individual phases is indicated.

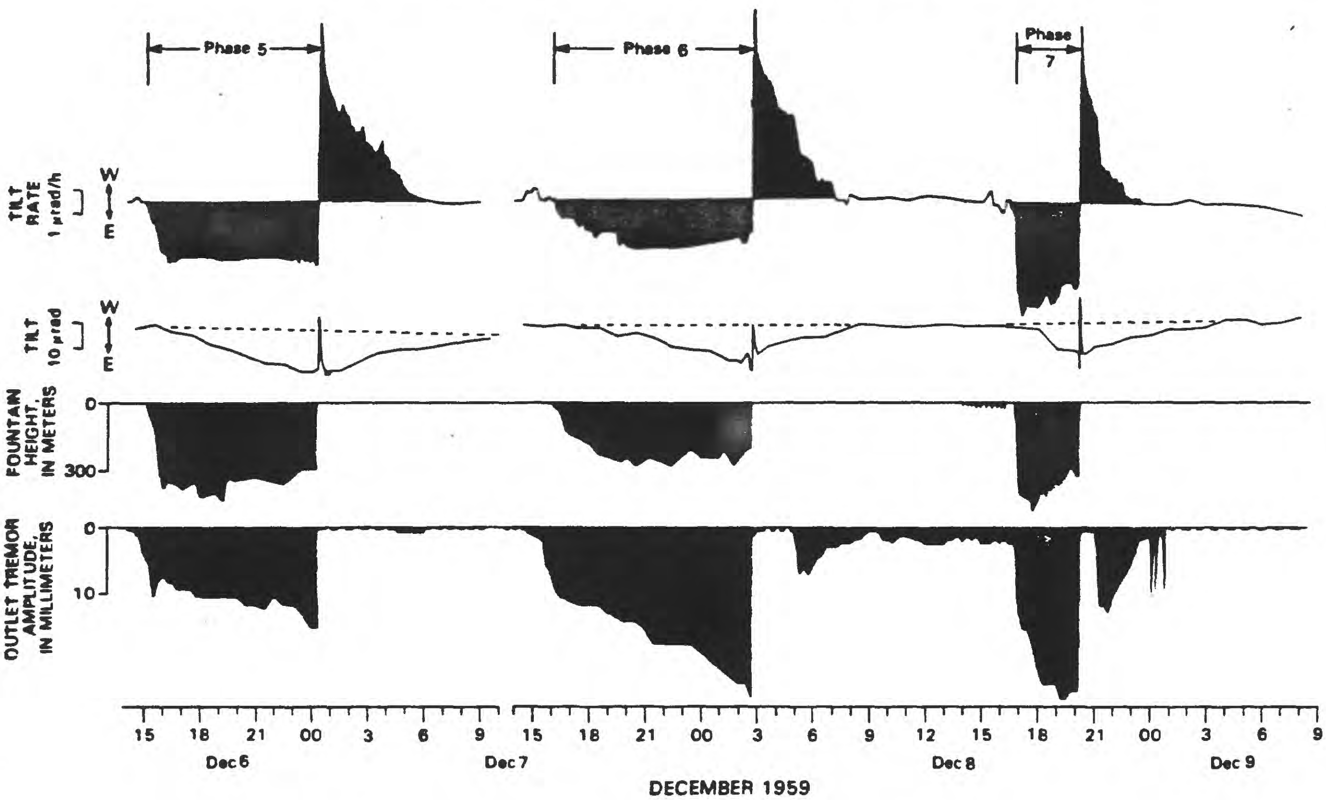


Figure 43. East-west rate of tilting, east-west cumulative tilt, fountain height, and Outlet tremor amplitude for phases 5-7.

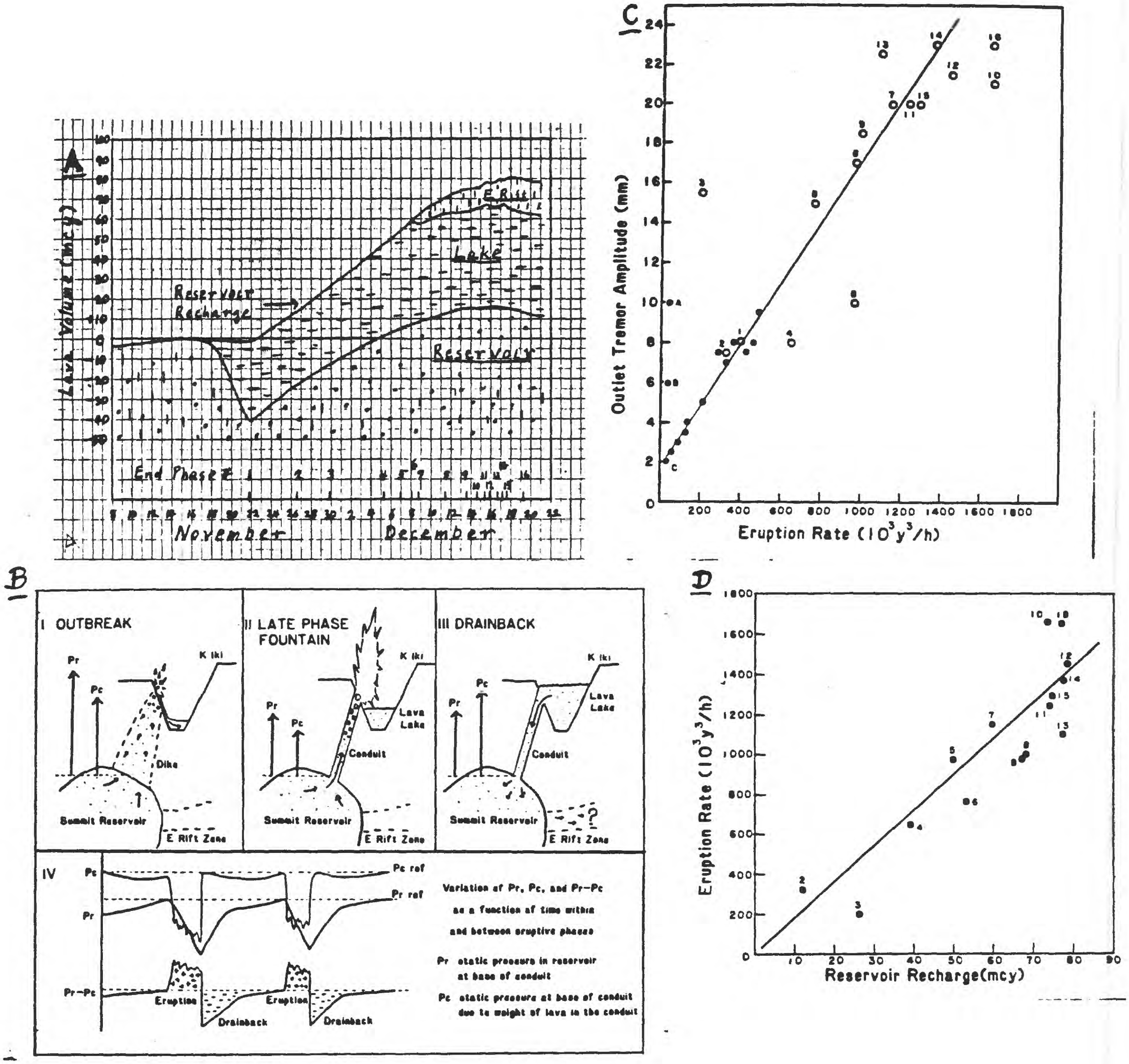


Figure 44. A. Distribution of new lava rising from depth among the summit reservoir, lava lake, and east rift zone (escaped from system?) and budget of lava moving between the summit reservoir, lava lake, and east rift zone during the course of the Kilauea Iki eruption.

B. Diagram illustrating the operation of the "vesiculation pump" that drove repeated eruption into Kilauea Iki lava lake.

C. Outlet tremor amplitude versus eruption rate into Kilauea Iki crater. Solid dots represent phase 1: points labeled A and B are from the early "outbreak" part of phase 1; point C and unlabeled solid dots represent periodic measurement of phase 1 after fountaining was confined to the principal vent, which increased its output steadily with time. Open circles represent subsequent phases: the single labeled point represents the average rate of outpouring sustained during the height of the phase.

D. Average eruption rate versus reservoir recharge (from Fig. 44A) for phases 2 through 16.

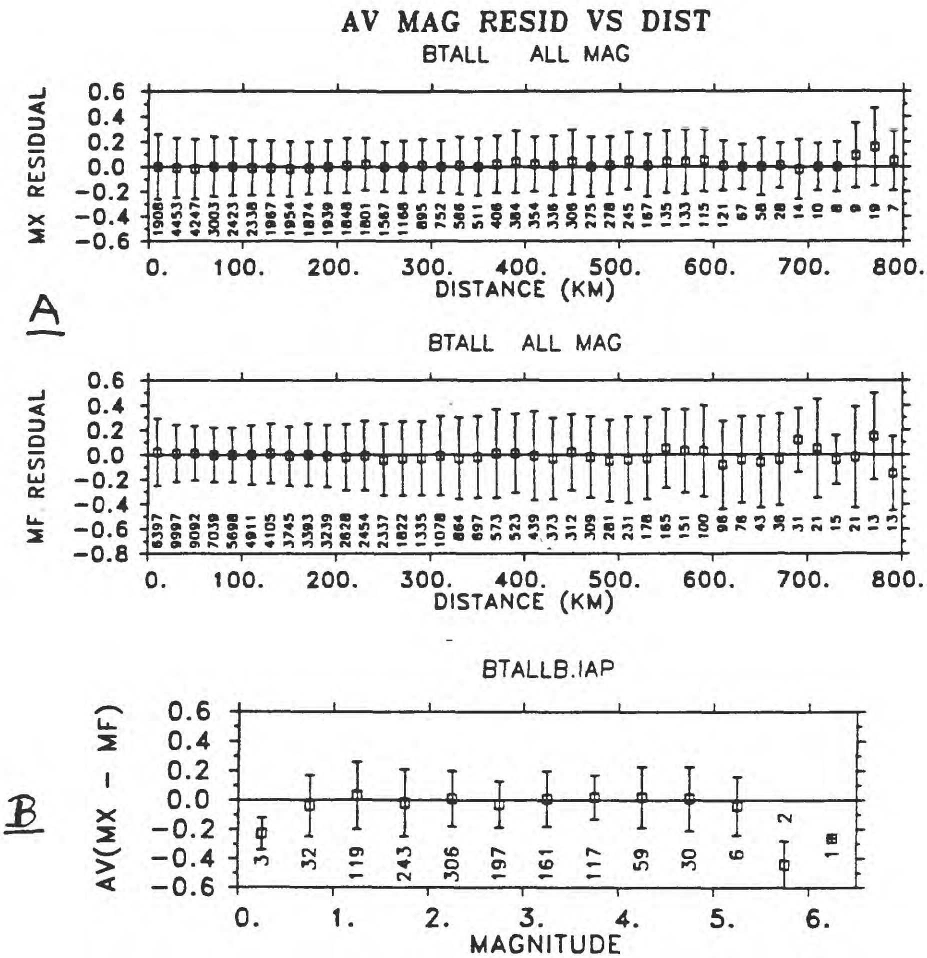


Figure 45. A. Average magnitude residuals and standard deviations of the residuals versus distance. (Upper panel): MX for the CUSP data set; (Lower panel): MF for the CUSP data set. The number of observations in each distance interval is indicated below the plotted point. The error bars indicate ±1 standard deviation.

B. Average MX-MF differences and standard deviations of the magnitude differences versus magnitude (MX/2 + MF/2) for the CUSP data set. The number of observations in each interval is indicated below the plotted point. The error bars indicate ±1 standard deviation.

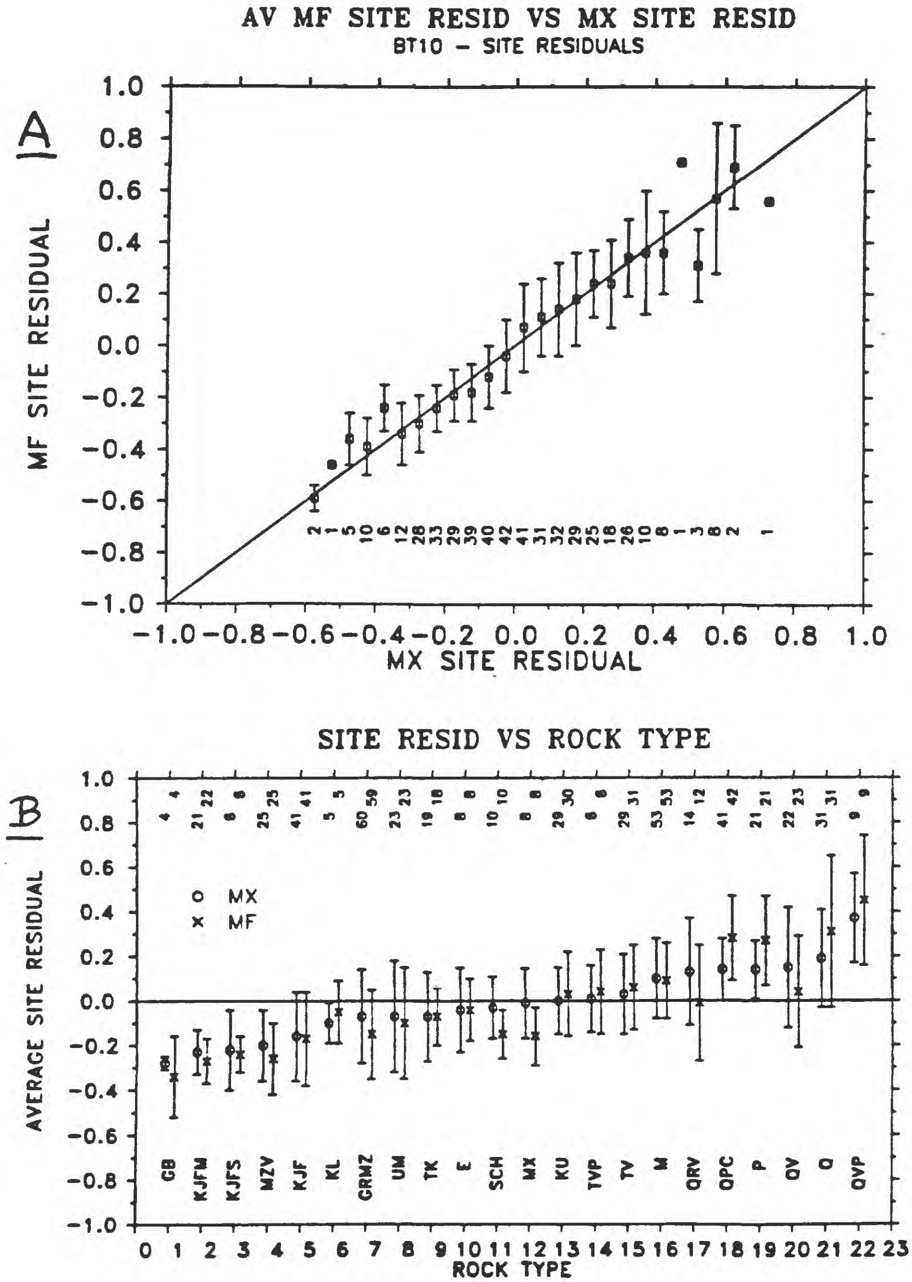


Figure 46. A. Average MF site residuals versus MX site residuals. Averages of MF residuals over 0.05 unit interval of MX site residual are plotted at the centers of the MX site residual intervals. The number of MF observations in each interval is indicated below the plotted point. Error bars indicate ±1 standard deviation.

B. Site residuals versus rock type. Average MX and MF site residuals are plotted as a function of rock type category, indicated below the plotted points. Rock types are arranged in order of increasing MX site residual. Numbers of observations for each rock type are indicated above the plotted points (MX, below and MF, above). Error bars indicate ±1 standard deviation.

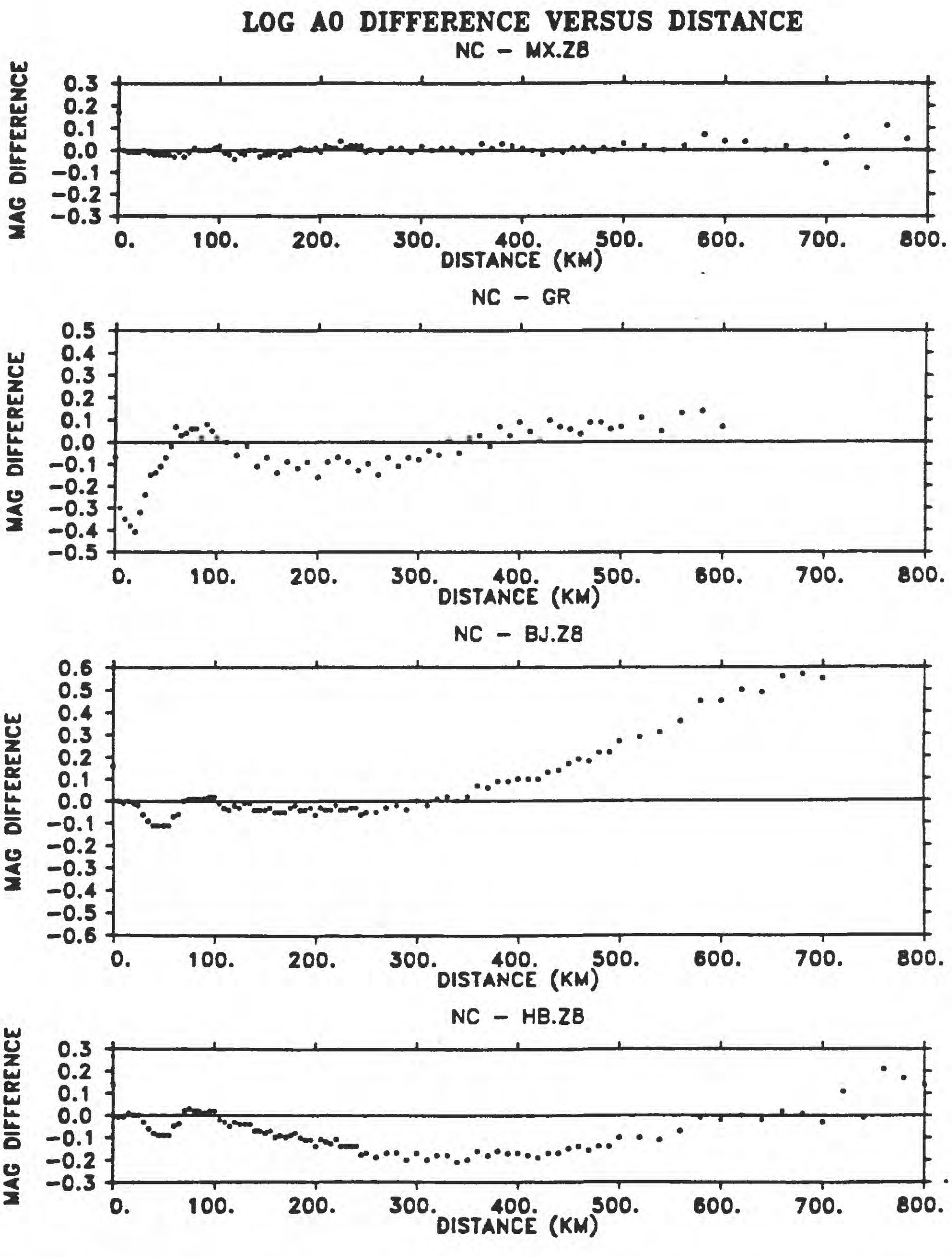


Figure 47. Comparison of the empirical northern California $\log A_0$ versus distance table (derived from the CUSP data set) with MX, BJ, and HB $\log A_0$ equations and with the Gutenberg-Richter $\log A_0$ table.

(Top) NC $\log A_0$ - MX $\log A_0$; (second from top) NC $\log A_0$ - GR $\log A_0$ table; (third from top) NC $\log A_0$ - BJ $\log A_0$; (bottom) NC $\log A_0$ - HB $\log A_0$. The $\log A_0$ equations were evaluated for a focal depth of 8 km.

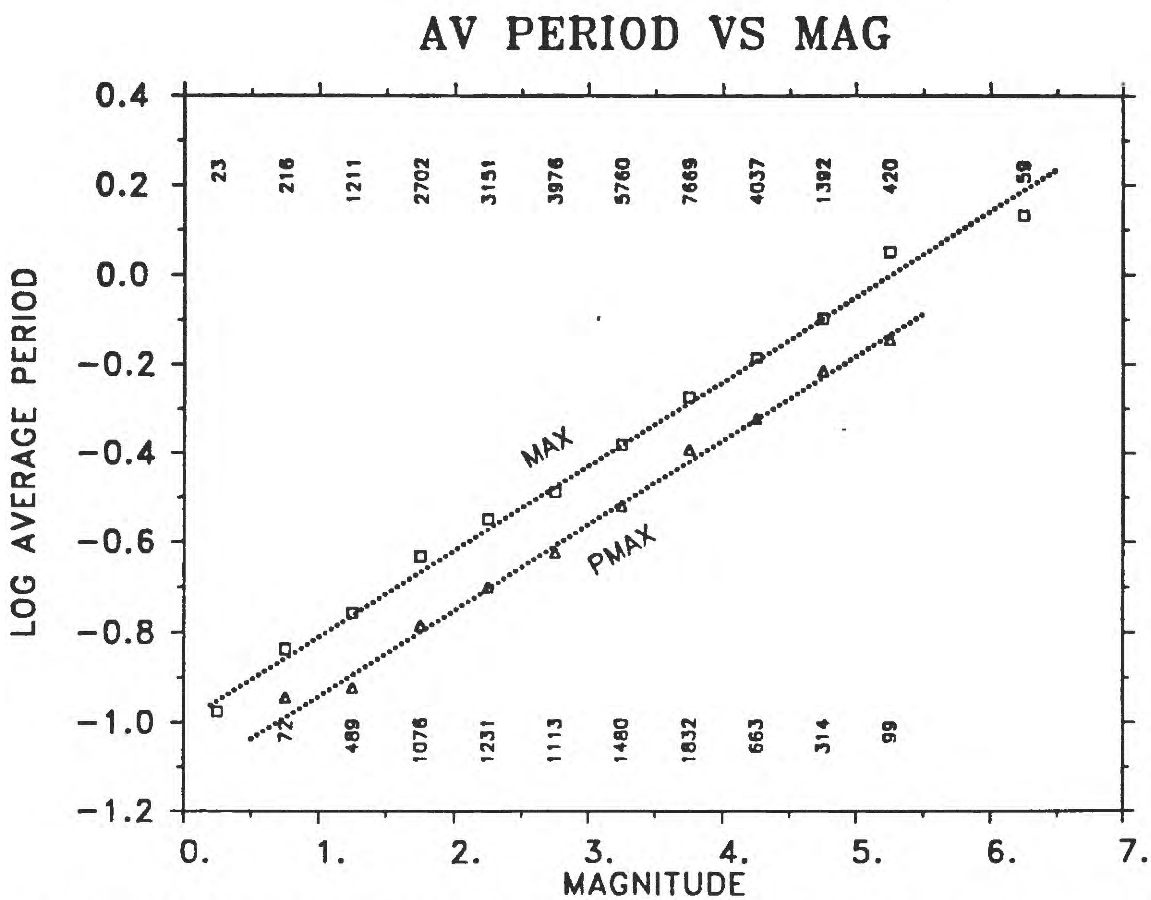
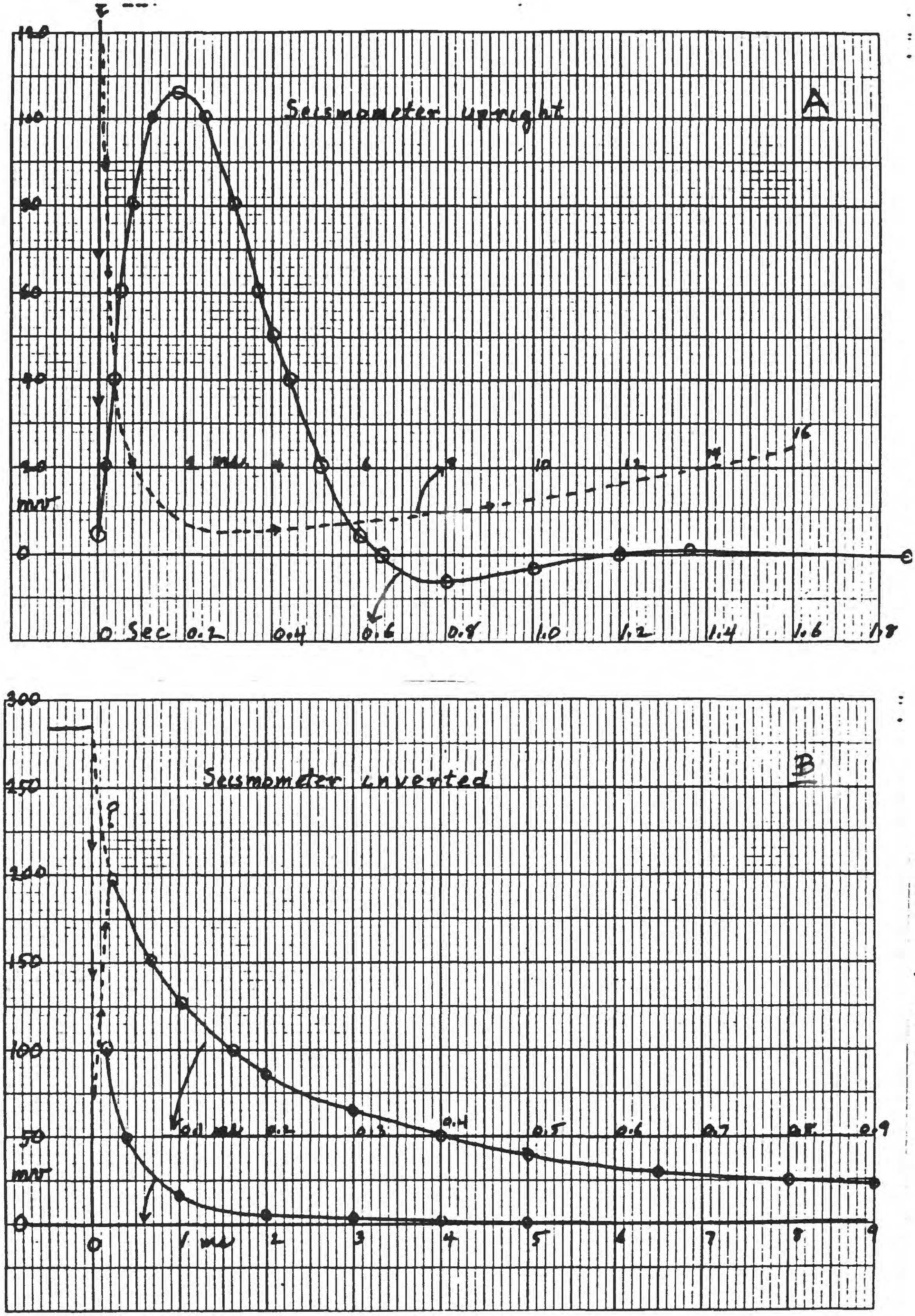


Figure 48. Log average period versus magnitude for the maximum wavelets used to compute MX. For the curve labeled MAX, the maximum wavelet occurred in S or later. For the curve labeled PMAX, the maximum wavelet occurred in P. Averages of log(period) were taken over 0.5 magnitude intervals and plotted at the centers of the intervals.

PMAX = -1.13 + 0.19 M

MAX = -1.00 + 0.19 M



$$27)\; E_s(t) = \frac{S}{R+S}\,\frac{G^2 i_0 e^{-\beta\Omega_0 t}}{M\sqrt{1-\beta^2}}\left[\frac{1}{\Omega_0}\sin\sqrt{1-\beta^2}\,\Omega_0 t - \frac{L}{R+S}\sin\left(\sqrt{1-\beta^2}\,\Omega_0 t - \tan^{-1}\frac{\sqrt{1-\beta^2}}{\beta}\right)\right]$$

Figure 49. Response of L4C seismometer to a current release test.

A. Seismometer upright to produce normal release test - note two different time scales.

B. Seismometer inverted to lock mass so that it does not move in response to release test - note two time scales.

Equation 27: Complete response of the L4C to a release test. The first term in the brackets represents the normal release test, and the second term represents an adventitious tapping test resulting from the inductance in the seismometer coil.

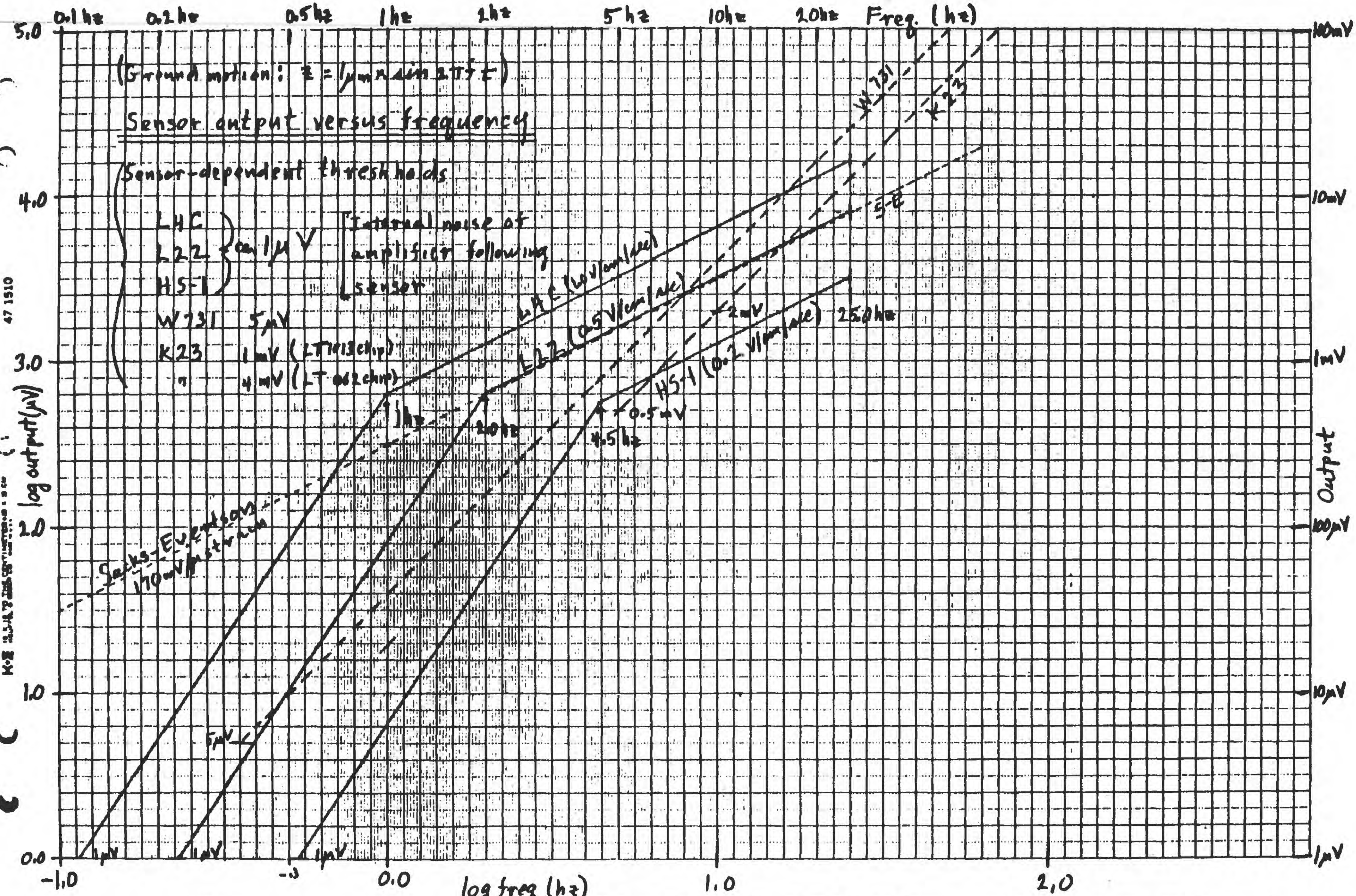


Figure 50. Output voltage of various sensors (L4C, L22, HS1, W731, K23, and dilatometer) as a function of frequency for a ground motion amplitude (zero to peak) of 1 micron (10^{-6} meter).

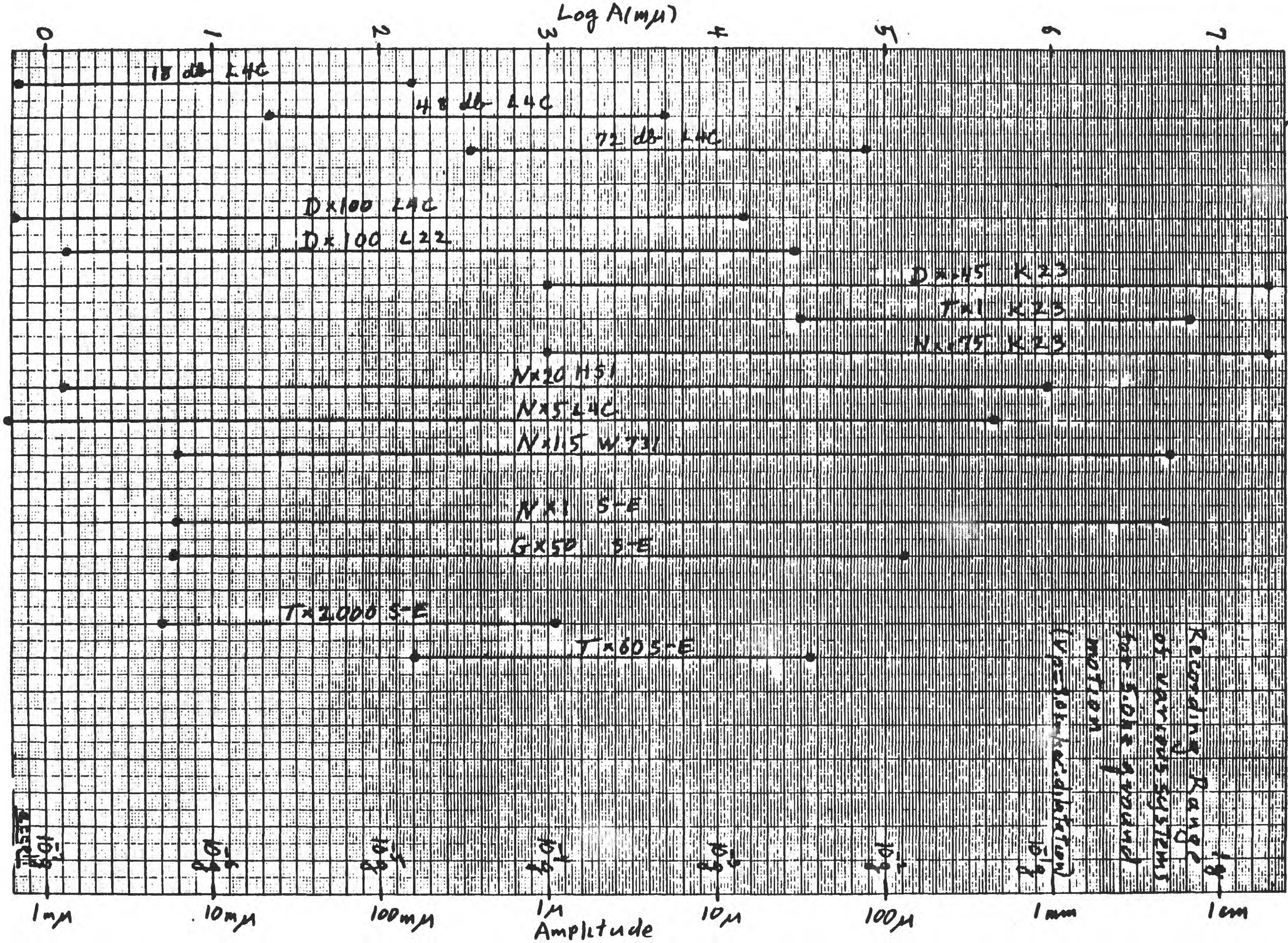


Figure 51. Recording range of various system configurations, expressed in terms of ground motion amplitude and acceleration, for 5 hz sinusoidal ground motion.