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DEPARTMENT OF THE INTERIOR
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PROCEEDINGS OF THE NATIONAL EARTHQUAKE PREDICTION EVALUATION COUNCIL

JUNE 6 - 7, 1988
Reston, Virginia

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JUNE 6 - 7, 1988
Reston, Virginia

by
Randall G. Updike

Open-File Report 89-144

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

1989

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PREFACE

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

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

The USGS has published the proceedings of previous NEPEC meetings as open-file reports; these reports are available from the USGS Open-File Distribution Center in Denver, Colorado.

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NATIONAL EARTHQUAKE PREDICTION EVALUATION COUNCIL

PROCEEDINGS OF THE MEETING OF JUNE 6-7, 1988

Reston, Virginia

Council Members Present

Dr. Lynn Sykes, Chairman, Lamont-Doherty Geological Observatory
Dr. John Filson, Vice-chairman, U.S. Geological Survey (USGS)
Dr. Keiiti Aki, University of Southern California
Dr. John Davies, University of Alaska Geophysical Institute
Dr. James Davis, California Division of Mines and Geology
Dr. William Ellsworth, USGS
Dr. Hiroo Kanamori, California Institute of Technology
Dr. Thomas McEvilly, University of California, Berkeley
Dr. Wayne Thatcher, USGS
Dr. Brian Tucker, California Division of Mines and Geology
Dr. Robert Wallace, USGS
Dr. Robert Wesson, USGS
Dr. Randall Updike, Executive Secretary, USGS

Invited Participants

Dr. V.I. Keilis-Borok, Institute of Physics of the Earth
Academy of Sciences of the USSR, Moscow

Dr. Leon Knopoff, University of California, Los Angeles

Dr. J. Bernard Minster, University of California, San Diego

INTRODUCTION

Several technical publications, authored chiefly by earthquake prediction researchers in the Soviet Union (e.g., Gelfand and others, 1976; Keilis-Borok and others, 1980a, 1980b; Gabrielov and others, 1986; Keilis-Borok and others, unpub.), have led to the prediction of a major earthquake in California or western Nevada prior to the end of 1991. The primary focus of the June 6-7, 1988, meeting of the National Earthquake Prediction Evaluation Council (NEPEC) summarized in this report was to review the earthquake prediction research methodology and conclusions by this group of scientists. At its February 1-2, 1988, meeting, NEPEC agreed that Dr. Keilis-Borok should be invited to the United States to give a summary presentation of his work and that of his colleagues to NEPEC (Updike, 1988). In advance of that meeting, NEPEC requested independent reviews of the Soviet research (i.e., Gabrielov and others, 1986) by Richard Haubrich and J. Bernard Minster. After careful discussion and review with Dr. Keilis-Borok and his U.S. research counterpart, Dr. Leon Knopoff, NEPEC would present a summary evaluation and general recommendations to the Director of the U.S. Geological Survey.

June 6, 1988

MORNING SESSION

L.SYKES called the NEPEC meeting to order and introduced the members and invited participants. He outlined the overall mission of the National Earthquake Prediction Evaluation Council (NEPEC) which is to serve as an advisory panel to the Director of the U.S. Geological Survey (USGS) on issues relating to earthquake prediction. Soviet Academician Dr. Volodya Keilis-Borok was introduced and invited to present a summary of his earthquake prediction research.

V.KEILIS-BOROK began his presentation by outlining the topics he would discuss, including a general statement, previous experience, general approach, tests of the statement, what generated the California-Nevada Time of Increased Probability (TIP), software involved, and what should be done next.

The original overall objective of the research was to develop the capability of long-term prediction of strong earthquakes in the USSR as well as other countries. "Long-term" refers to the prediction of an event that may occur in 3-5 years; the areas under consideration are on the scale of hundreds of kilometers in linear dimension. The algorithms of prediction under consideration that are used to diagnose the TIP utilize a series of traits related to the "earthquake flow," including:

- The level of seismic activity
- Temporal variation (fluctuation) of activity
- Space-time clustering
- Concentration in space
- Long-range interaction

The routine catalogues of earthquakes provide sufficient data to monitor these traits in most regions of the world. Because of the complexity of the calculations involved with multiple variables, only a limited number of the most obvious traits are currently used.

Two algorithms are used for the diagnosis of TIP's, both utilizing the traits listed above. The first, termed the CN algorithm, was used to predict earthquakes with magnitudes of 6.4 or greater in California and Nevada. The second algorithm, termed M8, was used to predict earthquakes equal to or greater than about magnitude 8 on a worldwide basis. Both algorithms are intended to be applicable within regions, focused on different seismic activity and different maximal magnitudes.

Because an adequate theoretical model for the earthquake process does not exist, a series of phenomenological analytical tests has been applied retrospectively to the seismicity patterns that preceded strong historic earthquakes. The algorithms were formulated and the numerical parameters adjusted in order to fit the existing data, i.e., to minimize the number of retrospective errors. To validate the methodology developed through data fitting, it was necessary to test the algorithms using independent new data. This is difficult because the necessary strong earthquakes rarely occur, and catalogues with sufficient detail for the algorithms have only been acquired for about 25 years. Establishment of a broader data base has required the utilization of events from a variety of different tectonic settings; this requires some readjustments but seems globally valid. Using the M8 algorithm, 21 historic strong earthquakes on a worldwide basis have been considered; 19 of these earthquakes, including the 1952 Kern County and 1980 Mendocino earthquakes, have been found to be preceded by exactly the same set of normalized patterns. In diagnosing 30 patterns, 22 were followed by a strong earthquake. Adding cases where a few parameters had to be slightly modified, 32 out of 36 earthquakes were preceded by the established patterns and 39 out of 59 patterns were followed by strong earthquakes. Based upon this algorithm and these tests, a magnitude 7.5 or greater earthquake is expected within 5 years from the date of diagnosis and in an area about 900 km in diameter in California. New data may extend the time or may shift the area of the prediction.

Using CN, 40 out of 51 strong earthquakes were preceded by the same patterns, including 9 out of 9 in southern California and adjacent Nevada. Twenty-five out of 35 such patterns worldwide, including 5 out of 6 in this region, were followed by a strong earthquake. Using the set of patterns diagnosed by the algorithm CN and using the California Institute of Technology (Caltech) data domain, a similar prediction was determined for southern California and adjacent Nevada for magnitude 6.4 or greater.

Questions directed to Dr. Keilis-Borok

L. **SYKES** asked whether the alarm has been cancelled for southern California, or does Keilis-Borok not have enough good magnitudes to determine whether the TIP is extended by the CN algorithm.

V. **KEILIS-BOROK** responded that he would have to look at the printouts to answer the question.

W. **THATCHER** questioned whether the answer then is that the TIP continues. (no reply)

T.MCEVILLY wanted to know how the M8 TIP length is set and where does it come from?

V.KEILIS-BOROK responded that a regional algorithm was developed by considering the world for magnitude 8 for a 5-year interval, and from then on, they data fitted for 5-year intervals.

T.MCEVILLY followed, data fitted for 5 years for the world?

V.KEILIS-BOROK said no, 5 years for the regional data.

T.MCEVILLY asked if the CN is based on 6.5 using the Caltech data catalogue.

V.KEILIS-BOROK responded yes, and the Berkeley Catalogue, at 6.4 to include San Fernando and Borego Mountain that were both reported at magnitude 6.4.

W.THATCHER stated that when he looks for empirical regularities in some observed phenomenon, like earthquakes, the goal is to explain a physical process; from the presentation we get the impression that looking for such empirical regularities may be misguided. What is your feeling?

V.KEILIS-BOROK said quite to the contrary, one must use it to understand physical processes but one must use the proper methods. We are looking for abnormal patterns; that is, we believe a collective behavior prior to large earthquakes exists. This is reminiscent of deterministic chaos. The traits we have chosen provide an adequate integrated description of the process. We also believe that there is a global similarity of "earthquake flow" so that the method is applicable worldwide.

L.KNOPOFF wanted to add to these views. To be successful, we must recognize that the deterministic chaos approach using non-linear equations still requires a very simple approach in terms of the number of equations; only a few equations can be handled. One starts with simple systems and finds clustering in both the CN and M8 methods; the fact that clustering is observed is a statement of hope that earthquake prediction is possible. If the clustering does not occur and this is a noise-driven analysis, then we might as well give up on earthquake prediction.

K.AKI raised the issue of time scale problems and the direction that the U.S. program has been pursuing, i.e., paleoseismology.

T.McEVILLY was still unclear on how the time windows are determined for the various traits; are they data fitted or are they somehow determined a priori?

V.KEILIS-BOROK stated that they are data fitted by the completeness of the data catalogue.

T.McEVILLY followed that these data-fitted time windows must then be short, 1, 2, or 3 years....or are some longer?

V.KEILIS-BOROK said no, they are the same for all catalogues. We have three time windows, 1-, 3-, and 6-year time windows depending on the trait. Some of the traits demonstrate temporal changes, others represent the level of activity.

L.KNOPOFF added that if one arbitrarily chooses a certain trait as being an indicator, the procedure has the capability of discriminating that hypothesis as being irrelevant and rejecting it. You don't have to a priori identify the traits to be used. The traits used are the only ones that survive the procedure of discrimination.

W.THATCHER indicated that it can't be that straightforward; if you state that six of these traits must be exceeded at some threshold level, then what can you say about any individual one? An objective judgement must be inserted about what traits you collect and what criteria you use for accepting or rejecting them as part of your algorithm.

V.KEILIS-BOROK responded that you cannot over-formalize this; you are making selections more or less at the minimum level necessary for the analysis, and allowing the process to discriminate the traits that are applicable.

T.McEVILLY felt that an important question is that the process you have characterized is a very deterministic process with a very few traits, i.e., a very powerful predictor that depends on the selection of windows. How does the window length influence the prediction determination; would 100 years, for example, work just as well as 5 years? He was intrigued that they are having success with the selected time windows; will other window lengths work just as well?

V.KEILIS-BOROK indicated that this is observational work that depends on the length of the data catalogues, which cannot be extended further than a 5-year interval.

W.THATCHER observed that there is a difference between the approach that fluid dynamicists and meteorologists are using for weather predictions and the earthquake prediction method being suggested here, namely that meteorologists use 4 or 5 linear relationships that interact in a complex manner to produce the non-linear chaotic behavior of the atmosphere. In your model a set of empirically derived non-linear relationships are being applied to a poorly understood chaotic system.

L.KNOPOFF emphasized that meteorologists not only have the constitutive laws to apply but also have an enormous abundance of data to apply. In earthquake prediction we not only don't have the constitutive laws, we lack the fundamental data.

W.THATCHER reiterated that his concern focuses on the traits. It would help to understand what the classes are, why you chose traits you did, how you narrowed them down to the ones you actually used, and what empirical basis was used to make your selection in the first place, e.g., clustering in space, clustering in time.

V.KEILIS-BOROK responded that the clustering of aftershocks is a simple example; other types of clustering must be examined such as swarms. Activation was an intuitive expectation. Variation is from experience. Different non-linear systems do not seem to behave in very dissimilar ways. He wouldn't be surprised if some other set of traits doesn't work better.

B.MINSTER raised the point, going back to the comparison with meteorology, that if you have a large data set you can try a number of models, but that if the population of study is less than 10 earthquakes, your conclusions may not necessarily be meaningful because of the limited data set.

V.KEILIS-BOROK agreed that only 10 earthquakes may not be enough, but with 20 or more you might be surprised. You can check it afterwards; this is a preliminary procedure which can be checked later.

How stable is the procedure? This is a difficult question because of data fitting. A diagnosis of the procedure using different tests was made, and the procedure withstands the variations applied including completeness of the catalogue used, definition of aftershocks in terms of different time and space windows, and the shape and shift of areas considered.

Where, specifically, are the earthquakes that trigger the TIP's? It is difficult to say, but we are considering strike-slip earthquakes in the Coast Ranges and normal fault earthquakes in the Sierra Nevada together. If we eliminate the Coast Ranges, we still have the TIP; if we eliminate the Sierra Nevada, the TIP disappears. It does not mean the earthquake will occur in the Sierra Nevada; it simply asks the question as to where the troublemakers are, and the Coast Ranges are not the only answer. (Here the map of California/Nevada showing the TIP centers was introduced.)

L.**SYKES** asked what is the period of time on which the map is based?

V.**KEILIS-BOROK** responded that they used the data from 1962 to 1986 the first 12 years to set up the algorithm and the period from 1975 to 1986 to calculate the TIP.

T.**McEVILLY** asked what success this procedure has produced for the region on the map, for magnitude 6.4 or greater.

V.**KEILIS-BOROK** corrected that it is for 7.5, and produced one alarm and no false alarms, i.e., one TIP, from 1962 to 1986.

R.**WALLACE** asked if there is a probability gain over random probability, and could he state any probability for the 7.5 during this TIP?

V.**KEILIS-BOROK** stated yes, there is a probability gain but we can not evaluate it directly. For 5 years, there certainly is an increased probability, but if you ask for a number you are vulnerable.

K.**AKI** emphasized that the Working Group report assigns a probability of a major earthquake in southern California of 60 percent in the next 30 years, and it is critical to know how the prediction here under consideration affects that probability statement. Even a range of probabilities would be helpful, and in fact, is needed to make the prediction meaningful.

V.**KEILIS-BOROK** responded that the nature of the algorithm used does not allow for calculations of a probability. He would tend to believe that the probability is high (above 50 percent), but there is no way of offering an actual probability. All he can say is that the 50-50 chance of a major earthquake in southern California should be absolutely greater; more than 50 percent probability in less than 30 years, although he is not making a prediction for 30 years.

J.**FILSON** asked what specific earthquakes have occurred to extend the TIP?

V.**KEILIS-BOROK** identified two groups of earthquakes by coordinates (confusion among NEPEC members on what earthquakes were being referenced) which he says are both necessary to extend the TIP.

J.FILSON reiterated the question that, if they were both removed, would the TIP disappear?

V.KEILIS-BOROK replied that he didn't know.

June 6, 1988

AFTERNOON SESSION

L.SYKES pursued the question of how the Mammoth sequence of earthquakes (Long Valley) was treated in the analysis. Was it treated as a single large earthquake with a number of unusually large aftershocks or as a swarm? This affects at least two of the traits used in the technique.

V.KEILIS-BOROK said he would have to consult his catalogue, no answer was given and he deferred to later.

L.SYKES asked how predictions of this type dealt with in the Soviet Union in terms of civil defense, earthquake preparedness, etc.

V.KEILIS-BOROK said that review begins with the research group, followed by general, more qualitative, discussion among researchers, and finally presented to and evaluated by the Soviet Academy of Sciences. The Academy would then make recommendations to the civil authorities as to what actions should be taken. The civil authorities would probably welcome having 5 years in which to prepare. He also stated that, at the present time, review committees in the Soviet Union do not consider a forecast with a very large uncertainty in the predicted location of a large earthquake (as is the case with the TIP approach) to be sufficiently accurate to be designated as an earthquake prediction.

L.SYKES AND V.KEILIS-BOROK had a dialogue regarding other groups of Soviet researchers working in earthquake prediction. It was agreed that both the United States and the Soviet Union could benefit from the interaction of different approaches underway in the respective countries. Other Soviet work was more along the lines of U.S. work in terms of study on specific faults and looking at the seismic gap approach (as in the Kamchatka region). Keilis-Borok gave the impression that these efforts were difficult to assess at the present time because of limited published results. On the subject of seismic gaps, Keilis-Borok said that his group had tried to apply seismic gaps in their prediction work and had no success. Sykes indicated that the recently completed work on fault segments in California was a major new step which the Soviets might want to consider.

T.McEVILLY AND V.KEILIS-BOROK discussed the sample area for the CN algorithm. The 9 for 9 success was a data-fitting exercise using a carefully defined area and all the data for that area. If the boundary of the area had been farther to the north, there would have been no alarm, but it is important to note that the area was fitted to the data set.

B.MINSTER proposed a hypothetical scenario to try to get at some measure of the probabilities. If you had 8 false alarms out of 30 globally, can an advisory report to the State of California say that the TIP has a reliability of 2 out of 3 that the prediction will result in the earthquake?

V.KEILIS-BOROK does not think that he can say that for California. He might say that worldwide the reliability might be 2 out of 3 but for California probably higher. He does not believe that he can give a probability on the scale of 5 years; maybe this is possible for 30 but not 5 years. Rather, he suggested that some actions begin by the government in California rather than be concerned with the probability. All he can say is that the situation in California is similar to 19 other situations worldwide that were followed by large earthquakes, but this is not a probabilistic exercise.

W.THATCHER commented that the current TIP is based on an increase in seismic activity within the circular map area for the period 1983-1986; would a similar pattern have preceded the 1952 Kern County earthquake (he noted increased activity in the years 1946, 1947, and 1948 when there were earthquakes larger than magnitude 6 in that area)?

V.KEILIS-BOROK said he would have to check.

R.WALLACE asked if a magnitude 8 occurred tomorrow, would that end the TIP, and if a magnitude 7 occurred, would that extend the TIP?

V.KEILIS-BOROK indicated that the magnitude 8 would end the TIP but for the magnitude 7, he doesn't know. If followed by extensive aftershock activity then it could extend it; if no aftershocks occurred, it would be just another earthquake. He then summarized the approach being taken to narrow down the area affected by TIP's. He showed a map from S. Smith of Cape Mendocino which plots on a grid the areas of seismic quiescence, potential areas of earthquakes. Aftershock patterns are being tested to see if areas of potential activity can be determined. Maximum magnitudes within grid areas are being plotted against time to establish incursions in the overall pattern for the area, thereby helping to narrow the definition of a TIP.

W.ELLSWORTH commented that the catalogue aftershock data being used in the illustration has been substantially narrowed down by relocation calculations. If the algorithm is also applicable to predict magnitude 7.0 as well as 7.5, will the occurrence of 7.0 close the TIP? (answer, no) Is there a higher probability of the 7.5 than the 7.0 and if so, why?

V.KEILIS-BOROK replied that he does not report probabilities. The TIP appears to have been run at a lower threshold than 7.5, but he would have to check. He did say that a TIP for 7.5 does not mean that there won't be a 6.5, but the 6.5 or 7.0 would be a false alarm for the TIP. The preparation area for an earthquake may be very large in comparison to the actual area affected by the earthquake.

J.DAVIS asked how many TIP's are in effect worldwide at the present time.

V.KEILIS-BOROK replied that there are three: Caucasus, California, and Vancouver Island. Each TIP has its own peculiarities - one may have no clustering of activity while another has waves of clustering. A difficulty is that you need 12 years of data to establish the function and another 6 years to evaluate for a TIP which means almost 20 years of data before a TIP is defined, and this all requires an extensive catalogue.

L.SYKES AND V.KEILIS-BOROK discussed the current TIP's in California. Magnitude 6.0 and 6.1 earthquakes in southern California have apparently caused the TIP under CN for southern California to go away. However, a new TIP is in effect for northern California under CN which was started in May 1986; at the time of this meeting, the data is processed through February 1988, and the TIP is open until the end of the year. Keilis-Borok says it will probably extend to about a year from now (May 1989). Sykes asked if the logic follows: since there is a CN TIP for northern California and an M8 TIP for 7.5 ongoing for all of California, the large earthquake should be expected with higher probability in northern California? Keilis-Borok said that the two calculations are independent and one would have to run a new test to confirm that logic; the question was not resolved.

T.McEVILLY asked if there was an earthquake of magnitude 6.0 with a cluster of aftershocks, followed a month later by a magnitude 6.1 with aftershocks, how would that sequence be treated?

V.KEILIS-BOROK concluded that the effect of the algorithm would be to remove all the aftershocks and leave only the 6.0 and 6.1 mainshocks.

J.FILSON asked if the 6.0 followed the 6.1, would the 6.0 be counted?

V.KEILIS-BOROK replied that the 6.0 would not be counted. A conversation between participants attempted to resolve whether there is in effect a TIP via the CN algorithm for northern California and if the TIP ended for southern California. Because the CN procedure is for a year at a time and it seems that the catalogue available in the USSR for California is not up to date, it is not possible to say whether the TIP continues or not. If the magnitudes are correct for the November 1987 Superstition Hills earthquakes in southern California, that TIP has ended. The confusion stems from the fact that Keilis-Borok says the TIP started in May 1986 and is for one year, and yet he says the TIP probably extends into 1989. There seems to be no resolution of the question of how much data the Soviets have in order to do the CN calculation for northern California.

L.SYKES then asked where we go from here.

V.KEILIS-BOROK suggested (1) the algorithms should be applied to a magnitude 6.4 threshold, (2) cooperative technology exchange programs including training, e.g., mathematical prediction school in Moscow, (3) modeling using super-computers, (4) global experiment for formal predictions, and (5) short-term precursors study, no results so far, but looking at micro-activation applied to CN algorithm.

L.SYKES reintroduced the issue of precisely what is the current TIP for California/Nevada. There was obvious confusion among the Council members which was not readily clarified by Keilis-Borok. The general consensus is that the TIP extends through 1991 for a magnitude 7.5 or greater earthquake in a 7 x 9 degree rectangle centered in California that has been extended to be a 900-km diameter circle with approximately 100-km extensions north and south of that circle (including most of northern and southern California and western Nevada). There was no good definition of how this TIP area actually looked because no map of the total TIP area exists, but the above defined area essentially includes all of the San Andreas and other known active faults of California, Los Angeles, San Francisco and north to Oregon, as well as the Transverse and Coast Ranges, the Sierra Nevada, and western Nevada.

At the conclusion of the afternoon session R. Updike met with V. Keilis-Borok to resolve the statement of actual predictions in effect. The status for California predictions, as June 1988 were:

<u>FOR M8</u> **A TIP FOR A 900-KM DIAMETER CIRCLE CENTERED IN NORTH-CENTRAL CALIFORNIA WITH A 100-KM EXTENSION NORTH AND SOUTH, WHICH INCLUDES MOST OF CALIFORNIA AND WESTERN NEVADA, FOR AN EARTHQUAKE GREATER THAN MAGNITUDE 7.5, IN EFFECT UNTIL THE END OF 1991.**

<u>FOR CN</u> **A TIP FOR NORTHERN CALIFORNIA, FOR AN EARTHQUAKE GREATER THAN MAGNITUDE 6.4, IN EFFECT UNTIL FEBRUARY 1989.**

<u>FOR CN</u> **A TIP FOR SOUTHERN CALIFORNIA, FOR AN EARTHQUAKE GREATER THAN MAGNITUDE 6.4, HAS BEEN CANCELLED BY THE SUPERSTITION HILLS EARTHQUAKES OF 1987.**

June 7, 1988

<u>MORNING SESSION</u>

R.WESSON and **J.FILSON** summarized the status of the final report on "Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault System" by the Working Group on California Earthquake Probabilities. This status report included the interaction of the USGS and the State of California, coordination with the press, the press release, distribution lists, and proposed press briefings.

J.DAVIS reported on the status of the report with respect to the State of California, including briefing to the California Earthquake Prediction Evaluation Council (CEPEC). Changes to the text prior to final status reflect the concerns of CEPEC. The California Office of Emergency Services (COES) seems to be solidly behind the release of the report.

R.WALLACE, J.FILSON, AND J.DAVIS discussed the problem of earthquake insurance in California. Apparently, premiums and deductibles have increased substantially because of recent earthquakes and increased awareness of the earthquake risk in the State.

L.SYKES encouraged the USGS and the State of California to work together to produce seismic risk maps in the State, particularly intensity maps for specific model earthquakes.

J.FILSON indicated that the USGS is trying to do loss estimations immediately after earthquakes, based upon evaluation of specific classes of buildings.

L.**SYKES** also suggested that there be some concerted effort to evaluate the location of the next one or few earthquake prediction experiments. There is a tendency at the present time to use the Cajon Pass borehole studies as the focus for the next prediction experiment. Before a decision such as that is made, there should be input from the best thinking in the country, perhaps through a workshop or advisory panel. Sykes was concerned that major decisions like this have often been made alone by a few persons within the USGS.

R.**WESSON** pointed to the fact that the budget for the earthquake program has remained essentially level, and in the next fiscal year may be lower. For FY90 the Director of the USGS has supported an initiative to increase the earthquake budget. One must ask, however, in the existing budget climate whether the emphasis on southern California is sufficient, i.e., should funds be shifted from elsewhere in the country to this specific need.

W.**THATCHER** asked what CEPEC had to say after reading the Working Group report and having the briefing provided by J. Dieterich. He had the sense that CEPEC was not vigorously endorsing the report.

J.**DAVIS** said that a letter had been sent to R. Andrews, COES, calling attention to the Working Group report and reiterating the earthquake hazard in the State. By providing input to the Working Group on suggested changes of the draft report, he felt there was an implied endorsement of the report. The reservations of CEPEC focused on two points: (1) there are other potential sources of earthquakes in California not cited in this report, and (2) there are other ways to determine probabilities besides recurrence interval assessments. CEPEC does feel that this report is a very useful contribution, and the language changes that have been made should eliminate the potential concerns.

L.**SYKES** introduced the topic of the evaluation of the Keilis-Borok prediction. He had asked R. Wallace and K. Aki to draft a summary statement based upon the discussions of that morning. First, he suggested going around the table to hear individual comments from members of the Council.

W.**THATCHER** said that in terms of the general scientific validity of the approach he is skeptical and would feel that a primary task is to assess the validity of the approach. Given that skepticism, any public policy action should be minimal. The prediction should be made public but due to the size of the area and the length of the time window, very little public policy activity can follow. There is a responsibility for the scientists in this country to assess the implications of this work. The long-term value of this work can only be assessed after some work, in the next year or so, by U.S. scientists. It is important to look at the concept that precursors may be regional as well as local.

K.AKI discussed the comparison of the paleoseismology approach and the Keilis-Borok approach. At first, he thought that the two approaches could be combined to yield a substantial probability gain over the next few years. However, it appears that the two cannot be combined. The alarm under the Keilis-Borok approach does not increase toward the end of the time window and actually decreases unless there is some additional precursory activity to extend the TIP. The Working Group probabilities may be increased by the Keilis-Borok prediction by 10 percent each year for the period of the TIP. He recommended that the USGS take the responsibility to provide to Keilis-Borok the data he needs to make more accurate calculations. He also felt that no public action was presently warranted.

H.KANAMORI expressed that despite the objective appearance of the Keilis-Borok method, there are many subjective judgments involved. The presentation by Keilis-Borok helped to clarify the methodology, and Kanamori feels that the work uses a standard seismicity approach rather than a pattern recognition approach. As such, someone in the United States should look at this approach more critically, but in order to do so, two things must first be done: (1) there must be a complete catalogue of seismicity for southern California and Nevada and (2) the global seismic catalogue must be cleaned up.

W.ELLSWORTH pointed out that the study is in a time interval that does indeed have an unusual pattern of seismicity, i.e., the seismicity distribution of 1980-1986 is truly unusual, but is it significant? The United States has already taken some prior action on the critical information which led to the TIP; the USGS declared an earthquake watch for the White Mountains seismic gap following the 1980 activity which was later cancelled. He does not feel that any additional public action should be taken and that the elevation of concern as a result of this work is not great. In comparing the results from long-term probabilistic and pattern recognition approaches, he thinks that they may not be independent. The probability gain of the TIP over those reported by the California Working Group is not large but may be significant in terms of increased awareness of the regional activity. The physics of the California tectonic system cannot be inferred from this method, and this is why we cannot apply a probability gain to the paleoseismic estimates of the Working Group report.

J. **DAVIES** was not convinced by the arguments of Aki and Ellsworth and believes that the Keilis-Borok method could narrow the time window determined by paleoseismology. He was concerned about how we should go about evaluating the statistics used.

R. **WESSON** raised the question of comparing annual probabilities from the two methods. The Working Group report gives a 5-year probability of 0.1 for southern California for the three capable faults, compared to a Poisson of 0.1 for all of California and compared to 0.1 for the 4-year time window for California/Nevada of Keilis-Borok. If a congressman were to ask if he should do anything different based upon the Keilis-Borok prediction, the response might be, "if you believe what I have told you about the southern California probability, then this report does not require additional action."

B. **TUCKER** does not feel that the same standards were applied to Keilis-Borok's prediction as would be demanded from that of someone of less stature; specifically, a statement of probability was not given. Further, this prediction is not as well-defined in terms of area, magnitude, and time window as might be required from an unknown scientist. The specifications of area, magnitude, and time that were settled on came mainly through questioning, and there still seems to be some ambiguity.

T. **McEVILLY** agreed with Tucker. However, we cannot leave this issue on the table; someone is needed to evaluate this technique in this country.

J. **FILSON** feels that the United States must be able to do this approach in order to evaluate its validity. Some physical modeling of the regional strain fields must be pursued to explain why it works, if it does work. Perhaps we should start now to get this approach on our computers.

R. **WALLACE** was impressed by the presentation. Seismicity patterns must be an integral part of our program. Although not a particularly useful prediction, this is a significant step forward. He was impressed that there seems to be a pattern worldwide. He feels that NEPEC should endorse this as a useful direction of research, and that even though this may not be a useful specific prediction, that work should be carried forward in this research area.

J. **DAVIS** was impressed by the great deal of data fitting that was required to obtain the results reported. Data fitting was required in geography, in time, and in terms of a threshold. This does not mean that he is negative on the approach but that it is premature to issue public policy actions based upon this prediction. He suggests distinguishing two responses: (1) a scientific response that includes recommending cleanup of the catalogues, additional retrospective work, and encouragement of future work and refinement, and (2) a public safety response that states that this work is not ready for public policy actions.

L.SYKES agreed that we should distinguish between public policy actions and science. He feels that the approach is coming of age and cannot be ignored; we must treat it in a serious way scientifically. It attempts to formally combine a number of patterns that have been worked on individually by several scientists, i.e., clustering. We must combine the applied mathematical strength of the Soviet Union with our more geological/seismological strengths. We cannot convert their work directly to public policy because of the geographic problem, i.e., the size of the area makes it not very useful. He expressed great appreciation for the technical review provided by B. Minster. We should stress the strengths of the Keilis-Borok approach; the work comes at a time when U.S. scientists are becoming increasingly concerned about the likelihood of magnitude 7 or greater earthquakes in California. We need to try to test the hypothesis that major precursors extend out several times the area of the fault zone.

T.McEVILLY felt that the off-the-cuff modification of the area of alarm, i.e., 200-km extension north-south of the original 900-km diameter circle, should be disregarded.

L.SYKES AND W.THATCHER disagreed, feeling that he has made an objective modification and that this should clearly be stated in the record in the event the earthquake does occur and the question arises as to whether the prediction was successful.

J.FILSON indicated that Keilis-Borok made a request for up-to-date data from USGS-Golden and that through the USGS-USSR cooperative agreement for earthquake prediction, the two issue global earthquake predictions. Filson indicated that the USGS would provide the data to Keilis-Borok but leave the prediction process to the Soviets.

K.AKI AND R.WALLACE drafted a summary evaluation of the Council for circulation to Council members for review with a final draft prepared by Sykes.

W.ELLSWORTH summarized the content of a discussion document that he had prepared for the Council (attached herewith as an

Appendix). He emphasized the utility of the Poisson distribution for describing the rate of earthquakes in California, and found that rate surprisingly high. Also, he noted that several of the large historic earthquakes have occurred on faults that the Working Group on California Earthquake Probabilities has been working on, but that several others were on poorly known faults. The Keilis-Borok approach may be analogous to two Poisson probabilities, one that is time-invariant and the other where there is a significantly higher value for a certain window.

L.SYKES, J.FILSON, R.WESSON, AND T.McEVILLY discussed cooperative research with the Soviet Union in earthquake prediction. We have been working with the Soviets under a cooperative agreement since 1972; over the years the cooperative activity has died down, but the agreement has remained in force. Recent interactions have primarily been in the form of exchange of scientists. Realistically, it seems unlikely that there could be a major new infusion of money for US-USSR cooperative research. It would be helpful if NEPEC could advise the USGS on what new steps could be taken if no new money becomes available. The Council does encourage that if there are U.S. scientists who are interested in researching this approach, some modest amounts of funding be made available so as to encourage that work.

K.AKI AND R.WALLACE presented the draft document they had prepared to the Council. The Council discussed the content of the document; changes suggested will be incorporated into the final draft. R. Wesson clarified that our basic statement is that this is a very interesting method that merits further study but that it has not sufficient maturity to currently influence public policy. (Note: The revised summary comments are attached as Appendix F to this report.) The members of NEPEC expressed their deep appreciation to L. Sykes for the excellent leadership he had provided to the Council over the past few years. He is resigning his post as chairman of the Council after this meeting.

L.SYKES thanked the Council and adjourned the meeting at 12:10 p.m.

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Updike, R.G., 1988, Proceedings of the National Earthquake Prediction Evaluation Council, February 1-2, 1988, Menlo Park, California: U.S. Geological Survey Open-File Report 88-438, 21 p. and 11 appendices.

APPENDICES

A) Handouts provided to NEPEC by Dr. Keilis-Borok during his presentation, June 6, 1988

B) Background communications between L. Sykes, V. Keilis-Borok, and L. Knopoff, November 1986 to February 1988

C) Review of Keilis-Borok methodology by R. A. Haubrich, April 6, 1988

D) Review of Keilis-Borok methodology by J. B. Minster, May 25, 1988

E) Comments submitted to NEPEC by W. L. Ellsworth regarding a priori probability of major earthquakes in California-Nevada

F) Letter from L. Sykes to D. Peck, regarding evaluation and recommendations of NEPEC with regard to the Keilis-Borok prediction

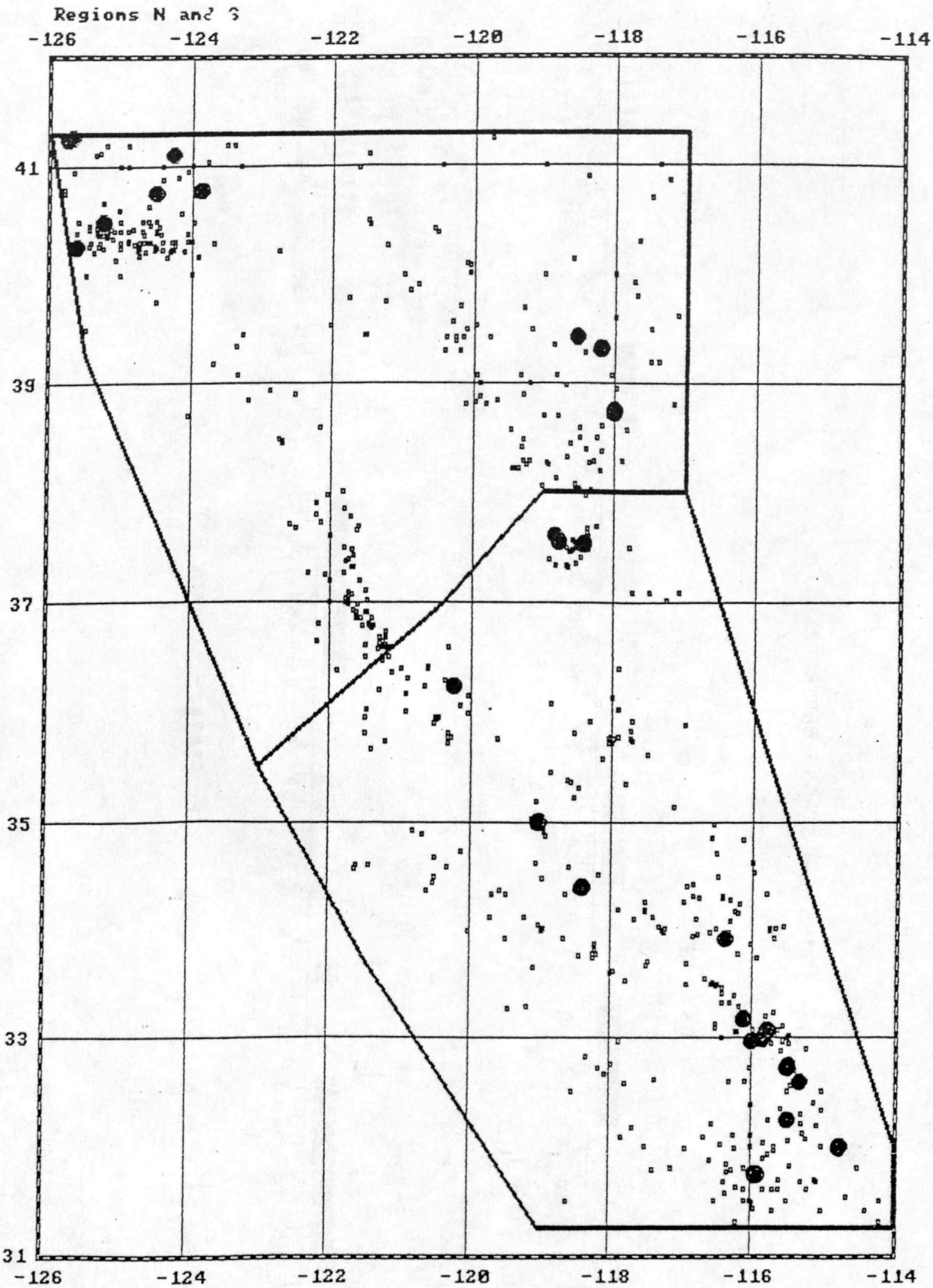
G) Telex from J. Filson to V. Keilis-Borok, and reply, regarding recommendations of NEPEC relative to his prediction

H) Letters of appreciation from L. Sykes to V. Keilis-Borok and L. Knopoff for presentations at NEPEC meeting

I) Communications relevant to the Working Group on California Earthquake Probabilities report, subsequent to the February 1988 NEPEC meeting

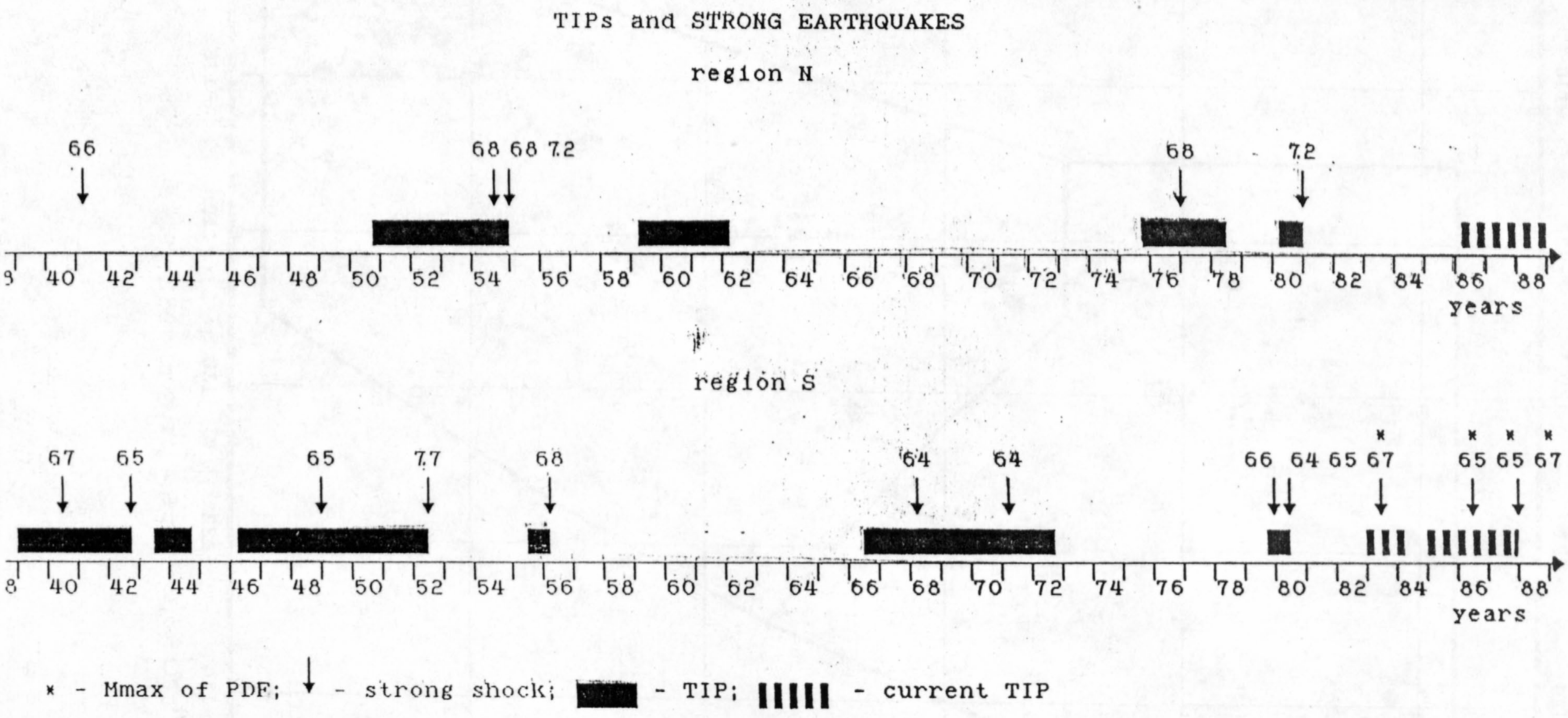
APPENDIX A.

HANDOUTS PROVIDED TO NEPEC BY DR. KEILIS-BOROK DURING HIS PRESENTATION, JUNE 6, 1988



▪-M ≥ 4.5; ○-M ≥ 6.4 , since 1932

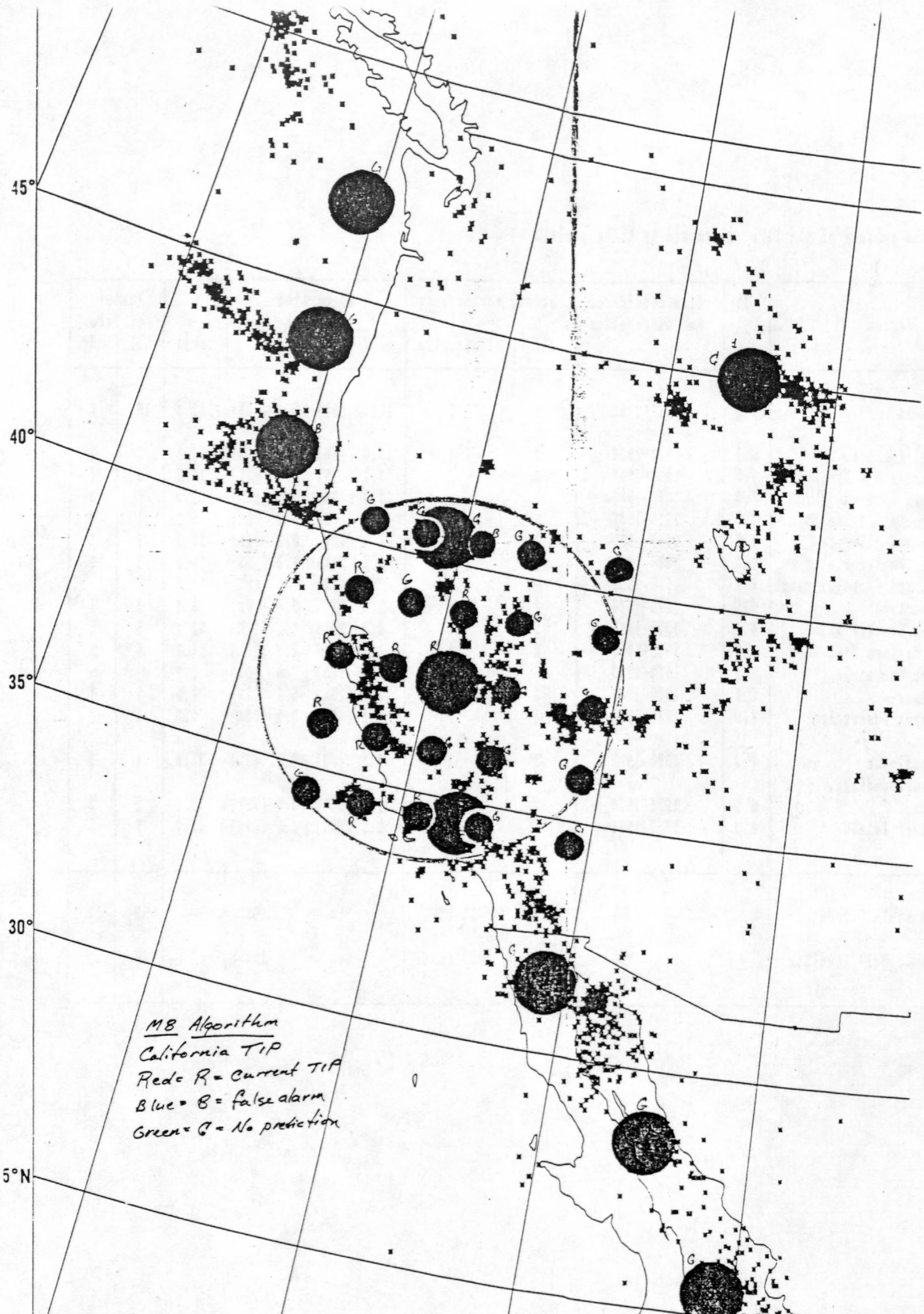
9



TIPs and STRONG EARTHQUAKES
region N
66
68 68 7.2
68
7.2
8 40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74 76 78 80 82 84 86 88
years
region S
67 65
65
7.7
68
64 64
66 64 65 67
65 65 67
8 40 42 44 46 48 50 52 54 56 58 60 62 64 66 68 70 72 74 76 78 80 82 84 86 88
years
* - Mmax of PDF; ↓ - strong shock; - TIP; IIIII - current TIP

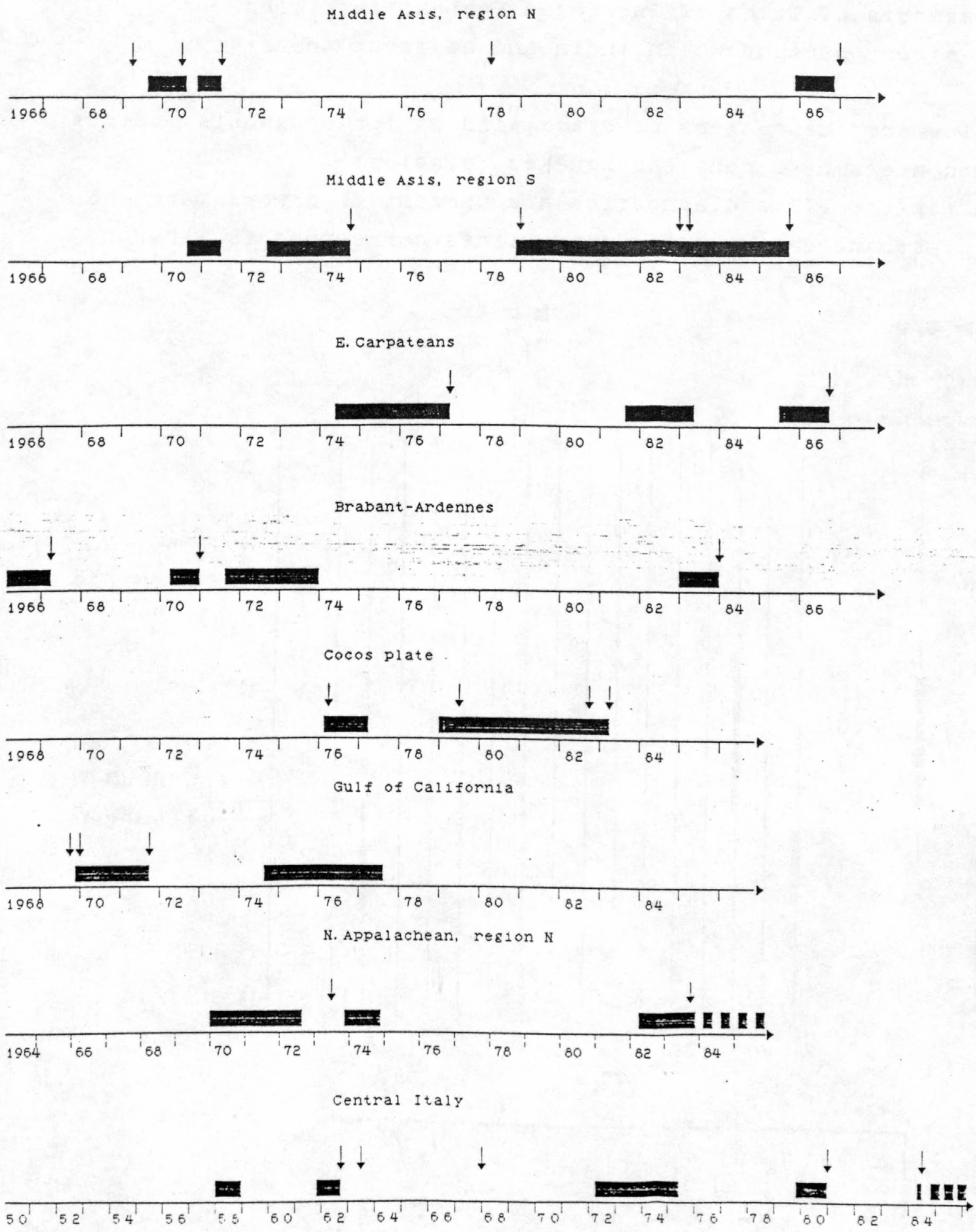
Diagnostics of TIPs by algorithm CN

Regions	Time considered	M_o	k/K	Average duration of TIPs per ea-ke years	Total duration of TIPs per reg. %	Adaptations
California, Nevada						
region S	1938-1983.1	6,4	9/ 9	2.2	42	learning
region N	1938-1983.1	6,4	4/ 6	1.7	22	learning
California, Nevada						forward diagnosic,
region S	1983-1988.4	6,4	3/ 3	1.4	69	none
Middle Asia						
region N	1966-1987.8	6,4	2/5	0,6	12	none
region S	1966-1987.8	6,4	5/5	1,9	43	none
Baikal	1966-1984.1	6,4	0/ 0	0	0	none
E. Carpatheans	1966-1986.7	6,4	2/ 2	3,3	30	none
Gulf of California	1968-1984.1	6,6	2/ 3	1,7	31	none
Cocos plate	1968-1984.1	6,5	4/ 4	1,6	38	none
W. Appalachean						
region N	1964-1985.1	5,0	1/ 2	1,6	28	none
region S	1964-1985.1	5,0	0/ 0	0	0	none
C. Italy	1954-1986.1	5,6	3/ 5	1,2	18	none
Brabant-Ardennes	1966-1987.1	4,5	3/ 3	1,9	25	Σ change
Caucasus	1966-1984.1	6.4	2/4	0,5	9	Σ change
other triplets						
Kopet-Dag	1966-1984.1	6.4	1/1	8,4	48	none
Kuril arc	1966-1984.1	7.5	2/3	1,0	17	none
Kamchatka	1966-1984.1	7.3	2/3	2,1	34	Δ = 6



First Edition, July 1, 1970.
Lambert Conformal Conic Projection

Epicenters have been thinned on a grid composed of 0.42 inch squares

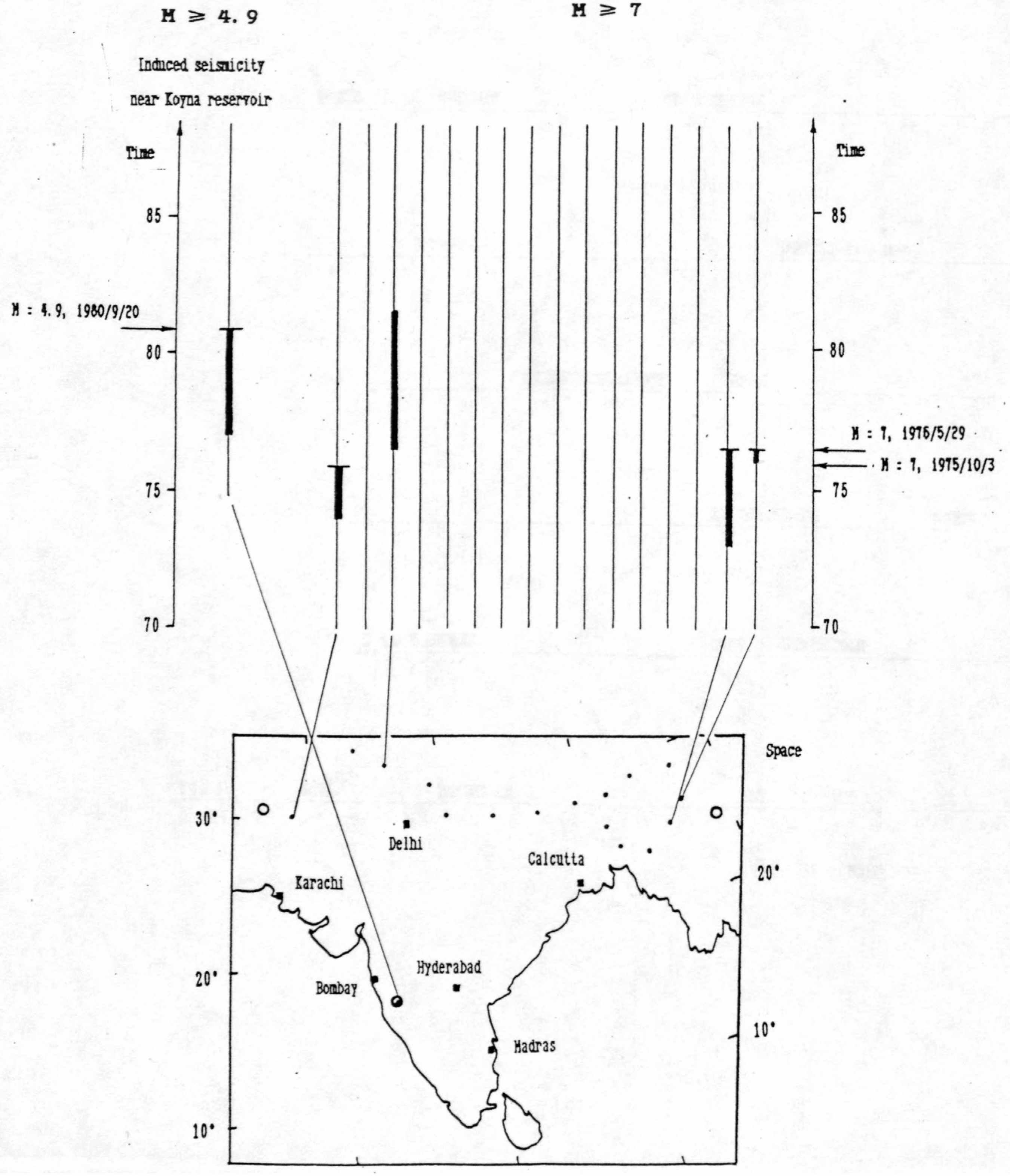


↓ - strong shock; ▬ - TIP; IIII - current TIP.

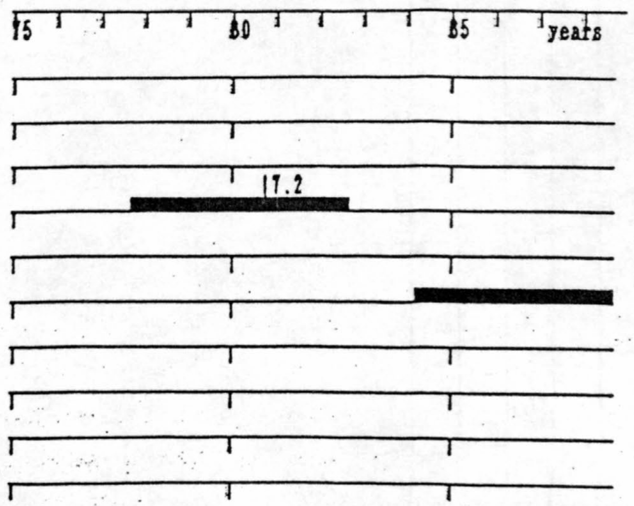
The diagnosis of Times of Increased Probability (TIPs) of a strong earthquake in India and adjacent areas

Below are the centers of space windows for diagnosis (points) and epicenters of strong earthquakes (circles).

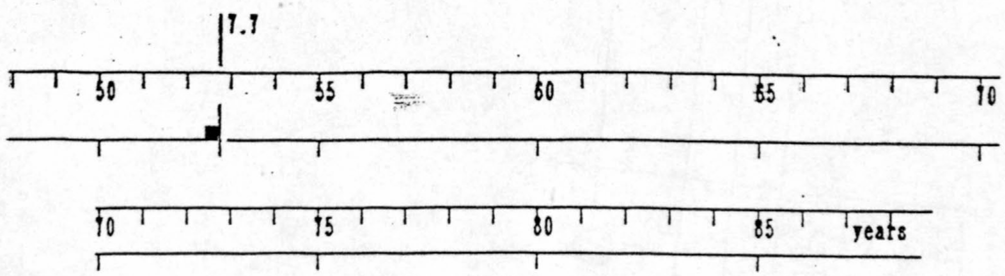
On top the TIP's diagnostics are presented: arrows marc the dates of strong earthquakes, heavy lines correspond to TIPs.

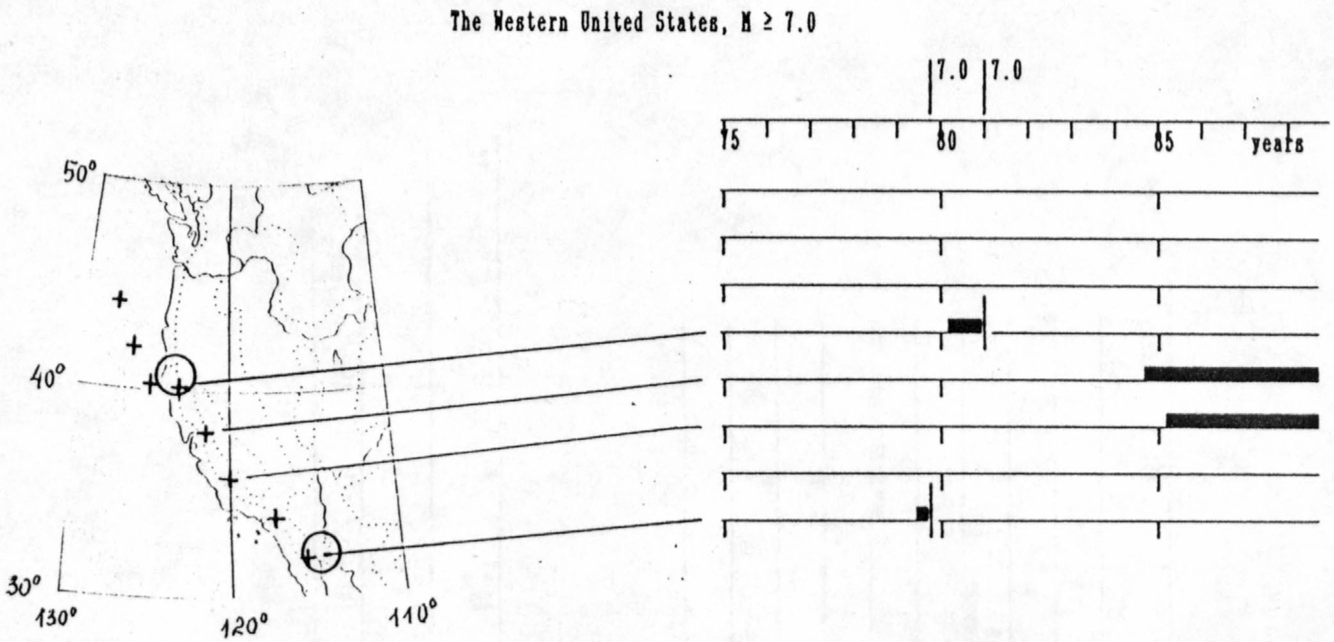


Western United States, $M \geq 7.5$



hern California, $M \geq 7.5$




The Western United States, M ≥ 7.0
7.0
7.0
75
80
85
years
50°
40°
30°
130°
120°
110°

```
1. Catalog of main shocks : mwest.dat          Mo = 7.50
2. Time interval (Tb,Te) = (1975 1990);
   Step in time  dt =  .50000 years
   Activity is estimated in a period from Tb up to Te
   Constants Ma, e are fixed in catalog; alfa = 0
   2*R(Mo) = (exp(Mo-5.6)+1) degrees
3.  1  circles    R =R(Mo) km
No.  lat      lon   T*: ye mo da ; No.   lat      lon   T*: ye mo da ;
(1) 37.50  -119.50   1988  1  1;
4.  7 functions in tip's diagnosis
No. func  m    mm    a    aa   s/dt  u/dt  To    beta   d
 1)   n  act         10          12
 2)   n  act         20          12
 3)   l  act         10          12         1964
 4)   l  act         20          12         1964
 5)   s  act  shf    10 0.50     12                0.46  0.67
 6)   s  act  shf    20 0.50     12                0.46  0.67
 7)   b  shf  shf  2.00 0.20      2
5. Constants of diagnostics:
 quantile p = (for b, for others) = ( .25  .10)
 time window for "simultaneous" extrema Tex =  6*dt
 thresholds (g and h) = (  4   6)
 alarm duration Tau = 10*dt
Radius of  circles        426 km
Magnitude thresholds in region No. 1    ( 38 quakes per year )
 1: 4.2010.00;  2: 3.8010.00;  3: 4.2010.00;  4: 3.8010.00;  5: 4.20 7.00;
 6: 3.80 7.00;  7: 5.50 7.29;
ctip  1 ( 37.50:-119.50)*(1987  7  2 : 1992  7  1)  4  6  4  7

        Region No.  1          1988        37.50      -119.50
                            1     2     3      4     5     6     7
 2: 3 4.6 1975  7  2       92*   156   -23    -58   606   572    -
 2: 3 5.7 1976  1  1       83*   147   -37    -71   575   544   17
 1: 2 6.1 1976  7  1       79    144   -40    -71   575   541   17
 1: 2 5.0 1976 12 31       69    137   -56    -79   564   524    -
 1: 2 5.0 1977  7  1       62    128   -62    -87   509   482    -
 1: 1 5.2 1977 12 31       57    123   -64    -89   518   484    -
 1: 1 4.4 1978  7  2       55    125   -62    -81   500   472    -
 0: 0 5.6 1978 12 31       55    131   -59    -69   520   483    1
 0: 0 4.7 1979  7  2       58    137   -53    -57   514   483    1
 0: 0 5.7 1980  1  1       62    144   -47    -46   530   496    7
 1: 1 6.1 1980  7  1       59    145   -49    -43   590   525  140*
 1: 1 5.0 1980 12 31       59    143   -47    -44   608   535  140*
 1: 1 4.5 1981  7  1       63    146   -40    -38   602   534    -
 1: 1 5.8 1981 12 31       63    144   -38    -38   608   536   18
 1: 1 4.7 1982  7  2       63    144   -36    -36   599   532   18
 1: 1 5.2 1982 12 31       66    151   -33    -29   603   538    -
 1: 1 6.5 1983  7  2       68    153   -28    -23   650   568   59
 2: 2 4.6 1984  1  1       73    158*  -20    -16*  637   566   59
 2: 4 6.1 1984  7  1       80*   162*  -11*   -10*  676   597   [illegible]7
 2: 4 5.7 1984 12 31       78    157*  -12*   -15*  669   591   43
 2: 4 4.6 1985  7  1       75    155   -15*   -17   670   587   43
 3: 5 5.9 1985 12 31       72    149   -19    -23   692*  597    5
 3: 5 5.5 1986  7  2       71    140   -19    -32   658   578    5
 4: 7 6.2 1986 12 31       73    138   -16    -33   690   603*  76*
 4: 6 4.6 1987  7  2       72    140   -17    -31   691*  601*  76*
 3: 4 5.7 1988  1  1       72    140   -16    -29   693*  604*  11
 2: 3 4.6 1988  7  1       71    134   -16    -34   691*  603*  11
 Number of stip's =  0; ttip's =  0; ctip's =  1; ftip's =  0; e.c.'s =  0
```

1. Catalog of main shocks : mwest.dat Mo = 7.50
2. Time interval (Tb, Te) = (1975 1990);
   Step in time dt = .50000 years
   Activity is es..mated in a period from Tb up to Te
   Constants Ma, e are fixed in catalog; alfa = 0
   2*R(Mo) = (exp(Mo-5.6)+1) degrees
3. 1 circles R =R(Mo) Km

No.	lat	lon	T*: ye mo da ;	No.	lat	lon	T*: ye mo da ;
1)	37.50	-119.50	1983 7 2;				

4. 7 functions in tip's diagnosis

No.	func	m	mm	a	aa	s/dt	u/dt	To	beta	d
1)	n	act		10		12				
2)	n	act		20		12				
3)	l	act		10		12		1964		
4)	l	act		20		12		1964		
5)	s	act	shf	10	0.50	12			0.46	0.67
6)	s	act	shf	20	0.50	12			0.46	0.67
7)	b	shf	shf	2.00	0.20	2				

5. Constants of diagnostics:
   quantile p = (for b, for others) = (.25 .10)
   time window for "simultaneous" extrema Tex = 6*dt
   thresholds (g and h) = (4 6)
   alarm duration Tau = 10*dt
Radius of circles 426 km
Magnitude thresholds in region No. 1 (38 quakes per year)
1: 4.2010.00; 2: 3.8010.00; 3: 4.2010.00; 4: 3.8010.00; 5: 4.20 7.00;
6: 3.80 7.00; 7: 5.50 7.29;

ctip 1 (37.50:-119.50)*(1983 12 31 : 1988 12 30) 4 6 4 6
ctip 1 (37.50:-119.50)*(1984 12 30 : 1989 12 31) 4 6 4 6
ctip 1 (37.50:-119.50)*(1985 12 31 : 1990 12 31) 4 6 4 6
ctip 1 (37.50:-119.50)*(1986 12 31 : 1991 12 31) 4 6 4 6
ctip 1 (37.50:-119.50)*(1987 12 31 : 1992 12 30) 4 6 4 6

Region No. 1 1983 37.50 -119.50

						1	2	3	4	5	6	7
3:	4	4.6	1975	7	2	92*	156*	-35*	-80	606	572*	-
3:	4	5.7	1975	12	31	83*	147	-47	-90	575	544	17
3:	4	5.1	1976	7	1	79	144	-50	-89	575	541	17
3:	4	5.0	1976	12	30	69	137	-65	-96	564	524	-
3:	4	5.0	1977	7	1	62	128	-70	-102	509	482	-
3:	4	5.2	1977	12	31	57	123	-72	-103	518	484	-
1:	1	4.4	1978	7	1	55	125	-69	-94	500	472	-
0:	0	5.6	1978	12	31	55	131	-66	-81	520	483	1
0:	0	4.7	1979	7	2	58	137	-59	-68	514	483	1
0:	0	5.7	1979	12	31	62	144	-52	-56	530	496	7
1:	1	6.1	1980	7	1	59	145	-55	-53	590	525	140*
1:	1	5.0	1980	12	30	59	143	-52	-53	608	535	140*
1:	1	4.5	1981	7	1	63	146	-45	-46	602	534	—
1:	1	5.8	1981	12	31	63	144	-42	-46	608	536	18
1:	1	4.7	1982	7	1	63	144	-41	-44	599	532	18
2:	2	5.2	1982	12	31	66	151	-37	-36*	603	538	—
4:	6	6.5	1983	7	2	68	153*	-32*	-30*	650*	568*	59*
4:	6	4.6	1983	12	31	73	158*	-24*	-22*	637	566	59*
4:	6	6.1	1984	7	1	80	162*	-14*	-16*	676*	597*	7
4:	6	5.7	1984	12	30	78	157*	-15*	-21*	669*	591*	43
4:	6	4.6	1985	7	1	75	155*	-18*	-23*	670*	587*	43
4:	6	5.9	1985	12	31	72	149	-22*	-29*	692*	597*	5
4:	6	5.5	1986	7	1	71	140	-22*	-38	658*	578*	5
4:	6	6.2	1986	12	31	73	138	-19*	-39	690*	603*	76*
4:	6	4.6	1987	7	2	72	140	-20*	-36*	691*	601*	76*
4:	6	5.7	1987	12	31	72	140	-19*	-34*	693*	604*	11
3:	5	4.6	1988	7	1	71	134	-19*	-39	691*	603*	11

Number of stip's = 0; ttip's = 0; ctip's = 5; ftip's = 0; e.c.'s = 0

```
 1: Spacial distribution
    Select from number -      1
    Input catalog - mwest.dat                    4346 events
    from 1978yr, im, id, Oh, Om  to 1984  1  1  0  0
 2: You choose magnitude mb
 3: Min,max magnitude:    3.80,    9.00
              depth: -10.00, 900.00
 4: Rectangle:
    lat.  from    33.50 to    41.50
    lon.  from  -124.50 to  -114.50
 5:        Axis parameters :
 6: ***** horizontal axis -
longitude *****
    from -124.50 to -114.50
    interval   0.500 and shift   0.500
    accumulation - n
 7: ***** vertical axis    -
latitude *****
    from   41.50 to   33.50
    interval   0.400 and shift  -0.400
    accumulation - n
 8: ***** list            -
No variable *****
 9: Count - nu
10: type-y;printout-y
11: name of output initial file - map.his
```

	-124.50		-123.50		-122.50		-121.50		-120.50		-119.50		-118.50		-117.50		-116.50		-115.50		-114.50	
41.50	1	.	.	.	.	4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	5
	1	.	.	.	1	.	.	.	.	.	.	.	.	.	1	.	1	.	.	.	.	4
40.70	2	1	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	4
	1	1	.	.	.	.	.	1	1	1	.	.	.	.	.	.	.	.	.	.	.	5
39.90	.	.	.	1	.	.	.	1	.	.	.	.	.	.	1	.	.	.	.	.	.	3
	.	.	1	.	.	.	.	.	3	2	1	.	.	1	.	.	1	.	.	.	.	9
39.10	.	1	.	2	.	.	.	.	.	3	1	1	.	1	.	.	2	.	.	.	.	11
	.	.	.	1	.	1	.	.	.	.	.	.	4	1	.	.	.	.	1	.	.	8
38.30	.	.	.	.	.	.	.	.	.	.	5	7	3	.	.	.	.	.	.	.	.	15
	.	.	.	.	2	3	.	.	.	.	.	21	1	.	.	.	1	.	1	.	.	29
37.50	.	.	.	.	1	6	.	.	.	1	.	2	.	1	1	.	.	.	1	.	.	13
	.	.	.	.	2	6	3	.	.	.	.	.	.	.	.	.	2	.	.	.	.	13
36.70	.	.	.	.	.	.	5	1	1	.	.	.	.	.	.	.	.	.	.	.	.	7
	.	.	.	.	.	1	.	.	3	.	.	.	.	3	.	.	.	.	.	1	.	8
35.90	.	.	.	.	.	.	1	.	.	.	.	.	3	10	.	.	.	.	.	.	.	14
	.	.	.	.	.	.	.	.	.	.	1	.	1	.	.	.	1	.	.	.	.	3
35.10	.	.	.	.	.	.	.	1	1	.	3	1	.	.	.	.	1	.	.	.	.	7
	.	.	.	.	.	.	.	.	.	1	.	1	.	1	.	2	1	.	.	.	.	6
34.30	.	.	.	.	.	.	.	.	.	.	.	3	.	1	2	5	2	.	.	.	.	13
	.	.	.	.	.	.	.	.	2	.	2	2	2	1	.	3	1	.	.	.	.	13
33.50	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0
190 events	5	3	1	4	6	21	9	4	11	9	13	38	14	20	5	10	13	0	3	1	0	

Why universal patterns for such diverse regions?

It is established; diagnosis is reproducible and stable.

It is not unusual in non-linear systems, after smoothing.
3 stages of it in our case: large time intervals and areas; functionals; rule of diagnosis.

Anyhow, prediction is claimed only for strongest earthquakes of a region so far.

Opposite would be unplausible (models).

* * *

Which earthquakes specifically generated the TIP? K. Aki

What now?

Global experiment (R.M)
Software
Models: theoretical (L.K)
        supercomputors (F.P)
Mendocino scenario
Short-term
Digital records

APPENDIX B.

BACKGROUND COMMUNICATIONS BETWEEN L. SYKES, V. KEILIS-BOROK, AND L. KNOPOFF, NOVEMBER 1986 TO FEBRUARY 1988

Received by
Hughes - Aug
1987

UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF PHYSICS

Telephone: 0223 - 337733
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CAVENDISH LABORATORY
MADINGLEY ROAD
CAMBRIDGE CB3 0HE

This is a report on my discussions with V.G. Kosobokov and V.I. Keilis-Borok on the Soviet prediction of a very large earthquake in California and/or Nevada in the interval 1984-1988. This report has appeared in the preprint Algorithms of Long -Term Earthquakes' Prediction, by 15 Soviet and one U.S. authors; the first author is A.M. Gabrielov[1] in alphabetical order of authorship. The preprint, 61 pages in length, has been distributed widely in the form issued by CERESIS in Lima, in September, 1986. At the time of my visit to the USSR, 14-21 October, 1986, I was informed that the paper has been submitted for publication; I assume, without direct information, that publication will be in Russian.

The prediction for California is to be found on pages 51 and 57 of the preprint. This section, written by V.I. Keilis-Borok, V.G. Kosobokov and W. Rinehart[2] indicates that an alarm exists for an earthquake with $M \geq 7.5$ in a roughly square region of dimensions $7^o x 9^o$, centered at 37.5^oN and 119.5^oW, and extending from January 1, 1984 to January 1, 1988. For reference, the center of the rectangle is just south of Yosemite National Park. The window spans both San Francisco and the northern part of Los Angeles.

The method of prediction is derivative from the better known method of pattern recognition. By way of aside, the method of pattern recognition first burst upon the western geophysical scene in a paper by Gelfand, Guberman, Keilis-Borok, Knopoff, Press, Ranzman, Rotvain and Sadovsky,[3] (Phys. Earth and Planet Interiors, 11, 227-283, 1976); the Gelfand, et al. paper was concerned with the identification of potential epicenters of future great earthquakes in California, without regard to the times at which these events were likely to occur. This more traditional method has been recently applied to temporal recognition of times at which large earthquakes are likely to occur, without regard to their epicenters, in California and Nevada. This paper, Prediction of Times of Occurrence of Strong Earthquakes in California and Nevada, by V.I. Keilis-Borok, L. Knopoff, I.M. Rotwain and C.R. Allen[4] is in the last stages of preparation for publication.

I indicate how the new technique differs from the old by reviewing the older version. In the "classical" pattern recognition procedure, a three stage process was followed. The first, called learning, involved writing down all the possible environmental attributes that one could think of that might be associated with the pattern of rare events that we are trying to predict. So, for example in the case of temporal recognition (see paper Prediction of times...), one cites swarms, gaps, foreshocks, remoteness in time of the last preceding large earthquake, etc., as a list. One then breaks up the time history into two parts, that corresponding to the times immediately before large events in the region, and that that does not. If an attribute from the list, or combination of these attributes is observed to occur frequently in one of the two groups of time intervals and infrequently in the other, then it is accepted as a pattern for future study. The second stage is called voting. In this stage, the patterns that have been accepted are counted to see how frequently they are successful or unsuccessful in each of the two time interval-groups. If the voting has a majority of one sign for one type of region and conversely for the other, then the procedure is deemed operative. The final stage involves testing the list of successful patterns by a variety of means. Perhaps the most critical of these tests, is the direct application of the pattern recognition procedure derived for one region, to a second region without further adjustment of parameters. This has been done both in the spatial predictions of Gelfand, et al.[3], and in the temporal predictions of Keilis-Borok, et al.[4], and with significant success in both cases.

However the success of the predictions for California and Nevada by Keilis-Borok, et al.[4] is restricted to earthquakes in the M=6.5 range and depends on the availability of a significant catalog of historical seismicity of reasonably high quality; in this case, the catalog spans more than 50 years. Although the "learning" episode was performed on only 14 earthquakes in California, a direct application of the procedure to earthquakes in Central Asia and the Caucasus, give an admirably high success rate of temporal prediction.

The methodology for California is not directly applicable for extension to prediction of larger earthquakes for several reasons. The principal reason is the obvious one: if one were to raise the magnitude threshold for prediction to, let us say, 7.5 for California, then there would only be one earthquake in the past 50 years to practice on, namely the Kern County Earthquake, instead of 14 or so. Therefore, one must consider very large earthquakes worldwide as a data set for learning and the other subactivities of pattern recognition. In this case, the program must be made robust, sufficiently so that its success (or failure) will not be locally or region-dependent on the choice of parameters. Robustness significantly reduces the number of parameters in the procedure. The new procedure is called M8; the older one, specific to California[4], is called CN. In the M8 program, most of the learning phases of the procedure are skipped. Instead, guided by program CN[3], they have fixed on 4 traits extracted wholly from program CN. These have now been made robust so that they can be applied to any region of the world with an adequate catalog. For example, several of the magnitude cutoffs for counting main earthquakes are set so that the average seismicity of main shocks above the threshold is the same number for all regions. Other thresholds are fixed at a given number of magnitude units below the threshold for definition of strong earthquakes.

The parameters in the algorithm were data-fitted to the precursory, main-shock seismicity of 6 earthquakes in the years 1965-1982 with magnitudes greater than 8.0. The time interval 1965-1982 spans that part of the NOAA catalog for which magnitudes are reported. In all of the work described, surface-wave magnitudes are used. (Three additional earthquakes in the above time interval with magnitudes greater than 8 were not used in the data fitting because they were not preceded by significant predecessor seismicity).

Main shocks are identified by removing aftershocks from the catalog according to a certain algorithm[5]; the details of the aftershock filter are not crucial to the procedure. For three of the four criterion traits, two magnitude-normalized thresholds are set so that two different measures of seismicity are obtained. For the fourth criterion, which is a measure of the number of aftershocks of predecessor earthquakes, only one threshold is set. When any of the first three criterion traits- with two different thresholds for the definition of mainshocks-exceeds the 90th percentile in a three-year sliding window, it is declared to be "abnormally large". In the fourth case, the aftershock seismicity is "abnormally large" if it exceeds the 75th percentile.

An alarm is sounded if
a) "abnormally large" signatures appear in 6 or 7 of the 7 components.
b) If these signatures appear in only 6 components, it must appear in all four traits. (If it appears in all 7 components, by definition it appears in all four traits).
c) a) and b) must be satisfied on two successive evaluations taken 6 months apart.

The alarm window is 5 yearsfrom the date of the second of the two consecutive 6 month evaluations.

Following the initial exploration for parameter adjustment, the algorithm was applied to seismicity in 9 different areas of the world, for which good documentation is available, but this time catalogs were used mainly in the interval 1975 to 1983. (In the case of California/Nevada, the catalog spanned 1947 to 1985+). The 9 regions have threshold magnitudes for the definition of large earthquakes that range from 6.5 to 8.0; earthquakes larger than threshold are targets for prediction; the method is constructed so that it depends on seismicity relative to the threshold magnitude and not to the level of absolute seismicity.

Results:

a. For three of the 9 regions, no strong earthquakes were predicted and none occurred.

b. For the remaining 6 regions, 9 earthquakes took place within the time span of the catalogs with magnitudes greater than the threshold, and 8 of these occurred within the time-space windows of the predictions.

In other words there was one failure-to-predict.

c. Three earthquakes occurred after the ends of the catalogs, and two of these were predicted. Thus there was one failure to predict an earthquake beyond the end of the catalog.

d. There were two false alarms.

e. There is one alarm in progress, for the Western United States.

Footnotes: Two of the earthquakes with magnitudes greater than or equal to 8 were used in the original data fit, and are also included among the 8 successes. Of the other 7 earthquakes (see b above), none had a magnitude greater than 8, although each was above threshold.

The total space-time reduction for the alarms is about a factor of 6. This means that there is a narrowing of the available space-time so that in 5/6 of the time and space, there are no alarms. If the earthquakes that occurred were imagined to terminate the alarms, then the improvement is about a factor of 9.

One of the 8 successes in b. above,was accomplished by a modification of the parameters in the algorithm since there were too few main shock earthquakes in the region annually to fit the normalized definition of mainshocks. The earthquake in question is a Romanian event, with larger depth of focus than the other cases. In the other cases, the algorithm was applied literally, i.e. without a posteriori adjustment of parameters.

Comments: The pattern recognition procedure is the systematization of all empirical procedures that have been developed and applied that have no or a not very significant theoretical basis. Examples in which such empiricism has been applied in postulated predictors taken singly in the last decade or two include searches for fluctuations in v_p/v_s, heights of well water, radon concentration, tilts, etc. The list is well-known. Such phenomenologies have been deemed promising or not in a series of case history studies; there is unfortunately an annoying paucity of tests of these procedures to be able to derive statistical tests of their validity, or lack thereof.

Because of its inherent, non-theoretically based phenomenology, the pattern recognition procedure is logically no better than any of the above methods taken singly. What the pattern recognition procedure does, is a)systematize the application of phenomenological searches, b) remove personal bias from the evaluation of the hypothesis under assessment, c)sort out relevant from irrelevant proposals hypothesized to be precursors, and d)take into account the possibility that a given method may no always work in every instance of a future strong earthquake. As long as we do not apply a theory of earthquakes to find out why these phenomenological methods fail on occasion, and as long as we do not have adequate numbers of case histories to provide a statistical evaluation of their success, we are restricted in our ability to assess the performance of pattern recognition, or indeed any other empirical/phenomenological procedure.

Despite the obvious advantages pattern recognition procedures have over the competing single-trait proposals, which latter may or may not be relevant, the pattern recognition procedures are strongly data-fitted. That is, they are highly parameterized and as such give the impression of being a model that is strongly tailored to the data (or catalog) at hand. Thus it should come as no surprise that the quality of the fit to the precursors of the events that were involved in the selection of the parameters has been good; to repeat, the number of successes on the fit earthquakes is expected to be high. Not very many earthquakes were used in this fitting process.

The decisive test of this method (or indeed of any method) is whether the scheme, data-fitted to one set of earthquakes, can be transferred boldly and without readjustment of the parameters to another set of earthquakes that may be set in a totally different tectonic environment. This has been done in the present case with the remarkable success scores noted above. I know of no other empirical procedure that can make this claim of success for large earthquakes.

The pattern recognition procedure raises many questions about physical mechanism as well as it does about the robustness of the procedure. The same comment can be made about any of the other, less-successful empirical procedures that also have negligible basis in theory, including those listed above. I have a long list of such questions.

Recommendation: Despite the reservations that I have attempted to summarize here, I believe that the phenomenal success rate of Pattern Recognition Procedure M8 is so outstanding on earthquakes not used in the data fitting that any prediction made under its umbrella must be taken very seriously. Clearly research must be done on why the method works and on reduction of the number of parameters in the model, but these comments must not and should not detract from the clearcut success that the Soviet scientists have had.

Leon Knopoff

Cambridge, England
November, 1986

References:

1. A.M. Gabrielov, O.E. Dmitrieva, V.I. Keilis-Borok, V.G. Kosobokov, I.V. Kuznetsov, T.A. Levshina, K.M. Mirzdev, G.M. Molchan, S. Kh. Negmatullaev, V.F. Pisarenko, A.G. Prozoroff, W. Rinehart, I.M. Rotvain, P.N. Shebalin, M.G. Shnirman and S. Yu. Schreider, Algorithm of long-term earthquake prediction, submitted, Nauka Press, Moscow (In Russian).

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4. V.I. Keilis-Borok, L. Knopoff, I.M. Rotwain and C.R. Allen, Prediction of Times of Occurrence of Strong Earthquakes in California and Nevada, submitted, Nature.

received by Sykes.
Dec 22 1986.

UNIVERSITY OF CALIFORNIA, LOS ANGELES

UCLA

sent to all NEPEC members Jan 14 1987

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INSTITUTE OF GEOPHYSICS AND PLANETARY PHYSICS
LOS ANGELES, CALIFORNIA 90024

December 16, 1986

Professor Lynn Sykes
Lamont-Doherty Geophysical Observatory
Columbia University
Palisades, N.Y. 10964

Dear Lynn,

I have recently returned from Moscow where I visited Volodya Keilis-Borok. He is extremely anxious to get a prediction he and his team have made into the machinery for evaluation of earthquake predictions. I understand that you are now the chair of the committee for such evaluations.

The prediction of his team is that an earthquake with magnitude $M \geq 7.5$ in a 7^{o} (lat.) by 9^{o} (long.) rectangle with center at 37.5^{o}N and 119.5^{o}W. The earthquake is predicted to occur in the 4-year window January 1, 1984 to January 1, 1988. The documentation is to be found in a booklet entitled Algorithms of Long-Term Earthquake Prediction by Gabrielov, Dmitrieva, Keilis-Borok, and 13 other authors, and published by CERESIS (The Regional Center of Seismology for South America) at Lima, September, 1986. The prediction is to be found on page 57 of the booklet with details on pages 51-52. If you do not have a copy, I will send you mine.

Information regarding this prediction reached the U.S. last winter/spring and part of the purpose of my trip was to assess the prediction. As indicated in the booklet, the method used in the prediction was pattern recognition of seismic histories. The pattern recognition method uses parameters that are data-fit to a small number of events. The parameterized system is then applied without further adjustment of parameters to other recent large earthquakes with huge success. It is then further applied to recent seismic histories worldwide, geographically systematically. The event in the Western U.S. then showed up. I haven't gone through the analysis in detail, but it is my guess that the Chalfont Valley earthquake (July 21, 1986) will keep the window open somewhat longer than 1-1-88.

4

I am writing a more detailed report of my discussions and exchanges in Moscow. If you want a copy I will be happy to send it to you when it is finished.

I reiterate, that K-B wants his prediction entered into the lists for consideration by your committee.

With very best regards,

Joyce Martini-Somers for/

Leon Knopoff

jms/
dictated but not read

Lamont-Doherty Geological Observatory of Columbia University | Palisades, N.Y. 10964

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Palisades New York State
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23 April 1987

Professor Leon Knopoff
Selwyn College
Cambridge University
Cambridge CB3-9DQ
United Kingdom

Dear Leon,

I distributed your letter of December 16, 1986 on the Soviet prediction of a great earthquake in California to members of the National Earthquake Prediction Evaluation Council in January 1987. I also distributed relevant portions of the Gabrielov et al. document to the Council prior to its recent meeting on April 3, 1987. I am enclosing a copy of my report as Chairman of NEPEC to Dr. Dallas Peck, the Director of the U.S. Geological Survey which includes our assessment thus far of the prediction.

As mentioned in the letter to Peck the Council hopes that the prediction can be discussed in more detail at an upcoming meeting of U.S. and Soviet scientists working on earthquake prediction in the United States in the Fall of 1987. Our hope is that Dr. Keilis-Borok will be able to attend that meeting.

I understand from a message relayed to me by your secretary at UCLA that you are preparing a write-up on the Soviet prediction. I hope you will send it to me in the near future. I will be happy to distribute it to the members of NEPEC.

Sincerely yours,

Lynn R. Sykes
Chairman, National Earthquake Prediction
Evaluation Council

LRS/llm
Encls.

cc: J. Filson
C. Shearer

Copy also sent to Professor Leon Knopoff, Institute of Geophysics and Planetary Physics, University of California at Los Angeles, Los Angeles, CA 90024

[illegible] 1987 Send copy to all NEPEC members [illegible] received Aug 1987

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August 6, 1987

Professor Lynn R. Sykes
Chair, National Earthquake Prediction
Evaluation Council

Dear Lynn,

Last mid-April I indicated that I was unable to locate a copy of the report I drafted last November of my observations and reflections on the Soviet prediction of a great earthquake in California, and that it was buried somewhere in this great mass of paper on my desk here. It has finally diffused to the near-surface and I am pleased, if only many months late, to send you a copy. Despite the fact that your request for a copy was dated April 23rd, I hope that there is still some interest in my report.

In your letter of April 23rd, you expressed the hope that the prediction could be discussed at a fall meeting of US and USSR experts I hope so too. I have not heard anything more about this meeting. Have you?

With best regards,

Leon

Leon Knopoff

Received by L. Sykes
Wed Oct 28
1987

UNION GÉODÉSIQUE ET GÉOPHYSIQUE INTERNATIONALE
INTERNATIONAL UNION OF GEODESY AND GEOPHISICS
Président *President*
V. I. Keilis-Borok

Geophysical Committee, Molodezhnaya 3, Moscow 117296, USSR
tel 1107795, tx 411478 SGC SU

Prof. L. R. Sykes
Lamont-Doherty Geological
Observatory, Columbia University,
Palisades, New York, 10964
USA

Dear Lynn:

1. It was nice to talk to you in Vancouver. I enclose the sales pitch on the IUGG business. I would be grateful for any responce from you personally and from Lamont community.

What do you think of IUGG if anything?

2. For the purpose of evaluating the IUGG activities - could you recommend a list of 5 to 15 youngsters under 33, from within or outside US, who to your personal opinion are really promising in IUGG scope.

3. Following our discussion I looked through the papers prepared for the Geneva meeting and can reproduce the conclusion precisely: M >7.5 in the whole California and adjacent Nevada, and also M >6.5 in Southern California as defined in my paper with Clarence Allen et. al.

Second one already materialized(21. 1. 1986; lat. =37. 65N; long. =118. 44 W; M = 6. 6) for the big one - the TIP ends on December 31, 1988 (year 1987 indicated in the subsequent preprint which you have is an obvious misprint; it can be easily verified since diagnosis is reproducible. L. Knopoff has programs, if you are interested to repeat the diagnosis yourself).

What would be essential is to launch forward diagnosis. Uncertainty in place can now be hopefully reduced to one or two source-dimensions, using Mendocino scenario. This may expand the possible disaster prevention measures.

Sincerely yours

Volodya

/V. I. Keilis-Borok/

5 Feb 1988

Note: Above date is incorrect for 21. 1., 1986.
Event is probably 21 July 1986 M_L = 6.6 CPA:

Lynn Sykes

To NEPEC members

Lamont-Doherty Geological Observatory | Palisades, N.Y. 10964
of Columbia University

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2 November 1987

Dr. V.I. Keilis-Borok
Geophysical Committee
Molodezhnaya 3
Moscow 117296
U.S.S.R.

Dear Volodya,

Thank you for your recent letter that arrived yesterday asking me for my opinion about directions for the International Union of Geodesy and Geophysics and relaying an update of predictions you and your group have made for earthquakes in California. Congratulations on your election as President of the IUGG; I can see already from your letter that you are determined to try and inject some vigor and life into the IUGG! I will send you my comments on the IUGG in about two weeks.

The main purpose of this letter is to follow up on the prediction that you and other members of your group have made for a large earthquake in either California or western Nevada. In this regard I am writing to you as Chairman of the U.S. National Earthquake Prediction Evaluation Council (NEPEC). My understanding is that that prediction was relayed from Secretary General Gorbachev to President Reagan at their meeting in Reykjavik, Iceland. NEPEC is very anxious to receive more details on that prediction both in writing and at a meeting, if such can be arranged, in which you and some of your colleagues would present the prediction and the data supporting it.

Dr. John Filson, Chief, Office of Earthquakes, Volcanos and Engineering of the U.S. Geological Survey has been corresponding with the Academy of Sciences of the USSR trying to set up a meeting in the United States for discussion of various aspects of earthquake prediction under the bi-lateral program in earthquake studies between our two countries. The members of NEPEC had originally hoped that your prediction could have been presented at that meeting, which was originally proposed for this Fall. Filson is now proposing to hold that meeting in March 1988 or soon thereafter. I would like to propose that NEPEC meet to hear the California prediction by you and your colleagues right after the joint Soviet-American meeting on the earthquake program. If it is not possible to have the two meetings one after the other or if the joint meeting on earthquake studies cannot take place next Spring, the members of NEPEC still want to hear a formal presentation on the California prediction. Thus, in any case, we very much hope that you and some of your colleagues would be able to present that prediction to us at a NEPEC meeting. Dr. Filson's Office would make arrangements for the travel, living and other expenses for you and your colleagues while you are in the United States. I will make sure as Chairman that there will be enough time for you and your colleagues to make a detailed presentation of your methods and of your California prediction.

Let me give you some information about NEPEC. NEPEC advises the Director of the U.S. Geological Survey about the scientific validity of predictions made for earthquakes in the United States. The Council makes recommendations to the Director of USGS but does not issue formal predictions itself. It consists of 15 members, of which half must be from outside the U.S. Geological Survey. I have been the Chairman for the past 3 years. Dr. Filson is the Vice-Chairman. We have found it very helpful to ask each person or group who makes a presentation or presents a formal prediction to NEPEC to put the prediction in writing including a summary of a few pages and copies of figures of all relevant data. We hope that you and your colleagues will send us such a write-up well in advance of the NEPEC meeting we are proposing. The minutes of that NEPEC meeting along with written material submitted will then be published as a U.S. Geological Survey Open-File Report. From past experience we have found that predictions NEPEC has considered have been changed by their authors over time. Thus we feel it is important to have a written record of the actual prediction presented to the Council and of the scientific basis for it.

Professor Leon Knopoff wrote to me in December 1986 stating his concern about the prediction that you and your colleagues had made for a large earthquake in California. I sent his letter along with the Gabrielov et al. article, Algorithms of Long-Term Earthquakes' Prediction that was published by Ceresis (Lima, Peru) in September 1986. The Council discussed Knopoff's letter and the prediction contained in the Ceresis article at our last meeting on April 1, 1987. The Council concluded that we needed more information on the prediction. Those two documents indicated that the period of the prediction would end January 1, 1988. Knopoff's letter indicated, however, that the occurrence of moderate-size earthquakes in California in 1986 might extend the prediction. In August 1987 Knopoff sent me a write-up on the prediction dated November 1986. I sent that write-up to members of NEPEC along with your latest letter. I note from it that the prediction (TIP) ends on December 31, 1988 rather than January 1.

I hope that Leon Knopoff will be able to attend a NEPEC meeting at which we propose to hear your prediction. Prior to that meeting I would like to send all of the written materials to several reviewers who are experts on pattern recognition. I hope the two of you can send me the names of experts in that field and others who would be in a position to make a wise evaluation of the methodology and scientific basis for the California prediction. Dr. Filson has asked one of his colleagues in USGS to send you an updated listing of moderate-size earthquakes in California. I hope it will be possible for you and your colleagues to use that information to update your predictions.

There are several topics related to the California prediction about which we would like to have additional information. Are there any ways in which you or others can reduce the size of the area for which the prediction is made? It is obviously of concern to us that the area is now very large and includes all of California and parts of western Nevada. How applicable is the methodology to major strike-slip or transform faults? What is the probability that the predicted event will occur in the time window specified and what is the probability that it would happen by random occurrence?

I and other members of NEPEC hope that you and your colleagues will be able to join us in the United States in the Spring of 1988 to learn about the details of your prediction of a large earthquake in California.

Sincerely yours,

Lynn R. Sykes

Lynn R. Sykes
Chairman, National Earthquake Prediction Evaluation Council

cc: Dr. John Filson Chief, Office of Earthquakes, Volcanos and Engineering
Professor Leon Knopoff
Dr. James Davis Chairman, California Earthquake Prediction Evaluation Council
Other NEPEC Members

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INSTITUTE OF GEOPHYSICS AND PLANETARY PHYSICS
LOS ANGELES, CALIFORNIA 90024

February 10, 1988

Professor Lynn Sykes
Lamont-Doherty Geological Observatory
Columbia University
Palisades, N.Y. 10964

Dear Lynn,

I enclose several reprints and one preprint that may be relevant to your consideration of the prediction in Gabrielov, et al. Item 1 is the original paper by Gelfand et al. on spatial pattern recognition. This sets forth the methodology of pattern recognition of rare events. This methodology is followed in the work described in item 4. Items 2 and 3 are the first efforts at finding temporal patterns. They concern the identification of large numbers of aftershocks of intermediate-sized earthquakes as precursors to larger events. These patterns were identified in widely separated parts of the world. Keilis-Borok tells me that Zhora Molchan has shown rigorously that this pattern is statistically significant (to appear in Computational Seismology, vol. 21).

In an effort to combine several methods of using patterns of precursory seismicity to identify upcoming occurrence of large events, we wrote preprint 4. This is also summarized in Gabrielov et al. as program CN (which stands for California-Nevada). Please note that alternative program M8 -- which was not the work of Paper 4 -- was used in Gabrielov, et al. to predict the very large earthquake that is your direct concern.

We give references to other work in Paper 4.

In my opinion the Pattern Recognition method is the epitome of phenomnology. It makes no pretenses whatsoever to understand or make use of mechanism. It is an organized search for correlations. Because the very strongest events are so rare, it is imposible to make statistical statements based on Pattern Recognition results. Thus the procedure gives a catalog of case histories. The success rate of case histories implied by pattern CN is given in Table 2 (page 26) of the preprint. For example we have a success rate of 9/9 earthquakes with $M \geq 6.4$ in Southern California with a space-time window that is open for 46% of the available amount. Similarly these figures are 4/5 with a window of 22% for Northern California.

But these results are obtained from a rather circular process of identifying these patterns from the California data set and then turning around and applying the results once again on the original data set. So we applied the procedure without change of parameters as determined from California earthquakes, blindly to 5 other areas of the world; and mainly in the Old World. We considered this to be a crucial test of our procedures. Our results were a success rate of 16 earthquakes predicted out of 20 occurrences (16/20) with windows of 25% or less of the available space-time.

I hope the enclosures are of help to you.

With all best regards,

Len

Leon Knopoff

LK:jms
Enclosures

P.S. The Magnitude cutoffs for the catalogs we used were M=2.8 for the aftershocks in pattern C1. All other magnitude thresholds were greater than this.

APPENDIX C.

REVIEW OF KEILIS-BOROK METHODOLOGY BY R. A. HAUBRICH, APRIL 6, 1988

Richard A. Haubrich
1101 San Leon Court
Solana beach, CA 92075

April 6, 1988

Professor Lynn R. Sykes
Lamont-Doherty Geological Observatory
Columbia University
Palisades, NY 10964

Dear Lynn,

The methods described as CN and M8 may be useful for prediction if they pass the tests of "direct application of the pattern recognition procedure derived for one region, to a second region without further adjustment of parameters." (1, p. 2).

Consider the tests for CN, for example (3), Table 2, Central Asia, $\bar{\Delta} = 5$, which predicted 7 out of 9 events, a result said to "give an admirably high success rate of temporal prediction" (1, p. 2). The result could occur due to chance alone with a probability range from p = .003 to p = .27 depending on the temporal probability distribution of the events. If the test was done with no a posteriori diddling, if the choice of data sets was not influenced by other unreported results and if the earthquakes are sufficiently random, then the lower range of p can be accepted and would indicate a successful test.

The test of C8 given in (2), Table 12, p. 42 predicts 8 out of 9 events in a space-time region restricted to 16%. Raw statistics says this happens at random with a probability of 3 in a million. I would make some adjustments, excluding California and the Western U.S. since these shaped CN which influenced C8. Also, take out Vrancea, which was post adjusted, and the two earthquakes used in the original data fit of C8 (1, p. 4). The lower limit of probability by chance now increases to p = .006, a result more in line with the test of CN for Central Asia.

I find the C8 test less convincing than the CN. C8 appears to be full of loose parameters. There are four traits chosen from CN. There are two threshold levels for each of 3 traits and one (different) level for the fourth. There are time windows for defining a TIP. There are different magnitude thresholds for different regions and two methods for choosing them (1, p. 2); for example, "thresholds are fixed at a given number of magnitude units below the threshold for definition of strong earthquakes."?? Changes of only a few tenths of a magnitude in these choices could wipe out the successes of the C8 test.

It's too bad seismologists can't be blinded like biologists in their tests. We could slip them a placebo or two and see how things go.

I see no reason for taking action based on this prediction.

Sincerely,

Dick

Richard A. Haubrich

References:

1. L. Knopoff, "This is a report on my discussions with V.G. Kosobokov and V.I. Keilis-Borok on the Soviet prediction of a very ...." Cambridge, England, November, 1986, 5 pages plus References.

2. A.M. Gabrielov, O.E. Dmitrieva, V.I. Keilis-Borok, ... and S. Yu. Shreider, Algorithms of Long-Term Earthquakes' Prediction, International School for Research Oriented ...., Sept., 1986, Lima, Peru.

3. V.I. Keilis-Borok, L. Knopoff, I.M. Rotwain and C.R. Allen, Intermediate-Term Prediction of Times of Occurrence of Strong Earthquakes in California and Nevada, submitted.

APPENDIX D.

REVIEW OF KEILIS-BOROK METHODOLOGY BY J. B. MINSTER, MAY 25, 1988

UNIVERSITY OF CALIFORNIA, SAN DIEGO

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INSTITUTE OF GEOPHYSICS AND
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LA JOLLA, CALIFORNIA 92093

Professor Lynn R, Sykes
Chairman, National Earthquake Prediction Evaluation Council
Lamont Doherty Geological Observatory of Columbia University
Route 9W, Palisades, New York 10964

May 25, 1988

Dear Lynn,

At long last, here is my review of the Soviet prediction of a large earthquake in California. This was much harder than I anticipated, and I have been working on it a large fraction of my time in the last month or so.

I am not yet satisfied with the review, but I have reached a point where I cannot make further progress without doing a lot of research. By far, the most time consuming task was to unravel what this prediction was based on, exactly. In spite of my considerable efforts, and cross-checks with other papers that use different notations, etc. I still have not quite resolved all the mysteries. Consequently, I thought it would be useful to try and commit to paper my interpretation of what the algorithm 'M8' consists of. It is quite possible that I am wrong in some instances, and Dr. Keilis Borok and his colleagues can use this as an opportunity to correct me and clarify these issues.

I am afraid that my comments on the method are not quite as scholarly as they could be. I have not commented on the virtues of the particular pattern recognition algorithm used in M8, relative to other approaches, such as those used in A.I. research today, because my knowledge and understanding of the latter is very superficial. I have also not sought to evaluate the results described in the various papers your sent me in the larger context of the literature pertaining to seismic gaps, and other patterns (e.g. Mogi "doughnuts", etc). This is because I perceive that the Soviet effort is in a class by itself, from the point of view of the approach they used, the scope of the analysis, and the persistence with which they have continually refined the same basic method over the years.

Instead, noting how often I thought "*I wish Volodya were here to explain this*" as I read the material for the nth time, I have focused my comments and recommendations along two main lines:

1. *Clarification of the work done to date by our Soviet colleagues*. By and large, I suspect that the most frequent sources of confusion are due to typographical errors, which can be cleared up easily. However, there are a few issues which may be fundamental to the results and which have not been considered, or at least not described in print, by the Soviet team.

2. *Ways to construct quantitative (objective?) measures of confidence in the significance of the patterns and associated predictions*. This will require additional research, and I

for one believe that joint efforts with the Soviets may be quite fruitful even in the short term. I make a few suggestions, but do not claim that I have found the best approach!

The main recommendation that I would make to the Council is that the prediction be taken as a serious and careful attempt to test a scientific methodology. We should try to understand it very well, devise stringent tests to demonstrate its worth or invalidate it, and try to help improve it. Some simple items have already been mentioned in the documents you sent me: for instance, the transmission of improved recent catalogs to the Soviet team. I suggest that we could go further, and take steps to conduct joint research. Considering the stakes, and considering the apparent preliminary successes of the approach, it seems to me that the risk of wasting our efforts and money are minimal, and well worth the goal in any case.

I would like to emphasize an area where the US can contribute rather unique expertise, namely the use of fast computers to construct statistical measures of confidence by a combination of clever theory and brute force numerical manipulations, as in the bootstrap methods developed by B. Efron and his colleagues.

As to whether further steps should be recommended, going beyond investigations within the scientific community, I believe that I personally do not understand the prediction well enough to support anything very explicit. A general awareness that this prediction has been made and is the object of scientific inquiry would probably be good, but I would not feel comfortable, as a responsible citizen, if the Government were to make substantial policy decisions based only on what I have seen to date. I would probably change my mind if some independent investigation showed that we have a *very* unusual seismicity pattern, or if the prediction is refined considerably by the seismicity of 1986-88 (although I am not sure what it would take to convince me).

As you can tell from the review, I have no objection to having my name attached to the review. In the event that I misrepresented the case, the authors will know whom to set straight!

Again, I apologize for the delay, and hope that it leaves you enough time to distribute the review before your meeting.

Sincerely yours,

Bernard

Bernard Minster

Encls.

Earthquake Prediction in the California/Nevada Region

by

Gabrielov *et al.*, (1986).

Review submitted to the

National Earthquake Prediction Evaluation Council

by

J. Bernard Minster

University of California, San Diego

Scripps Institution of Oceanography, Institute of Geophysics and Planetary Physics A-025

La Jolla California 92093

---

1. Introduction

In September, 1986, a document was prepared by A.M. Gabrielov and fifteen co-workers, reviewing in detail applications of some pattern recognition algorithms to the prediction of large earthquakes. The basic algorithms have been developed over approximately the last fifteen years by Soviet researchers and colleagues from various nations. This document, entitled *"Algorithms of Long-Term Earthquake Prediction"* – hereafter referred to as ALTEP for brevity– was published by CERESIS (Regional Center of Seismology for South America) in Lima, Peru.

It is of particular interest to US seismologists and Public Officials, because one of the algorithms leads to the identification of a *"Time of Increased Probability"* or *TIP* for the occurrence of a large earthquake at some unspecified location in California or neighboring Nevada. This *TIP* began on 01-01-1984 and is expected to end on 12-31-88.[1]

According to the wording of the second paragraph on page 20 of ALTEP, and to the data listed in Table 14, the assertion made by the authors can be stated as follows:

> Within the $7° \times 9°$ (latitude × longitude) rectangle centered at 37.5° N, 119.5° W, and during the period 01-01-84 to 12-31-88, there exists an increased probability for the occurrence of a large ($M > 7.5$) earthquake. This conclusion is based on the recognition that space-time seismicity patterns (smoothed using a 3-year sliding window) observed in that rectangle prior to 01-01-84 are similar to patterns preceding large events elsewhere. Seismicity patterns observed during the *TIP* may extend its duration beyond the nominal 5-year period ending on 12-31-88.

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[1] The period 01-01-84 to 12-31-88 corresponds to the 5-year TIP duration consistently mentioned in the report by *Gabrielov et al.* [1986]. Table 14 of the report only shows a 4-year *TIP* ending on 12-31-87. See the comments in section 3 concerning the *TIP* duration.

In this review, I shall argue that, in view of the importance of the issue, the earnestness of the prediction, and the obvious efforts of its authors to devise an objective methodology for dealing with the problem, the conclusion stated above should be dealt with as a serious scientific problem. A concerted, systematic and independent effort should be undertaken to duplicate it and measure its stability with respect to *all* adjustable parameters, *whether or not they were in fact adjusted in the original calculations reported in ALTEP*. In my view, this is what it will take to achieve a fully quantitative assessment of the algorithm and its output, which is needed if the test of the method offered by Soviet scientists is to result in public policy decisions.

Pattern recognition algorithms are notoriously difficult to express in plain language; they are most easily cast in terms of computerized rules. The particular algorithm used in this instance is no exception, and it takes much detective work to extract a statement of the exact sequence of steps leading to the prediction from the ALTEP document itself. Since trying to determine exactly what the M8 algorithm consists of is the task that consumed the most time in my review, I shall attempt to summarize my understanding of it in the next section. As will be seen, I do not believe that the presentation in ALTEP is free from ambiguities, and it is essential to understand exactly what was done before decisions are made as to what to do about the prediction.

Following this, I shall offer several comments on the general approach, the particular application at hand, and the ALTEP report, and summarize in my own words several issues and *caveats* raised by others. To be fair, I should state at the outset that many of these concerns have been recognized by others as well as by the authors of ALTEP themselves, and are in fact mentioned in the body of the report.

Finally, I shall venture a suite of recommendations concerning a possible way to proceed further toward an improved quantitative evaluation.

2. Summary of the prediction

The algorithm used for the prediction at hand is an application of a general approach to pattern recognition and analysis described by *Gelfand et al.* [1976] and subsequently extended and adapted to earthquake sequences in a number of studies. Rather than review the development of this approach, I shall try to state how the specific instantiation of the method known as *algorithm M8* was devised, review the *a posteriori* tests described in ALTEP, and then state explicitly how it was applied in California.

2.1 *Algorithm M8: A digest*

- M8 was constructed so as to generate *TIPs* for worldwide earthquakes of magnitude 8 or greater in the period 1965-1982. This algorithm is one of six variations of a basic algorithm seeking to identify seismicity patterns which precede large earthquakes.
- Out of the 9 earthquakes with $M \geq 8$ found in the world-wide catalog for that period, three are located in regions where the catalog is incomplete at the magnitude 4 threshold, and were therefore rejected. The learning population for M8 was thus reduced to 6 events.

- For each event, a rectangular region $\mathcal{U}$ of nominal dimension 12 geocentric degrees was centered on the epicenter, and the seismicity within this rectangle was characterized by the seven functions of time labelled f1 through f7 below[2], belonging to four groups:

Group 1: Measures of seismic activity, of the form $N(t \mid \underline{M}(a), s)$, where N is the number of main shocks[3] with magnitude greater than $\underline{M}(a)$ recorded in $\mathcal{U}$ during the time interval [t-s, t] and $\underline{M}(a)$ is the magnitude threshold such that the *average annual number* of events with magnitude greater than $\underline{M}(a)$ in $\mathcal{U}$ is equal to a [4]. Thus the expected value of N would appear to be the product $s \cdot a$. Two such functions were selected for M8, namely[5]:

$$N(t \mid \underline{M}(10), 6) \tag{f1}$$
$$N(t \mid \underline{M}(20), 6) \tag{f2}$$

Group 2: Measures of deviation in the seismic activity level from the long-term trend, defined by:

$$L(t \mid \underline{M}(a), s) = N(t \mid \underline{M}(a), t\text{-}t_0) - N(t \mid \underline{M}(a), t\text{-}t_0\text{-}s)\,\frac{t\text{-}t_0}{t\text{-}t_0\text{-}s}$$

where t_0 is the beginning of the catalog[6]. Again two such functions were selected for M8, namely:

$$L(t \mid \underline{M}(10), 6) \tag{f3}$$
$$L(t \mid \underline{M}(20), 6) \tag{f4}$$

Group 3: Measures of the spatial concentration of the seismicity.

---

[2] These functions were not introduced for the first time in M8; they have been selected from a more extensive list of functions used in other algorithms, notably the *CN* (California-Nevada) algorithm. This particular selection (f1 through f7) is thought to be especially robust to shortcomings (e.g. incompleteness) of the available earthquake catalogs.

[3] The separation of main shocks from aftershock sequences is performed according to a standard procedure described in the literature. I do not believe this is an issue here.

[4] The notation $\underline{M}(a)$ is used on page 19 of ALTEP, and is used thereafter in descriptions of tests of M8. I could not find a definition for it anywhere in ALTEP, but inferred its meaning from the discussion of normalization on page 12.

[5] All numerical values to time parameters are in units of years, except where otherwise noted.

[6] This definition, taken from equation (6) of ALTEP, is somewhat puzzling. It compares the cumulative number of main shocks from the beginning of the catalog to the present, to the number that would be predicted by simple extrapolation of the cumulative number from the beginning of the catalog to s years ago. I would have compared instead the number of main shocks in the last s years $N(t \mid \underline{M}(a), s)$ to the predicted number for these same s years, based on extrapolation of the first $t\text{-}s\text{-}t_0$ years. In other words, I would have computed $N(t \mid \underline{M}(a), s) - N(t \mid \underline{M}(a), t\text{-}t_0\text{-}s) \cdot s \cdot (t\text{-}t_0\text{-}s)^{-1}$. It seems that this would be a more sensitive comparison.

$$Z(t \mid \underline{M}, \overline{M}, s, \alpha, \beta) = \Sigma(t \mid \underline{M}, \overline{M}, s, \alpha, \beta) \left[N(t \mid \underline{M}, s) - N(t \mid \overline{M}, s)\right]^{-2/3}$$

where the numerator is an "energy-weighted" sum of events, defined by

$$\Sigma(t \mid \underline{M}, \overline{M}, s, \alpha, \beta) = \sum_i 10^{\beta[M_i - \alpha]}$$

Here the sum is taken over all events with magnitude between the lower threshold $\underline{M}$ and the upper threshold $\overline{M}$. The reference magnitude α is arbitrary and is chosen for convenience.

According to equation 10 (page 11 of ALTEP), the coefficient β is taken to be B/3, where B=1.37 (see page 21) is the slope of the magnitude-energy relation $\log_{10}E = A + BM$. This choice of β is intended to make each term in the summation proportional to the linear dimension of the fault. The function Z then compares the average linear source dimension to the average distance[7] between events within $\mathcal{U}$ during the interval [t-s, t] and is therefore some sort of measure of the local concentration of fractures in that space-time neighborhood. However, the functions selected for M8 are listed on page 19 of ALTEP show β to be 0.91, which is in fact 2B/3 and not B/3. There are therefore internal contradictions in ALTEP, and it may be that the function S defined by equation (9) on page 11 of ALTEP is actually used in M8. (S measures the average area of rupture surfaces). I could not resolve the contradiction with the documents in my possession. In any case, the functions selected for M8 listed on page 19 of ALTEP are:

$$Z(t \mid \underline{M}(10), M_0{-}0.2, 6, 5, 0.91) \qquad \text{(f5)}$$

$$Z(t \mid \underline{M}(20), M_0{-}0.2, 6, 5, 0.91) \qquad \text{(f6)}$$

A new threshold M_0 has been introduced; the algorithm seeks to predict (via *TIPs*) events with magnitude greater than M_0. This threshold depends on geography as we will see shortly.

Group 4: A measure of the number of aftershocks of predecessor earthquakes, defined by:

$$b(t \mid \underline{M}, \overline{M}, s, M_a, e) = \max_{\{i\}} b_i(M_a, e)$$

where the maximum is taken over the main shocks in $\mathcal{U}$ in the time interval [t-s,t], with magnitudes between the thresholds $\underline{M}$ and $\overline{M}$. The function b_i is the number of aftershocks with magnitude greater than M_a for the ith main shock, recorded within a time window of duration e after the main shock. The specific function chosen for M8 is:

---

[7] The exponent -2/3 in the formula is evidently a combination: -1 to estimate the average source dimension (i.e. divide the sum by the number of events), and 1/3 contributed by the mean distance between events in the neighborhood $\mathcal{U}$. This appears to imply that the distribution of events is analyzed in a three dimensional volume, and not as an areal distribution, which does not seem consistent with the analysis of shallow events over large (10 degrees) areas. The only reference which would seem to explain whether my interpretation is correct is in Russian (reference [7]).

$$b(t \mid M\text{–}2, M_0\text{-}0.2, 1, M_0\text{-}4, 30 \text{ days}) \qquad (f7)$$

Again, the magnitude thresholds appearing in the definition depend on M_0 which we describe next.

- The threshold M_0 that appears in (f5), (f6), and (f7) is fundamental in the sense that it controls explicitly the actual application of the algorithm. It was introduced so as to permit convenient scaling of the various magnitude thresholds used in the algorithm to match the earthquake population on a more regionalized basis. The fundamental idea is to predict the strongest events in a given region, but this is clearly a geographically variable concept. In other words we should consider M_0 to be a function of latitude ϕ and longitude λ. On page 20 of ALTEP, a statement is made that one or two earthquakes with magnitude greater than $M_0(\phi, \lambda)$ are produced collectively each year by all the areas studied. Locally, $M_0(\phi, \lambda)$ must clearly be related to the size of the rectangles used to analyze the seismicity. This relation is taken to be:

$$l = \exp (M_0 - c) + 2\varepsilon$$

where l is the linear dimension of the rectangle in geocentric degrees, the offset 2ε is taken to be 1°, and the constant c is adjusted to yield $l = 12°$ for $M_0 = 8$. For the western US, the rectangles were chosen to span 7° of latitude and 9° of longitude[8], corresponding to $M_0 = 7.5$.

- The seven functions f1 through f7 are collected as the components of a time-dependent vector P_u (t), and this vector is calculated as a function of time with a half-year sampling rate. The hypothesis is then made that this vector-valued time series behaves abnormally prior to strong earthquakes, and that, with the definitions chosen, this behavior will show up as abnormally *large* values of the components. The phrase "abnormally large" is defined as follows:

  In the learning phase, are defined to be abnormally large the values of f1 through f6 that exceed the 90th percentile of all values calculated for the rectangles surrounding the strong earthquakes of the learning population during the intervals between the beginning of the catalog and these earthquakes. For f7 the criterion is relaxed, and abnormally large values are those that exceed the 75th percentile.

  When exercising the algorithm, *the same values of the thresholds are used,* and geographical variations in the character of the seismicity are assumed to be completely described by the selection of M_0 and by the scaling (normalization) rules.

- We now define two additional time series h_u (t) and g_u (t), as follows:

  h_u (t) is the number of components of P_u (t) that have at least one abnormally large value in the interval [t-3, t]

  g_u (t) is the number of groups of components of P_u (t) that have at least one abnormally large member in the interval [t-3, t]

---

[8] On page 51 of ALTEP, a statement is made suggesting that these dimensions were selected so as to be comparable to the "width to the seismic zone". A more precise statement would be helpful.

A *TIP* is declared in $\mathcal{U}$ for the interval $[t_k, t_k + 5]$ if for two consecutive half-year samples at times t_{k-1} and t_k, we have:

$$\boxed{h_{\mathcal{U}}(t) \geq 6 \text{ and } g_{\mathcal{U}}(t) = 4}$$

In other words, at least 6 of the 7 components show two successive large values over a three-year sliding window, and all four groups are represented[9].

This completes the description of algorithm M8. It must be emphasized that the numerical parameters that appear above have been data-fitted to the seismicity preceding six large earthquakes ($M \geq 8$). Details of the data-fitting procedure are not given in ALTEP, and this is clearly an area where more information would be important[10].

2.2 *Tests of M8*

Following the data-fitting procedure, M8 was applied to 9 regions for which available earthquake catalogs were deemed to be sufficiently complete. These regions are listed in Table 12, page 42 of ALTEP. In most regions, the test was run for the period 1975-1983, except in southern California, where the catalog used covers the period 1947-1985.6[11].

The results are summarized in Table 12 (ALTEP page 42) and Table 14 (ALTEP page 56) with the following results[12]:

a. In three of the nine regions, no strong earthquakes were predicted, and none occurred. Although it is viewed as encouraging, this result is considered a weak test of the algorithm (see, for example, the top of page 47 of ALTEP)

b. In the other 6 regions, 9 earthquakes with $M \geq M_0(\phi, \lambda)$ are found in the catalogs, and 8 of these are preceded by *TIPs*. The ninth one is a "failure to predict".[13]

c. 2 out of 3 events actually recorded since the end of the catalog were preceded by *TIPs*; The third one is a "failure to predict".

---

[9] This means that the function f7 (number of aftershocks) *must* be abnormally large for a *TIP* to be declared, since it is alone in its group.

[10] See comments in section 3.

[11] A special run was made for southern California, in order to demonstrate that the algorithm would have predicted the 1952, M = 7.7 Kern County earthquake.

[12] The description of the results was given by Prof. Knopoff in his report, with minor differences. Note that it is important to read the detailed discussions for each of the *TIPs* listed in Tables 12 and 14, since *TIPs* declared in adjacent rectangles can be terminated simultaneously by a single earthquake, but this should count as a single success, with no false alarms.

[13] Prof. Knopoff states that 2 of the 8 successes have magnitudes greater than 8 and belong to the training data used to data-fit the algorithm. I cannot substantiate this from the numbers in Table 14, or from the detailed descriptions in Appendix A of ALTEP. As far as I can tell, the only M > 8 earthquake described in this appendix is the 09-19-85 event in central America, which took place after the end of the catalog, and was in fact preceded by a *TIP*.

d. There were two false alarms[14],[15]

e. There is one alarm in progress for the western U.S., and another one in the Caucasus.

The overall space-time volume of the *TIPs* generated in these tests is 16% of the volume available, and if *TIPs* are truncated when an earthquake actually occurs, then the percentage drops to 11%, or one ninth of the overall space and time considered for analysis. By any standard, this is a remarkable reduction.

2.3 The western US alarm

The ongoing alarm in the western US is described on pages 51-52 of ALTEP. The western US and Baja California seismic zone was scanned by eight overlapping rectangles of dimensions 7° in latitude and 9° in longitude, corresponding to $M_0 = 7.5$.

The alarm is triggered for the rectangle centered at 37.5° N, 119.5° W, but not for the neighboring rectangles which overlap with it. The center of the rectangle is located near Yosemite National Park and extends to areas of dense population in San Francisco and Los Angeles. Based on the correspondence from Prof. Keilis Borok, one gets the impression that this area should really be extended to encompass all of California and neighboring Nevada; this may be the result of more recent calculations than those depicted in ALTEP.

ALTEP does not give any details as to whether all traits f1 through f7 contributed to the trigger or whether one of them failed. It is certain, however, from the definition of M8, that the function f7 exceeded the 75th percentile defined from the learning population.

The authors of ALTEP applied M8 to the southern California catalog and showed that the algorithm generates a *TIP* before the 1952 Kern County earthquake. This, they argue, is supportive of the validity of the algorithm in southern California. As further support of the applicability of the method, we may note that, in their analysis of intermediate-term prediction in California, *Keilis Borok et al.* conclude that "large changes in seismicity, large numbers of aftershocks, and an increase in the dimensions of the sources are important"; these factors are those used in M8 *via* the functions f3 through f7.

3. Comments

This section contains a variety of comments on ALTEP and the prediction for the western US. The reader is urged to examine in some detail the comments made by Prof. Knopoff in his analysis of ALTEP, some of which are incorporated with my own below.

3.1 General comments

Perhaps the first comment which comes to mind is that the report is deceptively simple. At first reading, it seems reasonably straightforward, provided that the reader already has achieved fairly good familiarity with the general techniques involved, and is generally aware of other comparable publications. One has the impression that it should be easy to state in a few words exactly what features in the seismicity of California caused the *TIP*.

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[14] Several *TIPs* listed in ALTEP only extend to 01-01-88. It should be possible to re-examine them so as to determine if they turned into successes or failures, or had to be extended. See however, the discussion of *TIP* durations in section 3.

[15] See however, the discussion of *TIP* durations in section 3, since the *TIPs* for the Caucasus, listed in Table 14, are only 4 years long, in contradiction with the specifications of M8.

In fact, it turns out that the report is very difficult to read, and to understand at a level of detail sufficient to permit easy duplication of the results. As I mentioned earlier, this is not altogether surprising, since the algorithm is best cast in the form of a computer program. However, my claim is that ALTEP by itself constitute insufficient information to write a computer program that could duplicate the results. This comment should not be perceived as a severe criticism of ALTEP *per se*, insofar as a fully self-contained documentation would probably result in an enormous volume, and be unreadable.

On the other hand, I was distinctly unhappy with the presentation of the results, particularly with figures such as Fig 4. or Fig 7. It is extremely difficult to relate the dots marking the epicenters of strong earthquakes on the map with the arrows showing how these earthquakes are associated with *TIPs* on the time-lines. Further, it is not easy to see which earthquakes belong to which rectangles, and to gauge whether even a small change in the center would modify this association. I believe that the presentation would benefit much by devising a different graphical representation of the results.

Finally, there remains a number of points which require clarification; As pointed out in some of the footnotes to this review, I was not always able to resolve conflicting interpretations of the equations. It is likely that some confusion is attributable to typographical errors, and that relatively simple corrections would help greatly.

3.2 TIP durations

The period 01-01-84 to 12-31-88 for the western US alarm corresponds to the 5-year *TIP* duration consistently mentioned in ALTEP. According to the description of algorithm M8 on pages 18 and following, *TIPs* are declared for a duration of 5 years each, and according to page 19, lines 14–18, "a *TIP* is not terminated by a strong earthquake" in this algorithm.

In this light, Table 14 of the report is very confusing: out of 18 *TIPs* listed in the table, 9 are assigned a duration of 5 years as per the definition of algorithm M8, 8 have a duration of only 4 years, including the two *TIPs* listed for California, and one (lat. 44°, long. 80°] has a duration of 4.5 years. I have not been able to find anywhere in the report a reason for declaring a short TIP under the rules of algorithm M8.

I interpret ALTEP as follows: the *TIP* for California is indeed supposed to last until 12-31-88 and not 12-31-87; then there is no need to invoke the Chalfant Valley earthquake of 07-21-1986 to extend the *TIP*, the suggestion made by Prof. Knopoff in his analysis of the prediction notwithstanding.

It would be interesting to check whether the *TIP* should be extended *beyond* 12-31-88 because of recent activity in California, however. In fact, in a letter written in october 1987, Prof. Keilis Borok states unequivocally that this earthquake is associated with a *TIP* identified by the "CN" algorithm, applied to the CIT and NOAA catalogs for California and Nevada, with $M_0 = 6.5$. This association was only made very tentatively in the earlier paper by *Keilis Borok et al.*, and this serves to emphasize the need to update the analysis fairly frequently.

Further, the drawings accompanying the tests of M8 described in Chapter III do show *TIPs* to be terminated when a large earthquake occurs, and this is made explicit in the captions (e.g. Fig 25). Since the *TIPs* declared by M8 are not interrupted by earthquakes, the figures apparently show what is labelled in Table 14 (last column) as the "period of expectation". This is defined as the time elapsed between the beginning of the *TIP* and the earthquake or the end or the *TIP*, whichever comes first. The confusion can be cleared by either redesigning the figures, or at least fixing the captions.

3.3 *Learning (data fitting) procedure*

In ALPTEP, page 19, 16 lines from the bottom, a statement is made that 4 out of 6 events in the learning population were covered by *TIPs*. This is a rather curious fact, given the rather large number of adjustable parameters.

I would have expected better success. Did the designers of M8 accept a poorer fit to the learning population in exchange for a much smaller total space-time volume covered by *TIPs* ? If so, the trade-off should be described in quantitative detail.

3.4 *Statistics and stability of the results*

The weakest point of the prediction, one mentioned in ALTEP on page 5 and elsewhere, and raised in a different form by Prof. Knopoff, is that we are dealing with small samples, and therefore it is difficult to generate statistical tests of the method.

In particular, the large number of parameters which can be adjusted in the learning phase are in fact strongly data-fitted, and one should expect better than random performance of the algorithm simply because the learning data and the test data are not very independent. Certainly, the fact that one can transfer the algorithm to other regions by simply scaling the parameters according to a self-similar earthquake population is encouraging. However, one does not know just how encouraged we should be[16].

For example, the basic threshold M_0 is chosen for each region. I believe that, at least in some cases, the ratio of successes to failures must depend on M_0. To take an admittedly extreme example, one can always take M_0 so large that no earthquake is predicted and none is observed, but that is not a useful test. The algorithm is designed to predict only the strongest shocks, but that is in some sense a somewhat elastic concept. The authors of ALTEP have tried to select M_0 based on the dimensions of the scanning rectangles, and in so doing, to remove a source of arbitrariness; but since these dimensions can be themselves adjusted, we have a "chicken and egg" ambiguity. In addition, as noted in a couple of places in ALTEP, it is not always possible to place the scanning rectangles in such a way as to avoid splitting a seismic zone arbitrarily, in a geologically meaningless manner.

To take an explicit instance, the western US seismic zone is dominated by the San Andreas system, which is a long, narrow strike slip system. The diffuse seismicity extending eastward across the Basin and Range province from the Sierra front to the Wasatch front is considerably less intense, although it is quite capable of producing vary large events. In this case, it is by no means clear that one can choose unambiguously the dimensions of the scanning rectangles. Should they be elongated in the strike direction? Should we in fact look for anisotropic patterns? In southern California, should we take the rectangles to be equidimensional for small M_0 –say, 6.5– but elongated for large M_0 –say, 7.5–?

None of these questions are addressed directly in ALTEP, and it seems that they should be. I am reasonably certain that they have occurred to our Soviet colleagues, and would very much like to hear their views on the subject. Similarly, I would like to get a better feeling for how carefully all the scaling laws used to tie magnitudes to spatial dimensions (and to select the thresholds appearing in f1–f7) had to be tuned in order to achieve success. As it stands, there is still too much black magic for comfort in the descriptions of M8.

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[16] From the correspondence, it appear that Soviet statisticians have recently made some progress in terms of analyzing the statistical significance of the results. I am not familiar with this work.

On the other hand, if the seismicity is indeed sufficiently self-similar, then the same algorithm should work at different thresholds, provided that all times, distances, and magnitudes are scaled appropriately, and I must admit that to a fair extent, the results reported in ALTEP are supportive of the idea.

For a number of regions, ALTEP reports on attempts to check the validity and robustness of the results with respect to some of the parameters. I would have liked to see that done for the western US.

In fact, I believe that it would be very important to check the stability of the results with respect to small variations of *all* the parameters in the algorithm. This includes not only M_0, but also the percentiles used to define *TIPs*, the constants appearing in various equations, the center location and the dimensions of the scanning rectangles, *and the catalog as well.*

For example, it would be easy to check how many successes and failures would be achieved by M8 when applied to 10^3 differents randomized versions of the catalog, and it may even be possible to work out theoretical measures of uncertainty based on approaches similar to the *jacknife* or the *bootstrap* methods.

Furthermore, similar tests could be conducted on the properties of the time series f1 through f7. I am surprised that these are not even plotted in ALTEP.

3.5 Pattern recognition and physics

In his remarks, Prof. Knopoff makes the point that, in the absence of a "theory of earthquakes", we will not know *why* the method works or fails. This would seem to be inherent to any "blind" method that treats the data without prejudice of the underlying physical phenomena. I personally think that the place of pattern recognition in science is to recognize the phenomena, but that a true predictive capability will not be ours until we begin to get a handle on the physical aspects.

A case in point is the long-range interaction and long-range aftershocks described on Page 12 of ALTEP. The physical models of lithospheric mechanics examined to date do not seem to be able to account easily for such interactions. So either the statistics of earthquakes in space and time support coincidences that fool the algorithm, or there is a chance that pattern recognition will help us understand the physics better. Based on the work of *Kagan and Knopoff* at UCLA, and others, it would seem that many of the purely statistical aspects can be characterized fairly well by now, and we can begin to test for the frequency of occurrence of the patterns revealed by M8, using synthetic catalogs.

Nevertheless, I do not subscribe completely to the idea that algorithms CN and M8 are free from any prejudice about the physics. In fact, as noted in ALTEP, the regionalization is cognizant of geological boundaries, which are in turn controlled by the physics of crustal deformation. Further, the types of traits tested were obviously not chosen at random, but were selected by seismologists, who had a physical model of the earthquake process in mind. Even crude measures of event size, such as the various magnitudes, have substantial physical contents. Since some implicit notion of the physical process is buried within the choice of parameterization itself, one must ask to what extent this parameterization influences the outcome[17].

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[17] For example, consider applying the algorithm to weather prediction. Even if the application is made by deliberately ignoring what we know of the physics, it seems likely that the method would uncover physical relationships known to us and converge with what constitutes conventional wisdom about the physics of the atmosphere and the oceans. However, to what extent would that be controlled by the specific choice of parameterization, which will surely have implicit knowledge of the physics built into it?

3.6 *Testing the algorithm*

Whether we find the algorithm to be justifiable on statistical grounds and satisfactory on physical grounds or not, the fact remains that the only true test of success is to analyze the catalogs and really *try* to predict future strong shocks. Retrospective tests, as well as comparisons of the results obtained from the CN and M8 algorithms in southern California, leave too many doubts: the event populations are not sufficiently independent, the samples are small, and there is a good probability that the apparent success of the algorithm is merely due to chance.

In this respect, I find myself in full agreement with the philosophy adopted by Soviet researchers, and appreciate the caveats given at the bottom of page 6 of ALTEP. Such predictions simply cannot be kept "under wraps", but have to be made available to the scientific community for evaluation as soon as they are issued. This evaluation has to be approached as a scientific research problem, so that we learn from failures as well as successes. I find the way the western US *TIP* has been handled by the authors of ALTEP to be sound and acceptable, provided that further joint evaluation efforts are facilitated.

On the other hand, it is inconceivable that such a scientific exercise should be conducted with no attention paid to national and local issues of public safety and public policy. The problem has been addressed in several reports of the National Academy of Sciences, and I will simply refer the reader to these studies.

4. Recommendations

My primary recommendation is that the *TIP* which triggered the western US alarm be analyzed as a scientific observation to be duplicated in an independent effort. My concern is not with the correctness of the M8 calculations themselves[18], but with the establishment of a mechanism through which information that is hidden in M8, implied in the choice of parameters, and incoporated in the form of value judgments, can be communicated. In the development of a complex algorithm of this sort, much unspoken knowledge is brought to bear on the problem. Such knowledge arises from long experience with the problem[19], is introduced in a very subtle way whenever a human judgment or selection of parameter is made, and is not easily mapped into words or into codable rules.

In a way, we are faced here with a problem of knowledge transfer not unlike the situation encountered in the development of expert systems. Fortunately, I think that in this instance, a fast and straightforward approach to the problem will go a long way toward answering the questions raised earlier. As an possible example of how we could proceed, I propose the following steps:

1. A Soviet team intimately familiar with all aspects of the prediction should be invited to visit the US, bringing the most up-to-date set of programs on which M8 is based, and the catalogs. These programs and catalogs should be installed on a conveniently accessible computer.

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[18] I obviously do not mean that the algorithm should be coded afresh. There is not reason to doubt that the calculations are performed correctly, and there are other ways to test codes (e.g. with synthetic data sets). The exact M8 calculation can be repeated at UCLA since the codes are available there, but this would not be particularly informative.

[19] The Soviet team who developed M8 has been working on pattern recognition algorithms in this class for 15 years or more.

2. With these tools a team of US scientists could be led by their Soviet colleagues through the step-by-step procedure followed to construct the M8 prediction.

    - Correction of the catalogs (removal of errors). Comparison of the corrected Soviet version of the catalog with the US version.
    - Extraction of the main shocks (elimination of aftershocks)
    - Selection of the traits used in M8
    - Learning phase (data fitting)
    - Selection and justification of the space-time-magnitude scaling rules
    - Application (test) of the algorithm to selected areas

3. This should be followed by systematic testing of the stability of the results with respect to reasonable or justifiable variations of all parameters that were data-fitted.
4. The entire procedure should then be repeated, using a suite of randomized versions of the original catalogs. I believe that it is important to test at several levels:

    - First apply M8, unchanged, to a series of randomized (in space and time) versions of the original catalogs[20], and measure its performance. This should show to what extent the patterns on which M8 is based could occur in random populations.
    - Second, after randomization of the catalogs, repeat the learning phase, and investigate whether a new version of the algorithm can be constructed which exhibits a comparable level of success. If the remarkable success of M8 cannot be repeated, then we have grounds to believe that we could identify the physical processes at work.
    - In either case, attempt should be made to develop numerical measures of uncertainty and stability (e.g. *jacknife* or *bootstrap* estimates). This is likely to require the involvement of theoretical statisticians.

The preceding steps are probably all that is needed to achieve a much better understanding of what makes M8 work so well. It will not help understand *why*. I believe that a longer view would be to supplement these activities which research in at least two areas:

5. We should investigate ways to refine the analytical procedures for pattern recognition, possibly using synthetic catalogs with known statistical properties to test the efficiency of the learning procedure.
6. We should certainly couple this activity with ongoing investigations of the underlying physical mechanisms.

I am convinced that a concerted effort of this nature is going to be required in order to achieve a quantitative evaluation of predictions derived from M8 or similar algorithms. In particular, scientific advice to policy making bodies depends critically on having some sort of a quantitative measure of confidence in the results. In the present case, we might adopt

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[20] This is easier said than done ... considerable thought ought to be given to the randomization procedure, particularly when it comes to spatial patterns.

as a measure of reliability the impressive success rate of retrospective tests of M8 given in ALTEP. However, there are good reasons to suspect that this is a biased measure. To what extent this bias makes the M8 algorithm appear better than it really is, is anybody's guess at the present time.

APPENDIX E.

COMMENTS SUBMITTED TO NEPEC BY W. L. ELLSWORTH REGARDING A PRIORI PROBABILITY OF MAJOR EARTHQUAKES IN CALIFORNIA-NEVADA

Some Remarks on the *a priori* Probability of Major Earthquakes in California and Nevada

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The seismicity of the greater California and Nevada region during the period from 1812 to 1987 has been examined in an attempt to place some constraints on the *a priori* expectation of future large magnitude earthquakes. The catalogs used in the compilation include those of Toppozada et al. (1981) and Gutenberg and Richter (1954), as well as the bulletins of U.C. Berkeley and Caltech. During the 175-year period considered, at least 4 and possibly as many as 7 earthquakes M≥7½ occurred in the region. These events are listed in Table 1 and appear in Figure 1 along with all events of M≥6½. Coincidentally, all of the candidate events fall within the space window with an active TIP identified by Keilis-Borok, et al. (1986).

The assembled catalog of events M≥7½ is probably complete within the boxed region from the time of the 1857 earthquake onward, but may be missing events during earlier times. The magnitude of the 1812 earthquake (as well as it's location) is also uncertain, and so attempts to estimate the underlying event rate, λ, must consider the effects of both an inaccurate and an incomplete record. Despite these obvious difficulties, some preliminary conclusions may be drawn.

To estimate λ, we shall consider only the closed interevent intervals. The lowest rate consistent with Table 1 is obtained by accepting only the intervals formed by the 1812, 1857, 1872, 1906 and 1952 events. The latter four events are the only events with magnitudes clearly ≥7½. The 1812 event is accepted as a M 7½ so that the long interval between 1812 and 1857 is included in the calcuation. In this case λ=0.029/a. The highest rate is given by the five intervals between 1857 and 1952. Here, we accept all events in this interval as M 7½ events, but reject the 1812 event and obtain λ=0.052/a.

An alternative method for estimating λ is to use the frequency-magnitude relation log N = a - bM. Within the entire region and for M≥6½, b=1. As there are 24 events in the box in Figure 1, a=5.64 and so λ=0.014/a. It is noted that the use of events only within the box may produce too low an estimate of λ, and in this case corresponds to an expected number of only 2.5 events M≥7½ since 1812. If all of the seismicity appearing in Figure 1 is used, we find that λ=0.030/a. These values of λ are included only for the sake of completeness and will not be considered further.

If the seismicity in this region may be represented by a Poisson process, the probability of finding one or more events in any interval of t years is

$$P=1-e^{-\lambda t}$$

For a time period of 5 years duration the probability of at least one event is thus between P=0.13 (λ=0.029/a) and P=0.23 (λ=0.052/a). According to this model, the chance of having had at least one event since 1952 is in the range between P=0.64 and 0.85.

In their study of the prediction algorithim M8, Keilis-Borok, et al. (1986) found that 5/6ths of the events (10 of 12) occurred during periods of TIP's. They also found that the TIP's occupied 1/6th of the total space-time volume of the catalogs. Thus, it appears that the seismicity rate is 5 times higher than the background rate during TIP's and only 1/5th as great at other times.

Using the value for λ derived above we may estimate the probability gain of the M8 algorithm for the box in Figure 1 using the method introduced by Aki (1981). During a 5-year interval with λ taken as 5 times the background rate, the probability of one or more events is between P=0.52 for λ=0.145 and P=0.73 for λ=0.26. Compared to the *a priori* probability obtained above, these values represent probability gains of from 4.0 to 3.1, respectively.

References

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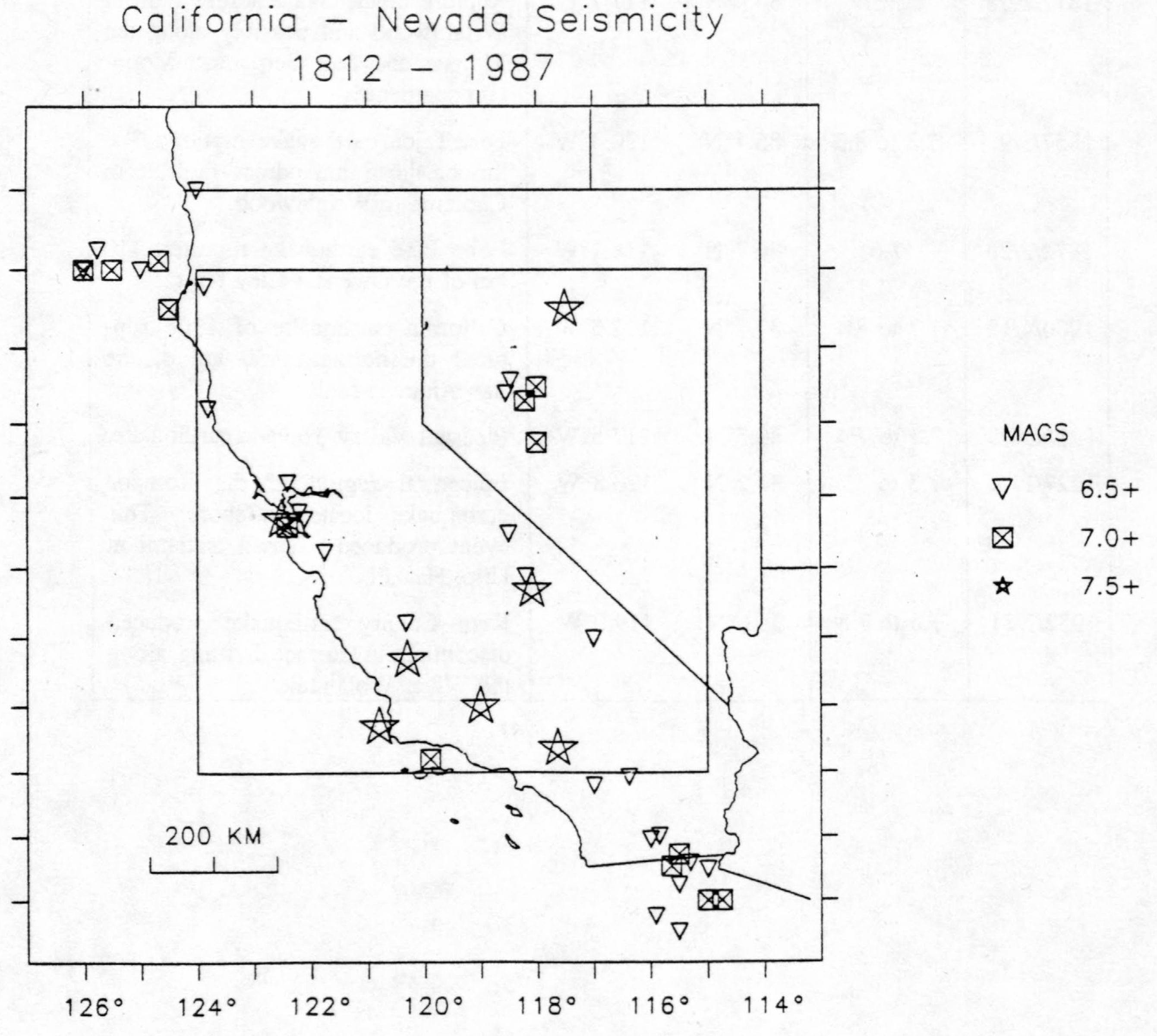
Toppozada, T. R., C. R. Real, and D. L. Parke, 1981, Preparation of isoseismal maps and summaries of reported effects for pre-1900 California earthquakes: California Division of Mines and Geology Open File Report 81-11 SAC, 182 p.

Table 1.

Major California and Nevada Earthquakes 1812 - 1987				
Date	Magnitude	Latitude	Longitude	Remarks
1812/12/8	7½?	34.4°N	117.7°W	Rupture of the San Andreas fault at Wrightwood and possibly along the Mojave and San Bernadino Mountain segments.
1857/1/9	7.8 to 8.3	35.7°N	120.3°W	Fort Tejon earthquake ruptured 300 km of the San Andreas fault from Cholame to Wrightwood.
1872/3/26	7.6	36.7°N	118.1°W	Lone Pine earthquake ruptured 110 km of the Owens Valley fault.
1906/4/18	7.7 to 8¼	37.7°N	122.5°W	California earthquake of 1906 ruptured the northern 450 km of the San Andreas fault.
1915/10/3	7.3 to 7¾	40.5°N	117.5°W	Pleasant Valley, Nevada earthquake.
1927/11/4	7.3 to 7.5	34.7°N	120.8°W	Epicentral region of the Lompoc earthquake located offshore. This event produced a small tsunami at Hilo, Hawaii.
1952/7/21	7.5 to 7.8	35.0°N	119.0°W	Kern County earthquake produced discontinuous surface faulting along the White Wolf fault.

Figure 1.



California – Nevada Seismicity
1812 – 1987
42°
41°
40°
39°
38°
37°
36°
35°
34°
33°
32°
200 KM
126°
124°
122°
120°
118°
116°
114°
MAGS
6.5+
7.0+
7.5+

APPPENDIX F.

LETTER FROM L. SYKES TO D. PECK, REGARDING EVALUATION AND RECOMMENDATIONS OF NEPEC WITH REGARD TO KEILIS-BOROK PREDICTION

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13 July 1988

Dr. Dallas Peck
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Dear Dallas,

The National Earthquake Prediction Evaluation Council (NEPEC) met in Reston, Virginia on June 6 & 7, 1988 to hear a presentation and to evaluate the Soviet prediction of a large earthquake in California. Academician V.I. Keilis-Borok made an extensive presentation on the Soviet prediction and discussed it along with relevant methodologies on June 6. More specific aspects of the prediction, copies of reprints and preprints and various letters related to this matter were distributed to NEPEC members, and to four external reviewers. Those materials will be published in the forthcoming Open-File Report containing the minutes of the June meeting.

Statement of the Soviet Prediction, Methodology, and Application to California and other Areas

NEPEC was first informed that various Soviet investigators had made a prediction for a large earthquake in California in late 1986. The original prediction was for an earthquake of magnitude 7.5 or greater in a large area of California for a four year period ending on December 31, 1987. In the Fall of 1987 we were informed that a misprint had occurred and that the original prediction was, in fact, for a five-year period ending December 31, 1988. On June 6, 1988 Dr. Keilis-Borok informed NEPEC in his presentation that the prediction or Time of Increased Probability (TIP) would be extended through 1991 and was for a somewhat larger area than originally published, i.e. for most of California and western Nevada.

The algorithm used for the California prediction is an application of pattern recognition methods first described by Gelfand et al. in 1976 and subsequently extended and adapted to earthquake sequences in a number of studies. The algorithm M8 used for this particular prediction was constructed to generate TIPs for worldwide earthquakes of magnitude 8 or greater in the period 1965 to 1982. The algorithm was then tested by applying it to 9 regions not used in the above learning stage. In three of the 9 regions, no TIPs were declared and no strong earthquakes occurred. In the other six regions, nine strong earthquakes were found in the catalogues and eight of these were preceded by TIPs. The ninth one occurred without a TIP (failure to predict). There were two false alarms (a TIP without an earthquake).

The M8 algorithm was applied to the southern California catalog for events of magnitude 7.5 or greater and generated a TIP before the 1952 Kern County earthquake. It also generated a false-alarm, which was taken to have been associated with a magnitude 7.2 earthquake off the coast of northern California in 1980. The TIP of present concern was generated by application of M8 to the global catalog.

The method is empirical and attempts to identify integral properties of various aspects of the seismicity in a broad area, which may be precursory to a major earthquake in the region. The method represents a distinctly different approach to the one based on the history and behavior of a particular fault segment which has been emphasized in the U.S. prediction program. Several members of NEPEC think that the method has a potential for supplying complementary information to that currently available in the U.S. earthquake prediction program.

Evaluation by U.S. Prediction Council

1. The present Soviet work documents a maturing of the pattern-recognition approach to the analysis of global seismicity as a possible predictive tool for long-term earthquake prediction. The approach presented by V.I. Keilis-Borok and his colleagues is a serious analysis of patterns of global seismicity as those patterns might be used in earthquake prediction. It is a valuable test of a scientific methodology. The time-space relations of seismicity recognized by the present analysis have produced very interesting scientific results, for example, that similar patterns and Times of Increased Probability (TIPs) precede large earthquakes in very diverse tectonic domains around the globe.

2. The conclusions about the potential for a large earthquake in the California-Nevada region are intriguing, but many uncertainties about the approach and conclusions exist. Many judgmental steps have been used in selecting and modifying parameters during the "learning" phase of the analysis, and we find it difficult to document and track all of these judgmental steps. The areal window for the potential large California earthquake is ambiguous at this stage: in one version it is approximated by a 9° by 7° (lat.-long.) rectangle, in another it is taken to be a circle centered at 37.5°N, 119.5°W that includes most of California and western Nevada. The present TIP ends in 1991, but the stability of this time window seems uncertain. According to the methodology, does the chance of a false alarm increase as the end of the window approaches, or does the chance of the earthquake increase? Such questions are not clearly resolved.

3. The present analysis does not appear to permit probabilistic statements to be made about the time, place and magnitude of the TIP or of the projected earthquake.

4. The review prepared by J. Bernard Minster for NEPEC is specifically noted and should be considered a supplement to this brief review. Among Minster's comments are the following:

> "The weakest point of the prediction, one mentioned in ALTEP on page 5 and elsewhere, and raised in a different form by Prof. Knopoff, is that we are dealing with small samples, and therefore it is difficult to generate statistical tests of the method."
>
> "...the report is very difficult to read, and to understand at a level of detail sufficient to permit easy duplication of the results...ALTEP by itself constitute insufficient information to write a computer program that could duplicate the results."
>
> "...I was distinctly unhappy with the presentation of the results...it is extremely difficult to relate the dots marking the epicenters of strong earthquakes on the map with the arrows showing how these earthquakes are associated with TIPS..."

Recommendations

1. The United States earthquake prediction program should consider additional, independent research to advance the use of patterns of seismicity at a global and large regional scale in earthquake prediction.

2. The U.S. earthquake prediction program should give further consideration to the idea that preparation zones for large earthquakes encompass regions of many square degrees. The Keilis-Borok et al. analysis suggests such a large region. A somewhat similar concept was presented by Ishibashi of Japan in which he distinguishes between "physical" and "tectonic" precursors. Tectonic "precursors" consider sets of phenomena in large preparation zones.

3. Continuing joint U.S.-U.S.S.R. research that lead to the Keilis-Borok et al. analysis is recommended, and additional, selected areas of supplementary research should be explored. We note that U.S. funds for participation in the bilateral research program have been at a low level for several years compared with those available in the 1970s. A stronger U.S. program could produce results of great value to the U.S. prediction program.

4. A continuing problem in this research is the status of global and regional catalogs. Additional effort should be made to clean up existing catalogs and to provide new and improved catalogs on a timely basis.

5. An independent study should attempt to duplicate the results of the Keilis-Borok analysis. A Soviet team intimately familiar with the Keilis-Borok approach might be invited to the U.S. and, in collaboration with a U.S. team, the algorithm and data should be installed on a conveniently accessible computer, and then tested in a variety of ways.

6. Efforts should be made toward coupling such empirical observations as the Keilis-Borok et al. analysis with ongoing investigation of underlying physical mechanisms.

7. The probability gain of the Soviet method for the California-western Nevada region is roughly a factor of 3-6 with respect to random occurrence of large events if all of their claims are accepted at face value. That probability gain is similar to that obtained for the next 30 years in the recent study "Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault."

8. Although the Soviet analysis is matured to the point of generating extremely provocative scientific results, the absence of sufficiently detailed documentation of both the methodology employed and the sensitivity of the results to input assumptions leaves NEPEC uncertain of the robustness of the prediction made using California seismicity. This along with the large area of the region included in the prediction leads NEPEC to conclude that the results do not at this time warrant any special public-policy actions in California and western Nevada. Existing U.S. analyses, and ongoing evaluations, of the seismic potential in the region constitute a suitable basis for the continuing development of public-policy planning and actions.

9. NEPEC re-iterates its previous great concern about the likelihood of a large and damaging earthquake in California during the next few decades as published in minutes of NEPEC meetings and stated in the forthcoming Open-File Report "Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault."

Various members of NEPEC had somewhat different opinions about the Soviet prediction. I am enclosing a copy of a letter from Dr. William Ellsworth, a member of NEPEC, that I received about a week after the last NEPEC meeting. While several of the above conclusions have been revised with respect to earlier draft statements in response to comments by Ellsworth and others, nevertheless, Ellsworth's letter is generally more skeptical than the opinions expressed by other NEPEC members. However, all of the NEPEC members who participated in the meeting on June 6 and 7 are of the opinion expressed in point 8 above that special public-policy actions in California and western Nevada are not warranted at this time.

Dr. Keilis-Borok is to be thanked and congratulated for spending a long and exhausting day discussing this prediction with NEPEC and its external reviewers. He was very forthcoming in describing both the strengths, weaknesses and uncertainties in the methodology and in the prediction. Dr. J. Bernard Minster is to be thanked for spending many days in preparing a detailed and thoughtful written review of the prediction.

Sincerely yours,

Lynn R. Sykes
Chairman,
National Earthquake Prediction
Evaluation Council

cc: Dr. Rob Wesson
Dr. Randall G. Updike

encls.

LRS/ls

APPENDIX G.

TELEX FROM J. FILSON TO V. KEILIS-BOROK AND REPLY REGARDING RECOMMENDATIONS OF NEPEC RELATIVE TO HIS PREDICTION

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INSTITUTE PHYSICS OF THE EARTH
ACADEMY OF SCIENCES - USSR
B. GRUZINSKAYA 10
MOSCOW, D-242, USSR

ATTN: V.I. KEILIS - BOROK

ATTACHED HEREWITH YOU WILL FIND THE SUMMARY CONCLUSIONS OF

THE NATIONAL EARTHQUAKE PREDICTION EVALUATION COUCIL (NEPEC)

RELEVANT TO THE PRESENTATION YOU DELIVERED TO THE COUNCIL ON

JUNE 6, 1988. PLEASE FEEL WELCOME TO TELEX TO ME ANY COMMENTS

THAT YOU MIGHT HAVE REGARDING THESE CONCLUSIONS.

SEE ATTACHMENT.

RECOMMENDATIONS

1. THE UNITED STATES EARTHQUAKE PREDICTION PROGRAM SHOULD CONSIDER ADDITIONAL, INDEPENDENT RESEARCH TO ADVANCE THE USE OF PATTERNS OF SEISMICITY AT A GLOBAL AND LARGE REGIONAL SCALE IN EARTHQUAKE PREDICTION.

2. THE U.S. EARTHQUAKE PREDICTION PROGRAM SHOULD GIVE FURTHER CONSIDERATION TO THE IDEA THAT PREPARATION ZONES FOR LARGE EARTHQUAKES ENCOMPASS REGIONS OF MANY SQUARE DEGREES. THE KEILIS-BOROK ET AL. ANAYLSIS SUGGESTS SUCH A LARGE REGION. A SOMEWHAT SIMILAR CONCEPT WAS PRESENTED BY ISHIBASHI OF JAPAN IN WHICH HE DISTINGUISHES BETWEEN "PHYSICAL" AND "TECTONIC" PRECURSORS. TECTONIC "PRECURSORS" CONSIDER SETS OF PHENONMENA IN LARGE PREPARATION ZONES.

3. CONTINUING JOINT U.S.-U.S.S.R. RESEARCH THAT LEAD TO THE KEILIS-BOROK ET AL. ANAYLSIS IS RECOMMENDED, AND ADDITIONAL, SELECTED AREAS OF SUPPLEMENTARY RESEARCH SHOULD BE EXPLORED. WE NOTE THAT U.S. FUNDS FOR PARTICIPATION IN THE BILATERAL RESEARCH PROGRAM HAVE BEEN AT LOW LEVEL FOR SEVERAL YEARS COMPARED WITH THOSE AVAILABLE IN THE 1970S. A STRONGER U.S. PROGRAM COULD PRODUCE RESULTS OF GREAT VALUE TO THE U.S. PREDICTION PROGRAM.

4. A CONTINUING PROBLEM IN THIS RESEARCH IS THE STATUS OF GLOBAL AND REGIONAL CATALOGS. ADDITIONAL EFFORT SHOULD BE MADE TO CLEAN UP EXISTING CATALOGS AND TO PROVIDE NEW AND IMPROVED

CATALOGS ON A TIMELY BASIS.

5. AN INDEPENDENT STUDY SHOULD ATTEMPT TO DUPLICATE THE RESULTS OF THE KEILIS-BOROK ANAYLSIS. A SOVIET TEAM INTIMATELY FAMILIAR WITH THE KEILIS-BOROK APPROACH MIGHT BE INVITED TO THE U.S. AND, IN COLLABORATION WITH A U.S. TEAM, THE ALGORITHM AND DATA SHOULD BE INSTALLED ON A CONVENIENTLY ACCESSIBLE COMPUTER, AND THEN TESTED IN A VARIETY OF WAYS.

6. EFFORTS SHOULD BE MADE TOWARD COUPLING SUCH EMPIRICAL OBSERVATIONS AS THE KEILIS-BOROK ET AL. ANAYLSIS WITH ONGOING INVESTIGATIONS OF UNDERLYING PHYSICAL MECHANISMS.

7. ALTHOUGH THE SOVIET ANALYSIS IS MATURED TO THE POINT OF GENERATING EXTREMELY PROVOCATIVE SCIENTIFIC RESULTS, THE ABSENCE OF SUFFICIENTLY DETAILED DOCUMENTATION OF BOTH THE METHODOLOGY EMPLOYED AND THE SENSITIVITY OF THE RESULTS TO INPUT ASSUMPTIONS LEAVES NEPEC UNCERTAIN OF THE ROBUSTNESS OF THE PREDICTION MADE USING CALIFORNIA SEISMICITY. THIS ALONG WITH THE LARGE AREA OF THE REGION INCLUDED IN THE PREDICTION LEADS NEPEC TO CONCLUDE THAT THE RESULTS DO NOT AT THIS TIME WARRANT ANY SPECIAL PUBLIC-POLICY ACTIONS IN CALIFORNIA AND WESTERN NEVADA. EXISTING U.S. ANALYSES, AND ONGOING EVALUATIONS, OF THE SEISMIC POTENTIAL IN THE REGION CONSTITUTE A SUITABLE BASIS FOR THE CONTINUING DEVELOPMENT OF PUBLIC-POLICY PLANNING AND ACTIONS.

8. NEPEC RE-ITERATES ITS PREVIOUS GREAT CONCERN ABOUT THE LIKELIHOOD OF A LARGE AND DAMAGING EARTHQUAKE IN CALIFORNIA DURING THE NEXT FEW DECADES AS PUBLISHED IN MINUTES OF NEPEC MEETINGS AND STATED IN THE FORTHCOMING OPEN-FILE REPORT "PROBABILITIES OF LARGE EARTHQUAKE OCCURING IN CALIFORNIA ON THE SAN ANDREAS FAULT."

"DR. KEILIS-BOROK IS TO BE THANKED AND CONGRADULATED FOR SPENDING A LONG AND EXHAUSTING DAY DISCUSSING THIS PREDICTION WITH NEPEC AND ITS EXTERNAL REVIEWERS. HE WAS VERY FORTHCOMING IN DESCRIBING BOTH THE STRENGTHS, WEAKNESSES, AND UNCERTAINTIES IN THE METHODOLOGY AND IN THE PREDICTION."

160443 USGS UT

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ATTN: FILSON
1. PRIZE LIST RECEIVED. PLEASE INFORM YOUR TELEFAX ADDRESS TO PASS PREFFERED CONFIGURATION.
2. THANKS FOR SUMMARIES CONCLUSIONS OF NEPEC. DO NOT UNDERSTAND POINT SEVEN. HOPE FOR NEPEC SAKE THAT IT IS NOT BASED ON RECENT NOTES BY ELSWORTH.
3. GRATEFUL INDEED FOR YOUR HOSPITALITY AND PATIENCE.
REGARDS = KEILIS-BOROK
160443 USGS UT

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APPENDIX H.

LETTERS OF APPRECIATION FROM L. SYKES TO V. KEILIS-BOROK AND L. KNOPOFF PRESENTATIONS AT NEPEC MEETING

Lamont-Doherty Geological Observatory of Columbia University | *Palisades, N.Y. 10964*

Cable: LAMONTGEO
Palisades New York State
TWX-710-576-2653

Telephone: Code 914, 359-2900

11 July 1988

Dr. V.I. Keilis-Borok
Geophysical Committee
Molodezhnaya 3
Moscow 117296
U.S.S.R.

Dear Volodya,

I want to thank you most enthusiastically on behalf of the U.S. National Earthquake Prediction Evaluation Council (NEPEC) for your coming to the United States and spending a very long day discussing your prediction methodology and the prediction that you and your colleagues have made for a large earthquake in the California - western Nevada region. You are to be congratulated for being very forthcoming on many different aspects of that work and for enduring what had to be tough questions from a large audience while you were undoubtably suffering from a serious case of jet-lag.

I understand that John Filson sent you a draft copy of the findings of NEPEC prepared for the meeting on June 7. A longer written version of the NEPEC meeting will be transcribed by the secretary in about one month. The various documents that we circulated including the two reviews will be published in a few months in an Open-File Report by the U.S. Geological Survey.

Thanks once again for being a part of our meeting and finding time for doing so in your busy schedule.

Sincerely yours,

Lynn R. Sykes
Chairman, National Earthquake
Prediction Evaluation Council

cc: Dr. Rob Wesson
Dr. John Filson
LRS/ls

Lamont-Doherty Geological Observatory
of Columbia University

Palisades, N.Y. 10964

Cable: LAMONTGEO
Palisades New York State
TWX-710-576-2653

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13 July 1988

Professor Leon Knopoff
Institute Geophysics & Planetary Physics
University of California
Los Angeles, California 90024

Dear Leon,

I want to thank you very much on behalf of the U.S. National Earthquake Prediction Evaluation Council (NEPEC) for coming to our meeting on June 6. I know that it took considerable time and effort on your part to make a trip East for that meeting.

I am sending you a copy of the conclusions that NEPEC reached concerning the Soviet prediction of a large earthquake in California and western Nevada. A longer written version of the NEPEC meeting will be transcribed by the secretary in about one month. It and the various documents that we circulated will be published in a few months in an Open-File Report by the U.S. Geological Survey.

Thanks once again for your participation in the meeting on June 6, for your comments about the Soviet prediction at various times and for bringing the matter to our attention.

Sincerely yours,

Lynn R. Sykes
Chairman, National Earthquake
Prediction Evaluation Council

cc: Dr. Rob Wesson
Dr. John Filson

encls.

LRS/ls

APPENDIX I.

COMMUNICATIONS RELEVANT TO THE WORKING GROUP ON CALIFORNIA EARTHQUAKE PROBABILITIES REPORT, SUBSEQUENT TO THE FEBRUARY 1988 NEPEC MEETING

Lamont-Doherty Geological Observatory | Palisades, N.Y. 10964
of Columbia University

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Palisades New York State
TWX-710-576-2653

Telephone: Code 914, 359-2900

30 March 1988

Dr. Dallas Peck
Director
U.S. Geological Survey
MS 106 National Center
12201 Sunrise Valley Drive
Reston, Virginia 22092

Dear Dallas,

I am writing to report on the last meeting of the National Earthquake Evaluation Council (NEPEC) on February 1 and 2, 1988.

NEPEC spent a full day with members of the Working Group on California Earthquake Probabilities discussing a draft of their document "Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault System." Members of NEPEC endorse the report as revised in late March and urge its prompt publication and public release. The members of the working group are to be complimented and thanked for the major effort they have made over the past year in preparing the document.

Members of the working group met again in February to revise several sections of the draft report. A draft of March 11, 1988 was circulated to NEPEC members as well as to members of the working group for comments. A few changes were made in that document in response to comments. Dietrich discussed those changes with all members of the working group and with all but one of the members of NEPEC (who was out of town). The final draft copy of late March 1988 should be considered the version that has been approved by NEPEC. That version will be sent to you separately as it goes through the final editing and review process in U.S.G.S., hopefully in one or two weeks.

The report of the working group is an important document and will require carefully executed joint release with the State of California. We believe that the report should be released to the public rather quickly. If it is not there is a risk of inadvertent public disclosure.

The report represents the present version of what is an evolving process of risk assessment. It is important to realize that the document will require updating and revision periodically as new data accumulate.

NEPEC also heard an updating of work on recent large earthquakes and earthquake hazards in Alaska and the Aleutians. Materials presented on those subjects will be included in an Open File report on the NEPEC meeting.

NEPEC discussed the Soviet prediction for a great earthquake in California and adjacent parts of Nevada which had come before the Council previously. Dr. Keilis-Borok, one of the co-authors of that prediction, has been invited to present his results before NEPEC in mid-May 1988. At the recommendation of NEPEC I have solicited extensive reviews of that prediction and associated documents related to its methodologies from four reviewers who are not members of NEPEC. It is important that the Soviet prediction be clearly distinguished from and not confused with the report of the working group on California earthquake probabilities. That would be facilitated if the report of the working group can be made available to the public well before the Soviet prediction is considered in May 1988.

Sincerely yours,

Lynn R. Sykes
Chairman,
National Earthquake Prediction Evaluation Council

LRS/ls

cc: Dr. John Filson

Pacific Gas and Electric Company

77 Beale Street
Room 2661A
San Francisco, CA 94106
415/972-2791

Lloyd S. Cluff
Manager
Geosciences

March 31, 1988

Dr. John R. Filson, Chief
Office of Earthquakes, Volcanoes and Engineering
U.S. Department of the Interior, Geological Survey
905 National Center
Reston, Virginia 22092

Dear John:

Final Report on "Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault System" By The Working Group on California Earthquake Probabilities

You will soon be receiving the Working Group's Final Report. When you and I were discussing the formation of the Working Group, I didn't realize the wisdom of your selecting Jim Dieterich as the person to take the responsibility for coordinating the Working Group's activities and seeing the project to completion.

Not only has Jim done an outstanding job in completing the coordinating aspects of this assignment, he has significantly contributed to improving the writing of many sections of the report, as well as diplomatically resolving differing points of view in interpreting the data. Jim has also done an outstanding job in presenting the results of the Working Group to NEPEC and CEPEC.

His hardworking, conscientious efforts, unending patience and diplomacy are a credit to the Working Group and the U.S. Geological Survey. Jim deserves the lyons share of the credit for completing the report, as well as the quality of the final product.

I believe the report and the actions that are likely to result will serve as a basis for and make a significant difference in reducing earthquake risks in California. It has been an honor to have been associated with such a competent and distinguished group.

Sincerely,

Lloyd

LSC:sjm

cc: Dallas L. Peck
Benjamin A. Morgan
Wayne Thatcher

State of California

MEMORANDUM

To: JOHN FILSON Date: APRIL 4, 1988
CHIEF
OFFICE OF EARTHQUAKES, VOLCANOES AND ENGINEERING

From: Richard Andrews Andrews
Assistant Director
Office of Emergency Services

Subject: CEPEC Review of " The Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault"

On March 11, the California Earthquake Prediction Evaluation Council [CEPEC] met and reviewed the report by the NEPEC working group on the probabilities of large earthquakes occurring on the San Andreas fault.

CEPEC concluded that, in general, the report was in good form, but in the discussion with Jim Dietrich several suggestions were made to enhance the effectiveness of the report as a public policy document.

CEPEC members raised the following issues in an effort to minimize confusion and dissent within the scientific community at the time of the report's release:

o An explicit statement is desirable indicating that these conclusions reflect a reasoned consensus based upon existing understandings, but regular revisions and updates can be anticipated as new data and interpretive frameworks become available.

o The report should include more explicit statements on the limitations of the characteristic earthquake model used in the report and the quality of the data available to the working group.

o It was especially emphasized that explicit statements are needed regarding the Magnitude 8+ events cited in the 1980 FEMA/NSC report. Since these events are not mentioned in the report but have been the subject of very extensive use and discussion throughout California's earthquake programs for the past 7 years, **NEPEC and the USGS should develop specific answers to questions regarding this issue. Answers to this issue should be closely coordinated with the Office of Emergency Services in California.**

o CEPEC recommends that the USGS work closely with California OES in planning the release of this document in order to assure the maximum positive results.

Jim Davis, CEPEC Chair, has discussed the above issues with Jim Dietrich. Davis believes that the first three issues cited above appear to have been dealt with in a post-March 11 revision of the text of the report. When the final draft becomes available we will examine it, but we do not anticipate, at this time, making additional requests for change.

We do, however, want to be directly involved in the planning and release of the report as previously discussed.

Thanks again for providing the opportunity to comment on this important document.

cc: William Medigovich, Director, OES
Jim Davis, Chair, CEPEC

United States Department of the Interior

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

April 7, 1988

Dr. Lynn R. Sykes
Lamont–Doherty Geological Observatory
Palisades, NY 10964

Dear Lynn,

Lloyd Cluff is currently out of town, but I spoke with him last week, before he left, concerning the status of the working group report. He was of the opinion that the report was in finished form and should be forwarded. In his absence, and on behalf of the other members of the Working Group on California Earthquake Probabilities, I enclose our final report: "Probabilities of Large Earthquakes Occurring in California on the San Andreas Fault System."

The report incorporates the changes agreed upon by the Working Group that we discussed by telephone. All members of NEPEC were contacted and concurred with final alterations.

Best Regards,

James H. Dieterich

Copy to: Lloyd Cluff
Wayne Thatcher
John Filson

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