Statistics vs. Case Histories: The Example of the Oaxaca Earthquake of November 29, 1978

by Leon Knopoff

One of the entries in the common-lore encyclopedia of homilies is the assertion, "You can prove anything you want to with statistics." Another entry, elsewhere in the same volume asserts what is almost the converse, "Statistics never lie", which offers the hope that truths will be unearthed by statistical methods. An especially fragile focus of mistreatment by such arguments is the problem of models for earthquake occurrence, and especially the area of the possibility of earthquake prediction.

Taken by and large, the part of reliable catalogs which records histories of seismicity is too short, in comparison with the times of recurrence of large earthquakes, to develop adequate statistical models for anything but the most obvious features. What are these "obvious" features? First, there are more small earthquakes than large ones in the same interval of time. Second, aftershocks are an evident clustering in time of smaller events following larger events. Third, once aftershocks have been identified and removed from catalogs, the remaining shocks appear to occur randomly in time. These statements can be given in mathematical versions and given as much statistical substantiation as is possible under the umbrella of both of the assertions in the first paragraph.

One of the truly difficult tasks in all scientific discovery is the coupling of physical models to statistical procedures. How does a physical model, slightly more complex than the simplest, influence statistical output, and conversely, how does one construct statistical procedures to test for substantiation of the model? A case in point concerns the hope -- is it any more than that? -- that evidence exists in the catalog history of seismicity of a given region for premonitory signatures in the seismicity which fortell the coming of a large earthquake. The statistical problem involves a search for deviations, which we may assume will be subtle ones, from the three statistical regularities listed above.

There is however an ontological problem in searches for precursors based on physical models, which is connected with the *a posteriori* nature of the models. It is relatively easy to construct physical models for precursory phenomena, especially in an *a posteriori* fashion. The argument is rather like this: presumed premonitors are observed to have occurred prior to a subsequent, particular, notable event. A model is afterward proposed which would seem to fit as many of the observations as possible. It would be more satisfying to have the model in advance of the event, so that one cannot be accused of tailoring a model to an observation. The next step in the procedure is to search for additional events to test the model. Occasionally -- just occasionally mind you -- reports are presented only of those events which are consistent with the model. Attempts to substantiate these models on statistical grounds often fail because of lack of data, i.e., insufficient length of catalog.

This has been a lengthy prologue, but your editor, in asking me to offer some comments concerning the events leading up to the Oaxaca earthquake of November 29, 1978, gave me relatively free rein to develop my topic as I saw fit. At last I come to a discussion of the earthquake itself. The seismic gap model seems to have been given first by Fedotov (1965), and later by a number of others including Mogi (1968), Tobin and Sykes (1968), Sykes (1971), Kelleher (1972), and Kelleher et al. (1973). In this model, the aftershocks of large earthquakes that have occurred over the past tens of years will continue to delineate the zone of rupture of these earthquakes. Large earthquakes and their aftershocks represent regions where accumulated deformational energy has been relieved. It is natural to conclude that any current inactivity in certain regions of the major seismic belts is indicative that the most recent large earthquake in this region is in the distant past, and hence that deformational energy has had an opportunity to accumulate once again. Thus in "seismic gap" models, seismic inactivity at plate boundaries should serve as an indicator of an incipient major earthquake. The time of the forthcoming earthquake is not indicated by the presence of the seismic gap, i.e., the absence of significant seismic activity; instead, the time of the forthcoming earthquake is supposed to be signaled by a resumption of small and intermediate scale seismic activity just prior to the event. The magnitude of the forthcoming event is correlated to the duration of the gap; the longer the duration, the larger the earthquake.

It was in this frame of reference that Ohtake et al. (1977a) of the University of Texas, Galveston announced that a seismic gap existed in Oaxaca at that very time. The authors clearly stated that they were unable to forecast the time at which the future earthquake would be likely to occur. They only asserted that a large earthquake of magnitude $7\frac{1}{2} \pm \frac{1}{4}$ would be likely to occur within the region $16.5^\circ \pm 0.5^\circ$N, $96.5^\circ \pm 0.5^\circ$W upon termination of the relatively somnolent (quiescent?) period of activity then present in Oaxaca.

At this stage an unfortunate and scientifically irrelevant episode occurred that cast a rather bizarre shadow on the story. Two individuals (seers?) sent a letter from Las Vegas to the President of Mexico on 7 February 1978, in which a major earthquake was predicted for the Oaxaca area on the precise date of 23 April 1978 based on "demonstrated scientific facts". The letter was almost contemporaneous with interviews in the press of the Texas team who continued to resist predicting the date of the event. The letter was forwarded to the attention of the local authorities of Oaxaca and the response was one which took the "prediction" seriously. Panic selling of property occurred. A vigil was held on the appointed day. Of course the date upon which the earthquake was forecast to occur, came and went without the transpiration of the event. An account of the events in Oaxaca was given by Garza and Lomnitz (1978).

Garza and Lomnitz (1978) reanalyzed the seismicity of the region using apparently the same catalog as the Texas group. Their conclusions were that, although a lowered seismicity did in fact exist, the level was not inconsistent with that which one might expect by a random process. A random process ought to produce, from time to time, episodes of high seismicity and at other times episodes of low seismicity. The conclusion of Ohtake et al. (1978), was that the probability that the lowered seismicity since 1973 could have been produced by a random process was 1/2000; this figure was disputed by Garza and Lomnitz (1978) and placed between between 1/100 to 1/12. While this latter value is low, it was not low enough to warrant identification of a seismic gap with certainty, in the view of Garza and Lomnitz (1978).

Garza and Lomnitz explored further the likelihood that low-level intermediate magnitude seismicity in the Oaxaca area can be correlated with subsequent large earthquakes and came to the conclusion that no persuasive correlation exists. The observations of Garza and Lomnitz were reported at a conference on seismic gaps convened at Cambridge, Mass. on 25-27 May, 1978.

At the same meeting, Ohtake et al. (1978) continued to maintain their position that a seismic gap in the area was indeed present and that the continued prolongation of the gap level indicated an even stronger event at the end of the interval. They continued to refrain from giving precise dates; instead they recommended a program of on-site monitoring to capture the onset or resumption of seismicity which would -- as indicated above -- signal the termination of the gap phase. An analysis of case histories in three regions showed that a 5 year gap, which was by now the duration of the Oaxaca event, indicated that the subsequent earthquake would be strong.

Ponce, also at the National University of Mexico, invited McNally of Cal Tech to participate in a joint program of installation of 7 seismometers in the region. Installation was completed during the first week of November and for about one week, the level of activity was low. During the second week of November activity increased significantly with earthquakes with magnitudes up to 4.5. The larger earthquake with $M_s = 7.8$ occurred on the 29th of the month, with epicentral coordinates within the rectangle given by Ohtake et al. (1977a).

Three questions may be raised in connection with the event and especially with regard to the bearing of statistics on the problem of prediction of large earthquakes on the seismic gap model.

1. Did the on-site observations validate the sesimic gap model and substantiate that a seismic gap did indeed exist in Oaxaca prior to November, 1978? All we can say with certainty is that for a week or more in November, seismicity at the $M \leq 4.5$ level was low. By itself, such low activity is not unusual over a period of one or two weeks. An observation of only a few weeks must be considered to be a case history of an earthquake precursory sequence and in no contradiction with either the likelihood that a gap in seismicity did or did not exist prior to November 29, 1978 in Oxaca. The likelihood problem is a statistical one and the case history approach gives no information that bears on this question. This is a slightly antiseptic interpretation of such observations, in view of the dramatic impact and excitement of the accomplishment by McNally and Ponce. The fact that the "capture" of the sequence of events immediately prior to the large earthquake itself is not wholly relevant to the question of the existence of a seismic gap, leads us to the second question which is:

2. Did a seismic gap exist from 1973 to 1978 in Oaxaca? Both of the calculations of Ohtake et al. (1978) and Garza and Lomnitz (1978) gave probability values that the level of seismicity was low compared to that of a random process. The question

at issue, concerns an acceptable threshold of absence of seismicity before an alarm is to be sounded; we expect the threshold to be appropriate to the particular algorithm used in the data processing. If two or more groups process a given data set differently, then different criteria for establishing an alarm should be established. These may exist at different thresholds for different models. This is a valid question for statistical investigation, but one which seems only to have been touched on by the investigators. However this question is imbedded in a broader one. Before sounding an alarm based on statistical criteria, we should ask how valid is the seismic gap model as an indicator of a forthcoming large earthquake? This can be restated as our third question.

3. Is the seismic gap a precursor to large earthquakes? This is an equally valid question for statistical study. Here one has the advantage of being able to get as far away from case histories as possible. The case history approach is about the only one possible at the present time if one wishes to study a particular (small) region, since there is a dearth of large earthquakes and an even stronger absence of information about lower level seismicity in present-day catalogs of such short span. But if one enlarges the scope of the investigation to study the principle of a seismic gap as a worldwide phenomenon -- as distinct from its applicability in a particular area -- then a large enough data set exists to draw some significant conclusions. Kagan and I performed this analysis (Kagan and Knopoff, 1976) and found that there was significant reduction in the rate of occurrence of earthquakes of magnitude 7.0 or greater prior to even larger earthquakes for the global catalog. We have not investigated whether this result applies as well for lower level seismicity as precursors to earthquakes with magnitudes in the 7.0 to 7.5 range because of the unavailability of the relevant catalogs. As far as we are concerned, the existence of the seismic gap as a worldwide phenomenon for large precursors to even larger main shocks is established on statistical grounds.

It is important to emphasize the differences between the case history approach, which is a study of a particular event in a particular region at a particular time, and the statistical approach which can only describe the probability, first of the existence of a natural phenomenon such as a precursor, and second that the precursor exists at a given place and time.

By all accounts the McNally-Ponce observation is a rare and remarkable one and they should be given much credit. The space-magnitude forecast of Ohtake et al. is very promising and is consistent with some statistical modeling. A collection of case histories of this type should provide a credible data base from which one can expect to analyze statistically the details of the seismic gap as a precursor. The collection will not be done instantly; one should not expect validation of the seismic gap model instantly.

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(arranged chronologically)

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