Forecast of Earthquake in Western Nicaragua

The destructive earthquake of 2 September 1992 off the Pacific Coast of Nicaragua appears to have ruptured the seismic gap that we identified 11 years ago (*1*) (Reports, 7 Aug. 1981, p. 648) as the likely site for a future large earthquake. Preliminary teleseismic locations of the main shock, and aftershocks of magnitude 5 or greater that occurred within 5 days of the main shock, outline the seismic gap that we identified on the basis of (i) deep seismic quiescence and (ii) historical earthquake activity. Within the quiescent area, virtually no earthquakes larger than magnitude 2 occurred during our period of monitoring (1975 through 1978), yet the area was surrounded by numerous earthquakes of magnitudes up to 5.7. The quiescent area extended 115 kilometers from the Middle America Trench downdip on the Benioff zone to a depth of about 50 kilometers and for about 50 kilometers along its strike. We assumed this area was a locked portion of the interplate thrust zone and was accumulating strain. Using a simple calculation, we estimated a possible magnitude of 7.6 to 7.9 on the basis of the size of the quiescent area and the time since the last major earthquake at that location, the magnitude 7.5 earthquake of 1898. This magnitude forecast is indistinguishable from the preliminary moment magnitude of 7.5 to 7.6 determined by H. Kanamori at the California Institute of Technology (*2*) for the recent earthquake.

Most damage and the loss of at least 200 lives during the recent earthquake resulted from a tsunami. We did not foresee the occurrence of a tsunami primarily because we found no mention of tsunami damage in historical accounts of past earthquakes along the Nicaraguan coast. In hindsight, we see that because the quiet zone extended up to the trench axis, the earthquake had the potential to displace the sea floor sufficiently to generate a destructive tsunami.

These results suggest that the combined study of historical accounts of past earthquakes and data from local seismograph networks can identify potential sites of future destructive earthquakes. Although the timing of the impending earthquake cannot be specified to better than a few decades, this information could help focus the limited resources of hazard reduction programs.

David H. Harlow
Randall A. White
U.S. Geological Survey,
345 Middlefield Road,
Menlo Park, CA 94025

Ines Lucia Cifuentes
Carnegie Institution of Washington,
5241 Broad Branch Road, NW,
Washington, DC 20015

Arturo Aburto Q.
Seismological Laboratory,
Mackay School of Mines,
University of Nevada,
Reno, NV 89557

REFERENCES

1. D. H. Harlow, R. A. White, I. L. Cifuentes, A. A. Q., *Science* **213**, 648 (1981).
2. H. Kanamori, personal communication.

■

Big Physics Collaborations

Neither sociology nor nostalgia for the sixties is a suitable basis for judging the development of the very large collaborations that now dominate experimental high-energy physics (News & Comment, 11 Sept., p. 1468). The last two decades of research have established the "standard model" of fundamental interactions. While this theory has withstood every experimental test so far, its real importance is not the questions it answers but the questions it allows us to ask: Why do particles have the masses they do? Are there forces beyond the ones we know today? Are there particles more fundamental than quarks? We don't know the answers to these questions, but we know where to look for the answers. We need to look at very high energies, in processes with enormous momentum transfer. From this follows the need for gigantic accelerators, detectors, and collaborations. Ingenious, smaller experiments may, with luck, provide clues to the basic questions, but the clues will be ambiguous, if they are not imperceptible.

Neither the Superconducting Super Collider (SSC) nor the European version, the Large-Hadron Collider (LHC), is being built just to find the Higgs particle. There may be one Higgs particle or many. There may be none. All we know is that there will be new phenomena at the SSC and the LHC that will tell us about the origin of mass. But the aims of these machines are much broader: to find new forces and new phenomena that might show up in just a few of the 10^{15} events produced each year. No less than in the past, ingenuity will be essential for making important discoveries.

Thousands of physicists from around the world have joined to develop experiments for the existing and future high-energy colliders, despite the difficulties, because the physics is compelling and because it intrinsically requires the talents and resources of many collaborators. It's easiest to look under the lamppost, but if you know the keys are someplace else, you better look where they are. The keys to understanding fundamental interactions are at the highest energies, where the search requires large-scale cooperative efforts.

Robert N. Cahn
Division Director, Physics Division,
Lawrence Berkeley Laboratory,
Berkeley, CA 94720

I agree with the sentiments in Faye Flam's article "Big physics provokes a backlash" and would not myself like to be in a collaboration of 100 or 1000 scientists. But the need for large collaborations in at least some cases does seem to speak to the request of the National Institutes of Health in recent prominent cases that all authors of papers be able to vouch for all aspects of the papers. This requirement would obviously be impossible and undesirable in huge collaborations. So as long as high standards are kept in all facets, I do not see why compartmentalization is not acceptable in biology as well as in physics.

Jay M. Pasachoff
Department of Astronomy,
Williams College,
Williamstown, MA 01267

■

Pioneering Work in Immune Tolerance

The inspiration for our studies of the thymus has been research on privileged transplant sites (for pancreatic islet allografts) and the earlier studies in which cellular inocula in the thymus were used (*1*). The elegant work of B. H. Waksman and his colleagues [who reported the establishment of long-lasting, antigen-specific tolerance to protein antigens by introducing bovine serum albumin and bovine gamma globulin into the thymus (*2*)] has also been important to us, and we have consistently referred to it in our publications (*3*).

We apologize, however, for the omission of reference to a 1970 publication by G. W. Ellison and Waksman (*4*) in our recent report "Prevention of autoimmune diabetes in the BB rat by intrathymic islet transplantation at birth" (29 May, p. 1321). In their 1970 paper (*4*), Ellison and Waksman noted a "slight but definite reduction in the severity of EAE [experimental allergic encephalomyelitis]" in rats after intrathymic injection of spinal cord homogenates. We note, however, that our model is somewhat different from that used by Ellison and Waksman. Our study was of neonatal animals rather than adults, we introduced vi-