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Earthquake Hazard Assessment: Has Our Approach Been Modified in the Light of Recent Earthquakes?

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Thank you, Mr. President, and let me say how honored I am at being designated as the EERI's Distinguished Lecturer for 1995.

This may be an historic event for many of you. It will be the first time that you have ever heard a geologist give a talk related to earthquakes that was not replete with Kodachrome slides of cracks in the ground and maps of active faults, or at least of *allegedly* active faults, or *potentially* active faults, or even *possibly* active faults! But I would like to go beyond the detailed discussion of individual earthquakes this afternoon, and instead discuss the broader problem of whether our studies of numerous recent earthquakes—here and abroad—are leading to modifications in our hazard assessment techniques, speaking from the point of view of a geologist or seismologist. I emphasize that I make no pretext of speaking for either the geotechnical or structural engineers.

You already know, of course, the answer posed by the title. It's both "yes" and "no." And I would like to focus on the question of: In what scientific areas, in particular, are our approaches *changing*, and in what areas do the *traditional* methods remain credible?

EARTHQUAKE SURPRISES

My colleagues in the audience know, that for at least 30 years, I've made the truly remarkable observation that every earthquake seems to be a *surprise*—a surprise not only in its timing, of course (and it may be a long time, if ever, before we solve the earthquake prediction challenge), but particularly a surprise to geologists and seismologists in the varied *physics* of earthquake processes, and in the varied *geology* of earthquake environments. And obviously our ultimate objective is *not* to be surprised by every earthquake in these regards, but to be able to put more realistic bounds than we can today on the kinds of occurrences that are likely and unlikely.

Certainly one of the most important seismological discoveries of the past 20 to 30 years is that large earthquakes are far more different from one to another than we had

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ever envisaged, particularly in the physics of the earthquake-rupture process. These differences include such parameters as stress drop, rupture pattern, directivity effects, fault roughness, and velocity of rupture propagation. And while this development is truly exciting from the point of view of seismological research, it is not particularly good news for those trying to be more systematic in hazard assessment. And particularly, I should add, it's not good news for those involved in earthquake prediction research, because the more earthquakes differ from one to another, the less likely it seems that they will have common precursors, if any.

Recent studies also reveal that some of our previously held basic geologic tenets are being seriously challenged. For example, most of us grew up religiously believing in the elastic rebound concept, which dates from studies of the 1906 San Francisco earthquake. It seems so intuitively obvious, if strain is accumulating along a fault at a relatively constant rate (which is consistent with modern concepts of plate tectonics), that when the breaking point is finally reached, the process will start all over again, in a relatively systematic way—resulting in roughly equal recurrence times between roughly similar large earthquakes caused by fault rupture. But some of scientists are now challenging this simple concept, arguing from laboratory experiments and theoretical models, as well as from some seismological and geological observations, that the friction on a fault at seismogenic depths (that is, the depth at which rupture initiates) is so complicated and so unpredictable, that something akin to *chaotic* behavior may be more typical than *systematic* behavior—not only in the time intervals between large earthquakes, but also in their magnitudes, rupture lengths, and locations along a specific fault system. And this, if true, is certainly not good news for those of us involved in hazard assessment, where the time and size of the last large earthquake has often been an important parameter in our prognostications for the near future.

Similarly, most of us have traditionally accepted the dogma that, at least during our lifetimes, tectonic processes are not changing with time. Yet Frank Press and I currently have a paper in press with the Journal of Geophysical Research which argues that, for earthquakes of magnitude 5½ or greater in southern California since 1915 (for which the record seems statistically complete), a marked change in the tectonic pattern occurred in about 1971: Prior to 1971, more than twice as many earthquakes were of strike-slip type—parallel to the San Andreas fault—than were of thrust type or of non-San Andreas trend. But since 1971, to the present time, more than twice as many thrust earthquakes, and earthquakes of non-San Andreas type, have occurred as compared to those of typical San Andreas type. And the difference seems statistically significant. One possible answer is that there has been a very slight shift in the direction of relative plate motion between the Pacific and North American plates, although we have no idea of whether this phenomenon is cyclical in nature. In any event, our study represents still one more reason to be suspicious of the short historical and instrumental record of earthquakes in prognostications for the future. Such records, of course, represent one of the primary bases seismic hazard analysis. Whether we like to admit it or not, earthquake hazard assessment is essentially a *predictive* effort, with all the frailties and uncertainties of any predictive science.

I spoke a moment ago about surprises. Let's take a look at the two largest worldwide earthquakes of 1994:

The June 9th magnitude 8.3 deep-focus earthquake beneath Bolivia, which caused no damage because of its great depth of more than 600 km, is by all odds the largest deep focus event ever recorded and, thus it obviously came as quite a surprise. In fact, considerable scientific controversy is still under way as to how brittle shear failure can occur under the very high-pressure and high-temperature conditions at this great depth.

And the October 4th magnitude 8.2 earthquake off the southern Kurile Islands—just north of Hokkaido—took place in a region where all previously recorded large earthquakes have been typical subduction-zone events of thrust type on the plate boundary. Yet the 1994 event was predominantly strike slip in nature, and was *within* the descending lithospheric slab rather than along its boundaries—quite unlike anyone would have predicted on the basis of either the plate-tectonic pattern of the region, or of the earlier seismographic record. Furthermore, it occurred only 25 years after an event of similar magnitude very nearby, which is a surprisingly short time interval for earthquakes of this magnitude.

If, on the other hand, we look at the largest earthquake to date of 1995 (at least to the time of this writing), the January 17th magnitude 6.8 earthquake near Kobe, Japan, was, at least from the geological and seismological points of view, a relatively "well-behaved" earthquake, and hardly a scientific surprise. It occurred on a long-recognized northeast-trending active fault zone, in part virtually within the city of Kobe, and with a sense of strike-slip displacement typical of earlier prehistoric events which had been indicated by neotectonic geologic field studies. Furthermore, it occurred with a magnitude consistent with the known mapped length of the fault zone. In fact, I personally walked along strands of the same fault zone in the company of Japanese geologists in 1969—more than 25 years ago—and I dare say that these geologists are hardly surprised, at least in a scientific sense, by this earthquake. And despite the very tragic damage in Kobe, the strong-motion records from Kobe that I have seen to date indicate no great surprises to California engineers and seismologists familiar with near-field records (that is, records obtained very close to the causative faults) that have been obtained here in recent years. Similar strong near-field records have been seen not only in California but also, for example, in Tabas (Iran), Nihani (Northwest Territories), and Erzincan (Turkey).

THE CHALLENGE OF BLIND THRUSTS

I argued in a paper published some 20 years ago that, in California, "virtually all large earthquakes (those exceeding magnitude 6.0) have occurred on late Quaternary faults that *had* been recognized, *could* have been recognized, or *should* have been recognized by field geologists prior to the events." And while I think that this generalization is still largely true, I wish now that I could go back and remove the words "virtually all" from the sentence, because this has certainly been shown *not* to be the case for recent *blind thrust* earthquakes, such as those at Coalinga in 1983, Whittier Narrows in 1987, and Northridge in 1994. We should also probably include the damaging Santa Barbara earthquakes of 1925, 1941, and 1978. For those of you not familiar with the geological jargon, let me explain that a blind thrust is one on which the fault displacement does not reach the ground surface, and never has, even during earlier earthquakes, but which instead terminates upward in the axial region of an anticlinal fold (that is, an upwarped bend). Repeated fault displacements at depth are, in fact, responsible for the continuing "active" rising of the folded structure, despite the absence of faults reaching the ground surface where geologists might otherwise observe and study them.

Blind thrusts obviously present a particularly difficult and important challenge to geologists and seismologists involved in hazard assessment, and there can be little question but that recent earthquakes on such buried structures *have*, in fact, stimulated some modifications in our approach to earthquake hazard assessment, at least in some geologic terranes. I would emphasize, however, that blind faults capable of producing large earthquakes are not lurking under every part of the western United States. In California, for example, areas of active folding—which represent the geologic environment of blind thrusts—have long been recognized in areas such as the western Transverse Ranges, the Los Angeles basin, and the western margins of the San Joaquin and Sacramento valleys. I don't mean to imply, however, that the early recognition of these areas of active folding carried with it the appreciation of their significance for earthquakes. We owe it to John Suppe and his students at Princeton University for recognizing and explaining the structural relationships between blind thrusts and overlying, actively growing, anticlinal folds, as well as their earthquake potential. But other areas, such as the Sierra Nevada, the Mojave Desert, and the Basin Ranges of Nevada, Oregon Idaho, and Utah are apparently *not* typified by such structures, so geologists can recognize with reasonable confidence those areas subject to blind-thrust earthquakes and those that are not.

From the point of view of hazard assessment, it is encouraging that most—although not all—areas of active blind thrusting are also typified by more traditional active thrust faults that *do* reach the ground surface, where they give the geologist obvious clues to the underlying dangers. For example, although the 1994 Northridge earthquake occurred on a buried structure that had not earlier been recognized as a potential seismic source, its epicentral area in the western San Fernando Valley is nevertheless marked by several throughgoing surface faults that are shown as breaking Quaternary gravels on the 1969 Los Angeles Map Sheet of the California Division of Mines and Geology. Thus a large earthquake in the western San Fernando Valley should have come as no great scientific surprise, and, in fact, the U.S. Geological Survey had assumed a magnitude 6.6 earthquake on the very nearby Santa Susana fault in one of its earlier study scenarios of regional seismic hazards.

Similarly, there has been much publicity in the past several months concerning the possibility of a large earthquake directly beneath downtown Los Angeles on a recently recognized blind thrust. This a geological concept with which I basically agree, but it's not as though this is the first time that anyone has recognized the threat of a large local earthquake to downtown Los Angeles. I myself wrote a consulting report almost 25 years ago for a proposed high-rise building in the downtown area, and I recommended consideration of a magnitude 7½ earthquake on the very nearby Hollywood-Santa Monica fault system. This was done on the basis of the demonstrated continuity and degree of activity of the fault system derived from geologic studies carried out, at least in part, as long ago as 1930, when Harold Hoots documented the offset of young terrace gravels by the Santa Monica fault at the mouth of Potrero Canyon in the Pacific Palisades area.

In the same vein, the Whittier Narrows area, site of the 1987 blind-thrust earthquake, was specifically pointed out as the area of a rapidly growing anticlinal fold in a paper published in 1927 by Frederick Vickery, a petroleum geologist. He correctly recognized the very young upwarped geomorphic surface of the Montebello Hills as indicative of an actively growing fold, although he did not specifically mention the related potential for seismic activity at that time. This came to light only at the time of the 1987 earthquake, although, in retrospect, we should have been more suspicious. Incidentally, Vickery also first pointed out in his 1927 paper that Cahuenga Pass, through which the Hollywood Freeway now traverses the Hollywood Hills between downtown Los Angeles and the San Fernando Valley, represents the ancient course of the Los Angeles River, and he postulated that the pass is now high and dry because of the very rapid and continuing uplift of the Hollywood Hills that eventually blocked the river and forced it to its present course around the eastern end of the hills near Griffith Park. What could be better evidence of rapid ongoing deformation and the modern earthquake potential of that area, whether generated by blind thrusts or by faults reaching the surface?

By the same token, I should point out that it was the demonstrated *absence* of deformation of the 80,000-year-old marine terrace that almost encircles the Diablo Canyon Nuclear Plant near San Luis Obispo, that led, in part, to the persuasive argument that active folds and blind thrusts do *not* represent the dominant seismogenic process at that particular locality today.

The quantitative hazard assessment of blind thrusts represents probably our most challenging hazard assessment task today, at least in parts of California and similar areas elsewhere, of which there are many worldwide. And, indeed, the challenge does call for some new and innovative approaches. Certainly it is important for us to find ways to recognize specific growing folds and to measure their growth rates—in the same sense that establishing long-term slip rates of surface faults has become so important in quantifying the hazard of *these* faults. Perhaps the most promising tool is that of *geodesy*, particularly taking advantage of the new GPS (Global Positioning System) in making geodetic determinations quickly and cheaply. Unfortunately, GPS accuracy in vertical measurements is considerably degraded as compared with horizontal measurements, and it will be necessary to wait for longer periods of time to collect meaningful data on the vertical growth rates of folds. But even GPS horizontal deformation rates are now becoming so accurate and so abundant that they have effectively been integrated into the recent seismic hazard analysis carried out by a team of scientists from the Southern California Earthquake Center; for the first time, data from seismology, geology, and geodesy have been used together to produce an *integrated* seismic hazard assessment. Although one might disagree with some of the models that have been used, and, in fact, I am personally skeptical of some of the conclusions, one has to applaud this pioneering effort. In my opinion, it represents the wave of the future in quantifying seismic hazard assessment in a meaningful, quantitative, and realistic way from a *variety* of relevant data sources.

Another promising area of study in the understanding of blinds thrusts and their associated growing folds is the more effective use of what might be called "micro-geomorphology" combined with radiometric age-dating techniques—that is, the clues in the detailed physiography and ages of geomorphic surfaces that tell one *where* and at what *rate* surficial folds are growing. Even a non-geologist can look at the front of Wheeler Ridge, south of Bakersfield, or at the Kettleman Hills, near Coalinga, and say, "these just don't look like ordinary mountains," particularly in the absence of deep stream channels and canyons cutting the slopes. Well, they're *not* "ordinary mountains," and they're the very type of rapidly growing anticlinal upwarps that are highly likely to be underlain by active blind thrust faults. Indeed, the initial point of rupture (the "hypocenter") of the magnitude 7.5 Kern County earthquake of 1952 was very nearly beneath Wheeler Ridge!

An additional technique that we are returning to—one might say out of desperation—is the concept of the *floating earthquake* in areas of complex active folding. In the region of Santa Barbara, for example, there can be little doubt of numerous blind thrusts regionally underlying the western Transverse Ranges. But we currently know so little about the exact locations and configurations of these faults, particularly at seismogenic depths of 10-20 km, and we also know so little about their individual seismogenic capabilities, that in order to be conservative, one is forced to assume the possibility of a blind-thrust earthquake at almost *any* locality in the folded region—the "floating earthquake." Such a technique is, in fact, being used by the Bureau of Reclamation in the ongoing re-evaluation of the seismic stability of Bradbury Dam at Cachuma Lake.

On the other hand, as we gain a better understanding of the local geology of a given area, as, for example, through the construction or more accurate geologic cross sections that portray faults and their inter-relationships at depth, we should be able—in the long run—to locate individual seismic sources more effectively and thus be able to discard the admittedly somewhat primitive technique of the floating earthquake.

You might ask why precise hypocentral locations of micro-earthquakes cannot be used to identify and delineate seismogenic blind thrusts. Don't we simply need a more sensitive and more densely distributed seismographic network? Unfortunately, even on well-delineated vertical strike-slip faults such as the San Andreas, the presence *or* absence of micro-earthquakes doesn't seem to tell us much, at least locally and for short time periods, about the potential for large future earthquakes. In this regard, we have looked carefully at the locations of small earthquakes in the Northridge area for the years prior to the 1994 earthquake, and, at least as yet, no distinctive pattern outlining the 1994 fault-rupture plane emerges. And the same thing appears to be true of the fault rupture zone of the much larger magnitude 7.5 Landers, California, earthquake of 1992.

DETERMINISTIC VS. PROBABILISTIC TECHNIQUES

Most of you are probably aware that at the present time there is somewhat of a controversy among geologists, seismologists, and earthquake engineers regarding the relative advantages and disadvantages of *deterministic* and *probabilistic* hazard assessment techniques. For those few of you who may not be familiar with these terms, I should explain that a deterministic assessment is one that usually results in single-valued recommendations, such as: "Consider in your design a magnitude 6.3 strike-slip earthquake on the Nonesuch fault as a distance of 22 km." A probabilistic assessment, on the other hand, might suggest several magnitudes and locations of earthquake with different probabilities of occurrence during a specific time period, such as 30 years. And it usually specifically addresses the important issue of *uncertainty*. That is, how confident can we be that the observations, analysis techniques, and resulting conclusions are right?

You might ask, incidentally: Where does this 30-year number come from that we keep seeing in hazard reports and in resulting newspaper stories? Is it the inevitable output of some complicated mathematical equation? I must admit to being at least partly responsible for the repeated use of the 30-year time interval in recent years, and let me assure you that its selection was entirely *political* and not *scientific*! For a strictly scientific or engineering audience, it would be much more typical to use one-year probabilities. But, in the case of earthquakes, one-year probabilities would appear so *low*, at least for a given area of limited size, that they simply would not attract much attention. Then how about 100-year probabilities, which would certainly be high enough to attract attention? But 100 years is too *long* a period for most people to worry about. Thus, in the first of the California probabilities reports, we arbitrarily picked a 30-year time period because it appeared to be within the attention span of the public and, particularly, within the attention span of legislators and political leaders! (On the other hand, with term limits now being imposed, we might have to reconsider and shorten that time span!)

I should tell you right off the bat that, in the controversy between probabilistic and deterministic approaches, I fall squarely into the camp of the probabilists, because their techniques, however imperfect, seem to me to offer the only hope of *quantifying* earthquake hazards in a meaningful way. And this is something we simply must do if we are to be effective in implementing realistic and equitable public policy, such as establishing realistic insurance rates. Having said this, however, I don't mean for a minute to imply that just because a hazard analysis has been probabilistic, and thus "quantitative," that it is necessarily *correct*. In fact, a simple deterministic statement by a single wise individual may turn out to be far closer to the real truth that the collective opinion of an army of less-wise experts aided by the world's best mathematicians. Ellis Krinitsky was quite right in pointing out that some of the hazard-analysis techniques that have taken refuge under the guise of "probabilistic" really don't make much seismological sense, such as the indiscriminate use of the ratio between large and small earthquakes recorded in a limited area over a limited period of time, giving so-called "b-values."

The important point, however, is that *both* approaches—deterministic and probabilistic—depend ultimately on the good judgment of those providing the input data. Many people don't seem to realize, for example, that the bastion of the deterministic approach—the so-called *maximum credible earthquake*—is almost completely a judgmental concept. And it's hardly surprising that what may be credible to (e.g.) the Friends of the Earth may not be credible to (e.g.) the Southern California Edison Company. There have been many definitions of the maximum credible earthquake (MCE) offered over the years, but I have heard only one that really seems to hit home: It is that earthquake which is only a shade smaller than the minimum *in*credible earthquake. And while I say this somewhat facetiously, it really does dramatize the problem with the deterministic approach and its MCE.

Last year, I was reading through some of my old reports and ran across one written in 1971 for Harry Seed, with a table of suggested maximum earthquakes for a particular engineering project. And I noticed for the first time (although Harry had not) that I had made a truly Freudian typo: In the column which I had intended to be titled "maximum credible earthquake," I had instead typed "maximum *dredible* earthquake" (emphasis added)! That is, the word began with a "d" rather than a "c." And now, in retrospect, I rather like that. What further definition is necessary? The one word says it all!

The accepted formal definition of the MCE is that it is the maximum earthquake that is capable of occurring in a specified area during the current tectonic regime. Any geologist will tell you, on the other hand, that the "current tectonic regime" is not easily defined, and, in any event, may last locally for a million years or more. And hardly any rational person would advocate designing even a critical structure for the once-in-five-million-year earthquake, for example.

Thus any geologist or seismologist specifying a "maximum credible earthquake" must, *in reality*, have a general time period such as 10,000 years in mind—if only subconsciously. This, however, is the very essence of the probabilistic approach, which takes *likelihood* into very specific account. The upshot is that the two approaches are not as distinct as some people would like to think.

Not everyone appreciates that the probabilistic approach, like the deterministic approach, is totally dependent on the validity of the input data, which may well be judgmental in nature. The old adage of "garbage in, garbage out" applies equally as well to probabilistic earthquake-hazard analyses as to any other complex procedure. One of the alleged advantages of the probabilistic approach is that the various inputs and judgments should be completely visible and identifiable as a matter of re cord—"transparent," so to speak—although just the opposite is sometimes the unfortunate perception. That is, the mathematics of a sophisticated probabilistic analysis may be so complex that there is a real danger of *hiding* important assumptions and *obfuscating* the overall logic in a seeming mathematical fog. Obviously the intent of a probabilistic analysis should be just the opposite, and these dangers should be recognized by those making such analyses. Concerted efforts must be made to overcome them and to make the process believable to the ultimate user. I recall a dam owner once telling me, in effect, "Don't give me all this gibberish about probabilities and acceptable risk. Just tell me whether my dam is *safe* or not." Would that the world were that simple!

Most probabilistic analyses are necessarily based on the assumption of certain *models* of earthquake occurrences, or a series of models to which different weights are sometimes assigned. For example, one model that is currently being extensively used is that of the so-called *characteristic earthquake*, which holds that a given fault or fault zone is typified by repeated maximum earthquakes of about the same magnitude and at the same approximate locations, although not necessarily with a uniform recurrence interval in time. The adoption of this model, like any other model, can have a profound effect on the calculated probabilities of local earthquakes of various magnitudes in the future. We can thus say that the conclusions are markedly *model-dependent*, and we can have no greater confidence in the conclusions than we can have in the model itself. I personally think that the jury is still out on the characteristic earthquake model, although it has been widely used in recent years.

Having said that, I must point out that a deterministic analysis may *also* be model-dependent, except that in this case the model may be buried deeply within the mind of the person making the deterministic decision. As such, it might be viewed by some as "good judgment" or "profound wisdom," while by others as "bias" or "prejudice." In any event, such models are not so likely to be forthrightly admitted or to be so visible as in a probabilistic analysis.

One of the problems with the probabilistic approach, of course, is that in a specific area, adequate geological and seismological data may simply not be available to do anything very meaningful (although, for exactly the same reason, a deterministic judgment may not be very meaningful either.) On November 24, 1987, those of us on a USGS-sponsored committee were meeting in Menlo Park preparing the first of the USGS probabilities reports for earthquakes on various segments of the San Andreas fault system--the first of the blue-covered reports that many of you will remember. After an extended discussion, we finally decided to eliminate from inclusion in the report the Superstition Hills fault, which is a major member of the San Jacinto fault zone on the western margin of the Imperial Valley. Our rationale was that simply not enough relevant data, such as fault-slip rates, were then available to be sufficiently meaningful in a quantitative analysis. Only about two hours later, as I was on the plane flying back to Burbank, the Superstition Hills fault ruptured in a magnitude 6.6 earthquake—as if to *get even* with that damned committee! And the moral of this episode, at least from the point of view of seismic hazard analysis, would seem to be that *ignorance should not be equated with bliss!* There are lots of seismic sources throughout the world that we do not yet understand very well, and specifically within the United States, and we should be careful not to downgrade their potential impacts simply because of our ignorance!

SUMMARY AND CONCLUSIONS

Let us return to the question of whether recent earthquakes have modified our approach to seismic hazard assessment, and now particularly with regard to the question of deterministic vs. probabilistic methods. I've emphasized that, in years long prior to their destructive earthquakes, both the Northridge area in California and the Kobe area in Japan were identified by geologists as prone to earthquakes of the magnitudes that did, in fact, subsequently occur. But we must face the reality that these early assessments were not as helpful, particularly to government officials, as if we had been able to attach some element of likelihood of earthquake occurrences there relative to *other* areas for which concern had *also* been expressed, and in which so-called active faults were *also* known to be present. How, for example, were Japanese officials and scientists to recognize that the Kobe area was, in fact, at least as deserving of their attention as the Tokyo and Tokai regions, where much of their attention had instead been focussed? And how were California officials and scientists to recognize that the Northridge area was, in fact, at least as deserving of their attention as members of the San Andreas fault system on which much of *their* attention had instead been focussed? These are difficult questions.

In fairness, I should remind you that both the Kobe and Northridge areas were, in fact, shown on official maps as being in their respective country's zone of highest potential seismic risk, at least for ordinary structures. But that, in itself, was clearly not enough, and I would only emphasize that the probabilistic approach to hazard assessment—however imperfect it may be at this time—represents the only realistic hope of allowing societal efforts in the future to be *concentrated* and *prioritized* in a quantitative, scientifically defensible, and socially equitable manner. This is, in my mind, one of the important lessons of recent worldwide earthquake occurrences.

Now, I've talked at some length about of problems and uncertainties, but I would like to close on a more general up-beat note. There's no question in my mind that the so-called "earthquake problem" is a *solvable* one, although we clearly have a very long way to go to reach that objective. And there's little doubt that we'll have more surprises—and more disasters—along the way, although hopefully with decreasing frequency. That is, we *can, in time*, identify our dangerous seismic areas and quantify the hazard for effective land-use planning and for providing the bases for suitable building codes. And we *can, in time*, confidently build structures to whatever degree of seismic safety that we deem appropriate, without, I am told, undue economic penalty. I'm not so sure, frankly, that I can be similarly confident for some other natural hazards such as tornadoes and hurricanes.

Thus I remain an optimist that earthquake-prone areas of the world can, albeit in the very long run, be among the safest of places to live and work. Thank you.

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