SSA Presidential Address
Peter Shearer

▲ SSA President for 2018 Peter Shearer.

SSA was founded following the 1906 San Francisco earthquake for the "acquisition and diffusion of knowledge concerning earthquakes … and to enlist the support of the people and the government in the attainment of these ends." These are worthy goals, but how best to achieve them is not always clear.

When people learn that I am a seismologist, they usually are excited, both because earthquakes are very interesting, but also because they want to know when the "Big One" is coming. This is awkward because I need to explain that earthquakes are generally unpredictable and I have no inside knowledge of impending events. They seem to accept this, but seismologists often struggle with how best to communicate the value of our science, while acknowledging the large uncertainties in earthquake risk to policy makers and the general public. There is tension between what is expected of us and what we realistically can provide, given our incomplete knowledge and imperfect models of earthquake occurrence.

There is value in basic research into earthquakes and Earth structure. But much of the support for seismology and seismic monitoring programs is motivated by concern about the destructive potential of earthquakes. Interest and even funding are sometimes driven by news reports that sensationalize and exaggerate the risk of earthquakes in some regions. How should we respond? Accuracy is important and we must as scientists work with journalists to get their stories right. But make no mistake—-the risk of earthquakes is very real, both here in the United States and abroad.

Projections of future damage and deaths from earthquakes require earthquake probability forecasts, ground-motion prediction equations, and population exposure and vulnerability data. Each of these factors has considerable uncertainty. Luck plays a big role. For example, Christchurch was unlucky in 2011 to have a magnitude 6.2 earthquake strike so near the city and Nepal was unlucky to suffer a magnitude 7.8 earthquake in 2015, but fortunate that the shaking at Kathmandu was less than average from such a big nearby earthquake. But in the long run, both past history and future projections tell us that many deaths are likely. For example, the work of Roger Bilham, Bob Yeats, and others has alerted us to the potential for million-fatality earthquakes in parts of the world with high population density and poorly built structures. Because we can't predict these earthquakes, our most useful role is to encourage better construction practices and show that they are cost-effective. This effort is handicapped by poverty and inefficiency in many communities and the fact that their most recent large earthquake may have occurred centuries ago, so that many people are unaware of the risk.

It's human nature to care more about people we know and statistics can be numbing and overwhelming. Simply tabulating hundreds of thousands of deaths from war, disease, and natural disasters doesn't have as much emotional impact as smaller tragedies that engage us with their personal details. This is why audiences cheered NASA's questionable decision in the movie, *The Martian*, to spend billions of dollars on a risky mission to save a single stranded astronaut. And this is why we should continue to tell the stories of individual tragedies—of the elderly couple that died because of the shoddy construction of their apartment building, of the family with twin daughters that perished because their home was built in an area known to have frequent landslides, and of the migrant worker crippled by falling bricks from an unsecured building facade. These stories can convey the terrible consequences of earthquakes, and the importance of mitigation efforts, in ways that raw numbers cannot.

Short-term prediction of specific earthquakes is not currently possible and there are plausible theories that suggest it may never be practical. The long history of failed earthquake prediction ideas should serve as a cautionary warning to us, and promote skepticism and clear thinking. We must guard against the human tendency to see apparent patterns in random processes, as experiments have shown that our intuition will often mislead us. But caution is also warranted in drawing any firm conclusions about earthquake predictability because there are many aspects of earthquake behavior that we don't yet fully understand, such as foreshock sequences, the role of slow slip and tremor, the fine-scale structure of faults, and the time scale of earthquake nucleation.

The lack of clear short-term precursors to earthquakes has motivated longer-term forecasts of earthquake and ground motion probabilities. Examples include Probabilistic Seismic Hazard Analysis (PSHA), and Operational Earthquake Forecasting, which includes a time-dependent component. As research tools, these models can help illuminate the relationships between earthquake occurrence rates and previously cataloged seismicity, locations of known faults, paleo-seismic constraints, and geodetic measurements of strain rate. But these models can also be used to generate seismic hazard maps and this has led to some controversy regarding the validity and value of these forecasts. Critics have pointed out that several recent large earthquakes have occurred in regions that were deemed to have relatively low hazard while earthquakes did not occur in high-hazard regions.

A traditional approach in science is to define a hypothesis and then perform an experiment to test for the statistical significance of the results. Ideally, properly assessing the validity of earthquake forecasts requires defining the test criteria *before* the test period and then waiting until enough time has passed to obtain a valid conclusion. In practice, however, this has not been done by either the proponents or the critics of current hazard forecasts. This highlights the value of the Collaboratory for the Study of Earthquake Predictability, or CSEP, a program started by the Southern California Earthquake Center that rigorously tests forecast models against future observations. This provides a framework for objective comparison among competing models and also forces us to define exactly how the success or failure of model predictions should be assessed, which is a surprisingly complicated issue. However, obtaining statistically significant results will take a long time because of the low occurrence rate of the large earthquakes that are of greatest concern. Thus, definitive conclusions are not yet possible concerning the relative performance of prominent hazard models compared to much simpler models of earthquake occurrence. But results so far suggest that their probability gain, if any, is relatively small. This implies that uncertainties in current forecasts are very large, a conclusion that I suspect both model proponents and critics would agree is true, even if these large uncertainties are not always communicated clearly to policy makers.

Well-formulated critiques of the hazard forecasts have a useful role because they force discussion of the problems in the models, clarification regarding how they are to be tested, and ultimately lead to better models. The limitations of our current forecasting models should motivate two things—to improve the models, but also to plan for the unexpected. In the real world, the problem with seismic hazard models is not so much that they overpredict the hazard in some regions, but that they may give false reassurance in other regions. We know that there is real hazard across the eastern United States—the historical record of large earthquakes tells us this. The question regarding to what extent the hazard may be even greater in the vicinity of the New Madrid Seismic Zone because of the major earthquake sequence there about 200 years ago should not distract us from this fact. Poor construction, including unreinforced masonry buildings, is a problem everywhere and should be a source of concern to local officials and the public across the U.S. Similarly, in designing new structures, models of ground motions based on PHSA should not allow engineers to ignore the possible effects of anomalous motions that lie outside the model predictions.

Making testable predictions is critical for science, and models need to be quantitative and precisely defined in order to be testable. But the challenge for seismic hazard models is that the parts that are easily testable—how well they explain past seismicity—don't necessarily tell us how well they will perform in the future. And we can't afford to wait hundreds of years to see which models are most accurate. We need to accept a large amount of uncertainly regarding the accuracy of our earthquake probability forecasts, even as we continue to refine them based on all available constraints. However, such humility concerning our models should not inhibit us from speaking out forcefully about earthquake risk. Poorly constructed buildings should be retrofitted regardless of whether the chance of damaging shaking is estimated to be 10% or 20% in a given period.

I will close with two somewhat contradictory thoughts about hazard mapping efforts.

We must be careful to avoid believing in models simply because they appear reasonable. The seismic gap hypothesis, that is, that earthquakes are more likely to occur on fault segments that have not ruptured in a long time than on parts that recently experienced large earthquakes, is intuitively sensible. However, so far forecasts based on this idea do not work as well as random- or clustered-event models in explaining earthquake occurrence. But we also should not dismiss physical insights completely in forecasting earthquake behavior. I suspect that most seismologists would assess the probability of another magnitude 9 earthquake in the same location as the 2011 Tohoku earthquake as low compared to other subduction regions, based on the fact that most of the accumulated stress has been released and it will take many years for it to come back. Most of us would be more surprised at a repeat of the 1906 San Francisco earthquake than a big event on the southernmost San Andreas Fault, where a major quake has not occurred for over 300 years. Thus, most of us effectively believe that the seismic gap hypothesis still has some validity. The problem is that confirming this belief will take decades if not centuries, given the fortunately low rate of occurrence of M 8 to 9 events, and we don't have the luxury of waiting that long. Our models must reflect our best estimates of constraints derived from the underlying physics, which includes strain observations and the locations of known faults, even if rigorously testing these beliefs is not possible for many years. This is how science progresses.

But I also worry that over-reliance on models and statistics in earthquake forecasting can blind us to larger truths. Ideally models should be well-defined, transparent, and reproducible. Models that evolve toward ever-more complex edifices of assumptions risk becoming irrelevant to the broader research community and misleading or opaque to policy makers and the public. We should accept that future events are likely to surprise us and prepare for the unexpected with resilient planning and engineering. We should worry about the effects of a magnitude 7 earthquake in a currently aseismic region far from known faults. We should consider what will happen to buildings if a future earthquake involves longer-duration shaking or a larger displacement pulse than current models predict. Lives, and not just abstract theories, are at stake.

Thank you.