Reply: Extraordinary Claims Without Extraordinary Evidence

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Carl Sagan observed that "extraordinary claims require extraordinary evidence". In our view, Frankel's [2003] arguments do not reach the level required to demonstrate the counterintuitive propositions that the earthquake hazard in the New Madrid Seismic Zone (NMSZ) is comparable to that in coastal California, and that buildings should be built to similar standards.

This interchange is the latest in an ongoing debate beginning with Newman et al.'s [1999a] recommendation, based on analysis of GPS and earthquake data, that Frankel et al.'s [1996] estimate of California-level seismic hazard for the NMSZ should be reduced. Most points at issue, except for those related to the costs and benefits of the proposed new IBC2000 building code, have already been argued at length by both sides in the literature [e.g. Schweig et al., 1999; Newman et al., 1999b, 2001; Cramer, 2001]. Hence rather than rehash these points, we will try here to provide readers not enmeshed in this morass with an overview of the primary differences between our view and that of Frankel [2003].

These differences are essentially philosophical differences about how to forecast and prepare for a future natural hazard about which much is not well understood. We think it desirable to accept and show humility in the face of the complexities of nature, admit the limitations of our knowledge, and help the public use this information to make a complex set of choices to which there is no unique or correct answer.

First, we believe that "haste makes waste" - that the proposed upgrade of the building code for Memphis and surrounding areas to a California level should not be implemented unless the large resulting costs are shown to yield commensurate benefits. This is the issue of immediate practical concern. Although the seismological issues are likely to remain unresolved for hundreds of years, upgrading building codes is under active consideration.

Key to our position is our view that the seismic hazard in the NMSZ is significantly less than in California, based (contrary to Frankel [2003]) on the combined effects of the relative seismicity and resulting ground motion. As illustrated in our paper, NMSZ earthquakes of a given magnitude are about 30-100 times less frequent than in southern California, but produce larger ground motions which reduce the effect of the rate difference by a factor of 10. Hence the earthquake hazard in Memphis is about 1/3 - 1/10 that in California. This estimate agrees with FEMA's estimate of annualized earthquake loss ratio (AELR), the ratio of annualized earthquake loss to the replacement cost of all buildings in the area. These values for Memphis and St. Louis, derived from Frankel et al.'s [1996] maps, are about 1/5 - 1/10 of for San Francisco and Los Angeles. This difference is illustrated in Figure 1, which contrasts the fractions of the regions that might be shaken strongly enough to seriously damage some buildings. In 100 years (upper panels) much of the California region will be shaken seriously, whereas a much smaller fraction of the NMSZ would be. After 1000 years (lower panels), much of the NMSZ has been shaken once, whereas most of the

California area has been shaken many times. Although the maximum shaking at a given location in the NMSZ over thousands of years may be comparable to that in California, a building in California is much more likely to be seriously shaken during its approximately 50-year life. Thus over the life of a new building in Memphis there is a reasonable probability of low to moderate shaking, but a significantly lower probability of severe shaking.

We believe that our initial cost/benefit estimate, which is the first to explore the issue, is reasonably robust. Our paper used a present estimate, criticized by Frankel [2003], showing that FEMA's estimate of $17M in annualized earthquake loss for the Memphis area (again based on Frankel et al.'s [1996]) is about 1% of the annual construction cost ($2B in 2002; $1.7B in 1990). A similar estimate results from a life-of-building approach using FEMA's AELR estimate of 4×10^{-4}, which corresponds to the annual fractional loss in value of a building due to earthquakes. Assuming that this loss is halved by the new code, than over 50 years a building loses 1% of its value. This estimated benefit is small compared to the cost of the new code, which seems likely to raise seismic mitigation costs to about 10% of total building costs. Hence we recommend that these issues be explored via detailed cost/benefit analysis that evaluates various strategies, including retaining the present code which provides the level of seismic safety recommended in the 1999 Standard Building Code. An important issue for such analysis is whether buildings should be designed only to prevent collapse and ensure life safety after a major earthquake, which has been the traditional standard, or whether they should be designed to a higher but costlier standard to ensure functionality, which the new code requires in some cases.

Second, we disagree with Frankel [2003] that a consensus among seismologists and engineers favors either a California-level hazard or California-level construction. This may be the consensus of those involved in developing the hazard maps and code, but we have found that both propositions are regarded skeptically by many in the broader community of seismologists and engineers. For example, the Memphis-based West Tennessee Structural Engineers Association has recommended not adopting the new code unless its costs can be justified, and alternatives to the hazard map methodology and the proposed code are under discussion [Hwang, 2002; Wang, 2002].

Third, we believe that the uncertainties involved in estimating earthquake hazards and probabilities for the NMSZ are so large that it is not presently possible to provide "unbiased seismic hazard information" [Frankel, 2003]. In our view, the crucial parameters are so poorly known that the results depend primarily on subjective parameter choices, and which one prefers is largely a matter of taste and preconceptions. As illustrated in our paper via several possible hazard maps, the predicted hazard depends on models that seek to describe the size and rate of large earthquakes and the resulting ground motion. A wide range of values for each has been proposed, as discussed both in our papers [Newman et al., 2001] and by Frankel [2003] and Hough [2003]. For example, Frankel et al.'s [1996] model, which was developed for the 1996 hazard calculations, predicts much larger ground motions than other models. Although models can be combined by using logic trees with subjective weights, the results will continue to change as opinions shift and new data become available. Hence the different and changing views on the magnitude and recurrence of the largest earthquakes and the ground motion illustrate that the real uncertainties in our knowledge are large. Moreover, the real validity of the models and resulting maps will remain untested until their predictions are compared to seismological observations of one (and probably more) future 1811-12 style earthquakes.

These points also apply to estimates of earthquake probabilities. Contrary to Frankel [2003] we do not favor any particular model, but simply show (as he admits) that the estimated probability depends significantly on the choice of model and parameters. At present, basic questions such as whether earthquake probabilities are constant or vary with time since the last major earthquake, and what probability distributions are suitable, are unresolved and the subject of active debate. The challenge is illustrated [Savage, 1993] by the non-occurrence to date of the Parkfield earthquake that was predicted in 1985, based on a probabilistic model, to have a 95% probability by 1993. Hence in California, where the earthquake record is better, Savage [1991] argued that earthquake probability estimates are "virtually meaningless." We think the situation is worse for New Madrid.

Fourth, we believe that the public should be presented with full discussion of these uncertainties to permit informed discussion of various alternative hazard mitigation strategies. These matters are too important to be left to "experts", even if such individuals exist given our limited knowledge. We think there are two reasons for not doing so. First, we believe that better policy decisions emerge from the "tough love of democratic discourse" [Pielke et al., 2000] than from a top-down approach. Second, local communities are impacted by the necessary policy choices. They, not the federal government, will bear most of the costs and hence should make the difficult tradeoffs on issues like strengthening schools versus hiring teachers and upgrading hospitals versus treating indigent patients. For example, as noted by the Memphis Commercial Appeal (May 29, 2003) "Don't expect state and local governments to follow Washington's lead at the Memphis Veterans Medical Center, which is undergoing a $100 million retrofit to protect the building against the potential threat of an earthquake."

In conclusion, we think it useful to stand back from the hotly-debated details of the New Madrid controversy, and recognize that the larger issues arise in any attempt to predict natural hazards far in the future and develop mitigation strategies. For example, Oreskes [2000] argues that:

"Forecasts of events in the far future, or of rare events in the near future, are of scant value in generating scientific knowledge or testing existing scientific belief. They tell us very little about the legitimacy of the knowledge that generated them. Although scientists may be enthusiastic about generating such predictions, this in no way demonstrates their intellectual worth. There can be substantial social rewards for producing temporal predictions. This does not make such predictions bad, but it does make them a different sort of thing. If the value of predictions is primarily political or social rather than epistemic, then we may need to be excruciatingly explicit about the uncertainties in the theory or model that produced them, and acutely alert to the ways in which political pressures may influence us to falsely or selectively portray those uncertainties."

"As individuals, most of us intuitively understand uncertainty in minor matters. We don't expect weather forecasts to be perfect, and we know that friends are often late. But, ironically, we may fail to extend our intuitive skepticism to truly important matters. As a society, we seem to have an increasing expectation of accurate predictions about major social and environmental issues, like global warming or the time and place of the next major hurricane. But the bigger the prediction, the more ambitious it is in time, space, or the complexity of the system involved, the more opportunities there are for it to be wrong. If there is a general claim to be made here, it may be this: the more important the prediction, the more likely it is to be wrong."

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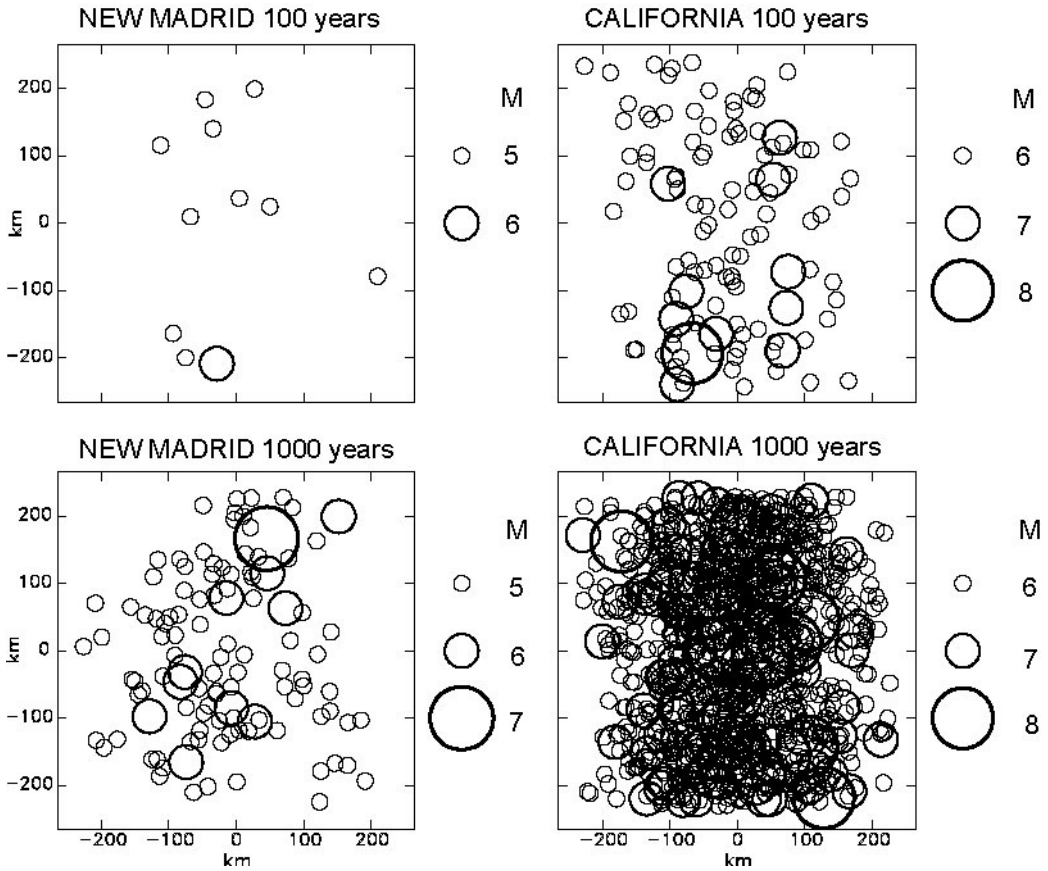

Figure 1: Schematic comparison of seismic hazard for the NMSZ and southern California on two time scales. Seismicity is assumed to be random with California 100 times more active but New Madrid earthquakes causing strong shaking over an area equal to that for a California earthquake one magnitude unit larger, shown by circles marking areas of shaking with acceleration > 0.2g.

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