Comment on the paper

Layered seismogenic source model and probabilistic seismic-hazard analyses in Central Italy

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Pace *et al*. (2006) define a seismogenic model aimed to provide a (time-dependent) seismic hazard assessment for Central Italy. One of the main novelties of the model is that it introduces seismogenic structures (faults) in seismic hazard calculations, and it imposes a time behavior to these structures, leading to a time-dependent seismic hazard assessment. In my opinion, the approach used in this paper (i) does not explain the "observed" time distribution of historical earthquakes (one of the few empirical evidence that can be used to check the reliability of any seismic hazard or earthquake forecasting model), and (ii) uses values of parameters of the statistical distribution that are clearly unreliable. These problems introduce significant biases that potentially make the hazard assessment made by the authors less reliable that the one obtained by more traditional approaches. In the following, I discuss in detail these points. I focus the attention only on earthquakes with M 5.5+ that are considered "destructive" for the Italian territory. Sometimes, for the sake of conciseness, I drop the specification of the magnitude of events that has to be always considered equal to or larger than 5.5.

Traditionally, seismic hazard and earthquake forecasting in Italy are based on regular spatial grids or on the definition of seismogenic provinces (Slejko *et al.,* 1998; Meletti *et al*., 2000; Scandone and Stucchi, 2000; Gruppo di Lavoro, 1999, 2004*;* Albarello *et*

al., 2000; Romeo *et al*., 2000; Lucantoni *et al*., 2001; Faenza *et al*., 2003; Cinti *et al*. 2004). This choice has two main rationales; first, despite it is commonly accepted that almost all earthquakes occur on faults that have been active before, a large number of seismogenic structures responsible for significantly damaging earthquakes (M 5.5+) do not have any noticeable superficial structure; second, presently there are not enough clues to determine (empirically and theoretically) the time behavior of each seismogenic structure.

Pace *et al*. (2006) select a specific region of Italian territory, where they assume a "good" completeness of the seismogenic structures identified. In particular, they recognize 28 structures and almost arbitrarily associate to them each past M 5.5+ earthquakes. The authors evaluate such completeness through a visual inspection of "geological" and "historical" rates. Here, I do not deepen the completeness of the "active faults catalog" that is obviously of primary importance in seismic hazard perspective, and it represents a hotly debated issue also in other tectonic regions. Despite I think that more rigorous tests and analyses are needed to check the reliability of this pivotal hypothesis, here I take it for granted, and only remark that the completeness hypothesis implies that all the future M 5.5+ earthquakes have to occur on the seismogenic structures reported by Pace *et al*. (2006). This implication will allow the completeness of the "active faults catalog" and, therefore, one of the basic assumptions of the model to be validated quantitatively in the next future.

My first main concern in this comment is related to the time behavior assumed for each seismogenic structure. In particular, the authors assume that seismogenic faults behave independently one from the others. In my opinion, the assumption of

independent seismogenic structures is strongly questionable, because it is not in agreement with empirical constrains (i.e., the real earthquake catalog).

Specifically, it is worth remarking that the choice of the statistical distribution on a single fault can be presently only hypothesized, because there are not enough data on each structure to test statistically any hypothesis. On the other hand, the time distribution of earthquakes reported in the historical seismic catalog provide a useful (and unique) empirical piece of evidence that allows the reliability of any earthquake model to be checked statistically. Recent nonparametric analyses (i.e., analyses performed without imposing any time structure to the data; see Faenza *et al*., 2003; Cinti *et al*., 2004) show that the time occurrence of earthquakes with M 5.5+ in Italy in the last four centuries is dominated by a time clustering that lasts few years (therefore, a time clustering longer than the one typical for aftershock sequences), and it is followed by earthquakes distributed as a Poisson process. Notably, comparable time evolution has been also found in the worldwide earthquakes distribution (Kagan and Jackson, 1991). All these analyses consider events inside regular spatial grids and seismotectonic regions, i.e., areas that contain different seismogenic structures. There is a general consensus in viewing this time clustering as due to some sort of interaction between structures inside (and between) seismic regions (e.g., Stein *et al*., 1997; Faenza *et al.* 2003; Santoyo *et al*., 2005). In the earthquake forecasting perspective, this implies that, in a time scale of years, the interaction between structures could be one of the predominant factors governing earthquakes occurrence. In other words, the calculation of the probability of occurrence on each single fault requires to account for the time and distance of last earthquakes and the mechanism of the physics interaction, not only the elapsed time since the last earthquake on the

structure considered, and the statistical distribution of the inter-event times as made by Pace *et al*. (2006). In their calculation, in fact, the authors assume that seismogenic structures behave independently one from the other, neglecting any interaction between faults; in this view, there is no way to reproduce the time clustering observed in seismic regions, by using both Poisson and/or Brownian Passage Time (BPT) models proposed by Pace *et al*. (2006).

My last comments are related to the parameters estimation. The authors estimate Gutenberg-Richter parameters by using a least squares method applied to the cumulative form of that law. Actually, this procedure produces significant biases and strongly underestimated uncertainty (e.g., Sandri and Marzocchi, 2006). However, my main doubt that I discuss here is related to the estimation of the parameter α of the BPT distribution, that represents the coefficient of variation, i.e., the ratio between standard deviation σ and average μ of inter-event times (time interval between consecutive earthquakes). Let us assume for simplicity, that BPT is a satisfactory distribution for the time behavior of each single fault. Pace *et al*. (2006) do not have inter-event times from single structures, therefore they calculate the mean and standard deviation from estimations of the mean recurrence time μ obtained through different techniques (see section *"Seismogenic boxes: medium-to-large earthquakes linked to individual structures"* in Pace *et al*., 2006). This is clearly a mistake, because the standard deviation calculated in this way is, at best, the standard deviation of the mean μ (indicated as σ_μ), and therefore it has nothing to do with the standard deviation of the inter-event times (indicated as σ). The parameters σ and σ_μ are usually very different, because the former gives the variability of the inter-event

times, while the second quantifies the variability of the estimation of one parameter of the distribution (the average).

I conclude my comment with a philosophical remark on seismic hazard assessment. The great importance of this scientific issue is due to its practical implications for society. In this perspective, it is fundamental that seismic hazard assessment is "accurate" (i.e., without significant biases), because a biased estimation would be useless in practice. On the other hand, seismic hazard assessment may have a low "precision" (i.e., a large uncertainty) that would reflect our scarce knowledge of some physical processes involved, from the preparation of a damaging earthquake to the derived ground acceleration to a specific site. It is my opinion that an accurate seismic hazard assessment could be realistically achieved by using some sort of "best picture" of the shared state-of-the-art. In fact, the use of commonly accepted and verified models allows the potential bias associated to personal convictions to be minimized. The inclusion of more sophisticated models that are not yet properly tested (as the one proposed by Pace *et al.*, 2006) certainly increases the precision, but they can introduce a significant bias making the estimation highly inaccurate.

Finally, I have no doubts that new clues of the spatial distribution and of the mechanical behavior of seismogenic structures can potentially lead to significant steps ahead in seismic hazard assessment; at the same time, for the reasons mentioned in this comment, I do not believe that the model proposed by Pace *et al*. (2006) allows these steps to be done in the right direction.

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