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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A detailed seismic zonation model for shallow earthquakes in the broader Aegean area" *by* D. A. Vamvakaris et al.

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REVIEW BY DARIO SLEJKO The paper presents a new seismogenic zonation for the Aegean region which is quite new and differs from those available in the literature, also published by the same authors of the present paper. Although the zonation itself is well documented and supported by good data, some also new, some aspects are not well convincing, mainly about some methods applied. GENERAL 1) Although the authors state that they use a wide variety of data, from geology to geophysics and seismology, the zonation is substantially based on the information from focal mechanisms. It is well known that different types of focal mechanisms can be in agreement with the regional stress field and, consequently, it is not the type of mechanism important but the

stress tensors to identify the tectonic regime. A detailed analysis based on stress tensor inversions should have been the proper way to identify homogeneous areas, better than the qualitative study of the focal mechanisms. Moreover, no information from recent studies based on seismic surveys (e.g., papers by Makris in the reference list) are considered and only one geological source (neotectonic map of Greece) is taken into account (see e.g., additional possible papers in the reference list). In addition, no geodetic data are properly evaluated. A kinematic model explaining the present movements of the region, and then justifying the seismicity, to be used as starting point for the seismic source identification is missing. For all these reasons, I see this zonation as coming mainly from focal mechanisms only: this should be clearly stated in the paper. 2) No comparison is presented with other zonations, sometimes very different (see e.g., Seahellarc Working Group, 2010), available for the region and especially with the database of the Greek seismic sources (Caputo et al., 2013). 3) Although the present zonation refers to shallow earthquakes, seismic sources used for hazard assessment are often modelled as 3D planes: this aspect is not treated in the paper. 4) The treatment of seismicity is rather questionable. No historical analysis is considered for the estimate of the completeness periods before 1981 as, the authors say, "extensively studied by Papazachos et al. (2000)". The presence of some very narrow sources reduces the possibility of obtaining robust estimates of the GR parameters. For this reason, the b-value in literature is generally computed merging together some close sources with seismotectonic similarities. The application of the least-squares method for the computation of the GR parameters is formally wrong, mainly because the GR parameters are computed considering the cumulative number of earthquakes, not on independent samples, then, as requested by the least squares method. Conversely, the maximum likelihood method is suitable (see Weichert 1980 among many). 5) Some statistical parameters presented in the paper, as a1, Tm, Mt, do not refer to zonation but better to hazard. They are useless in this paper. Conversely, Mmax, based also on tectonic information, is important to characterize a seismic source but it is not cited in the paper. 6) Almost half of the cited papers refer to scientists of the same institutes

of the authors. As some papers are hardly available to the international audience, they could be easily eliminated. DETAILED Page Line(s) Remark 6722 3-14 The zonations cited are geographical and not seismological: this part is useless. 6724 10-12 I do not see how a new zonation can reduce the epistemic uncertainties. 6738 25 195 is probably 1950.

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