

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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In the following we provide our reply to the Interactive comment on “A detailed seismic zonation model for shallow earthquakes in the broader Aegean area” by D. A. Vamvakaris et al., sent by Dario Slejko.

We would like to thank Dr. D. Slejko for his thorough review of the manuscript and his helpful comments.

Reply to the general comments (following the order of the comments):

1) The Referee states that in our paper is substantially based on the information from focal mechanisms. We think that this assessment does not reflect the content of our

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work, as our study is not based only on fault plane solutions, which simply form an ingredient of our analysis. The local stress field derived by the analysis of the corresponding P and T principal kinematic axes was also taken into account and the azimuthal distribution of these axes helped to divide the earthquakes into homogeneous type of ruptures related with the seismic events. On the other hand, major active faults and rupture zones were also considered for the separation of the study area in smaller, seismotectonically coherent area, in order to create the new seismic zonation model. Two different types of data were used; the one with seismic faults (Papazachos et al., 2001) which relates known active faults with strong earthquakes in Greece since the 6th BC century and the other with active neotectonic faults in the broader area of Greece (Mountrakis et al., 2010) as a result of the combination of a large number of local or general neotectonic studies with active faults exhibited a significant surface fault trace. These two different approaches for the faults database (seismic and neotectonic) act parallel and contribute to the new proposed seismic zonation model. The Referee proposed a number of studies mainly related with local faults (e.g. Corinthiakos gulf, Saronikos gulf, Pylos, Parnitha mt.) that mostly focus in detail in a small study areas. This very local scale of study was out of the main target of our work, since our study area concerns the largest part of Balkan Peninsula. Moreover, the neotectonic fault database used (Mountrakis et al., 2010) has already incorporated the most important parts of the specific local faults of the papers proposed by the Referee. Finally, apart from fault plane solutions, principal stress P and T axes, seismic and neotectonic faults, the most important ingredient of our analysis was the historical and instrumental seismicity which was also considered. Seismicity, in terms of geographical distribution of epicenters and magnitudes, was studied in details and contributed to the division of the proposed seismic zonation model.

2) For the Seahellarc zonation, the Referees' comment is practically identical to comment number 5 of the Referee #1 (Anonymous Referee), hence we would like to kindly redirect Dr. Slejko (Referee #2) to the corresponding section of this reply.

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Concerning the database of the Greek seismic sources (Caputo et al., 2013), these authors recently presented a new work on fault zones, collecting information on neotectonic and seismic faults. The largest part of the major faults referenced in this work had been already incorporated in our dataset, as earlier explained.

3) In the present work we do not examine the depth distribution of the shallow seismicity in Greece, hence no information of the 3D seismicity distribution is included in the revised manuscript.

4) The Referees' comment is practically identical to comments number 2 and 3 of the Referee #1 (Anonymous Referee), hence we would like to kindly redirect Dr. Slejko (Referee #2) to the corresponding sections of this reply.

5) Referee noticed that some statistical parameters presented in the manuscript as a_1 , T_m and M_t are not related to zonation but to hazard and for this reason they are useless in our paper. Initially, we consider that a_1 is a typical seismicity (not hazard) parameter, for each zone, since it is the reduced value which refer to the reduced time period, t , of complete data and the seismic zone surface, S , for a typical area of 10000 km², according to the simple well-known relation $a_1 = a - \log(tS) + 4$. Moreover, we employed several typical seismicity measures such as the mean return period, T_m , of earthquakes with magnitude greater or equal to a specific magnitude, M , and the most probable maximum magnitude, M_t , observed for a specific time period, T , in order to illustrate the spatial variation of seismicity measures for each proposed zone of the study area. Such measures (T_m and M_t) are of course indirectly affecting the seismic hazard, however at the same time they provide a direct insight on the detailed distribution of seismicity in each seismic zone of the study area, and could be considered as characteristic seismicity quantities for each seismic zone.

6) Actually, almost half of the cited papers refer to scientists of the same institutes of the authors, but we do not think that this is a reason to reduce this list of references, since all of them are related with the specific parts of the paper. Concerning the "hardly

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available to the international audience" papers (the Referee probably means the papers written in Greek) this corresponds only 6 references from which 3 of them contains original fault plane solutions, necessary to our FPS dataset. Another paper is the main work containing the major neotectonic faults and the other is the published PhD thesis of the first author which provided the revised magnitude estimates for the present paper. We think that none of these papers can be easily eliminated from the revised manuscript.

Reply to the detailed comments:

1) Referee notes that the zonations cited are geographical and not seismological, so that the corresponding part is useless. We agree with the Referee in most cases, but there are some references that are really related to the presented work. More specifically:

a. Flinn and Engdahl (1965) actually proposed a geographical zonation, however they proposed it on a basis of a seismic regionalization system which closely follows the Gutenberg and Richter's (1954) system, so seismic regions are defined as combinations of geographical regions. In this term, we think that this reference is related to our work.

b. Flinn et al. (1974) and Young et al. (1996) made some update on the Flinn and Engdahl (1965) first regionalization, in the same term of geographical and seismic zones definitions, hence we considered these references as also useful.

c. Bird and Kagan (2004) and Kagan et al. (2010) indeed did not propose a seismic zonation model, but a geographical branching model for earthquake occurrences in terms of tectonic deformation for different tectonic regimes. Thus, we decided to eliminate these references, as the Referee proposed.

d. Stock and Smith (2002), same as before. The referee is correct and we removed this reference, as he proposed.

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Moreover, SEHELLARC Working Group (Slejko et al., 2010) developed a new seismogenic zonation in order to evaluate and compute seismic and tsunamis hazard and risk for SW Peloponnese (SW Greece). Hence we cited this work in our revised manuscript in the corresponding section for seismic zonation models for the Aegean area.

2) This comment was rather puzzling for us: Epistemic (systematic) uncertainty is broadly defined as the uncertainty which is caused by things that we could know (in principle) but actually lack adequate information (in practice). Essentially, it reflects our incomplete knowledge about a phenomenon that affects our ability to model it (e.g. National Research Council. Review of Recommendations for Probabilistic Seismic Hazard Analysis: Guidance on Uncertainty and Use of Experts. Washington, DC: The National Academies Press, 1997. In this sense, the new zonation essentially attempts to reduce our epistemic uncertainty, at the extent that it manages to describe the physical system of earthquake generation in an improved, more accurate manner. Perhaps the Referee can further elaborate on his comment, so that we can provide a better answer and response.

3) We adopted the technical correction marked by the Referee, in L. 25, p. 6738, where the correct year should be 1950.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 6719, 2013.