

Interactive comment on “Assessment of potential seismic hazard for sensitive facilities by applying seismo-tectonic criteria: an example from the Levant region” by Matty Sharon et al.

Anonymous Referee #1

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The paper presents two very different types of analyses – the ‘seismicity criteria’, based on the earthquake kernel density and the seismic moment density, both using a 35-year time-window. The other one is mapping of active or ‘capable’ faults, based on geological maps and literature review. While the analyses themselves seem professional and accurate, the overall context and connection is either unclear or even misleading in places. For example – the title of the manuscript: “Assessment of potential seismic hazard for sensitive facilities” is misleading and erroneous. The paper does not contain any hazard analysis, or a comparison to existing hazard assessments for the area. There is no discussion on how these results will affect hazard or any direct practical connection between the presented analysis and hazard calculations. Moreover –

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throughout the paper, surface rupture and ground shaking are intermixed as ‘seismic hazard’ and the fault mapping is presented as the answer for both. However – ground shaking and surface rupture are two very different types of hazard. They require different considerations in planning, etc. Is it wise to treat both as one? Seems to me that your mapping methodology is more appropriate for surface rupture analysis than for shaking (which also takes into account faults that did not rupture the surface, etc.). Please be more accurate in describing your contribution and its expected useage.

What is very much missing is a thorough discussion on the relationship between the two types of analysis (seismicity based criteria and faulting) – how do you suggest combining the two datasets that you have created ?

(1) In places where they overlap (e.g. DST), should they both be accounted for in the hazard analysis? If not – what should be the interaction ? (2) In places where they do not overlap (e.g. east Sinai), do you ignore the seismicity criterion? Do you add a ‘seismogenic zone’? What is your suggestion? (3) What about places in which the kernel density is zero? Do you think there is really a zero probability of an earthquake occurring there, keeping in mind the short time window used for the kernel density? These are all very important hazard decisions, which this paper does not address.

The abstract says: "our analysis allows revealing the tectonic evolution of a given region". Therefore, it is expected that you will show this later in the results. Nowhere in the paper do you "reveal" anything new about the tectonic evolution that wasn't already known. Therefore – please clarify what exactly is new knowledge gained by this paper? This is typically done by comparing to previous studies or discussing the specific contribution presented in this study.

Other comments:

Table 1: title of 2nd column should be ‘slip rate’ rather than ‘strike-slip’. Also, seems to me that the first slip rate that is mentioned for the Yammuneh fault is too low. It references Gomez 2007 but I think his numbers are higher. How exactly did you reach

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2.8 mm/yr?

Conclusion number 3 is not exactly a conclusion. It's an opinion, or a suggestion. While important and relevant, it isn't based on any analysis or data and hence cannot be presented as a conclusion of the paper. Please rephrase.

Line 296 – the symbol V_s is typically used for shear-wave velocity in the geotechnical earthquake engineering community. I suggest using something else for slip rate.

Line 454 – remove 'many'

Line 455 – 'could have entered the map' rather than enter

Line 460 – 'Quaternary activity exists'.

Line 462 – siting of what? What is siting? Why is this related?

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-67/nhess-2019-67-RC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2019-67>, 2019.