

## ***Interactive comment on “Calibrating a New Attenuation Curve for the Dead Sea Region Using Surface Wave Dispersion Surveys in Sites Damaged by the 1927 Jericho Earthquake” by Yaniv Darvasi and Amotz Agnon***

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We thank the reviewer for pointing out many importing issues. In order to be clear, the response structure is in the prescribed sequence:

- (1) Comments from Referee
- (2) Author's response
- (3) Author's changes in the manuscript

C1

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(1) One major question that arises from this manuscript is what is the contribution of the new GMPE? Can this GMPE be used for intensity prediction in the future?

(2) We thank the reviewer for pointing this out. The new GMPE is essential to emphasize how shear wave measurements are important in Israel and also in every modern GMPE. This work is preliminary. Therefore, the main contribution will be after collecting more data regarding additional earthquakes. For now, according to the new equation, 11 sites, which constitute 58% of our measured samples, move into the 60% prediction boundary. This suggests that the prediction boundary actually encompasses over 80% of the macroseismic observations. Hence, on the one hand, statistically, this new GMPE can be used. But on the other hand, in order to be a part of any standard, it should base on more data and also includes additional terms and not only a site condition term.

(3) We added a sentence in the conclusion section to clarify these points.

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(1) No sensitivity analyses were performed.

(2) We thank the reviewer for the point and we include the sensitivity tests in the new version.

(3) See page 8 (section 4.2). Also, fig11

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(1) No validations of the GMPE were performed.

(2) At the moment a moderate earthquake will occur in the area we will be able to validate the GMPE.

C2

(1) Can the authors compare macro-seismic data from other historical and perhaps instrumental earthquakes to their GMPE and discuss the results?

(2) This work focuses on the 1927 event and it is part of wider research which extends to additional potentially earthquakes. Certainly, comparing macro-seismic data from other historical events will enhance the attenuation relation and that is the future plan. Please keep in mind though that 1927 was the only destructive earthquake for which we have relatively reliable data.

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(1) The authors took a formerly derived GMPE, with its regression and constants, and simply added another term, regressing only for its constant. Such an action means the authors think that the magnitude, attenuation, geometrical spreading and site response are all independent. That is not fundamentally wrong, but should be stated.

(3) Had been stated in the new manuscript.

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(1) The authors adopt 760m/s as the Vs reference for their new GMPE, although this value is probably not suitable for Israel, as the Judean group is close to the surface in many regions, and its shear wave velocity is higher.

(2) We thank the reviewer for this point.

(3) This part had been corrected.

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(1) After developing their own site term, the authors try to explain why 8 points did not “converge into the prediction boundary” of their equation. They explain that Boore et al.’s GMPE is constrained to a distance of 70 km, and some their data is further away. This argument is weak, as they did not adopt Boore et al.’s equation, but Hough and Avni’s intensity equation, and only Boore’s site term, therefore using their distance con-

C3

straint does not seem to explain the misfit. Also – 5 sites beyond 70 km did converge to their GMPE.

(2) You are definitely right about using Boore et al.’s equation.

(3) We dropped out this explanation from the new manuscript.

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(1) The authors fail to discuss or at least mention other research regarding this exact earthquake, such as Kadmiel et al. (2015, SCEC). I think it would be interesting if the authors try to use the data from Zohar and Marco (2011), which attempted to correct the intensities of the 1927 EQ to site conditions, such as surface geology, slope and construction. Zohar and Marco’s work should at least be mentioned, as they did try to deal with the site response biasing the intensity reports.

(2) We mentioned the significant work of Zohar and Marco (2012 instead of 2011) and used their modern epicenter location for our calculation.

(3) We added a comment about Kadmiel work.

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(1) The manuscript should be English and grammar proofed before re-submitted.

(2) The original manuscript was English and grammar proofed.

(3) The new manuscript is English and grammar proofed.

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(1) The magnitude of the earthquake in the abstract and in the Introduction is different. Although the difference is small- please pick one.

(3) Had been corrected in the new manuscript.

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C4

(1) In the abstract and several more instances MASW is explained as Multi Analysis of Surface Waves.

(2) Some of the authors use it.

(3) Had been corrected in the new manuscript.

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(1) In the abstract and in several more instances the authors state that (line 15-16) “based on 1927 macroseismic data integrated with modern measurements”. This phrasing is somewhat misleading, since it hints that new seismic data is being used, where I believe the authors are referring to the velocity profile measurements they performed.

(2) Comment accepted.

(3) We redefined it in the new manuscript.

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(1) Abstract – line 25 –seismic hazard and not risk.

(2) Comment accepted.

(3) Had been corrected in the new manuscript.

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(1) Introduction – some opening paragraph is missing.

(2) Comment accepted.

(3) We added an intro section to the manuscript

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(1) Page 2 line 3 – how about source distance?

C5

(2) Comment accepted.

(3) Had been corrected in the new manuscript.

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(1) Page 2 line 10 – that is one definition of what amplification is, I think the authors should mention the other, common definition, relative to a near rock outcrop.

(2) Comment accepted.

(3) Had been changed in the new manuscript.

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(1) Page 2 lines 10-11 – site amplification can be attributed to many factors, such as basin effect, focusing effects, topography and on and on, and not only due to “reverberation of the seismic waves in the upper layers according to acoustic impedance differences”. The decrease in shear wave velocity when getting closer to the surface is alone a reason for amplification of magnitudes, without any resonance in the upper layers.

(2) Comment accepted.

(3) Had been changed in the new manuscript.

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(1) Figure 1 is somewhat “overcrowded”.

(2) Comment accepted.

(3) Had been changed in the new manuscript.

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(1) Page 2 lines 17-20 – this should come earlier in this section.

C6

- (2) Comment accepted.
  - (3) Had been changed in the new manuscript.
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(1) Page 2 lines 26-27 – several claims were made as to how is Vs30 suitable for Israel (i.e. Zaslavsky et al. 2012, Natural Science).

(2) Although Zaslavsky et al. (2012) claimed that the use of Vs30 is a simplification that cannot be justified in the complex geological conditions as in Israel, no alternatives were proposed. Therefore, in this scenario, the Israel Standards Institute still adopts the Vs30 parameter.

(3) We clarified this adding a statement to the manuscript.

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(1) Where is the Hough and Avni (2011) reference in the list?

(2) Comment accepted.

(3) Had been changed in the new manuscript.

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(1) Vs30 notation is not consistent throughout the manuscript.

(2) Comment accepted.

(3) Had been changed in the new manuscript.

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(1) Is the equation of Hough and Avni based on Bakun (2006) (page 3 line 28) or Bakun and Wentworth (1999) (page 5 line 2-3) or both, and where are these references in the list?

#### C7

(2) The equation is based on Bakun and Wentworth (1997).

(3) References had been updated to the new manuscript.

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(1) Page 50 line 6 – what do the authors mean by 60% prediction boundary? Is that the boundary for which 60% of the data is included within? Unclear.

(2) Exactly.

(3) We redefined it in the new manuscript.

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(1) I'm not sure that section 4.1-4.2 belong in the discussion section. They seem to be more suitable in the methods section, since they are part of the modeling method.

(2) We agree about that.

(3) We moved this section to the proper section in the new manuscript.

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(1) Page 5 lines 17-18 – what kind of data from the GII was yours compared to? This is very unclear. Is it borehole data? Refraction? HVSR? Is there a reference to the data?

(2) The GII report (Aksinenko and Hofstetter, 2012) present all available geological, topographical, geophysical, geotechnical and borehole information to identify site-specific characteristics. This includes refraction and borehole.

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(1) Figure 10 – for which site are these results?

(2) This figure refers to Beit Alfa site.

(3) We clarified it in the new manuscript.

#### C8

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- (1) Page 7 line 3 – Hough’s name is spelled wrong. Also the citing is not complete.  
(2) Comment accepted.  
(3) Had been corrected in the new manuscript.

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- (1) Page 7 line 9 – Boore’s equation is equation (4) and not (2). Also citing not complete.  
(2) Comment accepted.  
(3) Had been corrected in the new manuscript.

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- (1) Figure 11 – the legend is not clear – does the curve represent the new GMPE, with the site term? Also I believe there is no need to use different symbols for amplified and de-amplified, they can be put in the same category as “MMI before site correction” or similar. Same goes for figure 7 – there is no need to use different colors for the symbols, and the use of the term “amplified” or “de-amplified” is not accurate – amplification is relative to reference rock conditions, and here I believe you mean the sites were “amplified” comparing to the GMPE.  
(2) Comment accepted.  
(3) Had been changed in the new manuscript.

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- (1) Page 7 line 15 – again Boore’s equation is #4.  
(2) Comment accepted.  
(3) Had been corrected in the new manuscript.

C9

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- (1) It would seem to be useful if the authors use index numbers to identify their measurement locations in the figures, as they are numbered in table 1.  
(2) We thought over this but unfortunately, this leads to “overcrowded” figures.

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- (1) In table 1 include the epicentral distance of the measurements. This will allow the reader to understand the statements they make regarding the misfit of the different locations.  
(2) Comment accepted.  
(3) Had been added in the new manuscript.

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-52>, 2018.

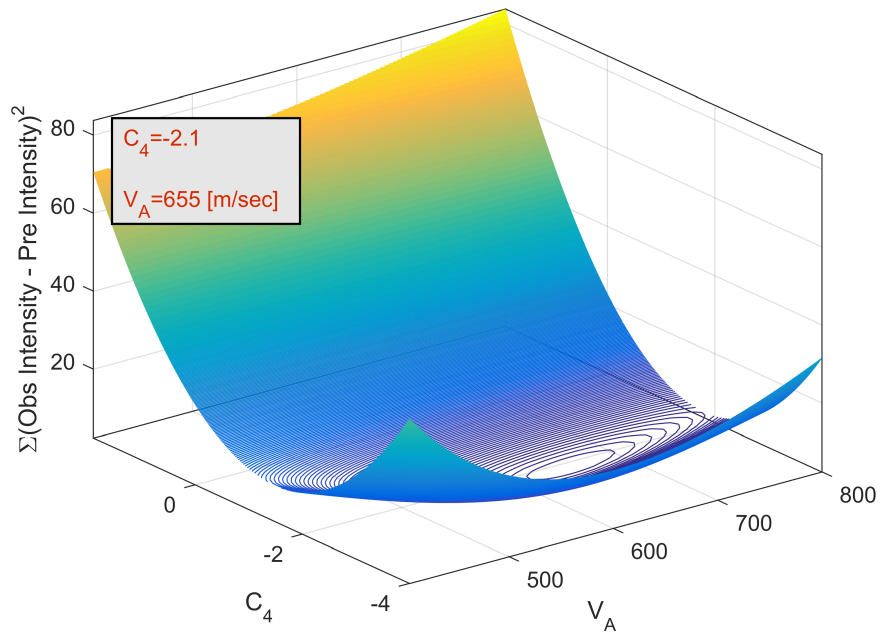


Fig. 1. Sensitivity analysis for calibration the new equation.

C11

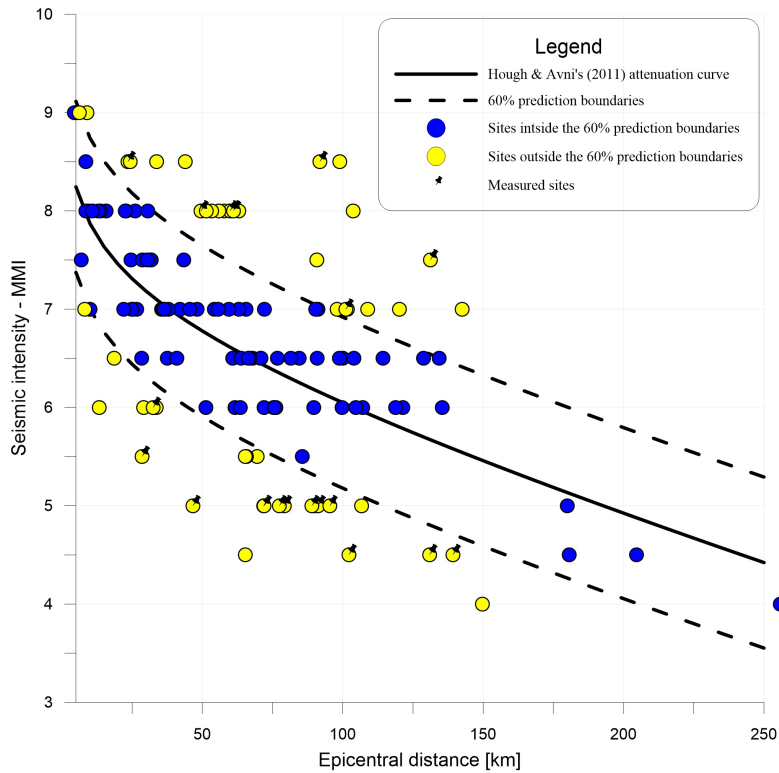


Fig. 2. Avni's seismic intensity (MMI) estimates of all 133 sites. Distance is corrected according to Zohar & Marco epicenter. Yellow dots suspected amplified or de-amplified sites. Sites with pin are sites w

C12