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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

Anonymous Referee #1

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Review of manuscript se-2018-52 titled Calibrating a new attenuation curve for the Dead Sea region using surface wave dispersion surveys in sites damaged by the 1927 Jericho earthquake by Darvasi Y. and Agnon, A.

The above-mentioned manuscript describes a study where an attenuation relation for the Dead Sea was calibrated, using intensity data from the 1927 north Dead Sea earthquake, along with shear wave velocity measurements from the sites where intensity was documented. An former intensity GMPE for the Dead Sea was taken, and a site term was added to it, to account for site response effects. The new GMPE shows a

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better fit for 60% of the data points.

General comment

The topic this manuscript addresses is important, especially for regions where strong-motion data is scarce, like Israel. Finding a way to utilize historical data can contribute much to seismic hazard estimations in those regions. However, one major question that arises from this manuscript is what is the contribution of the new GMPE? The original equation by Hough and Avni was based on a single event and about 130 data points. This manuscript attempts to improve their GMPE and include a site term using only 19 data points, while obtaining a better fit for only 60% of them. In the opinion of the authors – can this GMPE be used for intensity prediction in the future? I feel the step taken in this manuscript is too small – the dataset it is based on is small, no sensitivity analyses were performed and no validations of the GMPE were performed. Can the authors compare macro-seismic data from other historical and perhaps instrumental earthquakes to their GMPE and discuss the results?

Other more specific comments:

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. If the authors would perform the regression on all variables, they probably would get slightly different values.

The authors adopt 760m/s as the Vsreference 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. Perhaps a sensitivity analysis for this value would come up with a value better fitting the data, and would contribute to the point the authors are trying to make.

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

The authors fail to discuss or at least mention other research regrading 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.

Technical comments

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

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

In the abstract and several more instances MASW is explained as Multi Analysis of Surface Waves, please correct to Multichannel Analysis of. . .

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.

Abstract – line 25 –seismic hazard and not risk.

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Introduction – some opening paragraph is missing.

Page 2 line 3 – how about source distance?

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.

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.

Figure 1 is somewhat "overcrowded".

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

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).

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

Vs30 notation is not consistent throughout the manuscript.

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?

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.

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. I don't think that anything novel is presented in these subsections.

Page 5 lines 17-18 – what kind of data from the GII was yours compared to? This is

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very unclear. Is it borehole data? Refraction? HVSR? Is there a reference to the data?

Figure 10 – for which site are these results?

Page 7 line 3 – Hough's name is spelled wrong. Also the citing is not complete.

Page 7 line 9 – Boore's equation is equation (4) and not (2). Also citing not complete.

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.

page 7 line 15 – again Boore's equation is #4.

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, and also 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.

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