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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Soils' seismic property research on the basis of investigation of their nonlinear properties" *by* V. B. Zaalishvili

Anonymous Referee #1

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Overall, It is hard to follow the manuscript. The title itself is very confusing so that it is hard to tell what the author is arguing. The title says about "soil nonlinearity". However, the soil nonlinearity is nowhere defined. Usually the soil nonlinearity is considered in the shear stress-strain behavior (therefore related with shear modulus change with shear strain) of soils subject to earthquake ground motions. I am not sure if the author is referring to this phenomenon. The nonlinearity of soils' stress-strain behavior becomes more pronounced for softer soils and stronger ground motions. It is well recognized that the time-averaged shear wave velocity (Vs30) and natural period of the site (Tn) representing soil conditions are good indices for the soil nonlinearity. Therefore, it is a common effort to measure/estimate shear-wave velocity profiles within a region for the purpose of seismic micro-zonation (SMZ).

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Discussion Paper

The author introduced a term “intensity increment”, and some empirical tools to estimate the intensity increment. However, the author did not provide a definition of the intensity increment, it’s relevance to soil nonlinearity, and it’s usage in SMZ.

It seems that the author does not propose a new tool, but just introduces existing empirical tools to estimate the intensity increment which are pretty old. The references are those published in 90’s and older except for a few publications by the author himself. This makes me suspicious of that there is no novelty in this paper.

The author introduced some equations along with figures. Many of these are referred to Georgia 1991 and 1992. However these references are missing in the list.

It is sometimes questionable how the equations are derived from the data show in figures. For example, first of all, in Figure 2, the author argues that the trends of data for Oni and Iri are different. However, it looks like that the trend line for Iri is strongly biased by just one point at a low normalized amplitude. Overall, the data are two few to evaluate the trends. There are so many earthquake motion data that are publicly available. So the author could have taken advantage of them and derived his own relations. It is also problematic because of that it looks like the trend lines are not developed by a rigorous fitting procedure. There is not enough information for the sites. Where are they located? Showing a map of locations of sites would be valuable. The author only mentioned that some of sites are on rocks and the others are on soft soils. I think these two categories are not enough to indicate soil conditions. These days, people use Vs30 (which is continuous value ranging usually from 100 m/s for very soft soils to 3000 m/s for hard rocks). At least, five NEHRP site classifications (A through E depending on Vs30) would have been more appropriate for a study dealing with SMZ. Lastly, it is not clear what each of the data represents. The same thing applies to other figures. Figure 3 contains only a dozen of data which are not sufficient to generalize the trend. A log scale would have been better for x-axis given the high concentration of the data at lower normalized amplitudes.

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I don't know what Figures 5 and 6 tell us. Figure 6 is not even cited in the text. I think these two figures can be deleted. I also don't know the necessity and relevance of Figure 7 and Figure 8. Figure 9 is missing units for the spectra and time series. Figure 10: What do the hollow and solid circles represent in (c)? Figure 11: Again, it is not clear that what kind of nonlinearity that the author is looking for. In Figure 11, the author points out the nonlinear curve for the relation between areas of ground motion spectrum and ground accelerations. However, I don't understand why this trend is so important for SMZ. In addition, the curve at accelerations higher than 0.1 g is dominated by only a couple of data points. The origin of the data is not clear either. The caption says "Taiwan", but which locations and which earthquakes the data are associated with? Figure 12: Again this is from others' work. Therefore, it should be clearly cited in the caption. The ground motion time series look strangely simple, and the amplitude is very small ($\sim$0.02 g). I am not sure how meaningful it is to discuss the soil nonlinearity for such a small acceleration. The caption says (b) shows Fourier spectra. Then the unit for y-axis is incorrect. Figure 13: I guess the author is showing response spectra. However, all of the spectra do not look realistic. The author needs to explain more details on where these spectra are from. The unit for y-axis (c.u.) is not defined. I don't know what four figures represent. In the third figure, it is very strange that the high frequency component of the acceleration is amplified for soft soils compared to the spectrum for rocky soils. Figure 14: This spectra are typical examples of site effects, which is not surprisingly new. More information is needed to demonstrate the site effects due to different types of soil deposits. How closely the two sites are located? What earthquake motions are these? Table 1: I don't know why the first and second rows represent.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 425, 2015.