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Interactive comment

Interactive comment on "Estimation of path attenuation and site characteristics in the north-west Himalaya and its adjoining area using generalized inversion method" *by* Harinarayan Nelliparambil Hareeshkumar and Abhishek Kumar

Anonymous Referee #1

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General comment:

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The paper addresses relevant questions concerning the evaluation of the seismic hazard in the area that extends approximately from Delhi to the north-western tranche of Himalaya in North India. The authors focus on two arguments: 1) the attenuation of seismic waves travelling through the Earth's crust in that area (i.e. the "path attenuation") and 2) the spectral amplification of seismic waves at the sites of several

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Discussion paper

accelerometric stations deployed there (i.e. site characteristics). Unfortunately the quality of the results presented in the paper is not up to the international standards. The part concerning the evaluation of site characteristics is completely flawed by erroneous assumptions and misconceptions, whereas the part concerning the attenuation lacks the estimation of the uncertainty in the results. Moreover the authors neglect possible considerations that may arise from the points of view of geology and seismotectonics and fail to insert appropriately their work in the framework of seismic hazard studies. As a consequence, the presented results are insufficient to support any significant interpretation or conclusion and the contribution of the work to the current state of the art is almost irrelevant. The English language is poor in many critical points and the manuscript appears to be compiled with insufficient attention for potential readers. In consideration of the numerous inconsistencies, I recommend the rejection of the manuscript and suggest the authors a more critical approach in the elaboration of seismological data.

Specific comments:

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Title:

Considering that the article is submitted to a journal with a wide and diversified audience the title is inappropriate, since it does not mention seismicity or seismic hazard (neither in the keywords!). Apart from this, the title is also inaccurate, referring to NW Himalaya and "adjoining regions”, but in the paper only regions belonging to states of India are taken into account.

Abstract:

The abstract does not illustrate the motivations of the work and does not evidence the meaning of the obtained results. Line 2: what is PESMOS? Lines 4-5: was the study performed to demonstrate the presence of the Moho discontinuity in the region? If so, why do not the authors explicitly declare that (in the title and in the introduction)? If not, why is the presence of Moho discontinuity presented as a foremost result in the abstract? The results presented in the paper are not analyzed with sufficient rigor to support such a claim, anyway. Lines 9-13: sincerely, I do not understand the meaning of these sentences. Lines 13-14: the cited maps have no scientific basis (see later).

Introduction:

Apparently the authors have forgotten that the seismological hazard of an area is strictly linked to geological features. In fact they do not provide any overview of the geological and seismological phenomena that may represent an issue for the seismic hazard evaluation in the studied area. For instance, we know that the Himalayan area is characterized by a peculiar seismo-tectonic regime, which produces earthquakes in a wide depth range and I would expect the authors to discuss how this feature affects the evaluation of the propagation term. And what about the role of the alluvial deposits of the Ganges basin on the seismograms recorded in the stations located in the southern part of the area? Considerations like these would allow the reader to understand better the factors that affect the seismic hazard of the studied area and which methods are most suitable to quantify them. But in the present paper the reader can find only trivial lists of "earthquake parameters" to be inverted with "some spectral modeling" (line 51) and a tedious list of previous works. The importance of the data availability and data quality is also neglected. Line 41-42: what is IS 1893:2002 ? Line 54: again, what is PESMOS? Authors should provide explanation for acronyms and some references would be appreciated, too. Line 61-64: the authors claim that lack of knowledge about geology beneath the recording station does not allow them to identify a reference site, however later in the paper (lines 347 and following, on page 12) they cite the articles of Pandey et al. 2016 (missing in bibliography), which should contain a good analysis of the site characteristics of 27 stations of those used in the present work. Why did non the authors exploit that data to constrain their inversion? That would certainly gave them more reliable results than their "non-reference generalized inversion approach" (line 63), based on the minimum norm criterion, to be discussed later in this review.

Database:

The authors declare that in the estimation of site characteristics they considered 341 records with corresponding hypocentral distances ranging from 10 to 85 km (line 98), but for the path attenuation they considered only 207 records out of 341, line106), with hypocentral distance ranging from 9 to 200 km (line 109). How can a subset of records be defined over a wider distance range than the initial set? No discussion is given about the signal to noise ratio of the accelerometric data considered in the inversion. Considering the involved distances (up to 200 km) and magnitudes (down to MW=2.3) the quality of the records must be verified. No description is given of the enviroment where the stations are located. Apparently no correction for the instrumental response was performed. No references for the hypocentral coordinates are given and PESMOS is again not adequately cited.

Path attenuation:

The inversion for the path attenuation was performed following word by word the approach proposed by Castro et al. (1990). There is no estimation of the uncertainties and the lack of this information deprives the results of their scientific relevance! The authors describe a kink in the attenuation curve which is the only interesting result of their investigation, from the perspective of the application to seismic hazard studies as well as from a purely academical research perspective. Unfortunately the authors do not deepen the analysis of the observed feature and they get rid of the question with a hurried explanation concerning "reflected or refracted waves from the Moho” (line 173) that are observed also elsewhere in the World by other authors. By doing this, the authors miss an opportunity to characterize quantitatively a path attenuation feature which is specific of the studied area and which could have a possible impact on the seismic hazard in this part of India. Instead, they produce a limited range (up to 105 km) parametric attenuation curve which, as they recognize, “falls in between existing attenuation curves" (line 216), obtained by other studies for more limited regions inside the studied area. This result is indeed plausible, but it does not represent an advancement in the state of the art! Line 144: The character "omega" is usually reserved for the quantity "circular frequency", please use "w" to denote the "weight" terms (Castro et al. (1990)), that are intended here. Lines 154-155: The authors claim they have selected the "length" ("width" I guess) of the bins in a way "such that there is almost equal number of data points in every bin", but according to the histogram in figure 2, where the number of records in each distance bin ranges from zero (!) to 28, I believe the selection could have been done better.

Site effects:

The chapter on the site effects is even less scientifically accurate. Instead of following an established approach (e.g. Hartzell (1992)) the authors attempt a site by site inversion and further interpretations which are conceptually flawed in almost all points. The first mistake consists in the site by site inversion described in the paper (Lines 253-257), which implies different solutions for the same source term involved in wavefroms recorded at different stations (whereas the "sharing" of the source contribution in the waveforms recorded at different sites for the same event represents the "strength" of the approach used by Hartzell (1992)). A second mistake lies in the adopted "minimum norm solution", which has no proper justification: are we perhaps searching amplification functions with minimum norm? A third mistake consists in the introduction of the "site amplification factor" defined as the ratio of the amplification functions inverted from the horizontal and the vertical components (line 300), which is by construction a proxy for the already computed HVSR (I won't spend time in demonstrations here, but it is not difficult to prove it). There is no surprise that the authors found a 1:1 matching in peak frequencies obtained with the two methods (line 307). We can find another big mistake in the part where the parameter Vs30 is evaluated from the relation fpeak=Vz/4H with H taken as 30 m (line 324), which can be understood as a total misinterpretation of the meaning of the relation between fpeak and H. One more mistake consists in the interpolation of the fpeak (and Apeak) values obtained at each station site in a continuous distribution over the territories of some Indian states (lines 330-345 and figures 9 and 10). The site characteristic has in fact strictly local validity and it is unevenly distributed over the territory, following in principle the geological and morphological features. Whatever extrapolation method was used (the authors do not inform us about that, but no geology seems involved), it has no scientific basis if applied to fpeak and apeak values collected in a restricted set of stations that are located tens or hundreds km apart. The lack of significance of these maps is evident from a mistake in the mistake: the distributions are discontinuous across the borders, as if the site characteristics were dependent on the political borders! The chapter ends with a futile and unmotivated attempt to find an empirical law relating the inverted f-peak and Apeak values and the effectively measured Vs30 obtained by other authors (lines 346-363), an attempt which appears completely detached with the rest of the article. Line 252: equation 12 represents an excessively complicated system. In fact, the problem can be solved for each frequency separately (as it was done for attenuation). Figures 6a-6f: The y-axis is labeled as PSA (pseudo acceleration?) in units of g whereas the legend indicates spectral amplification obtained with GINV method. The values are however inappropriate for spectral amplification (they must be ranging around the value 1). The tick-marks on the X (logarithmic) axis are only two without any grid, thus making the plot nearly useless!

Conclusions:

The conclusions provide an incomplete summary of the manuscript contents without any interesting discussion or meaningful conclusion. The authors claim the "presence of reflected and rifracted arrival from the Moho" (line 369) without any quantitative argumentation and without any discussion about the significance of this effect in respect of the seismic hazard.

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