Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-148-AC1, 2018

Natural Hazards and Earth System Sciences
Discussions

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

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Received and published: 23 November 2018

General Comment: 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 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 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. Response: The authors thank the reviewer for the valuable time in reviewing the work and giving expert comments. Going with first observation of the reviewer as highlighted above (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), authors want to highlight here that specifically this paper does not attempt at all the seismic hazard of the study area. Rather the objective of present work is to quantify the path attenuation and site characteristics of the study area. Though detailed seismic hazard requires the knowledge of path attenuation and site characteristics, but other informations are also required while attempting seismic hazard of an area, which are neither attempted nor claimed in this work. Authors nowhere conclude anything on seismic hazard of the study area based on present findings. Further, authors want to highlight that regional information on path attenuation as well as site characteristics, for the present study area are still missing and thus are attempted in this work. For this reason, either for site specific ground response analysis or for ground motion simulation, still researchers are practising utilization of above parameters from other regions. Authors believe that unless above parameters are estimated on regional scale, end results will have no to limited application in understanding ongoing seismicity mapping of the study area. English of the manuscript has been improved throughout the manuscript while revising. Comment: 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 Seismotectonic 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. Response: Authors want to highlight that in the current paper site effects are characterised based on the predominant frequency obtained from HVSR and non-reference Generalized inversion technique (GINV). The effectiveness of both HVSR and GINV with regards to estimation of site characteristics is well documented in literatures (For eg. Field and Jacob, 1995; Harinarayan and Kumar 2017b). Further, the reviewer commented that the outcome of the present study does not give possible considerations that may arise from the points of view of geology and Seismotectonic. Authors appreciate the reviewers comment and agree that incorporating geological and Seismotectonic consideration would be good. However, authors want to emphasise here that numerous published literature highlights that PESMOS recording stations are lacking with geological information. The site class given by PESMOS is based on physical description of surface materials, local geology following Seismotectonic Atlas of India (GIS 2000), Geological Maps of Indian and not based on actual field investigation (Kumar et al., 2012). Geophysical subsurface exploration studies on some of the recording stations in northwest Himalaya reported by Pandey et al., 2016a; b have highlighted the flaws in the site class given by PESMOS, where recording stations classified as on rock site were found to be on soil sites. The lack of accurate information with regards to geology for the recording stations maintained by PESMOS prevent the scope for any studies from the point of view of geology. Authors would like to highlight here that it is one among many reasons which motivated the authors to go for site characterization of the PESMOS recording station of the study area, as done in this work. The above discussion has been added in Line 50-64 in the revised manuscript. In addition, present paper estimates path attenuation and site characteristics based on earthquake records. There is no discussion made with regards to source parameters in the manuscript. Hence, authors feel that incorporating the Seismotectonic of the study area is not relevant referring to present findings and are beyond the scope of the present study. Further, the reviewer suggested to incorporate the outcomes of the present work in the frame work of seismic hazard studies. As

highlighted in earlier comment that present findings are not sufficient alone for seismic hazard quantification and thus presenting the presenting findings in terms of seismic hazard analysis is not possible and is beyond the scope of this work. Further, with limited and partial information available from present study, if seismic hazard is attempted, authors believe that such seismic hazard will not be appropriate and does not convey anything related to actual seismicity of the region. However, considering reviewer's suggestion, possible comment on seismicity of the region, in a broader sense, based on the quality factor obtained in the present study, is made in Line 242-244 in the revised manuscript. Comment 1: 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!). Response: As per reviewer's suggestion, keywords have been modified incorporating the term "seismicity". Comment 2: 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. Response: As per reviewer's suggestion, the title of the paper has been modified to: "Estimation of path attenuation and site characteristics in the northwest Himalaya and its adjoining area within India territory, using generalized inversion method". Comment 3: Line 2: what is PESMOS? Response: PESMOS stands for Program for Excellence in Strong Motion Studies. PESMOS maintains ground motion records from recording stations installed in various regions within India by the Government of India to monitor the ongoing seismicity. Earthquake records since 2004 are available in PESMOS data base. A brief description of PESOM database has been added in Line 49 – 60 in the revised manuscript. Comment 4: 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? Response: Authors want to highlight here that the foremost objective of this paper is to estimate path attenuation and site characteristics based on strong motion records which has been clearly mentioned in the title as well. Identifying the presence of Moho discontinuity based on the nature of the attenuation curve is an additional finding of the work. Authors believe that though this is an important finding, still it is based on estimation of path attenuation and hence should be mentioned in the abstract and not to be included in the title. Comment 5: Lines 9-13: sincerely, I do not understand the meaning of these sentences. Response: The line 9-13 from the original manuscript has been rewritten as follows; "The ratio of the horizontal and vertical site amplification components is computed to determine the amplification function and predominant frequency for each of the recording stations. The amplification function and predominant frequency based on generalized inversion method is compared with that obtained from horizontal to vertical spectral ratio (of S wave portion of the accelerogram) method." in Line 9-13 in the revised manuscript.

Comment 6: Lines 13-14: the cited maps have no scientific basis. Response: As per reviewer's suggestion, discussion on spatial distribution of predominant frequencies and amplification functions has been removed in the revised manuscript. Comment 6: 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. Response: The Authors thank the reviewer for this suggestion. As highlighted earlier, information on geology of the recording station is not available. However, an overview of Seismotectonic settings of the study has been added in line 95-113 in the revised manuscript. Comment 7: 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. Response: Authors completely agree that the characteristics of alluvial deposits in the Ganga basin will change the seismogram characteristics. In case such characteristics are known based on suitable in-situ investigation, it can be studied further. Since present work attempt to understand subsoil characteristics completely based on ground motion records in terms

of fpeak and Apeak, incorporating alluvial characteristics is beyond present work's objective. Possible change in seismogram if any will also be reflected in fpeak value determined in this work. Collectively, neither data required for such study is not available at present nor it will affect the present findings of fpeak. Comment 8: Line 41-42: what is IS 1893:2002 ? Response: IS 1893:2016 is an Indian seismic code. The reference for IS 1893:2016 is: IS 1893: Part 1–2016. Indian standard criteria for earthquake resistant design of structuresâĂŤpart 1: General Provisions and Buildings, Bureau of Indian Standards, New Delhi, India. Comment 9: 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. Response: Based on the reviewers comment on "lack of knowledge about geology beneath the recording station", the authors want to highlight that the PESMOS database lacks accurate information of subsurface for majority of recording stations. The site class given by PESMOS is based on physical description of surface materials, local geology following Seismotectonic Atlas of India (GIS 2000), Geological Maps of Indian and not based on actual field investigation (Kumar et al., 2012). Geophysical subsurface exploration studies on some of the recording stations in northwest Himalaya reported by Pandey et al., 2016a; b have highlighted the flaws in the site class given by PESMOS, where recording stations classified as on rock site were found to be on soil sites. The above discussions have been incorporated in line 55-64 in the revised manuscript. Authors also want to highlight here that in order to perform conventional generalized inversion method (Andrews, 1986; Hartzell, 1992), reference site (usually rock site) is required in-order to remove the trade-off between the source and site parameters. However, the field study by Pandey et al., 2016a; b used 27 stations and which are actually soil sites. Thus, it is not possible to identify reference site (rock sites) based on the findings of Pandey et al., 2016a; b. For this reason, authors feel non-reference generalized inversion approach is a better alternative to estimate the site term in this work. Based on above discussion, the referred statements are corrected accordingly in the revised manuscript. Comment 10: 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? Response: The authors are thankful to the reviewer comment. Following correction have been made in the above referred statement; "For estimating site characteristics, 341 records from 86 EQs, with magnitudes ranging from Mw=2.3 to Mw=5.8, having focal depths ranging from 2 to 80km are used (Line 121 in the revised manuscript). The database for estimating path attenuation consists of 207 records from 32 EQs, with magnitude ranging from Mw= 3.1 to Mw=5.5, focal depths from 3 to 55km, hypocentral distance from 9 to 200km" in line 130 in the revised manuscript. Comment 11: 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 environment where the stations are located. Response: Authors want to highlight that signal to pre-event noise (all of equal window length) ratio (SNR) for all the records were computed and records with SNR greater than 5 similar to (Ameri et al., 2011) are considered for the present analysis. The discussion on SNR has been incorporated in Line 136 in the revised manuscript. Comment 12: No references for the hypocentral coordinates are given and PESMOS is again not adequately cited. Response: As per the reviewer's suggestion, references for the hypocentral coordinates have been added in the revised manuscript. Authors want highlight here that the information regarding the coordinates of earthquake epicentre and recording stations are obtained from PESMOS. Comment

13: There is no estimation of the uncertainties and the lack of this information deprives the results of their scientific relevance! Response: Authors agree with the reviewer's observation. As per the reviewer's suggestion, the uncertainties in the quality factor estimation has been added in the line 227 in the revised manuscript. Comment 14: 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 academic 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. Response: Authors want to emphasis here that the scope of this paper is to estimate path attenuation and site characteristics based on strong motion records. Identifying the presence of Moho discontinuity based on the nature of attenuation curve is an additional finding of the work. Any further information regarding Moho discontinuity cannot be deducted from the present study. Comment 15: 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. Response: As per the reviewer's suggestion, weighing factors have been denoted by w in the revised manuscript. Comment 16: 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. Response: Authors agree with the reviewer's suggestion regarding replacing the term "length" in Lines 154-155 to "width". As per the reviewer's suggestion the term "length" has been replaced to "width" in Line 547 in the revised manuscript. Authors want to highlight here that the width of the bins are selected such that there is sufficient number of data points in every bin. Moreover, it can be observed from Figure 2 that there are only 2 records at 125km bin, 0 records in 135km bin, 1 record in 145km bin, and 3 records in 155km bin. Collectively there very few records in bins beyond 115km. For this reason, EQ records with hypocentral distance only up to 115km are considered for the analysis. Comment 17: 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. Response: The reviewer commented on the use of non-reference GINV over the established approach (reference GINV), which requires earthquake data recorded on rock site. The authors have earlier highlighted the problem in identifying recording stations on rock sites. Authors want to highlight here that performing inversion similar to Hartzell (1992) in this context is not feasible. Authors feel modifying the generalized inversion method such that analysis can be carried out without reference site condition is a better alternative as done in this work. Comment 18: 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)) Response: Authors partially agree with the reviewer’s remark about 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). Authors want to highlight here that the objective of the approach used by Hartzell (1992) is to simultaneously determine the source and site component. Authors want to emphasis here that the objective of the non-reference GINV is to determine the site component alone and no source component. Further, authors nowhere claim to determine source component in the manuscript. Hence, obtaining different solutions for the same source term involved in waveforms recorded at different stations will not affect the obtained value of site component, as obtained in this work. Comment 19: A second mistake lies in the adopted “minimum norm solution”, which has no proper justification: are we per-

haps searching amplification functions with minimum norm? Response: Authors want to highlight that minimum norm inversion procedure (also known as Moore- Penrose matrix inversion) used to estimate soil term is in accordance with Penrose, (1955). A brief description regarding the inversion procedure is given below. Eq. 11 (in the revised manuscript) represents a linear system of the form: Ax = B, where B is the data vector containingãĂŰ dãĂŮ_ijj, x is the vector containing the model parameters [g(f) and s_i (f)], and A is the system matrix relating x and B as described in eq. 12 (Menke, 1989). ãĂŰ ãĂŰlnUãĂŮˆA (f)ãĂŮ_(ij)= ãĂŰln S(f)ãĂŮ_i+ ãĂŰln G(f)ãĂŮ_j (11) ← A → ←x→ ←B→ ← 1st event → ← nth event → ← Site effect→ 1 2 . . . m 1 2 . . . m 1 2 . . . m 1 0 . . . 0 . . .... 0 0 0 1 0 . . . 0 s1(f1) d1(f1) 0 1 0 . . .... 0 0 0 0 1 . . . 0 : 0 0 0 1 0 0 . . . 0 0 0 . . . 1 s1(fn) d1(fm)

sn(f1) = : For nth earthquake sn(fn) dn(fm) 0 0 . . . 0 . . ... 1 . . . 0 1 0 . . . 0 g(f1) dn(fm) 0 0 . . . 0 . . ... 0 1 . . . 0 0 1 . . . 0 g(f2) : 0 0 . . . 0 . . . 0 0 . . . 1 0 0 . . . 1 g(fm) dn(fm) (12) The matrix form in Eq. (12) represents a purely under determinate system since there are $(n+1)\times m$ parameters for ‘$m \times n$’ data (here m is the number of sample frequency and n is the number of EQs recorded at a particular recording station). Further, the model parameters at the selected frequencies are obtained using Moore- Penrose matrix inversion (or Minimum norm inversion) method given Penrose, (1955) as given below. x=(AˆH.A)ˆ(-1).AˆH.B In the above equation, AˆH is the conjugate transpose of matrix A . Comment 20: 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). Response: Authors want to highlight here that in the present study site amplification curve for each recording station is calculated based on two different methods i.e., HVSR and non-reference GINV. Effectiveness of HVSR method in providing good estimate of predominant frequency (fpeak) is well documented (for eg.

Field and Jacob, 1995; Zhao et al., 2006). Authors feel that similarities in the value of fpeak obtained using non-reference GINV with that obtained using HVSR show the robustness of the non-reference GINV used in the present study. Comment 21: 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. Response: The relation fpeak=Vz/4H given by Kramer (1995) is used to estimate the range of fpeak values corresponding to the range of Vs30 for site class as per NEHRP classification scheme. Similar to the present work Zhao et al. (2006) carried out site classification of 874 recording stations in Japan based on HVSR method. Zhao et al. (2006) gave possible range of fpeak for rock sites as greater than 5Hz and soil sites less than 5Hz respectively. Further, the range of fpeak obtained using the relation given by Kramer, (1996) in the present study is: fpeak $\geq$ 6.35Hz for rock site and fpeak< 6.35Hz for soil sites. The possible range of fpeak for rock sites and soil sites given by Zhao et al. (2006), are closely matching with fpeak range obtained based on the equation fpeak=Vz/4H.

Comment 22: 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! Response: Authors agree with the reviewer's observation. As per reviewer's suggestion, discussion on spatial distribution of predominant frequencies and amplification functions has been removed from the revised manuscript. Comment 23: 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! Response: The y axis represents spectral amplitude (acceleration). Figures 6a-6f have been modified considering the reviewers suggestions. Comment 24: 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 refracted 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. Response: Authors would like to highlight here that observation about Moho in the present study is purely based on the kink observed in attenuation curves in this work and observing similar trend by earlier researchers. Further comment on this observation required detailed studies and is beyond the scope of this work. While revising the manuscript, "Conclusion" section is completely rewritten clearly citing the important observations from the present work. In addition, need for detailed investigation for Moho discontinuity, which is beyond the scope of present work, is also highlighted clearly in the conclusion.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-148/nhess-2018-148-AC1-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-148, 2018.