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Natural Hazards and Earth System Sciences
Discussions



# *Interactive comment on* "Integration of HVSR measures and stratigraphic constraints for seismic microzonation studies: the case of Oliveri (ME)" *by* P. Di Stefano et al.

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According to comments of both referees manuscript has been revised, shortening some sections and the references and including a detailed description of the clustering procedures. A new version of the manuscript is attached as supplement file. A step by step reply to the referee follows.

Anonymous Referee #2

Manuscript nhees-2-2597-2014 Integration of HVSR measurements and stratigraphic constraints for seismic microzonation studies: the case of Olivieri (ME)" by Di Ste-



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Interactive Discussion

Discussion Paper

fano P. et al. (2014) 1. Adequacy and relevance of the paper. The article focuses on the seismic microzonation of the urban area of Oliveri through a series of HVSR measurements and applying a clustering algorithm to optimize the calculation of the HVSR curves with the purpose of identifying reliable HVSR peaks/reflectors in the 0.6-10 Hz frequency range. A second clustering technique based on the central frequency and amplitude of each peak is applied to identify subsets which can be attributed to continuous spatial phenomena. Furthermore, the authors state that the results are constrained by stratigraphic data extracted from a borehole. The stated objectives are: to identify those places of the explored area which could suffer significant site effects, in addition to define a preliminary Vs-depth subsurface model valid for the investigated zone (page 2). However, the authors say at the beginning of the conclusions (lines that should be part of the introduction): "The main purpose of this work is not, of course, to present the complete results of the first level microzonation study carried out in an urban center of an area of high seismic hazard, but to show how a survey performed by the HVSR method applied to ambient seismic noise, thanks to some significant improvements affecting data processing, can provide results that go far beyond the simple identification of possible site effects at frequencies of interest in earthquake engineering. In this study, the most innovative aspects concerning seismic signal processing are based on the use of clustering techniques which are applied in two stages" (page 22). Thus, I understand that the key geophysical problem consists of checking a clustering procedure with the target of improving substantially the results provided by the standard HVSR method. That said, I am surprised by the contents of the manuscript. Sincerely, how the clustering technique works is a mystery for me. Not even Figure 5 is well explained, which is supposed is key in the performance of the proposed working method (it is obvious the lack of explanation in the figure caption and of course in the principal text). It is true that authors make reference to other two manuscripts about the same topic, but still are unpublished. As a general rule, references to unpublished studies are not admissible. In this sense, the manuscript contains some deficiencies in arguments and is not entirely intelligible, at least is not clearly presented (sorry, this is



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my opinion). Logically, this greatly hampers the study now commented. Furthermore, the whole article is written in a fuzzy way, unclear, difficult to be understood, not well organized or presented, and on the other hand the implementation of the indicated methodology is far from being clear for a potentially interested reader in the subject. The manuscript is clearly unbalanced and is especially cumbersome as it relates to sections 1 (introduction) and 2 (geology) when compared to other subsequent sections related to the physics. Moreover, some title or subtitle of section or subsection should be rewritten/changed for clarity. In summary, the paper, although in principle is interesting to the extent that proposes a significant improvement ("an innovative procedure") when using HVSR measurements, is however technically deficient because all is not enough contrasted, since some theoretical-practical issues need a thorough revision before admitting the real significance of the working method and to think about future practical applications. 2. Style and presentation issues The manuscript is not properly organized as mentioned above. The text, leaving aside its inherent difficulty and peculiarities, can be substantially improved both formally and technically. The literature list can be considerably shortened including only the necessary bibliographic references (but not other studies or unpublished works to date). Of course, all references cited in the text must be given in the final list and vice versa, and also they must be all written according to the standards of the journal. Lastly, most of the figures and their respective captions can be improved. 3. My recommendation is that the manuscript must be carefully revised according to the above comments and the more specific ones that are given below.

Authors: The paragraph "The main purpose of this work ...." in the conclusions has been removed. In addition, Introduction and Conclusions have been modified to clarify the purpose of the paper and obtained results. According with referee suggestion a new section "3.2. Implementation by cluster analysis" has been added in the text to clarify how the proposed clustering technique works, eliminating references to unpublished papers. Also Fig. 5 has been modified. Finally references have been reduced and figures with respective captions improved. Generally, the manuscript has been



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reorganized, shortening introduction, HVSR technique description and geology. Furthermore those parts related to the clustering procedures have been improved.

Major issues

First of all, the frequency range of interest is not clear for me. I read 0.6-10 Hz in the abstract (page 2). I section 3, I see 0.1-20 Hz (page 13), after 0.6-20 Hz (page 14), after 0.7-2.6 Hz (page 14). In section 5 a reference to the range 0.7-1.4 (page 18) is made. This should be clarified satisfactorily giving account for any variation.

Authors: The analysis were limited to the frequency range of 0.1-20 Hz, but all the peaks were identified in the range 0.6-10 Hz and the main peaks in the range 0.7-1.4 Hz. To avoid misunderstanding, some correction have been made in the text.

Leaving aside the great amount of text devoted to geology in previous sections, my main criticism concerns sections 3, 4 and 5. After dealing with a long dissertation in previous pages, we finally arrive to the essential point of the problem here addressed (in page 14 nothing less). This point is the "Agglomerative Hierarchical Clustering (AHC) algorithm, which is selected by testing different hierarchical and non-hierarchical clustering algorithms. We used as proximity measure the Standard Correlation (SCxy) together with the average linkage (AL) criteria (D'Alessandro et al., 2014a). This procedure allowed us to split, almost automatically, peaks probably linked to site effects from other perhaps related to source effects". As I already said, I can hardly understand this part (pages 14-16). How the clustering technique works is a mystery for me. This has to be explained in order and with detail, step by step, with special emphasis on the discrimination between each other peaks, and with particular attention to the stratigraphic peaks characterized by a minimum in the spectral component of the vertical ground motion because of the Rayleigh-wave annihilation at the resonance frequency. The tale of how AHC operates on the HVSR is key to understand the mechanics of the technique applied. Otherwise the work would not meet the announced improvement and would be difficult to understand for a potential reader. Again we arrive to



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other essential issue as is a second clustering (pages 14-15) “to group peaks of HVSR average curves related to different sites, so as to identify areas characterized by site effects, which because of the similarity in frequency and amplitude of the corresponding HVSR peaks and the not excessive distance between the measuring points, are probably caused by the same buried structure”. The idea is good. I agree with this approach. In other words, it is an attempt of assessing the lateral continuity of possible seismic reflectors. The authors say: “We implemented a clustering procedure to be applied to the parameter vectors that characterize the individual peaks. Each vector contains: period/central frequency, amplitude, coordinates and a parameter indicative of the outcropping lithology. The number of vectors is equal to the number of peaks considered significant”. O.K. “The procedure is based on an Agglomerative Hierarchical Clustering (AHC) algorithm, using the average linkage (AL) criteria”. I agree. The text continues: "To disadvantage (?) the inclusion in the same cluster of more peaks relative to the same measurement point, the relative distance of coincident points was placed equal to 1 (?)”. It is only after this point that the text is not clear to me and consequently the way to proceed. Again I find a very confusing text. Furthermore, the attribution of weights to the various parameters involved in the calculation is not justified at all.

Authors: A new section “3.2. Implementation by cluster analysis” has been added to better explain how AHC works and to clarify how input parameters are linked to each other’s. The implemented procedures are based on the AHC algorithm and used the Average Linkage (AL) criterion. For the identification of the optimal set of analysis windows for the determination of the average HVSR curve, we have adopted as a measure of proximity the Standard Correlation defined in equation (1) in the manuscript, which takes into account both the position and amplitude of the peaks present on the HVSR curves. To define clusters of HVSR peaks the process of clustering considers the period and the amplitude of each peak and also the same parameters of the neighbouring points, as defined in equation (2). Moreover in the section “4. Frequency maps” it’s been explained how weighting factors were selected.



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Moreover, to what extent it is possible say that the same HVSR peaks at 2 close sites are coming from the same reflector? This should be well clarified.

Authors: It's unlikely that two HVSR peaks at 2 close sites with similar resonance frequency are linked to different reflectors especially if there isn't an evidence of tectonic contact.

Figure 5 is key regarding the performance of the proposed working method. What mean the numbers of the horizontal scale? Either in the text, or in the figure caption, the results obtained by clustering, especially the three clusters called R, G and B, have to be presented with more detail.

Authors: Labels of the axis have been added and caption improved.

One can read: "...adopting a cut threshold slightly lower than the optimal one adopted by the calculation code...". What optimal threshold? What does this mean? Do the authors want to say that the resulting clusters depend on a threshold that nobody knows how it is chosen? This has to be made clear.

Authors: We added the following clarifying sentence in the text: "Different criteria can be used to find the optimal threshold. In this paper, the optimal threshold was identified seeking in the dendrogram the large gap between two successive levels of the hierarchy."

One can read: "...in the diagram H/V vs. Fo...". What is Fo?

Authors: There was an error: "H/V vs. period".

Which is finally the adopted model to start the inversion process of the HVSR curves (page 18)? I understand the model is constrained by the information extracted from a borehole, and "From this model estimates of the S wave velocity were obtained, which were subsequently used to define the starting model for the inversion of the other (?) HVSR curves". Well, I would like to see a description or better yet a graphical representation of this starting model. Another possible mistake (page 19): "...evaluation of



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the thickness of the cover did not take into account the available seismic down-hole data. . .” How to reconcile this with the above?

Authors: To invert the HVSR curves we used only the stratigraphic information obtained by the available borehole, retaining the seismic velocity information not reliable. The ambiguous statements in the text have been resolved.

Lastly, I miss a thorough discussion about the reliability of the solution obtained by inverse modeling (Figure 9). This subject is intrinsic to any inverse problem and both the error of the solution and the resolution achieved in the approach are issues that need an analysis before admitting the velocity-depth and density models. I do not see anywhere the treatment of so important aspects. In addition, due to the nonuniqueness of the solution of any inversion problem, I consider this topic is worthy to be thoroughly studied.

Authors: We considered reliable inverse models with misfits about equal or less than 1. Misfits were calculated as a sum of squared differences between observed data and calculated ones, normalized by the variance. Furthermore the inversion processes were constrained by stratigraphic information and values of shear-wave velocity of the cover available for the studied area. In particular, we considered about 200 to 250 m/s for alluvium and about 350 to 400 m/s in correspondence of silty sands. We added a clarifying sentence in the text and improved the caption of figure 9.

Minor issues (style and presentation issues)

First of all, which is the meaning of ME? This acronym (page 1) should be removed if it is not relevant. Where is Oliveri? In Italy, yes, but where? It would be better write Oliveri (north Sicily) as a part of the title.

Authors: We modified the title as follow: “. . .the case of Oliveri (North-Est Sicily).

The authors often use very long phrases and this hinders the understanding of the text. Please, use shorter phrases throughout the text if possible. Also, they abuse of



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separate paragraphs systematically when should try to give more continuity to their arguments and use point and followed. This is observed along all the text and should be fully corrected.

Authors: Most of the long phrases have been shortened and the text revised.

Pages and pages have been written (often repeating known concepts or else results already obtained in previous work) before entering fully into the treatment and resolution of the problem. The paper is really long (many concepts are repeated several times, up to 3 or 4 times throughout the text) and quite unbalanced in the contents. Sections 1 and 2 have to be shortened but keeping the key items that enable a good and well ordered introduction to the problem, and providing the corresponding references (which not always are given, for example in the second paragraph of section 1, top of page 3, and in other cases). I understand that the problem is essentially of geophysical character, which is solved by applying a typical geophysical analysis method, HVSR,b although in this case with the help of a clustering algorithm. If so, I sincerely want to see more physics and less geology.

Authors: Sections 1 and 2 have been shortened and repetitions removed. Moreover the paper has been substantially modified to better highlight the geophysical character.

In any case, my suggestion is to make a right introduction of the HVSR technique in section 1 together with a good description of the nature of the ambient seismic noise making a short reference to diffuse wavefields. Other thing: the correct expression is "This paper focuses on. . ." (top of page 5).

Authors: The introduction has been reduced and modified.

Another suggestion is a new title for section 2, "The study region", and its division into two subsections 2.1 and 2.2 (from the line "The Oliveri territory is located in the area 932. . .", page 7) with the subtitles "Geological setting" and "Regional seismicity", respectively, both of them properly organized and shortened.



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Authors: Section 2 has been split in the two suggested subsections and generally reduced by deleting those stratigraphic information that are irrelevant for the work, keeping only a synthetic geological framework of the area.

By the way, magnitude Mw=6.15 (page 9) is at least strange (since it has more than two significant digits) and must be probably erroneous for a historical earthquake dated in the 18th century (1786). It could be 6.1 after being estimated from a previously tested intensity-magnitude relationship and applying right conversion formulae.

Authors: The Mw is that reported on the DBMI11 catalog (http://emidius.mi.ingv.it/DBMI11). Actually, the value reported on the catalog is MW=6.15$\pm$0.38, so in the manuscript we have replaced the magnitude value with MW=6.1$\pm$0.4.

The completeness threshold "equal to about 2.6" (page 9) requires a convincing explanation and the corresponding reference if appropriate.

Authors: The completeness threshold was estimated on the basis of Schorlemmer et al., (2010) and D'Alessandro et al. (2011). A clarifying sentence and references have been added in the paper.

Likewise, section 3 can be simply titled "HVSR measurements" for after distinguishing two subsections 3.1 and 3.2 (from the line "To try to identify areas of the town of Oliveri...", page 13) with the subtitles "Background and points under discussion" and "Implementation", respectively, both of them equally organized and shortened, especially the first of them, that is section 3.1. Many concepts can be found in SESAME (2004) and therefore section 3 can be properly shortened.

Authors: Section 3 has been split in two section, the first named "HVSR measurements" has been reduced by deleting the almost all the general considerations about HVSR method. The second section, now titled "implementation by cluster analysis" has been practically rewritten.



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Sorry, I do not understand the sense of the words "capability of the HVSR curve to mimic the HVESR curves" (section 3, page 12). There are other phrases in the text that I neither understand very well. For this reason, let me insist on polishing the language as much as possible.

Authors: . . .earthquakes HVSR curves. However this paragraph has been removed in the text and the language was generally revised.

Another cryptic phrases for me are (section 3, page 12): "In particular, increasing of the processing duration of the (?); producing results whose reliability depends on the skill of the operator. The permanence of abnormal (?) windows can cause bias of the estimates of the expected value and the dispersion of the set of selected curves can lead to overestimates of the true uncertainty of the spectral ratios".

Authors: This paragraph has been removed in the text.

Section 4 should be entitled "Clustering technique and frequency maps". But here the key issue is to explain in detail the procedure to be easily understandable (and without making reference to unpublished papers).

Authors: reference to unpublished papers were removed and two new sections "3.1. Background and points under discussion and 3.2. Implementation by cluster analysis" were added to clarify in detail the clustering procedures.

Section 5, now titled "Bedrock mapping by inversion of HVSR measurements", is essential and deserves any effort aimed to clarify the inversion process of the H/V curves. By the way, "the available geological information", so mentioned in the text (page 17), is not "reported" in Table 1.

Authors: According to referee's suggestion, a new section "5. Bedrock mapping by inversion of HVSR measurements" has been improved. Furthermore the column of the bedrock depth has been added to the table 1.

Section 6 (pages 20-21): Is really necessary more geology after everything said in the



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opening sections? Could not this section be shortened at least a bit?

Authors: The section 6 with the tectonic setting at a smaller scale has been reduced and modified highlighting the new geological information obtained thank to geophysical data.

The conclusions must be written only one time (in section 7, not before) and all together in a summarized way, and in a direct and understandable style. In fact, the first 8 lines in section 7 (page 22) can be removed from the text (it is supposed that are part of the introduction). Only the following paragraphs deserve to be carefully presented.

Authors: According with referee suggestion the first 8 lines in section 7 have been removed and Conclusion section has been revised.

The bibliographic references (pages 24-29) have to be the fair ones, neither many nor few, only those necessary (of course, those works in preparation or in progress have to be removed from the list).

Authors: According to the suggestion of the referee, references have been reduced and works in preparation or in progress have been removed.

About the illustrations: Geographical coordinates must be included in the corresponding illustrations (Figures 1, 2, 7 and 10). As a general rule, sizes of letters and numbers on scales should be increased (Figures 3, 4, 5, 6, 7, 9, 10 and 11). In the caption of Figure 6 must be: ..."for the three HVSR clusters...". Additional text should be added in some figure captions in order to allow the easy understanding of the results that are shown in each illustration (without having to back to the main text of the article) (Figures 3, 4, 5, 6, 7, 8, 9 and 10).

Authors: In figure 1 now a frame reports the geographic location of the studied area. Geographical coordinates have been included in the figures 7 and 10. Moreover additional text has been added in some figure captions.



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Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C1487/2014/nhessd-2-C1487-2014-supplement.pdf

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