



Interactive
Comment

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.

P. Di Stefano et al.

raffaele.martorana@unipa.it

Received and published: 14 July 2014

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 #1

REVIEW OF THE MANUSCRIPT "Integration of HVSR measurements and stratigraphic constraints for the seismic microzonation studies: the case of Olivieri (ME)"

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



by Di Stefano P. et al. (2014)

This paper presents a sort of seismic microzonation of the town of Olivieri based on a clusterization algorithm that should (if I understand well) 1) optimize the calculation of the HVSR curve and 2) identify the continuity of the HVSR peaks/reflectors in space. How this clusterization algorithm works is unknown: the authors readdress the reader to other 2 manuscripts in preparation about the same topic and one therefore wonders what the aim of the present paper, where results are presented as coming out a black box, is.

Authors: The paper has been reorganized in a way to better explain the clustering algorithm used, avoiding to refer to unprinted manuscripts.

MAJOR ISSUES: The paper is very long (many concepts are repeated 3 or 4 times throughout the text) and severely unbalanced in the contents.

Authors: The paper has been substantially reduced by eliminating repeated concepts and synthesizing the geology.

- Section 1 (Introduction): here we have a first description of the HVSR technique and of the nature of seismic noise (2 pages),

Authors: The introduction has been reduced and modified.

- Section 2: 5 pages to describe the geological settings are not interesting at all considering that the goal of the authors is not the present the seismic microzonation of the town (as they write on p. 22) but the improvement brought in by a clustering technique,

Authors: Section 2 has been split in two subsections with the subtitles “Geological setting” and “Regional seismicity” and generally reduced by deleting those stratigraphic information that are irrelevant for the work, keeping only a synthetic geological framework of the area.

- Section 3: here the HVSR technique is described again in 6 (!) pages. This looks

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



more like a historical review of the method but since all these concepts have already been discussed in several papers (and are already summarized, e.g., in SESAME, 2004), this section does not add anything new. Also, the different hypothesis are acritically presented as if they were at the same level why they are not: presently it is well acknowledged that surface waves are dominant in ambient noise (for the simple reason that they attenuate less with distance). What is variable and a priori unknown is the proportion of different waves but the original explanation of the HVSR proposed by Nakamura in terms of SH waves only has been abandoned. The presence of Rayleigh waves in ambient noise is recognizable in the local minima of the vertical spectral component (which cause the H/V peaks) while local maxima in the horizontal components are due to Love/S waves (see also Fah et al., GJI, 2001; Tuan et al., GJI, 2010 etc.). HVSR is mentioned as a method to estimate the resonance frequencies of buildings while this was attempted just at the initial stage and soon abandoned because rocking can modify the vertical component, thus affecting the amplitude of the modal shapes (and sometimes the frequency) calculated with this method (e.g., Todrovska, BSSA, 2009). Eigen-frequencies in structures are commonly measured through other techniques (SSR etc.). But my main concern remains: what is the aim of this paper? At page 13 we are still dealing with the review of previous papers.

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.

- P. 14: we arrive to the apparently central point: “an Agglomerative Hierarchical Clustering (AHC) is applied – after several tests – to split almost automatically peaks probably linked to site effects from other perhaps related to source effects”. After 14 pages of discussion the central point is described in 4 lines. How does this AHC method work? What are the input data? Why did the authors select the SC as proximity measure and AL and discarded other options? On what basis? Did they have a control set to assess

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the performance of the methods? How can the AHC separate between stratigraphic peaks and anthropic peaks (if this was the point, I am not even sure)? As widely discussed in SESAME (2004) and other papers quoted therein and following, H/V peaks of stratigraphic origin are characterized by a minimum in the vertical spectral component (due to the Rayleigh wave annihilation at the resonance frequency) while anthropic peaks are much sharper and show narrow maxima in all the 3 spectral components. As far as I understand, the AHC operates on the HVSR, not on the single component spectra. How can it distinguish the 2 kinds of peaks? To be the central part of the paper, the provided description is definitely insufficient.

Authors: We added the section 3.2, titled “implementation by cluster analysis” in which we better explain our proposed procedure.

- Section 4: a second clustering procedure is now applied to assess the lateral continuity of the seismic reflectors identified by the HVSR peaks selected in Section 3. In this case a few more lines are devoted to the description of the input parameters (essentially peak frequency, amplitude, coordinates and outcropping lithology) but the physical basis that should relate these parameters to the lateral continuity of the reflectors is not clear to me. If we observe the same HVSR peaks at 2 close sites, they are probably due to the same reflectors but this is just a supposition because they might also be linked to different reflectors at the same depth or to completely different VS-depth profiles that result in the same resonance frequency. Again: what is the aim of the authors? What does this AHC really do? Does it assess the geometric similarity of the peaks in the examined area? Why do the author link the peaks to the outcropping lithology only? H/V peaks are linked to the underground geology, not only to the outcropping material. How were the weighting factors selected? I read $w_{per}=0.4$, $w_{ampl}=0.2$ etc... where do these numbers come from? The authors also mention the “optimal threshold”: on what basis did they assess its value? The number of clusters depends on this threshold. It is very hard to understand the physical and mathematical bases of the whole procedure because they are not described and I am not even sure

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

that they exist if the underground geology is not known or taken into account.

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. It’s unlikely that two HVSR peaks at 2 close sites with similar resonance frequency are linked to different reflectors especially there isn’t an evidence of tectonic contact. Moreover in the section “4.Frequency maps” it’s been explained how weighting factors were selected. Outcropping lithology was not considered in the clustering procedure, but only in data inversion step as the underground geology.

- Section 5: p. 16-17... again the seismic noise composition? This is mentioned for the 4th time in the paper. On the bottom of p. 17 the concept is repeated for the 5th time. Also in this case I think that we do not need to read 2 pages that resemble the Geopsy manual (p. 17-18) or the other quoted papers. But, more importantly, I do not really understand if to invert the HVSR curve the authors used the borehole data or not. They state that one borehole only is available but not at the surveyed sites. Then they say that they used this borehole as constraint (p. 18) but on p. 19 they state that “the evaluation of the thickness of the cover did not take into account the available downhole data”. It is all so confusing...

Authors: The description of seismic noise composition has been removed and the information about Geopsy substantially reduced. To invert the HVSR curves we used only the stratigraphic information obtained by the available borehole, retaining the seismic velocity information not reliable. 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. The ambiguous statements in the text have been resolved.

According to the Occam’s razor principle, among competing hypotheses, the one with the fewest assumptions should be selected, in absence of further information. The

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



HVSR curve presented by the authors (p. 39, fig. 9) shows essentially a single peak at 1.4 Hz. Any HVSR curve showing N peak can be fitted by models with N+1 layer (the only exception being the presence of velocity inversions). In this case a 2 layer model is enough to fit the experimental curve (see figure below). The layer at 170 m depth proposed by the inversion procedure may also ultimately prove correct, but – in the absence of information from alternative sources that we do not have here – the fewer assumptions that are made, the better.

Authors: According with referee advice Fig. 9 has been modified, fitting the curve presented by a 2 layer model. The third layer were considered to verified a geological hypothesis.

- Section 6: after 20 pages another section with the tectonic settings at a smaller scale? There were 6 pages in Section 2 to describe this. 8 pages of geological description are really too much and out of topic.

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

OTHER ISSUES

- Title: what is ME? Where is Olivieri? That it is located in Italy is mentioned only on page 7!

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

- P. 4: anthropic noise is dominant also at frequencies well below 10 Hz (usually the threshold is put at 1 Hz, cfr. SESAME 2004 and much older works by Gutenberg, BSSA, of the '50s)

Authors: Different sources of noise may overlap at frequencies below 10 Hz, as reported in more recent paper (Webb, 2002). For example, a strong source of noise for frequencies between 1 and 10 Hz, especially on island or near coastline (like Oliveri), is

the wind (McCreery et al., 1993). So, it is not generally correct to put the threshold at 1 Hz. However this part in the text has been removed to synthesize the paragraph. Webb, S. C. (2002). Seismic noise on land and on the sea floor, in International Handbook of Earthquake & Engineering Seismology, W. H. K. Lee, H. Kanamori, P. C. Jennings, and C. Kisslinger (Editors), Academic Press, New York. McCreery, C. S., F. K. Duennebieer, and G. H. Sutton (1993). Correlation of deep ocean noise (0.4–30 Hz) with wind, and the Holu spectrum—A worldwide constant, J. Acoust. Soc. Am. 93, 2639–2648.

- P. 5: “substrate seismic”, what is this?

Authors: “seismic bedrock”, changed in the text.

- P. 9: how can the authors write $MW=6.15$ for an earthquake dated 1786??? We cannot use more than 2 significant digits for instrumental events (see all the problems of magnitude calculation listed e.g. in the New Manual of the Seismological Observatory Practice, P. Bormann ed.), how can they assess a magnitude with 3 digits for a non instrumental event?

Authors: The M_w 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 $MW=6.1\pm 0.4$.

- P. 9: completeness analysis of the catalogue: who and how found that the completeness threshold is $ML = 2.6$?

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

- P. 12: “capability of the HVSR curve to mimic the HVESR curves”: ?

Authors: ...earthquakes HVSR curves. However this paragraph has been removed in the text.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

- P. 12: “duration of the; “... something is missing here.2w

Authors: This paragraph has been removed in the text.

- P. 13: “the presence of the site effect due to a particular cause”. What is this cause that is mentioned several time? Do the authors mean a seismic reflector?

Authors: Yes, this was the meaning. However also this paragraph has been removed in the text.

- P. 13: “3C seismic digital station“ what is 3C? 3 components?

Authors: 3C has been changed in 3 component.

- P. 13: using 10 s windows to see 0.1 Hz components after the FFT is quite optimistic with H/V from ambient noise. The classical signal theory (and SESAME, 2004) recommends to work on windows that contain at least 4 time the minimum frequency that one wishes to detect (i.e. 40 s in this case)

Authors: This error has been corrected.

- P. 13: “to have a minimum numerosity of the sets of sampling windows selected for the analysis”, what does this mean?

Authors: This sentence has been changed in the text.

- P. 17: Rayleigh, not Reyleigh

Authors: Also this paragraph has been removed in the text.

- References: acknowledging previous work is definitely important but 7 pages of bibliography for a paper which is not meant to be a review paper but an original research paper look weird to me

Authors: According to the suggestion of the referee, references have been reduced.

- Table 1: H/V frequency and amplitude values are presented with 3 digits: is this

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



correct? Where are the standard deviations of data (they must be calculated and shown for each experimental curve and I am quite convinced that presenting them will reduce the number of significant digits to 2)

Authors: According with referee advice H/V frequency and amplitude values are now presented with 2 digits. All reported peaks respect standard deviation criteria for a reliable H/V peak (SESAME, 2004) as now specify in the caption of table 1.

- Fig. 1: one has no idea about where this place is located in the world

Authors: In figure 1 now a frame reports the geographic location of the studied area.

- Fig. 3: we see an H/V curve but so far location of the 23 sites surveyed in the town has not been shown yet (it is shown only in fig. 7)

Authors: Considering the paper structure, none of the previous figures are appropriate to indicate the location of the 23 sites surveyed. Anyway the UTM coordinates are reported in Table 1.

- Fig. 5: cannot be understood. What is the meaning of the axis?

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

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/2/C1478/2014/nhessd-2-C1478-2014-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 2597, 2014.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

