Reply to comments of the anonymous Referee #2
Review of Resubmission (version 4)

Referee:
The authors have addressed most of the comments of this reviewer. My recommendation is to accept for publication, but I also include a list of suggested modifications to increase the readability and technical content of the paper. Overall, I think this is an interesting model and I hope that the authors continue its development. To me, the biggest limitation of this model is that it is a model of models, and its validation relies uniquely on matching other model predictions. For this reason, the model has to be used with caution.

Reply:
We are again grateful to the anonymous referee. Your comments have greatly improved the paper and have helped in defining the potentiality of this model, highlighting its advantages but also the critical issues of the metamodels that require a general knowledge their base and therefore their best application contests. We believe that a continuous application on many cases and continuous development of the model can contribute to producing better performances and make more evident its advantages. In addition, we think that a future development in greater probabilistic analysis can be an additional strong point of this type of model. Hence, the words of the referee encourage and motivate us to move forward in this direction.

Referee:
One overarching comment is that the model aims to train site response in terms of the spectral shape of the response at the surface (or at depth zo). I am not sure this is the best option, because the output spectral shape will change significantly with each input motion. An alternative would be to train the model to capture the amplification function (e.g., spectral acceleration at the surface divided by the spectral acceleration of bedrock) for each spectral period. This is more stable across different input motions than the response spectra at the surface.

In general, this is true, the model proposed (metamodel) better performs when emulating the acceleration spectra with a regular shape; thus the code aims at analysing acceleration spectra response obtained using trainer spectra derived by using the input motion matched, alone or as an average, on regular probabilistic spectral targets (e.g. as those given by a building code). Hence,, preserving the actual computational setting is obtained that: i) it is not recommended the use of arbitrary input motions (also due to the fact that it is difficult to envelope their output with the design code spectral shape); ii) it is given the possibility to generate maps of different probabilistic return periods; iii) it is possible to considerate the different non linear response (equivalent linear in this cases) product using different input motions. For the next version of the code, we are considering the possibility to train the model to capture the spectral amplification function and return it as an acceleration seismic response spectra in the mapping developing module. This can be given as an option.

Added to the text:
(pag.3 row 33 to 37)
" The hybrid nature of the code shows a high performance in metamodeling when it uses an input motion with a regular (modal) acceleration response spectrum: a better performance is obtained when an input motion, matched (or fitted) in frequency with a design spectra shape (as is required in the EC8 and FEMA building codes), is given. In addition, many input motions can be inserted and processed using a partially different procedure (multi-input mode) as explained in paragraphs 3.1 and 3.2. "

(pag.7 row 31 to 37)

" On this subject, the multi-input motion mode performs the stratigraphic seismic response analysis for each input motion on all the VS-h selected profiles in a separate way. Therefore, average acceleration response spectra are obtained from a set of output acceleration response spectra computed for each zone; these average spectra are the trainer models used in the subsequent metamodel procedure.

However, it is worth noting, as previously stated, that better performances of the metamodel are given using input motions that provide an average response spectra matched (or fitted) on the design code spectra shape (a complete example is illustrated in figure 8)."

Referee:

1. Throughout the paper the authors use "seismic response" to denote the computed spectral acceleration at the surface. I am not sure this is common use. The seismic response can relate to any response to seismic input (e.g., the stress-strain curve of a soil element can be "seismic response"). I recommend that the authors use more direct terms. (e.g., "computed spectral acceleration").

Reply

The word "seismic response" was substituted with "acceleration response spectra" in the manuscript

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Referee:

2. The reply of the authors to comment 6b is factual (e.g., that is what their model implements), but does not address the larger issue of whether geometric parameterization at a constant scale can capture topographic effects across a wide range of frequencies. Analytical work has shown that longer period waves are affected differently by topographic features than shorter period waves.

Reply

In the topographic amplification module, the range of frequency captured by the model is balanced (centred) on the dimensionless frequency near to values $H/\lambda=0.2$, as in the Geli et al. model. In the model, the use of the height of relief H and the relief ratio $r_H=H/H_R$ (the latter is similar to a relative position index) permit the use of a univocal scale of the morphometric parameters. The use of different scales in the resolution causes that for low periods (recognized with notable pixels) the uniform value of big pixels joins, in a unique value, the ridge with part of the slope where theoretically it should be less. In addition, this condition tends to make uniform the different effects showed between the ridge, edge and slope when steep slopes are present.

Added in the text:

(pag.10 row 18 to 20)

"Thus, in SiSeRHMap, the topographic sub-module permits the simulation of the 3D surface amplification mainly on the basis of morphometric data and using an assigned uniform stiffness of the reliefs with the task of shifting the frequency distribution of the amplification data."

Referee:

3. *In the abstract, line 19, the authors state: "… the one-dimensional linear equivalent analysis produces acceleration response spectra of shear wave velocity-thickness profiles…". This should be modified because equivalent linear analyses need more input than simply profiles of shear wave velocity and thickness. The resulting response spectra is also a function of: a) input motion, b) modulus reduction and damping versus strain curves, c) soil density. I would recommend modifying the sentence to simply state that 'the one-dimensional linear equivalent analyses produces acceleration response spectra for site profiles for a given input motion.'*

Reply

This was corrected in the abstract, as suggested by the referee:

"In this process, the one-dimensional linear equivalent analysis produces acceleration response spectra for a specified number of site profiles using one or more input motions".

Referee:

4. Page 1, Line 37. What is meant by "local grassroots hazard"? the term grassroots, at least in the United States, tends to be used to define political movements that arise from the people (rather than led by party leaders). It is not clear how it applies to hazard.

Reply

This was corrected, pag.1 row 38 to 40 :

" Many building codes, such as Euro Code 8 and FEMA 356 (2000), require seismic design actions defined by simplified elastic acceleration spectra deriving from local base seismic hazard (as reference natural or virtual stiff rock site which are defined in term of horizontal acceleration probability of exceedance in specified time interval) and site amplification effects."

Referee:

5. Page 6, line 23. 1D site response assume vertical propagation of a plane wave, not a line wave.

Reply

This was corrected, pag.6 row 43 to 44 :

The module computes the dynamic acceleration response which refers to a one-dimensional soil column using a vertical planar wave propagation model.

Referee:

6. Page 1, Line 58, please clarify the sentence "contextualized to the applied seismic response". Do you mean to say "Contextualized to practical application in seismic site response studies …"?

Reply

This was corrected in the abstract, as suggested by the referee, pag.1 row 60:

"Contextualized for a practical application in site seismic response studies,...."

Referee:

7. The statement in page 2, lines 4-6: "Therefore, the map-sets of 4 seismic response provided by SiSeRHMap are the result of an advantageous compromise between intrinsic and 5 epistemic uncertainties and the accuracy and robustness required." Are valid insofar as they refer to spatially distributed analysis, which is obviously the focus of this study. For site-specific studies, such as those for nuclear power plants or other critical facilities, the interplay of intrinsic (aleatoric) and epistemic uncertainty is much more complex. The authors may want to highlight that the intended application of their code is not site-specific studies but geographically distributed studies.

Reply

This was added to the text, pag.2 row 8 to11:

"This last aspect reflects the aptitude of the proposed methodology which is suitable for analysis of urban areas or relatively vast areas. In general the level of accuracy of the SiSeRHMap response increases with the number and quality of the surveys; however it is suitable to be used in areas with common and non-strategic facilities (e.g. nuclear plants); for strategic facilities, a detailed analysis may be required due to the fact that the use of a metamodel might not ensure the level of accuracy required."

Referee:

8. Some of the terminology used is not familiar to me (I am an expert in earthquake engineering, not in GIS). For example, page 3, line 14 uses "rigid reliefs". I am not sure if the term rigid means that the reliefs are on exposed rock or if "rigid relief" refers to some type of relief. Given that the readership of the paper is likely going to reflect my own professional profile, I advise that the language be made clearer.

Reply

This was corrected in the text, pag.3 row 21 to22:

.... : the term " rigid /quasi rigid " refers to the shear wave velocity values of the material constituting the relief......

Referee

9. Page 3, line 25. The use of the word "bedrock" usually implies the rock layer that is underlying soil layers and is the base of the profile in a site response analyses. Since the authors are introducing two bedrock layers, I recommend to use the term "rock layer", and reserve bedrock to the layer termed "rigid bedrock"

Reply

This was corrected in the text.

Referee

10. Sentence in page 3, lines 26 to 31 is very convoluted and difficult to understand.

Reply

This was corrected and added in the text, pag.3 row 40 to 41:

"The number and spatial distribution of the survey points are assumed coherent in the parametric characterization, and in the geometric features of the lithodynamic units, in reference to the simple subsoil setting of the SRS. For example, if in the first analysis a lithodynamic unit is defined taking into consideration only one lithological feature, and the regression analysis does not fit well the V_S-z points distribution, it is possible to re-associate two or more lithodynamic units to the same lithology with the follow criteria: i) clustered spatial distributions of stiffness (V_S) are recognized (horizontal accuracy), ii) different regression curves result as being more appropriate for characterized different depth level steps (vertical accuracy). However, in real case analyses and ignoring the ability of the modeller in the subsoil model prediction which is based on using and/or interpreting direct or indirect survey data, the number, typology and spatial distribution of data must be taken into account in relation to the geological complexity of the real area and the required reliability accuracy degree desired ."

V_S-z distribution is reported in figure 1

Referee

11. Page 3, lines 53-54. The use of "formal physic dynamic behavior" is wrong. All behavior is physical, so the qualifier is not needed. I would suggest replacing the whole sentence by simply saying "the term qualifier "rigid" for the bedrock implies only relative rigidity and does not imply a rigid (e.g., infinite stiffness) layer"

Reply

This was corrected and added in the text, pag.4 row 11 to13:

".... $V_{S_{rig}}$; in general terms, the aforesaid bedrocks typology can represent lithodynamic units composed respectively of massive rock or weak rock. Accordingly, the term "rigid" qualifies a relative and not absolute stiffness (e.g. infinite stiffness) of the bedrock."

Referee

12. Equation 2 should be properly justified. The text states (line 21). “The linear law used for bedrock (Eq. 2) meets the linear nature trend of the stiff soil in depth.” This statement is confusing. If it is used for bedrock, why is it relevant that it would meet trends in stiff soil? Moreover, I am not aware of any publication that justifies a linear increase with depth for shear wave velocity in stiff soil. The authors should give a reference or show data to prove their claim.

Reply

Thank you, this justification is needed: it is reported in pag.4 row 44 to 48:

"The assumption that the uniform layers that have a progressive increase in strength and stiffness with depth is due to the increase of the effective stress and to the weakening of the material near to the surface when it is in outcropping. This assumption is well noticeable in the progressive increase of SPT N60. Hence , taking into consideration the SPT N60-VS correlation equations for all soils, including stiff soils (Ohta and Goto, 1978; Imai and Tonouchi, 1982; Lum and Yam, 1994; Rollins et al., 1998), it can be seen that the non linearity correlation occurs only with regards to low N60 values; conversely, a good linear correlation is observed for high N60 values. It is worth noting that the relation of Vs increasing with N60-SPT values is independent from the depth. Therefore, for the material constituting the non-rigid bedrock, the Vs-depth linear increasing relation can be considered valid both in the buried and outcropping condition. "

Referee

13. Is Equation 3 correct? How do we infer a constant velocity for bedrock from this equation?

Reply

Equation 3 has been better clarified

Referee

14. A standard deviation is mentioned in page 4, line 23. This is the first reference to standard deviation and it is not clear what it refers to.

Reply

Phrase deleted in that position and reported in the previous position, (see reply 10 r; standard deviation is shown in figure 1.)

Referee

15. Page 4, line 33. Why is the output requested at a depth z(out) and not at the surface? This is not consistent with traditional output of microzonation or with building code recommendations (they all provide surface motions). If the output is at depth zo, are the authors providing “outcrop” or “within” output motions?

Reply

This has been specified in pag.5 row 6 to7:

".... in 1D seismic response analysis (mod.3 paragraph 3.1), the $h_{(min)}$ is returned in the corresponding outcropping lithodynamic unit for the computation."

see you also pag.7 row 10 to 12:

"..The output response (fig. 6) is provided at the outcropping of the surface detected by the assigned z_{out} depth; this surface is within the upper layer."

Referee

16. Page 4, lines 38-44 are not clear to me (again, I do not have background in GIS). For example, the Layer_n.txt files refer to the extension of the covered layers, which to me would mean a length unit, however the authors say that the input is in terms of zeros and ones. Also, the extension of Zones.txt is given as an integer. Why? Why not a real number? My lack of understanding is likely because of my limitations in GIS, but I would recommend that the authors try to make the paper accessible to earthquake engineers.

Reply

This has been specified in pag.4 row 8:

"Summarizing, the georeferenced input raster data (ASCII grid file format) is:"

Referee

17. Page 5, line 55. Clarified what the term "dispersed" is used for. Page 6, line 1, clarify what "circumstantiated" is used for.

Reply

This was specified in pag.5 row 26:

1D subsoil models as selected in random uniform way

"*circumstantiated,*" we have substituted the phrase

Referee

18. Page 8, line 38. Is the choice to envelope the computed response spectra (the authors use seismic response spectra, which I find confusing, since the input motion is also a seismic response spectra) dictated by code decisions? Or is it arbitrary? The authors should document this. Note that modern codes generally have probabilistic targets (e.g., 2% probability of exceedance in 50 years), so an envelope is not generally the most adequate solution. I am not recommending that the authors perform a probabilistic analysis, but they should document the reasons for their choice of enveloping the computed response spectra.

Reply

This has been clarified, pag.7 row 29 to 35:

However, the smoothed responses, generated by the trained metamodel, suggest a better performance for input motions with the acceleration response spectra nearest, or matched, to the simplified code design spectra. .On this subject, the multi-input motion mode performs the stratigraphic seismic response analysis for each input motion on all the VS-h selected profiles in a separate way. Therefore, average acceleration response spectra are obtained from a set of output acceleration response spectra computed for each zone; these average spectra are the trainer models used in the subsequent metamodel procedure. However, it is worth noting, as previously stated, that better performances of the metamodel are given using input motions that provide an average response spectra matched (or fitted) on the design code spectra shape (a complete example is illustrated in figure 8).

Referee

19. Page 8, line 49. What does it mean "lithodynamic is not present in the layer?" please clarify. In my understanding a layer is lithodynamic if it is characterized by Vs depth curve (page 2, line 32).

Reply
This has been clarified, pag.4 row 3 to 6:
"In each zone, the presence or absence of the lithodynamic unit is defined in a binary way with attributes respectively value1 and 0. Hence, the layer, the computational entity always present in the matrix, assumes a physical entity inside it where the lithodynamic unit formalizes its presence assuming value 1. "

Referee
20. Page 10, line 47. Maufroy is misspelled. (this occurs elsewhere in the text as well)

Reply
This has been corrected

Referee
21. Figure 13 is visually fascinating, but understanding it is akin to building a puzzle; a wonderful and fun enterprise when the pieces fit nicely, and a horribly frustrating one when they don't. In either case, it baffles this reviewer why the authors would want to put this burden on the reader. For example, it is not clear at all in Page 11, line 5 what the labels (seven) and (three) imply. Reference in the caption to "top right of the panel" makes it confusing because usually the panel refers to the whole figure, not just part a or b. In part a, insert captions directly into the figure (top right of the panel) so that it is easier to read. What are the units on the figure in part b, at the middle of the three figures? What is "empty cycles" (in caption)? Does "senddle" mean "saddle"?

Reply
The figure has been corrected and modified as suggested by the referee.

Referee
22. Page 12, line 8. What does "over imposing" mean?

Reply
The word has been substituted with " it is possible to hypothesize a net overlapping spectra between the stratigraphic and the topographic effects"

Referee
23. Page 13, line 8. There is an orphan "fig."

Reply
It has been deleted.

Referee
24. Explain what Figure 16b is. The contours plot shown are, I assume, computed from the proposed model. Where is the comparison with Quake-W?

Reply

In part b) of the figure the comparison analysis in the PGA-graphic has been added; Quake-W report spectral values (graphic) have been included only in some limited specified points.

Referee

25. *The computation of the response spectra is generally computed using an algorithm by Nigam and Jennings (1969, in BSSA 59, 2, pp 902-922). This algorithm is exact for piecewise linear input, which is what is generally assumed for digital acceleration time histories. The deviations in the computed response spectra from other algorithms are not significant, so this is not a big issue.*

Reply

Thank you. We will take this into consideration in future developments of the code. In this version, we use the tompy.py library module (Irvine, 2014) analytically based on "an improved recursive formula for calculating shock response spectra " by David O. Smallwood. The author specified that:

"Currently used recursive formulas for calculating the shock response spectra are based on an impulse invariant digital simulation of a single degree of freedom system. This simulation can result in significant errors when the natural frequencies are greater than 1/6 the sample rate. It is shown that a ramp invariant simulation results in a recursive filter with one additional filter weight that can be used with good results over a broad frequency range including natural frequencies which exceed the sample rate."

Reply to comments of the anonymous Referee #3
Review of Resubmission (version 4)

Referee:
The paper by Grelle et al. propose a computer code for the simulation of both topographic and stratigraphic amplification effects on seismic waves. The core of the model is a spatially-extended 1-d computational code, complemented with a simulation model and a topographic effect estimator, taking advantage of GIS techniques for data handling (input and output). The paper may be suitable for publication, provided that some corrections are implemented as suggested in the following comments:

1- GCM for Vs-h trainer models The sentence "it shows relatively high values of the shear wave velocity in the Vs-z dispersion curve" may be misleading. The term dispersion curve is usually referred to the variation with depth of the phase velocity of seismic waves. Here is probably used instead of "depth-varying uncertainty".

Reply:
This was corrected (pag.6 row 13) :
.....values of the shear wave velocity in the Vs-z uncertainty curve.

Referee:
The authors mention that "A horizontal polarized propagation of the shear waves through a site with infinite horizontal layers is assumed". In their GIS Cubic model, strata are not horizontally unbounded, and lateral variation of velocity may occur. The authors should justify while this is not taken into account.

It should be mentioned that the simplified spectral shape provided by this model are valid only under several assumptions: 1) the site response is 1-d only, without influence of 2-d effects like closed valleys, sharp variation of the buried morphology 2) independence of site response to azimuth and incidence angle 3) absence of velocity inversions.

Reply:

The current limit of the model is addressed in the discussion paragraph. Taking into account the afore-cited suggestion of the referee, we have explained in more detail the subject regarding the limit in the use of the 1D model in subsoil characterized by a notable gradient (L/H≤ 8-10). The next development of the model will be focused on the prediction of these aggravating effects mainly in the buried basin.

This was explained in the discussion paragraph attempting to meet the suggestion of the referee:
"The maps producted by SiSeRHMap may suffer of substantial uncertainties when high complex subsoil features are present. The latter are summarized in the high slope degree of the interfaces (L/H< 8-10 in Hasal and Iyisan, 2014) and in general by sharp variation of the buried morphology. On this effects, it is noted as 1D seismic response seems to be underperformed mainly at the edge of the valley (Gelagoti et al., 2010). iv) independence of site response to azimuth and the wave-incidence angles with subsoil interfaces. "

The SiSeRHMap is capable of computing VS profiles showing velocity inversion with depth; emul-spectra is an adaptive prediction acceleration spectral model (metamodel) which is not dependent on the physical model that provided the trainer target spectra.

Referee:

The sentence "aims at predicting the spatial amplification effect on the seismic response of reliefs considering them to be constituted by homogeneous material" is not clear. Does it means that on part of the model the variation of Vs with depth modelled by GCM is not accounted for? This is also important, because the numerical model quoted in this section provide the maximum value of amplification when the wave is vertically incident on the slope. The verticalisation of seismic ray path occurs thanks to the lower velocity encountered in the surficial strata. This is why the assumption of vertical incidence for stratigraphic model is almost always satisfied. This is not true for a slope of uniform rock. A vertical incidence can be obtained at the epicentre only, and any other angle of incidence will be preserved in an uniform velocity model, giving substantial overestimation of the topographic effect (as observed in real earthquakes, as shown in some of the paper cited, e.g. Gallipoli et al.).

Reply:

Locally, the GCM model can reproduce only the outcropping bedrock, therefore the optimized prediction (uniform material) of the topographic amplification model results as being independent from the GCM. However the general comment of the referee on the ability prediction of the model is right, and a more detailed clarification has been included on this topic. We are grateful to the referee for highlighting the critical issue of the proposed computation model that aims at predicting a surface seismic response via GIS tools and metamodels.

Regarding this, the large/high reliefs are usually made up of massive rocks having high stiffness which are near to the uniform stiff condition; these reliefs are naked or present a thin weak covered layer at the near surface. These reliefs frequently sustain part or whole urban area. In the illustrate case of Albion Plateou Area is showed as in "near-field" in presence of different incident angles can developments an high range of the topographic amplification values also in relation of the slopes and the aspects of the reliefs. At the "far-field" conditions is very improbable that propagation of the waves largely diverge from vertical; theoretically, the ray direction is not imputable to direct vector propagation from the source, but deep reflection and refraction permits that seismic energy transfers are propagate at long distance and vertical incidence of the seismic rays occurs. However for the reliefs constituted by subsoil with notable stiffness variations, SiSeRHMap permits an optimized spectral distribution assigning their an equivalent uniform shear wave velocity obtained by means the analysis of the seismic noise or instrumental earthquakes.

Referee:

Appendix In the description of formula 2A substitute "dumping" with "damping"

Reply

This has been corrected.
