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Interactive Comment

Interactive comment on "SiSeRHMap v1.0: a simulator for mapped seismic response using a hybrid model" *by* G. Grelle et al.

Anonymous Referee #2

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General Comments

The authors propose a methodology for performing site response analyses for distributed sites. The inputs to the model are obtained from a GIS system. The site response is computed using equivalent linear analyses along with a numerical adaptive simulation model to generate maps of seismic response. Topographic amplification is also included through analytical solutions for idealized topographic features. The output of the code are seismic response maps and sets of seismic design response spectra that are obtained as an envelope to the mapped response spectra.

The paper is well written and presents an interesting model. However, the entire model is postulated on a purely deterministic framework. This presents challenges to the stated applicability of the model. For example, the design spectra postulated by the model is based on a single realization of the 3D GCM. Given that there is always uncertainty on subsurface conditions, it is important (in fact, key) that engineers account for this uncertainty in selecting an eventual design spectra. The same can be said for other uncertainties in the model, for example those resulting from the use of a single input motion, given that it is known that there is a large degree of motion-to-motion variability in the amplification actors resulting from site response.

This reviewer recommends that these limitations should be discussed up-front. The proposed model does not replace a careful accounting of the uncertainties that are inevitably present in site response modeling. It simply provides a computational platform for conducting analyses that can inform the choice of design spectra or a microzonification study.

The comments below list more specific questions/issues raised by this reviewer.

Specific Comments

1) Comment related to uncertainty: a) Please discuss how uncertainty in lithological properties is accounted for, and, if applicable, whether this uncertainty propagates into the resulting spectra at the surface. b) The use of models on top of models (e.g., the adaptive simulation model on top of a 1D site response model) implies the presence of epistemic uncertainty in the predictions. Since the authors are not accounting for these uncertainties, they should clearly state the limitations of their model

2) How are the modulus reduction and damping curves needed in equivalent linear analyses selected for different lithological classes?

3) The use of the adaptive simulation model implies the use of a model to reproduce results of another model (equivalent linear 1D site response). How is this justified? The authors do not explain with sufficient clarity the justification for building the adaptive simulation model to reproduce 1D site response. Is it for computational efficiency? Or to obtain values that have a smoother spatial variation?

4) Equation 11 uses an average shear wave velocity that results from a weighted average (where the weighting factor is profile thickness). This is not common practice in earthquake engineering. Average shear wave velocity is defined using the travel-time average [e.g., sum(h,i)/sum(h,i/Vs,i)]. This way o computing average shear wave velocity is coded in US and European building codes and is the basis for the computation of Vs,30. Note that the choice of using travel time to compute average shear wave velocity is not arbitrary, it reflect the fact that average velocity computed in this way will result in more realistic fundamental periods.

5) First paragraph, page 4499. What is meant by dispersion curve? Generally the term dispersion curve is reserved for the change in surface wave velocity with wavelength or frequency. However, the term appears to be used here to the statistical uncertainty in the Vs,z value.

6) On the topographic model a) Equations 17 to 19 were developed by the team with the intent of reproducing topographic effects postulated by other authors (Geli et al. 1988, Ashford and Sitar 1987). The only validation presented in the paper for these equations is in Figure 10, where the fit of the proposed model to those of Geli et al. is very poor at some periods. A stronger justification for the choice of the model is needed. The authors also mention a validation through comparison with Maufroy et al. (2012 and 2015), but this comparison is not given in the paper. b) It is not clear how the parameters of the model (H and Hr) are computed for different frequencies, since the scale of the topographic feature will depend on the frequency. For example, at high frequencies a small feature may affect simplification, while the same feature will not have an effect at larger frequencies. Hence, H and Hr should be frequency dependent. It is not clear from the formulation that this is the case. c) The models such as those proposed by Geli et al. (1988) and others are based on idealized topographies. IN the experience of the reviewer, it becomes very difficult to select parameters such as H and Hr when the topographic relief becomes very complex. Even parameters as simple as slope and curvature will be a function of the scale of the DEM. d) The authors mention

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“topographic fundamental period” in Figure 10 and Page 4509. How is this computed?

Editorial Corrections

1) Line 1, Page 4493: change “liner” to “linear”

2) The use of the term rigid in the way it is being use (e.g., to define bedrock velocities higher than a certain threshold) can be confusing because the word rigid would imply an infinitely high shear-wave velocity. A truly rigid boundary does not exist in nature, but some numerical models postulate rigid boundaries for simplicity. Moreover, the threshold of 800 m/s does not make a very rigid bedrock in engineering terms (for example, shear wave velocity for bedrock in the Eastern United States can be as high as 3000 m/s).

3) Line 2, Page 4501. Separate “therefore” and “it”

4) Line 14, page 4501. The use of the word “experimental” brings to mind laboratory tests. In this case, the authors are referring to a “trial” strain level. Please modify the wording.

5) Line 14, Page 4512. The reference should be to Figure 13.

Interactive comment on Geosci. Model Dev. Discuss., 8, 4487, 2015.