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Solid Earth
Discussions

Interactive comment on "Regional wave propagation using the discontinuous Galerkin method" *by* S. Wenk et al.

S. Wenk et al.

wenk@geophysik.uni-muenchen.de

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Overall, the paper presents a good start for these kind of studies, however, I have the feeling after reading that, in its current form, it seems a bit incomplete in terms of discussions on the performance and simulation results to present the advantages of the proposed method. My detailed comments and suggestions are as given below:
1- The authors compare synthetic seismograms computed by the presented method using 1D PREM model to those from SEM which were benchmarked with normal mode seismograms. The authors could have directly compared their 1D experiments to the normal mode seismograms as well. What is the reason of using spectral element seismograms for 1D comparisons?

See answer to comment of reviewer 2 p. 1140, line 20

2- In Figure 6, the comparisons of observed data with synthetics computed in 3D crustal model EPCrust (Molinari & Morelli 2011) are nicely illustrated. However, it is not clear what the main message is here. The comparisons are not very quantitative and do not tell much about the validity of simulations in 3D models. The main advantage of the method described in the paper is the flexibility of meshing complex structures with unstructured tetrahedral meshes which can be critical to honor especially crustal structure, Moho and surface topographies etc. Following the previous remarks, 3D simulations could have been compared to the SEM simulations which would lead to interesting discussions on the implementation of 3D crust in numerical simulations and the advantage of the flexibility of unstructured tetrahedral meshing to honor crustal structure.

At the time the simulations were carried out only the global version of SpecFEM was available, but a regional SE version would have been necessary to perform the requested simulations.

3- It is not surprising that body wave agreements between synthetic and observed data are quite good since they are less sensitive to the crustal heterogeneities and even 1D models are good at explaining the body waves. This can easily be verified by also plotting seismograms computed for the same paths using a 1D background model in Figure 6. However, it is worthwhile to mention that surface wave misfits should also be biased due to the use of 1D mantle model in simulations since the period range considered in the paper ($> 33s$) is sensitive to both crustal and uppermantle structure. I suggest to add 1D seismograms to Figure 6 to see the effect of 3D crust and even 3D crust + 3D mantle on waveforms.

Due to the demands of the DG modelings, we would like to desist from addi-

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tional simulations, also under consideration of the scope of the paper. As already mentioned, the paper is a feasibility study of the DG method applied to regional wave propagation problems. We think it clearly shows the applicability of the method, as an alternative to the established SE method.

4- Since the authors take SEM method as a reference for the validation of their method, that would be good to mention the computational cost and performance analysis compared to SEM method which help better understand the advantages and disadvantages of the method.

See answer to comment of reviewer 1 page 1142, lines 3-8, and reviewer 1 page 1145, lines 8-10

5- Figure 2 & 3: Show the source and receiver locations or at least denote the epicentral distance of each station-source pair. From the plots, it looks like the focus is on minor arc surface waves but please explain the reason of not using the same duration of simulations for all source-station pairs.

Changed figure

6- Figure 4 needs some clarification. A map on top of the surface mesh would be helpful to locate the area of interest. Instead of density, shear-wave speeds would make more sense to show the complexity of the reference 3D crustal model.

Changed figure

Interactive comment on Solid Earth Discuss., 4, 1129, 2012.

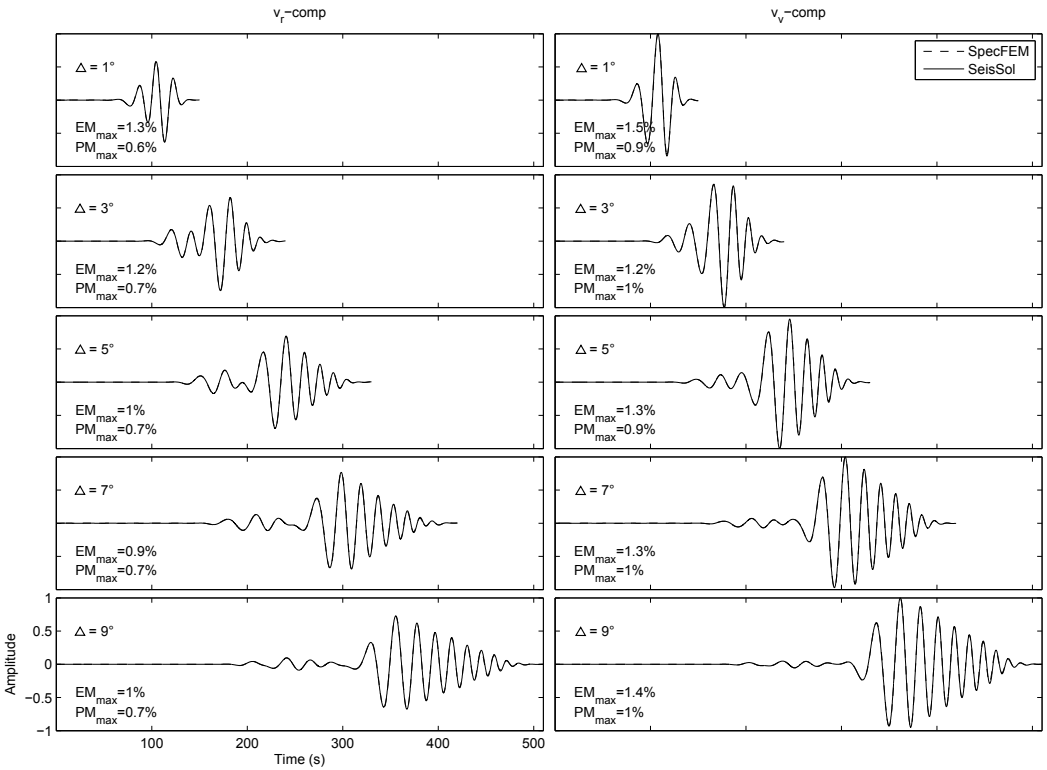

Fig. 1.

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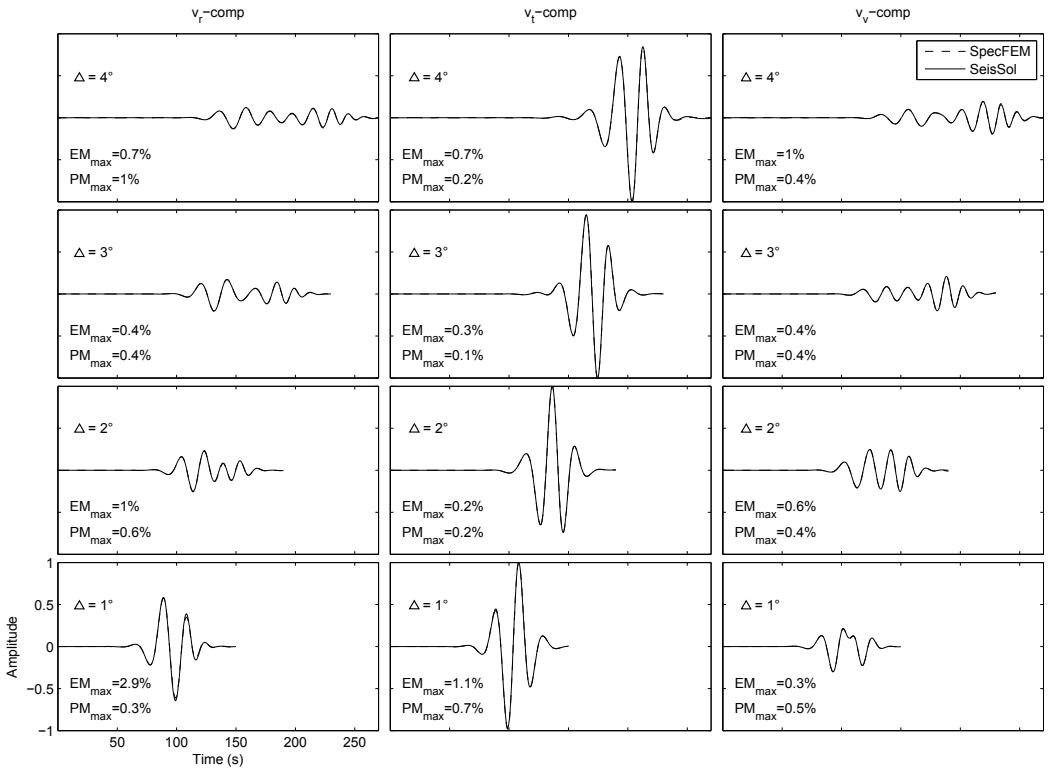

Fig. 2.

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