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Solid Earth Discussions

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

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I'd suggest clarifying two points:

1) differences in performance, and
2) differences of the results.

About 1): We know that the efficiency of a numerical scheme depends on the particular problem under consideration, and on how much effort has been spent on optimizing a particular coding. The authors should still keep this disclaimer in the text, but nevertheless I think they should give the numbers for the particular case they studied. There are some useful suggestions about how to do this in the comment by Vincent Etienne.

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

About 2): The authors compare ADER-DG and SEM using 1-D isotropic PREM. The main discontinuities are honoured in both meshes. In this way, proof of the high numerical accuracy of both methods can be achieved, but nothing can be said about the different meshing strategies. I'd suggest simulating the L'Aquila waveforms with SpecFEM, and comparing the results in fig. 6. Again, we know that a rigorous comparison is difficult, and no general conclusions can be drawn from one particular application. But it would give us a qualitative impression of the differences between a target solution (ADER-DG) and a simulation that does not honour the Moho explicitly (except for certain ranges in SpecFEM V5).

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

Independent of 1) and 2), the introduction of ADER-DG to regional problems is a valuable step forward, given that it has the desirable property of permitting a more precise implementation of a given Earth model. I enjoyed reading this manuscript, and hope that my suggestions might be useful for the authors. Some minor comments to the authors (trying to avoid overlap with the very detailed comments by Vincent Etienne):

P. 1133, line 16: You say that ADER-DG is a Galerkin scheme, but to make clear the hierarchy of terminology (for the readers less familiar with such schemes), you might say explicitly that SEM is a Galerkin scheme, too. The conceptual difference is not due to the "G" (Galerkin vs. non-Galerkin), but due to the "D" (discontinuous vs. continuous, as you appropriately explain in the next paragraph)

Corrected
Old: It is referred to as a Galerkin scheme, because test functions are chosen from

the same basis function space.
New: As most of the FE type methods used in computational seismology (e.g., the classical FE, SE and DG method) also DG is a Galerkin scheme, because test functions are chosen from the same basis function space.

p. 1134, line 15: "the size of the elements can vary tremendously..." How much is tremendously? Is there any limitation in practise? What range of element sizes is actually used in your regional model, for example within the crust?

Added
Depending on the problem and the refining strategy, within the computational domain the size of the elements can vary without any known restrictions, e.g., up to an element size ratio of 1000 (Dumbser et al., 2007).

p. 1136, line 13: produce -> produces

Corrected

p. 1137, line 15: effort -> effort, (comma)

Corrected

p. 1139, line 5, imprinting surfaces, conforming mesh: Please explain. You might introduce the concept of conforming meshes and its relation to ADER-DG before.

Reformulated, see answer to comment of reviewer 1 page 1139, lines 4-6

p. 1140, line 8, boundary conditions: Absorbing? More information required.

Added
In our implementation of the DG method, absorbing boundary conditions are applied (Käser et al., 2006).

p. 1140, line 20, SpecFEM: it is not clear why you choose to benchmark the code against another numerical method, instead of to benchmark against a quasi-analytical solution directly. Especially since you interpret very subtle waveform differences later on.

Originally the study was designed as a quantitative comparison of the DG and SE method for a different purpose. However, our general idea was to use already computed data (shown in section 3) to benchmark ADER-DG with a well established method to support the reliability of the results in section 4. We agree that a more sophisticated verification and validation with quasi-analytical reference solution would have clearly improved the importance of the manuscript, but this was not feasible within the given time to react on the review. Furthermore, we tried to be more carefully in the interpretation of subtle waveform differences.

p. 1142, line 10: peak frequency -> corner frequency

Corrected
cut-off period

p. 1148, line 17: Explain/ interpret fig. 5: The shape of the STF does not matter, since you use a low pass (33s) clearly longer than the source duration, so seismograms are affected by a time shift, only.

That's right. It was added for the sake of completeness.

p. 1148, line 23, P-wave fits: Good P-wave fits for periods >33s are not such a big deal, you would get them from any 1D code as well.

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

p. 1149, line 5, boundary reflection: I'm not sure if I'm looking at the right wiggle, but what is it's origin, i.e. what type of wave, and from what boundary?

Corrected, see answer to comment of reviewer 1 Page 1149, lines 4-6

In fig. 6, three-component waveforms should be shown, definitely.

Added figure

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