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Interactive Comment

Interactive comment on "AxiSEM: broadband 3-D seismic wavefields in axisymmetric media" *by* T. Nissen-Meyer et al.

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Dear editor, Dear authors,

First of all, I am sorry to be late for this review process. The manuscript entitled "AxiSEM: broadband 3-D seismic wavefields in axisymmetric media" describes the consecutive developments and efforts on the 2.5 spectral element method, which has been one of the authors' long-term projects. I am very happy to know that now AxiSEM can go to very high frequencies. I find this paper very nice in the sense that the authors explain one by one technical issues that users would address, in order to see where are the limitations of this method and to what kind of problem we could implement AxiSEM easily, etc. This paper will surely help AxiSEM users and make them feel easy to get access to it. Personally speaking, as a developer of a coupling method (Monteiller et al. 2013), I would like to explore some further future projects and limitations of this method.

The authors could describe a little bit more thorough review on 1D to 3D global waveform modelling since the main purpose of this paper is to show pros and cons. For example, it could be fair enough to include 2.5D FD code (Jahnke et al. 2008) even though it is just for SH waves for the time being.

For quasi-3D wave propagation methods, one can also refer to Takeuchi et al. (2000, PEPI) paper since they tried to use high order Born approximation, using the Direct Solution Method, in order to reproduce waveforms for a 1D Earth model with 3D heterogeneities. The problem (or question) there is that we do not really know how the truncation of Taylor expansions behave in reality under the existence of strong lateral heterogeneity. But the authors can mention this methodology for the possibility of extension of AxiSEM for quasi-3D methodology, which should be interesting to implement, since we can expect a better convergence in high order Born approximation with exact 2.5D solutions. With the help of partial derivative calculation for discontinuity topography (Colombi et al. 2012), will it be possible to model quasi-3D wave propagation with 3D discontinuity topography?

Also, it might be beyond the scope of this paper, but I would be happy to participate (as I discussed with T. Nissen-Meyer some while ago) to do apple-to-apple comparisons of existing codes for global seismic wave propagation simulation, which, of course is a challenging project, rather than Fig. 1 which I think is a little bit tricky.

Here are some minor comments:

i) Fig. 1: Why is the normal mode is not faster than other codes even in low frequencies? If we extrapolate the AxiSEM behaviour, one could reach much below the modes around 100 s period and it is, at least to me, counter-intuitive. Is it the combination of catalogue calculation + reconstruction of waveforms?

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ii) subsection 4.6: Although authors submitted to GJI about anelastic attenuation, it is very important to see what kind of relaxation mechanisms are used and how they are implemented, so here authors are encouraged to describe some fundamental equations for readers to be sure about these questions.

iii) subsection 4.7: The lack of ellipticity is rather a big issue, but one can understand the difficulty. But still I would like to see coming strategies that the authors will attempt to do.

iv) subsection 5.8: Could it be possible to precise how well could authors accelerate kernel calculations with the help of NetCDF4? I could compare that with Fuji et al. (2012) methods. . .

Interactive comment on Solid Earth Discuss., 6, 265, 2014.

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