Review of the paper "Simulation characteristics of seismic translation and rotation under the nonlinearity in small deformation" by

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The authors present an updated version of their work studying the non-linear effects in seismic wave propagation. This is a much clearer work and it is evident the large amount of efforts that the authors put on this updated version. Without stating whether they are correct or not, one can clearly read the motivation, the mathematics and the results. This means that now the paper has a clear structure, which did not have before.

In the next, I elaborate on each part of the structure. Before doing so, I have to acknowledge that the authors have done a monumental work, and despite this I will recommend that there is still work to do. This I understand, can be very frustrating and painful when one is writing this kind of work. However it is a process that, I believe, all of us that work in this line of research have to learn and to endure.

1 The motivation of the work

The motivation and importance of the work is clear now. The authors have added pertinent references and highlighted why this work is important to do and to publish. To the best of my knowledge, it has not been done before. So good on this side.

2 The mathematics

The very complicated mathematics has been removed, however there are still errors /modifications to be done in the equations.

- In eq. (9) the authors insist on using the symbol θ inside the equation. This on the one hand is very unusual and on the other hand is very confusing and mathematically inconsistent. The symbol θ is defined as the principal strain in the previous paragraphs. It is define in terms of the sub-indexes 1,2,3. Eq. (9) on the contrary is written in terms of i, j, k. So imagine now that I write Eq. (9) in terms of i, j only. What happens to the definition of θ then? which ones are the two sub-indexes that are going to survive? can you see the inconsistency?

  I have to add that I do not see any need of writing eq. (9) using θ, I find it confusing, inconsistent and additionally, nobody does it this way. So why the authors have to add this complexity into the very well known linear elastic equations? Please write it in the conventional way or in a consistent way, but keep in mind to keep it simple and understandable.

- What is dt doing there in those equations of motion (eq. (11)) that have not yet been discretized?

- Can you please add with an underbrace which are the new terms included in the equations? In comparison to the linear elastic case. I mean, something like these are the old terms and these are the new terms. Use underbrace or use colors, blue for the linear elastic equation and red for the new terms. This will add clarity to the equations. You can also split the equation con continue in the next line. This will allow to write an equation with larger letters. Currently the equations of motion are very small in comparison to the main text.

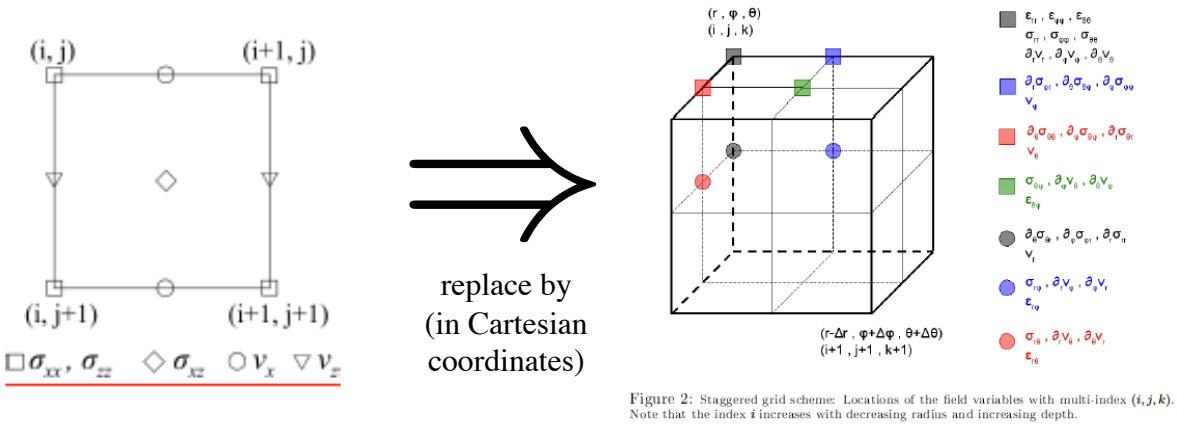

Figure 2: Staggered grid scheme: Locations of the field variables with multi-index (i, j, k). Note that the index i increases with decreasing radius and increasing depth.

Figure 1: Please replace by the 3D staggered grid instead of the 2D. The 3D figure is taken form the manual of the code FD3S.

3 The results

The results are interesting however they need a new presentation and the authors still need to, I am very sorry to say (because I do understand how painful this is), to validate the code.

3.1 Validation of the code

So let's start from the (most painful part) validation of the code first and then go to the easy part (the graphics). The authors include a plot of the not successful validation fo the code. Yes only people who work with specfem know how difficult this part is. The authors tried to directly benchmark their code with specfem and this is of course difficult to do (not impossible however). The point is simply, one cannot believe any results of a code that has not been benchmarked and the staggered grid discretization is not clearly explained.

In the next I offer three different paths that the authors can take, which in my opinion, are easier to do in comparison against learning how to properly use specfem3d.

- There are some folders inside specfem3d/EXAMPLES/ that give you the analytical solution. The authors can take this path and try to do a match of these analytical solutions found in the EXAMPLES folder.

- There is, in my opinion, an easier path. The authors can go to the SPICE Project webpage SPICE Project and download the package FD3S FD3S exactly here FD3S-here. The FD3S model seismic wave propagation in spherical coordinates using sponge absorbing boundary conditions and including viscoelasticity. It has been initially written by Prof. Heiner Igel. It is already benchmarked and most importantly it uses staggered grid in 3D. This comes because it is not clear how the authors do the staggered grid in 3D. There are numerous ways to do it: partly staggered, full staggered, so how the authors did it? The authors add a 2D discretization plot (Fig. 2) of a 3D wave equation. This is simply not acceptable because it says nothing about what the authors did. Pleas replace the 2D figure by the proper 3D staggered discretization (see Fig. 1).

  With the FD3S code the authors will see how the staggered grid is done in 3D, they can see how to implement future viscoelasticity and MPI parallel distribution with their own code (in case they want to further develop their C++ code). The authors can do a benchmark of their own code using this one. The simply need to install the code, run an example and make a conversion between Cartesian and Spherical coordinates. It is the authors decision however.

- The authors can also write Prof. Moczo (or his group) nuquake webpage and ask for help for the benchmark of the code.

3.2 The simulations and graphical representations

I have to say here that is very clear the nice effort done in trying to explain everything. It is a nice work. However, please understand that one cannot believe any result if the code has not been properly benchmarked. But I do acknowledge the effort done by the authors. Next are my suggestions.

First of all the authors need to clarify the number of points per wavelength that they are using (and why this selection) and the Courant number. Next, the authors present several plots that are nice but not very informative, for example, Figs. 3 to 5 are OK but they are qualitative plots, they serve only as a qualitative analysis of a certain time step during the simulations. So, I do not really know how much effort should be put on a certain snapshot and in this kind of analyses.

Next, for the time series comparisons, those plots are very blurry. This is just a technical detail yes, but it is important. Plots need to be clear in all possible ways. Numbers should be readable, the plot should not be blurry and they have to be scientifically informative.

The data comparison is a very ambitious Section. The authors have a code that it has not properly been benchmarked, does not include attenuation, does not include the Earth curvature, the earth model included is 1D layered and they want to compare to data. Ok that is ambitious. It can be done but if the authors properly discuss the limitations of the physics of the comparison, which they clearly try to do. That is very nice but the results are shown in not the most informative way.

Every observational seismologist would like to see the three components separated and rotated to Transverse, Radial and Vertical directions and not XYZ. This is crucial, because in the Transverse component one should not see a P wave for example. In the Radial and Vertical component one should see the interaction between P and S waves and it is only then that one can really talk about the physics of rotations and compare to observations. Otherwise, those separated XYZ plots are very little informative and basically all theoretical work is not properly used.

I will stop here. I think it is more than enough corrections.

4 Summary

I do believe that the authors have made a significant amount of work but the paper is not yet ready for publication. The reason is because the papers is just too ambitious. Basically the authors are writing a 3D code from scratch, doing a theoretical analysis and a data comparison. This are three papers:

1. The publication of the code properly benchmarked and explained and with a manual (made publicly available).

2. The publication of the theoretical analysis: the authors can use the code for doing synthetic calculations in 1D models and analyzing results, then changing to a 3D model and analyzing results and then using hypothetical 3D models to see the effects on the rotations and analyzing results.

3. The publication of the data and simulation comparisons: Now the authors will have an idea of the results to expect from the second paper, then they can go directly to talk about the physics of the observations and how the non-linearities are properly taken into account or not.

On the contrary, the authors put these three papers into a single paper making a ridiculously amount of work for the author (or person) who is in charge of the project and for the reviewers. Of course, basics things like testing of the code, availability, benchmark and all details previously mentioned are not going to be covered because it is simply too much work for a single paper. In addition, for the first two papers one has to think as a theoretician but for the last paper as an observational scientist. Those are two completely different ways of using our brains and it is an additional challenge.

In any case, I can continue writing but I will stop here. The authors can do what they consider best and more appropriate for them. I simply hope these lines help the authors to improve and make easier their (current and future) work.

A reviewer