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Interactive comment on "A new open-source visco-elastic Earth deformation module implemented in Elmer (v8.4)" *by* Thomas Zwinger et al.

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This paper presents a new module implemented in Elmer, termed Elmer/Earth, that allows users to compute the solid Earth's deformational response to the applied surface loads. Given the observation of rapid response of solid Earth to ongoing ice mass loss and its possible stabilizing feedback to ice sheet dynamics (e.g., Barletta et al., 2018, doi: 10.1126/science.aao1447), Elmer/Earth is a welcome addition to Elmer particularly in light of Elmer/Ice (Gagliardini et al., 2013, doi: 10.5194/gmd-6-1299-2013) that can simulate evolving ice load subject to atmospheric and oceanic forcings.

For the reasons that follow, however, I am not so sure about the utility of this new module to fulfill the purpose of improving our understanding of ice-sheet/solid-Earth interaction. Change in ice mass directly loads the underlying solid Earth, and hence, induces its deformation. Ice mass change also modulates the ocean mass, satisfying mass conservation in the Earth System. The change in ocean load contributes to the solid Earth deformation. Ignoring ocean load may underestimate the magnitude of modeled displacement field by about 10%, at least around the ice-bedrock-ocean interfaces. Elmer/Earth clearly lacks the ability to capture mass conserving ocean load induced by ice mass change, limiting its utility for the rigorous analysis of ice-sheet/solid-Earth interaction. Furthermore, both the ice and ocean mass change deform the geoid field, which further amplifies the strength of stabilizing feedbacks of the solid Earth to marine ice sheet dynamics. This element is also overlooked in the current version of Elmer/Earth. At a minimum the authors should acknowledge this limitation, with reference to recent works on the topic of ice-sheet/solid-Earth/sea-level interaction (e.g., Adhikari et al., 2020, doi: 10.5194/tc-2020-23). Elmer/Earth perhaps is more suitable for predicting local- or regional-scale hydrology (including ice) induced displacement fields.

I find that the lateral boundary conditions imposed in Elmer/Earth may be problematic for its application to continental-scale ice sheet. They have simply considered a "large enough" horizontal extent of the domain and set displacement vector to zero at the lateral boundaries. For Antarctic Ice Sheet, for example, one may require horizontal extent of the domain to be on the order of tens of thousands of kilometers. In such situations, the effects of Earth's sphericity are not certainly negligible unlike in the test case considered in the paper (line 135). Either a justification about this inconsistency or an acknowledgement of this limitation is required.

Providing a bit more elaborative description of Theory (Section 2) would be useful, especially for those who are not familiar with Wu (2004, doi: 10.1111/j.1365-246X.2004.02338.x). Section 2 of the Wu paper is very informative, and all I see in this paper is a list of equations (with minimal explanation) that are deduced from the

Wu paper for the case of incompressible viscoelastic Earth that lacks self-gravitation and sphericity. Also, missing in this section is the (mathematical) description of boundary conditions.

A few suggestions on the usage of terminologies: Visco-elastic => viscoelastic; Earth => solid Earth; Finite Element => finite element. So, the title would be "…viscoelastic solid Earth module…"

There is something about the word "flat-earth" that does not look right to me (especially in the era of social media). I would consider avoiding it.

Line 25: Is this timescale required for a "complete" relaxation of mantle? Or e-folding relaxation?

Around line 30: Evolving bedrock also modulates the gravitational driving stress of the ice sheet and hence its dynamics (see Figure 6 of Adhikari et al., 2014, doi: 10.5194/se-5-569-2014)

Line 33: Provide references, e.g. Schoof (2007, doi: 10.1029/2006JF000664).

Line 41: Best performing => in terms of computational ability? Or, its ability to match, say, bedrock GPS data?

Line 41: widely used => Not sure about this(!). Provide references at least. Again, for the reason I noted in the beginning of this review, even if this method is "widely" used it certainty is not the most accurate one.

Line 53: suggested rewording: "…a full Stokes ice sheet model capable of yielding high wave number loads that is essential to model high-res solid Earth rebound"

Line 56: high gradients => of what?

Line 65: Cauchy stress => Cauchy stress tensor

Line 66 (and elsewhere): deformation vector => displacement vector

Line 69: Suggested rewording: "…motion that conserves linear momentum for a non-gravitating… layered material…"

Line 74: Why do you need to introduce d_z? Simply write "…vector product e_z . d"

Line 79: Given the limited description of Theory (as noted above), not sure I follow what you exactly mean by "avoids singularity… as Poisson ratio approaches $\frac{1}{2}$". Either refer this statement to some equation or delete it altogether.

Line 84: Why 10 layers? Be generic.

Line 121: The model is fixed in all directions => What do you mean by fixed? Dirichlet conditions with zero displacement? Again, you need to talk about boundary conditions in Theory section.

Line 135: "sea-level equation" => Need to provide a qualitative description of what it means if it is relevant at all (else simply delete the sentence). Not all readers would get what it means.

Line 137/138: Looks like it is 40,000 km (see Figure 1); and that would be 800 times larger? Again, Is the sphericity effect negligible for such a large spatial scale?

Line 144: "has a resolution equivalent" => "has a spectral resolution equivalent"

Figures 1/2: Combine these as Figure 1a and 1b?

Line 152: zero time => t = 0

New Figure (that corresponds to Figure 4): Would be useful to show a new figure with displacement vs. distance away from the load center for select times (including at t=0 to show the elastic displacement fields).

Around line 175: Acknowledge that ABAQUS uses compressible Earth (see Table 1 caption). Elmer/Earth solves for incompressible Earth.

Section 5.2: I was wondering whether you maintain the same mass for different mesh experiments (by adjusting ice height). Otherwise the discrepancy in solutions may be (at least partly) due to the fact that you are loading the solid Earth with slightly different loads (i.e., net mass) and not necessarily do to the coarseness or fineness of computational mesh.

Figure 6 caption: Vertical deformation => Vertical displacement

Lines 241: For the reasons noted early on, I am afraid that the utility of Elmer/Earth to accurately capture solid Earth's feedback to ice sheet dynamics (within Elmer/Ice) is limited. At least, it should be acknowledged. I would highlight the utility of Elmer/Earth for general (regional/local) loading studies (hydrology, ice load, atmosphere loads, etc).

Last paragraph: I am not sure whether this should be part of the conclusion. It may be sufficient to say that Elmer/Earth performs well in parallel computation.

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