Response to reviewer 2 (Surendra Adhikari)

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.

We thank for the generally positive assessment of the reviewer and are grateful for the detailed work invested to improve the manuscript. Please, find our response inline to the suggestions:

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 modelled 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.

That is a correct statement. We have clarified that we recommend the model's application only in a regional context by adding the following sentence in the conclusions:

> *Elmer/Earth for the time being is a so-called flat-earth model (Wu, 2004). In its current state it ignores sphericity and self-gravitational effects as well as neglects to account for the deformation induced by redistribution of ocean water masses. Consequently, future applications of this particular model version should be confined to regional studies of ice-sheets or highly localized loads, such as glaciers and ice-caps.*

For the suggested reference (Adhikari et al., 2020), we would like to point out that this is a paper that currently is in review and was not available for citation at the time this manuscript was submitted.

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 testcase considered in the paper (line 135). Either a justification about this inconsistency or an acknowledgement of this limitation is required.

We will mention the neglected sphericity and its limitations in the text presented in the conclusions (see above) and also drop a note on this limitation in the new text describing the implementation of boundary conditions (see further below and response to comment on line 137/138).

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 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.

We already described the complete model as derived in Section 3 of Wu (2004). In order to make it easier for the reader to evaluate which simplification have been taken place, we will add the following paragraph to the existing presentation of (currently) equation (4) in Section 2:

> *The linearized elastic equation of motion for earth deformation (Wu, 2004) is given by*
>
> $$\nabla \cdot \boldsymbol{\tau} - \nabla(\rho_0 \boldsymbol{g}_0 \cdot \mathbf{d}) - \rho_1 \boldsymbol{g}_0 - \rho_0 \nabla \phi_1 = \mathbf{0} \quad (4)$$
>
> *Where ρ_0 and $\boldsymbol{g}_0$ are hydrostatic background density and gravity, respectively, and ρ_1 is the perturbed density. The direction of $\boldsymbol{g}_0$ is in negative radial direction. According to Wu (2004, section 3), a flat-earth model is derived from (4) by assuming incompressibility and ignoring self-gravitational effects (i.e., redistribution of mass), making the third and fourth term vanish. Further, sphericity is ignored, leading to changes aligned with the unit vector of a Cartesian system in vertical direction, $\boldsymbol{e}_z$. This leads to the equation of motion for a non-self-gravitating flat-earth model with layer-wise constant material. It reduces to a balance between the divergence of the stress (first term) and a restoring force due to the advection of pre-stress of the material (Wu, 2004)*
>
> $$\nabla \cdot \boldsymbol{\tau} - \rho g \, \nabla(\boldsymbol{e}_z \cdot \boldsymbol{d}) = \mathbf{0} \qquad (5)$$
>
> *Here, $\rho = \rho_0$ and $g = |\boldsymbol{g}_0|$ is the magnitude of the local acceleration by gravity. …*

For the boundary condition, we will add the following sentence directly after (currently) equation (9) in Section 2.1:

> *The system is completed by boundary conditions that are either provided by a value for any component of the stress-vector, $\mathbf{t} = \boldsymbol{\tau} \cdot \boldsymbol{n}$, in the second integral (Neumann condition) of equation (10 , former 9) or by imposing a value for any component of the deformation vector, $\mathbf{d}$ (Dirichlet condition).*

And we explicitly state the boundary conditions for the benchmark in Section 3:

> *At the free surface we apply the load of the disc for the first 100 years into the simulation. Thereafter, the natural boundary condition, namely a vanishing stress vector, applies to the whole surface. At all other boundaries we impose a zero-deformation condition, i.e., $\mathbf{d} = \mathbf{0}$.*

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..."

We will change accordingly

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.

We are well aware of the distorted connotation of the term in some communities with a – let us say - weird alternative approach to Earth science. Yet, the term has a scientific significance as it is used in the main reference (Wu, 2004) and it would be difficult for the reader to make the connection therein. We have retained the standard use in model studies of 'flat earth'. We are sure that the readers of this journal will apply the correct interpretation of this word.

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

This is referring to a typical "Maxwell" relaxation time, rather than a complete relaxation. It is intended to be merely an indication of the timescales one could expect to observe viscoelastic relaxation over due to changes in ice loading with the point that some regions experience a very quick response due to low viscosity underlying mantle. We do not feel it is necessary to include this in the text as it may be confusing for some readers. We have however quantified the more rapid timescale as follows:

> *…although typically thought to occur over several thousands of years (Whitehouse, 2018, and references therein), recent studies have shown some regions undergoing much more rapid (decadal) rebound in response to present-day changes (Nield et al., 2014; Barletta et al., 2018)….*

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)

We include this reference

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

We will include this citation on line 33.

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

Both, but mainly in terms of numerical performance. The shortcomings on physics are mentioned in the sentence to follow. We will add:

> *Of these, Le Meur and Huybrechts (1996) found the best performing is the "ELRA" model (elastic lithosphere with relaxing asthenosphere) which is widely used in ice-sheet modelling, mainly due to its simplicity and fast computations.*

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.

It is in our view an up until recently widely used method. We will add another reference (Greve, 2001) to the already existing references (Le Meur and Huybrechts, 1996; Rutt et al., 2009). In no way do we claim that its popularity compensates for its shortcomings, which we believe are sufficiently mentioned in the sentence that follows: *However, Bueler et al. (2007) found significant differences …*

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"

We reword accordingly

Line 56: high gradients => of what?

We reformulate:

> *Finite Elements have the advantage that they in general can use unstructured meshes in order to provide the needed resolution in regions where either physics or geometry demand it while keeping the model size limited.*

Line 65: Cauchy stress => Cauchy stress tensor

We will change the text according to this suggestion

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

We will change the text according to this suggestion

Line 69: Suggested rewording: "...motion that conserves linear momentum for a non-gravitating...layered material..."
As shown above, we completely dropped the brackets.

Line 74: Why do you need to introduce d_z? Simply write "...vector product e_z . d"
We dropped this sentence.

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". Either refer this statement to some equation or delete it altogether
We reformulate to:

> *… avoids the singularity of the first Lamé parameter* $\lambda = E\eta/((1+\eta)(1-2\eta))$ *in the limit of the Poisson ratio approaching* $\eta \rightarrow ½$ *(e.g., Greve and Blatter, 2009; p 41).*

Line 84: Why 10 layers? Be generic.
Added:

> *… of several layers due to the resolution of changing material parameters.*

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.
We reformulate:

> *…and has zero deformation imposed on its lateral boundaries.*

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
We delete this sentence.

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?
No, 2×10^6 m make 2,000 km in each direction, which gives 4,000 km and not 40,000 km in total span. Following the request of reviewer 1, we will increase the resolution of the annotation in Fig. 1 in order to make it better readable.
The flat-earth approach, with the necessary large lateral extents, has been shown to be accurate when computing deformation for ice loads as large as the Laurentide ice sheet (Wu and Johnston, 1998). Nevertheless, we are computing a benchmark here and – as should be implicitly clear by load's small size of only 50 km in radius - in no way claim that this resembles a continental ice-sheet. Placing the boundaries so far out simply avoids any influence of boundaries on the displacement close to the centre. That motivation is reflected by the already existing line (and reference therein): *This distance is 80 times the diameter of the test load which is more than sufficient to allow mantle deformation below the ice load (Steffen et al., 2006).*

Line 144: "has a resolution equivalent" => "has a spectral resolution equivalent"
We will change the text according to this suggestion

Figures 1/2: Combine these as Figure 1a and 1b?
We will evaluate whether it is possible to combine them and still introduce larger annotation (as requested by reviewer 1).

Line 152: zero time => t = 0
We will change the text according to this suggestion

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).
We will include such a figure.

Around line 175: Acknowledge that ABAQUS uses compressible Earth (see Table 1 caption). Elmer/Earth solves for incompressible Earth.
This was an error in the text. Whilst ABAQUS can implement some aspects of compressibility (e.g. with a Poisson's ration < 0.5) it cannot change the density of elements with time. We use a high Poisson ratio in the benchmarking case to simulate incompressibility. We therefore remove "compressible" from Table 1 caption.

Section 5.2: I was wondering whether you maintain the same mass for different experiments (by adjusting ice height). Otherwise the discrepancy in solutions maybe (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.
We are not completely sure we understand this question. We assume that it is about whether we change the net mass of the ice-disc in order to compensate for a lower resolution in the centre of the domain. We do not think that this is the case, since we impose a load and the mesh resolution (i.e. what is being tested) is inherent in how the disc load is represented in the model.

Figure 6 caption: Vertical deformation => Vertical displacement
We will change the text according to this suggestion

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).
We included a statement on this in the conclusions (see earlier).

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.
As this is a technical paper that also should inform the reader on the applicability – including computational aspects – of the new code, we believe that this paragraph has a place in the conclusions.

New References

R. Greve, Glacial Isostasy: *Models for the Response of the Earth to Varying Ice Loads*, in B. Straughan, R. Greve, H. Ehrentraut, and Y. Wang (edt.), Continuum Mechanics and Applications in Geophysics and the Environment, Springer, Berlin, Germany etc., 393 pp. (2001). ISBN: 3-540-41660-9

R. Greve and H. Blatter, *Dynamics of Ice Sheets and Glaciers*, Springer, Berlin, Germany (2009) DOI 10.1007/978-3-642-03415-2

Wu, P. & Johnston, P., 1998. *Validity of Using Flat-Earth Finite Element Models in the Study of Postglacial Rebound*. in Dynamics of the Ice Age Earth, pp. 191-202, ed. Wu, P. Trans Tech Publications Ltd, Switzerland.

Schoof, C., 2007. *Ice sheet grounding line dynamics: Steady states, stability, and hysteresis*, Journal of Geophysical Research: Earth Surface, 112, F03S28.