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Interactive comment on "An ice sheet model validation framework for the Greenland ice sheet" *by* Stephen F. Price et al.

Anonymous Referee #3

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1 General statement

The manuscript "An ice sheet model validation framework for the Greenland ice sheet" by S. Price and others presents a new framework for comparing ice flow model results with observations of altimetry and gravity acquired since 2003. They apply it to several modeled and prescribed representations of the Greenland ice sheet and show that comparison of results with altimetry observations is similar for all the representations of the Greenland ice sheet, while gravity observations seem to distinguish the more sophisticated representations of the ice sheet evolution.

The paper is clear and well written, the new framework explained in details and the figures usually appropriate. However, there are several major points that are either not

accurate or not supported by the results. My main concern is that the "most sophisticated" dynamic model reproduces the trends in altimetry and gravity for the wrong reasons. Correctly initializing ice flow models remains a challenge today and an active area of research (Seroussi et al., 2011; Aschwanden et al., 2013; 2016) due to the lack of reliable observations and the long response time of ice sheets. In order to bypass this problem, the authors impose an additional steady-state surface mass balance (SMB) on top of the climate derived SMB, therefore forcing the model to remain close to its initial conditions. Without this unphysical forcing, the model would likely diverge quickly from this initial state. Furthermore, the physical model has to be not only forced with an unphysical SMB, but also constrained with imposed velocities over the most dynamic areas, in order to get close enough to the observed changes in gravity. At this point, there is not much physics left in this model.

So if I don't question the development of this framework or the benefits to have a tool that can easily compare observations and model results, I am wondering if any validation of numerical models can be done today given the many improvements still needed by these models to become more accurate. I would therefore recommend to reduce the emphasis of this framework as a validation tool and rewrite the manuscript accordingly.

2 Specific comments

In the abstract (l.6) as well as several other places in the manuscript, the author qualify the static representation and the SMB only applied to this static representation as "non-dynamic models", which is misleading, as these representations do not include any physical model. The text should better distinguish between physical models and other representations.

p.1 l.17: "simulations of varying complexity": there is no varying complexity found in the models presented here (same stress balance approximation, ...). What varies between these simulations is the degree of "forcing" of the models, the velocity being applied as a Dirichlet forcing for one of the simulations.

p.2 l.20-21: The "most sophisticated" models are actually forced by imposing the velocity to be equal to observations at the border of the domain. Showing that such a simulation exhibits mass trends similar to gravity observations does not prove any predictive capability of the model. First, the velocity is not produced by the model but forced in all the dynamic areas. Furthermore, the flux correction applied to the physical models introduces a large mass change that prevents to compare simulations to observations. What this result mainly shows is that observations are rather consistent and that a large part of the mass change signal can be explained by changes in SMB and acceleration.

p.2 l.23: What about velocity changes? I would imagine that comparing the dynamic signal would be an important part of such a tool, as the main objective of an ice sheet model is to reproduce the dynamic signal, not the SMB.

p.2 l.33-37: I don't agree with this statement. The main problem is that ice flow models are still unable to reproduce the observed changes, and that improving their initial conditions is a very active area of research. Models therefore have to either run spin-up or apply flux correction (similar to what is done in this manuscript), to get closer to observations. Accurately comparing model results and observations is therefore beyond the scope of most studies since lots of models cannot even capture the correct trend (Bindschadler et al., 2012).

p.7 l.208: The authors explain how they apply an SMB correction to get closer to a steady-state equilibrium of the ice sheet. It is not clear what this correction represents, how large it is, and how it evolves with time as the model evolves from its steady-state. Also, how can the dynamics of the model be compared to observations that show clear evolution trends, if the model is artificially forced to be close to a steady-state?

p.7 l.220-225: This part is not very clear. Also it does not seem very natural to force the velocity. If the velocity at the flux gates is prescribed there is not much freedom anymore in the model. It would make more sense in my opinion to force the ice front position for example.

p.8 l.249: What is the treatment of the calving front? What is the calving law used and how does the ice front evolve in the code? Why is the ice front allowed to retreat but not advance?

p.13 l.431-433: This sentence is not clear as it seems that all models start from the same thickness and same datasets in general. As the flux correction is applied to ensure that the model remains close to a steady-state, it is not surprising that the dynamic model and persistent representation remain close to each other.

p.14 l.465: The physical ice sheet models have not yet reach a state where they can be reliable and compare with observations and capture the dynamic processes at play. Some important physical processes could even be missing. So it might be unfortunately a little premature to pretend to compare observations with models, even if this remains a long-term objective. For these reasons, the distinction of more accurate models with this tool seems difficult to achieve, and results presented here are largely influenced by the flux correction applied in the physical models.

p.14 l.466: "dynamic models that account for known changes in ice dynamics": this statement is a little biased in my opinion, as the model is forced with observed velocities to reproduce the dynamic signal, while the physics alone should be able to reproduce this effect.

p.14 l.501: As mentioned above, it would be more natural to constrain the evolution of ice front position with observations instead of constraining the velocity. Ice front retreat triggers acceleration and would have a similar effect but would include the physical processes involved in this process.

p.15 l.518: It seems difficult to asses if sophisticated physical models perform better as their evolution is largely influenced by the flux correction. When such a correction is not applied, many models are not even capable of reproducing a mass loss for the Greenland ice sheet (Bindschadler et al., 2013).

3 Technical comments

Model names "SMB-only" and "RACMO-SMB-only" are rather confusing, this should be clarified.

p.5 l.138: What is C_20?

p.7 l.213: How large is FC?

p.8 l.249: How is the ice margin retreating in CISM? This is not something commonly used and should be detailed. What is the criterion for calving? for moving the ice front?

p.9 l.281: Remove one "thickness"

p.10 l.298: How are the values averaged? What happens when cells are split between several GRACE-like grids?

p.11 l.355: add parenthesis after Figure 11

p.13 l.403-404: Rephrase

p.14 l.440-441: No clear: why do they only partly account for surface elevation changes? The RACMO data should include that. What is missing?

p.15 l.495: Missing parenthesis after (Joughin et al., 2004)

p.16 l.334: "in prep." → the paper appeared in GMDD

p.16 l.541: What about velocity observations? That would be a very valuable metric for

dynamic changes.

p.18: It would be easier to have references listed alphabetically.

p.26 Fig.7: Rescale the yaxis

p.29 Fig.12: It might be easier to add letters on the subplots (instead of e.g. lower-right)

p.14 l.455: that they see HALF of the GRACE signal

4 References

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