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Final author response for: *Data assimilation and prognostic whole ice-sheet modelling with the variationally derived, higher-order, open source, and fully parallel ice sheet model VarGlaS*

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1 Introduction

We thank the editor and both anonymous referees for their careful and insightful reviews. All of the suggestions and comments have helped to make this manuscript much better and more complete. In the following section, we resond to both the general and specific comments made in each review.

2 Response to reviewer 1.

2.1 General comments

Reviewer 1 suggested that VarGlaS' reliance on the FEniCS project be highlighted earlier in the paper, and that a more in depth treatment of the advantages of automatic differentation be incorporated. We agree that our reliance on the tools particular to FEniCS should be highlighted more effectively. To this end, we have incorporated language that highlights when we are depending on FEniCS-specific captabilities, most notably automatic differentiation, into both the abstract and several sections of the introduction.

2.2 Specific points

- P1030,l11: *"predicts an overall mass evolution ... that matches well with observational data...". Add that it still requires a relaxation period of 100 years.*: We have included a statement in the abstract that mentions the requisite 100 year relaxation period.

- P1031,l5: *among the other references of higher order ice flow model add a reference to Gillet-Chaulet et al., Greenland ice sheet contribution to sea-level rise from a new-generation ice-sheet model, TC, 2012*. Added a reference to Gillet-Chaulet et al. (2012), which seems to be the definitive reference for the inner workings of Elmer/Ice. We had intended to cite Elmer via Seddik (2012) and recognize the importance and capabilities of Elmer/Ice.

- P1031,l23-29: *Automatic code generation and FEniCS should be introduced before to better explain why "the procedure of generating the code ... is as simple as making a change to the variational principle"*. Please see the response to this point in our response to general comments above.

- P1032, l15: *ref to "Greve and Hutter (1995)" should read "(Greve and Hutter, 1995)"*. We fixed this typographical error.

– P1032,l18: *idem for ref to "Rutt et al. (2009)"*. Also fixed this typographical error.

– P1035,Eq. 1: *Is it really P the lagrange multiplier that is used to impose the non-penetration condition (last term of eq. 1, $Pu \cdot n$)?*. Impenetrability is indeed enforced using the same Lagrange multiplier as Incompressibility. In fact, impenetrability can be viewed as the natural boundary condition for incompressibility:

$$\nabla \cdot \mathbf{u} = 0. \tag{1}$$

Imagine enforcing the above with a Lagrange multiplier in a variational principle:

$$\int_{\Omega} P \nabla \cdot \mathbf{u} \mathrm{d}\Omega. \tag{2}$$

Integration by parts yields

$$-\int_{\Omega} \nabla P \cdot \mathbf{u} \mathrm{d}\Omega + \int_{\Gamma} P \mathbf{u} \cdot \mathbf{n} \mathrm{d}\Gamma \tag{3}$$

Assuming a non-zero pressure at the basal boundary, Impenetrability states that $\mathbf{u} \cdot \mathbf{n} = 0$ at the bed. Therefore:

$$-\int_{\Omega} \nabla P \cdot \mathbf{u} \mathrm{d}\Omega + \int_{\Gamma \backslash \Gamma_B} P \mathbf{u} \cdot \mathbf{n} \mathrm{d}\Gamma = -\int_{\Omega} \nabla P \cdot \mathbf{u} \mathrm{d}\Omega + \int_{\Gamma} P \mathbf{u} \cdot \mathbf{n} \mathrm{d}\Gamma - \int_{\Gamma_B} P \mathbf{u} \cdot \mathbf{n} \mathrm{d}\Gamma. \tag{4}$$

Integrating by parts in reverse yields the form we used in Eq. 1:

$$\int_{\Omega} P \cdot \mathbf{u} \mathrm{d}\Omega - \int_{\Gamma_B} P \mathbf{u} \cdot \mathbf{n} \mathrm{d}\Gamma. \tag{5}$$

Thus impenetrability is naturally incorporated using the pressure variable, so long as basal pressure is non-zero.

- P1035,Eq. 1: *Explain the motivation, or provide a ref. to scale the basal friction with* h^r. We have added a justification for the scaling of the sliding law by h^r. As seen in Larour et al. (2012), model inversions produce basal traction fields that covary with the thickness field such that any details about spatial distribution of traction are masked. We prefer to interpret the basal traction parameter as relating the ratio of driving and normal stresses, or:

$$\tau_b = \tau_n \beta^2 u \tag{6}$$

Since both basal shear stress and driving stress depend on thickness, this form eliminates the dependence of the traction parameter on thickness. Assuming hydrostatic pressure, $\tau_n = P \approx \rho g h$, so

$$\tau_b = \rho g h \beta^2 u \tag{7}$$

We have found that folding ρg into β^2 yields basal traction parameters of $\mathcal{O}(1)$, so we do that, and are left with

$$\tau_b = h \beta^2 u \tag{8}$$

We incorporate the exponent r in order to easily switch between the unscaled and scaled version of the sliding law; it is clear that for $r = 0$, we get the original unscaled version. In practice we use $r \in \{0,1\}$.

- P1035,Eq.1: *At the marine termini, a Neumann boundary condition should be imposed (sea water pressure below sea level). Is it included in the Greenland application? How does it appear in the variational principle? (this could also affect the discussion on the future implementation of grounding line migration in VarGlas).* Upon the initial writing of this paper, the model domain ended at the grounding line. We have now updated our code such that, for the Stokes' model, the suggested (and appropriate) seawater pressure boundary condition is used. This also necessitates an update to the treatment of free surfaces. The grounding line is still held fixed until we can determine an efficient and effective algorithm for grounding line migration. Note that due to assumptions of the Blatter-Pattyn approximation, a coupled sheet-shelf treatment isn't really possible (e.g.

Dukowicz et al. (2010)). As such, for the BP, we still make the same assumption of wholly grounded ice.

- P1039,l20: *It would be more correct to say that the whole ice sheet is treated as grounded with a non penetration condition and a sliding law, so no shelf and no grounding line. This is more strict that a fixed grounding line (where proper treatment of the shelf could exist)*. For the Stokes' model, this has been updated, such that the existing text is true with minor revisions. We've provided additional explanation about the treatment that occurs in the first order model, as mentioned in the above comment.

- P1041, section 2.2.2 Mesh Refinement: *It could be explicitly stated that the method allows only refinement and no coarsening, this is why the initial Green- land mesh is made coarse in the interior*. We have added to this section an explanation that our algorithm only refines a mesh, and does not have the capability to coarsen it.

- P1041,l21: *"the classic anisotropic error metric", please provide a ref.* Added a reference to Habashi et al. (2000), from which the metric is taken.

- P1042,l12: *"using Gauss-Seidl iterations", please explain and/or provide a ref.* Idem above.

- P1047,Eq.28: *please provide refs. for this shock-capturing artificial viscosity. Is Dshock(Eq. 28) added to Eq. 13? in the whole domain or just at the boundaries?* Added a reference to Donea and Huerta (2003) for nonlinear shock capturing artificial diffusion. This is applied over the whole domain, but due to the non-linearity of the term, it is only non-negligible in the vicinity of sharp solution gradients.

- P1049,l13-21: *inverse methods test with ISMIP-HOM C; is it done with the first order approximation or Stokes?* The inversion is done for both, and at all length scales. The plot shown was for BP at L=80km. We've reduced the length scale of the inversion to L=10km to stress the system more. We've included a more precise statement of which problem is being solved.

– P1051,l15: *"with gradients between the two reduced by systematically exploring" please explain with more details*. We used a steepest descent algorithm to minimize the misfit between the edges of the InSAR velocities and the corresponding balance velocities by varying the surface mass balance subject to the constraint that it remain within its reported pointwise error bounds. Added this description to the text.

– P1052,l10: *see previous comment on the meaning of h^r in Eq. (1). H is "h" in eq.1*. It should be h. This was a notational error and has been corrected. Included a motivation for the rescaling earlier in the text (which is somewhat repeated here).

– P1052,l20: *which cost function is used (19) or (20)? please justify. Could you give a mean rms erro in m/a?* Logarithmic for Greenland, because the velocity varies over several orders of magnitude. Included justification in the text, and also added a similar statement to the ISMIP-HOM C section, where a linear functional was used. Included the RMS error for both experiments.

– P1052, Section: Data Assimilation, 1: *how the weighting parameter α in Eq. 22 has been chosen?* The selection of the regularization parameter α is motivativated by the results of Balise and Raymond (1985), which indicate that below the one ice thickness length scale, variations in basal traction do not propogate to the surface. As such, we select $\alpha = h^2$, which corresponds to applying smoothing with a Gaussian kernel with standard deviation h to the basal traction field. We have added the above justification to the text.

– P1052, Section: Data Assimilation, 2: *"The velocity field matches the observed closely". Please show the observed velocity in the Figure (or show the same area in Fig. 1)*. We now report RMS for velocity mismatch, as well as showing a side by side comparison of the measured and modelled velocity fields.

– P1053, l7: *Replace Fig.12 by Fig. 11*. LATEXnumbering error, corrected.

– P1053, l10: *"An ice sheet model should have relaxed at least to this level..." Please say how long is this in your application*. Relaxation to a $\partial_t S$ level for VarGlaS takes around

100 years. We draw this conclusion from Fig. 11, and have added a reference in the text making this explicit.

– P1053, l16: *Add ref to Fig. 12*. Added a reference to Fig. 12.

– P1053: *After the initial mass increase is seems from fig 11 that the ice sheet is increasingly loosing mass; Looking at Fig 12, after 500 years the margins in the south and the west coast are considerably thicker with higher velocities. It seems that the increase in mass loss is due to the increase of the ice flux leaving the domain through the edges (it would be interesting to compute this flux). So that if the total mass loss agrees with the observations it is at the expense of a divergence of the ice thickness and velocities from the observations. This could be looked at more precisely and stated in the abstract and conclusion*. This a particularly valid concern, and it seems to be an issue in other comparable ice sheet models, such as Elmer/Ice and ISSM, as well. What we see is either an overall slight mass loss (VarGlaS), or a slight mass increase (Elmer, ISSM), but a qualitative pattern of mass distribution that disagrees with current data products that suggest a thickening interior and thinning margins (e.g GRACE). There are multiple possibilities for why the ice sheet model would produce such a configuration; imprecise data, an inappropriate treatment of margins, and a lack of appropriate temperature history, are a few examples. Work is ongoing to determine a more precise explanation. In any case, we added a discussion of this qualitative pattern, and placed it in context with the results of similar ice sheet models and experiments.

– P1054, l2: *"In better agreement with a pseudo-analytical ..." this is not shown in the paper*. This conclusion was based off of a visual comparison between our results and the figures in the original publication. We were not able to find the underlying data or compute this analytical solution in such a way that was suitable for inclusion in the ISMIP-HOM F results figure, so we have removed the statement about it. After more careful consideration, we are not certain that the asymptotic result from Gudmondsson is 'better' than an ice sheet model.

– P1055, l26: *"imposing a known-thickness boundary is the thickness at the boundaries really imposed? The calving rate balance the ice flux leaving the domain but is not necessary constant if the velocity is not constant.* Thickness on the boundary is imposed as a Dirichlet condition in order to maintain fixed margins. No assumption is made about the flux across the boundary, and velocity is free to change to whatever the upstream mass and momentum balance mandates it should be. The size of the gate through which this mass leaves the ice is held constant though.

3 Response to reviewer 2.

3.1 General comments.

Reviewer 2 calls into question a few aspects of the model that, in the reviewer's view, prevent it from being considered 'next-generation.' In our view, a next generation ISM takes advantage of recent increases in computing power and technology to apply a more detailed set of governing equations to a more highly resolved model domain. Rather than circumventing computational complexity of a full 3D treatment by using the SIA or SSA, which was the standard way of doing things in the ice sheet modelling community for many years, a next generation ISM applies parallelism and new numerical techniques in order to make the problem computationally tractable. The fact that this paradigm shift has occurred over a rather localized and recent time scale suggests a natural partition in terms; hence, models that demonstrate a decided shift towards embracing computational complexity and choosing to place a premium on applying these techniques at a continental scale are deemed next-generation.

This does not necessarily imply that unique suitability (or suitability at all) to addressing the specific phenomenon of mass loss over the ice sheets due to a change in grounding line dynamics or any other phenomenon. We are aware of none that make that specific claim, and for good reason: the theory of marine margins is incomplete, the numerics difficult, and the means to verify solutions is not forthcoming. Rather than make this claim ourselves, we attempted to present a new numerical platform that can be used as a starting point for the numerical investigation of glaciological processes. The structure of the paper is subservient to this goal; we show that VarGlaS simulates model physics in accordance with benchmarks, and has the necessary features for transient continental scale modelling. Further scientific applications are in progress, but these require that the model and its performance on relevant benchmarks be published first.

It was of great importance to us to make the methods that we used as transparent and reproducible as possible, with a particular emphasis on the new and unique techniques that we employed, in hopes that other investigators might be able to use some of these techniques in applications to their own processes of interest. This is why we did not devote more time to performing the entire suite of SeaRISE experiments, and why this paper is not directly comparible to Gillet-Chaulet et al. (2012). In that work, much of the mathematical material treated is familiar to most mathematical glaciologists. Elmer/Ice has been available for some time now, and the numerical implementation of these techniques within Elmer/Ice has been well documented. As such, this paper is a results-oriented publication, as opposed to a methods paper. VarGlaS has no preceding publications and the mechanical formulations are somewhat more exotic. This paper aims to be the definitive publication for VarGlaS, and all subsequent results-oriented papers will reference it. This seems to be a fairly standard practice in the glaciological literature. Rather than relegate our methods to an appendix, this work is intended to be the appendix for further work, such that a lengthy derivation of model physics in an otherwise concise piece is not necessary.

Semantics aside, we are in the process of addressing the moving boundary issue, but have not made sufficient progress to include any results in this paper. As to the issue of a non-linear sliding law, we have incorporated a power-law type dissipation functional of the following form into the model and manuscript:

$$\mathcal{F} = \frac{h^r}{m+1}\beta^2 \mathbf{u}\cdot\mathbf{u}^{\frac{m+1}{2}} \tag{9}$$

This functional, when varied, yields the sliding law

$$\tau_b = \beta^2 h^r (\mathbf{u}\cdot\mathbf{u})^{\frac{m-1}{2}}\mathbf{u} \tag{10}$$

which is similar to the one seen in Schoof (2007), for example. Setting $m=1$ recovers the linear sliding law. Since all of the benchmarks use a linear sliding law, and the procedure of inversion for basal sliding allows an acceptable reproduction of InSAR data without inclusion of non-linear effects, we retain the assumption that the sliding law is linear for the entirety of the paper, recognizing that for future simulations, it will be interesting to allow this to change. Note that changing the sliding law was very easy; we had only to change the definition of the friction functional in the code. We hope that this illustrates VarGlaS' extensibility.

We agree that Geoscientific Model Development (GMD) would be an excellent journal for this manuscript. However, The Cryosphere (TC) has been more commonly used for the publication of glaciological models, and we submitted to it to TC due to this precedent.

We agree with the reviewer that adding information about computating times is important in determining the efficiency of the model. We have thus added a table with computing times for both FO and Stokes' for all of the ISMIP-HOM experiments, FO times for the transient EISMINT runs, as well as the FO times for data assimilation and transient runs for Greenland.

Our choice to perform only ISMIP-HOM experiments A, C, and F was motivated by brevity, and we chose only the experiments that made use of the model's 3D capabilities (since B and D are primarily intended for flowlines). Since there appears to be interest in the model results, we have now performed experiments B and D, and added the associated results to the manuscript. VarGlaS does indeed show the inversion of the velocity profile relative to the first order approximation for experiment B at the $L=5km$ length scale. We agree with the reviewer's assessment that the discussion on the grid dependence of higher order models while performing the EISMINT-II experiments should include a reference to Saito et al. (2006), which we have included, along with a brief discussion of that work's implications to this one. Overall, this paper is not about thermo-viscous instability, and the spontaneous generation of ice streams. Such a topic could be (and has been) the subject of entire manuscripts, and rather than include a treatment that is both too long for the given context, and too short to say anything meaningful, we omit a more in-depth comparison to both Saito et al. (2006) and Hindmarsh (2004). We would very much like to pursue the discussion of thermoviscous instability as it pertains to unstructured grids in a future paper based on this one.

We have added a symbol table to the manuscript.

3.2 Specific Comments

- P1030, L13: *The prediction of the mass evolution of Greenland cannot be supported by observational data (which is lacking for the future). Should be rephrased. The present-day state or evolution over the last decades can be supported by observations*. Changed the assertion to reference present day estimates of mass loss.

- P1030, L21: *define shallow-ice approximation. What are the major characteristics of this approximation and its validity?* Added a reference to Hutter (1983) and Schäfer et al. (2008). The first is one of the original publications for the SIA, and the second gives a good treatment of its applicability. A more in-depth treatment of the SIA is not included, since we do not use it in the model, and presume that any reader of this manuscript likely has some familiarity with it.

- P1031 L17: *change ; into ,*. Correct in our original LaTeXfile. We will make certain that the final typesetting makes this correction.

- P1032 L15: *reference between brackets*. Corrected.

- P1032 L18: *idem*. Corrected.

- P1034: *Rephrase some of the things our model does well. Mention advantages and/or breakthroughs*. Rephrased the description of the discussion section to be more specific that we use this space to mention the way in which combinations of model advances produce a better ice sheet model, as well as to discuss model limitations.

- P1034, L13: *VarGlaS solves for the ...*. Changed 'can solve' to 'solves'.

- P1034, L13: *does it solve for temperature or enthalpy? I thought the latter and that tempera-ture was derived from the former*. It solves for enthalpy, and derives temperature. This has been changed in the text.

- P1037, L8: ... *yields significant* .... Changed wording in accordance with reviewer's suggestion.
- P1038, L9: *'or some constant much less than' could be better written as* $\nu \ll \frac{k}{C_p}$. Changed to $\nu \ll \frac{k}{C_p}$.
- P1039, L5: *(Which can be negative to account for basal accretion)*. Changed to (which can be negative to account for basal freeze-on).
- P1040, L4: *Remove the first sentence. Start with the second and rephrase by 'In the following sections, we discuss how the continuity equations are ...'*. Changed wording in accordance with reviewer's suggestion.
- P1041: *Remove each time '*$1\times$*' before the exponent.* 10^{-6} *is sufficient. See elsewhere throughout the manuscript*. Removed leading ones from exponential notation throughout the manuscript.
- P1041, L9: *is the relaxation parameter = 1 the same as Picard iteration? Are lower values similar to under-relaxation? It should be defined, because it doesnt make much sense to the normal reader.* Our nonlinear solver uses Newton's method, as opposed to a Picard iteration, so there is no correspondence at all between the relaxation parameter R and a Picard iteration. The effect of reducing R is already mentioned in the text, and a reference is given for Newton's method. As such, comparing Newton's method and a Picard iteration does not seem appropriate here.
- 1043, L21: ... *This functional to satisfy* .... Changed wording in accordance with reviewer's suggestion.
- 1045, L4: *define ALE*. Defined ALE in the text.
- 1046: α *isnt defined in the first place. Secondly it is just mentioned that it is equal to one. In short, it could be left out altogether, or it should be defined what the meaning of* α *is.* α is a weighting parameter that reduces the amount of upstream weighting in the SUPG

method in the presence of physical diffusion. In our case, there either is none, or it is much less than the advective term, so α can just be unity. This is irrelevant to the paper, so we removed instances of α.

- 1048: *Maybe this give a better stability, but what are the consequences by doing so? Has this an effect on sudden stress changes (for instance slip/no slip boundaries), or sudden changes from simple shear to plug flow?* Removing the GLS stabilization term's dependence on strain rate should not strongly affect the solution, since all it is doing is providing a small amount of diffusion over the pressure field. Moreover, the stabilization is consistent, which is to say that for linear finite elements, the extremum of Eq.(30) is also the extremum to Eq.(1).

- 1049: *r equals zero*. Changed to $r = 0$.

- 1049, L19: *is there any particular reason for the choice of L=80? Wouldt it be more appropriate to check the convergence for a more challenging experiment in which the friction field changes over short distances (high frequency) to make it more realistic? In that case a L=5 or 10km experiment would be interesting to look at*. No particular reason for the choice of $L = 80$km. We agree with the reviewer that the shorter length scales are more interesting, and switched the result shown to $L = 10$km.

- 1049, L22: *It should be mentioned that the F experiment is done for a linear rheology (n=1)*. Noted that ISMIP-HOM F is linear.

- 1050 (and following): *I find the use of $\%a^1$ quite disturbing. A percentage change per year. Why not using the real change in velocity/mass as a measure?* We fail to see the issue with using percentage change (as opposed to absolute change) in mass over time as our chosen reporting metric. Reporting a percent eliminates the use of very large, highly context dependent numbers, and is equivalent to non-dimensionalization. We find $\frac{1}{100}\%$ per annum to be a much more intuitive figure than, say $50\mathrm{km}^3$ per annum. Nonetheless, we have added an additional axis to the plot that reports the absolute value of gta^{-1} for the reader that is more familiar with these units.

- 1052: *the section on data assimilation is rather hastily written (definitely compared to the model description). The reader is referred to three graphs in one sentence and should make up his/her mind on what can be learned from it*. We were not sure what additional information about the data assimilation procedure and results as applied to Greenland the reviewer was looking for. We attempted to include some discussion of the spatial variability and notable features of the basal traction field.

- 1053 L4: *Awkwardly written. What is exactly meant by not in exact alignment with model physics? So the transient is not resolved, but what about the initialization through inversion?* We tried to make the point more clear that errors inherent in the model and errors in the data products produce a diagnostic solution that is not in a state of mass balance. Another way of saying this is that the flux divergence is quite large at the beginning of the run.

- Discussion: *an evaluation on the model performance (calculation time) should be given.* We have added a statement of computing times for a given number of processors for each of the problems included. We have also added a paragraph on efficiency to the discussion about what these run times mean for practical computation.

- P1055, L15: *I dont understand this sentence: accurate positioning (and its changes) does have a major effect on the evolution of an ice sheet on a continental scale; A large portion is of course mass balance driven, but if you disregard dynamics, then a shallow-ice model will suffice*. We do not attempt to discount the importance of grounding line migration as a dynamical process here. We merely mention that the sub-grid grounding line position is of little importance in many situations, such as terrestrially terminating glaciers, and glaciers that are relatively stable. We also note that it is fundamentally important in some scenarios. We have added text to the manuscript attempting to clarify our meaning. Quite right that neglecting dynamics means that simpler models, such as balance velocity, suffice for approximating the velocity field. However, for prognostic simulations, dynamics are necessary, and higher order dynamics are better than shallow approximations, much like a model that incorporates grounding line dynamics is better at prognostication than a higher order model that neglects them.

- P1055, L26: *Not sure about it. If you wait until the (Greenland) ice sheet has retreated away from the coastal boundary so that its effect is not sensible anymore, then the imposed BC at the edge makes sense. Short and medium time scales are rather ill-defined measures.* We clarified the meaning of the boundary condition by saying that the flux across the boundary becomes only a function of velocity, and that the height of the terminus remains always remains constant. We have removed vague references to undefined time scales, instead trying to more specifically outline scenarios where this assumption might be valid, and where it almost certainly is not.

- P1057, L15: *Not sure that this is a next generation ice sheet model. There is an inversion scheme which is suitable for initialization of the model, but the use linear sliding and the absence of dynamic boundary conditions does not make the model apt to cope with a number of challenges in glaciology. The higher-order scheme and the finite element grid construction are not sufficient as a condition for large-scale simulations. It may well become a next-generation model whenever important dynamical features are implemented.* Please see our response to this issue in the General Comments section.

4 Response to editor comment

In addition to detailed comments suggested by the editor prior to publication of the manuscript in TCD, the editor suggested the incorporation of a numerical verification scheme using the method of manufactured solutions (MMS), as in Leng et al. (2013). We have incorporated an MMS module into the code base, and have updated our numerical methods section to include the results of an MMS experiment for the both the Stokes' and FO approximations in order to verify the spatial discretization scheme that we use, and also an MMS test for our free surface evolution time stepping. We hope that this adds an additional level of code verification beyond that of the included benchmarks. This section has been added to the numerical methods section of the paper.

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