Author's response to Anonymous Referee 1 Comment 1

April 20, 2023

[Title - The title should be shortened to something like 'Numerical issues in modeling binge-purge behavior in ice streams.' No other instabilities are addressed and 'binge-purge', 'cyclic', and 'surging' are all redundant.] We have updated the title to 'Numerical issues in modeling ice stream surge cycling'.

[L20 - I think it is strange to lead with validation as a means of motivating the current manuscript, when no validation occurs here. Validation is not the same as sensitivity testing.] 'Model validation' was replaced by 'Determining model sensitivities'.

[L26 - What is 'numerical noise'? Something random? Pseudo-randomness in a chaotic system? Numerical error? This is a critical consideration but it's not really clear what it means in this paper here and elsewhere.] The following sentence has been added to the revised draft: 'By numerical noise, we refer to any non-physical differences in the model solution induced by numerical aspects such as rounding errors and convergence criteria of the numerical solver.'

[L38 - Define 'meaningful'] Removed.

[L44 - The quote from Soucek and Martinec is relevant, but it misses the fact that there are a great many approximations that appear in, for example, the solution of the Stokes' equations. Why the emphasis on numerical rather than model error?] The model error can only be determined when the 'true' solution of the equations is known. No matter what model is used (BISICLES (sliding everywhere, minimal heat treatment at the bed (just a vertical flux, which in temperate regions produces water)) and ELMER ice (too expensive for ensembles needed) are not suitable), an analytical solution does not exist for this context. Additionally, uncertainties associated with the numerical aspects of a model have received limited attention (compared to the effect of different approaches to the Stokes equations). We have updated this paragraph to:

'Modeling of binge-purge type HEs and surges in general is challenging. While the effects of different approaches to the Stokes equations have been previously addressed [e.g., Brinkerhoff and Johnson, 2015], uncertainties associated with the numerical aspects of a model have received limited attention in studies examining ice sheet surging [e.g., Payne, 1995, Marshall and Clarke, 1997, Calov et al., 2002, Papa et al., 2006, Steen-Larsen and Dahl-Jensen, 2008, Calov et al., 2010, Robel et al., 2013, Feldmann and Levermann, 2017]. Sensitivity in model response to different numerical choices are evident [Calov et al., 2010, Roberts et al., 2016, Ziemen et al., 2019] and small perturbations of the system can significantly vary the form, amplitude, and period of binge-purge oscillations [Souček and Martinec, 2011, Mantelli et al., 2016]. The exact cause of the numerical sensitivities is often unclear. Souček and Martinec [2011] thus rightfully conclude that *"... the implementation of surge-type physics in large-scale ice-sheet models is rather problematic since the information about the physical instability may be lost in the numerics"*. Furthermore, the theory underpinning the understanding of the instability mechanisms is not fully developed (no analytical solution exists), especially in the context of a spatially extended 3D system, thus precluding systematic benchmarking of numerical models.'

[L60–72 - While I recognize that this paper focuses on the ISMIP-HEINO setup, it would be worthwhile to try to contextualize this work with respect to the EISMINT-F experiments as well. There is a great deal of insight there regarding thermal sliding instabilities and the circumstances under which they appear.] The coldest PISM runs do not show any signs of temperature spokes (spokes in the EISMINT-F/H experiments are more pronounced the colder the surface temperature [Payne et al., 2000]). In the GSM, there are some warm-based areas at the margins, but this is likely due to a steep surface slope leading to a large driving stress, high velocity, and then consequently, a basal temperature increase. A short discussion of this will be added to the revised draft.

[Sec 1.1 - I find the organization of the paper according to research question to be quite challenging to follow, perhaps mostly because there are so many (11) research questions. I think it would be better to group these into open questions rather than yes/no, and this might make for more comprehensible themes. For example, Q1,11 could be grouped as 'what aspects of simulated surges are due to numerical considerations?', while Q2–6,9 could be grouped as 'what modeling and solution choices influence surging?' and Q7,8,10 as 'what parameterizations of basal physics leads to the most robust conclusions?' or something similar.] The idea behind specific research questions is to make it easy for the reader to jump right to the sections they are most interested in. Grouping these individual questions into main themes will likely increase readability, but the suggested themes do not work because several research questions fit into more than one category. Instead, the manuscript will be restructured as follows: 1) strictly numerical aspects (Q1 and Q11), 2) numerical/modeling choices with a significant effect on the results (Q3, Q4, Q5, Q8), 3) numerical/modeling choices without a significant effect (Q2, Q6, Q9, Q10). As suggested by the second reviewer, the third theme will only be a short summary, with the details moved to the supplement.

[L166 and elsewhere - I don't find it helpful to reference a manuscript that is 'in preparation' because such manuscripts are not readable and sometimes fail to ever get published. Is there some source code that could be referenced instead? An instruction manual? An older manuscript from which the ideas in the in prep manuscript are adapted?] The source code of the model version used in this manuscript can be found in the supplementary material [Tarasov et al., 2023] as stated in the Code and data availability section. Additionally, we have added this reference to the GSM description section. Older manuscripts on which the current GSM version is based on are also mentioned in this section [e.g., Pollard and DeConto, 2012, Tarasov et al., 2012, Bahadory and Tarasov, 2018].

[L198 - I am deeply skeptical of a model that 'activates' stress terms based on a heuristic that in turn depends on whether the bed is soft or hard (whatever that means). Does it not seem that such an obviously non-physical choice could lead to just as much variability in surging behavior as any of these other mechanisms? Validation is mentioned in the introduction, but what about verification? How does the reader know (especially given that there is no current reference to the model description) that this model converges to the true solution of some physically and mathematically justified system of equations under discretization refinement?] Different SSA activation criteria are available in the GSM. Sensitivity to this choice will be described in the revised supplement.

[L215 - I don't know what 'legacy' means here.] Changed to 'values used in previous GSM modeling studies [e.g., Bahadory and Tarasov, 2018]'.

[Eq. 5 - Is this supposed to be $F_{T_{\mathrm{ramp}}}$? Otherwise F_{warm} is defined twice. Also, I think it's really awkward (ignoring subscrips) to have F depend on T , which depends on a different F . Maybe consider different notation?] F_{warm} is correct. We compare our definition of F_{warm} to the one used by Fowler [1986], Mantelli et al. [2019]. 'F' represents different 'factors' in the equations. Since they are clearly distinguishable by their subscripts, we prefer to stick to the current notation.

[Sec. 2.1.2 - I generally find the phrase 'vector' to be unhelpful here. I think it would be better to describe how the ensemble is created (i.e. by selecting different values for each of eight parameters) and then referring to different members of the ensemble as, well, 'ensemble members'.] For precision/accuracy and lack of alternatives, we prefer to stick to the phrase 'parameter vector' (note that a parameter vector and ensemble member are different things).

[Sec. 2.1.2 - It takes quite a bit of flipping around to understand why we're talking about ensembles at all. I think this section could use a clear explanation of the fact that you're running each subsequent experiment with multiple different parameter settings.] Note that the text already explains the benefits of an ensemble: *'To partly address potential non-linear dependencies of surge cycling on model parameters, we use a high variance subset of 5 base GSM parameter vectors (each comprising 8 model input parameters) for our numerical experiments'.*

However, we have added the following sentence as introduction to this section. *'Each experiment uses a small ensemble of simulations.'*

[Sec. 2.1.3 I think that 'reference simulation' might be more clear than 'base setup'.] Note that 'reference simulation' and 'base setup' are not the same thing. In this study, there are 5 reference simulations (one for each base parameter vector) but only one base setup. To avoid potential confusion, we will stick to 'base setup'.

[Sec. 2.1.3 - I think that the very frequent referencing to future sections is not very helpful.] The forward referencing was meant to guide the reader and allow them to skip sections they are not interested in. However, this is better suited at the end of the introduction and was removed here.

[Sec. 2.1.3 and elsewhere - There are far too many references to supplementary information in this manuscript. SI is intended for things that either cannot appear in the manuscript itself due to medium (e.g. code or videos) or that offer additional insight or detail into some aspect of the work but that is not essential to the results. In this case, the mass balance forcing (the single most important thing in determining long term ice sheet extent) is relegated to the supplement, but really should be in the main text.] We suspect most readers will not want to read about the surface mass-balance details, and that is our criteria for main text inclusion. However, we now have more completely spelled out the temperature and surface mass-balance forcing in S1. Note that the climate forcing is already described earlier in the text: *'The GSM is run with an idealized down-scaled North American geometry (Fig. 1, modified after the ISMIP-HEINO setup [Calov and Greve, 2006]) and simplified climate representation. The temperature forcing is defined by a domain wide surface temperature (rTnorth, Tab. 1) and a specified vertical temperature gradient (atmospheric lapse rate (lapsr in Tab. 1)). The surface temperature forcing is asymmetric in time (Fig. S1), enabling the analysis of the timing of cycling onset and termination under different physical and numerical conditions (a comparison of ice stream ice volume evolution under constant and assymetric temperature forcing is shown in Fig. S2 for one parameter vector)'.*

Furthermore, Fig. S1 only shows the asymmetric aspect of the temperature forcing (atmospheric lapse rate and parameter vector dependency are not considered). Due to the simplicity of the plot, we do not deem it important enough to be in the main manuscript. However, we slightly adjusted the above text: '[...] *The surface temperature forcing is asymmetric in time (maximum difference of* $10^\circ C$*, Fig. S1)* [...]'

[2.2.1 - It's strange to imply that PISM is not also optimized for computational speed. It's the parallel ice sheet model, after all.] That was not the intent of this statement. While both models are optimized, the optimizations are not for the same contexts. The idea behind using two different models is to minimize the possibility that drawn conclusions are solely a result of the used optimization schemes. Furthermore, the GSM is optimized for computational resource use to enable large ensembles over paleo timescales and therefore not parallelized (not the case for PISM).

To make this clearer, we have adjusted: *'The GSM is an ice sheet model developed specifically for glacial cycle ensemble modelling. The GSM is therefore numerically optimized for computational speed.'*

to: *'In contrast to PISM, the GSM is an ice sheet model developed specifically for glacial cycle ensemble modeling. The GSM therefore uses a distinct set of numerical optimizations for computational speed'.*

[L303–310 - I think it would be helpful not to mix units of measurement (m/a and m/d). What is a 'stable solution of the numerical matrix solver' ? Is 'observed range' the heuristic from Cuffey and Paterson?] Agree and done.

Removed *'[...], indicating a stable solution of the numerical matrix solver even for runs with very high velocities.'*.

Yes, the corresponding part in K.M. Cuffey and W.S.B. Paterson. [2010] is: *'Speeds and displacements also vary widely. High velocities are about 100 m/day for short periods, and 5 km/yr maintained for one or two years. Low velocities are only several tens to a few hundred meters per year, values typical of many nonsurging glaciers'*.

[L318 - 'event' and 'HE' seem to be used interchangeably in the manuscript. I think it would be better to just use 'HE'.] They are not interchangeably. The term HE should be exclusively used when refering (to at least some extent) to the ocean sediment records/IRD layers. The abbreviation 'HE' and most instances of the term 'Heinrich Event' were removed.

[L330 - This is another circumstance where including the supplemental figure in the main manuscript would be very helpful.] Done.

[L341–342 'ice-free when no surge occurs.' I'm not sure it's possible to ascribe a date to when something doesn't happen.] *'a large fraction of the pseudo-Hudson Strait area is ice-free when no surge occurs'* changed to *'a large fraction of the pseudo-Hudson Strait area is only ice-covered when a surge occurs'*

[3.1.1 What about PISM? Can surges be understood similarly to those in GSM?] A short description of the PISM surges will be added to the revised draft.

[3.1.2 - I'm not sure it's reasonable to try to state a specific justification for why the time scales of this highly idealized and not-observationally-constrained experiment are dissimilar to geological records: many different factors may be in play here (some improving the fit, some to its detriment) and (as an example) saying that the period mismatch is just the result of domain size seems likes its missing a broader set of possibilities.] While several factors influence the period (e.g., bed thermal model, basal temperature ramp, basal hydrology, ...), the domain size seems to be the controlling one here. Previous experiments with a non-downscaled model domain (but otherwise identical experimental setup) yielded results within the limits of geological records. We have added: *'Exploratory GSM runs with a non-downscaled model domain (but otherwise identical experimental setup) yielded results within the limits of geological records.'* to this paragraph.

[3.1.3 - I again struggle with the notion of 'numerical noise'; the paper would be well served by having a much more in-depth description of what is meant by this and where it comes from. With respect to the latter, in most modelling exercises, the numerical error is something that can be well quantified through comparison to exact solutions or by a theoretical analysis of the interpolation properties of numerical method. Yet here, what we're effectively measuring is the system's sensitive dependence to small perturbations. In that sense, it doesn't necessarily follow that stricter convergence criteria (in the case of GSM) would necessarily lead to any less 'noise'. Could an equivalent result be achieved by just adding white noise to the initial conditions?] As mentioned previously, an exact/theoretical solution for hybrid SIA/SSA (as well as full stokes or anything in between) ice dynamics does not exist. A theoretical analysis of the interpolation properties of a numerical method is not straightforward for a coupled non-linear system of thermodynamics/ice dynamics. However, a discussion of this will be added to the revised draft. Model experiments with noise added to the surface temperature are shown in section '3.1.4 Surface temperature noise'. Timestep and resolution convergence experiments are presented in section 3.3. However, since the term 'numerical noise' seems to be causing general confusion, we have added a short definition (see above: L26). Furthermore, we have added *Surge cycling is sensitive to numerical aspects (e.g., numerical solver error).* as an introduction to the numerical noise research question.

[3.1.5 - This section on implicit coupling is so vague as to be useless. What even is 'implicit coupling' in this context? Is this the same as implicit time stepping, i.e. Backward-Euler?]

The text already makes this clear: *'The GSM has a default explicit time step coupling between the thermodynamics and ice dynamics but also includes an optional implicit coupling* *scheme'* and *'we test the impact of implicit coupling (via an iterative implementation) between the thermodynamics and ice dynamics'*. But to make this even clearer, we've changed the above to:

'As is standard for thermo-mechanically coupled glaciological ice sheet models, the GSM has a default explicit time step coupling between the thermodynamics and ice dynamics but also includes an optional implicit coupling scheme' and *'we test the impact of approximate implicit time-step coupling between the thermodynamics and ice dynamics via an iteration between the two calculations for each timestep'*.

[3.2.3 - I can't figure out what TpmTrans, TpmInt, or any other Tpm thing are. If they are described earlier, such a description needs to be here instead or also. If they are not, they need to be defined (and not in the supplement).] We have added more details here: *'In contrast to TpmTrans and TpmInt, the most straightforward approach, TpmCen (Sec. S3.2), determines the grid cell interface temperature by calculating the mean of the two adjacent basal temperatures with respect to the pressure melting point at the grid cell centers (instead of applying the pressure melting point correction after the interpolation of the adjacent grid cell center temperatures)'*. However, we prefer to keep the in depth description including the equations in the supplement.

[3.2.4 - It's not my preference, but if you prefer to have the actual equations describing GSM in the supplement, I suppose that's fine. However we need at least a little bit of a qualitative description of what these different 'weights' imply. What is the context for understanding why these different choices should yield different surging behaviors?] This section refers to Q6. To clarify the purpose and context, we restructured and slightly adjusted this section to: *'Depending on the location of the adjacent minimum grid cell center basal temperature, either the ice flow (when the adjacent minimum basal temperature is downstream) or upstream propagation of the surge should be affected (decreasing basal interface temperature with increasing weight). For the large-scale surges, the adjacent minimum basal temperature is almost exclusively located upstream (e.g., video 02 of Hank [2023]). Changing the weight of the adjacent minimum basal temperature, therefore, affects the surge propagation rather than blocking parts of the ice flow.*

Here we compare the effect of three different weights on the GSM event characteristics (Eq. (S5)): no consideration of adjacent minimum basal temperature ($W_{Tb,\mathrm{min}} = 0.0$), basal temperature at the interface depends to 50 % on the adjacent minimum basal temperature at the grid cell center (base setup, $W_{Tb,\mathrm{min}} = 0.5$), and basal temperature at the interface is equal to the adjacent minimum basal temperature at the grid cell center ($W_{Tb,\mathrm{min}} = 1.0$).'

[Fig. 7 - I honestly can't figure out what this figure is trying to convey. Part of this is that I also can't figure out what the part of the text that references it is trying to convey either (L534–540). Please try to make this a little bit more clear.] The reasoning behind Fig. 7 is described in L523-533: *'We complement the above analysis by upscaling the* 3.125 km *base runs. For example, a* 25*x*25 km *grid cell contains a patch of* 64 3.125*x*3.125 km *grid cells. The scatter plot of the warm-based fraction (basal temperature with respect to the pressure melting point at* 0 °*C) and the mean basal temperature with respect to the pressure melting point of the patch can be used to estimate the parameters T_{ramp} and T_{exp} of the basal temperature ramp (Eq. (3)). [...] Consequently, this estimate for the basal temperature ramp should be a lower bound to the points in the scatter plot. [...]'*. The scatter plot described here is what is shown in Fig. 7. To make this clearer, Fig. 7 is now referenced right after the first 'scatter plot'.

[3.3 - Brinkerhoff and Johnson, 2015 suggest that the inclusion of membrane stresses leads to convergence under spatial grid refinement, whereas without them, the SIA does not lead to convergence. Can you place those results in the context of this section? Are the relatively weak convergence results here a result of GSM and PISM velocity solvers being insufficiently 'higher-order'?] Both the GSM and PISM use a velocity dependent switch between pure SIA and a membrane stress approximation. Analysis of GSM sensitivity to SIA/SSA switching rules will be added to the revised draft.

[Sec. 4 If you maintain these research questions as an organizing principle, I would like to see them revisited as they are resolved by the experiments rather than all at once at the end.] We suspect that not every reader will be interested in every detail of the results section. The summary section provides an easy way to get the most important results and allows the reader to then jump to individual results for more details. Therefore, we would like to keep the summary section.

[L738–746 - I think that this section is kind of weird: none of the results presented in this work actually refute the resolution dependency conclusions of Hindmarsh (2009) or Brinkerhoff and Johnson (2015), yet the paragraph is written as if they did. As mentioned before, it seems just as reasonable to assert that those works saw more robust numerical convergence due to the use of more consistent membrane stress resolution schemes rather than because they fortuitously (or nefariously) made parameter choices that suppressed resolution effects.] This paragraph was added to provide possible explanations for the different conclusions, not necessarily to refute the conclusions of Hindmarsh [2009] or Brinkerhoff and Johnson [2015]. However, the fact that different parameter choices can yield very different results remains. To clarify this, we have updated *'This is in contrast to the findings of other studies examining thermally induced ice streaming [Hindmarsh, 2009, Brinkerhoff and Johnson, 2015]. However, both of these studies analyze just one parameter vector, and it is relatively easy to find a parameter vector for which, e.g., the GSM exhibits only a minor resolution dependence.'* to *'While other studies examining thermally induced ice streaming do not find a strong resolution dependence [Hindmarsh, 2009, Brinkerhoff and Johnson, 2015], these studies are not directly comparable. The different results are likely due to differences in the experimental design. For example, the hybrid SIA/SSA ice dynamics used in the GSM and PISM might lead to a stronger resolution dependence than the schemes used in Hindmarsh [2009], Brinkerhoff and Johnson [2015]. Additionally, both of these studies analyze just one parameter vector, and it is relatively easy to find a parameter vector for which, e.g., the GSM exhibits only a minor resolution dependence.'*

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