Author's response to Clemens Schannwell's Comment 1

June 28, 2024

1 General Comment

[The manuscript by Hank & Tarasov presents a suite of simulations that aims to investigate the sensitivity of the Glacial Systems Model (GSM) to processes that have previously been shown to have an influence on Heinrich Event characteristics in other models. The novelty in comparison to previous studies is that the authors use transient forcing and present an ensemble that covers a much larger parameter space. Based on this, they confirm earlier findings from e.g. Mann et al. [2021], Schannwell et al. [2023] that GSM also exhibits two different states - a streaming and cycling surging state. Moreover, they show that in their model, geothermal heat is a major controlling factor while ice shelves and their buttressing matter very little.]

[Overall, the topic of the paper is interesting and I believe that the science behind it is sound. However, in its present form the reader is drowned in the sheer amount of simulations that are presented in a rather unstructured fashion. The lack of structure in the paper and particularly in the methods sections leaves the reader confused as to what exactly the authors did and makes it challenging to find a common thread through the manuscript as well as evaluate the scientific results. Therefore, I do not think the paper is ready for publication in its current form and a substantial restructuring and part-rewrite is required. I recommend the authors take into account my comments listed below. I hope the authors find my comments helpful.]

We thank the referee Clemens Schannwell for their constructive comments. A point-by-point reply is reported below, with referee comments in orange and our replies in black. We agree with the specific referee comments not listed here and will revise the manuscript accordingly.

We will restructure the manuscript according to the referee's helpful suggestions, including a short roadmap at the end of the introduction and a table summarizing the reference setup and all experiments.

2 Specific comments

[The entire methods sections felt really incoherent almost as if sections were being added as new ideas for sensitivity simulations were designed. By the time I got to the results, there were so many simulations and sub-ensembles that I was utterly confused what was done in the end and what the different simulations were actually referring to. The absence of an experiment table or a flowchart of how the different sub-ensembles were generated just confounded my confusion. My suggestion is to remove section 2.1. "Modelling approach" and really start with the model description (your current section 2.3). In there, I would like to see what type of model GSM is! I believe it is an ice-sheet model but this is never really spelled out. This section should also incorporate all of the coupling that is included e.g. subglacial hydrology etc. as well as time-steps, resolution and the like. Then, I would follow that by all the boundary conditions e.g. your current sections 2.4 to 2.5.2. Then move on to the section that you call "Ensemble parameter vector" which I suggest to rename to "Creation of sub-ensembles" as I believe it makes it more accessible. In this subsection, you absolutely need a table with the different sub-ensembles as well as a flowchart illustrating how each of these sub-ensembles was generated, e.g

Baseline ensemble -> sieve 1 -> sieve 2 etc. Also, I find the section title "Bounding experiments" somewhat confusing. Do you mean end member scenarios? If so, why not name it like that?] The methods section in the revised draft will be restructured following some of the above suggestions, including a table summarizing all experiments, merging the *Modeling approach* and *Model description* sections, and re-naming of sub-sections. Furthermore, a description of the GSM will be submitted to GMD for open review prior to submission of our revisions.

[In your results section you often speak about specific parameter vectors, for example in the caption of Figure 4 you mention "parameter vector 1". It is unclear to me to what sub-ensemble this refers to as parameter vector 1 is never really defined in the manuscript. So, I strongly recommend to define these parameter vectors somewhere in the methods section (e.g. in a table), so that the reader can go back and check what this refers to.] In the current model setup, the GSM has 52 input parameters (Table. S1). A parameter vector holds one value for each of these input parameters. As such, each parameter vector fully specifies how the configuration of the GSM varies between sub-ensemble members. Our reference ensemble contains 20 parameter vectors (20 GSM runs). For our experiments, we vary other aspects of the GSM configuration (e.g., a different geothermal heat flux (GHF) than used in the reference ensemble) but use the same 20 parameter vectors. This allows us to determine the effect of the change in model configuration while (albeit partly) considering associated parametric uncertainties. Fig. 4 in our submission shows the GSM output when using the first parameter vector (first set of 52 input parameters) in our reference ensemble. The revised draft will include a more detailed description of this terminology and design.

[This issue was already raised by the other reviewer, but I believe that the effect of the transient climate forcing certainly warrants a discussion. First of all, it is never mentioned what climate forcing is used to drive the model. This goes back to my previous point that I am not sure whether GSM is an ice-sheet model or a coupled climate-ice sheet model. In any case, it remains unclear to me whether the differences you report here are due to your parameter changes or due to the changing transient forcing and it seems quite likely that there is at least some signal from the transient climate forcing in your results. This ought to be acknowledged and discussed.] To answer this comment, we expand on the logic described in the comment directly above. Fig. 7, for example, shows the effect when applying different GHFs for parameter vector 18. Therefore, the input parameters and the applied transient climate forcing are the same for all runs shown in Fig. 7. The differences are, therefore, not caused by a different climate forcing.

The climate forcing, however, varies between different parameter vectors depending on the values of the climate input parameters in Table S1. Or to rephrase, each experiment is carried out with the same set of 20 different transient climate forcings (20 parameter vectors). As indicated in our response to the first referee comment, *The key idea behind this approach is to identify physical processes that show a significant effect across simulations with different ensemble parameters. This both increases confidence in our results and reduces the possibility that an observed modeling response is only due to, e.g., the chosen climate forcing.* The ensemble description will be updated in the revised draft, including a plot showing the ice volume of all parameter vectors when run in the reference configuration, and MIS 3 and LGM example time-slice map plots.

[On a related note, is it correct that your reference setup (e.g. Fig 7) does not show any Heinrich events, but simply a steady ice stream? I believe it is never mentioned, but your algorithm does not detect any surges in that time series right? But then it is unclear to me how you calculate the period and number of surges presented in Table 1.] It is correct that the algorithm does not detect any surges (or, more precisely, detects less than 3 surges) when parameter vector 18 is run in the reference configuration. As described in the text and our response to the first referee comment, the reference ensemble (20 runs) can be divided into two high-variance (in parameter values and run metrics) subgroups: one subgroup of 10 for which all runs have more than 2 surges (at the higher resolution), and one for which this is not the case for the higher resolution run but is the case for the corresponding lower resolution run (i.e., with the same parameter vector). Parameter vector 18 belongs to the second subgroup, which is not included in the results presented in, e.g., Table 1. However, Fig. 7 clearly shows that surges occur when a smaller GHF is applied.

[You introduce the term "Minimum Numerical Error Estimate (MNEE)" which at least to me is a new concept. I think it should be clearly stated whether this is a common concept from the field of numerical analysis or something that you have introduced (I see that you used a the same approach in your previous paper). If you introduced it, it would be helpful to briefly(!!) motivate why this is a useful quantity. Because from what you mention in between lines 274 – 279, this seems a rather arbitrary choice (increasing your Picard iteration by one). I am also confused that your only parameter to measure MNEE is the "number of iterations". At the very least, I was expecting a combination of "number of iterations" and "residuals". The reason for this is that I expect the residual to be higher after 4 iterations if you are in a rapidly changing state than after 2 iterations in a relatively stable state. In my view, the way you have defined it is inconsistent because it does not tell anything about whether your solver has actually converged or not. For example, you can increase your number of iterations to 50, but that does not say anything about the quality of your solution. Also since GSM is run in serial, why not compare it to the solution from a direct solver like UMFPACK?] Yes, the concept of MNEEs was introduced in Hank et al. [2023]. They are specified thresholds for determining whether a difference in a sensitivity experiment is within a minimal estimate for the numerical uncertainty of the ice sheet dynamics solution. Given the non-linearity of surge onset and termination, this plays a potentially important part in our analysis. More specifically, as described in the text, MNEEs for the GSM are the maximum model difference across two different model simulations: 1) imposing stricter (than default) numerical convergence (what the referee refers to as smaller residual) and 2) imposing stricter numerical convergence with increased maximum iterations for the outer Picard loop (from 2 to 3, solving for the ice thickness) and the non-linear elliptic SSA (Shallow-Shelf Approximation) equation (from 2 to 4, solving for horizontal ice velocities). Therefore, the MNEEs are based on simulations with 1) smaller residual and 2) the combination of increased maximum number of iterations and smaller residual, but never just on an increased number of maximum iterations. The description of MNEEs will be updated in the revised manuscript to clarify this.

[At the end of the introduction you list five research questions that your paper aims to answer. This is admittedly a bit of a subjective matter, but my impression was that these questions are very generic and could be summarized with something like are our Hudson surges sensitive to perturbations in geothermal heat flux, GIA, ocean melting, etc. While certainly worth exploring, to me, this type of question layout is more suited to a thesis format but not necessarily for a paper. I also could not help, but get the impression that you do a bit of everything which results in a lack of focus what you are really trying do address here. For example, I do not think that your paper actually addresses Q3 as you solely focus on the Hudson ice stream. As an add-on, I am also in favour of a short paper roadmap at the end of the introduction especially when it is as complicated and long as this paper.] We will add a short roadmap to the end of the introduction.

Yes, what we are addressing is broad, but we do not see an alternative given our stated intention: *However, an extensive study simultaneously investigating the relative role of each proposed HE hypothesis is still missing. Furthermore, previous model-based tests of HE-related Hudson Strait surge cycling have not addressed uncertainties in key potentially relevant processes and inputs. These include the deep geothermal heat flux under Hudson Strait, glacio-isostatic adjustment, and the form of the basal drag law employed.* In short, instead of tidy, idealized experiments addressing single hypotheses and ignoring the role of other possible mechanisms, we aim to address the lack of a comprehensive assessment of the relative role of proposed mechanisms while addressing key relevant uncertainties. Ignoring the latter negates the interpretative value of the former. We believe the stated research questions provide a clear structure for this assessment, and the question presentation succinctly summarizes the motivation for each question.

Furthermore, we suspect that not every reader will be interested in every detail of the results. Organizing the discussion section according to the research questions outlined in the introduction provides an easy way to get the most relevant information and allows the reader to then jump to individual results for more details.

As the majority of HE IRD in the North Atlantic is attributed to Hudson Strait, examining the ice shelf volume around it allows us to, at least partly, address Q3, especially considering that proxy records indicate an seasonally ice-free Labrador Sea during MIS3 [Hillaire-Marcel et al., 1994, Hesse et al., 1999, De Vernal et al., 2000, Gibb et al., 2014]. Additionally, we will include the Labrador Sea and Baffin Bay ice shelf volume at key time slices in the revised draft.

3 Technical corrections

[L2: I think instead of using the term Glacial Systems Model, it would be better to refer to the type of model like coupled ice sheet-subglacial hydrology model. Of course, if you'd like to keep GSM in there, you could combine these two.] Additional details about the exact model type will be added to the revised draft. However, Glacial Systems Model (GSM) is simply the name of our model (e.g., comparable to Parallel Ice Sheet Model (PISM)). Since this name has been used in several publications [e.g., Drew and Tarasov, 2023, Hank et al., 2023], we prefer to keep it.

[L38: Not to be too picky, but I would argue that we did test the sensitivity of mPISM to the geothermal heat flux in our Schannwell et al. [2023] paper] We thank the referee for pointing this out. We will adjust the text accordingly.

[L89: What is the physical mechanism for the choice to allow sliding at sub-freezing temperatures?] Observational and experimental evidence suggests that basal sliding starts below the pressure melting point and ramps up as the pressure melting point is approached [e.g., Barnes et al., 1971, Shreve, 1984, Echelmeyer and Zhongxiang, 1987, Cuffey et al., 1999, McCarthy et al., 2017, Mantelli et al., 2019]. Furthermore, Mantelli et al. [2019] show that an abrupt sliding onset at the transition from a cold-based ice sheet to an ice sheet bed at the pressure-melting point causes refreezing on the warm-based side and, therefore, cannot exist. An additional numerical argument can be made on numerical grounds for coarse horizontal grid resolutions. It is unlikely that an entire grid cell reaches the pressure-melting point within one time step [e.g., 25×25km in 1 year Hank et al., 2023]. All of these aspects are described in detail in Hank et al. [2023]. To clarify this, we will add this reference to the corresponding sentence.

[L93: Be precise here! What components are included in the current setup? Also asynchronous coupling mean different things to different communities. Does that mean you run your GIA model accelerated? Moreover, for your GIA model what kind of ice thickness distribution do you prescribe in the southern hemisphere or the other northern hemispheric ice sheets? This confusion originates from the fact that you never specify what your modelling domain is. In addition, it would be helpful to provide more detail about the GIA model, because out of the blue in the results section, you start talking about local and non-local GIA.] There is no acceleration. Visco-elastic GIA models are generally run on spherical harmonic grids and, therefore, must be global. We use the ice sheet chronologies from recently completed history matching for the non-NA inputs into the GIA calculation. As stated in section 2.1 of our submission: *The topography and sediment cover of the entire model domain are shown in Fig. S1.* As an aside, we have never seen "asynchronous" alone imply acceleration in modeling. Our revised submission will explicitly answer the reviewer's questions.

[L121: What is that resolution in kms for the Hudson ice stream. How does this coarse resolution affect your ability to model surging. We saw quite dramatic changes when we increased resolution from 50 km down to 20 km some years ago.] The grid cell size in Hudson Strait is roughly 25x25 km. As stated in the text *Due to the inclusion of a resolution-dependent basal temperature ramp [Hank et al., 2023], the differences in surge characteristics between the coarse resolution runs (horizontal grid resolution of* $\Delta_{\mathrm{lon}} = 1.0°$, $\Delta_{\mathrm{lat}} = 0.5°$*) and the reference runs* ($\Delta_{\mathrm{lon}} = 0.5°$, $\Delta_{\mathrm{lat}} = 0.25°$*) are generally within the MNEEs (Fig. 6). While finer (than the reference setup) horizontal grid resolutions are currently unfeasible in the context of this study, given the results of resolution response testing of surge cycling down to* 3.125 km *horizontal grid resolution in Hank et al. [2023], the differences in surge characteristics for finer resolutions are also expected to be within the MNEEs.*

[L167: From your description it is unclear whether or not you do "Schoofing", meaning whether you prescribe the Schoof flux as an internal boundary condition at the grounding line. Please clarify.] As stated in our response to the first referee comment: *The GSM uses the Schoof [2007] grounding line flux condition as implemented in [Pollard and DeConto, 2012]. The authors only recently became aware of issues around this approach for complex 2D geometries likely of most consequence for Antarctica [Reese et al., 2018], and the revised validated treatment [Pollard and Deconto, 2020] has subsequently been implemented in the GSM. Given the geometry of Hudson Strait, we do not expect this change to have much impact. However, to be safe, we are in the process of testing sensitivities for Hudson Strait and will document this in the revisions.*

[L197: What do you do if you do not have any ocean points under your floating ice shelf? Do such situations arise?] We are not clear on what context the referee is referring to. However, for each of sub-shelf melt, GIA within the ice sheet grid, and ice dynamics, the GSM handles a changing ocean mask, and sub-marine temperatures are spatially extrapolated as needed for the subshelf melt calculation. This will be detailed in the forthcoming GSM description submission.

[L199: Do you mean you tune your parameters for present-day ice sheets, because there is no Laurentide today.] The text will be corrected to *computed melt brackets present-day observations* for major Antarctic ice shelves (e.g., ...).

[L223: This makes it sound as if GSM has an ocean component coupled to it, where in fact you are using ocean temps. from a GCM simulations, right? If so, please rephrase.] Yes, the ocean temperatures are derived from the TraCE deglacial simulation run with the Community Climate System Model Version 3 [CCSM3, Liu et al., 2009]. We also already state in our submission *Using a glacial index approach, the ocean temperature chronology is interpolated between full glacial (last glacial maximum) and present day conditions for all other time slices.* However, to better avoid the confusion the reviewer is indicating, we will change the phrase to *GSM ocean temperature forcing.*

[L230: Even for me as an ice-sheet modeller, this is getting quite hard to follow here. I am not sure, but is your ice shelf removal simply a very high basal melt rate that melts your ice shelf away? If so, what is the time it takes for the ice shelf to disappear and how might this potentially rather gradual removal affect the response in comparison to a sudden removal of the ice shelf?] The ice shelf removal experiments are simply based on increased ocean temperatures (as shown in Fig. 3). This experimental design is in line with proxy records indicating ocean temperature increases over a 1 to 2 kyr interval prior to HEs [e.g., Marcott et al., 2011]. The mechanism is also explicitly motivated in our initial presentation of Q_2: *A* $2°C$ *increase in the sub-surface ocean temperature has been shown to cause a* 6 *fold increase in the ice shelf basal melt rates in front of Hudson Strait* ($\sim 6\ \frac{\mathrm{m}}{\mathrm{yr}}$ *to* 35-40 $\frac{\mathrm{m}}{\mathrm{yr}}$*) in simulations with an ocean/ice-shelf model [Marcott et al., 2011].* Since the GSM incorporates the relevant physics for rapid ice shelf disintegration, we consider this approach physically more defensible than artificially removing the entire ice shelf.

As stated in the text *The ocean temperature at the relevant depth is then used to calculate the sub-shelf melt* M_{SSM} *and terminus face melt* M_{face}. Since both M_{SSM} and M_{face} depend on ensemble parameters and the ocean temperature of the current experiment, the time it takes for the ice shelf to disappear varies between different parameter vectors and experiments. The ice shelf response to a change in ocean temperature and calving varies from no significant change to rapid (< 100 yr) ice shelf disintegration (e.g., Fig. 13).

[L349: Again, this is pretty much what we showed in Schannwell et al. [2023].] A brief comparison will be added to the discussion.

[L365: What does crashing mean here? Solvers did not converge anymore?] Yes, the solvers did not converge with the specified minimum model time step of 0.015625 yr. This threshold could be lowered, but given how far this is already below nominal CFL for our grid resolution, such small time-steps raise concerns about non-linear numerical instabilities that will distort our analysis.

[L366: I think, this certainly needs some discussion why you think the switch from Weertman to Coulomb makes such a big difference in the run time.] As mentioned in our response to the first referee comment: *As the regularized Coulomb law negligibly increases basal drag beyond the order of the regularization threshold ($UV_{\mathrm{C,reg}} = 20$ m/yr), we expect it to be much more unstable than the Weertman law according to CFL constraints. This is compounded by the schoofing grounding-line flux iteration in the SSA solution.*

It should also be noted that the GSM SSA solution imposes an upper bound of 40 km/yr on SSA ice velocities for this configuration. We suspect that this is higher than most other models. The imposition of this upper bound is itself another non-linearity in the solution that can contribute to both instability (as adding non-linearities will generally decrease convergence of iterative solutions) and stability (by limiting ice velocities). A numerical challenge with Coulomb plastic (regularized or not) is that, unlike Weertman, there is a constant basal drag term in the SSA equation that does not automatically/implicitly have the correct sign. We will add a brief description of all this to the revised draft.

[L392: In the interest of shortening the paper, consider removing everything regarding the regularized Coulomb and simply state that run times were too long to make these runs feasible.] We respectfully decline this suggestion. A major motivation of this paper was to comprehensively address key uncertainties not (or at least mostly not) addressed in previous tests of hypotheses for explaining Heinrich events. Uncertainties in basal drag law are one such uncertainty; thus, we consider it important to retain this subsection.

[L457: Are you sure your mean "increase" here? It is possible, but I would argue an elevation decrease is more often than not associated with an accumulation decrease.]

Yes, increase is correct here, but the statement should have been *precipitation (and therefore accumulation over most of the ice sheet) generally increases ....* Assuming everything else is the same, a lower ice sheet surface elevation will have a higher precipitation rate (relation between atmospheric lapse rate and the Clausius–Clapeyron formula for saturation vapour pressure) though this is complicated by slope orographic forcing dependencies of precipitation in the GSM.

[L569: What are pseudo-Hudson Strait surges? Bassis et al. use an idealized setup that is based on the geometry of the Hudson Strait. Does GSM have the marine-ice-cliff instability mechanism implemented? This is an integral part of the Bassis et al. mechanism and renders the comparison pretty far-fetched if it doesn't.] "pseudo"-Hudson Strait surges is an earlier notation and will be removed in the revised manuscript.

Yes, the GSM does have the MICI, but only for iced grid cells adjacent to neighbouring open ocean. This will be made explicit in the revisions. However, as stated in the text: *Due to the numerous differences in the model setup (e.g., model domain considered, grid discretization near the grounding line, GIA model, calving and sub-shelf melt implementations, and the lack of ice thermodynamics in Bassis et al. [2017]), we do not aim to directly replicate the experiments in Bassis et al. [2017]. Instead, we examine the role of SSOW in a HE context by applying a sub-surface ocean temperature increase for every DO event.*

[L574: I find these melt-rates unreasonably high. I mean maximum present-day melt rates are around 100 m/yr and you have four times the melt during the glacial? That seems very hard to believe.] 400 m/yr are the highest melt rates occurring in all of our experiments, including the end-member scenarios. Since some end-member scenarios were specifically designed to increase the melt rate, it is unsurprising that the maximum modeled melt rates exceed the maximum observed present-day melt rates. The melt rates in the reference setup are generally below 100 m/yr. This will be made explicit in the revisions.

[L606: I am pretty sure that you did not show synchronization of your HEs with the Dansgaard-Oeschger cycles.] We agree that we did not show a synchronization of HEs with the coldest phases of the Bond cycles. However, the timing of surges is affected by the applied additional ocean forcing, indicating the possibility of synchronization. We will clarify this in the revised draft.

4 Figures

[The Figures are overall of good quality. What I am missing is a Figure of the modelling domain. If it is a global setup, this is not needed, but then this needs to be stated clearly in the text.] As stated in section 2.1 of our submission: *The topography and sediment cover of the entire model domain are shown in Fig. S1.* Additionally, as indicated above, in our revised version, we will include some whole NAIS (showing whole model grid) example time slices.

[Fig. 4: and throughout. I am not a big fan of pythons default option to have the scientific notation on top of each subplot in pretty small font. For a second I thought that your flux was as high as 2 Sv. You could try using mSv instead or work that exponent into your axis titles.] Agreed, we will change to clearer labelling.

[Fig. 5: Consider removing repetitive colourbars and axis labels for the benefit of larger panels.] Given the number of panels, we find interpretation easier when the colorbars and labels are in each plot frame. However, we will test removing repeated (within row or column) grid axis labels for the revised draft. We will also defer to the editor and Journal staff for further guidance.

[Fig. 7: Judging from this, your reference simulations has no Heinrich events? But why is there an ice volume increase at the same time as the ice flux increases? What is the origin of this?] As already addressed in our response above, parameter vector 18 (base vector shown in Fig. 7) is part of the ensemble with < 3 surges detected. In the Fig. 7 plots, the reference run is shown in orange. The most significant growth in Hudson Strait ice flux occurs between 70 and 60 kyr BP. This is also the time of the most significant whole ice sheet volume growth in the model. The increased ice volume eventually increases ice flux to and, consequently, through Hudson Strait (Fig. 1 in this response). As this is peripheral to the focus of our submission, we see no need to add this brief analysis to it.

[Supplementary Figures: I can also only reiterate what the other reviewer mentioned, 40 Figures in the supplement is a lot and you should really make sure that each of these Figures serves a purpose. For example, what does the Figure S8 add? In the text it is referenced to show a more gradual increase in ice flux, but I have no idea how a snapshot of the velocity field could convey such a message.] As mentioned in our response to the first referee comment, we will reduce the amount of figures by merging plots with similar content. We have verified that every supplemental figure is referenced at least once in the main text. We are aware this is on the long side, but we are generally going by our rule of thumb that if more than one reader is likely to want to see the plot and less than a dozen are likely to, then stick in the supplement. Most readers are free to ignore the supplement and just note that there is graphical documentation of the relevant claim. We will, however, carefully go through all the supplemental figure referencing and reconsider if at least a few can be eliminated.

Fig. S8 is used in the following sentence: *During these surges, ice transport from Hudson Bay and Foxe Basin through Hudson Strait (and other outlets) towards the ice sheet margin increases (e.g., Fig. S8).* The confusion may stem from the abbreviation S8 for surge 8 (appearing in *"gradual increases in Hudson Strait ice flux (S8 and S9)"*), which will be rectified in the revised draft.

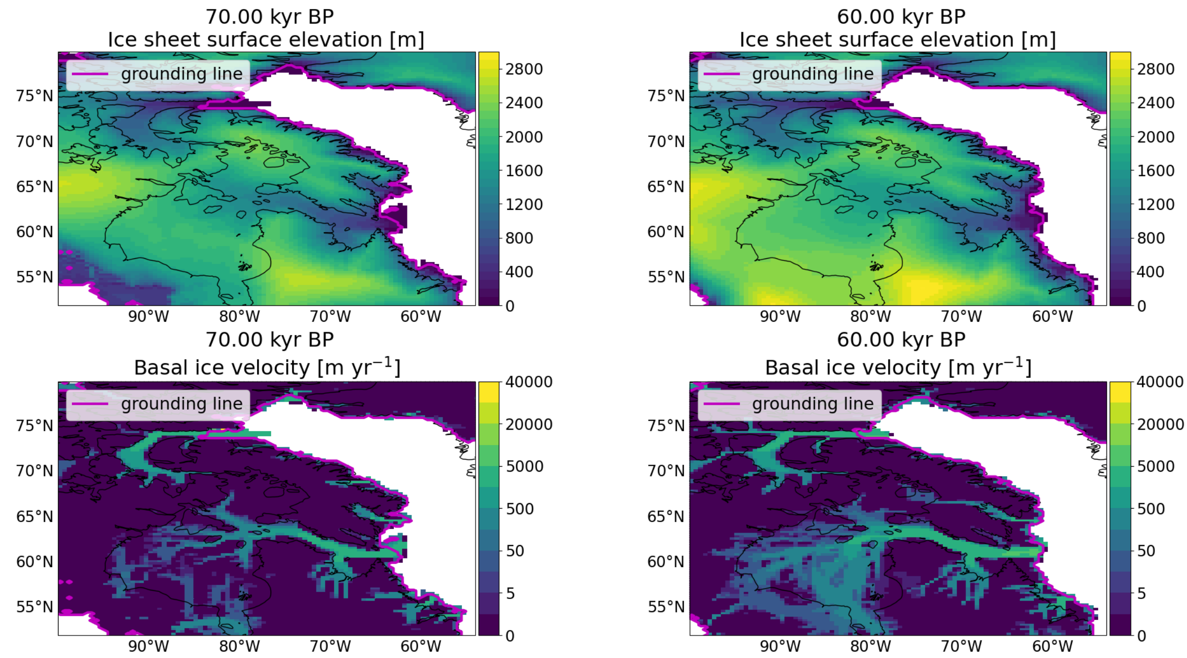

Figure 1: The ice sheet surface elevation and basal ice velocity of parameter vector 18 are shown at 70.0 kyr BP and 60 kyr BP. The black contour is the present-day coastline provided by *cartopy* [Met Office, 2010 - 2015].

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