Review of Hank & Tarasov "The comparative role of physical system processes in Hudson Strait ice stream cycling: a comprehensive model-based test of Heinrich event hypotheses"

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General comments:

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.

Specific comments:

1. 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 timesteps, 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 endmember scenarios? If so, why not name it like that?

2. 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.

3. 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.

4. 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.

5. 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?

6. 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 summarised with something like are our Hudson surges sensitive to perturbations in geothermal heatflux, 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.

Technical corrections:

Abstract:
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.

L20: Add a reference to the original Heinrich paper?

L35: You could add for completion our atmospherically driven mechanism recently published in Schannwell et al. [2024].

L38: Not to be too picky, but I would argue that we did test the sensitivity of mPISM to the geothermal heatflux in our Schannwell et al. [2023] paper

L85-86: All models are parallelisable, but GSM in its current incarnation is not parallelised.

L89: What is the physical mechanism for the choice to allow sliding at sub-freezing temperatures?

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.

L110: It took me quite a while to realise that what you refer to as mid-Hudson Strait ice flux is a flux gate that is some distance upstream from the grounding line. I think it would be much clearer if you spell this out explicitly somewhere.

L116: Maybe worth defining that # stands for number of surges

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.

L133: Delete second "with"

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.

L164 and L205: Here and throughout, I am a supporter of self-contained papers and hence not a big fan of excessive referring to papers that are in preparation and may or may not get published in the future. But I leave this to the editor to decide what CPs policy concerning this is.

L194: d_{OF} has not been defined yet.

L197: What do you do if you do not have any ocean points under your floating ice shelf? Do such situations arise?

L199: Do you mean you tune your parameters for present-day ice sheets, because there is no Laurentide today.

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.

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?

L310: As mentioned in my main points above, I simply do not know what parameter vector 1 refers to? Is this a single simulation, a composite, or a sub-ensemble?

L344: Again an explanation, definition what your refer to as parameter vector would be highly appreciated.

Table 1: Again it is unclear to me what the reference setup really is?

L349: Again, this is pretty much what we showed in Schannwell et al. [2023].

L365: What does crashing mean here? Solvers did not converge anymore?

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.

L392: In the interest of shortening the paper, consider removing everything regarding the regularised Coulomb and simply state that run times were too long to make these runs feasible.

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.

L569: What are pseudo-Hudson Strait surges? Bassis et al. use an idealised 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.

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.

L606: I am pretty sure that you did not show synchronisation of your HEs with the Dansgaard-Oeschger cycles.

L614–616: Are you sure you want to end with something like this?

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.

Fig. 2: The contours are very hard to see in the left but primarily in the right panel.

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.

Fig. 4 caption: Again, it is unclear what parameter vector 1 is. I think one way of helping the reader other than a clear definition is a more intuitive name such as GIA sub-ensemble.

Fig. 5: Consider removing repetitive colourbars and axis labels for the benefit of larger panels.

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?

Fig. 9: Somewhere you should mention what type of kernel you use and what your bandwidth is and how/why you chose 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.

Sincerely, Clemens Schannwell

References

L. E. Mann, A. A. Robel, and C. R. Meyer. Synchronization of heinrich and dansgaard-oeschger events through ice-ocean interactions. *Paleoceanography and Paleoclimatology*, 36(11), Oct. 2021. ISSN 2572-4525. doi: 10.1029/2021pa004334. URL http://dx.doi.org/10.1029/2021PA004334.

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