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

Kevin Hank and Lev Tarasov present a comprehensive study on the potential causes of Heinrich events. Their aim is to investigate several hypotheses which have been proposed in the literature, mainly: the effect of geothermal heat flux and basal drag, buttressing effect, subsurface melt and GIA. For this, they use the glacial system model (GSM) and for a set of experiments (20) they conduct a test of sensitivity experiments.

While I believe the manuscript holds significance and aligns with the focus of Climate of the Past, I suggest some restructuring prior to publication. While the introduction effectively outlines the study's goals, the subsequent sections lack cohesion, making it challenging for readers to follow. There are too many figures which are referenced several times without following a chronological order necessarily which makes it easy to lose the guiding thread. Additionally, the interpretation of the experiments may be difficult to follow.

My biggest concern is related to the investigated ensemble parameters. I think the choice of parameter space feels a bit arbitrary and unnecessarily extensive for the paper's aims. I want to emphasize that I recognize the value of this manuscript but stress the importance of enhancing its readability for a broader audience. Below you can find my main concerns.

General comments:

Main manuscript:

Parameter choice:
I do not understand how you choose your 20 parameter vectors. First you run ~15000 simulations, from which you consider ~200 simulations as realistic based on your sieves. Then you redo these 200 simulations at higher spatial resolution and apply new sieves based on IRD layers. From those you hand pick 20 simulations. Have I understood it correctly?

I am a bit skeptical about some of your parameters. A lot of your parameters are related to climatologies (for example: global LGM temperature scale factor, desert-elevation exponent, temporal Empirical Orthogonal Function weight 1), but you are not assessing the role of different climatologies in your study. Since this is not the focus of your manuscript I think you need to apply the same climatologies to all of your experiments. If not, the occurrence of surges in your experiment could be caused by different climatologies rather than ice dynamics or other forcings. This would be also very beneficial for the readers since you introduce a lot of parameters (52 in Table 1) which you do not explain and are very technical.

I get the feeling that many of your selected parameters are the same as those of Tarasov et al. in preparation describing the model GSM. Though investigating such a large parameter space makes sense for a description paper, I do not think it is intended for this manuscript.

Reference state

This point goes a bit in line with my previous comment. Many of your plots only show one vector parameter but it's not always the same vector parameter. To me this feels confusing, I would prefer to have a reference vector over which you change conditions rather than different states. If you are trying to assess the effect of different boundary conditions, friction laws or oceanic forcings it would be more useful to have one reference state over which you change conditions.

Hydrology model:

Based on Figure 4 and Figure 7 it seems that basal melt below grounded ice plays a major role in your surges. You need to explain how you compute basal melt for grounded ice points and your hydrology model. Since it is a local hydrology model and you state that you saturate your water thickness at 10 meters i assume that you do not conserve mass, right? How does your water thickness affect your surges? I guess it will play a major role in your surges and I think it is necessary to investigate that parameter.

Additional

What does your LGM ice sheet look like? How does it compare to other studies? I am also missing your forcing index. You could add it in your time series plots.

Supplementary Material:

41 figures in the Supplementary Material is way too much and makes it difficult to follow the paper. These figures are referenced too many times and cuts the flow for the reader. Please consider reducing the amount of figures with those which are strictly necessary for your article. For example, you could merge figures S2-S6.

Table S1 is complicated to understand for the reader. First, I think you investigate too many parameters, but if the authors decide to follow this approach, then I would suggest splitting it in different sections, such as ice dynamics, climatologies, GIA, GHF (you could add a horizontal line). In addition, you define parameter names which you do not use in your manuscript, I do not think this is necessary.

Technical comments:

Section 2.3

Please describe the equation of your Weertman-type power law. You state that your parameter C_warm depends on other parameters such as C_rmu and C_fslid but you do not give further detail. Either you remove that sentence or you explain how C_warm depends on those parameters.

Do you apply any basal-stress scaling at the grounding line? If you are using a coarse resolution, scaling basal stress at the grounding line has shown to help to simulate grounding line migration in agreement with high resolution. Actually, this could help to simulate more surges potentially. Do you apply any melt at the grounding line?

Section 2.6

I do not understand your MNEE method and this sentence is confusing to me, please rephrase to make it clearer.

"As in Hank et al. (2023), the final MNEEs (shaded grey regions in Fig. 6) are the maximum percentage difference for the metric in question of a setup with stricter numerical convergence and a setup with 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). These MNEEs are then used as a threshold to determine if a change in model configuration leads to numerically significant differences in the surge characteristics. Differences smaller than the MNEEs should be interpreted as model response not resolvable given the numerical sensitivities."

Section 3.3

I am surprised that using a (regularized) Coulomb law leads to model crashes and a model run time much slower than a Weertman law. Do you have an explanation for this?

Section 3.6

I would try to avoid using acronyms as section titles.

Figures

As a general comment for your figures I would suggest indexing. Figure 4 for instance has 6 subplots but when you refer to it I do not know which panel I should be looking at.

Figure 4

- Please use another naming for your "surges" since notation S9 can be confused between you Figure S9 or your surge S9.
- How is it possible that you obtain the highest buttressing value when your ice shelf is small?

Figure 7, 8, 13, 15

- Put the legend in one plot, you do not need to put it in every plot.

Figure 9

- You do not reference Figure 9 in your manuscript. Do you need that figure?

Figure 10

- You do not need Figure 10 since you are already showing your reference state in Figure 12.