Author's response to Anonymous Referee 1 Comment 1

May 20, 2024

[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.]

We thank the referee 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.

To clarify, we use the GSM with a set of 20 parameter vectors (not experiments) for the base ensemble and conduct an ensemble-based sensitivity analysis in more than 40 experiments. As such, each experiment is carried out with a 20-member ensemble.

As suggested by the referee, we will reduce the amount of figures by merging plots with similar content. Where possible, the figures will be rearranged to follow a chronological reference pattern. However, breaking this pattern will be required in some instances (e.g., when highlighting details of figures in the methods section (e.g., line 273)).

The choice of ensemble parameters and parameter ranges within this study is based on extensive model development and testing [Tarasov and Peltier, 1999, 2007, Pollard and DeConto, 2012, Drew and Tarasov, 2023, Hank et al., 2023, Tarasov et al., 2024, in preparation]. While the parameter space is more extensive than in previous Heinrich Event modeling studies, the authors argue that it is still insufficient to fully capture the uncertainties involved. However, computational expenses restrict our ability to further increase the parameter space.

1 General Comment

1.1 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?] That is generally correct. However, we refer to the 200 simulations as *Not Ruled Out*

Yet (NROY) rather than *realistic*. The latter is imprecise, and the former better communicates the contingent nature of our inferences about past earth system evolution, as well as a reminder that explicit criteria are being used for this designation. Furthermore, as stated in the text, the 20 runs were hand-picked to provide 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 choice - 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.] Relying on only one climatology neglects the uncertainties associated with the climatic forcings during the last glacial cycle. Otherwise, how does one justify arbitrarily chosen climate forcing given the uncertainty in glacial cycle climate? And how does one know if their results have any validity for a different climate history? To (albeit partly) account for these uncertainties, our analysis is based on ensemble results. 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.

[Parameter choice - 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.] The reviewer does not seem to appreciate that the ensemble parameters in the GSM are effectively in all paleo ice sheet models not coupled to an EMIC more advanced than the EBM (energy balance climate model) in the GSM. Most models implicitly set most of the parameters in the model to a fixed value, but that does not make the uncertainty associated with that parameter go away. For those ISMs that are coupled to a moderate to advanced complexity EMIC, there will be plenty of new EMIC parameters to replace the climate-forcing parameters in the GSM.

While investigating the parameter space is not the main aim of this paper, Table S1 intends to show the parameter ranges used within this study.

[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.] While we agree that using the same parameter vector might be less confusing, it is not straightforward to determine one base vector. Different parameter vectors respond differently to changes in model configuration. Generally, we plot the parameter vector that best resembles the ensemble response for a specific change in model configuration. However, for every time series (single vector) plot, we also show an ensemble mean plot. The single vector plots are intended to give a physically self-consistent example (which, e.g., an ensemble mean does not provide). On the other hand, the ensemble means and standard deviations provide more robust statistical results for the ensemble response to the change in model configuration.

[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.] The revised draft will include a brief description of the basal melt rate (which is simply from applying conservation of energy to the ice sheet). However, we are confused by the reviewer's question. The basal meltwater from the ice sheet is fed directly into the basal hydrology model, and we cannot envision what else could be done. Furthermore, the GSM is configured as all ice sheet models not fully coupled to a dynamical ocean model: all meltwater that is not refrozen leaves the ice sheet, and therefore mass is not conserved. The GSM internally does conserve mass: integrated: accumulation − melt + refreezing − calving = change in ice mass.

[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.] The LGM ice sheet configuration differs between parameter vectors. A plot showing the ice volume of all parameter vectors when run in the reference configuration, along with MIS 3 and LGM example timeslice map plots, will be added to the revised draft. However, a detailed comparison of the LGM ice sheet configuration to other studies and the details of the glacial forcing index are not the primary goal of this study and will be addressed in manuscripts currently in preparation.

1.2 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.] While we agree that the Supplementary Material is quite long, it provides valuable information for readers interested in specific details of the study. However, reducing its length by merging figures with similar content is possible and will be applied in the revised draft.

[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] We will re-order and divide the ensemble parameters according to the suggested process category approach.

2 Technical comments

[Section 2.3 - 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?] 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.

Submarine basal melt is not applied to the grounding line grid cell. Sub-marine face melt is applied at the grounding line if it is a tidewater outlet with an exposed (i.e., no ice shelf) calving face.

[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?] 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).

[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?] We thank the referee for highlighting this issue. The naming of the surges will be adjusted in the revised draft.

The plotted buttressing time series is given as a fraction of the grounding line longitudinal stress. Therefore, a small ice shelf can lead to large buttressing when the grounding line longitudinal stress is small. We will adjust the y-label in the plot to make this clearer.

[Figure 7, 8, 13, 15 - Put the legend in one plot, you do not need to put it in every plot.] We find interpretation easier when the legend is in each plot. However, we will defer to the editor or Journal staff for guidance on which option is preferred.

References

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