Author's response to Jorge Alvarez-Solas's Comment 1

June 28, 2024

[In this article, Hank and Tarasov, perform several simulations of the Laurentide ice sheet of the last glacial period in order to elucidate the most likely candidates to explain the origin of Heinrich events. The subject and the way it is addressed in the introduction and discussion are of very high relevance. The paper is potentially very well suited for publication in Climate of the Past. I see, however, two major deficiencies of the current manuscript: A questionable and irreproducible experimental setup and the lack of adequate conclusions. I expand these aspects in the following.]

We thank the referee Jorge Alvarez-Solas for their constructive comments. A point-by-point reply is reported below, with referee comments in orange and our replies in black. We address the claimed *two major deficiencies*, which was likely due to a misunderstanding of the experimental design in our detailed responses below.

1 General comments regarding the experimental setup and the exploration of the parameter space:

[Your experimental setup is firstly very hard to understand and secondly not reproducible at all. It is hardly understandable because your entire exploration of the parameter space relies on perturbing the values of the parameters of your table S1. However the majority of these parameters are not explained at all in the manuscript. The very pertinent questions you are addressing in this article are conveniently described at the end of the Introduction paragraph. They summarize the different mechanisms triggering Hudson Strait ice surges in the context of HEs. However, it is difficult to understand how exploring, for example, the values of the "northwestern desert-elevation cutoff" is relevant in this context. The equations on which these not-defined parameters apply are not present in the manuscript, so the reader can not even guess what influence they have on the scientific questions you are addressing. This is the case because the exploration of these parameters only helps to see whether a given realization of the model has fulfilled or not your sieves, but does not give any mechanistic picture of their influence on ice stream behavior. Nor these experiments can be reproduced with other models of similar physics. An article should be as shelf-contained as possible, so if you analyze an ensemble of simulations that have fulfilled a given sieve, the reader can understand why that is the case and the implications for your conclusions without having to open the experimental setup of another manuscript or guess that it is going to be more deeply explained in another article in preparation.] and [In my opinion, both these two problems would be solved by the following strategy: You could fix several of the parameters that have already allowed you to fulfill the sieves and that are mechanistically irrelevant for the ice stream behavior to a single documented value. The equations on which these parameters appear could be summarized in an appendix. Then, you could systematically explore the parameter space of the processes that are suspected to be very influential on the ice-stream dynamics (e.g. sliding laws, effective pressure, dependence on bed type and base thermal state…; see below). And finally you could describe how these different values affect the nature of the simulated Hudson Strait ice stream variability] The reviewer apparently does not understand that our experimental design is precisely what they suggest: "fix several of the parameters"; however, we do this twenty times over our 20 base parameter vectors for all 52 parameter vector components. Upon re-reading our section 2.2, we can now see that the text would benefit from more clarity that we use the same 20 GSM parameter vectors for all experiments (to do otherwise would make no sense).

This clarification should address the reviewer's concerns. However, in case they were clear on the experimental design and still have issues with the use of an ensemble, we add the following points.

A flaw in the reviewer's reasoning is their presumption that most of the ensemble parameters are apriori "mechanistically irrelevant". Thermodynamic surge cycling will depend on energy balance, which depends on surface temperature, precipitation, and ice thickness. As such, most of the ensemble parameters related to climate forcing (the majority of our ensemble parameters) are potentially relevant. This also holds for the remaining ensemble parameters related to GIA, mass-balance processes, ice deformation, and basal drag. The key point of our ensemble design is to address parametric uncertainties. This is done by extracting a high-variance sample of non-implausible model configurations (each specified by a parameter vector) and then conducting sensitivity experiments (with respect to inputs or process suppression, not ensemble parameters) on this same sample of parameter vectors. Otherwise, it would remain unclear whether the results obtained through a set of sensitivity experiments are only valid for a single chosen single configuration (i.e., a single fixed parameter vector).

As to *"An article should be as self-contained as possible"*, this would entail every modelling paper describing every parameter in the model, every equation, and every discretization. We will add more overall description of the ensemble parameters, but it is unreasonable to expect a detailed description of each parameter (which would extremely bloat every GCM-based modelling paper). Furthermore, a GSM description will be submitted to GMD before submission of our revisions.

In summary, and as already indicated in our responses to the other referee comments, the goal of this study is not to explore the effect of ensemble parameters on HEs. Instead, the ensemble-based approach aims to (albeit incompletely) account for the uncertainties associated with these ensemble parameters (ranges in Table S1). In comparison, other modelling studies often set the values of model parameters comparable to the ones presented in Table S1 to a single value and, therefore, completely ignore the associated parametric uncertainties.

2 Specific comments regarding the experimental setup and the exploration of the parameter space:

[Have you explored different values of $n_{\mathrm{b,hard}}$? I assume not (from what is shown in table S1). If not, why not simply use the same value as for $n_{\mathrm{b,soft}}$? That will reduce one degree of freedom.]

The hard-bedded sliding exponent has not been explored in this context. Based on our reading of the literature, we judge the form of the soft bedded sliding as less constrained with more potential impact on results given the generally lower basal drag and higher fluxes.

[According to equations (2) and (3) your sliding coefficient, C_b is a function of the bedrock type (soft sediments vs hard), the thermal character of the base and the effective pressure. Why do you need additionally to change the exponent of the sliding law over soft and hard bedrocks?]

Weertman's reasoning of controlling obstacle sizes places the hard bed exponent at 2 or 3. However, based on the results of a basal drag inversion for Greenland [Maier et al., 2021], a value of 4 appears to be more representative (at least for Greenland). Soft bedded sliding at small scales is likely Coulomb plastic, but the appropriate form at large scale has been subject to long ongoing debate. Therefore, there is no reason to assume that the exponent should be the same for both hard and soft beds.

[I deduce from table S1 that your maximum allowed value for $h_{\rm wb,Crit}$ is 1.0. Why limit it to that relatively low value? What happens if you significantly increase it?] and [Additionally, what does the model do if $h_{\rm wb}$ reaches $h_{\rm wb,Crit}$? Neff could not be reduced further (it would be purely 0!! From equation (4)), but what happens with the excess of heat? Can you create additional basal water? If not, could you diagnose the "free" values of $h_{\rm wb}$? How sensitive are the surging cycles to this basal water limit?] To clarify, $h_{\rm wb,Crit}$ is an estimated effective bed roughness scale, not the maximum basal water thickness. The maximum basal water thickness is $h_{\rm wb,max} = 10$ m. All additional sub-glacial meltwater ($h_{\rm wb} > 10$ m) leaves the ice sheet. We will re-do a set of simulations with the 10 m limit raised to 100 m to examine what impact this has.

While it is true that $N_{\rm eff}$ in Eq. 4 can reach 0 kPa, the addition of $N_{\rm eff,min} = 10$ kPa in the denominator of Eq. 3 enforces that the effective pressure used to determine the basal sliding coefficient C_b never falls below $N_{\rm eff,min}$.

Eq. 4 and its exponent 3.5 are based on the work of Flowers [2000]. The parameter range for $h_{\rm wb,Crit}$ expands around the $h_{\rm wb,Crit} = 0.1$ m value used by Flowers and Clarke [2002].

The effect of the local basal hydrology model and its parameters (e.g., $h_{\rm wb,max}$, $h_{\rm wb,Crit}$, $N_{\rm eff,min}$) on surges has also been extensively examined in previous studies [Drew and Tarasov, 2023, Hank et al., 2023]. We will clarify all of the above in the revised manuscript.

[You introduce a Coulomb dragging law in equations (5a) and (5b). How is C_c variable? In space or in time? According to what? (not deducible from the manuscript or table S1)] C_c is a scalar (does not vary in space or time) ensemble parameter.

[The necessity of equation (6) is justified in the following manner: " To account for possible Weertman-type sliding when Coulomb drag is high ..." A high Coulomb drag will potentially stabilize the flow and prevent the appearance of a limit cycle. Why convoluting a Weertman-type sliding law and a Coulomb one? What is the effect of using the "pure" Coulomb law (without limiting τ_b ad hoc) on the surges?] We will try to better convey the reasoning in the revised manuscript. To our knowledge, the use of the minimum of the two computed basal drags was first posed and motivated in a glaciological context by Tsai et al. [2015]. In part the motivation is: if Coulomb friction is high, Weertman-type enhanced deformation around controlling obstacles can still occur (especially given the physical separation of the Coulomb plastic deformation process within the till layer) and dominate the basal sliding. We do not follow the reviewer's reasoning on how a high Coulomb drag could stabilize the flow given the thermo-mechanical coupling and further non-linearity introduced by basal hydrology. Concretely, high Coulomb drag in Hudson Strait would not stay high as basal meltwater accumulates.

3 GHF reconstructions:

[Illustrating the dependence of your results to different GHF values seems completely adequate. However, as described above, spontaneous cycling ice stream behavior seems to appear only (given aside other questionable choices of your basal sliding laws) for the lower limit of available constraints.] and [Blackwell and Richards, 2004 is just a map without any reference to a published peer-reviewed work. Shapiro and Ritzwoller, 2004 show values around 55 mW/m^2 with a standard deviation around 20 mW/m^2. The minimum value for the whole region in Pollack 1993 is higher than 45 mW/m^2 and the Hudson Bay and Strait regions would show a mean value closer to 60 mW/m^2. Goutorbe et al., 2011 showed a Hudson Bay around 40-45 mW/m^2 and higher values in Hudson Strait for their first method, and a regional mean around 50 mW/m^2 for the same region with some hot spots of more than 70 mW/m^2. Lucazeau, 2019 reconstructed values in Hudson Bay are around 50 mW/m^2 in their first method and around 40 mW/m^2 in the other two methods, while they show significantly higher values (around 60 mW/m^2) for the three methods in the Hudson Strait Area. Finally, as far as I could see, Cuesta-Valero et al., 2021 do not show any reconstruction of geothermal heat flow nor seems the intention of their paper] The map of Blackwell and Richards [2004] was published by the American Association of Petroleum Geologists (AAPG) in 2004. While there is no peer-reviewed article associated with this map, other studies show a similarly low GHF in and around Hudson Strait and Hudson Bay with a negative northward trend [Jessop and Judge, 1971, Levy et al., 2010, Jaupart et al., 2014]. The GHF in Hudson Strait and Hudson Bay presented by Shapiro and Ritzwoller [2004] varies between 40 and 50 mW/m^2, not 55 mW/m^2. This, along with the approximate 20 mW/m^2 standard deviation, implies there is a 18% chance of the GHF being 20 mW/m^2. The mean Hudson Strait/Hudson Bay GHF in Pollack et al. [1993] is 56.1 mW/m^2. The values in Lucazeau [2019] are difficult to interpret towards the lower end because all values below 45 mW/m^2 have the same colour. The reference to Cuesta-Valero et al. [2021] was added to show the sparsity of geothermal borehole data in Hudson Strait and Hudson Bay. This can also be seen by browsing the IHFC Global Heat Flow Database [e.g., Fuchs et al., 2023]. The key point of this discussion is that the GHF for Hudson Bay is highly uncertain.

[Line 345 reads: "The exact transition point [to the binge-purge mode] depends on the parameter vector in question but generally requires a Hudson Strait/Hudson Bay GHF ave $\leq$ 37 mW/m^2. And line 357 states: "Therefore both types of Hudson Strait ice stream surge cycling are consistent within available GHF constraints."] and [In light of the reconstructed values described in your referenced studies, this last sentence seems highly inaccurate or simply wrong. It should rather say something like: "The binge-purge mode is, under our experimental setup, only accessible if GHF¡37 mW/m^2 which represents the lower bound of available constraints".] The additional references above [Jessop and Judge, 1971, Levy et al., 2010, Jaupart et al., 2014] indicate that some GHF estimates in and around Hudson Strait and Hudson Bay are indeed as low as 20 mW/m^2 (as stated in the manuscript). We will add these references to the revised manuscript and, therefore, stand by our current statement.

4 Conclusions of the manuscript in light of the experimental setup

[What is the influence of $h_{\text{wb,Crit}}$ on the transition to binge-purge? Would this transition be possible if basal water is conserved?] As mentioned above, the influence of $h_{\text{wb,Crit}}$ has been examined in previous studies [Drew and Tarasov, 2023, Hank et al., 2023]. In general, the model results show only a small sensitivity to a change in $h_{\text{wb,Crit}}$.

[Your basal friction laws are designed in a way that effective pressure can be reduced several orders of magnitude (even to purely 0!) when there's enough basal water. Also, C_b will accordingly reach extremely high values (equation (3)) when effective pressure drops. Do you think this is glaciologically realistic (as opposed to Leguy's parameterization, for example, or even the PISM treatment?] As outlined previously, we ensure that the effective pressure used to calculate C_b never falls below $N_{\text{eff,min}} = 10$ kPa (Eq. 3). Eq. 3 further states that the multiplicative effective pressure term in the applied basal sliding coefficient is limited to a value of 10. Therefore, we consider the sliding law used within this study as glaciologically realistic or at least as realistic as other implementations.

[The regularized Coulomb law is thought to perform well, even capturing the heterogeneous character of the bed without needing to guess different friction coefficients in soft and hard beds (Joughin et al., 2019). In section 3.3 it is stated that the majority of the Coulomb runs crashed. And that it significantly increased the needed computational time for those who survived. Why is this? Could it be because of equation (6)? Or because of the additional complexity of the parameterizations over the friction coefficient and effective pressure introduced in equations (2), (3) and 4? Again, what would be the effect of letting the Coulomb law do its job, without limiting τ_b?] Joughin et al. (2019) only examined Pine Island Glacier. Maier et al. [2021] also found major sectors of Greenland adhere to Weertman-type hard bed basal drag as compared to a hard bed with cavitation Mohr-Coulomb-like law. Therefore, we see no basis for the claim

"The regularized Coulomb law is thought to perform well ..." for our context. Since we have already partly addressed this issue in our response to the comment of referee 1, we will restate this reply here: *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).*

[Runs that are subjected to severe numeric problems can often illustrate that the physical problem is not well-posed. Can you discard this is what is happening here? How many of your reference ensemble runs crashed?] Ice stream surge cycling in itself is a highly non-linear physical mechanism. Therefore, we expect some runs to crash, especially since we are also probing a large parameter space. However, one of the criteria for the runs in the reference ensemble was a successful completion (no reference ensemble runs crashed).

Since we slightly vary the model configuration for each experiment, we can not guarantee a successful completion for all parameter vectors for all experiments. However, only runs that did not crash are included in the analysis. We will clarify this in the revised manuscript.

[Looking at figure 5, the reader might notice that the upper limit of the basal velocities bar is 40,000 m/yr. It is not known whether the simulated velocities ever approach that value, because the manuscript does not contain the time series of velocities over a whole ice stream cycle, but I imagine that they are not far from the upper limit of the depicted scale. From figure 5 it is clear that they reach up to 20,000 m/yr for several hundred of kilometers upstream of the grounding line. Current observed surface velocities in Antarctica do not go beyond 1,500 m/yr downstream of the grounding line. Do you think your simulated velocities are realistic?] The upper limit of the basal velocities colorbar is set to 40 km/yr because it is the upper bound on the SSA ice velocities imposed in the GSM. While observed velocities of surging glaciers can reach several hundreds of meters per day for short periods [K.M. Cuffey and W.S.B. Paterson., 2010, , e.g., 100 m/d = 36.5 km/yr], we do not have a clear present-day analog for Hudson Strait ice stream. Can the referee confidently ascert such a velocity was never reached in the past? We can not. However, we agree it is important to document maximum velocities in the experiments and will add that documentation to the revised manuscript.

[All in all, unless the authors show otherwise, their experimental setup is constructed in a way that the relevant-for-the-problem parameter space (and thus the phase space of the associated physical problem) is very narrow and situated in a very specific region.] We are unsure what the referee is referring to here. If the referee is arguing about the limited parameter space, given that all previous studies for this context outside of our own research group have used a much more limited parameter space, on what basis should they have been published? To reiterate, we sample 52 input parameters over wide parameter ranges based on extensive history-matching results. While others might not have such a large explicit parameter space, given system uncertainties, it is still implicitly there if one is actually trying to make meaningful inferences about Hudson Strait ice stream surge cycling. Additionally, we included the *bounding experiments* (or end member scenarios, Sec. 2.5.3) to bound the effects of the ocean forcing experiments and increase confidence in our model results. Furthermore, our conclusions generally hold for near-continuous ice streaming with occasional shutdowns and subsequent surge onset overshoot ($\mathrm{GHF_{ave}} > 37\,\mathrm{mW\,m^{-2}}$) and the classic binge-purge surge cycling ($\mathrm{GHF_{ave}} \leq 37\,\mathrm{mW\,m^{-2}}$). As we examine various sliding laws and as it has been previously shown that in a Hudson Strait ice stream context *sub-glacial hydrology matters but the process details mostly do not* [Hank et al., 2023, Drew and Tarasov, 2023], we do not see why the relevant-for-the-problem parameter space would be very narrow and situated in a very specific region, especially in comparison to previous studies.

[In the absence of a rigorous illustration of the influence of the particular assumptions of the experimental setup (basal sliding laws, basal water, effective pressure, dependence of the friction coefficient on the nature and temperature of the bed, numerical issues...) on the mechanisms favouring spontaneous ice stream cycling, the conclusions reached here are based on the analysis of a very particular experimental setup. (Which in my opinion is, by construction, prompt to oscillate in a questionable physical manner).] If the above is the standard required to credibly examine Hudson Strait ice surging, then no paper to date on the subject should have been published. Our aim is to bound the possible behaviour within the full system complexity of the North American ice sheet. We, therefore, intentionally use an ensemble approach to test the relative role and robustness of key surge cycling mechanisms/hypotheses given the uncertainties in climate forcing, basal drag, basal hydrology, and such. These are uncertainties that previous studies have largely ignored. Based on the reviewer's reasoning, it is fine to ignore uncertainties, but if you try to address them, then you have to isolate and analyze each contribution. We strongly disagree.

As mentioned previously, the effects of basal water, effective pressure, dependence of the friction coefficient on the nature and temperature of the bed, and model numerics have been extensively studied by Hank et al. [2023] and Drew and Tarasov [2022]. Although limited by the number of successful runs, the effect of different sliding laws is examined in this study.

[And this leads me to my next point: What happens if you run the Antarctic ice sheet under such a configuration?] and [To answer this question, you would need to make some assumptions concerning the bed type. One reasonable choice would be to assume the presence of soft sediments in every Antarctic marine sector. Do you expect that such a simulation would give reasonable results?] and [In my modelling experience, this is unlikely.] and [GRISLI and Yelmo can also show spontaneous ice stream oscillations under extreme conditions (e.g, limiting basal water and having very distinct spatial basal frictions). But when you apply such a physics to Antarctica, it becomes very difficult to approach observed velocity values, and furthermore some ice streams become very noisy and others dramatically oscillate in a manner that so far is not observed in Antarctica.] and [Therefore, I believe the current experimental setup extremely conditions the current conclusions of the paper.] An initial paper examining GSM history-matching results for Antarctica is currently under open review in egusphere [Lecavalier and Tarasov, 2024]. The ensemble parameters in the NROY (Not Ruled Out Yet) set for Antarctica bracket, on both ends, the soft and hard bed sliding parameters for our 20 base parameter vectors used here. Moreover, the Antarctic configuration assumes soft sediments in marine sectors (at least when marine after full unloading) with possible pinning points according to available DEMs. Before submitting revisions, we will check if the Antarctic simulations have the unstable ice streaming the referee describes. The caveat is that Antarctica was history-matched before the inclusion of basal hydrology. Future work with Antarctica will include the updated configuration with basal hydrology. What the reviewer may not be aware of is that the GSM configuration is informed by modelling of all last glacial cycle major ice sheets as well as major ice caps (or minor ice sheets) such as Icelandic and Patagonian.

[The introduction section nicely ends with the presentation of very relevant questions regarding the different mechanisms exciting the Hudson Strait ice stream variability during HEs. The discussion section also nicely re-addresses those questions (see some minor comments below) in the context of the new results shown in the manuscript. The conclusions, however, do not fairly summarize the findings and are biased towards the spontaneous ice-stream cycling mechanism.] and [After everything that is shown in the paper, and given all the limitations of the experimental setup pointed out in this review, concluding that "Based on our results, Hudson Strait ice stream surge cycling is the most likely Heinrich Event mechanism..." seems unjustified.] and [The questions Q2, Q3 and Q4 of your discussion are compatible with each other. When answering Q3 you state that your maximum simulated ice shelf (leaving aside the one with inhibited calving) is close to the minimum required by Hulbe 2004 to explain the IRD signal. So, can you discard that a combination of Q2, Q3 and Q4 produces both the icebergs from breaking the ice shelves and the increase in the flux at the grounding line necessary for explaining HEs?] We thank the referee for raising this point and will discuss a combined mechanism of Q2, Q3, and Q4 (sub-surface ocean warming leads to the breakup of fringing ice shelves and, consequently, a sudden reduction of the buttressing effect) in the revised conclusions. Since the mean Labrador Sea ice shelf volume is significantly smaller than the maximum volume (Fig. 1; and, therefore, the minimum required by Hulbe et al. [2004]), and since the fringing ice shelves in front of Hudson Strait provide only minor buttressing, our results indicate that even when considering a combined mechanism, HE can not be explained without Hudson Strait ice stream surge cycling. However, as stated in the conclusions, even the small changes associated with sub-surface ocean warming *can affect the timing of surges and provide a means to synchronize HEs with the coldest phases of the Bond cycles.*

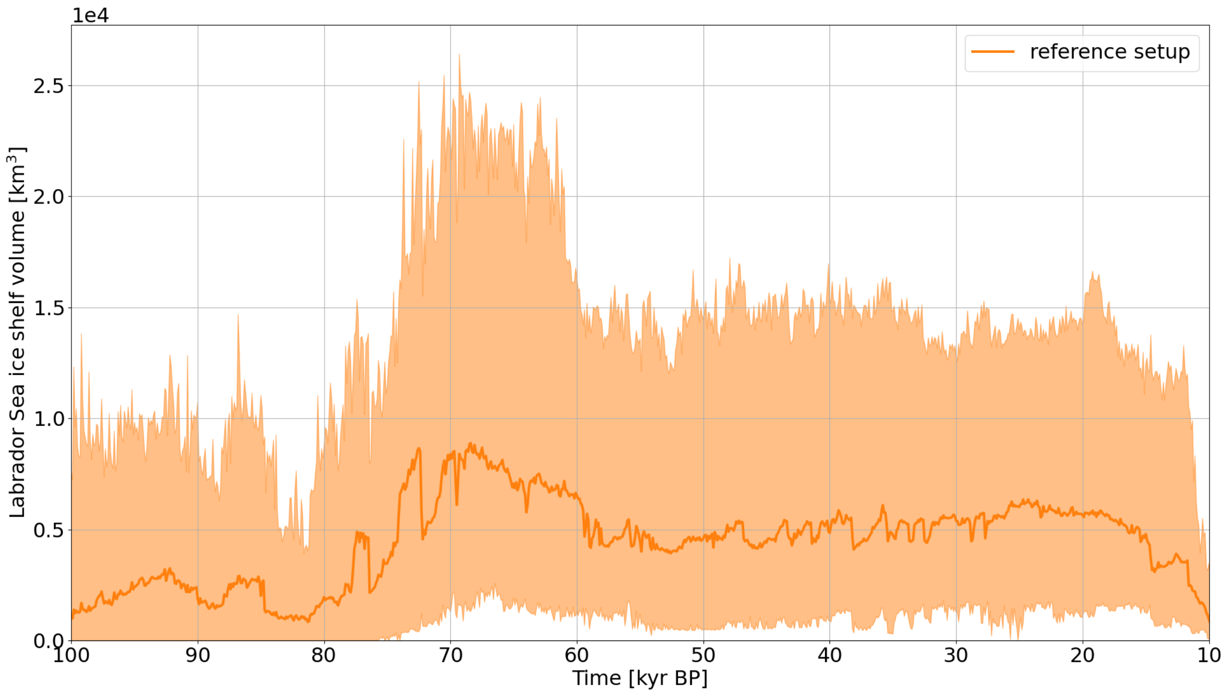

Figure 1: Labrador Sea ice shelf volume in the *Labrador Sea ice shelf area* outlined in Fig. 2. The thick line represents the mean of the 20 run ensemble. The shaded area marks the minimum and maximum of the ensemble.

[Under a questionable and very particular experimental setup, your simulations show that you have to go to the very low bound of Geothermal heat forcing for the ice stream surge cycling to emerge. But, even assuming that a binge-purge like oscillation is a good candidate to explain HEs, there is a remaining puzzling question that you completely ignored. Why are HEs happening at the middle of the cold NH phases, or stadials? Some synchronization mechanisms have been explored in the literature to potentially answer this question (.e.g. the work of Calov and Ganopolski and Shanwell et al., 2024) but you do not inform whether these synchronization mechanisms are captured in your simulations. So, under your experimental setup, the question of HEs occurrence during the surface NH cold phases is still of concern for your conclusions (see also Barker et al., 2015; Nature). Therefore, the authors seem to be evaluating certain hypotheses with a level of criticism and rigour not present in the case of their announced more likely mechanism.] While a low GHF (but as we have argued above, not extremally low given available literature) is essential for the classic binge-purge surge mechanism, it is not a requirement for the second ice stream surge cycling mode discussed in this study (near-continuous ice streaming with occasional shutdowns and subsequent surge onset overshoot). As stated in the manuscript: *ocean forcings can affect the timing of surges and provide a means to synchronize HEs with the coldest phases of the Bond cycles.* However, we will expand on this in the revised draft, including a comparison with the recently published results of Schannwell et al. [2024, not published at the time of manuscript submission]. Additionally, we will clarify that the issue of HE synchronization with the coldest phases of the Bond cycles (though not always the case, e.g., HE1) is the least resolved issue from our experiments.

[You discard the ice-shelf breakup related hypothesis (Q2) because your simulated ice shelves are not big enough to significantly buttress the Hudson strait ice stream and because the paleo reconstructions of the Labrador Sea conditions during MIS3 do not seem compatible wit the existence of a big ice shelf in the Area. This is a fair criticism, but I would like to point out here that in GRISLI and Yelmo (both codes are available; the first upon request to the former developer, Catherine Ritz or myself, and the second here: https://github.com/palma-ice/yelmo) the emergence of a big Labrador Sea ice shelf is a pretty natural characteristic provided oceanic temperatures are low enough. Even with a relatively warm ocean, Yelmo simulates the existence of very developed fringing ice shelves around the mouth of Hudson Strait (Moreno_parada et al., 2023; The Cryosphere).] The paleoceanographic constraints and oceanic temperatures being low enough are the crux. If temperatures are low enough, it is not surprising to have Baffin Bay covered by an ice shelf along with the confined part of the Labrador Sea, especially if one considers the comparative area of say the inferred LGM Ronne-Filchner ice shelf. However, is it reasonable to assume that conditions over the LGM Labrador Sea were similar to those of LGM Ronne-Filchner? The paleoceanographic constraints appear to rule this out. Furthermore, the mouth of Hudson Strait is at the edge of Labrador Sea confinement. As stated in our submission, the base simulations do have fringing ice shelves (including at the mouth of Hudson Strait); the critical issue is whether these are large enough to exert enough back stress to affect Hudson Strait streaming.

For our revised submission, we will review/document the Labrador Sea temperature range (based on TraCE deglacial simulation run with the Community Climate System Model Version 3 [CCSM3, Liu et al., 2009]) used in the GSM for these experiments. Depending on this range, we may test further cooling to at least document what is needed to get a pan Northern Labrador Sea ice shelf.

[I believe that referencing our paper (Alvarez-Solas et al., 2013) in lines 95 and 548 in a technical and discussing context respectively, without having it cited in the introduction, when the different hypotheses are explained, is academically incorrect. Note, we also explored the conceptual idea of triggering HEs through changes in the oceanic temperature (Alvarez-Solas et al., 2010; NatGeo), and the effects of an ice-shelf breakup during H1 (Alvarez-Solas et al, 2011, ClimPast), coetaneous with Marcott et al., 2011] We thank the referee for raising this issue and will add the references to the introduction.

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