EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-332-RC2, 2022

Review of Schannwell et al.

Alexander Robinson (Referee)

Referee comment on "Sensitivity of Heinrich-type ice-sheet surge characteristics to boundary forcing perturbations" by Clemens Schannwell et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-332-RC2, 2022

This authors present experiments performed with an ice-sheet model coupled with a GIA model over the North American domain pertaining to representative MIS-3 boundary conditions. As built, the ice sheet exhibits very large magnitude, quasi-periodic oscillations primarily in the Hudson Strait ice stream and the Mackenzie ice stream that are supposed to correspond to Heinrich Events. Along with an in-depth analysis of the dynamics of these ice surges, different purturbations in the boundary conditions are tested to assess their influence on the characteristics of the surges. The topic is interesting and open in the literature, and the contribution here with a state of the art model setup is valuable. The paper is also well written and flows nicely. Nonetheless, I believe framing of the context of these experiments needs to be refined and the credibility of the model setup producing the surges themselves needs more support.

Framing. The Abstract and Introduction is very nicely written. However, the description of Heinrich events here based on strong assumptions resulting from the authors' own modeling work. Not all characteristics are known to be true, and thus should not be presented as definitively representative of reality. This is particular of the sentence on L3 in the Abstract and the paragraph on L43-56 in the Introduction. It is well known that the AMOC collapse related to Dansgaard-Oeschger Events occurs prior to Heinrich Events in sediment records (e.g., Barker et al., 2015, http://dx.doi.org/10.1038/nature14330). In other words, glacial stadials and the dominant climatic impacts recorded globally, are mainly related to ocean circulation changes. It may be that freshwater discharge during Heinrich Events can act to extend the duration of the stadial, but this is far from clear in the paleo records. Thus, discussing all of the "impacts" of Heinrich Events is not really relevant, at best, and rather misleading at worst. I would simply recommend removing this sentence and this paragraph from the manuscript, as it is not necessary for motivating the study of Heinrich Events, which are large scale and enigmatic in any case.

Another example is the phrase "during which large amounts of ice are discharged" on L2 in the Abstract. We have no constraints from reconstructions on the total ice mass that was discharged during HEs. Some modeling studies do not show such large fluctuations in

volume (e.g. Alvarez-Solas et al., 2013), while the model the authors use does. So, again, I would suggest more precision when discussing what we know versus what comes out of this work. This will facilitate putting the results in context of the open questions on this topic, and will increase the value of the work.

Experimental setup. The model components used appear to be state of the art and include many/ most processes one would be interested in for studying this problem. However, the results exhibit ice velocity values that sound simply incredible. In the surge phase, it appears that maximum values of up to 40,000 m/yr are reached, with inland values over a distance of 2000 km of > 1,000 m/myr (see Fig. 4, bottom-center panel and Fig. 5b). This seems to me extremely implausible, and at the very least should be acknowledged as such in the text, or explicitly justified and explained.

Furthermore, I think such a result is predominantly dicated by the choices regarding basal friction, which are only described in a very cursory way here. In particular, I think the reader should see more information about how τ_c is calculated in the basal friction law, as well as the relation used to calculate effective pressure, which I assume is an input to τ_c. Rather than "basal sliding is discouraged", I think actual values should be given and any spatial maps used should be included here. The point is, not only do I think this is valuable information for understanding the nature of the surges presented here, but all information should be provided to allow others to reproduce the results.

I am also surprised by the choice of a constant value of GHF of 42 mW/m2 everywhere. In the motivation for the range of the Geo+/- experiments, the authors cite a spatial GHF map (Lucazeau, 2019) of which several exist. Why not apply such a map as the default distribution and scale from that?

Specifically with regards to Geo-, GHF values of < ~40 mW/m2, and particularly of 0 mW/m2 do not seem likely to exist in reality. The motivation of "Geo-" is therefore difficult to understand. Since a very low value of GHF is imposed by default, I would expect only a "Geo+" purturbation to be needed.

With regards to setting the imposed value of GHF to -1000 mW/m2, I am very surprised that this did not cause numerical problems for the model. It seems that the authors were able to achieve their goal of deactivating streaming, but out of curiosity, what does the vertical temperature profile of ice look like in these regions? To keep conditions within more realistic bounds, the authors may consider simply modifying the spatially-defined friction coefficient in these regions for these tests.

Finally, I think overall some additional figures would help. The readers should see some maps of the whole ice sheet being simulated in Ctrl (surface elevation, velocity, basal temperatures, τ_b) and possibly in the different phases (Quiescent, Pre-surge, Surge). For example, a supplementary movie, showing the surge behavior in the control simulation would be very valuable as well.

In summary, with a change in framing combined with additional information, this paper will be a valuable contribution to the literature on the still open question regarding the nature of Heinrich Events. Note that some additional specific comments are listed below.

Specific comments

L1 and L18: "among the most dominant" <= What makes them most dominant? Consider rephrasing simply to "prominent"

L35: More recent theories propose => A more recent theory proposes

L38: tidewater glacier => tidewater glaciers

Paragraph L43-56: As mentioned above, this paragraph gives a misleading characterization of HEs. Climate impacts are largely due to AMOC shutdown. In addition, MWP1a is a very specific event during the deglaciation (L53-56). It cannot be considered as representative of Heinrich Events in general. Thus citing it here is not really appropriate.

L74: cylce => cycle

L106: 36 ka => 36 ka ago [or before present?]

L109: bathymetrie => bathymetry

L164: Consider also citing Feldmann and Levermann (2017), as they describe this mechanism in detail with experiments using PISM.

L184-185: What about the impact greater thickness, insulating and warming the base upstream? This seems like it would also contribute significantly.

L214-215: Map of Lucazeau (2019) only goes down to values of about 40 mW/m2.

L252-253: It is hard to compare magnitude sensitivity, given that the units are totally different. Consider removing this sentence, as it is not necessary, or at least reformulating. Perhaps you can quantify the sensitivity in terms of Δt surge time as a function of percent change in boundary variable. Maybe this would allow a more appropriate comparison.

L258: sensitive => sensitively

L293: What kind of frequencies are imposed on GHF simulations? Are these not constant? Or rather this is GHF at the based of the ice sheet, which can evolve transiently, as opposed to GHF deep in the bedrock? Please clarify here more explicitly.

L298: sensitive => sensitively

L367-370: Again, this statement may be true, but it cannot be justified by the units used. Is a perturbation of +100 mm/yr of SMB equivalent to a surface temperature increase of +5 K? Find a way to rephrase, or rescale.