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Climate of the Past Discussions

Interactive comment on "Heinrich events show two-stage climate response in transient glacial simulations" *by* Florian Andreas Ziemen et al.

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Review of "Heinrich events show two-stage climate response in transient glacial simulations" by Ziemen et al, 2018.

I am sorry for the delay in finishing this review and I apologize to the authors for the derived inconvenients.

The work of Ziemen et al, analyses the effects of internally-produced ice surges of the Laurentide ice sheet on the Northern Hemisphere climate around to the LGM. It does so in a fully coupled (asynchronous) ice-sheet / climate framework. Such a modelling framework has an inherent merit and it is of sufficient interest to make this contribution worth of being published. The paper nicely analyses the effects of a freshwater injec-

tion on the North Atlantic behavior and, because being fully coupled, it also describes the impacts of such an oceanic change on the ice sheet at the same time as the show the impacts of the lowering height of the Laurentide on the Northern Hemisphere climate. This represents an important contribution under a more realistic framework when compared to the classic hosing experiments done with climate models alone. Hosing experiments have been useful in order to understand what are the consequences of a reduction of North Atlantic density (by means of prescribed freshwater fluxes) on the rest of the climate system. The current work has the advantage of providing such a flux in a physically-based maner within the context of a coupled ice-climate system.

That being said, I think there are two main assumptions in the current manuscript that need to be discussed:

1) The authors somehow assume in the discussion section that Heinrich events in the real world arise as self-sustained cyclic surges of the Hudson strait ice stream.

2) Following the logic of 1), the observed climatic changes during Heinrich stadials are interpreted to be merely the consequence of the above mentioned surges (page 1, lines 2 and 19; page 9 lines 26-32)

Regarding 1): There are in the literature relatively recent papers not cited here defending that the triggering of Heinrich events lies on an oceanic forcing (Bassis et al, Nature 2017; Alvarez-Solas et al, PNAS 2013), rather than on a binge-purge-like mechanism. Furthermore, making the ice streams of the Laurentide ice sheet oscillate in a 3D thermomechanical model is subjected to technical nuances in the way the basal movement of the ice is treated. In particular, I see one choice (inherited from the experimental setup described in Ziemen et al, 2014), that deserves further attention or at least a caveat in the manuscript. I.e. C = 1 m/yr/Pa is a very high (and likely unrealistic) value for a linear sliding law because:

a) In Calov et al, 2002 (the first to show binge-purge-like oscillations in a 3D thermomechanical ice sheet), the chosen value of C was 0.1 m/yr/Pa (10 times smaller than in the current manuscript). And then, as sensitivity tests, the effects of considering even smaller values of that parameter (until 0.01 m/yr/Pa; 100 times smaller) were discussed.

b) One could wonder what would the magnitudes of the simulated velocities in present-day Antarctica following a linear sliding law (U_b = C tau_b) with C = 1 m/yr/Pa be. Taking a look at Morlighem et al, 2013, for example, and using their inferred basal stresses (tau_b), the reader would be surprised by the resulting velocities of the antarctic ice streams, ranging from 20 to more than 100 km/yr. In fact, this approach can also be followed inversely. I.e. given the observed velocities and the inferred basal stresses, one can deduce what the values of the sliding parameter would be. So, dividing the observed velocities (U_b) by the basal stresses (tau_b) in Morliguem et al, 2013, the resulting median value of C is 0.02 m/yr/Pa. This is 50 times smaller than the one used for producing the cycling Laurentide surges in the current manuscript.

I guess (because ice velocities are not shown here) that, thanks to including the non-local SSA solution and its propagation of longitudinal stresses (as opposed to the propagation of the surface slope under the local SIA solution), the ice flow is stabilized and therefore such extremely high velocities are prevented to appear in the model. Additionally, I am aware that the realism of the cyclic Hudson strait ice streams surges produced here (called Heinrich events in the manuscript) is not the main focus of the paper. Thus, producing new ice-sheet simulations with smaller values of C is probably not necessary for the current paper. Nonetheless, what seems necessary is to acknowledge that the robustness of the glaciological mechanisms producing self-sustained Laurentide ice surges is (at least) under debate and that therefore simply calling these oscillations Heinrich events could be premature.

With respect to 2): Heinrich events always occur during cold phases of the observed millennial variability in Greenland (during stadials) but not for every stadial. A convenient explanation of the phenomenon would simply be (and has been) that the ice surges from the Laurentide trigger (or facilitate) the shift into stadials. However, more

evidence is growing pointing to the fact that icebergs in the North Atlantic appear in sediment cores significantly after the cooling of the stadials is already observed (Barker et al, 2015). This implies that the iceberg discharges from Laurentide surges are not the responsible of the observed cold phases neither during stadials nor during Heinrich stadials. This is a bit in contradiction with what the current manuscript suggests (see for example page 1 in the introduction: "... iceberg armadas spread detritus from the Hudson Strait area across the North Atlantic seafloor and caused large-scale climate changes."). The value of the simulations shown and analysed in the manuscript is not affected by what is exposed above. However, acknowledging that the chain of causes and effects explaining the observed climate features during HEs might be not as simple as previously thought (the ice sheet surges, circulation and density drop and thus the ocean cools) would, in my opinion, improve the paper.

Specific comments: Section 2.2 is not very clear to me: What is the purpose of having 3 different realisations of the model forced with the same boundary conditions and internal parameters? Is the spin-up procedure shared between exps B and C? Do they have the same initial conditions as well? If yes, why are they producing the surges at different times?

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