Dear Jorge, thank you very much for the kind comments and helpful suggestions. Below please find a point-to-point response to the comments.

I am sorry for the delay in finishing this review and I apologize to the authors for the derived inconvenience.
Thank you very much for taking your time to review this manuscript.

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 injection 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 manner 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)

We agree that we were too superficial in discussing these aspects, and therefore expanded on their discussion in the introduction (largely rewritten) and the discussion. We consider the causation of Heinrich events by an internal oscillation as the most plausible mechanism proposed so far. We are aware that an additional mechanism is needed to explain the phasing and consider the triggering by sub-surface ocean warming described in your works and  the related work of Hulbe et al. (2004) by Bassis et al. (2017) as the best explanations so far. Calov et al. (2002) have shown the possibility of having a self-sustained oscillation together with ocean triggering. We rephrased the introduction and discussion to clarify that we are aware of the discussion regarding the mechanisms of Heinrich events, and the importance of precursory climate changes (largely the effects of Dansgaard-Oeschger stadials) in the analysis of the climatic signals. We will detail on these changes below.

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.

We added:
*Also the friction coefficient controlling the basal sliding is lower than in Calov et al. (2002, 2010), partly because about 60% of the driving stress are compensated for by membrane stresses (not shown). While the value of the friction coefficient is substantially lower than values commonly obtained for Antarctica or Greenland, the geological history of the Hudson Bay area vastly differs from that of Antarctica or Greenland. This might explain for different basal conditions.*

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 Morlighem 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.

We added a paragraph to clarify this:
*The pure self-oscillating system does not explain for the occurrence of Heinrich events in Dansgaard-Oeschger stadials. Sub-surface warming ocean warming observed in proxies has been identified as a possible trigger (Moros et al., 2002) with mechanisms ranging from ocean-induced melt triggering a rapid retreat of the ice stream (Bassis et al., 2017), via repeatedly collapsing ice shelf controlling the flow speed of the Hudson Strait Ice Stream (Álvarez-Solas et al., 2011; Alvarez-Solas et al., 2013) to the whole Heinrich event being the break-up of an ice shelf (Hulbe et al., 2004 ). As the glacial ocean circulation is very stable in MPI-ESM (Klockmann et al., 2018), we cannot study the relationship between the modeled Heinrich events and Dansgaard-Oeschger cycles. Thus, this study is not meant to provide an answer on the exact mechanics behind the ice sheet collapses, but as an investigation of the consequences of an ice-sheet collapse on the climate system.*

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.

(see above) We added a paragraph to clarify this:
*The pure self-oscillating system does not explain for the occurrence of Heinrich events in Dansgaard-Oeschger stadials. Sub-surface warming ocean warming observed in proxies has been identified as a possible trigger (Moros et al., 2002) with mechanisms ranging from ocean-induced melt triggering a rapid retreat of the ice stream (Bassis et al., 2017), via repeatedly collapsing ice shelf controlling the flow speed of the Hudson Strait Ice Stream (Álvarez-Solas et al., 2011;Alvarez-Solas et al., 2013) to the whole Heinrich event being the break-up of an ice shelf (Hulbe et al., 2004). As the glacial ocean circulation is very stable in MPI-ESM (Klockmann et al., 2018), we cannot study the relationship between the modeled Heinrich events and Dansgaard-Oeschger cycles. Thus, this study is not meant to provide an answer on the exact mechanics behind the ice sheet collapses, but as an investigation of the consequences of an ice-sheet collapse on the climate system.*

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?

We added paragraphs describing the reasons for the choice of experiments and their differences.
Introduction:
Experiments:
*We chose these simulations as technically quasi-identical subset from various simulations that were performed when working on a model that is able to simulate the last deglaciation. As the simulations consumed considerable resources, we refrained from performing a dedicated ensemble, but made use of the available data.*

Composite analysis:
*The mechanisms related to the surging of the ice sheet are highly non-linear, leading to variability between individual realizations of the modeled events even under quasi-identical conditions (Soucek and Martinec, 2011). This variability is further amplified by feedbacks in the fully coupled ice sheet–climate model. To reduce the influence of variability and thus obtain more robust results, we perform all further analysis on a composite of all four events.*

We expanded the Discussion:
*The surges show a very similar peak discharge rate. This is most likely set by the geometry of the Hudson strait limiting the flow. Despite this, they are surprisingly dissimilar. ExB and ExC are intialized shortly before the surge, and are virtually identical until the beginning of the surge (Fig. 1b)). Then, however, their evolution diverges due to the extreme nonlinearity of the processes involved in the surge with switching between fast sliding and non-sliding basal conditions. The similarity in basic shape and peak discharge as well as the differences between individual realizations resulting from the non-linearities are in perfect agreement with idealized studies (Calov et al., 2010; Soucek and Martinec, 2011) as well as Roberts et al. (2016).*

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