Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-143-RC1, 2018
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Interactive comment on "Oceanic forcing of the Eurasian Ice Sheet on millennial time scales during the Last Glacial Period" *by* Jorge Alvarez-Solas et al.

Anonymous Referee #1

Received and published: 29 January 2018

The study of Alvarez-Solas et al. is well written and of high scientific quality. Using an ice sheet model forced by idealised ocean and atmosphere climates through the last glacial period the authors investigate the response of the European Ice Sheet to millennial scale climate changes. A major finding of the study is that the European Ice Sheet, and in particular fast flowing ice in the vicinity of Bjørnøya trough in the Barents Sea, is highly sensitive to changes in ocean conditions with a minor contribution from changes in atmospheric surface mass balance.

The paper clearly merits publication in climate of the past as it provides important insight into the dynamics of Dansgaard-Oeschger events and the interplay of the ocean

with marine terminating ice sheets of Eurasia. As opposed to previous studies focusing on the dynamics of the Laurentide Ice Sheet this work is novel in providing clues to the contrasting role of the Eurasian ice sheet to these millennial scale climate events of the last glacial. This said, there are a few important comments which have to be addressed before the manuscript can be accepted.

GENERAL COMMENTS: The main conclusion of the work is that the response of the Eurasian Ice Sheet (EIS) during the last glacial period is dominated by changes to the ocean forcing in the Nordic Seas and Arctic Ocean. The study includes a sensitivity study testing the tuning factor used in controlling the ocean melting of ice. However, there is no assessment of the atmospheric forcing of the ice sheet as given by the model. In particular, what is the potential impact of different SMB parameterisations, different atmospheric climate realisations, and the potential impact of ablation on sub-glacial and basal and submarine melt (e.g. Bondzio et al., GRL, 2017).

As stated in the manuscript, the authors use the PDD method to calculate surface ablation. However, the validity of the associated tuning parameters of this parameterisation for the LGM and stadial/interstadial climates investigated is not assessed or discussed. This analysis must be included in order to properly assess the relative role of atmospheric and oceanic forcing.

The climate forcing used is not clearly assessed either. E.g. how do the MIS3 and stadial-interstadial climate changes from the model applied (figure 3) correspond to observations?

Also, the authors chose to apply SSTs as ocean forcing, whereas the ice shelves off the EIS reach a considerable depth (authors state 500m) where ocean temperatures will be significantly different from the surface. The authors should therefore assess the impact of using sub-surface ocean temperatures instead of SSTs for their results. Note that in previous studies by the authors (e.g. Alvarez-Solas et al., CP, 2011), the sub-surface ocean temperature is used as a forcing. Although it is postulated in the manuscript that sub-surface temperature is more correct for Laurentide type ice shelves, this has to be tested and thoroughly discussed.

In the discussion the authors briefly comment on the difference between freshwater and IRD. This needs further detail. It would be an advantage to include a plot of the amount of meltwater released so as to compare it with the calving. I.e. how much discharge is calving versus melting and what are the different timescales of the two components of the mass balance? This is important in assessing the potential impact on ocean circulation and sea ice.

Basal friction

SPECIFIC COMMENTS:

Page 2, Line 34-35: stated here that IRD peaks found at end of stadials (with reference to von Kreveld et al., 2000) - OBS! This requires an extremely well dated chronology or tephra to assess the phasing between ocean and ice cores - i.e. such inferences relating to timing are extremely important and must be documented carefully.

Page 5, Line 1-5: The forcing is composed of a difference between glacial and present day temperature (and Precip etc) with reference to CLIMBER model runs. However, in figures there is a reference to MIS3 (e.g. fig.1). The use of LGM vs MIS3, stadial and interstadial climate states should be better defined. E.e. what is the stadial mode? The same as LGM from CLIMBER? Also, the interstadial model (with intensified NADW) must be documented - why is NADW stronger and what are key differences with LGM/stadial?

Page 5, Line 27-30: The spin-up of the ice sheet model needs to be clearly described. This is essential for the response of the model to changes in climate forcing. What choices were made for basal friction and ice temperature, rheology etc. What was the spin-up procedure used and how does this impact the results and response of the ice sheet? All these aspects are important to document.

Figure 1: What is shown here? Annual mean values? Clearly define MIS3 as well as the stadial-interstadial experiments (see notes above).

Figure 2: The Greenland ice core index (beta) is key to all the model simulations, but is not well described in the manuscript. The index is quite different from published NGRIP d18O and temperature conversions - it should be clearly stated why the authors choose not to use the normalised ice core temperature data? Also, why is the present value of the index nearly the same as that of the stadial/glacial value?

Figure 2: the ATM experiments shown hardly any oscillations. However, previous work by the the authors as well as other studies show binge-purge like oscillations of the ice sheets given a constant forcing. Why is there no self-sustained oscillations in the version of GRISLI applied here?

Figure 2: Define the dashed lines in the figure and give details in the caption.

Figure 3: The two regions are named SW and NE. This should be clearly stated in caption of figure. However, if possible these names should be more descriptive - e.g something relating to Bjørnøyrenna would be more logical. Also define where this feature is on the map. What is yellow circle?

Figure 3: Note that the land topo in figure 1 and 3 are different. Would be better to use one land topo - or comment on why different (ice sheet vs climate model) and potential impact of this on results.

Figure 5: Not necessary to repeat formula from main text in the caption.

Figure 5: the discussion of the relationship between ice sheet height/velocity and foundling line retreat would benefit from including a discussion of changes in the position of the calving front through time. How does this differ from the grounding line and how does it relates to any of the assessed ice parameters?

TECHNICAL COMMENTS: Line 21: Better to use DO-events and H-events or similar nomenclature. D/O is not a standard form.

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