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:

1) 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).

We agree with the reviewer and thus have now studied the sensitivity of our results to the uncertainties in the atmospheric forcing by assessing the potential impact of different PDD parameters, the mentioned potential interplay between ablation and the sub-glacial state, and the response of the ice sheet to different oceanic sensitivities in terms of its dynamics (see below).

In relation with the assessment of the atmospheric forcing, the use of different atmospheric realisations is subject to the availability of climate simulations with different models for the three climate states needed: glacial (stadial), present, and interstadial. The latter is only available for a reduced number of models. For this reason we have not assessed this issue in the present study. Nevertheless, we think that an intercomparison among these should be in the scope of future work, and have mentioned it in the Discussion section: :

"The use of different atmospheric realisations is subject to the availability of climate simulations with different models for the three climate states needed: glacial (stadial), present, and interstadial. The latter is only available for a reduced number of models. This makes the assessment of this issue difficult in the present study."

Regarding the relationship between ablation and the ice dynamics, according to Bondzio et al, 2017, the displacement of the calving front is expected to cause an acceleration of ice streams (Jacobshavn in their case) both due to the direct impacts of a reduced back force and to the decrease of the viscosity near the shear margins. The first of those phenomena is well captured by GRISLI-UCM. The second one is only indirectly captured (through the reduced viscosity from warming the ice due to an increase in the strain heating caused by the acceleration). Capturing it fully would require a complete consideration of the membrane stresses which is nowadays computationally not-affordable for long simulations and only feasible with full-Stokes or ice-flow models on local problems.

Another aspect discussed in Bondzio et al. (2017) concerns the effects that an increase in the microscopic water content would have on the viscosity furtherly increasing the acceleration of the ice stream through a thermomechanical feedback. A direct computation of this phenomenon requires a sophisticated treatment of the non-linear rheology which should be in the scope of future work, in the frame of the development of a new ice-sheet model which we are carrying out in parallel (Robinson et al, in preparation).
Finally, to assess the uncertainty associated to the PDD approach (see below), the potential impact of ablation on the basal state of the ice sheet and thus on its dynamics, we performed a new ensemble of simulations considering different values of the refreezing parameter (csi) and the sensitivity to the ocean (kappa). Note that the proportion of the water produced from surface ablation that is allowed to refreeze in the ice does not directly change the viscosity in our model but the surface mass balance. Thus our ensemble is intended to illustrate the potential relationship between a larger ablation (for a given surface warming) and the dynamic response of the ice sheet to an oceanic perturbation. The analysis of this new ensemble shows, however, a negligible amplification effect from reduced (or increased) refreezing from surface waters on the dynamic behavior of the ice sheet.

This is now illustrated in figure S3. And the associated discussion has been treated in the supplementary text, and reads:

*"The potential interplay between the amount of refreezing from surface ablation and the response to an oceanic warming has also been investigated by considering different values of the refreezing parameter, **r**, and the sensitivity to the ocean, **κ**, (Figure S3).*

Note that the proportion of the water produced from surface ablation that is allowed to refreeze in the ice does not directly change the viscosity in our model but the surface mass balance. Thus our ensemble is intended to illustrate the potential relationship between a larger ablation (for a given surface warming) and the dynamic response of the ice sheet to an oceanic *perturbation. The analysis of this new ensemble shows a negligible amplification effect from reduced (or increased) refreezing from surface waters on the dynamic behavior of the ice sheet."*

2) 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.

We understand the reviewer's concern with the validity of the PDD tuning parameters. Thus, we have studied the effects of varying the main parameters of the PDD model. These (together with the refreezing parameter, see above), and the explored values range are
σ: 4,5,6 K; f_PDD_snow: 0.0015, 0.003, 0.006 mwe/PDD; f_PDD_ice: 0.004, 0.008, 0.016 mwe/PDD

This new ensemble is illustrated in figure S2. This new figure shows the millennial-scale response to different PDD realisations. The amplitude of the FIS response (characterized through the standard deviation of the time series) varies from ~2x10^14 m^3 (for low values of f_PDD_ice, f_PDD_snow and **σ**) to ~6x10^14 m^3 (for high values of the parameters. Simulations in which the oceanic component has been activated show larger amplitudes for kappa > 3 m/yr/K, thus confirming the more important role the oceanic forcing plays on millennial scales compared to the atmosphere. The discussion of this aspect has now been addressed in the supplementary text, and reads:

"*To assess the uncertainty associated to the PDD approach, the potential impact of ablation on the basal state of the ice sheet and thus on its dynamics, we performed a new ensemble of simulations"*

"Figure S2,right shows the variability of the 91 simulations exploring the uncertainty of the PDD model in terms of the standard deviation of the time series for the period 100 - 10 ky BP. This amplitude is compared with the one shown by exploring the values of $\boldsymbol{\kappa}$ *(from 1 to 9 m/yr/K) in a OCNsrf ensemble. A greater amplitude when forcing with the ocean is found from* $\boldsymbol{\kappa}$ *= 3 /m/yr/K."*

Note that the potential impact of exploring the atmospheric uncertainties in our conclusions has now been deeply assessed. So, first we slightly modulated the strength of our conclusions regarding the major role the ocean plays with respect to the atmosphere. Second, the main conclusions being still robust, the analysis concerning the uncertainty of the parameterisations (both oceanic and atmospheric) is now shown in the Supplementary Information part of the paper.

3) 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?

This issue was partially already raised in the first General Comment. As stated above, we have not carried out an analysis of the uncertainty with respect to the climate forcing. However, in a different manuscript currently under review (Banderas et al. 2017) we have assessed the performance of the method that we use to built the synthetic climate forcing for the ice-sheet model, based on the use of the three climate states: glacial, interglacial and interstadial, as compared with previous approaches. The climatic forcing time series derived from these methods (temperature, precipitation) were compared at several locations with the available proxy data: the Greenland ice-core record and a reconstruction of temperature and precipitation based on delta18O variations from speleothems located in central Europe and southwestern North America, respectively. By construction, our method provides a perfect agreement with the ice-core record, improving the performance of previous methods. For temperature in Greenland the old and new methods follow a similar evolution, as dictated by the Greenland ice-core record, but elsewhere the new method shows a larger orbital and smaller millennial-scale amplitude. For precipitation, our new method yields a very different time evolution as a result of the spatial millennial-scale anomaly pattern which successfully reproduces the phasing and timing of delta18O variability in southwestern North America on millennial time scales, a result that cannot be achieved by the old method. Finally, in terms of the extent of NH ice sheets at the LGM, our new method appears to perform best, showing the most satisfactory agreement with reconstructions: ICE-5G (Peltier, 2004) for the LIS and DATED-1 (Hughes et al. 2016) for the FIS (Figure 4c; see also the Supplementary Material of Banderas et al. 2017). Thus we think that our method provides a clear improvement with respect to previous ones.

4) 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.

We have now repeated the OCN and ALL experiments with a sub-surface forcing centered around 400-meters of depth. This new 2D-field is now included in the new figure 2 which now contains the surface oceanic field (called OCNsrf) and the subsurface oceanic field (OCNsub). According to this, panel d) of current Figure 2 shows the stadial-interstadial anomalies of the subsurface ocean temperatures. This new field has now been used to force the model again. New results are now included in all current figures.

The abstract has been accordingly modified, and now reads:

“Our results show that the EIS responds with enhanced ice discharges in phase with interstadial warming in the North Atlantic when forced with the surface of the ocean. Conversely, the discharges are found to happen both during stadials and at the beginning of the interstadials when the subsurface of the ocean is considered.”

And:

“While the atmospheric forcing alone is only able to produce modest iceberg discharges, warming of the ocean leads to higher rates of iceberg discharges as a result of relatively strong basal melting within the margins of the ice sheet.”

We have also changed accordingly the experimental setup section

“In our simulations, a large part of the NE sector of the EIS is is marine based with shallow bedrock depths between 500~m and less than 100~m in several locations further south. It is therefore unknown whether this marine ice sheet was more susceptible to changes in the surface or the subsurface of the ocean. To investigate the effect of this uncertainty we decided to perform two different simulations considering different depths: one corresponding to the surface (OCNsrf) and the other one considering deeper (subsurface) oceanic waters by averaging temperatures within the range of 550 - 1050 m depth (OCNsub). Therefore we hereafter distinguish between Delta T_mil^ocn for surface or subsurface millennial-scale temperature anomalies, respectively (Figure 2). The realism and convenience of applying one or the other is addressed in section 5.”

And several places in the results section where we described the results of applying this new forcing (please see manuscript)

5) 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.

We have now rearranged Figures 1, 2 and 3 and included new figures (Figures 4, 5, 7 and 8 of the current manuscript). Figure 5 now focuses explicitly on the time series of the different mass balance terms: released meltwater, surface mass balance, calving and basal melting.
The manuscript includes now a description of this aspect, and reads:

“The response of the EIS has been analyzed in terms of its mass balance decomposition for the all-forcing runs (Figure 5). The surface ocean temperature varies in phase with the atmosphere. Thus, during stadial-to-interstadial transitions the high negative values of dV/dt can be explained by the conjunction of an initial sharp increase in ablation together with pronounced increases in

basal melting and calving, which allow a large grounding line retreat in the Bjornoyrenna basin (Figure 5, mid panel).
The rate of ice loss by basal melting is similar to that resulting from the increase in ablation (as reflected in the surface mass balance) during the peak of a stadial-to-interstadial period. However, basal melting is much more efficient than surface mass balance in decreasing volume along the whole duration of an interstadial. This is due to the fact that ablation is restricted to the southern borders of the EIS. Thus, when the ice sheet has retreated to areas of no ablation, in spite of a slight further loss provided by the elevation feedback it rapidly equilibrates and a negative surface mass balance can not propagate further inland. In contrast, when enhanced basal melting from higher oceanic temperatures is applied, the associated retreat can propagate further inland occupying a large proportion of the Bjornoyrenna basin and facilitating high rates of volume loss (although similar in amplitude with respect to SMB) during the whole interstadial period (see the animation in the Supplementary Information)."

SPECIFIC COMMENTS:

1) 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.

We agree and have thus refrased the statement concerning the conclusions of Kreveld et al. 200). It is now stated with more caution, and reads:

"Correlating IRD occurrence with temperature changes registered in Greenland remains, however, difficult because it requires an extremely well dated chronology to assess the phasing between ocean sediments and ice cores."

2) 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 stadialmode? 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?

Figure 1 refers to MIS3 (corresponding to 40 kya BP), because this is the time period representing our background climate conditions. As stated in the manuscript this initial state is to a certain extent arbitrary and only intended to provide stable initial conditions. We then superpose millennial-scale variations. For these, we indeed consider the stadial (interstadial) state to be the weak (strong) Atlantic Meridional Overturning circulation (AMOC) state obtained with the CLIMBER3-alpha model under LGM boundary conditions. In our study the stadial state is represented by a climate simulation of the LGM with CLIMBER-3α (Montoya and

Levermann, 2008) while the interstadial mode was taken from a recent glacial transient simulation performed with the same model under glacial climatic conditions, but with intensified NADW formation (Banderas et al., 2015). This state was achieved by forcing the model with enhanced atmospheric CO2 concentration levels, which in our model is found to lead to a northward shift of deep water formation and and intensification of the AMOC.As suggested by the referee, we have expanded the definition and explanation of our climate fields intended to represent the interstadial and stadial modes, and added the following to the manuscript:

"*The key differences between these climate modes are that in the stadial, North Atlantic Deep Water (NADW) formation is relatively weak and takes place south of Iceland. Accordingly the sea-ice front in the North Atlantic reaches 40S. In the interstadial state there is a northward shift and intensification of NADW formation. Northward oceanic heat transport increases, and the North Atlantic and surrounding areas warm at the interstadial relative to the stadial state, in particular the Nordic Seas. The simulated interstadial state is thus characterised by a more vigorous AMOC, deeper convective areas together with reduced sea ice in the Nordic Seas, and a temperature increase of up to 10 K in the North Atlantic relative to the stadial state, with a maximum anomaly in the Nordic Seas (Figure 2)"*

In order to clarify this aspect we have now split the old Figure 1 in two figures. New Figure 1 includes the climatic fields used to build the background equilibrium climate state used to produce the ice sheet spinup. We also included the initial ice-sheet state in a third panel of this figure. Current Figure 2 focuses on the fields used to perturb the equilibrium state and shows anomalies in temperature and precipitation induced by the atmosphere the surface of the ocean and the subsurface.

3) 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.

Basal friction choices followed Alvarez-Solas et al, 2011, CP. A description of the rheology in the model can be found in Ritz et al, 2001. This information along with the references has now been added in the model section.

The spin-up procedure is now more clearly described in the experimental set-up section.
We have added the following to the manuscript in the Experimental Setup section:

"The ice sheet was forced with the resulting climatologies for 100 kyr previous to the starting of the perturbations described below. This allows the vertical temperature profile within the ice sheet to be equilibrated with the climate"

We also included a panel of the initial ice-sheet sate after the spin-up in current Figure 1.

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

The referee is right. The spatial components of the forcing shown in Figure 1 have been calculated at annual resolution. This has been now specified in the figure caption. The timing of MIS 3 and the stadial interstadial experiments have been properly described in the new version of the manuscript (see point 2 above).

5) 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?

The index shows only variations on millennial time-scales, since variability whose periodicity is greater than 19 kyr (the beginning of the orbital spectrum) has been removed. The reason for this is that our study attempts to asses to effect of millennial-scale variability alone during the last glacial period on the Eurasian Ice Sheet (EIS). As explained above, our experimental setup consists of an initial (i.e., control-run) MIS3 simulation corresponding to 40 kya BP achieved by choosing a value of -0.1 for our orbital index (alpha-star). To investigate the effect of millennial scale variability we then impose millennial-scale variations as represented by the beta-star index. Only by separating orbital (given by alpha-star) from millennial-scale variations (given by beta-star) variations are we able to isolate the effect of the latter in the EIS, which is our goal. The present and glacial values of the beta-star index are similar simply because at those two stages the contribution of millennial-scale is similar (and small). Finally, a transient study considering together the orbital and millennial-scale forcing would also have been possible, but this is in the scope of future work. Note that this is explained in the manuscript in the Experimental Setup section, where we state:

“Our forcing method allows to investigate the response of the EIS solely to millennial-scale climate variability at MIS 3 by keeping constant the orbital component of the forcing (\alpha^{\star} = \alpha^{\star}_{40K}) and letting \beta^{\star} vary throughout the LGP”

6) 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?

As explained by Alvarez-Solas et al. 2013 PNAS, in the hybrid (SIA +SSA version) of GRISLI (and GRISLI-UCM) there are no binge-purge-like oscillations. The inclusion of the longitudinal stresses together with the computation of the ice streams by the SSA uniquely (with no SIA sliding) makes the ice-stream behavior much more stable than in SIA-alone models (see material and methods in Alvarez-Solas et al, 2013 PNAS). Nonetheless some internal variability is still present. This is now more clearly illustrated in the time series of Figure 3.

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

Figure 2 is now improved (current Figure 3)

8) 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?

This has now been redone. We no longer focus on the SW region. Velocities shown in current figures 7, 8 and 9 correspond to the Bjørnøyrenna region of the Barents sea, that we have named Bjørnøyrenna basin and highlighted in current Figure 1.

9) 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.

Current Figures 1 and 6 show now the same topo.

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

We agree and have suppressed the formula here.

11) Figure 5: the discussion of the relationship between ice sheet height/velocity and grounding 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?

The movements of the calving front usually are accompanied by a grounding line displacement. For some minor ice-shelf breakups this close relationship can be broken, but with almost no effects upstream inland. Thus we consider that the grounding line position is the best indicator for characterizing the dynamic behavior of the marine part of the EIS. We have included this in the text to make this clear in the Results section

“Note that changes in the position of the calving front are usually accompanied by a grounding line displacement. For some minor ice-shelf breakups this close relationship can be broken, but with almost no effects upstream inland. Thus we consider that the grounding line position is the best indicator for characterizing the dynamic behavior of the marine part of the EIS.”

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

Done. We have replaced this in the manuscript everywhere.

References:

Bondzio, J. H., Morlighem, M., Seroussi, H., Kleiner, T., Rückamp, M., Mouginot, J., Moon, T., Larour, E. Y., and Humbert, A.: The mechanisms behind Jakobshavn Isbræ's acceleration and mass loss: A 3-D thermomechanical model study, Geophysical Research Letters, 44, 6252–6260, https://doi.org/10.1002/2017GL073309, 2017GL073309, 2017.

Anonymous Referee #2
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Alvarez-Solas et al. investigate the respective role of atmospheric forcing and oceanic induced sub-shelf basal melting (and refreezing?) rates in the variability of the Eurasian ice sheet during the last glacial period. The paper is scientifically very exciting as whilst a fair amount of climate records exist only little is known about glacial ice sheet variability and in particular the role of the ocean in this variability. However, I have serious doubt on the experimental setup and in particular concerning the basal melting perturbation chosen. If the authors really use an oceanic perturbation allowing for refreezing (and at a greater rate than snow accumulation!) the validity of the paper findings can be largely questioned. I suggest that the authors clarify their methodology as I will not support publication of this paper with the OCN experiment as presented.

I would recommend the authors to perform again their OCN and ALL experiment with an alternative basal melting rate perturbation (e.g. based on a ratio as for precipitation or at least with a positive threshold).

We opted for the simplest possible experimental setup, but we understand the reviewer's concerns. Thus we have followed the referee's suggestion and repeated all our main experiments (see below) limiting the potential accretion by one order of magnitude with respect to the melting, as recently suggested (Obase et al, 2017). As is shown our main conclusions still hold, in the sense that the ocean remain the major driver of ice-volume variations.

General comments

1) I am worry about your experimental setup: as it is written in the manuscript, the sub- shelf basal melting rate perturbation allows for refreezing under your ice shelves, and in huge

amount! In the Nordic seas you have a temperature difference interstadial minus stadial which is about +1 to +6 ◦ C. Your kappa is set to 5m/yr/ ◦ C in your standard OCN experiment which means that for negative beta you can easily end up with refreezing rate greater than 5 m/yr which seems completely off-scale (largely greater than snow accumulation). If refreezing is observed locally under ice shelves (due to recirculation of ice melt induced fresher waters along the ice), I think that a 40x40km shelf with more than 5 m/yr refreezing is completely unrealistic. I hope that I misinterpreted your equations, but if I am correct this is a serious flaw in your study. I may be wrong but I think that what we see in Fig 2d is due to your very large basal melting perturbation (with an amplitude st/is of more than 40 m/yr!): in the OCN experiment, your artificial refreezing allows for a rapid growth of ice shelves followed by a rapid disintegration. A side note: this is somehow rather peculiar to see that your ice sheet re-growth is actually faster than ice sheet collapse! Maybe you could show a figure showing the evolution of the spatially integrated value of the basal melt and how does this number compare with snow accumulation, ablation and calving rate. I strongly suggest the authors to use an oceanic perturbation written in a similar way to precipitation, based on a ratio of basal melting rate instead, preventing negative values.

The referee was right in pointing out that we were probably overestimating refreezing. We have accordingly changed the experimental design of the oceanic perturbation to avoid this. Following the recent paper of Obase et al. (2017), we have limited ice accretion by one order of magnitude compared to melting. The new approach is described now at the end of the experimental setup section:

"*Note that for* $\kappa\beta\Delta T < -B0$ *refreezing is allowed (B(t) < 0). Following Obase et al (2017), in order to avoid unrealistic values of ice accretion under the ice shelves, the sensitivity to the ocean is decreased by one order of magnitude when basal melting becomes negative…*"

By introducing this more realistic formulation, one can see that the peculiar behavior pointed out by the referee (a faster regrowth than collapse) has now disappeared in Figure 3. The new figure concerning the rate of volume changes for the different forcings (current Figure 4), clearly shows that during episodes of retreat its speed reaches more than 3 mm/yr, while during re-growth the rate is as much as 1 mm/yr. We have also added to the current manuscript a figure (new Figure 5) showing the evolution of the different terms of the mass balance: basal melt, surface mass balance and calving. Note that despite this change our main conclusions are not affected, in the sense that oceanic changes remain the major driver of ice volume changes. We have, nevertheless, modulated our conclusions in light of the referee's suggestion, and added the description of new Figure 4 showing the derivative of the volume in time:

"The magnitude of these changes for the MIS 3 period is illustrated in Figure \ref{fig4}. The simulation forced with the surface of the ocean (OCNsrf) and that including the rest of the forcings (OCNsrf + ATM + SL) show the largest amplitudes, with peaks of sea-level rise above 4mm~yr^{-1} during DO-events and sustained contributions well above 1~mm~yr^{-1} during entire interstadial periods. In ATM, a decline of the EIS during stadial-to-interstadial

transitions is still observed but presents a smaller amplitude of 1-2~mm~yr^{-1}. The simulations in which the ice sheet is forced with the subsurface of the ocean present a decline of their volume during stadial periods and regrowth during interstadials as a consequence of the inverted spatial pattern of temperature anomalies with respect to the surface. In the case of OCNsub the amplitude of these changes is smaller than in the OCNsrf case, on the order of 0.5-1~mm~yr^{-1}, and reaches more than 1~mm~yr^{-1} during pronounced stadials (as ca. at 44 ka BP). The OCNsub + ATM + SL simulation shows a slight volume loss during interstadials, as a consequence of the atmospheric forcing, that is superimposed onto the OCNsub behaviour."

2) - More generally, this is not clear to me if you distinguish correctly calving flux from melt. The two perturbation you applied (ATM and OCN) in the experiments impact the melt. Because you don't mention how your calving rate is calculated nor you give the extent of the ice shelves, it is difficult to quantify the respective role of melt vs. calving. Please provide the two fluxes separately in Fig. 2 and with the same units so that we can clearly measure the impact of the oceanic perturbation on your calving flux. This addition would be very useful as basal melt cannot explain the IRD concentration in marine sediment cores. Also, as mentioned in my previous comment you should show the evolution in time of your basal melting rate along with the other components that explain the ice sheet volume evolution (surface ablation, accumulation and calving).

We have followed referee's suggestion and made a new figure containing all this information, with the two fluxes given separately in the same units. The new figure (Figure 5) shows the different components of the mass balance. It shows that, thanks to the new oceanic approach (see above), rates of oceanic-induced re-growth remain lower than those induced by the surface mass balance. The comparison of the contributions is discussed in the following point.

3) - I guess that in the model it does not matter if it comes from below or from above: melt is melt. It seems to me that if you get a larger response from the oceanic perturbation it is because your perturbation is also larger, am I right? I tried to get my answer from Fig. 4 but the color scale makes difficult to read the value of the basal melt along the coastlines: about 30 m/yr for the Scandinavian ice sheet and about 4 m/yr in the Bjørnøyrenna region (where you have no surface ablation at all)? If you impose a much larger oceanic perturbation than the atmospheric perturbation it is somehow expected to get a larger response? Please discuss. More or less related to this, how you maintain unconfined ice shelves with such high values of basal melting rates?

Concerning the caveat of "a larger perturbation, a larger response", accumulation and ablation compared to basal melting are now illustrated in Figure 5. The rate of ice loss by basal melting is similar to that resulting from the surface mass balance change (from an increase in ablation) during the peak of a stadial-to-interstadial period. However, basal melting is much more efficient than surface mass balance in decreasing volume along the whole duration of an interstadial (in the case of OCNsrf) or a stadial (OCNsub). This is due to the fact that ablation is restricted to the southern borders of the EIS. Thus, when the ice sheet has retreated to areas of no ablation, in spite of a slight further loss provided by the elevation feedback, it rapidly equilibrates and a negative surface mass balance can not be propagated further inland. In contrast, when enhanced basal melting from higher oceanic temperatures is applied, the associated retreat can propagate further inland occupying a large proportion of the Bjonorema bassin and facilitating high rates of volume loss (although similar in amplitude with respect to SMB) during the whole interstadial (or stadial in the case of OCNsub). To make this clear this discussion has been added in the Results section:

"The rate of ice loss by basal melting is similar to that resulting from the surface mass balance change one (from an increase in ablation) during the peak of a stadial-to-interstadial period. However, basal melting is much more efficient than surface mass balance in decreasing volume along the whole duration of an interstadial (in the case of OCNsrf) or a stadial (OCNsub). This is due to the fact that ablation is restricted to the southern borders of the EIS. Thus, when the ice sheet has retreated to areas of no ablation areas (in spite of a slight further loss provided by the elevation feedback) it rapidly equilibrates and a negative surface mass balance can not be propagated further inland. In contrast, when enhanced basal melting from higher oceanic temperatures is applied, the associated retreat can propagate further inland occupying a large proportion of the Bjonorema bassin and facilitating high rates of volume loss (although similar in amplitude with respect to SMB) during the whole interstadial (or stadial in the case of OCNsub)."

Concerning ice shelves, the temporal extension of the ice shelves is now included in Figures 7 and 8. Their extension is greatly reduced during periods of enhanced basal melting as it is now illustrated in these figures. There are not large unconfined ice shelves surviving during these episodes. Some ice shelves can however survive during these episodes of high basal melting thanks to an increase in advection from the Bjonoram ice stream triggered by a grounding line retreat (see Figure 6). The grounding line has now more clearly been depicted so no misunderstanding between grounded and floating parts can happen. To make this clear we have made a new video that is now included in the Supplementary Information, and we have included this discussion in the Results section:

"*The extension of ice shelves is greatly reduced during periods of enhanced basal melting (Figures 7,8), with no large unconfined ice shelves surviving during these episodes (see also the Supplementary Information). Some ice shelves can however remain, in spite of the enhanced basal melting, thanks to an increase in advection from the Bjonoram ice stream triggered by a grounding line retreat (see Figure 6).*"

4) - Sub-shelf basal melting rate is not the only control exerted by the ocean on ice dynamics. What about sea level variability (and/or glacio-isostasy)? Some authors present the marine based Kara-Barents complex as an analogue for present-day West Antarctic ice sheet for which bedrock topography is a major control for stability. Of course marine ice sheet instability is generally triggered by a sub-shelf basal melt perturbation but is largely amplified by local

bedrock depth with respect to sea level. In addition to provide more information on how your model deals with grounding line dynamics and glacio-isostasy, I think you should add a discussion about marine ice sheet instability of the Kara-Barents complex.

We thank the referee for the suggestion of including a discussion on the Barents-Kara / Antarctica analogy. We have followed it and this discussion is now more explicitly addressed in the Discussion section.

"Some authors present the marine based Kara-Barents complex as an analogue for present-day West Antarctic ice sheet for which bedrock topography is a major control for stability. Marine ice sheet instability is generally triggered by a sub-shelf basal melt perturbation but is largely amplified by local bedrock depth. We have shown, in this sense, that the Bjornoyrenna basin is highly susceptible to changes in the oceanic temperatures. The timing of this response with respect to changes registered in Greenland depends, however, on whether the surface or the subsurface of the ocean is considered as the relevant forcing of the ice sheet."

We also explored the mentioned dependence on sea-level variability, and compare it to a control simulation (see new Figure 3). Changes in the imposed sea level appear not to be sufficient to cause any substantial change in EIS volume. The manuscript has been accordingly modified, including this aspect.

In the Offline forcing section we added:

"Finally, millennial-scale sea-level variations are prescribed according to the reconstruction by Grant et al. (2012; Section 2.3). The specific details of the experimental setup used are described below".

In the Experimental Setup section we added:

In addition, varying sea-level forcing is considered (Figure 3b), both alone (SL run) and in combination with the previous forcings (ATM+OCN+SL).

"Under constant forcing, the CTRL run shows negligible millennial-scale sea-level equivalent (SLE) variations, although a lower frequency SLE fluctuation is found related to internal ice-sheet variability (Figure 3). When the model is forced only by changes in sea level, (SL run) a slight response is observed on millennial-scales. These changes appear not be sufficient to cause a substantial migration of the grounding line, thus not affecting ice velocities."

5) - Please provide more model information. SMB: what is the parameters used in the PDD model? Do you have a fixed daily variability (sigma)? Do you take into account refreezing? What is the value of your vertical lapse rate?

The main parameters used in of the PDD model, for refreezing and the lapse rates are now described in the main text:

"*Its main parameters are the standard deviation of daily temperature, σ, and the conversion factors from PDDs to melt for snow and ice, f_PDD_snow and f_PDD_ ice. Here, sigma = 5 K, f_PDD_snow = 0.003 mwe/PDD and f_PDD_ice 0.008 mwe/PDD. Refreezing is considered, with a value of **r** = 60%. GRISLI-UCM accounts for changes in elevation at each time step considering a linear atmospheric vertical profile for temperature with different lapse rates in summer and in the annual mean (0.0080 and 0.0065 K/m, respectively) to account for the smaller summer atmospheric vertical stability (see also the Supplementary Information)".*

We have now also explored the sensitivity of our ice sheet model to the following range of values of these parameters:

σ: 4,5,6 K; f_PDD_snow: 0.0015, 0.003, 0.006 mwe/PDD; f_PDD_ice: 0.004, 0.008,0.016 mwe/PDD
This information has now been added in the Supplementary Information, where we address all the new sensitivity test devoted to explore the atmospheric uncertainties.

6) GRISLI-UCM: how do you define the calving rate in the model?
Maybe more importantly for ice sheet dynamics:
how is computed the grounding line position?

Calving is the result of a threshold criterion together with a sem-Lagrangian diagnosis of the advection on the ice shelf. The grounding line position is the result of applying the flotation criterium after the mass conservation equation is solved. This information has now been added to the new version of the manuscript in the Model description section, and reads:

"The grounding line position dynamically evolves following the flotation criterion after the mass conservation equation is solved. Ice on the ice-shelf front calvs following a double criterion: Its thickness must first fall below a threshold (H_calv = 150 m , in the standard setup: see Supplementary Information for further information about the dependence of our results on this parameter), and second the upstream advection must fail to maintain the ice thickness above this threshold following a semi-Lagrangian approach (Peyaud et al, 2007)"

7) How do you combine SIA and SSA?

SSA is systematically and only applied for regions of floating ice. Ice streams are simulated as "dragging" ice shelves. The criterium to activate SSA inland holds on the presence of water above 1 meter in places of soft sediments (Laske et al. 1997) and above 400 meters in absence of these sediments. This information has now been included in the manuscript in the Model description , and reads:

"*The criterion to activate SSA inland holds on the presence of water above 1 meter in places of soft sediments (Laske et al. 1997) and above 400 meters in absence of these sediments"*

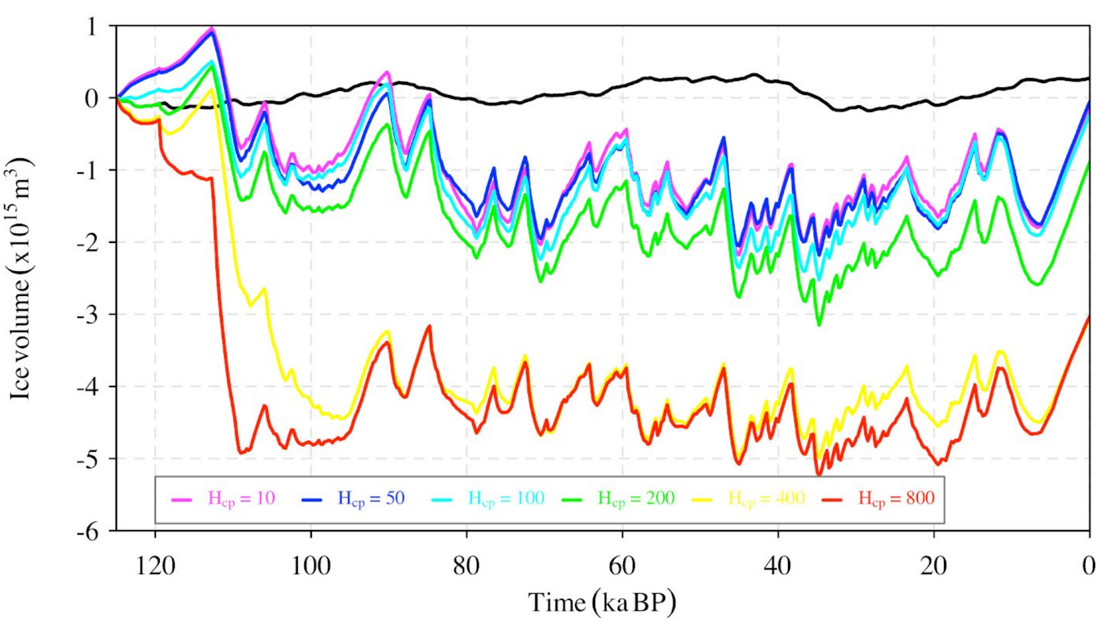
And

"The effects of varying this proportionality factor on the simulated ice streams are discussed in Alvarez-Solas et al (2011)"

Please see Alvarez-Solas et al, 2011 for further information.

8) - You should assess the sensitivity of your results to the calving parametrisation / parameters.

We have performed a new ensemble varying the main parameter controlling calving: the threshold in thickness below which the ice is calved. We have now explored a wide value range from 10 to 800 meters. Values of this threshold above 400 m allow an efficient disintegration of the Barents-Kara complex due to its relative shallow bed. The typical value of this parameter is however 150 meters (see Peyaud et al, 2007). The overall effect of this sensitivity test around the prefered value is to modulate the amplitude of the response to the oceanic perturbations (see figure below) but we think this does not change the main conclusions of this paper. A more thorough exploration of different calving laws is in the scope of future work that we are currently planning.



9) You justified the use of SST instead of sub-surface temperature because the Eurasian ice shelves are shallow. This is not really convincing. SST might be more correlated to surface processes (e.g. SMB) than to sub-shelf basal melting rates. Please provide a plot of sub-surface temperatures anomalies (Fig. 1) and a more robust justification for the use of the SST.

This issue was also raised by reviewer 1 and has motivated as pointed out above carrying out a new set of experiments. We have now repeated the OCN and ALL experiments with a sub-surface forcing centered around 400-meters of depth. This new 2D-field is now included in the new figure 2 which now contains the surface oceanic field (called OCNsrf) and the subsurface oceanic field (OCNsub). According to this, panel d) of current Figure 2 shows the stadial-interstadial anomalies of the subsurface ocean temperatures. This new field has now been used to force the model again. New results are now included in all current figures.

The abstract has been accordingly modified, and now reads:

"Our results show that the EIS responds with enhanced ice discharges in phase with interstadial warming in the North Atlantic when forced with the surface of the ocean. Conversely, the discharges are found to happen both during stadials and at the beginning of the interstadials when the subsurface of the ocean is considered."

And:

"While the atmospheric forcing alone is only able to produce modest iceberg discharges, warming of the ocean leads to higher rates of iceberg discharges as a result of relatively strong basal melting within the margins of the ice sheet."

We have also changed accordingly the Experimental setup section where we described the results of applying this new forcing.

"*To investigate the effect of this choice we decided to perform two different simulations considering different depths: one corresponding to the surface (OCNsrf) and the other one considering deeper (subsurface) oceanic waters by averaging temperatures within the range of 550 - 1050 m depth (OCNsub). Therefore we hereafter distinguish between Delta T_mil^ocn for surface or subsurface millennial-scale temperature anomalies, respectively (Figure 2). The realism and convenience of applying one or the other is addressed in section 5.*"

Finally, we have included the corresponding results in the Results section:

"In contrast, the oceanic forcing in OCNsrf induces pronounced changes in the dynamics of the EIS on millennial time scales, with episodes of a large volume reduction occurring during interstadials. The combination of sea level, atmospheric and oceanic forcings (SL + ATM +

OCNsrf run) results in a similar response of the EIS to that obtained in OCNsrf (Figure 3) as a consequence of the larger effect of the oceanic forcing in OCNsrf with respect to ATM. OCNsub shows an anti-phase relationship with respect to OCNsrf, with the largest reductions in ice volume occurring during prolonged stadial periods and regrowth phases happening during interstadials. This behavior can be explained by the fact that ocean waters at the subsurface warm (cool) during episodes of reduced (enhanced) convection at the Nordic Seas as a result of variations in the AMOC strength. When considering the forcing at the subsurface of the ocean together with the atmosphere (SL+ATM+OCNsub), slight reductions of the EIS volume (less than 1 m of s.l.e) during interstadials are superimposed to the previous behavior (Figure 3)"

10) Please improve on your figure quality. The plots are generally blurry (Fig. 3 and 4) and the color scales are not necessarily suited for the interpretation of the results (Fig. 4). The projection chosen is somehow unorthodox and you should draw the meridians and parallels.

We have followed the reviewer's suggestion and modified all figures.

Specific comments

1) - P1L14-16 please moderate: the larger response is expected as you impose a much larger oceanic perturbation compared to the atmospheric perturbation

We have modulated this discussion now:

"*While the atmospheric forcing alone is only able to produce modest iceberg discharges, warming of the ocean leads to higher rates of iceberg discharges as a result of relatively strong basal melting within the margins of the ice sheet*"

And also moderated the Abstract and Conclusions in several places.

Nevertheless, we should point out here that we do not impose a larger oceanic perturbation per se. The temporal index is the same for the atmosphere and the ocean and the amplitude is given by an OGCM simulation of two different oceanic states mimicking a stadial and an interstadial. We then translate those fields into ablation (through PDD, whose uncertainty has now been largely explored, see General Comment 5 and response to Reviewer 1) and into basal melting (through a linear equation). The values of the oceanic sensitivity parameter (kappa) we used here are in the range (or even below in most cases) of those suggested by data in Antarctica (Rignot et al. 2002). Note, in particular, that even for low-mid values of kappa of 3 meter/year/Kelvin the response to the ocean appears to be of greater amplitude than that to the atmosphere, making our main conclusions robust. We have added this discussion as well in the Discussion section to make this point clear:

"*Our results indicate that the ocean is the major driver of the EIS ice-volume changes during MIS-3. The larger response to the could be expected to result from imposing a much larger oceanic perturbation compared to the atmospheric perturbation. However, we note that the temporal index used is the same for the atmosphere and the ocean and the amplitude is given by a OGCM simulation of two different oceanic states mimicking a stadial and an interstadial. We then translate those fields into ablation (through PDD, whose uncertainty has now been largely explored) and into basal melting (through a linear equation). The values of the oceanic sensitivity parameter (kappa) we used here are in the range (or even below in most cases) of those suggested by data in Antarctica (Rignot and Jacobs 2002). Note, in particular, that even for low-mid values of kappa of 3 meter/year/Kelvin the response to the ocean appears to be of greater amplitude than that to the atmosphere, making our main conclusions robust*"

2) - P3L24-26 these sentences are misleading: as you do not assess the impact of sea level variations and its impact on grounding line migration, you do not explicitly test "dynamic processes related to ice-ocean interactions". You quantify the effect of ice melt (and refreezing) scenarios on the dynamics of the ice sheet. Please rephrase.

This has now been done (please see the responses to General Comment number 4 above).

3) - P4L8 What is the value of the proportionality factor?

Please see the response to General Comment 7. We refer here to Alvarez-Solas et al, 2011, CP where this dependence is described and tested.

We have added the following sentence to the manuscript:

"The effects of varying this proportionality factor on the simulated ice streams are discussed in Alvarez-Solas et al (2011)"

4) - P4L9 How do you know where the sediment layer is saturated?

The water content of the sediment layer is prognosed by the model. The sediment is considered to be saturated in the presence of more than 1 meter of water. Please see Alvarez-Solas et al, 2011 for further information. We have clarified this in the Model description section and added the reference to the sediment map on which this is based (Laske et al, 1997)

"*The criterium to activate SSA inland holds on the presence of water above 1 meter in places of soft sediments (Laske et al. 1997) and above 400 meters in absence of these sediments"*

5) - P4L9-10 "explicitly calculates grounding line migration": how?

Please see the response to General Comment 6 above.

6) - P4L10 how calving is computed?
Please see the response to General Comment 6 above.

7) - P4L18 Please list the PDD model parameter values. Do you use an atmospheric lapse rate?
Please see the response to General Comment 5 above.

8) - P4L18-19 Please provide a reference or show the equation for the inland basal melting computation

This information can be found in Ritz et al., (2001). We have added the reference in the pertinent place.

9) - P4L19-20 A study from 2004 is not “recent”
Recent has been deleted.

10) - P6L6-8 I understand that the present day basal melting rate in the Arctic is difficult to quantify. However, you should present a map of B0 and B40k computed from your expression in order to quantify the role of kappa. This figure could also help to choose the right kappa value: being close to 0.1 at 40k and not too strong for present day (as we have sea-ice and you use SSTs).

The suggestion made her by the referee is indeed one possibility, but we consider than calibrating B40 with respect to B0 via comparison to the present-day distribution of sea ice is difficult and that our simpler approach is still pertinent for the purposes of our paper.

11) - P6L25-26 You should show a map of ice thickness in the CTRL experiment clearly showing the extent of the grounded part of the ice sheet.

We followed referee’s suggestion and added the 2D CTRL map in current Figure 1

12) - P6L26 What is the depth of CLIMBER first oceanic level? Please justify better the use of SSTs.

The first oceanic level of CLIMBER-3alpha goes from 0-50 meters depth. Concerning SSTs versus subsurface, please see General Comment number 9 above.

13) - P7L4-7 Please show a map of the anomaly in surface ablation and in basal melt rate for beta=1 and beta=-1. This is important to quantify the imposed perturbation in your ATM and

OCN experiment. Unlike Fig. 4, use for this the same topography (for example your spun-up initial topography).

The anomalies of ablation and basal melt are subjected to feedbacks triggered by the thermomechanical state of the ice sheet. Therefore a quantification of the atmospheric and oceanic perturbations can not simply be made by plotting those fields for a given beta (see also the response to GC 3).
Nevertheless the required information for quantifying the perturbations can now been found in a new figure. Thus, this caveat is now addressed by adding a figure concerning the mass balance terms (see Figure 5).

14) - P7L13 Is this total ice volume? What is the volume of your spunup topography.

Yes, this is the total (grounded plus floating) EIS SLE change. The volume of our spunup topography is 8.3 x 10^15 m^3.

15) - P8L23-24 It is generally assumed that the melt anomaly is not linear with the temperature perturbation (e.g. Holland and Jenkins, J. of Climate, 2008). You should put more references in here and try to quantify your chosen sensitivity with respect to other melt models available in the literature. I agree that the basal melting rate is potentially highly variable but the fact that you use SST instead of sub-surface temperature added to the fact that you choose a high kappa value might lead to an overestimation of the oceanic induced melt sensitivity?

The discussion on the submarine melting has been extended, and our choice justified, as follows:

“Several marine basal melting rate parameterizations can be found in the literature. The submarine melt rate is thought to be directly influenced by the oceanic temperature variations below the ice shelves. Accordingly, most basal melting parameterizations are built as function of the difference between the oceanic temperature at the ice–ocean boundary layer and the temperature at the ice-shelf base, generally assumed to be at the freezing point. The dependence on this temperature difference can be linear (Beckmann and Goosse, 2003) or quadratic (Holland et al., 2008b; Pollard and DeConto, 2012; DeConto and Pollard, 2016; Pattyn, 2017). The linear marine basal melting rate parameterization used in this study is the simplest case that allows testing of the ice-sheet sensitivity to past oceanic temperature changes. Nevertheless, it accounts separately for sub-ice-shelf areas near the grounding line and for purely floating ice (ice shelves). the basal melting rate for purely floating ice shelves (Bsh) is given by the grounding-line basal melt Bgl scaled by a constant factor γ : Bsh(t) = γ Bgl(t). In this study, γ is set to 0.1. Thus, we consider that the submarine melting rate for ice shelves is 10 times lower than that close to the grounding zone, which is qualitatively in agreement with observations in some Greenland glaciers (Münchow et al., 2014; Rignot and Steffen, 2008; Wilson et al., 2017)”

The SST aspect is now included by the new experiments showing the response to subsurface temperatures as well.

16) - P8L28-29 Apart from sub-shelf refreezing, what is the mechanism for ice-sheet re-growth? Please discuss in light of your ice volume gain of about 0.7 mSLE / 1000 years deduced from Fig 2d.

Please see new figures 4 and 5. We also have explicitly mentioned the mechanism for regrowth in the results section.

Concerning OCNsrf:

“During stadial periods, both enhanced positive mass balance and negative oceanic anomalies (producing a slight refreezing) favor the regrowth of the EIS.”

And concerning OCNsub:

“Subsequently, reduced basal melting in the NE part of the EIS (allowing even a slight refreezing at its grounding line) favors regrowth of the Bj{\o}rn{\o}yrenna basin during interstadial periods.”

17) - P10L6 Be more specific: atmospheric and oceanic induced melt (you did not test the impact of sea level variations).

The impact of sea-level variations has now been included; please see General Comment number 4 and Specific Point number 2 above.

18) - P10L13-14 Show that this is still the case when you don’t have refreezing under ice Shelves.

We have modulated this sentence. The new sentence reads now:

“[...]. Added to the smaller contribution of the SW retreat, this results in sea-level changes on the order of several meters.”

Nevertheless, as explained in the new manuscript, refreezing is now decreased by one order and magnitude and new Figures 4 and 5 show that a large refreezing is not a necessary condition for the mentioned sentence to be true.

19) - Fig 1 Annual means? Do you really have +12 ◦ C In Scandinavia at 40k?

This was a mistake in the previous plot. It has now been corrected.

20) - Fig 1 In this figure or in a new one: annual mean SMB anomaly (interstadial minus stadial) along with annual mean basal melting rate anomaly.

Please see the response to Specific Comment 13.

21) - Fig 2 Basal melting and surface ablation here as well, integrated over the whole ice Sheet.

Please see new figure 5 and see the response to Specific Comment 13.

22) - Fig 2 Maybe in a separated plot: grounded and floating ice extent evolution for the different experiment

Done in new Figures 7 and 8.

23) - Fig 2 2d Dashed lines?

This figure has been remade and improved

24) - Fig 2 2d is this grounded or total ice volume ? Please show the floating ice.

It was the total ice volume. Floating ice is shown now in new Figures 7 and 8.

25) - Fig 3 Which “stadials” and “interstadials” is represented here?

The stadial corresponds to 45.6 ka BO and the interstadial corresponds to DO 12 (ca. 47 ka BP) and the This has been now included in the Figure caption.

26) - Fig 3 Please clearly show the grounding line and the ice extent everywhere.

All the 2D spatial plots are now re-done and improved.

27) - Fig 3 Why the selected velocity point is not in the middle of the ice stream? Is it vertically integrated velocity?

This has now been changed. We consider velocities of the whole basin. And yes, it is vertically integrated velocity.

28) - Fig 3 We see an increase in velocity in the ATM experiment but we cannot see anything in Fig 2. Why?

This is was a consequence of considering a grid point which was not located right in the middle of the ice stream. However a small velocity increase of ~0.1 km a^{-1} is observed at the approximate timing of the interstadial associated to DO 12. The associated figures have now been re-done.

29) - Fig 3 Your ice shelf in stadials seem to have a very limited extent. Do you have certain specific boundary conditions, such as a depth criteria? If yes, this can be another problem as you will only have ice shelves if you start to retreat inland (which seems to be the case looking at your OCN (Is) snapshot). Conceptually, do you expect a larger ice shelf extent in interstadials relative to stadials?

We only consider depth for basal melting as an additional boundary condition above 750 meters, a deeper (thus more conservative) value than in Peyaud et al. 2007. This has been now included in the Model description:

"The melt rate in the open ocean, that is considered as being beyond the continental shelf break, is prescribed to a high value (20 m a−1) to avoid unrealistic ice growth beyond 750 m of ocean depth, following Peyaud et al. (2007)"

30) - Fig 3 I am surprised to see that the British ice sheet presents generally very low velocities. I am guessing that it has a frozen bed, which is unexpected due to the warm climate in this area. Can you comment on this?

For some periods of ice expansion (either from reduced ablation or a decreased in basal melting) the initial low thickness of the ice sheet in this region does not allow to isolate enough the base from negative temperatures at the surface (the climate is warm but presents low but negative values of its temperature during stadials). Therefore the base can be frozen and velocities low. Nevertheless this is not the common situation. Velocities are generally high in this region (please see new Figure 6 and the supplementary movies).

31) - Fig 3 In a separated plot: please show a map of ice thickness (with limit of grounded part) for the same selected glacial and respective interglacial as in Fig 2.

We do not understand here the precise request. We have nevertheless now clearly illustrated the position of the grounding line in new Figure 6.

32)- Fig 4b I do not understand why there is a band of high basal melting rate (near the coastlines?). Also, why there is a wide area in the Nordic Seas with a relatively high basal melting rate: this area is not supposed to be grounded? Perhaps you have two different topographies here? This has to be clarified but I strongly suggest to plot the anomalies computed on the same ice sheet geometry (ideally the spun-up one). Please change the color scale.

The reason for this is that the basal melting rate for purely floating ice shelves (Bsh) is given by the grounding-line basal melt Bgl scaled by a constant factor γ : Bsh(t) = γ Bgl(t). In this study, γ is set to 0.1. Thus, we consider that the submarine melting rate for ice shelves is 10 times lower than that close to the grounding zone, which is qualitatively in agreement with observations in some Greenland glaciers (Münchow et al., 2014; Rignot and Steffen, 2008; Wilson et al., 2017). This discussion has been now included in the Model description:

"*The marine basal melting rate parameterization used in this work follows a linear approach that accounts separately for sub-ice-shelf areas near the grounding line and for purely floating ice (ice shelves). the basal melting rate for purely floating ice shelves (Bsh) is given by the grounding-line basal melt Bgl scaled by a constant factor γ : Bsh(t) = γ Bgl(t). In this study, γ is set to 0.1. Thus, we consider that the submarine melting rate for ice shelves is 10 times lower than that close to the grounding zone, which is qualitatively in agreement with observations in some Greenland glaciers (Münchow et al., 2014; Rignot and Steffen, 2008; Wilson et al., 2017)*"

33) - Fig 5 "region of Bjørnøyrenna": be more specific (maybe show this region on a map).
This is now shown in new Figure 1.

Technical corrections

1) - P7L26 SATs

Done

2) - P9L22 amount

Done

References:

Obase, T., Abe-Ouchi, A., Kusahara, K., Hasumi, H., & Ohgaito, R. (2017). Responses of Basal Melting of Antarctic Ice Shelves to the Climatic Forcing of the Last Glacial Maximum and CO2 Doubling. *Journal of Climate*, *30*(10), 3473-3497.