Response to Reviewer #1's comment on "The sensitivity of the Greenland ice sheet to glacial-interglacial oceanic forcing" by Tabone et al.

The paper by Tabone et al. describes a series of experiments over the past 2 glacial cycles performed with an ice-sheet-shelf model for the Greenland ice sheet (GrIS). The authors explore the influence of ocean temperature, through a sub-shelf melt parameter, on the advance and retreat of the Greenland ice sheet. Simulations are performed over 240 kyr, for which atmospheric forcing is prescribed through a glacial index derived from a combination of ice-core data reconstructions. The same index is used to vary oceanic temperatures, although this time variability is also switched off in some experiments.

The paper has a clear and relevant scientific question within the scope of CP, exploring the variations of the GrIS over glacial cycles, using a novel concept (at least for Greenland) using an ice-sheet-shelf model that is capable of grounding line migration and simulation ice on land and afloat. The conclusions are clear but need further exploration, and some additional comments in the text. Methods and results are clearly presented, but I do suggest additional experiments need to added that take into account changes in sea level. Already in the Huybrechts (2002) study, sea level is used as external forcing, so why not do it here? Particularly when running an ice-sheet model over multiple glacial cycles this should be included.

The paper's title is clear and overall presentation and language is good. References are appropriate and the introduction is presented very well. All in all, I think this is a well written and good paper that deserves publication in CP. As said, I do suggest that the authors include additional experiments that include an external sea-level as forcing to further explore the influence of changes in sea level. Besides my four main comments I have attached a pdf with comments included (I hope you can read/find them clearly).

We thank the reviewer for his valuable comments and suggestions. We fully understand his concern regarding the use of a constant sea level. We initially used this approach for the sake of simplicity in experimental design. However we agree that this may lead to an oversimplification. Thus, we have redone all the experiments by forcing the ice-sheet-shelf model with a variable relative sea-level (RSL) change reconstruction from Grant et al. (2014). Under these conditions, the GrIS is found to be somewhat more sensitive to the ocean, however the main results of our work do not change.

Specific answers to the main comments are listed point by point.

Main remarks:
1. Description of basal melting
As I noted in my comments in the pdf, it is unclear to me if you use spatially varying basal melting and how Bref depends on kappa. As given in Table 1, for the first set of experiments you set kappa to 0, which as I imply from equations (9) and (10) that basal melting at the grounding line is zero everywhere, all the time. Please clarify this in the paper.

Following Eq. 10, when kappa is set to 0, the basal melting at the grounding line is equal to the value set for B_{ref}, for the entire length of the experiment. Therefore, in the first set of the experiments the basal melting rate at the grounding line is equal to 0 m a $^{-1}$ only when both B_{ref} and κ are null. This point is explained by adding the sentence in section 2.4:

"The resulting basal melting rate is thus equal to the tested B_{ref} value set in the simulation and a condition of no oceanic basal melting around the GrIS is achieved only when both B_{ref} and κ are set to zero."

After reading section 2.3, it seems that you use the equations 6-10 as a general description of how basal melting can be calculated. Overall reading this I thought that all parameters mentioned are used in the model, or not? Please make clear how you actually prescribe basal melting in the model, is it a uniform single value or is it a function of x and y or locations? State explicitly what is used in the model and which variables you mention in equations 6-10 are actually not used. It is rather confusing that you mention these variables and then actually not use them.

The representation of basal melting used in the study is described by equations Eq. 10, for the grounding line, and Eq. 11, for the submarine melting rate below the ice shelves. All previous equations (Eqs. 1-9) were included in order to show on which physical principles our parameterisation is founded and how we derived it from those, but we have *not directly applied* these equations in our study.

To make this point clearer we have rephrased the derivation of the basal melting rate parameterisation in the way:

"The marine basal melting rate parameterisation 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 formulation is derived from the net basal melt rate B_{gl} [m a^{-1}] for ice-shelf cavities close to the grounding line and terminating in shallow ocean zones, expressed by Beckmann and Goosse (2003) as [...]. Since knowledge of past T_{ocn} and T_f is challenging for the complex heat-flux transfer between ice shelves and the surrounding water, we opted for substituting these quantities and rearranging the equations to make them more suitable for paleo studies. [...] Combining and re-organizing these equations we can finally retrieve the expression for the basal melting rate at the grounding line B_{gl} as used in this work [...]."

The free parameters of the basal melting rate equations (Eq. 10 and 11) are B_{ref}, κ, ΔT_{ocn} and γ. For the sake of simplicity we decided to consider all of them as spatially uniform. Since we want to test the sensitivity of the GrIS only to κ and B_{ref}, we set ΔT_{ocn} and γ to one scalar value which is constant in time and space. Nevertheless, different scalar values (constant in time and space as well) were tested for B_{ref} and κ. The result is that the basal melting rate at the grounding line is constant in space (uniform) but varies in time through the climatic index α.

"In a more realistic setup, all the parameters in Eq. 10 could be described by 2D spatially variable fields. However, for the sake of simplicity, all of them are assumed here as spatially uniform around all the GrIS marine borders, as described in section 2.4. The glacial-interglacial temperature anomaly $T_{LGM,ocn} - T_{PD,ocn}$ (Eq. 8) is set constant to −3K, which corresponds to the mean value of the reconstructed LGM Sea Surface Temperature (SST) anomalies for the Atlantic Ocean between 60ºN and 80ºN of latitude (MARGO, 2009). [...] These simplifications allow here for a spatially-uniform, but time-dependent B_{gl}."

2. Influence of Insolation on surface melt

In Section 2.2 the melting scheme is shortly discussed. I do wonder why insolation changes are not included (as I noted in the pdf as well). Why not include the correction as suggested in Robinson and Goelzer (2014)? It is shown by them (and other studies) that particularly for the GrIS insolation changes are important, especially for the last interglacial. I expect the authors to at least discuss this in more detail in the discussion/conclusions.

We agree that we did discuss melting scheme in detail. The PDD scheme has been proven inadequate for paleo simulations since it neglects insolation-driven melting a priori, which becomes important in past warming periods. Nevertheless, we decided to keep using this scheme since this inaccuracy does not compromise the study as we are focused on ocean-induced melting.

We have now discussed this point with much detail in the Model description section by adding the sentence:

"This melting scheme is admittedly too simple for paleo simulations, as it omits the contribution of insolation-induced effects on surface melting, which are emphasized in past warmer periods such as the Eemian (Robinson and Goelzer, 2014). However, since this study focuses on the melting effects induced by past ocean temperature variations, the PDD melt model is sufficient to give a first approximation of surface melt that allows the ice sheet to retreat during interglacial periods in a realistic way."

and in the Discussion:

"The simulated retreat during this phase is influenced by the choice of the surface melt scheme used in the model. At the peak of the Eemian, the melt determined by the PDD scheme can be 20-50% lower than the melt calculated if past insolation changes are taken into account (Robinson and Goelzer, 2014). This inaccuracy therefore influences the RSL values shown for the last interglacial, which could be underestimated."

3. Comparison to Bradley et al.
About the same time a study investigating similar issues has been published in CPD, Bradley et al. I see that the first author has actually reviewed this paper. I want to note that I was not involved in this paper, although it does make use of the same ice-sheet model (although a different version) that I use. It might be good that some comparison would be made with this other paper for example in the discussion and in Figure 11. At least mentioning this paper in the introduction and the discussion.

At the time we submitted the manuscript we were not aware of the publication by Bradley et al. in CPD. But it is true that the paper from Bradley et al. investigates a similar issue thus many correspondences between the two papers are present. We have now included several references to that study in the following ways:

First, their work is reported in the Introduction by adding the sentence:

"Recently, Bradley et al. (2017) simulated the GrIS evolution for the two last glacial cycles by considering a sub-shelf melt parameterisation which is a function of the water depth below the ice shelves. Under this assumption, the submarine melt rate increases when the past sea level rises. However their approach does not take into account ocean temperature changes."

Second, analogies/differences with our work are discussed in the Discussion section:

"A recent study shows the necessity to make the basal melting decrease smoothly to zero when approaching the grounding line from the ice shelf to avoid resolution-dependent performance (Gladstone et al., 2017). That approach can be obtained for example by considering the submarine melt to be dependent on the water-column thickness beneath the ice shelf, as Bradley et al. (2017) suggested in their work. It is interesting to compare our results with theirs, as we address the same scientific problem, i.e. the impact of submarine melting on the evolution of the past GrIS, from two different points of view. Our submarine melt scheme is implicitly a linear function of the water depth, as, going down through the water column, the melt rate maintains the same value until it reaches a critical zone at which the sub-shelf melt is set to 50 m a^{-1} to avoid improbable ice expansion (Section 2.3). Our work shows that without melting/freezing at the grounding line (for B_{ref}=0 and κ=0), the GrIS is not able to reach the continental shelf (Fig. 12 b). However, it is able to extend past the present-day coastline, as Bradley's model does. Moreover, experiments performed under the same oceanic conditions with increased basal sliding at the margins show that our model allows further expansion during the glacial periods (not shown). On the other hand, Bradley's model has the capability of making the GrIS retreat during interglacial periods only if the submarine melt-water depth relation is exponential and if RSL variations due to both local and non-local effects are considered. On the contrary, a proper retreat during the deglaciations is always achieved in our simulations (Fig. 3 and Fig. 6-7), although the GIA does not account for global effects. These discrepancies are probably due to not only to the different submarine melt scheme considered in each model, but also to the features of the model dynamics, such as the sliding law and the 2D grounding line migration scheme."

4. External sea level forcing
As I noted in the pdf, it seems to me that using a constant sea level during the entire simulation time is unrealistic. I would highly suggest to at least include eustatic (i.e. global mean) sea level variations as external forcing in these simulations. (for example, from Grant et al: http://dx.doi.org/10.1038/ncomms6076 or other studies you might find). I would be very interested in how the results will change. It provides you with additional experiments to also show the sensitivity to ocean forcing in terms of sea level.

As mentioned above, we agree with this point about the use of a constant sea level. Now, a variable relative sea-level (RSL) change reconstruction from Grant et al. (2014) has been used to force the model and all the experiments have been redone. The new results confirm that the GrIS evolution is primarily driven by the ocean. Moreover, the ocean influence on the ice sheet is rather enhanced by the use of variable sea-level forcing. Therefore, the main results of our work do not change.

References:

Grant, K. M., et al. "Sea-level variability over five glacial cycles." *Nature communications* 5 (2014).