Interactive comment on "Impact of millennial-scale oceanic variability on the Greenland ice sheet evolution throughout the Last Glacial Period" by Ilaria Tabone et al. Anonymous Referee #2

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Tabone et al. use a three dimensional hybrid ice-sheet-shelf model forced by oceanic fluctuations derived from paleo records. The work studies the role of millennial scale climate variability in ocean-ice sheet interaction and is a topic that is of great interest to the glacial climate variability. These type of studies provide a basis to test our earth system models under past climate conditions in order to validate projections of anthropogenic climate change or to develop a better process understanding of climate components that are critical for assessing future anthropogenic climate change. The introduction provides a nice review of the literature on the subject and reviews the key research questions on ocean-ice sheet interaction. The major problem problem I see with the ocean-ice sheet coupling is that the oceanic forcing is not realistic and the study would be better suited to investigate the sensitivity of the model to marine shelf instability as a result of sub-surface temperature variations. Page 11, Line 22 states that the model seems to lag Heinrich Events by 2-3 ka. The manuscript goes to great length to compare the model with sediment cores when the forcing (Figure 1c) is representative of surface temperature changes. The authors state that (Page 12, L2) that they do not aim to precisely reconstruct the timing and spatial distribution of ice discharge during D-O events. I don't think it is satisfactory to show that oceanic millennial scale variability is influencing the GrIS evolution alone unless something can be said about a process based explanation of what is happening in the real climate system based on the modelling results.

We are grateful to the reviewer for their valuable comments. Indeed, the objection regarding the usage of an oceanic forcing that follows surface instead of subsurface temperature variations is legitimate. Since ice shelves are usually hundreds of meters thick, their instability is likely more affected by subsurface rather than surface waters. This assumption is also valid for the effect that the ocean may have on grounding-line fluctuations, since grounding zones are typically found several hundreds of meters deep in the ocean (at least at the present day, e.g. Wilson et al., 2017). We therefore agree that perturbing the model through a surface-dependent oceanic forcing might have been a weakness of the experimental design. Using surface or subsurface oceanic temperatures has a strong impact when investigating the role of the ocean during the last glacial period. Evidence points to a strong surface-subsurface temperature decoupling during the Dansgaard-Oeschger (DO) cycles (e.g. Vettoretti and Peltier, 2015; Brady and Otto-Bliesner, 2011; Knutti et al., 2004), suggesting that subsurface waters have experienced warming during stadials and cooling during interstadials (e.g. Ezat et al., 2014; Sessford et al., 2018; Dokken et al., 2013). To account for this decoupling and to consider subsurface oceanic temperatures instead, we now force the ice-sheet model through an oceanic temperature anomaly signal assumed to be in antiphase with respect to that of the oceanic surface, which in turn reflects atmospheric changes (following e.g. Alvarez-Solas et al. (2010), Alvarez-Solas et al. (2013), Bassis et al. (2017) and Boers et al. (2018)).

Although this modification did not change the overall conclusions of the work and it mainly affected the quantitative description of the results, the manuscript has been deeply changed in this respect. For example, current Figure 8 shows now the comparison between our simulated ice flux and IRDs. The good agreement between the two indicates that GrIS likely contributed to the observed iceberg discharges of the Northern Hemisphere during the last glacial period (LGP), and that these were triggered through oceanic millennial-scale forcing. We believe this aspect together with the rest of our conclusions configure "a process based explanation of what is happening in the real climate system based on the modelling results". We recommend checking the new version of the manuscript for a complete view of the improvements.

Answers to specific comments are reported below.

Other comments:

Section 2.1 Page 4, Lines 15-31: The description of coarse grid points is not clear here. In line 30, the ice sheet model is given (20x20km) but the ocean resolution forcing is not described clearly.

The oceanic forcing is considered as spatially homogeneous, i.e. simply given by the time evolution of the submarine melting rate as specified by Eq. 6. In that equation, since both orbital and millennial-scale temperature anomalies (ΔT^{orb}_{ocn} and ΔT^{mil}_{ocn}) and the reference basal melting rate B_{ref} are considered as spatially constant, the resulting basal melting rate B(t) is spatially homogeneous too. Therefore, the spatial resolution of the ocean is irrelevant.

To make this clear we added the sentence in Section 2.4:

"*These variables are here all considered as spatially uniform around Greenland for the sake of simplicity, leading to a spatially homogeneous basal melting rate.* "

Section 2.1 and 2.2: P4, L31-32; P5 L2; P13 L13 The statement "we consider the atmosphere as modulated by orbital changes". This statement should be rewritten to downplay the impression that there is an atmosphere in the model. Statements such as L31-32 that describe millennial variability in the atmosphere are also misleading. One mainly thinks of atmospheric dynamics in terms of atmospheric climate/weather variability which happens on short timescales. Trace gases, insolation and other slowly evolving atmospheric properties are the result of the internal and external forcing of the earth system.

We agree with the referee and have modified the text accordingly:

The sentence of P4, L31-32 has been rewritten. It now appears as: "*Since the goal of this work is to investigate the sensitivity of the GrIS to past millennial-scale variability in the ocean, we force the ice-sheet model through spatially variable surface atmospheric temperatures and precipitation that only reflect orbital variations.*"

The sentence of P5, L2 has been rewritten as: "*The atmospheric forcing only includes the orbital-scale evolution of temperature and precipitation over the last glacial cycle, thus changes associated with short (millennial) timescales are not taken into account here.*"

The sentence of P13, L13 has been rewritten as: "*Finally, atmospheric precipitation and temperature used to perturb the ice-sheet model vary only at long (orbital) timescales, while shorter (millennial-scale) variations related to the DO events are not considered here.* "

Section 2.3 P5 L26: "... changes in ocean temperature into..."

This sentence has been changed accordingly.

P6 L4-7: The construction of beta is a nice measure of millennial scale variability. But if you really want to influence the basal melt rate during the glacial as proposed by some of the studies in the introduction in a realistic fashion the millennial index in Figure 1c should be inverted to reflect changes in subsurface temperature during Heinrich and D-O stadials. This is a major problem with the study. There is some of this discussion in Section 4.2 on Model limitations and caveats but this detracts from realism of the science and what the study can actually say about what processes are important for millennial scale variability.

As already pointed out in the general comments, we agree with the reviewer that considering subsurface rather than surface oceanic temperatures might be more accurate when considering the effect of oceanic variation on ice-shelf destabilisation and grounding-line retreat. Since oceanic variations at the subsurface associated to DO events are assumed to vary in antiphase with respect to those at the surface (as suggested by several proxies and model results), it is reasonable to simply invert the submarine melting rate signal in the way that subsurface warmings correspond to stadials and subsurface coolings to interstadials. This is possible for example by considering the interstadial-stadial oceanic temperature anomaly ΔT^{mil}_{ocn} as negative (warmer waters during stadials). In this way, and leaving the millennial-scale climatic index β as it was in the previous version of the manuscript, the submarine melting rate is now of the form that warming peaks appear during stadials.

The experimental design, part of the analysis of the results and discussion have been changed accordingly.

P6 L7-8: What is meant by the statement that both deltaTorb,ocn and deltaTmil,ocn are both assumed to be in phase with the atmosphere when there is no deltaTmil,atm in equation (1) and (2).

We agree that the sentence may be misleading. Since now ΔT^{mil}_{ocn} reflects changes in subsurface waters, the sentence has been modified to: "*ΔT^{orb}_{ocn} and ΔT^{mil}_{ocn} are the glacial-interglacial and interstadial-stadial oceanic temperature anomalies (K), respectively. $(1 – \alpha(t)) * \Delta T^{orb}_{ocn}$ reflects the long timescales variations resulting from orbital changes. $\beta * \Delta T^{mil}_{ocn}$ expresses the millennial-scale temperature changes at the subsurface assumed to be in antiphase with respect to the Greenland atmospheric temperature inferred from Greenland ice cores (e.g. Johnsen et al., 2001; Kindler et al., 2014). Thus, we are assuming that subsurface water temperatures increase during stadials and decrease during interstadials. This is in agreement with the presence of warming subsurface waters during stadial periods as suggested by several records of the North Atlantic and Nordic Seas (Ezat et al., 2014; Jonkers et al., 2010; Rasmussen and Thomsen 2004; Rasmussen et al., 2016; Sessford et al., 2018; Dokken et al., 2013) and supported by modelling work (Vettoretti and Peltier, 2015; Brady and Otto-Bliesner, 2011; Mignot et al., 2007; Knutti et al., 2004; Marcott* *et al., 2011). The result is a submarine melting signal that peaks during DO stadials. This is in line with the temporal evolution of oceanic forcings used to inspect the effect of subsurface warming during the coldest stadials, i.e. Heinrich events, by perturbing other ice-sheet models (Alvarez-Solas et al., 2010; Alvarez-Solas et al., 2013; Bassis et al., 2017), or, as done recently, to investigate the origin of DO events through a conceptual model (Boers et al., 2018).*"

P6 L12: Adding a short description of how changes in RSL on the orbital timescale are prescribed might be more helpful instead of just the reference.

This sentence has been changed to: "*Changes in global sea level at orbital timescales are prescribed in the model and are used to compute the grounding-line position, which is based on a simple flotation criterion. The applied sea-level variation time series is taken from Bintanja and Van de Wal (2008). The signal is inferred from a marine benthic oxygen isotope record reconstructed for the last 3 Myr through an ice-sheet model coupled to a simple marine temperature model.*"

Section 2.4 P6 L15-20: The authors state that the basal melt is dependant on 4 parameters. Another problem I see is in the variation of the parameter changes during the LHS sampling. The reference basal melt is given as Bref =kappa(Tclim,ocn - T_f) where T_f is fixed. Equation (6) has B proportional to kappa*deltaTorb,ocn. So variations in kappa and Bref are not independent. Changes in kappa will make inverse changes in Tclim,ocn (the mean climatology of the ocean) if Bref is varied in an inconsistent manner. So am I missing some understanding of the variational procedure or is the LHS sampling (which considers previous choices) taking care of this discrepancy? Again in section 2.4 L5 , it states that parameter values are samples from specified ranges assuming they are independent from each other. Also same thing on P8L17.

The reviewer makes a good point here. Probably simply stating that the four key parameters (B_{ref}, κ, ΔT^{orb}_{ocn} and ΔT^{mil}_{ocn}) of the basal melting rate equation (Eq. 6) are independent from each other is not entirely accurate, since, as the reviewer says, B_{ref} does depend on κ. However, B_{ref} also depends on $T_{clim,ocn}$ and T_f. $T_{clim, ocn}$ is the climatological mean of the water temperature and T_f is the freezing point temperature, both at the grounding line. These two terms are largely unconstrained since they may be at least depth-dependent (Beckmann and Goosse, 2003). Defining B_{ref} in the equation is, therefore, a simplification made to elude the hard choice of assigning values to these two variables. If $T_{clim,ocn} – T_f$ is set as constant, B_{ref} is directly proportional to κ (as we did in another work, Tabone et al., 2018b, where $T_{clim,ocn} – T_f$ was set to 1K). However, if this quantity is left free to vary (implicitly), B_{ref} can be considered as *conditionally* independent from κ, where the condition is considering $T_{clim,ocn} – T_f$ as an additional degree of freedom. The choice of decoupling B_{ref} from κ follows the aim of investigating as many cases as possible to have a good sample of simulations to work with. Nevertheless, the investigated values of B_{ref} (0-10 m a^{-1}) and κ (0-10 m a^{-1} K^{-1}) are chosen to be consistent with plausible values of $T_{clim,ocn} – T_f$ (the median of all analysed values ~ 0.9 K), while those that are less probable ($T_{clim,ocn} – T_f$ > 2 K) are discarded from the analysis a posteriori since they show a very low millennial-scale variability (high B_{ref} and very low κ). Thus, in a certain way we do take care to avoid the discrepancy pointed out by the reviewer, although not through the LHS sampling technique.

To clarify this concept, Section 2.4 has been rewritten as:

"*Following the discussion above, we can rewrite the basal melting equation (Eq. 4) as:*

$$B(t)= B_{ref} + \kappa \left((1 - \alpha(t))\, \Delta T^{orb}_{ocn} + \beta\, \Delta T^{mil}_{ocn}\right)$$

Written in this form, the basal melting formulation depends on the choice of four parameter values: B_{ref}, κ, ΔT^{orb}_{ocn} and ΔT^{mil}_{ocn}. These variables are here all considered as spatially uniform around Greenland for the sake of simplicity, leading to a spatially homogeneous basal melting rate. To assess the GrIS response to millennial-scale variability in the ocean we could simply consider varying the value of κ, which is the sensitivity of the oceanic forcing (Tabone et al., 2018a). However, by construction of Eq. 6, increasing κ does not necessarily mean increasing the millennial-scale oceanic effect alone, since this would enhance concurrently both the millennial and the orbital-scale components in the ocean. Therefore, investigating the oceanic millennial-scale variability effect on the past GrIS is not as straightforward as expected. Moreover, none of the four parameters of Eq. 6 is perfectly constrained in reality, and a sensitivity study on the influence of their chosen values on the GrIS evolution would be required. For these reasons, it is first useful to characterise the impact of millennial-scale variability in the ocean on the GrIS by testing a broad range of values of the key-parameters in Eq. 6.

Some considerations need to be made before describing the experiments. First, it should be noted that B_{ref} depends on κ (Eq. 4), thus any change in κ results in a change in B_{ref} too. However, B_{ref} also depends on the water and freezing-point temperatures at the grounding zone ($T_{clim,ocn}$ and T_f, respectively), that are largely *unconstrained, since they mostly depend on the characteristics of the considered grounding line, e.g. on the depth (Beckmann and Goosse, 2003), and in principle can be represented by a broad range of values. We could have set $T_{clim,ocn} – T_f$ to a constant value to make B_{ref} scale directly with κ, but this would limit the range of submarine melting rates to be investigated and the possibility of better constraining these two terms, which are still poorly known around Greenland. On the contrary, here, $T_{clim,ocn} – T_f$ is treated as an additional degree of freedom, that, although not explicitly perturbed in the equation, allows us to consider B_{ref} as independent from κ. Of course, decoupling these two variables requires additional attention in considering a range of values for B_{ref} and κ that allow for realistic $T_{clim,ocn} – T_f$. The simulations that do not fulfill this requirement will be discarded a posteriori in the analysis.*

Second, by construction, the α and β indices share the same normalisation. Thus the glacial-interglacial and the interstadial-stadial subsurface oceanic temperature anomalies have the same amplitudes. This is also supported by estimates of subsurface temperatures at both short (millennial) and long (orbital) timescales. Reconstructed LGM-present day subsurface anomalies, which at orbital timescales are considered to follow those at the surface, are between 0 and -3 K around Greenland (Annan and Hargreaves, 2013; MARGO 2009). A similar range of values is found for the interstadial-stadial subsurface temperature anomalies (Alvarez-Solas et al., 2018; Brady and Otto-Bliesner, 2011; Vettoretti and Peltier, 2015; Zhang et al., 2014). The problem is therefore reduced to three degrees of freedom: ΔT^{mil}_{ocn}, set to vary between 0 and 3 K, B_{ref}, between 0 and 10 m a^{-1} (chosen as a reasonable climatic mean between Rignot et al. (2010), Rignot et al. (2016), Straneo et al. (2012) and Wilson et al. (2017) for the largest tidewater glaciers around the GrIS, and Liu et al. (2015) and Rignot et al. (2013) for Antarctica) and κ, between 0 and 10 m a^{-1} K^{-1} (following Rignot and Jacobs (2002) for Antarctica).

To test a wide range of combinations between the three key parameters, we perform a large ensemble (LE) of model simulations using the near-random Latin Hypercube Sampling (LHS) technique (McKay et al., 1979), which allows us to efficiently explore the phase-space of the key parameters minimising the LE computational cost with respect to the full-factorial sampling technique. The LHS method has already been used to constrain different ice-sheet model parameters and to assess their influence on the model's behavior (Applegate et al., 2012; Stone et al., 2010; Stone et al., 2013; Robinson et al., 2017). The parameter values are sampled from the specified ranges and, assuming, as discussed, that they are independent from each other, they are randomly combined to generate a total LE of 100 simulations, named TOT simulations. At the same time, we perform another set of identical simulations, except for the fact that the climatic index associated with the millennial scale variability (β) is set to zero. These are named ORB simulations and are used for direct comparison with the TOT simulations, as discussed in Section 3. See Table 1 for a full list of the phase-space of parameters investigated within the two LEs. To initialise the model we use the present-day topography of Greenland from Schaffer et al. (2016). All the simulations of the LE cover the last two glacial cycles, with the first considered as a spin up and therefore not analysed. "

P6 L24: " ... on the GrIS evolution by testing..."

Sentence changed accordingly (see Section 2.4).

P6 L31: language: " This is also supported by estimate of both surface temperature anomalies" By and estimate??? by estimates of both surface temperature anomalies and... This part needs clarification.

This sentence has been changed since the forcing method has been modified (now we consider subsurface instead of surface oceanic temperatures). See Section 2.4.

P7 L7: "except for the fact that the oceanic changes associated with the millennial scale variability (deltaTmil,ocn) is set to zero"

This sentence has been changed to: "*At the same time, we perform another set of identical simulations, except for the fact that the climatic index associated with the millennial scale variability (β) is set to zero*". See Section 2.4.

P10 L31: small ocean temperature variations?

Sentence changed accordingly.

Figures:

Figure 2: This figure of the cube doesn't provide a clear visual of the distribution. I would like to see a something like figure 4 here, but since the information is already in figure 4 the paper needs some

modification. Figure 2 can be removed but there would have to be some major restructuring of the text in Sections 2.4 and 3.1.

Figure 2 has been substituted by one table (Table 1), that reviews the parameter values investigated in the two LEs. Since the information of Figure 2 is still reported, we don't think there is the need of reorganizing the text much.

Figure 3a: The black and blue colours are a poor choice as the lines are indiscernible. Contrasting colours would be much better or add more transparency to the lines.

Blue lines of this figure (now Figure 2a) are now changed to red lines.

Figure 7, SM2 and SM3: Same colour choice as in Figure 3.

Blue lines of Figure SM2 and SM3 are now changed to red lines.

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