Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-89-RC2, 2018

Climate of the Past Discussions

Interactive comment on "Oceanic forcing of the Eurasian Ice Sheet on millennial time scales during the Last Glacial Period" *by* Jorge Alvarez-Solas et al.

Anonymous Referee #2

Received and published: 8 November 2018

Alvarez-Solas et al. investigate the millenial scale variability of the Eurasian ice sheet during the last glacial period. They use an ice sheet model forced offline by a combination of two glacial climatic snapshots, stadial and interstadial. The relative importance of the two snapshots is weighed by an index constructed from a Greenland temperature reconstruction. In their model framework, Alvarez-Solas et al. show that oceanic perturbations induce much greater ice volume changes compared to atmospheric perturbations. They discuss their ice volume variations with respect to IRD layers in marine sediments.

The paper tackles definitively very interesting questions regarding the role of the ocean

in the (in)stability of large marine ice sheets. Little has been done with this respect on the Eurasian ice sheet while a fair amount of geological constraints exist. I think the paper is well written and generally nicely illustrated but I have a few important comments that I would like to see addressed.

General comments

- Basal melting rate and ice volume. I am very happy to see that the authors have chosen to change their basal melting rate formulation compared to their previously submitted version of the manuscript (doi: 10.5194/cp-2017-143) so they no longer use a negative sub-shelf melting rate (ice accretion). However I am surprised that the change in setup, and subsequent change in results, does not relate to any change in conclusion nor discussion. In the previous version of the manuscript, during the transient simulation, the ice volume was oscillating around the 40k spun-up ice volume. In the new version, the ice volume is now perpetually decreasing from 110k to 10k when using the oceanic forcing with kappa>1. As far as I understand your methodology, we expect the 40k ice sheet to be representative of a mean state of the MIS3 ice sheet and your millenial scale index should translate into waxing and waning of the ice sheet around the mean state. The fact that you have a negative trend in ice volume suggests that the model is unable to regrow ice after the imposed oceanic perturbation. I understand that is a complicated issue that cannot be resolved with such a simple index perturbation. However, it seems to me that it is not straightforward to draw robust conclusions on the physical mechanism for MIS3 ice volume oscillations when the model is currently unable to simulate an Eurasian ice sheet that survive to these oscillations. I might be missing something but I think this issue should be clarified and clearly discussed in the paper. As a side note: I could not find the volume your 40k spun-up ice sheet. This is needed to interpret the importance of the trend (8 to 12 m sle!).

- On the method, 1. Because CLIMBER3-α underestimate the stadial to interstadial temperature change at NGRIP, beta* in the paper has been scaled to match the recorded amplitude. One can wonder if this scaling is appropriate for oceanic fields.

In the atmosphere the millenial anomaly simulated by CLIMBER at NGRIP is about 5-6 degrees, this is why you have roughly a beta that oscillates between -1.5 and 1.5 (amplitude 3) to reproduce a stadial to interstadial of about 15 degrees. In the ocean, CLIMBER also simulates SST anomalies of about 5-6 degrees around the British Isles, meaning that your oceanic temperature during certain DO events can increase by more than 15 degrees. Is this supported by any SST record? This makes me wonder about your experimental design that puts a critical weigh on the ocean. . .

- On the method, 2. Your base value for sub-shelf basal melting rate is 0.1 m/yr. Since you have a linear basal melting rate perturbation (Eq. 14), given your oceanic anomalies and a Kappa at 5 m/K/yr, for negative values of Beta (roughly half the time) you end up with B(t) <0 (i.e. B(t) imposed to 0). Your perturbation is then mostly going towards one direction (more melt). This might explain why you have this negative trend in ice volume in OCN experiments. I think this base value of 0.1 m/yr play an important role in your model setup but is not convincingly justified nor discussed. Also, why this parameter has be spatially homogeneous? Without knowing the actual value, we can expect very different sub-shelf basal melting rates in the Kara area compared to the British Isles area.

- Figure missing. It is hard to have a clear picture of what the actual forcing looks like as there is an important piece of information missing. I strongly suggest you to add an additional figure right after Fig. 2 in which you show the SMB and oceanic perturbations for a typical DO event (e.g. beta* from 1.5 to -1.5). I understand that there is a geometry feedback and that beta* is not constant but you can easily take your spun-up 40k ice sheet and show deltaSMB=SMB(beta=1.5)-SMB(beta=-1.5) (along with the equilibrium line in the stadial). And the same for Bmelt. This is a way to show the forcing that the ice sheet model is experiencing. I would ideally like to see the same kind of anomaly for the SAT, SST and sub-surface temperature.

Specific comments

P1 L18-20 This is a strong assertion which seems overconfident to me based on the limitations of the experimental design. Please remove.

P2 L2 Do you mean BKIS?

P2 L19-21 This is arguable. Climatically speaking, the two ice sheets are in a very different context (latitude, AMOC, storm tracks...)

P2 L31-32 No direct evidence for ice volume but ice extent.

P3 L1-2 Since the Greenland ice sheet is included in your geographical domain, is this also reproduced in your simulations?

P4 L2-3 Perhaps you could include a section in the discussion on the limitation of the floatation criteria on a 40km grid resolution, as this is though to be inaccurate to compute grounding line migration. Do you think you would have different grounding line migration sensitivities with a much higher resolution at the grounding line or with a analytical flux at the grounding line?

P4 L26 When using the PDD method, you are discarding the role of insolation changes. Could you add a justification on why this is negligible?

P5 L23 Again, it could be nice to have a plot of the stadial to intersadial temperature change in the atmosphere and in the ocean from a “typical” DO event (beta from -1.5 to 1.5).

P6 L7 To facilitate the reading your standard value of Kappa can appear here.

P6 L17 Bgl not presented before.

P6 L21 Why 750m? It seems relatively low as we have ice shelves today at much greater depth in Antarctica.

P7 L11 See general comments. Justify/discuss the importance of the chosen value.

P7 L29-31 P8 L1-3 This is not clear to me why you did not use the 3D field computed from CLIMBER3-α. Since the ice sheet model provides you the depth of the ice base you can easily read the temperature simulated by your climate model at this depth.

P7 L5 Section 5 is the conclusion.

P8 L25 What it the volume of your spun-up ice sheet? How small is 1.5 m sle relative to this volume?

P9 L21-25 This is unconvincing because a map of SMB changes from stadial to interstadial is missing. SMB is negative at the continental margins, from the BIIS to the BKIS. From your equations, it seems that you impose an important change in surface temperatures (please show as well annual and July temperature changes!) so it is hard to picture why melt is restricted to a narrow band in the South as you imply.

P9 L25-28 Does CLIMBER3-α provide oceanic temperature changes below the 40k ice sheet? How this is possible? If not, how do you compute the sub-shelf basal melting rate when the ice sheet retreats from its initial position?

P9 L25-28 It might be worth noting that if basal melting is more efficient than surface mass balance this is because you have calving in the ocean. Calving is a very efficient way to remove ice (confirmed by your Fig. 5).

P10 L8-9 The retreat pattern of Fennoscandian ice sheet is somewhat surprising. It seems that the ice sheet retreats increased basal melting in the Baltic sea (which is a lake in your setup right?)?

P14 L5 Alvarez-Solas et al. (2013) show that is the subsurface warming caused by AMOC slowdown is responsible for LIS H-events. When subsurface temperature is used here you end up basically with the same synchronisation for EIS and LIS. It is not really convincing to use subsurface temperature for one ice sheet and surface temperature for the other. Again, GRISLI gives you the depth of the ice shelf base so you can use the CLIMBER3-α layer corresponding to this depth, for the LIS and for the EIS. In this case, the study would have been more convincing. Consider reformulation

here.

Fig. 2 Please mention in the caption that these fields are later scaled to reproduce the NGRIP stadial to interstadial temperature change (temporally variable factor but roughly 3 times the changes simulated by CLIMBER3-α). Otherwise this figure might be misleading.

Fig. 2 Around the coasts of Scandinavia you have a CLIMBER SAT anomaly of about 9 degrees which means that during certain DO events you have episodically a local temperature change of about 30 degrees (beta* from -1.5 to 1.5). I am surprised that such a temperature change do not translate in large SMB perturbations. Any comment?

Fig. 5 What are the dashed grey lines? They do not seem to relate to the major tick marks.

Fig. 6 The southern edge of the BKIS (Taymyr peninsula / Ob river) seems almost not changed in ATM before and after the DO event. You have a beta* change of almost 2.5 (roughly -1 to 1.5) meaning that you have a change in annual temperature of at least 4x2.5=9 degrees. The southern extension of the BKIS is limited by melt. With an additional 9 degrees in annual temperature (how many in July?), it is not obvious to me why you do not have any melt increase there.

Fig. 7 Episodically the ice shelf extension is abruptly rising (e.g. 45 kaBP) not necessarily linked to any significant change in beta*, ice sheet velocity nor calving. What is the reason for that?

Supp. Mat. Fig 3 : the standard deviation in ice volume is not a good indication of the amplitude of millenial oscillations. You should correct from the background linear trend or simply compute the standard deviation of the dVdt variable. From the graph on the left, it seems that you do have oscillations of about 2 m sle for certain PDD parameter combination but maybe at a lower frequency. Could you comment on that?

Technical corrections

P6 L31 boundary

Fig. 5 Problem in the caption.

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