The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-112-RC2, 2020

The Cryosphere
Discussions

Interactive comment on "Sensitivity of the Antarctic ice sheets to the peak warming of Marine Isotope Stage 11" *by* Martim Mas e Braga et al.

Anonymous Referee #2

Received and published: 18 June 2020

Review Braga et al.

Braga et al. manuscript focuses on the Antarctic Ice Sheet contribution to MIS11. To understand what are the main driver of a potential AIS retreat during the MIS11c interglacial peak, they propose to explore the impact of climate forcing, initial geometry and global mean sea level history on AIS dynamics. They used a stand-alone SIA-SSA ice sheet model forced by idealized climate forcing: Pre-industrial climate is varied using the anomaly between Pre-Industrial and Last Glacial Maximum, scaled back to older times using Antarctic ice core records. Conclusions based on the simulations is that WAIS retreat is driven by the duration of the warmth during MIS11.

Although the aim of the manuscript is highly interesting, I find that many aspects have not been treated with care and part of the discussion is not conclusive because of the

lack of deeper analysis of the simulations and their forcing. It is clear that a tipping point appears in the simulations, probably resulting from oceanic forcing. Thus because of this, it is impossible to answer to the questions posed and impossible to conclude that the WAIS retreat int he simulations results from the duration of the interglacial during MIS11c. Some additional simulations are necessary to answer the 2 question posed in this manuscript. Especially, simulations testing the impact of the ocean forcing on the AIS retreat, thus by selecting lower ocean forcing. As they are, most of the results here could apply also for MIS5.

Many methodological aspects are not well described: methods section is poorly written, many details are missing about the ice sheet models (calving, sliding, sub-glacial hydrology, grounding line, surface mass balance) and about the forcing themselves. The generation of forcing deserve more figures in the supplementary since they are key to the conclusions of this manuscript. In the supplementary, comparison is made between simulated pre-industrial climate and present-day ERA5 fields, which is clearly incoherent. The generation of ocean forcing is also poorly documented here with again a comparison done between Pre-Industrial parameterized ocean forcing and ocean forcing inferred from observations (I guess here Rignot et al, but this is not mentioned).

Analysis is sometimes superficial: no really discussion of the impact of oceanic forcing in the simulations, no comparison between the various terms of the mass balance through time (it could be done for different sectors of the AIS). Thus based only on the figures and analysis in the manuscript, many conclusion remains highly speculative. I think the manuscript has some potential. But substantial work is needed in addition to the one presented here before publication. Therefore I recommend major revisions.

General Comments:

I find inconclusive the set of experiments to determine whether or not the duration of the interglacial is responsible for AIS retreat rather than a warm peak as for MIS5. This is because the index derived from ice core records mainly impact on the oceanic forcing of the simulation which generates a tipping point. Once the tipping point is crossed, then the duration of the interglacial does not matter at all to explain the amplitude of ice sheet retreat in the simulations. In simulations using Vostok-GI, ice volume is lower but because the GI does not yield too warm temperature. Thus the retreat is moderate. And in the case of EDC and DF, the retreat is comparable although DF presents a much longer peak for the interglacial. Actually, in question 2, ocean forcing is not mentioned at all as a potential driver of the ice sheet retreat. Same for ocean forcing: I would like to see a Figure in the supplementary of the oceanic forcing derived with the GI for the main ice shelves. You should discuss the impact to force WAIS with EAIS ice core records on the amplitude and timing of this retreat. For example, comparing them with WAIS divide ice core record. In those simulations, all forcing co-vary: your surface climate forcing and your oceanic forcing are modulated with the same index. It is likely not the case as atmosphere cools or warms faster than ocean does. This is not accounted for here. You could perform some interesting tests that would provide a nice discussion about the interaction between ocean and ice sheet. A plot showing air and ocean temperature forcing versus WAIS ice volume evolution; same for EAIS for all simulations is really necessary to support or explain better some aspect of this manuscript and provide answers to questions 1 and 2. Methods: in general the Methods are poorly explained, as well as boundary conditions (geothermal heat fluxes, bed topography etc…). Physics of the model is poorly described but some aspects are discussed within the manuscript. I would like to see a comparison between present-day simulated climate forcing and ocean forcing and observation from ERA5, and not between simulated PI and present-day ERA5.

Comments:

Line 47: please cite cite Tzedakis, P. C., et al. "Interglacial diversity." Nature Geoscience 2.11 (2009): 751-755.

Line 74: please cite De Boer et al. (2015) PLISMIP-ANT paper on which Dolan et al (2018) is largely based.

Line 74: please correct with "agree with how ANTARCTIC surface air temperature evolved"

Lines 79-82: I strongly disagree with this paragraph. Lost of long-term transient simulations have been performed, including MIS11, and you cite all those contributions in your introduction. I think what you mean is that no study really tried to improve the current simulations of MIS11-AIS, neither with climate forcing or ice sheet modeling, in absence of geological constraints on both climate and ice dynamics. Please reformulate this way, this much more honest. State that your aim is to improve by exploring aspects on which nobody really focused on so far (e.g. the two questions you pose at the end of this paragraph).

Line 89: correct as follows "of uncertainties in sea level reconstruction, and of uncertainties of the geometry…"

Figure 1: If the starting AIS topography is present-day (BEDMAP2 or other) please state it in this caption as well.

Figure 1: Are you sure that the glacial tongue in the Wilkes Land corresponds to Ninnis Glacier and not to Mertz Glacier instead?

Lines 101-102: please invert the order of the two sentences (put together everything about ocean forcing and then put the rest).

Line 105: "i.e., apply a transient surface temperature signal from the EDC ice core (Jouzel et al., 2007)". But Jouzel et al. only provide a temperature anomaly, what is your baseline climate forcing here for this thermal spin-up tase and then for the 5,000 kyrs geometry adjustment afterward?

Line 107: geometry is that of present-day, please specify which one and cite the reference (BEDMAP2, ALBMAP…).

Line 107: "We then let the AIS freely adjust for 5 kyr, between 425 and 420 ka": what is the ocean forcing for this 5,000 years free run? It seems to me that the topography shown in Fig1 is really present-day. Is this really the AIS topography that you obtain after those 5,000 years of geometry evolution?

Line105-107: Please detail ALL the forcing, boundary conditions (geothermal heat fluxes, etc..) used for the entire spin-up procedure this 5000 years (even in the supplementary if you prefer). All experiments presented here, including the spin-up, must be reproductible.

Table 1: Do you really use only one enhancement factor (the same for both SIA and SSA)? If yes please indicate it within the Table.

Table 1: what about calving? How is this done?

Table 1: Why is the relaxation time set at 1 ayr while characteristic time is 3 kyr? Please provide a detail description in the supplementary about the choice of your parameters. Also provide a description of the sliding law, surface mass balance in the Supplementary.

Table 1: Units for salinity is "PSU", please fill the missing units.

Table 1: Please explain in the Supplementary how you choose the value for the thermal mixing coefficient (it varies quite a lot and this one of the main important parameter of oceanic parameterisation)

Table 2: Please substitute "Age scale" with "Age model".

Table 2: please provide a more detailed caption for this Table. What does "Age (ka)" corresponds to?

Table 2: Add a column to state what is the nature of the record (either dO18 or dD and it record is glaciological or marine).

Subsection 2.2: In this paper your focus is on MIS11. Can you explain why you chose to scale the ice cores isotopic records to the difference between LGM and PI? Thus because of this, how much do your glacial index scaled surface temperature differs

from the temperature form ice core records at DF, EDC and Vostok? I would like to see a Figure showing the derived surface air temperature for each GI and in comparison with each temperature reconstructions from dD for each ice cores used in this study.

Lines 123-124: I don't understand the choice of CCSM3 since many other runs form CCSM4, even earlier versions of CESM, were released by Otto-Bliesner's group for contribution to PMIP3 on CMIP5 platform for both PI and LGM, run by NCAR, on the same computer. CCSM4 presents strong improvements relative to CCSM3. I would like to see a discussion about this and related literature for both version CCSM3 and CCSM4 in the Supplementary.

Lines 126-127: On the contrary, I would like to see a few panels about simulated Antarctic climate and associated biases since it is also highly important to your study. Thus I am expecting you to also provide a bias correction to your forcing field (assuming the bias correction propagates linearity back in time). This is something that you did not do, but it needs to be done. I also expect to see a figure of surface air temperature over Antarctica and comparison with all available ice core records for LGM (not only the few that you consider here), to have a comprehensive view of the performance of your climate forcing.

Line 134: Please substitute "age scale" with "age model".

Line 140-141: I don't understand how you can compare dO18 from marine sediments and dD from ice cores and deduce that Holocene temperature history is inconsistent between those two. First of all, it is not straightforward to compare marine and glacial records togethers. To me this figure 2a does not make any sense, remove it.

Subsection 2.3.2: Please refer to Figure 7 to show the filtered GI.

Subsection 2.3.3: I find the choice of your sea level curve a bit awkward. Why not considering also Waelbroeck et al (2002) which also encompasses MIS11 and which is one of the best curve we have with Bintanja et al. (2008).? Actually, many other new isotopic reconstructions have been done (e.g. Sutter et al., 2019), which is performed with more recent versions of models that Bintanja. Please redo some simulations also considering at least Waelbroeck et al (2002) in your ensemble.

Subsection 2.3.4: The methodology to provide intermediate geometries is definitely highly science-fiction. One can provide geometries, even though idealised, but with a more appropriate approach. For example: you could have done an equilibrium simulation with LGM conditions scaled with your GI for a few tens of thousands of year, and then transiently vary your climate forcing as in your control experiment until beginning of MIS11. This is a much better alternative than what you propose here. Or, alternatively, you can start one glacial cycle ahead and transiently vary your climate forcing with your GI. Then you could have used your the various GI generated with your scaling ensemble to vary the slope of transition from glacial to MIS 11. I strongly suggest you to try this way since, at least, you can justify much better your geometry ensemble than how you defined it currently.

Line 194: I think that the Figure number is wrong, it should not be Fig. 6.

Line 191-204: I am not sure in which Figure I can see the corresponding GI. Figure 4? If yes, I don't understand why you say that LR04-GI does not warm above PI temperature. PI temperature in Fig4a is given by 0 (the dashed horizontal line) right? To me LR04-GI goes beyond, even if not a lot.

Figure 4: Please put a horizontal dashed line corresponding to present-day AIS volume (26.9 x10ˆ6km3).

Lines 205-213: Actually, the amplitude of T° increase between al curve is broadly the same, as shown on your Figure 4a. The difference resides in the fact that LR04-GI starts with colder conditions that the others. However the ice volume evolution also decreases for a long time event with LR04-PI, however, because initially it the AIS grows, then it can not retreat beyond present-day extent during the peak of MIS11c. Vostok-GI yields a decrease in ice volume of the same order than LR04-GI, about

2x10ˆ6 km3.

Line 218: What I see on Fig 4b is that there is a tipping point, a threshold from which the AIS retreats very fast. Thus, instead of warming rates, I see that when temperature reaches a certain threshold, the ice sheet reacts fast. For example, the Vostok curve is initially the warmest and thus the initial crease in volume is the strongest. Then the GI stabilises compared to the other and the volume decreases slow down. I would thus reformulate the analysis more in terms of tipping points and thresholds.

Line 224: What about surface melt? Do you have any in your simulations? What method is used to calculate surface melt? Please provide detail about it in the Supplementary.

Line 223-228: Could you provide a figure.

Figure 5: I would be nice to have a contour for SMB= 0m/yr, so to understand which area are subject to surface melt.

Figure S2: you can't comparison between PI climate and ERA5 fields. . .this makes no sense. Please modify this figure and show a comparison between present-day CCSM3 fields and ERA5 instead. Same for basal melting comparison: you can't use PI fields and compare with present-day inferred basal melt rates from Rignot et al. By the way, Which reference did you used in c) for basal melt rates?

Line 241-244: Thus why did you use an average over the last 10 kyrs. . .this does not make sense, because orbitals are varying so much. Please remove the corresponding results from the manuscript.

Line 249: I disagree. Trajectories are the same, they are only delayed, please reformulate.

Line 254: "This effect seems to be non-physical, and a result of the delay introduced by the low-pass filter. " âĂŤ-> The effect is physical, this is the result of your delayed curve. Please remove this sentence. Because it is not the point here.

Line 256-259: “The 1 kyr low-pass GI is the only one that still preserves some higher-frequency variability ”. I don’t this on the Figure, I disagree. None of the filtered curve preserve the high frequency visible on the original EDC record.

Subsection 3.5: the only difference visible is before the threshold at 412k for EDC and DF index. This is because there is this threshold that initial geometry does not impact on your results. Basically, ocean forcing is driving all your scenarios. To see the difference in initial ice sheet geometry, you should turn-off the ocean forcing. But this wouldn’t make sense. So the conclusion here is that ocean forcing is driving the initial trajectories until 412k, the tipping point. Thus is it not surprising that initial geometry does not matter too much. There is one thing you have not tested though here, is the variation in ocean forcing. Those tests makes also a lot of sens because ocean forcing has this tremendous effect on your simulations. Thus I would like to see a couple of other transient simulations with lower ocean forcing. And thus, try again your geometry scenarios with the difference ocean forcing rather than with EDC-GI or DF-GI.

I also would like to see a figure in the supplementary showing the Tforcing for each GI.

Figure 12: Please also add total AIS sea level contribution on the figure for each geometry.

Line 321-323: Please show some calving fluxes against oceanic warmth because you never really discuss calving, neither describe the calving method used here. Put this in the supplementary.

Line 340-353: I really would like to see a specific figure in the Supplementary of temperature forcing derived from GI for each ice core records and compared with the temperature reconstructed from dD of those ice cores.

Line 349-350: I completely disagree with this statement. On your Figure 1, you can definitely see that this is because EDC-GI and DF-GI yield temperature warmer than those I Vostok and thus it is a matter of tipping point rather than duration. . .

Line 391-392: “WAIS collapse was caused by the duration rather than the intensity of warming ”. I don’t see how you can conclude this here. I find the entire set of simulations rather inconclusive for this aspect. There is a tipping point in all the simulations shown in Figure 4. However, the amplitude of contribution to sea level is determined then by the magnitude of the warmth during the peak rather than the duration of the peak itself. Actually the ice sheet retreat in a very comparable way when using EDC and DF, which have a different peak duration. . .IN fact there is almost no significant difference between them in Figure 12 as well.

Line 395-400: Instead of just stating it, show it. Plot air and ocean temperature forcing versus WAIS ice volume evolution; same for EAIS.

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