Antarctic climate response in Last-Interglacial simulations using the Community Earth System Model (CESM2)

by Berdahl M., Leguy G. R., et al.

General Comments

In this study the authors present CESM2 simulations of the Last Interglacial climate following the CMIP6-PMIP4 protocol. Compared to previously published CESM2 PMIP4 simulations, they use here a 2degrees nominal resolution for the atmosphere and land models and replace the PI vegetation (as per protocol) with a 'potential vegetation' considered to be more representative of the LIG climate since urban areas are not present. All other boundary and initial conditions as per protocol.

Authors present results from a LIG climate equilibrium simulation (at 127ka) and a LIG climate freshwater experiment (FW). A preindustrial (PI) simulation is also discussed. They focus on analysing the Southern Ocean and Antarctic response to the LIG and freshwater forcing. Particularly, the authors draw the reader's attention to a possibly overlooked climate system response to the freshening of the North Atlantic: a cooling in the subsurface ocean near the Antarctic ice sheet.

Generally, I found the manuscript clearly written, motivated and with a good introduction to the topic. The length of the simulations here presented (1000 years for PI and LIG, and 4000 years for the FW experiment) is certainly a point of strength of this study, given that fully coupled climate simulations this long are not frequent.

I have a number of (overall) minor comments that I ask the authors to address before recommending the manuscript for publication.

The feeling I had while reading the paper is that often differences between the model runs are described without providing a thorough physical explanation of why they exist in the first place. Particularly, I refer to the warming of the Southern Ocean, at depths, for the LIG experiment and to the subsurface cooling of the Antarctic waters for the FW experiment (which is also the main result of this study).

Specific Comments

Line 12: is ‘new’ the right word? For example, Guarino et al. 2023 showed this too for PMIP4 HadGEM3 simulations (see their Figure 3 and Suppl.Fig.1).

Lines 56-75: you mean similar runs using CESM2? the literature is quite vast for freshwater experiments under LIG conditions using different models.

Line 66: I don’t understand this sentence: “in anticipation of running coupled experiments in future work”. These experiments are already coupled, aren’t they?

Section 2.1: the length of these simulations is a point of strength of this paper. However, what is the model spin-up? (see also my comment below).

Line 112: why using only 50 years when you have 1000? 50 years will not be enough to sample the climate variability of processes that manifest on longer timescales (e.g. centennial climate oscillations). Is the spin-up period included in these 1000 years? this is not mentioned in methods.

Lines 112-113: Please add more details about the analysis presented in section 3.2, as it was done for 3.1. For example, how are anomalies computed? (FW-LIG?), how many years of simulation did you use for your analysis?

Lines 130-132: can you show the increase in the westerlies in your 2degree simulation too? In Otto-Bliesner et al., 2020 this point is indeed made but I could not find a figure showing the stronger westerlies (I have not checked if there are SI figures, but there is no mention of this in their paper).

Line 133: “(..more vigorous transport, not shown)”
I suggest you include this in your Appendix, this is a rather crucial aspect for your following analysis of the SO (see also my comment about this at line 189).

Line 134: where is this shown?

Lines 168-169: important point, if robust, add to conclusions(?)

Line 170: is the regional cooling in these sectors caused by sea ice changes?

Line 189: SST warm anomalies can be linked to the LIG insolation forcing, but what is the dynamical link between LIG forcing and SO warming throughout the water column?
Is it maybe the increased global oceanic heat transport under LIG forcing discussed in Otto-Bliesner et al. (2020) that was mentioned earlier? Is the increase in ocean heat transport for the LIG versus the PI shown in Otto-Bliesner et al. (2020)?

General comment on section 3.1.2: up to this point in the paper changes between mean state PI and LIG are described but not discussed, why are we seeing these changes? what is driving them? why is the ocean warmer at depth during the LIG? why are the westerlies weaker?

Figure 4, caption: "Climatologies are computed over the last 50 years of each simulation."
You can omit this, as it is in your methods.

Figure 5, panel e: I do notice at the very beginning of your 127kaFW-SH timeseries a small bump, suggesting SH sea ice initially increased before starting declining. Have you looked into this? (I am asking out of personal curiosity).
It is difficult for me to appreciate from this graph how many years it took for the sea ice to begin melting. This might be the transient sea ice response that we discussed in Guarino et al., 2023, and the subsequent decline in your simulation might confirm the two time-scales response of sea ice first proposed by Ferreira et al. 2015 and then invoked by Guarino et al. 2023 to explain the increase of Antarctic sea ice under H11 forcing.

Line 225: should "127ka" be 127kaFW?

Figure 7, panel a: "over the Antarctic continent"
How is this defined? land points only?

Figure 8: This figure is key to the paper results, and I would like to understand it better. In particular, I would like the authors to better substantiate some of their analysis. There will be some repetition between my remarks here and some comments below.

If we look at the Weddell and the Ross/Amundsen Sea sectors, they behave in opposite ways, and I feel this aspect is not sufficiently discussed in the manuscript.

Even more important, I don't understand why the authors talk about upwelling of cold waters (line 359). Generally, an increase in surface winds (and particularly in the negative wind curl in the SH) means, yes, increased upwelling (because the flow is divergent at the surface) but of relatively warm waters. This is a well-known mechanism for triggering open ocean deep convection and polynyas formation in CMIP models in the Weddell Sea.

In fact, if I had to guess, I would think that the weaker (with time) winds and wind stress over the Weddell weaken the Weddell Gyre and discourage the occurrence of open ocean deep convection in the FW experiment compared to the LIG. Less Deep convection could imply less warm water entering the 200-800m water column, and thus the negative MOT anomaly persists in the Weddell.

The same is not true for the Ross Sea.
In the Ross, as I also say in my comment at line 281-283, I can't really appreciate a strong trend in wind stress curl, but if there is one (as I think the authors say) then I would expect the same mechanism as above to be active: upwelling of warm waters as the Ross gyre spins up. Some models (not all) do simulate deep convection also in the Ross Sea (see de Lavergne et al., NCC, 2014) although I don't know where CESM stands on this.

Is it possible that the upwelling of warm waters in your bottom panels is linked to this?

Finally, I believe the establishment of the initial MOT negative anomaly needs explaining.

Line 264: "Within the first 500 years, a bulwark of cold subsurface water wrapping around the continent"
What is driving this?

Lines 280-281: is this shown? does this mean that the SAM remains more positive than the LIG during the whole LIG-FW experiment?

Lines 281-283: Okay the physical mechanism, but in Fig.8 the wind stress weakens with time, particularly in the Weddell. As for other regions, Ross and AS, if there is a trend (i.e. wind curl becoming more and more negative) this cannot be appreciated from Figure 8 where I see almost no difference from 500 to 4000yrs.

Lines 286-287: I feel that the reasons for the subsurface cooling in the SH are not well explained anywhere in the manuscript, if this has been shown in other studies references plus a short description of the physical mechanisms at play must be included.

Line 291: has ‘CDW’ been defined before?

Line 338: Is this truly unexpected? There is some literature about this.

Guarino et al.,2023 (as you mention later on) and some of the references in there provided:

Crowley, T. J. and Parkinson, C. L.: Late Pleistocene variations in Antarctic sea ice II: effect of interhemispheric deep-ocean heat exchange, Clim. Dynam., 3, 93–103, 1988.

Renssen, H., Goosse, H., Crosta, X., and Roche, D. M.: Early Holocene Laurentide Ice Sheet deglaciation causes cool- ing in the high-latitude Southern Hemisphere through oceanic teleconnection, Paleoceanography, 25, PA3204, https://doi.org/10.1029/2009PA001854, 2010.

Lines 341-342: I agree on this.

Lines 353-355: Well, this point exactly is the one we make in Guarino et al. 2023, in which the short timescale response is analysed and where HadCM3 results are invoked to explain the long time scale response.
In fact, it would be quite interesting to take a look at your first 200-300 years of simulations and see if results are consistent with HadGEM3 ones, confirming the transient sea ice response.

Line 370: Again, I feel this dynamical response of the system has been well explained. Why does this happen in the LIG experiment?

Lines 385-389: This paragraph seems a repetition, the Discussion section begins in the same way. Consider keeping only one of the two.

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