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Comment on cp-2021-187

Juan Muglia (Referee)

Referee comment on "The first 250 years of the Heinrich 11 iceberg discharge: Last Interglacial HadGEM3-GC3.1 simulations for CMIP6-PMIP4" by Maria Vittoria Guarino et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-187-RC2, 2022

Guarino et al. produced a manuscript on simulations of Heinrich 11, a period of land ice melting during the Last Deglaciation. Because of computational constrains, the authors only show results of a 250 y simulation, a limiting factor for a study of global climate that includes the deep ocean. This limitation is somewhat mentioned in the title of the manuscript, which explicitly states the length of the simulations.

Comments

In general, 350 y, which is the time that the LIG simulation was run, is not enough time to equilibrate the physics of the deep ocean. See for example Marzocchi and Jansen (2017, GRL), where they have to run CCSM4 with LGM boundary conditions for several thousand years to achieve physical equilibrium in the deep ocean. The authors should give evidence that their LIG simulation is in equilibrium, or explain why such requirement is not needed in this study.

Concerning the 250 y H11 simulation, it is necessary to distinguish between the 250 y of a computer simulation and the first 250 y of H11 in climate history. If the authors want to claim that their 250 y simulation represents the actual first 250 y of H11 they should give evidence of the correct representation of the timing of physical processes in the atmosphere and ocean. If this can't be done, then I would switch the title to "A 250 yea simulation of the Heinrich 11 iceberg discharge: Last Interglacial HadGEM3-GC3.1 simulations for CMIP6-PMIP4", and all suggestions in the manuscript that this is an actual simulation of the first 250 y of H11 should be changed accordingly.

In several places of the manuscript the authors mention that a longer simulation cannot be performed due to computational constrains. However, conjectures are made about the evolution of some processes beyond the length of the H11 experiment (e.g., lines 255 and 295). Could a 500 y H11 experiment be performed before the publication of this paper? I think it would benefit the science shown, increase the relevancy of this work, and would be beneficial for the science community. In its current state, the analysis is limited to a transient 250 y run, and not all processes governing the H11 climate have manifested (according to the authors). In addition, little evidence is shown that the 250 y presented in the manuscript is a correct representation of what happened during the onset of H11, and not just an abstract modeling study (see my comment about correct representation of timing).

Lines 15-20: The authors should better describe the LIG and H11 climate periods. What was the length of LIG, and of H11? Did H11 occur at the onset of the LIG, or somewhere in the middle? This is important in order to put the reader in context of the climate period we are discussing.

Lines 16-24: "warming of the Southern Ocean during this time is attributed to a slowdown of the Atlantic Meridional Overturning Circulation (AMOC), which has been suggested as a mechanism to explain the 2-3°C Southern Ocean warming found in Southern Ocean and Antarctic climate records" Are the papers mentioned to justify this statement modeling-based or data-based? A brief description of them would be important to understand the scientific basis of this work.

Line 79: N_2O not N20.

Line 125: The relation between heat transport and AMOC in interesting. However, a more complete analysis should include salt fluxes as well. How do they change between the LIG and H11 simulations? Do they have any relevancy for AMOC? How are they affected by the North Atlanitc hosing?

Line 195: Could you show the changes in meridional heat transport for the other ocean basins? (in the supplement is OK) Although this work focuses on the Atlantic, effects in other ocean basins may be relevant for readers.

Line 208: In Figure 10 I don't see any subtropical gyre intensification for H11.

Lines 249-255: Could the absence of a dipole in the Antarctic sea ice response to a positive SAM be due to issues with the sea ice model, like for example lack of resolution or limitations in the parametrization of processes? Please expand in the paper. Could you show the sea ice in winter months in the supplementary material?