Comment on cp-2021-94
Anonymous Referee #2

In this paper the authors present and elaborate on a conceptual box model of the ocean-atmosphere coupled to a simple ice sheet model, where ice sheet mass balance is tied to ocean temperature. The ocean-atmosphere model is bistable and the different circulation modes result in growing or melting of the ice. Switches between different ocean states can be triggered by changes in orbital forcing. The model is then applied to explain some of the major transitions in Quaternary climate dynamics by arguing that the global convective flux changed with Pleistocene cooling, giving rise to different model dynamics.

The argument that continental ice sheet dynamics is controlled by ocean dynamics is new and goes against the rather well-established Milankovitch theory that postulates that glacial cycles are controlled by summer insolation through its effect on ice melt. This paper still represents an interesting, although highly controversial, piece of work, but should not be sold as having solved the major 'Pleistocene puzzles'. Conceptual climate models (and this is just one of many out there) can be useful to understand particular features of a complex system but I don’t think that those are the tools that will allow us to resolve all 'Pleistocene puzzles'. Physically based, spatially resolved, coupled climate-ice sheet models, whose parameters can be directly inferred from observations, are required for that.

General comments

Changes in the AMOC state do affect summer temperatures over North America and Europe (e.g. Jackson et al., 2015), but the magnitude of these changes is comparable or smaller than the direct changes in surface air temperature over land induced by changes in orbital configuration. Also, incoming solar radiation in summer is undoubtedly important for the mass balance of an ice sheet. It therefore seems unlikely that the thermal state of
the ocean alone should determine the position of the southern ice sheet margin, as assumed in the conceptual model presented in this paper. Also, there are some reconstructions of AMOC variations over glacial cycles. So the question is: should this different MOC states, implied by the conceptual model as drivers of the glacial cycles, not be reflected in proxy reconstructions of the AMOC? Is this not an ‘observable’ that would allow to test the model?

I find the discussion about role of CO2 for Quaternary glacial cycles on Lines 53-62 misleading and incomplete. To better understand the causes of the MPT it would be fundamental to know how CO2 changed across this transition. There are large uncertainties in CO2 reconstructions for the pre-ice core era, and a gradual CO2 decrease over the Pleistocene is still a possible scenario (e.g. Fig.6 in Berends et al., 2021). A step forward in this respect will be represented by the planned drilling of Antarctic ice cores with ice as old as 1.5 Myr. Also, the fact that a possible long-term decrease in CO2 is a consequence of the higher amplitude glacial cycles rather than the opposite, as suggested by the author, is highly speculative. The claim that CO2 variations have only a minor effect on global temperature and on glacial cycles in general is not corroborated by evidence and no references are provided to support this claim. Simplified coupled climate-ice sheet models forced with observed CO2 do produce realistic variations of global temperature between glacial and interglacial states (e.g. Fig. 8 in Ganopolski et al., 2010).

The author mentions several times the need for tuning diapycnal diffusivity in ocean GCMs, and how that makes these models lose credibility. I can’t see how all the assumptions and approximations made in deriving the simple model described in the paper are better than tuning a single parameter like diffusivity in an ocean general circulation model.

The long-term changes in the global convective flux play a crucial role in the explanation of the various Pleistocene climate regime shifts in the model. A more quantitative description of how this parameter is supposed to change under global cooling would be desirable. It is mentioned that it would depend on downward LW radiation, but then a 10°C (!) mean Pleistocene cooling is assumed, which, considering that temperature difference between glacials and interglacials is ~6°C, seems unreasonable. Furthermore, I imagine e.g. clouds, wind etc would also play a role.

A continuous 3 Myr long time series of the results of the model would be interesting to see. In principle such simulation should be possible to perform by simply prescribing a scenario for the convective flux evolution over time.

Minor comments
Lines 221-223: sentence is not clear

Lines 308-310: the warming threshold for Greenland melt is probably at ~2°C (Gregory et al., 2020; Robinson et al., 2012)

Lines 318-320: ‘This is because the cooling implies a drier air hence a smaller downward LW flux, which then requires a smaller global convective flux for the ocean heat balance --- all else being equal.’ Could the author elaborate on this? It seems far from obvious to me. How reasonable is the assumption of ‘all else being equal’?

References


