Climate of the Past Discussions

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Comment on cp-2021-94

Anonymous Referee #2

Referee comment on "A theory of glacial cycles: resolving Pleistocene puzzles" by HsienWang Ou, Clim. Past Discuss., https://doi.org/10.5194/cp-2021-94-RC2, 2021

1. *I want to thank the referee for the highly constructive comments, which point to significant deficiency in my presentation. Some key points are not adequately made, which may lead to misreading of the paper. I attach my response and planned revision below in italics.*

In this paper the authors present and elaborate on a conceptual box model of the oceanatmosphere 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.

2. *As I stated in the first paragraph of the paper, I fully subscribe to the orbital forcing of the glacial cycles, my only difference is that this linkage is through the ocean and not through direct radiative forcing. That the ocean plays a central role in glacial cycles is not new, but strongly argued by Broecker and Denton (1989) more than thirty years ago. The reason that it remains overlooked could be because an interactive MOC in a turbulent ocean remains out of reach for numerical models. I will add this point in revision.*

3. *I agree that direct radiative forcing remains widely ascribed, but there are at least two arguments against it that seem fatal: First, the SST ranges over 10C through glacial cycles, which incurs similar change in the SAT (basically tracking each other, see Ruddiman et al. 1986, Fig. 4; Johnsen et al. 1995, Fig. 2), the latter is greater than that can be induced by direct forcing (no more than a few degrees, see Abe-Ouchi et al. 2013, Fig. 2b). And if the observed SAT were by direct forcing, then you have to reverse the*

above causality – against the second law. Second, the yearly ablation from the direct forcing has no precession signal because of Kelpler's second law (Huybers 2006). Even sophisticated models that use positive degree day (for example, Abe-Ouchi et al. 2013) may not have incorporated this constraint since one cannot assume a sinusoidal seasonal insolation. I will add this discussion in revision.

4. *I fully appreciate the value of numerical studies of glacial cycles, and a successful simulation without tuning key parameters would suggest the capture of proper physics. The present theory however has an opposite objective: instead of adding maximum physics for a realistic simulation, it seeks to isolate minimal physics sufficient to explain the observed phenomenon. The essential physics of the theory is contained in a single regime diagram (Fig. 2), and the only new element is a closure based on nonequilibrium thermodynamics necessitated by a turbulent ocean (discussed in detail in Ou 2018, including a synthesis of numerical results of the MOC hysteresis). Despite its initial guesswork more than forty years ago, MEP has entered the mainstream in recent years considering the books, special issues and symposia dedicated to the subject, so although its foray into the paleoclimate research may be new, it need not be controversial and certainly is justified as a working hypothesis. And then the potency of this principle in resolving seemingly unrelated glacial puzzles attests to its utility.*

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 undoubtably 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?

5. *Regardless the model result, it is an observational fact that SAT varies over 10C through glacial cycles, which is greater than that can be induced by direct forcing (see response 3). Solar insolation affects the mass balance only through summer SAT (Pollard 1980, ice absorption of SW flux is an order smaller), and this mass balance, in combination with the ice dynamics, is translated to the ELA in our model, which in turn determines the summer isotherm of the ice margin. Since summer SAT is anchored on SST, it is likely that the latter controls the ice margin. Indeed, in our theory, it is the freezing-point SST that causes LIS to extend to the subtropical front; I don't think this mid-latitude fixation of the maximum ice extent can be explained by direct forcing.*

6. *Absolutely the reconstructed MOC should provide an observational test of the model, and I have stated that SST, SAT and MOC all covary strongly, which are like that can be inferred from our regime diagram, and MOC of our warm MEP (Eq. 2) has been compared quantitatively with the observed one. On the flip side, simulations that do not allow interactive MOC or fail to reproduce its variation obviously fall short in capturing this key process.*

I find the discussion about role of CO_2 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 CO_2 changed across this transition. There are large uncertainties in CO_2 reconstructions for the pre-ice core era, and a gradual CO_2 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 climateice 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).

7. *The discussion indeed is too terse, which I would expand in the revision. Berends et al. (2021) use inverse model to determine what CO2 should be, but there is already observed CO2, so I don't exactly follow the logic. The tectonics is the only process of long enough timescale to cause the Cenozoid cooling, and given the fast CO2 equilibration, its trend through MPT (if present) or variation through the glacial cycles is likely the response than the driver of the climate change. Physically, this may be through solubility of CO2 as water cools and Honisch et al. (2009) have reproduced the observed CO2 of glacial cycles from SST via the carbonate chemistry. Observationally, CO2 correlates strongly with SST but lags by 2 ky (Petit et al. 1999); there is no plausible reading of this other than the stated causality, which thus is far from speculative.*

8. *CO2 change of 100 ppm amounts to several $W \cdot m^{-2}$ greenhouse effect, which would be dwarfed by that of the moisture (several tens of $W \cdot m^{-2}$), so it's unclear to me why the latter is overlooked in comparison with the CO2 effect (I like to be illuminated on this). Such CO2 change only causes 1-2C change in SAT (Broccoli and Manabe 1987; Petit et al. 1999), which nonetheless may shift the bistable thresholds to alter the glacial cycles, as seen in numerical models. This however returns us to my primary critique about glacial cycles anchored on an ice-free state. Clearly, the observed interglacial is not ice-free, and if it were, then there will be no ice-volume signal in early Pleistocene, as seen in numerical simulations --- in contradiction to observations. Our ice bistates however are not between finite and zero ice sheet, but between polar ice cap and LIS, which have avoided the above problem.*

9. *The foregoing problem notwithstanding, applying the CO2 trend to simulate the glacial cycles may be justified by differing timescales, but imposing observed glacial CO2 signal to simulate the glacial cycles strikes me as usurping the basic causality.*

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.

10. *The problem with the sensitivity is that even within the observational bound of the diapycnal diffusivity, the MOC can be either on and off, which suggests to me that the diapycnal diffusivity is not the control parameter of the ocean state, and it emerges as such only because the models do not resolve eddies. In a turbulent ocean, it is MEP that specifies the ocean state, which has no dependence on the diapycnal diffusivity. Any theory of the scope to explain glacial cycles necessarily involve myriad assumptions, but we have tried to justify them with physical arguments when necessary.*

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.

11. *This is a valid point, and I will expand the discussion to include quantitative estimates. The global-mean surface heat balance is among the absorbed SW, the net LW and the convective fluxes. During the Pleistocene cooling, an increase of planetary albedo by 0.1 by the expanding glaciation would decrease the absorbed SW flux by 30 $W \cdot m^{-2}$, a 10C cooling would increase the net LW flux by also 30 $W \cdot m^{-2}$ (due to drying air, Ou 2001, Fig. 2), so they reinforce each other to reduce the convective flux by 60 $W \cdot m^{-2}$. This is huge, which in fact supports the robustness of its decrease even though it may not be as large. As the above estimates should at least hold in their orders of magnitude, there is no longer need for the qualification "all else being equal". Based on above estimates, the MPT markers derived in the theory may be crossed to offer a quantitative validation of our proposal.*

12. *The Pleistocene cooling is of order 10C, and superimposed on it, there is 10C range in the glacial cycles (Ruddiman and Raymo 1988, Fig. 3). The interglacial thus has similar temperature through Pleistocene, and the glacial of course is floored by the freezing point. There is nothing unreasonable about these temperature ranges. In addition, I have provided an explanation of this feature in the paragraph following Eq. (9), which is consistent with my Fig. 5.*

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.

13. *The timeseries are presented to provide a visual aid to the Stage 2 and 3 glacial cycles with their different convective fluxes. They are trivial products of applying the linear relaxation equation to the equilibrium state determined from Fig. 5. Producing a long timeseries amounts to juxtaposing the shown timeseries and smoothing their transition, which contains no additional information.*

Minor comments

Lines 221-223: sentence is not clear

14. *I will modify the sentence in revision.*

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

15. *I will modify the discussion to stress that it is an observational fact that Greenland is not ice-free either during interglacial or early Pleistocene, the only relevant point in our discussion. On the other hand, both mentioned studies are consistent with the modified statement that several degrees warming is needed for an ice-free Greenland (noting that global warming needs to be multiplied by the polar amplification factor of about 2 to get the regional temperature).*

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'?

16. *See response 11.*

References

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