Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
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Comment on cp-2021-94

Anonymous Referee #1

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-RC1, 2021

1. *I want to thank referee #1 for highly insightful comments, based on which I plan to overhaul the paper. In the following, I juxtapose my responses (in italics) to the referee's comments.*

The article claims to resolve "long-standing puzzles" with a new dynamical system model presented as the combination of an ocean bistable system coupled with an ice accumulation model. In essence, switches between two ocean circulation modes, emerging by application of the maximum entropy principle, and triggered by the astronomical forcing, control the growth and melt of continental ice. The direct response of ice sheets to insolation changes (Milankovitch theory) is neglected: everything is mediated by the ocean dynamics. The mid-Pleistocene transition is obtained by a reduction of the average convective flux and the 'long-standing puzzles' resolved here are (a) the absence of 400-ka signal (dominating eccentricity), the gain in 100-ka strength while eccentricity decreases, a so-called 'variable termination problem' associated with the variable length of ice age cycles, and another so-called 'polar synchronisation problem'.

The issue at stake here is that many dynamical system models may 'resolve' these puzzles, either with a synchronised oscillator or with non-linear resonance forced by the astronomical forcing. For convincing us of an actual 'resolution', the present model should have a clearly superior mechanical background compared to other models.

2. *I have provided a brief review as to why observed phenomenon has weeded out some previous resolutions of the glacial puzzles. One problem with the dynamical-system approach is that it contains free parameters that do not correspond to measurable quantities, so many such models are unfalsifiable (hence cannot be validated). Myriad mechanisms have been proposed to resolve different puzzles, the present paper in the least simply adds to this long list; but perhaps more significantly, it shows that by incorporating the well-justified ocean role and MEP, the paper may resolve all major puzzles at once. I venture that the quantitative derivation of the MPT and parsimony of the model physics represent a progress. I will further sharpen the above discussion in the revision.*

Neglecting entirely the direct ice sheet response to astronomical forcing is a provoking proposal, because there is so much evidence of direct insolation forcing of the net ice balance. The originality of the present setup is to use (following Ou, 2018) the maximum entropy production principle as a better way to capture the emerging heat transport by turbulent eddies. MEP is a fascinating but controversial topic. There is indeed a series of articles dating from the 2000-2010 decade that suggests a good success of MEP in predicting heat fluxes in systems with many degrees of freedom. However, some of its main proponents, including Deware and Jupp, follow the Jaynesian interpretation of statistical mechanics: they use it more as an inference than as a prediction principle. A match between observed macro-trajectories and MEP predictions is a suggestion that the right effective constraints on the flow have been identified (see, e.g. Jupp and Cox 2010).

3. *Observations only show that the ice volume and Milankovitch insolation are correlated, not whether the linkage is direct. The only direct forcing is through the atmospheric absorption of the SW flux, as manifested in PPD, but largely overlooked is that such PPD has no precession signal because of the Kepler's law. And then whatever the summer air temperature, it is still anchored on the SST, which varies over 10 C through glacial cycles. Leaving the causality question aside, it is hard to see how SST variation does not dominate the PPD signal. I will add this discussion in the revision.*

4. *One advance of Ou (2018) is to show that MEP could be a deductive outcome of the fluctuation theorem. Since the latter is of considerable mathematical rigor and has been tested in laboratory, the MEP is more than an inference but a realizable physical state. I recently came across a DNS by Hogg and Gayen (2020), which would provide additional computational support of the MEP. Although I have reviewed MEP in Ou (2018), I plan to update the review in the revision to highlight the above points.*

5. *MEP is no longer a quirky out-of-the-mainstream idea --- considering the books, special volumes and symposia dedicated to the subject in recent years, but despite its utility in addressing the generic climate state, including that of other planets (Ou 2001; Lorenz et al. 2001), it has not entered the arena of paleoclimate research. As this paper represents arguably first such foray, it naturally would meet resistance, but I hope the transparency of the physics and its potency in resolving all major glacial puzzles would ease this resistance.*

6. *In a forthcoming paper on abrupt climate changes, I will show that MEP may explain many their salient features as well, including post-Heinrich warming, the ensuing gradual cooling that anchors D/O cycles, and the dramatic reversal of deglaciation by YD. The result should further support the utility of MEP in our understanding of the paleoclimate.*

This is important as for example, l. 170--172, the authors argue that sea-ice presence at the LGM would imply heat loss and weakened entropy production, thus "in contradiction with the MEP". Not necessarily. If it were to happen, it would not be a contradiction with MEP. It would imply that an additional constrain (here, the fact that sea-ice can actually develop) needs to be taken into account. MEP does no magic, alas !

7. *I was wrong about the sea-ice, which I realized during my current research on abrupt climate change. That is, so long as SST is hovering around the freezing point, there is invariably winter sea-ice. The MEP thus states only that for millennial or longer timescale, there can be no perennial sea ice, a deduction that is in fact consistent with LGM observations (de Vernal et al. 2005). My argument about the summer open water however still stands, and I will add the above discussion in the revision.*

Hence, my preliminary assessment is that although it is plausible that ocean dynamics have gone through some distinct states during the Pleistocene and that switches between these states have been somehow paced by the astronomical forcing, the proposal remains too speculative for claiming to have resolved "long standing puzzles". Line by line comments

8. *The new physics is that the ocean is the primary regulator of the summer air temperature, and the coupled climate would tend to MEP, both are sufficiently grounded to be deemed speculative (see responses 3 - 5). In the least, they are justifiable as working hypotheses*

and it's their deductive outcome that resolves glacial puzzles. I will add the "working hypotheses" qualifier in the revision.

line 8: climate system is presumably "ocean"

9. *I will retain "climate system" since a bistable ocean hinges on air-sea coupling (via the convective flux).*

line 33: 'should emerge from fundamental physics... which is yet to be delineated'. Not sure exactly what the sentence claims. Of course a model that refers to so-called fundamental principles (of physics and, perhaps, of biology ?) is to be preferred to a statistical fit. But the attempt here is not the only one to refer to such fundamental principles. Verbitsky et al. 2018 ESD claims also to provide a model rooted in fundamental principles and scaling laws, but with a focus on ice sheet dynamics and basal heat flux.

10. *I will drop "fundamental" and rephrase the sentence. I will add a reference to Verbitsky et al (2018) in the Introduction. They have a free parameter (variability number) that can be arbitrarily set to match MPT and, since eccentricity plays no role, they cannot explain why rising eccentricity paces terminations.*

ll. 57-60 : It is correct that we can't tell for sure what the pCO2 before 800 ka, but the claim of a long-term decrease in CO2 is reasonable given Pliocene proxies for CO2, even if they are very uncertain. The "no evidence" seems excessive. Atmospheric CO2 having "only a minor effect on the temperature" is a strange sentence. Yes, the radiative forcing of a 20 or 30 ppm change is small compared to the radiative effect of large ice sheet swings, but there are enough numerical simulations to claim that it is still likely to be enough to make a difference between a deglaciation terminating a 40-ka cycle, and an aborted termination that merely produces an interstadial, eventually leading to an 80-120ka cycle.

11. *I will rephrase "no evidence" to 'equivocal". A 100 ppm change in CO_2 only generates about 1 C temperature change (Petit et al. 1999) while observed one is 10 C. Both the carbon-cycle simulation and observed time lag suggest that CO_2, with its short equilibration time, is likely a response rather than a driver of the temperature signal. Although even a small CO_2 effect (several Wm^{-2}) can amplify the hysteresis (a threshold phenomenon), one may not overlook the much larger radiative effect of the drying air (several tens of Wm^{-2}).*

l. 79 'bistability has been demonstrated by coupled models'. Demonstrated is certainly too strong. That particular experiment by Manabe and Stouffer used the controversial freshwater flux adjustment.
Yin and Stouffer, Journal of Climate 2007, for example saw CM2.1 having no stable 'off state' (though whether two states could be obtained with a different freshwater flux background is another question). The Rahmstorf et al. 2015 is an authoritative intercomparison that remains citable today and indeed shows hysteresis for all models, but only EMICS.
This said, the recent article by Alkhayuon et al. 2019, Proceedings of the Royal Society A, presents a nice bifurcation structure for a box ocean model that may be of interest to the author.

12. *In Ou (2008, Section 4), I have provided an extensive synthesis of numerical results of the MOC hysteresis. Manabe and Stouffer's (1988) bistability indeed is a happenstance depending on the diapycnal diffusivity (hence admittance) and the strength of the air-sea coupling. Regardless the bistability however, strong enough hosing always shuts off MOC, but this off mode may persist (that is, stable) only if the initial state is bimodal, which is the source of the different stability properties found by Yin and Stouffer (2007). My reference to Manabe and Stouffer (1988) is merely to show that their off mode is indeed characterized by vanishing convective flux, hence it supports my convective bound.*

On the other hand, the hysteresis discussed by Rahmstorf (1995) has no relevance to the glacial cycle since it is of the opposite sign: obviously a stronger insolation and warming should cause interglacial, not the cold off mode. This glacial hysteresis can only be facilitated by an admittance that is not fixed but propelled by MEP.

13. *One problem of the ocean box model, as considered by Alkhayuon et al. (2019), is that the off mode is characterized by a reversed THC, which has no practical relevance. It is for this reason I argued in Ou (2018) that Stommel's model (1961) falls short in providing a dynamical basis for the hysteresis produced by coupled models. It is the air-sea coupling (as seen in my regime diagram) that allows a weak but normal-signed THC, which can represent a glacial ocean.*

l. 102: 'glacial cycles are dominated by the subpolar temperature' : please expand on this.

14. *I shall rephrase the sentence to "since temperature variation during the glacial cycles is dominated by subpolar over subtropical water..."*

l. 136 - 137: see above

15. *See response 12.*

l. 141: admittance could be better defined.

16. *A key element of Ou (2018) is to identify the admittance as the ocean property that is subjected to microscopic fluctuations, which is based on the observed efficacy of random eddy exchange across the subtropical front. I shall add this discussion to better define the admittance in the revision.*

l. 155 : "veritable generalization" : see introductory comments

17. *I will change "veritable" to "plausible", also see response 4.*

l. 172 : see introductory comments section 3.1 and l. 401 : Numbers are not quite right

(though order of magnitude are ok).

The range of mid-June insolation at 65N over the last million years is 435W - 559W/m2, so 123.2 W/m2.
The range of, e.g., mean insolation over the summer season (JJA, defined astronomically) is about 80W/m2.
The range of annual mean insolation at that latitude is 7 W/2m.

18. *The actual forcing is the absorbed solar flux. I use the Milankovitch insolation only as a convenient proxy hence only its crude range is needed.*

l. 302 - 304: references on the stability of the Greenland ice sheet and its future fate definitely need an update (see, e.g. Van Breedam et al. 2020, Payne et al., 2021). Clarify also whether we speak of local temperatures or global averages. There is consensus for long-term commitment to melt Greenland for globally-averaged temperatures above around 2 deg C. Is the author disputing this claim ?

19. *It's the local temperature that drives the mass balance. The required warming from Van Breedam et al. (2020) and Letreguilly et al. (1991) are not all that different: 4 C for the former and 6 C for the latter. They also are not inconsistent with the required global warming of 2 C if one applies a polar amplification factor of 2. I will rephrase the sentence to "several degrees", which does not alter my contention that an ice-free*

Greenland is improbable during Pleistocene, as indeed attested by paleo-data. The latter calls into question numerical simulations of the glacial cycles anchored on an ice-free bistate. In our model, however, ice bistates simply reflect the ocean ones, which produce more distinct ice bistates (a polar ice cap versus an ice sheet extending to mid-latitudes) hence a stronger ice signal. I shall add above discussion and a reference to Van Breedam et al. (2020) in the revision.

l. 316 : if the 'cooling is tectonic in origin', what would be the mechanism ? Generally tectonically-forced cooling implicitly refers to a tectonically-forced decrease in pCO2, though I concede there could be other mechanisms.

20. *I have referred to Ruddiman and Raymo (1988) for a discussion of the Pleistocene cooling, which they attribute to, among other things, the uplift that alters the albedo and planetary waves. Not surprisingly, they have not mentioned CO_2, which, given its fast equilibration, is likely a response rather than a driver of the cooling. The Pleistocene cooling of course is a big subject with extensive literature, whose addressing lies outside the scope of the present study; I am simply taking the cooling as an observational fact and examine its effect on the glacial cycles. While the cooling would lower CO_2, its greenhouse effect would be dwarfed by that of the drier air (several Wm^{-2} versus several tens of Wm^{-2}), but without incorporating the latter, the numerical calculations are compelled to prescribe a CO_2 trend or its observed glacial variation in simulating the glacial cycles, which have muddled the causality.*

l. 319 : the ocean convective flux does not need to balance changes in net IR if the atmosphere heat flux divergence absorbs some of this change

21. *It is the global-mean convective flux, which, together with the net LW flux, balances the absorbed SW flux; the lateral heat flux divergence plays no part in the global mean balance. During the Pleistocene cooling, both absorbed solar flux and downward LW flux are decreasing (the former by ice albedo and the latter by the drier air, see also Ou 2001) while the upward LW is largely unchanged (it varies as the fourth order of the absolute temperature), the convective flux thus must decrease. I didn't include the changing absorbed solar flux in my original discussion, which will be added in the revision.*

l. 343 : the physical interpretation of the cause of a reduction in convective activity remains elusive.

22. *See response 21.*

l. 369 : "differing physics" : in what sense other models require differing physics ? Clearly the state of the ice sheet differs near full glaciation from early glaciation state, and it is therefore natural to expect different effects of the forcing. This does not require 'differing physics' but merely accounting for 'different states'.

23. *I will remove the sentence since it is wrong. The glaciation is smooth, occurring over millennial entropy adjustment time, but the termination can be hastened by Heinrich events and possibly punctuated by YD, which entail decadal abruptness (the ocean overturning time).*

l. 421 and ll. 442 - 443: The lack of figure with a simulated time series covering the last 800 ka is disturbing.

24. *The two times series are simply to show how glacial cycles differ in Stage 2 and 3 when global convective flux is lowered. Showing a longer timeseries with continuous global convective flux amounts to pasting the two timeseries and smoothing their transitions, which contains no new information.*

l. 502 : The argument would be convincing if an alternative explanation was used to justify the change in q'_c.

25. *See response 21.*

l. 496-497 : The tri-state was certainly overly schematic, but there are some explanations to the unstable character of a deeply glaciated state; glaciological interpretations evoke bedrock depletion and basal flow, and proposals giving a role to the circulation in the southern ocean / carbon cycle have also been made (Bouttes, CPast, 2012, Paillard and Parennin 2004 ,EPSL).

26. *A mode can be defined only as an attractor; different equilibria do not signify distinct modes if they vary continuously with forcing. I have not seen a dynamical basis for tri-states except quite nuanced ones discerned from numerical models, such as different convection sites or multiple ocean basins. Nor is such tri-states necessary to explain the glacial cycles, as I have demonstrated.*

l. 539 : Are Antarctic volume fluctuations driven by sea-level not a well-accepted resolution of this so-called polar synchronisation problem ? Kawamura et al. does not actually mention a 'synchronisation problem'. They made their best to accurately date terminations and confirmed indeed a northern hemisphere trigger to southern hemisphere variations.

27. *There are myriad propositions to resolve the "synchronization problem", which is premised on hemispheric anti-phase of the solar insolation. My argument is that there is no such problem in the first place if the relevant forcing is the annual absorbed SW flux. Since the ice volume signal is dominated by that of the northern hemisphere, the synchronized Antarctic signal necessarily involves global balance, which may include the sea-level.*

All that considered, it seems to me that the article makes no convincing case of a plausible alternative to the more classical approach focusing on the direct insolation forcing of nonlinear ice sheet dynamics.

28. *All studies of direct forcing link the air temperature to the Milankovitch insolation, which however overlook the fact that PPD of the direct forcing contains no precession signal because of the Kepler's law. And then the observed SST exhibits 10 C change through the glacial cycles, which would dominate PPD hence the ablation. Since such SST change hinges on an interactive MOC, models employing fixed SST or slab ocean are inadequate to capture its effect. For these reasons, numerical models that have produced realistic glacial cycles may not serve as an arbiter for the proper physics. With above responses, I hope to convince you the plausibility of the proposed physics, which in the least is justifiable as a working hypothesis.*

29. *I find your comments to be highly stimulating, which has led to much refinement in my thinking. Because of the time limit of the open discussion and the time needed for my overhaul of the paper (several weeks), I am unable to attach a revised manuscript for your perusal. I however would welcome your continuing input during this open discussion, which undoubtedly would further aid my revision effort.*