

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 Hsien-Wang Ou, Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-94-RC1>, 2021

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.

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).

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 !

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

line 8: climate system is presumably "ocean"

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.

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.

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 Alkhayouon 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.

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

this.

l. 136 - 137: see above

l. 141: admittance could be better defined.

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

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.

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 ?

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.

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

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

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'.

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

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

I. 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 Parrenin 2004 ,EPSL).

I. 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.

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 non-linear ice sheet dynamics.