

Earth Syst. Dynam. Discuss., referee comment RC2
<https://doi.org/10.5194/esd-2021-2-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on esd-2021-2

Anonymous Referee #2

Referee comment on "Reduced-complexity model for the impact of anthropogenic CO₂ emissions on future glacial cycles" by Stefanie Talento and Andrey Ganopolski, Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-2-RC2>, 2021

Review of "Evolution of the climate in the next million years: A reduced-complexity model for glacial cycles and impact of anthropogenic CO₂ emissions" by Stefanie Talento and Andrey Ganopolski.

The authors developed the simple model (which consists of three differential equations) which reproduces the last 800-kyr evolution of the global ice volume, atmospheric CO₂ concentration and global mean temperature. Based on this model, the authors accessed the anthropogenic influence on the deep future glacial cycles. This is a challenging attempt and I enjoyed reading the manuscript. Although there are many issues which need to be investigated further, this is a nice study which gives us valuable inspirations about the climate evolution in the deep future. Therefore, I can recommend the publication of this manuscript. Followings are my comments which I hope will be useful for the authors to prepare the final manuscript.

Specific comments

Line100-101: The statement "This is why we do not consider future sea level rise ..." is not clear.

Line126 (Eq3): Please explicitly describe the physical explanation about the first (b_{01}^*v) and the last (- b_{06}) terms, which I think was missing or not very clearly stated in the manuscript.

Line136 (Eq6): Why did the authors re-wrote the equation?

Line 144 (and Lines 150, 181, 182, etc): Carbon -> carbon

Line 231: (10) and (11) -> (9) and (10)

Line 248-249 (Eqs 11, 12): Different treatment about minimum values (i.e., 0 or 0.05) seems somewhat artificial and its effect on the results appeared very small. Is this different treatment really required?

Line 257-258: The meaning of the statement "the conditions for the new glacial inception will not be met in the near future" was not clear for me.

Line 286: Why? (Is optimization of "CO₂" and "temperature" in addition to "ice volume" technically difficult?)

Line 312: "respectively). ." -> "respectively)." ."

L311-312: It might be useful if you can discuss the reason for the overestimation in MIS 18 and 14.

L351-353: It was difficult for me to understand the details about how the authors calculate (estimate) the value "K" in their model. Additional explanation might be helpful.

L378-379: I feel that prediction of CO₂ changes appears not very successful because the simulated amplitude of CO₂ changes tends to be always overestimated. I'm curious about effects of CO₂ errors on the ice volume. For example, if you "prescribe" the paleo-recorded CO₂ changes instead of predicting it, how much does this improve the reproducibility of ice volume?

L503-504: What does the authors mean by "data not used for training"? (temperature and CO₂?)