Earth System Dynamics Discussions

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 CO_2 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-comlexity model for glacial cycles and impact of anthropogenic CO2 emissons" 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 (b01*v) and the last (-b06) 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 (Eqs11,12): 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?)