We thank the reviewer for the insightful evaluation and constructive comments. Please find below a point-by-point response, marked in blue. In order to address some of the reviewer's questions and assess the robustness of the results to several criteria involved in the design of the model or parameter selection strategy we performed a series of sensitivity experiments. The results from these experiments are briefly discussed here in response to specific comments and will be included in a revised version of the manuscript.

Response to reviewer 2

Review of "Evolution of the climate in the next million years: A reduced-comlexity 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.

In our modelling approach the variable v is zero at present and, thus, interpreted as the ice volume of Northern Hemisphere continental ice sheets which cannot be negative. As we stated in the manuscript, we do not explicitly account for Antarctic and Greenland ice sheet. Since reviewer#1 asked a similar question, a full answer to this question is given in response to the first reviewer. The short answer to this question is the following: we designed our model for simulations of future glacial cycles, not for global sea level rise projections. The later would require separate treatment of Greenland and Antarctic ice sheets because they have very different forcings and response mechanisms. Greenland is rather small and for our purposes can be neglected anyhow. Antarctic ice sheet is not. The problem is that the long-term impact of deglaciation of Antarctica on the global climate and carbon cycle is not investigated yet. Fortunately (for us) we do not consider scenarios which can lead to deglaciation of Antarctica. The most extreme case considered in the manuscript (a 3000 PgC emission) would cause less than 10% of Antarctic melt even in a very long perspective. With this model we are not intended to study scenarios with larger cumulative CO_2 emission because in a view of recent technological and political development we consider 3000 PgC scenario to be already too pessimistic. Thus the condition that v>=0 is not a problem in our case but we will explicitly discourage others to apply our model for more catastrophic CO_2 emission scenarios. We will clarify this in the revised manuscript.

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.

The term b01*v follows from the fact that total ablation depends on the size of ice sheets. The term –b06 is simply a constant. We will clarify this in the revised version.

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

The re-writing of equation (1) follows from substituting equations (2-4) into it. It was meant to help the reader and clearly identify the 6 tuning parameters: b1…b6.

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

Agreed.

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

Agreed, thank you for noting the mistake.

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?

Indeed this is an ad-hoc constraint, designed to account for the fact that interglacials before and after the MBT have, according to paleodata, different characteristics which cannot be explained by changes in orbital forcing.

To better address the reviewer's concern, in the new version of the manuscript we also investigate the sensitivity of the results to this choice. In particular, we design a new experiment in which the whole optimisation process is repeated lifting the ad-hoc imposition related to MBT (that, is modifying equation (11) so that the minimum ice volume is 0 at all times).

While, naturally, the correlation between modelled and paleo ice volume in this new version is slightly lower than with the ad-hoc MBT constraint (the ensemble mean across "Valid" drops from 0.76 to 0.73) the main results are not affected in any significant manner. Results will be shown in the revised manuscript.

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.

We will modify the sentence and give further insights on the research in which this statement is based.

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

The reason for not using an optimisation target encompassing the three variables it is not of a technical nature. Naturally, there are many possible choices for the optimisation target function. We selected to optimise the correlation between modelled and paleo ice volume because our main objective is to produce a forecast of future glacial cycles and because the ice volume paleorecord has small uncertainties. To investigate the impact of this selection on our results, we perform a sensitivity experiment in which the whole optimisation process is repeated in order to maximise both the correlation between paleo and modelled ice volume and between paleo and modelled CO_2. Results will be shown in the revised manuscript version and are not significantly different from the ones obtained with the model version which optimises just ice volume performance.

We note that we decide against including as a possible optimisation target the correlation between paleo and modelled temperature, because the temperature paleorecords have large uncertainties (as easily noticeable by the two paleorecords in Fig 1c).

Line 312: “respectively). .“ -> “respectively).”

Agreed.

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

As has been stated above, there are non-negligible differences between pre- and past-MBT glacial cycles. Two pre-MBT glacial cycles (MIS18 and 14) are much weaker than post-MBT glacial cycles (it should be noticed that uncertainties in sea level reconstructions of pre-MBT glacial cycles are also much higher than for the most recent ones). Since this cannot be explained by differences in the orbital forcing, it cannot be expected that the model will simulate these cycles correctly. And this is not a problem of our conceptual model but also other similar and more complex models (e.g. Willeit et al., 2019). Since the causes for the differences between pre- and post-MBT glacial cycles remain unknown, it is not possible to say what implications this model-data mismatch has for our future simulations.

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.

We agree that this was not properly explained in the text, the explanation will be added in the revised manuscript.

For the estimation of K we refer to equation (6), assuming equilibrium and interglacial conditions (i.e. $dv/dt=0$, $v=0$, $M_v=0$) it is derived that the critical insolation threshold:

$$f_{cr} = -\frac{b_3}{b_4}\log(CO_2) - \frac{b_6}{b_3} + \bar{f}$$

It follows then that K is estimated as $-b_4/b_3$.

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?

When parameters in equation (6) are selected using prescribed CO_2, the ensemble mean correlation between modelled and paleodata ice volume is 0.85 (compared to 0.76 when CO_2 is not prescribed). We will include this information in the new manuscript version.

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

Please see answer before.