We are thankful to Alan Kennedy-Asser for the interest in the paper and comments. Please find below a point-by-point response, marked in blue.

Comment on esd-2021-2
Alan Kennedy-Asser

This is an interesting paper and I feel the many steps in the modelling process are relatively well described. While I think it is reasonable to use conceptual models for this kind of study, there are a few assumptions which I find a little questionable and should be justified better (or the implications of which should be discussed in more detail). After having read the paper, I have read the comments made by other reviewers and I am inclined to agree with many of the points raised by Reviewer 1.
In particular, I think the following assumptions require further discussion:

1. The constraining the minimum ice volume to the pre-industrial levels is not well justified in my opinion and its impacts are unclear. As others have commented here in the reviews, perhaps at least considering other past warmer periods is necessary if this study is to be seriously considered as realistic, particularly for the warmer high emissions scenarios.

   The data from the warmer periods would be useful indeed but such data (accurate enough for our purposes) are unavailable. This is why we used modeling results from CLIMBER-2 instead.

   As we stated in the manuscript, imposing v>=0, we do not account for Antarctic and Greenland ice sheets. Since reviewer#1 asked a similar question, a full answer is given in the response to that reviewer. To summarize, 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 Antarcic ice sheets because they have very different forcings and response mechanisms. Even a complete deglaciation of the Greenland ice sheet will contribute not more than 7 m in sea level equivalent, which is small compared to the magnitude of glacial-interglacial variability and, therefore, is not problematic for our approach. On the other hand, a complete deglaciation of Antarctica (around 55 m in seal level equivalent) is not negligible and, thus, we do not consider scenarios which can lead to this situation. In fact, 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.

2. Likewise, constraining the ice volume to not glaciate for the first part of the record – what effect does this have if this constraint not included? Does the model often glaciate without it? Although it seems unlikely, I don’t think there is so much evidence against this being a possibility that it can simply be prescribed.

   We investigated the effect of not including this constraint by repeating the optimisation process modifying equation (14) so that glacial inception could potentially occur in the next 20 kyr (but keeping the condition of no glacial inception at present). Under this scenario, the new “Valid” ensemble contains three solutions (out of 400 ensemble members) for which glacial inception occurs at some point between present and 20 kyr into the future. So, it is indeed possible to find parameter combinations that generate an ice-volume time series in good agreement with the paleorecords, have an interglacial state at present and for which glacial inception occurs before 20 kyr into the future. However, those solutions are only a small fraction of the “Valid” ensemble. We conclude then that keeping or lifting the condition of no glaciation in the next 20 kyr does not affect the main conclusions of the manuscript.

3. What might be the impact of the assumption that natural and anthropogenic CO2 signal are separate and can be linearly combined? This was also raised by the reviewers.

   We understand this concern. As there are currently no model simulations that allow for the analysis of how the global carbon cycle will operate during future glaciations under elevated

CO_2 level resulting from the anthropogenic perturbation, it is not possible to assess the accuracy of this assumption.

However, considering that the carbon cycle shows a relatively linear behaviour we expect this assumption to be a reasonable one. Under this assumption, after a CO_2 anthropogenic pulse is applied, the system evolves in two phases. In the first phase, the exponential decay of the anthropogenic perturbation is dominant, being the natural CO_2 variability negligible in comparison. In the second phase, the initial anthropogenic perturbation is negligible and the natural glacial-interglacial variability is dominant with the resuming of glacial cycles. New simulations would be needed to evaluate how good the assumption about linear superposition of the anthropogenic and natural components during the transition between the two phases might be.

4. One point which I had not thought of, but was mentioned by Reviewer 1 (their comment 3) and I think is worth echoing relates to the choice of the Pearson's correlation threshold of >0.7 and the 'accepted' simulations out of the full 'valid' set. Are the 'accepted' simulations in general those with higher correlations?

No, they are not. Please see Fig. 1b in the response to reviewer #1 showing that the "Accepted" solutions correlations with paleo data are not statistically significant different from the rest of the solutions in the wider "Valid" ensemble.

I have a few other queries which the authors may want to consider clarifying:

5. I am curious to know do you have an explanation of why the 10kyr time lag between the natural and 500PgC scenario in Figure 8?

   As expected, it is more difficult for the model to glaciate in the 500 PgC scenario than in the natural one (as the critical orbital forcing for glacial inception is lower in the case of higher atmospheric CO_2 concentration). From Fig. 8 it is possible to appreciate that during the next half a million years a 500 PgC emissions scenario causes some glacial cycles to be completely skipped while others develop later and slower than under natural circumstances. In particular for the cycles around 200 kyr and 300 kyr, while the glacial inception occurs at similar times under both the natural and 500 PgC scenarios, the latter ice growth is slower. This combined with the strong non-linear behaviour of the system generates a ~10 kyr lag in the occurrence of the maximum ice volume (which also happens to be ~10% larger for the 500 PgC scenario). After half a million years into the future, the glacial cycles under the natural and 500 PgC scenarios are almost identical.

6. Line 422: Does the low 500 PgC scenario suggest a scenario where some of the CO2 already emitted is drawn back down? This value is less than what has already emitted as quoted from the Le Quere et al. 2018 paper. I think this should be clarified.

   Indeed, Le Quéré et al. (2018) estimate total cumulative CO_2 emissions for the period 1750-2017 at 660 +/- 95 PgC. We selected 500 PgC emission scenario just to demonstrate that already emitted anthropogenic CO_2 will have long-term consequences on future climate evolution. Of course, a possibility of future carbon storage, makes any cumulative CO_2 emission (even negative) possible. But this is beyond the scope of our paper.

7. Finally, it might be useful to reference a technical report on a similar topic that was produced for SKB (similar to Nagra who funded this work), where probabilistic future projections (or maybe 'scenarios' is a more appropriate word, following on from Reviewer 1's comments) are shown. The report is available here https://www.researchgate.net/profile/Jens-Ove_Naeslund/project/Climate-and-radioactive-waste/attachment/5dd2830fcfe4a777d4f1f887/AS:826544842870784@1574075055393/download/TR-19-09.pdf?context=ProjectUpdatesLog

Thank you for the link, we were aware about this report but not that it was publicly available. We will include it as a reference.