We thank the reviewer for the insightful 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 manuscript.

Response to reviewer 1

This manuscript presents possible scenarios for the Earth's climate during the next million years. It brings interesting and new material to this rather overlooked and under-researched area. Overall, I am favourable to publication, but I have nevertheless several important comments that the authors should consider in a revised version of the manuscript. Most importantly, the whole exercise is based on shaky hypotheses: I am aware that there aren't many alternatives, but it is all the more important to present and discuss them thoroughly.

1 – Using Quaternary climate (and more precisely the last 800 kyr period) to calibrate a conceptual model to be applied to the future is certainly not the ideal choice since there are no very hot periods, but mostly glacial ones. The (partial or complete) melting of Greenland or Antarctica is therefore not considered, and the effect of high CO_2 levels cannot be calibrated. In other words, the whole exercise is more an extrapolation than an interpolation. This is something known to be quite dangerous. I perfectly understand this choice, based on the availability of data, but it remains nonetheless not satisfactory. This should be stated much more clearly in the paper.

We fully agree with the reviewer that the late Quaternary paleoclimate records alone are insufficient to develop and calibrate a semi-empirical model suitable for future simulations. This is why we used a hybrid approach, based on a combination of paleodata and model simulations. Namely, the description of natural climate variability (CO_2 around or below preindustrial) is calibrated against paleodata while model behaviour in the greenhouse world is derived from the results of the physically-based CLIMBER-2 and cGENIE models. (The role of future Greenland and Antarctic mass loss will be discussed below). Thus, we believe we use a scientifically sound methodology which does not require any "extrapolation". We will better address the topic in the revised version.

2 – Our knowledge of the long-term carbon cycle and of the ultimate fate of fossil-fuel carbon is also very thin, shaky or uncertain. The manuscript uses model results (Lord et al. 2016; based on Lenton et al. 2006) that have unfortunately no "real world" tests. The imposed exponential decay of carbon is therefore based on the (mostly theoretical) idea that the carbon cycle is regulated uniquely through silicate weathering. This approach neglects many other important processes that are known to have played a critical role on these timescales in the past and even today, like organic matter burial or kerogen weathering. Again, I do not contest the value of using a simple hypothesis, but this should be explained and discussed.

We agree with the reviewer that long-term (10^5-10^6 yrs) carbon cycle dynamics is poorly understood, primarily because of the lack of accurate empirical data. This is why we do not anticipate a significant breakthrough in this field in the near future. Since the issue of the the long-term future evolution of Earth system is now not only an academic one, but also of great practical importance, one cannot simply wait till all relevant problems will be resolved. In any case, the major uncertainties in the future Earth trajectories originate from the fundamentally unreducible uncertainties in the future human activity. This is why any future projections can represent only a tentative answer to the question of what can happen in the future under certain assumptions (this is also true for the "IPCC reports"). Of course, we fully agree with the reviewer that the manuscript will benefit from a more substantive description and discussion of the most important assumptions and their limitations. This will be done in the revised manuscript.

To summarize, sentences like "we produce a probabilistic forecast" (line 14 in the abstract) are not acceptable. This is obviously not a "forecast" but only a possible scenario, based on our very limited knowledge of the dynamics of geological transitions in the past.

We are clearly not able today to "forecast" what the Anthropocene era will be, and this should be stated much more clearly.

We used the term "forecast" in a very broad sense – under forecast we meant future simulations to distinguish them from past climate simulations (hindcast). Obviously, no one expects to make accurate climate forecast for million of years when it is not possible even for the next 100 years. But since the term "forecast" can cause confusion, in the revised manuscript we will change "forecast" to "possible future scenarios".

Other comments:

3 – It appears that one of the most critical parameter, K, is not well constrained using the conceptual model or the chosen paleoclimatic dataset, as explained in §3.2.

« Our results indicate that with the model derived in this study the possible values of the coefficient K range between -1279 and -31 W m-2, with a median of -393 W m-2 »

Using results from an Emic model (CLIMBER-2, Ganopolski et al, 2016) the authors decided to select only a very small subset of solutions ("Accepted") that are all in the tail of the distribution of "Valid" solutions as shown on Fig.2. This appears as a strong shift in the overall strategy and raises a few questions:

First of all, as discussed above, using of CLIMBER-2 results (together with the cGENIE model) is an essential part of our modelling strategy, not a "shift". After reading reviewers' comments we realised that this misunderstanding likely arises from how we presented the methodology of model calibration. Our idea to present first all model realisations consistent with paleodata ("Valid" ensemble) and then to apply the additional constraint on the value of K based on CLIMBER-2 results was to demonstrate that paleodata alone are insufficient to calibrate the model intended for future projections. This is fully consistent with the first comment of the reviewer. Unfortunately, the reviewer interpreted the fact that most of model realisations which work for the past, have been then rejected by CLIMBER-2 constraint, as a problem of our method. In fact, our methodology works well: after applying all constraints we still have a sufficiently large ensemble of model realisations which successfully simulate past climate evolution and is consistent with the physically-based model. Thanks to this reviewer comment, we now understand how to explain our modelling strategy better.

- How does the correlation to data vary across the histogram on Fig.2? Are the "Valid" solutions close to the 0.7 correlation limit and the center of the distribution farther away from this limit?

  Please see Fig. 1b below showing the requested information on the variability of correlation between ensemble members and paleo data, according to the parameter K. It is clear from this plot that the "Accepted" solutions correlations with paleo data are not statistically significant different from the rest of the solutions in the wider "Valid" ensemble, which reinforce the statement that past climate data alone are insufficient to calibrate the model designed for the future.

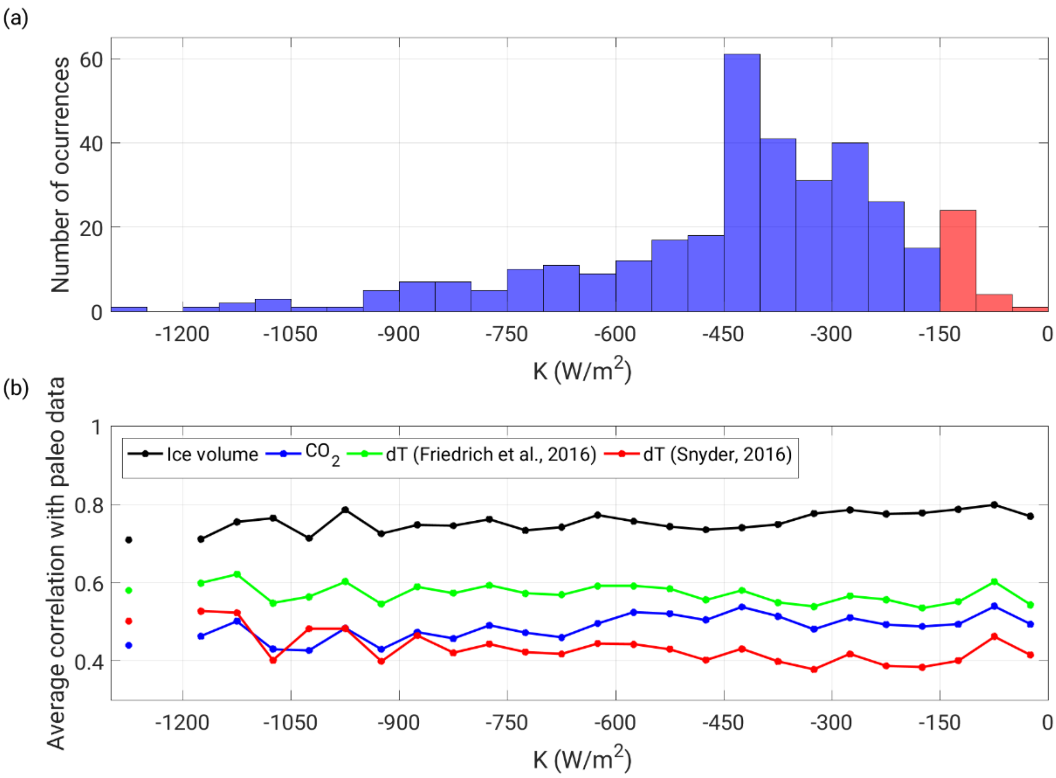

Fig. 1 (based on Fig. 2 from manuscript): (a) Histogram for K (see Eq. (17)) across the different members of the *Valid* ensemble of solutions. Red bars correspond to the *Accepted* solutions. (b) Correlation between modelled and paleodata, for all the solutions in *Valid* averaged within the bins in the histogram shown in (a).

- The Ganopolski et al (2016) insolation-CO2 threshold is also based on a parameter selection using a comparison to (basically) the same ice volume data. The problem is therefore not that the paleodata does not constrain well the K parameter but that the chosen conceptual model and the CLIMBER-2 model do not represent the role of CO_2 onto the dynamics of ice sheets in the same way. Why do the authors choose to trust one model against the other? And to adjust on model on the other?

  Here we must respectfully disagree with the reviewer. Fig.2 (based on Fig.3b from Ganopolski et al. (2016)) shows that paleodata do not provide any constrain on K value at all. Only one parameter (namely parameter β in the formula for the critical insolation–CO2 relationship derived in Ganopolski et al. (2016)) is reasonably well constrained by the paleodata. Another parameter α which defines the slope of the curve (in our manuscript it is named K) is not constrained by paleodata at all (see Fig. 2). This is why, this parameter in the critical insolation–CO2 relationship is derived in Ganopolski et al. (2016) solely from the result of model simulations and it is fully determined by the physics of surface mass balance module of CLIMBER-2.

Note, that in the simple model, the value of K is defined by the combination of two parameters – b3 and b4 in the Equation (6): K=-b4/b3. At first glance, it may look strange that equally good simulations of the past can be obtained with model versions which have completely different combinations of the b3 and b4 parameters as shown in Fig. 1. In fact, this is not surprising because three values entering equation (6) – ice volume, CO_2 and insolation – are closely related to each other and ice volume record alone is insufficient to accurately determine the role of each of the factors. As the result, we come to the conclusion that paleodata cannot constrain K but this value is of crucial importance for future simulations. This leaves us with no other option than to trust CLIMBER-2 value, the only source of information about this characteristic available at present.

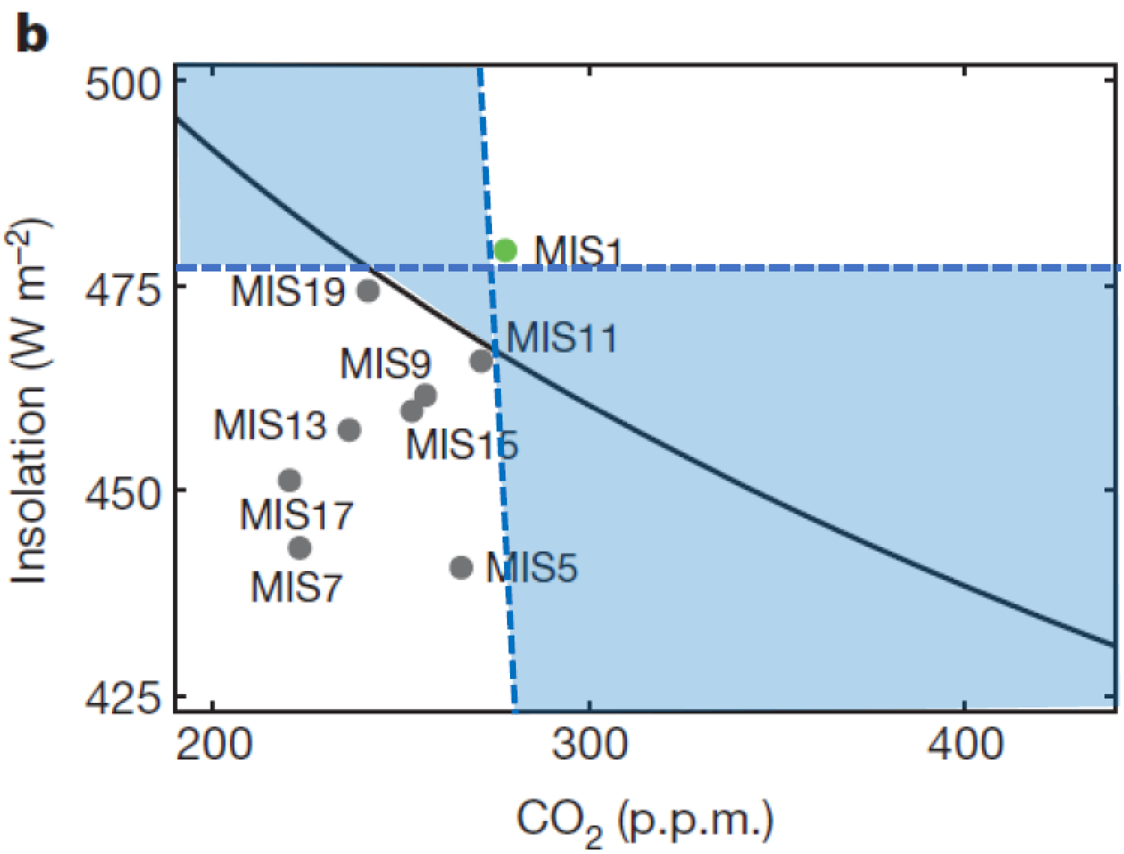

Fig. 2. The locations of previous glacial inceptions in the insolation– CO_2 phase space. Any line (corresponding to K from 0 to minus infinity) passing though the blue domain is consistent with the paleodata. This is because glacial inception during the past 800 kyr occurred under rather similar CO_2 concentrations but very different orbital forcings.

Reviewer asks “Why do the authors choose to trust one model against the other?” Because one model – CLIMBER-2 – is the Earth system model which simulates surface mass balance of ice sheet through the physically-based energy-balance approach. This model accounts for the effect of short-wave and long-wave radiation, sensible and latent heat fluxes, effects of snow aging and impurities on surface albedo of snow. To the contrary, the model developed for this study is just a simple, semi-empirical model based partly on CLIMBER-2 results, partly on paleodata. But since paleodata provide no constraint on K, how these two fundamentally different models can be treated equally?

- More technically, how are inceptions defined in the conceptual model?

  In the conceptual model we define that full glacial conditions occur when the ice volume (v) reaches 0.5 in normalized units. If full glacial conditions are reached at a time T, then the corresponding glacial inception is defined as the first time before T in which v>0:

$$Glacial\ inception = \sup\{t\ /\ \ t < T\ \&\ v(t) > 0\ \}$$

  We will clarify this definition in the revised text.

4 – Another strong limitation concerns the simple addition of “natural” and “anthropogenic” carbon, as presented line 182:

« In addition, we assume that natural and anthropogenic CO2 anomalies can be simply summed up and that at the preindustrial time the global Carbon cycle was in equilibrium. This is, obviously, a very strong assumption since even a rather small imbalance in the global Carbon cycle which is impossible to detect at the millennial timescales can result in a very large “drift” of the Earth system from its preindustrial state at the million years timescale. »

Indeed. This is actually why the anthropogenic CO2 decreases through a small imbalance between silicate weathering and carbonate preservation. The conceptual model assumes that there is NO natural dynamics in the carbon cycle besides glacial cycles. On Fig.8b the CO2 is just following the imposed decrease in the absence of (northern hemisphere) ice-sheets. But what about a possible role of Antarctica? What about some internal dynamics? And even on the calibration period (Fig.4b) the CO2 results are quite different from the data. In other words, the added value of a dynamic CO2 component in this conceptual model is not obvious.

The reviewer here rises several different issues.

1. Indeed it is unknown how close to the equilibrium the preindustrial state of the global carbon cycle is. This is why we consider our key assumption that it was an equilibrium state as a “strong assumption”. But, at the same time, this is not an unreasonable assumption. Based on all available reconstructions the CO_2 concentration was not higher than 400 ppm 3 million years ago, then the average trend of CO2 concentration for the interglacial conditions is only 40 ppm per million years which does not represent significant problem for our approach.

2. Our assumption is that during the next million years the CO_2 concentration is controlled by the removal of the anthropogenic pulse through solubility, weathering and the CO_2 response to the glacial cycles through solubility, changes in biological pump, deep ocean ventilation, etc. ALL these processes are “natural”. Unfortunately, we do not know which other “natural processes” reviewer meant here.

3. Concerning a "possible role of Antarctic". In short, we do not expect it will play significant role. A more detailed discussion is given below.

4. Indeed, the match of CO_2 data is not as good as for the ice volume. (The average correlation between modelled and paleo data for CO_2 is 0.5, considering the full period [-800 kyr, 0 kyr]). This is because we optimised model parameters for the best fit to ice volume, not to CO_2.

5. "added value of CO_2 ... is not obvious". This is a rather surprising statement. The model described in the manuscript is designed to simulate long-term response of the Earth system to anthropogenic CO_2 emission. How the model can do this job if it does not include CO_2 concentration? It is of course possible to develop a one-equation model for the evolution of ice volume with the insolation as only external forcing (e.g. Paillard 1998) and obtain a good fit to paleo data. However, this type of models cannot account for the effect of a possible external CO_2 input into the system and, therefore, is inadequate for our purposes.

The inclusion of a dynamic CO_2 within the model, in which the global ice volume, CO_2 and temperatures evolutions have influence on each other allows us to evaluate the possible impact of fossil fuel CO_2 emissions.

5 – It seems to me that the 3 variables (v, CO2 and T) are almost identical (up to scaling) in the natural and in the no-anthropogenic cases. Are these 3 variables necessary at all to express the dynamics of the system? I believe only one variable could have produced almost the same results.

In case of natural glacial cycles, the reviewers is perfectly right. Due to a rather high correlation between ice volume and CO_2 during the last 800 kyr (-0.63), it is possible to assume that CO_2 is implicitly accounted for by ice volume and develop one-equation model. Paillard (1998) model is a good example of such approach. But in the case of Anthropocene, ice volume and CO_2 are not correlated and a separate description of ice volume and CO_2 is absolutely necessary, As far as the global temperature is concerned, this is indeed not a prognostic characteristic but rather a useful diagnostic.

Other comments:

Line 86 :

« the last 800 kyr (see below). This period was selected because it is dominated by the long glacial cycles which are expected to continue in the future »

This is not the case with anthropogenic forcing… I would prefer the authors to acknowledge that this is the only period where we know both the ice-sheet and CO2 evolutions. Using another time period (much warmer) would be preferable for the next million-years.

We assume (and the model confirms) that even with the anthropogenic forcing, at some time during the next million years, the natural glacial cycles will resume. This is why glacial cycles of the Late Quaternary are crucial to calibrate the model. The data from the warmer periods would be useful indeed but such data (accurate enough for this purposes) are unavailable. This is why we used modelling results instead.

The reviewer is of course perfectly right that the last 800 kyr is the only period of time for which CO_2 is accurately know. But, just by chance, this is also the duration of the period after the mid-Pleistocene transition when long asymmetric 100 kyr cycles began. Since we assume that these cycles will continue in the future after decline of anthropogenic CO_2 anomaly, it is natural to use data from this period of time. We now clearly see the need to discuss this issue in the manuscript.

The choice of the last 800 kyr period for model calibration was motivated by the Clark and Pollard "regolith hypothesis". Given the long time-scales (millions of years) needed for regolith build-up we assume that it is unlikely that a regolith layer will be developed in the next 1 million years and, as a consequence, the last 800 kyr could be considered an adequate calibration choice.

Line 99 :

« This approach, obviously, is not applicable for a possible future Antarctic and Greenland melting under high CO2 concentrations. This is why we do not consider future sea level rise above the preindustrial level and it is required that $v \geq 0$ at any time»

This appears a strong limitation of the study and it should be acknowledged as such in the abstract and in the conclusion. A discussion on how to lift this problem would also be appreciated.

About Greenland and Antarctica: Greenland contribution (not more than 7 m in sea level equivalent) is minuscule compare to the magnitude of glacial-interglacial variability and there is no reason to expect that even a complete melt of the Greenland ice sheet could cause any troubles for our approach. Antarctic is a completely different issue: sea level rise by 55 meters will without doubts affect also climate in the Northern Hemisphere and the global carbon cycle. The problem is that we are not aware about modelling studies of this sort and the last time Antarctica was ice free was more than 30 million years ago. This is why we did not consider in our study scenarios which can lead to a complete deglaciation of Antarctica. According to the recent study by Garbe et al. (2020), significant Antarctic mass loss occurs only if global temperature anomaly stays above 8°C for a very long time. In our 3000 PgC scenario simulation, sustained global warming is only about 4°C. For 4°C global warming, Garbe et al. (2020) found only 6.5 m of Antarctic contribution to sea level rise which is only 10% of the maximum Antarctic contribution. We now see that this issue should be more explicitly discussed and we will make a "disclaimer" for potential users of our model that scenarios with cumulative CO_2 emission above 3000 PgC are not recommended. We agree that this point was not clearly acknowledged in the text. In the revised manuscript version we will stress this limitation and provide a discussion.

Line 151 :

« Namely, we assume that on the relevant timescales (103-105 yrs), the natural component of CO2 concentration is in equilibrium with external conditions and can be expressed through a linear combination of global temperature and global ice volume »

This is probably why the "natural CO2" results are not so good (Fig.1b & 4b)…? They are mostly simple "mirrors" to the ice-volume and temperature ones.

We agree that the agreement could be better but this is what we were able to achieve (see above).

Equa (7) :

Change dT into a T ?

Agreed.

Line 227

« We select a climate sensitivity equal to 3.9 C, which coincides with the multimodel mean in the Coupled Model Intercomparison Project 6 (CMIP6; Zelinka et al., 2020) »

How sensitive are the results to this choice? This could be critical and should be discussed a bit more.

To address this question, in the revised manuscript we will provide a sub-section devoted to the investigation of the sensitivity of results. One such sensitivity experiment consists in re-doing the optimisation procedure but with a selected climate sensitivity of 3°C (that is modifying equation (10)). The results of this experiment indicate that the main conclusions are not sensitive to the change in the selected equilibrium climate sensitivity. Under the natural scenario, even with the lower equilibrium climate sensitivity, next full glacial conditions will most likely not occur before 50 kyr in the future. Under anthropogenic CO_2 emissions scenarios, the higher the cumulative emissions the larger the delay in the onset of the next ice age.

Line 240 :

« First, we assume that for the recent interstadials and any future time, v cannot be negative. »

See above comments: what about melting currently existing ice-sheets?

Please see discussion above.

Line 245

« This is why we prescribe that before the MBT the minimum ice volume must be 0.05 in normalized units: »

This seems a very ad-hoc assumption: I do not understand the reason for this adjustment, beyond providing artificially a better correlation.

Indeed this is an ad-hoc constraint designed to account for the fact that, according to paleodata, interglacials before and after the MBT present were different. This fact cannot be explained by changes in orbital forcing and the cause of MBT remains debatable. Thus it is not possible to simulate equally good pre- and post-MBT glacial cycles. On the other hand, we did not want to restrict the training period to the last four glacial cycles only. This is the motivation for introducing of minimum ice volume prior to MBT.

To 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 the model versions without different treatment of pre- and post-MBT glacial cycles is slightly lower than in the original formulation (the ensemble mean across "Valid" drops from 0.76 to 0.73) all our main results are not affected. Results will be shown in the revised manuscript.

Line 258 :

« the new glacial inception will not be met in the near future even in the absence of anthropogenic influence on climate. »

Again, this seems a very ad-hoc constraint: the physical explanation is to be found in the insolation forcing, and the tuned models should provide this mostly as a result, not as an a priori constraint.

The most important constraint is that the present state is the interglacial one (i.e. v=0 at t=0). This is an observational fact. The notion that the next glacial inception will not occur in the near future is based only on modelling results (Loutre and Berger, 2003; Cochelin et al., 2006; Ganopolski et al. 2016).

In order to assess the importance of this additional constraint, we repeat the optimisation process modifying the equation (14) so that it does not include the condition of no glacial inception in the next 20 kyr, we keep however the condition of no glacial inception at present. Without this constraint, 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. The main results remain largely unchanged.

Line 266 :

« corr(x,y) denotes the linear Person correlation »

The correlation is not always the best metric, though it is simple to compute… Why only using the correlation with ice-volume data and not the two other paleoclimatic data?

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 simulate future glacials. 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 new 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).

Change "*Person" into "Pearson"*

Agreed.

Line 286 :

« For the selection of solutions, no conditions are imposed on the goodness of fit »

Well, it seems to me that Equation (16) is a condition on the goodness of fit! This also contradicts line 265:

In fact the complete sentence is: “For the selection of solutions, no conditions are imposed on the goodness of fit between modelled and paleo CO_2 or temperature”.

« We wish to find P to maximize the optimization target function Cv »

Probably the authors should clarify their language: they are only choosing parameters that satisfy all the constraints (including (16)): this is a feasibility problem, not an optimisation problem (though it is usually provided in optimisation packages).

The optimisation problem does not include equation (16) as a constraint. Equation (16) only comes in the picture after all the optimisation searches have been performed, in order to select only those solutions that we deem as good quality.

Why choosing correlation > 0.7 (or why selecting 353 parameter sets)? Is there a need to have a large enough parameter set with a large enough dispersion? Or does this relates to the parameter K problem (see above comment)?

The selection of correlation higher than 0.7 is designed to single out only those solutions which reproduce the ice volume behaviour in the last 800 kyr reasonably well (considering also that the paleorecord used for this variable is of course not perfect). The number 0.7 was selected prior to any further analysis of ensemble size or dispersion across the possible K values. With this procedure we did not aim to produce as many model versions as possible. Rather, our intention was to have significantly diverse model versions. Table 1 shows that even after applying all constraints, each important free parameter (especially parameters b_i) vary in a wide range.

Line 293 :

« For global mean surface temperature anomalies (with respect to preindustrial conditions) we use two reconstructions »

Some discussion on the nature and on the accuracy of these proxies could be useful. In particular why using “ice-volume” as a preferred target? Overall, the temperature does not have any dynamic role in the model (it can be replaced by v and CO2). So why using it?

We agree and in the new manuscript version we will include a discussion on the nature and accuracy of the paleorecords utilised in the paper and subsequent selection of ice volume as preferred target.

As expressed by equation (8) in the model the temperature variable is a combination between ice volume and logarithm of CO_2 concentration and the reviewer is right, it can be eliminated from the model equations. However, our intention was not to minimize the number of equations but rather to make them more physically sensible and to show clearly which processes are modelled. It is a valuable part of the model, as we account for a direct impact of temperature on CO_2 levels in equation (7). As mentioned in the text, temperature can affect CO_2 levels in several ways: changes in the CO_2 solubility in ocean water, changes in ocean circulation and ventilation rate of the deep water, changes in relative volume of different water masses and effects on metabolic rates of living organisms. In addition, global temperature is a very useful diagnostic which can become necessary in other potential applications of our model.

Line 315 :

« some solutions display an amplitude range significantly larger than the observed one, reaching the imposed lower limit of 150 ppm »

Actually, not "some" solutions, but "most" or even "all" solutions.

In fact, the exact number of solutions in the Valid ensemble that reach the imposed lower limit of 150 ppm is 179 (over a total of 353), i.e. ~50% of the solutions.

Line 314-317:

Correlations of 0.5 or 0.56 appear not very good to me. Why optimizing only the correlation with ice volume?

To address this question an experiment was performed designing the optimisation problem to maximise a combination of correlation with ice volume and CO_2. Results will be shown in the revised manuscript. In particular, we found that modifying the optimisation target function to also account for the correlation between modelled and paleo CO_2 does not yield overly different results.

Line 327 :

« In general, we conclude that the model has a satisfactory ability also when used in predictive mode and, thus, we confidently venture to utilize it as a tool for the forecast of the next 1 Myr climatic evolution. »

This is overly optimistic : the climate system is very different in the anthropogenic case. The word "forecasting" is fully inappropriate: the system is obviously non-stationary and a "statistical forecast" has here no meaning at all. At best, you can call this a possible scenario.

Along the revised manuscript we will exchange the word "forecast" for "scenarios".

Line 356 :

« the relationship between critical and log(CO2) is not well constrained by paleoclimate data »

See my comment above: Ganopolski et al (2016) was based on the same data, so the paleoclimate data CAN constrain the K parameter. But possibly the conceptual model does not capture well this threshold behaviour…

Our simple model is designed in such a way that it has a stability diagram qualitatively similar to CLIMBER-2 where glacial inception represents a bifurcation transition from interglacial to glacial state (Calov and Ganopolski, 2005). But since, as we show above, paleodata cannot constrain K, we have to use CLIMBER-2 results to select finally only those model versions which are consistent with CLIMBER-2 in respect of K value.

Please see comments done before.

Line 383 :

« The high positive temperature anomalies during some previous interglacials, however, are questionable. »

There was no discussion on « data » in the manuscript, except this sentence... Why is it questionable ? This should certainly be explained a bit more.

Because global surface air temperature is reconstructed based on a patchy information, usually from the ocean only. This is not the case for ice volume and CO_2.

We agree and in the revised version we will discuss potential problems with the paleoclimate reconstructions used.

Line 449 : lesss -> less

Agreed.

Line 450 :

« Most of the solutions agree that the planet will remain in a long interglacial state for the next 50 kyr »

Not « most » but « all » since it was built into the assumptions (something questionable, see above). I do not understand this statement: the contrary would be problematic.

In fact the constraint imposed on the model was that no glacial inception should be possible in the next 20 kyr (not 50 kyr). In particular, there is one solution in which glacial inception starts at 20 kyr and full glacial conditions (i.e., ice volume >= 0.5 in normalized units) are reached by 36 kyr. We will add one sentence in the manuscript mentioning this case.

Line 532 :

« the past does not perfectly constraint the future evolution of the climate – ice sheets – Carbon cycle system. »

In particular using only the last 800 ka Quaternary period. The main question is the choice of the time window used in the past.

Of course, we meant here only the last 800 kyr. We will make it clear. Unfortunately, we do not have reliable CO_2 reconstructions for the "rest" of the past.

Please see comments done before.

Line 534 :

« The selected model versions exhibit a large sensitivity to fossil-fuel CO2 releases »

How does this relate to the K parameter choice (based on Climber results)? It seems to me that the "Valid" set is even more sensitive. This should certainly be discussed in much more details since it represents a large part of the manuscript.

The parameter K is a measure of the sensitivity of the critical orbital forcing for glacial inception to atmospheric CO_2 concentration. The higher K is, the more important CO_2 is. This mean that for the same CO_2 emission scenario, the effect of anthropogenic perturbation on glacial cycles will last longer. Since paleodata provide no constraint on the K value, some of "Valid" model versions have K an order of magnitude higher than reported in Ganopolski et al. (2016). With such high K, even a small CO_2 emission scenario will prevent glacial cycles over the next million years. This is absolutely unrealistic. By using an additional constraint on the K value based on CLIMBER-2 results, we eliminated all model versions with K values too large (in absolute terms, since K is negative). The remaining ("Accepted") ensemble has models with reasonable K values. Still, these model versions reveal a long-lasting effect of anthropogenic perturbation on future glacial cycles.

In the revised version of the manuscript we will clarify further the implications of the parameter K and exemplify the very different behaviours solutions with different K could have under an anthropogenic emissions scenarios.

Conclusions:

« this relationship is poorly constrained by the paleoclimatic data because during previous interglacials CO2 was close to or lower than the preindustrial level. »

« Reducing this uncertainty by performing experiments similar to those described in Ganopolski et al. (2016) but with more advanced Earth system models can help to reduce uncertainties in future projections. »

As explained above, I do not like this conclusion. The *Ganopolski et al. (2016)* threshold was based on the same data, so the difficulty is not so much within the data, but much more with the model. Besides, enlarging the scope outside the Quaternary would certainly help a lot. The authors should better highlight the key difficulties (my main comments 1 & 2).

As we explained above, the "difficulties" are indeed with the data. Since the slope of the relationship between the critical insolation threshold and CO_2 cannot be constrained by the paleodata and because this parameter is of paramount importance for the future evolution of the Earth system, it makes perfect sense to check the value derived in Ganopolski et al. (2016) with a more advanced model than CLIMBER-2.

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