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Interactive comment on "Reconstructing the Evolution of Ice Sheets, Sea Level and Atmospheric CO_2 During the Past 3.6 Million Years" *by* Constantijn J. Berends et al.

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Rebuttal to the review by Andrey Ganopolski

We thank the reviewer for their comments on the manuscript and would hereby like to address the concerns they raised.
Comments in italics, below our rebuttal. Page and line numbers refer to the revised manuscript.

Method description. One of the problems for the readers of this manuscript is

that the method used in this study has been developed over a long period time and its comprehensive description are scattered among a number of previous publications. Even although I was familiar with some of them, it took me a lot of time to get a more or less clear understanding of what authors are doing. Of course, one cannot expect such efforts from a typical reader. However, without a proper understanding of the method, the results presented in the manuscript are not very useful. This is why, I would suggest to make a more detailed description (including the key equations) in the appendix or supplementary information. In particular, I am curious how the effect of orbital forcing has been accounted for by the "matrix method".

We agree that a thorough understanding of our methodology relied too much on concepts explained in earlier publications. We will revise and extend the sections describing both the inverse modelling routine and the matrix method.

Orbital forcing is included in our model in two separate ways. First, it is included as a term in the calculation of surface melt in the mass balance module (the insolation-temperature model). Second, it is included in the matrix method in the interpolation equation. The matrix method contains two separate interpolation equations that are used to combine the GCM snapshots; one for temperature, and one for precipitation. The one for precipitation is based on ice-sheet geometry, to account for orographic forcing of precipitation. The interpolation routine for temperature is based on "absorbed insolation": the product of insolation at the top of the atmosphere, and (1 – albedo). This accounts for both the changes in albedo resulting from ice-sheet advance and retreat, and the changes in insolation resulting from orbital forcing. While an extension of the climate matrix that includes GCM snapshots that have been calculated for different orbital configurations (so that the direct effect of insolation changes on surface temperature is also include) is planned, it is not included in the model version described here.

We will make sure that this is properly explained in the Methodology section.

P5 L10 – P6 L21: extended and clarified Sect. 2.1 (Methodology – Inverse modelling)
P19 L19 – P21 L8: added Appendix A, which presents and (briefly) explains the equations governing the matrix method

The model validation is based on the comparison of reconstructed CO2 over the past 800 kyr with the ice core data. The authors compare the results of their current study with several others and conclude that they are the best. However, it is obvious that comparison results of inverse modelling with forward modelling presented in Willeit et al (2019) is the same as comparison of apples with cucumbers. The inverse model is forced by benthic d18O which is already highly correlated with CO2 (correlation coefficient is 0.86). The authors should make this point very clear. The only surprising thing in this table is the extremely poor performance of Stap et al. (2017). Unfortunately, the authors themselves admit on page 15 that they cannot explain this fact. In fact, it is much more instructive to compare the result of a rather complex inverse modelling approach used by the authors to a simple linear regression of d18O from LR04 stack. Surprisingly (or maybe not) this simple "model" outperforms Berends et al. Indeed, it has R2=0.71 (versus 0.68 in Berends et al.) and rms=13.8 ppm (vs. 15.3) for "simulated" CO2 concentration over the last 800 kyr. After such a comparison, the numbers in Table 1 do not look very impressive. For the rest of Quaternary, results of Berends et al. also do not differ much from this simple regression model. After all, it is rather expectable (and have been demonstrated by Willeit et al., 2019) that CO2 also followed ice volume variations during 41-kyr world but with a smaller amplitude.

We apologise to the reviewer if our manuscript seemed to suggest that our own method is "best"; we agree that there is no meaningful way to declare one modelling approach "better" than another. We also agree that the results of the simple linear

regression proposed by the reviewer should be included in the comparison. Indeed, this helps to illustrate what we believe is the main conclusion from this comparison; more complex models, including more elaborate physics and describing more components of the Earth system, are useful for studying large-scale relations between these components, but are not necessarily better at resolving the evolution of a single component or parameter. We will clarify this in the text.

P13 L24 – P13 L31: added a simple linear regression to the statistical comparison

We will also follow a suggestion from Matteo Willeit (the main author of Willeit et al., 2019, who contacted us shortly after the discussion version of our manuscript was published, with a similar question about the comparison of correlations to the ice core record), to include an optimised time lag for each model CO2 reconstruction before calculating the correlations with the ice core record. This mainly affects the results of Stap et al. (2017) and Willeit et al. (2019), where the coefficients of determination R2 for both studies increase from 0.25 to 0.45. While this extra step does not alter the conclusions we draw from this comparison, it does more properly give credit to the results of the different studies.

Following a comment by Anonymous Reviewer 1, we have also updated all figures and numbers to use the more recent ice-core CO2 record by Bereiter et al. (2015), rather than Lüthi et al. (2008).

The real question is what was CO2 concentration at the end of Pliocene. And here I see a real problem with the results presented in Berends et al. Indeed, if during the entire Pleistocene, CO2, ice volume and d18O variations were essentially identical, during the late Pliocene CO2 get really wild. Figure 4 shows several CO2 oscillations with the amplitude above 100 ppm. Of course, this is not 200+ ppm as in Stap et al (2017) but still a lot. As the scientist who has been heavily involved in *explaining glacial-interglacial CO2 variability, I must confess that it is extremely difficult to explain 80 ppm change in CO2 concentration even for the full glacial cycles of the late Quaternary. What could cause even larger Pliocene variations in CO2 without any obvious external forcing, the authors do not explain. This is why I strongly suspect that the reason for such weird behaviour of CO2 before Pliocene-Pleistocene transition is that the inverse modelling of CO2 concentration based on benthic d18O beyond Quaternary represents an ill-posed problem.*

We agree that our results for the late Pliocene are by no means the definitive answer to the question of how the world looked like in terms of CO2, global climate and ice sheet geometry. In our view, the main problem here is the relatively large changes in benthic d18O. Explaining these requires either very large changes in deep-sea temperature, moderately large changes in global ice volume, or (more likely) a mix of both. Our model results tend towards the "temperature" end of this spectrum, resulting in large changes in CO2: almost 100 ppmv difference between the coldest point of the Pliocene during M2, 3.3 Myr ago, and the warmest point during KM5c, 3.205 Myr ago (for our default simulation; in the low-CO2 end member, this difference reduces to about 85 ppmv).

We suspect that the relative sparsity of our climate matrix for warm worlds might result in a bias towards lager ice sheets in warm climates. This means that increasing modelled CO2 above present-day levels does not cause as much ice-sheet retreat as it maybe should, so that benthic d18O does not decrease so much. In order to reproduce the observed d18O record, the inverse routine will compensate by increasing CO2 until the resulting deep-sea temperature change is enough to produce the required change in d18O. This also explains the very large uncertainty range resulting from our sensitivity analysis; an additional change of 0.1 per mille in d18O, for constant ice volume, requires a very large change in deep-sea temperature, mean annual surface temperature, and CO2. Changing our climate matrix such that warm climates will lead

to more ice-sheet retreat will essentially shift the blame for the high d18O variability from the temperature end of the spectrum towards the ice volume end; it will reduce the modelled CO2 variability during the late Pliocene, but it will also increase the sea-level high stands.

Lastly, looking at proxy-based reconstruction, the boron isotope data by Martínez-Botí et al. (2015), which has both the highest temporal resolution and longest temporal range of all available proxies, shows a variability during the late Pliocene of about 150 ppmv (albeit with an uncertainty of about 100 ppmv in either direction). While certainly not definitive, especially considering the large discrepancies between difference boron-based reconstructions (as also discussed in our manuscript), their data does seem to suggest strong CO2 variability in warmer-than-present worlds.

We will extend the Discussion section of the manuscript to reflect these thoughts.

P18 L15 – P18 L28: added a paragraph to the Discussion section, discussing the variability and uncertainty in our CO2 reconstruction during the late Pliocene.

The authors wrote on page 9 that "uncertainties are conservative in this study". What the authors mean under "conservative" is not clear to me. To me, the estimate of uncertainties in this study is overoptimistic at best. Even if the maximum error in benthic d18O is indeed only 0.1 promile, the methodology has a number of other uncertainties related both to forward model and to conversion between climate characteristics (ice volume, temperature) and d18O. For the large glacial cycles of Quaternary even a larger uncertainty still does not prevent a reasonable estimate of CO2 but the situation is very different prior to 2.7 Ma. Before Quaternary, the model "assumes" very little variability in global ice volume and thus most of d18O variability has to be attributed to CO2 change and this is precisely what the model does. However, in this case, even uncertainty of +-0.1 promile already constitutes a serious problem. Indeed, *0.2 promile correspond to about 1C change in the deep-water temperature which in turn corresponds to 1.5C in global air temperature. The later number corresponds to change of CO2 (assuming climate sensitivity =3C) from 280 to 400 ppm. Thus, even with a very optimistic estimate of the method uncertainty, for pre-Quaternary climates this method cannot distinguish between a possibility that CO2 was as low as the preindustrial one or that it was as high as the current one. Obviously, such "reconstruction" is not very helpful.*

The statement that "uncertainties are conservative" was intended to indicate that the uncertainties we report are only those that arise from the sensitivity analysis described in the manuscript. This is simply the sensitivity of the model to uncertainties in the d18O record, which we showed in our 2019 publication to be larger than the sensitivity to uncertainties in other model parameters. We agree with the reviewer that this is not at all the same as the real uncertainty in our results; there are many other factors introducing uncertainties that cannot be quantified through such sensitivity analysis, and these are likely to be larger still than the numbers we report.

We will extend the Discussion section of the manuscript to reflect this, especially in relation to the previous comment about the difficulty of interpreting the d18O record in the late Pliocene.

P18 L15 – P18 L28: added a paragraph to the Discussion section, discussing the variability and uncertainty in our CO2 reconstruction during the late Pliocene.

"80/120 kyr cycles". Although this is not very essential for the manuscript under consideration, but the authors used the expression "80/120 kyr cycles" (actually it should be 82/123) several times in this and previous papers which provokes me to make the following comment: The durability of "two or three obliquity cycles" myths is amazing since it is not supported by real data! Glacial cycles of the late Quaternary

have average periodicity close to 100 kyr which explains strong 100 kyr peak in the frequency spectra of ice volume. It is true that the durations of individual glacial cycles deviate significantly from 100 kyr but they also do not cluster around 80 and 120 kyr (see for example Table 1 in Konijnendijk et al., 2015). In fact, durations of individual glacial cycles are relatively uniformly distributed between 80 and 120 kyr with half of the cycles been closer to 100 kyr than to 80 or 120 kyr.

While we believe that declaring the 80/120 kyr hypothesis to be a "myth" is overly dismissive of the studies supporting this hypothesis (especially when considering the difficulties in constructing insolation-independent age models, described by Huybers and Wunsch, 2004), we agree with the reviewer that it is important to mention the ongoing discussion about the nature of the late Pleistocene glacial cycles. We will clarify this in the manuscript.

P14 L11 – P14 L17: added a few lines about the 100 vs 80-120 kyr discussion.

P.3, L.8 "proxies for global mean temperature"? Greenland and Antarctic records present proxies only for local temperatures which differ significantly from the global one
Changed this.

P3., L.10. "In that case ocean water temperature can be resolved as closure term from the benthic signal" This is not clear
Changed this.

P. 4, L.9 The definition of "entire climate system (atmosphere, ocean, cryosphere, carbon cycle, etc.)" is not consistent with contemporary terminology. Such system is named Earth system and Earth system models describe not only "physical processes"

(L. 10).
Changed this.

p. 4, L. 21 "the known relations between atmospheric CO2, global temperature and climate, and ice-sheet evolution". Why authors think that these relations are "known". Even the relation between CO2 and global temperature is still not well-known.
We agree that our phrasing was unclear. We will clarify this in the manuscript.

p. 5. I am not sure I understand why the authors put "data" and "model" in quotes.
These words are put in quotes because they constitute rather informal, but also obvious, descriptions of the aim and general approach of the studies we describe. While it is not possible (or desirable!) to draw a clear line between data studies and model studies, many publications about paleoclimate either rely mostly on the presentation and interpretation of proxy data, or on the development and application of modelling methods. We feel that it is important to explain the distinction between them, and how our work relates to both.

P.7 L. 11 "The reconstruction by Laskar et al. (2004) is used to prescribe time- and latitude-dependent insolation". Insolation is not reconstructed by computed using physical laws. This is why orbital forcing can be calculated for the past and future with the same (very high) accuracy.
We will replace "reconstruction" by "solution", in line with the phrasing by the authors of Laskar et al. (2004).

P. 11, L. 2. "so any possible contribution from Antarctica to changes in sea-level ... is not accounted for in their reconstruction". This is an incorrect statement. It is written in Willeit at al. (page 6) "Sea level is computed from the volume of modeled NH

ice sheets assuming an additional 10 percent contribution from Antarctica".
We will correct this in manuscript.

P. 15, L. 15. "... show a CO2 "threshold" for glaciation and sea-level drop around 250 ppmv". Our studies (e.g Ganopolski et al., 2016) do not support the existence of a single CO2 threshold for glaciations. To the contrary, we found that glacial inception is determined by a combination of insolation and logarithm of CO2 concentration.
We will correct this in manuscript.

Fig. 4. It is not explained what shading shows in this figure.
Shaded areas indicates the uncertainty in the LR04 benthic d18O stack, and the resulting uncertainty in the reconstructed CO2 and sea level. We will clarify this in the manuscript.

The reference Stap et al. (2017) is not in the reference list.
Added this reference.

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