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Climate of the Past
Discussions

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 Anonymous Referee 1

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.

The study by Berends et al. - if I understand it correctly - does not need either

of the two to get the transitions and the change in cyclicity right. The entire record (including changes in amplitude and cyclicity) is entirely controlled by CO2. In particular, it has a constant relationship between d18O_benthic and CO2 over time and does not change the flow conditions at the bed. I highly recommend that the authors elaborate on this extensively in the Discussion and the Conclusions and discuss what this potentially implies for our understanding of the glaciation history.

We use the "inverse modelling method" that does indeed use the benthic d18O record as forcing to calculate a CO2 history, but this is not based on a constant relationship. Eq. 1 relates the rate of change of modelled CO2 to the discrepancy between modelled and observed d18O. The fact that it is the rate of change of CO2 rather than the value itself mostly serves to suppress high-frequency oscillations in the benthic d18O record, resulting in a more "smooth" CO2 reconstruction. What's more important is the fact that this rate of change is determined by the difference between modelled and observed benthic d18O. Our modelled benthic d18O value is derived from both the deep-sea temperature (which is derived from the mean annual surface temperature, which depends on modelled CO2), and from modelled global ice volume. If, for example, the modelled benthic d18O value is too low (not "glacial" enough), then the inverse routine described by Eq. 1 will slowly decrease the modelled CO2. This will increase modelled d18O first by cooling the climate, lowering the deep-sea temperature (with a prescribed time-lag, using a moving average), and also due to the increase in global ice volume resulting from that cooling. When modelled benthic d18O is in line with the observed value, the downward trend in CO2 stops and the model stabilises, until the observed d18O value changes again.

We will extend and clarify the text describing the inverse routine, so that the reader will be able to comprehend the concepts behind it without having to read our earlier publications to which the manuscript referred.

P5 L10 – P7 L7: extended Sect. 2.1 (Methodology – Inverse modelling) so that it can be understood by readers unfamiliar with our previous work.

Regarding the implications of our CO2 reconstruction: the reviewer is correct in stating that our model does not account for possible changes in basal conditions during the MPT. This means that, in our view, our CO2 reconstruction represents how CO2 should have evolved over time in order the produce the observed d18O record, if no changes in basal conditions occurred. If we were to repeat our simulations and prescribe more basal sliding in pre-MPT Eurasia and North America, the reconstructed CO2 would look different, likely both in terms of the glacial-interglacial amplitude, and the background trend. This is something we plan to investigate in future work, as it would provide useful context for interpreting the expected new ice-core record.

We agree that this is something that needs to be discussed in the manuscript. We will add a paragraph to the Discussion section.

P19 L4 – P19 L15: added a paragraph to the Discussion section.

Related to this, the authors set out in the (very nice!) introduction that these experiments will help us to understand and quantify Earth System Sensitivity. I regard it a missed chance that they do not pick up on this issue in the Conclusions as they seem to have all data at hand to contribute to this discussion. Given that the paper in its current form is relatively short, there is enough space to elaborate on this.

The relation between atmospheric CO2, ice sheet geometry, and global climate is not explicitly included in our model. Rather, it is a result of the model physics of HadCM3, the GCM which was used to generate the "snapshots" included in our climate matrix. The Earth System Sensitivity that would be derived from our results would therefore just be that of HadCM3. Our model mainly provides insights into

the long-term relation between CO2 and sea-level, which we believe we adequately discuss in the manuscript. The mention of earth system sensitivity in the abstract of our manuscript was inaccurate, we will change this.

P1 L9: changed phrasing in abstract to remove inaccurate mention of "Earth System Sensitivity".

I am puzzled by the way the deep ocean temperature, which influences the d18O_benthic signal, is calculated. In the manuscript and in Berends et al. (2019) the authors say that they used the global average of the surface temperature anomaly. Either there is some detail missing here (some scaling) or this seems to be at odds with the measured deep ocean temperature today. Here it is important to take into account that deep ocean temperatures have a strong bias towards the sea surface temperatures at deep water formation sites, which are located in high latitudes. There is also eddy diffusive transport of heat in lower latitudes, but the low deep water temperatures clearly point to a dominating role of deep water formation. In fact, the resulting deep ocean temperature, which is caused by the balance between advective transport of cold water from deep water formation and the diffusive entrainment of heat downwards, is also dependent on the strengths of the Atlantic Meridional Overturning Circulation (Galbraith et al., GRL 2016), which is also not included in the approach by Berends et al. The value of the deep ocean temperature used in the study by Berends is not mentioned in the paper. It is likely too warm, but this is also likely compensated by the CO2 sensitivity (120 ppm/permille) with which their approach is optimized. Even if there is some scaling involved that is not described in the manuscript, using the global average surface temperature appears to be an oversimplification. Accordingly, I think it is important to discuss this issue and use some alternative sensitivity runs, where the deep ocean temperature is parameterized by high latitude temperatures and the model CO2 sensitivity is recalibrated to show that the final result is not sensitive on the choice of the deep water temperature template.

Our modelled deep-sea temperature anomaly, which is used to calculate the modelled d18O, is derived from the northern high-latitude temperature anomaly (which is calculated as the mean temperature anomaly over the North America and Eurasia model grids), multiplied with a scaling factor (0.25 in our model) and smoothed over a 3,000 yr moving time window. This was not made clear in the manuscript; we will rectify this. The actual values this yields for deep-sea temperature changes are discussed in two of our earlier papers (Berends et al., 2018, 2019), and comparable with other studies showing a deep-sea cooling of 2 - 2.5 K during the last glacial maximum, which is agreement with proxy-based results (Shakun et al., 2015) We will add these numbers and references to the text.

P6 L19 – P6 L21: explained the relation between surface temperature, deep-sea temperature and d18O.

We also agree our method greatly simplifies the realistic relation between the global climate and benthic d18O. A more elaborate parameterisation based on ocean currents (which could be modelled ocean currents from the GCM snapshots, so that the relation can change over time using the same climate matrix) and global, spatially variable temperature anomalies, might be a significant improvement on this, without going so far as to run a fully isotope-enabled GCM. However, we believe that such work, while undoubtedly very interesting, lies beyond the scope of the current study. We will add a paragraph to the discussion section discussing this.

P18 L30 – P19 L2: added a paragraph to the Discussion section.

abstract line 2: "...over geological time scales... "
Added this.

abstract line 3: "...past CO2 concentrations, thus its radiative forcing, only... "
Added this.

abstract line 16-17: please do not use unexplained abbreviations such as KM5c and M2 in the abstract. Please also indicate such stages after you introduced them in the main text in the figures.
We have removed M2 and KM5c from the abstract. Since they were not mentioned anywhere else in the text, we did not add them to any figures.

page 2 line 4: "... atmosphere from the time..."
Changed this.

page 2 line 6 and throughout the manuscript: Here you cite Bereiter et al. (2015) for the CO2 record, but later you cite LuÌĹthi et al., 2008. Note that in Bereiter et al., a correction of the EPICA Dome C CO2 values by LuÌĹthi et al. was introduced for ice older than about 600 kyr. You should use the corrected Bereiter et al. data throughout the manuscript.
All figures, model-data comparisons, reported correlations, and citations have been updated to use the corrected Bereiter et al. data.

page 2 line 9: "... have measured d11B..."
Changed this.

page 4 line 14: "... can help interpreting..."
Changed this.

page 6 line 16: Here the d18O/CO2 scaling parameter is introduced. While it has been mathematically introduced in equation 1, it would be helpful to discuss the meaning of this parameter in more detail and also discuss what it implies if this

parameter is assumed to be constant over time.
The meaning of this scaling parameter is explained in the new, extended section described the inverse modelling method.

page 8 line 14: it is not entirely clear to me what you mean by "combining the GCM snapshots according to position of the ice-sheet model in the climate matrix". I am sure there is an easier way to explain this.
Following a suggestion from reviewer 2, a short appendix has been added where the equations of the matrix method are presented and (briefly) explained.

page 10 line 6: "... which cover the last 500 kyr and 5.5 Myr, respectively."
Changed this.

page 11, line 2-4: here you say that the model study by Willeit does not include the Antarctic ice sheet, but the results look quite similar. What does this imply?
Reviewer 2 informed us that we missed a line in Willeit et al. (2019), stating that the Antarctic sea-level contribution is assumed to be 10
page 11 line 10: "... close to those..."
Changed this.

page 12 line 9: "d11B"
Changed this.

page 12 caption Figure 6: The references are assigned wrongly in the caption. HoÌĹnisch et al. (2009) and Bartoli et al. (2015) use d11B, not alkenones. It is correctly referenced in the main text.
Changed this.

page 13 line 10: You write "which they prescribed". Who is "they" in this case,

please provide the reference.
“They” are Willeit et al. Added this reference to the text.

page 13 line 19: "... different boron isotope based records is such..."
Changed this.

page 15 Figure 8: It would be helpful if the data younger than 800 kyr and those older than 800 kyr could be discerned in the model runs. Use different symbols or colors.
Changed this.

page 15 line 12: Here you say that the reconstructions by van de Wal and Willeit have a smaller spread. Do you mean in CO2 or sea level? In particular, for the reconstruction by Willeit I do not see a significantly smaller spread except that the sea level is capped.
van de Wal and Willeit have less spread in sea level for each CO2 value (or less spread in CO2 for each sea level value), though this is indeed more obvious for van de Wal. We will clarify this in the text.

page 16 line 21: "d11B"
Changed this.

page 16 line 22-24: Here you say that the data/model comparison in terms of CO2 is not conclusive, but before you showed that the highest resolution d11B data by Chalk et al., show perfect agreement with your reconstructions. I think you undersell the d11B values. Clearly each individual d11B based CO2 value has an analytical uncertainty on the order of 20 ppm, but measured in high enough resolution/replication, these data are quite useful to validate your model results.
Chalk et al. (2017) published a collection of boron isotope proxy data around the MPT,

shown in orange in the middle panel of Figure 6 of our manuscript:

While our results (and those of the other models) agree well with data points below 280 ppmv, there is a clear mismatch in both magnitude and timing of the high interglacial values around 1150 and 1100 kyr (which probably is related to our model's poor performance for warmer-than-present climates). We therefore do not believe that the agreement between these proxy data (yellow dots) and our model results (black line) can be called "perfect". Even if we assume that the autocorrelation of the individual proxy data points allows us to assume a lower measurement uncertainty, the difference between the median of the proxy data and the multi-model mean is much larger than the differences between the different models. This justifies our assertion that these proxy data cannot be used to choose one model reconstruction over the others.

We will clarify this line of reasoning in the manuscript.

P18 L15 – P18 L28: added a paragraph to the Discussion section.

page 16 line 32: "... for a colder-than-present..."
Changed this.

page 17 line 4: The last sentence is weak and does not give credit to the work performed in this study. The authors should elaborate much more on this, as outlined in my general comments above.
We have added two paragraphs to the Discussion section, discussing the various sources of uncertainty in our results, and how we believe our reconstruction should be interpreted.

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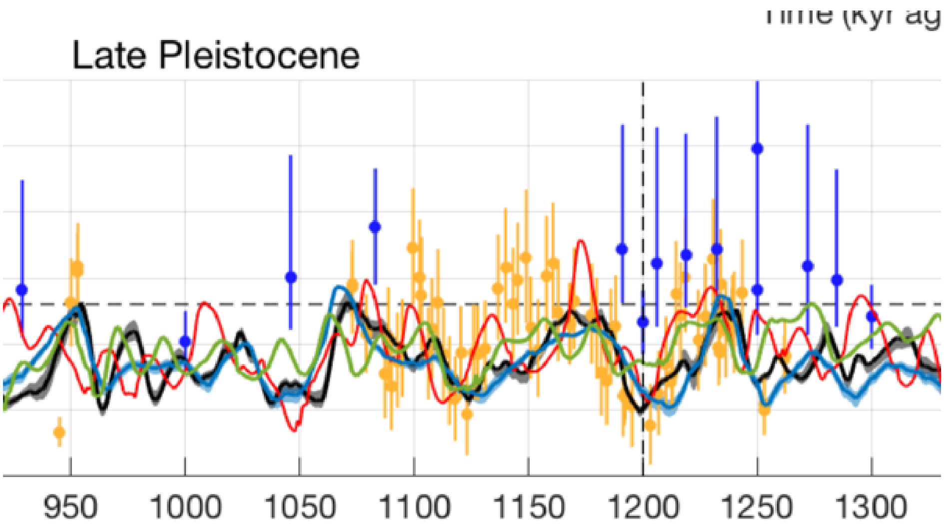
Time (kyr ag
Late Pleistocene
950
1000
1050
1100
1150
1200
1250
1300

Fig. 1.