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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.

Anonymous Referee #1

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Overall assessment

The paper by Berends et al. represents an extension of a previous study by the same authors published in CP with the aim to obtain a consistent history of CO2, ice sheet size/sea level and deep ocean temperature over the last 3.6 million years using an inverse modeling approach. As such the paper is well suited for publication in CP and the results have great potential to improve our understanding of Earth System Sensitivity and ice sheet development over several time intervals critical for the glaciation history: the Pliocene/Pleistocene Transition (PPT), the Mid Pleistocene Transition (MPT) and the Mid Brunhes Event (MBE). The paper is also well written and supported by

sufficient figures and tables. Having said that, I feel the paper in its current version does not make full use of its potential. Moreover, I have some methodological questions/suggestions for improvements, which should be addressed before acceptance. Accordingly, I recommend the paper for Major Revisions. Although these revisions may require a bit more time, I regard them as relatively straightforward.

General Comments:

Regarding my first point that the paper could become of greater impact, I raise the following questions:

a) The three transitions mentioned above represent enigma in our understanding of the glaciation history. In particular, the shift in glaciation cyclicity from the obliquity driven cycles prior to the MPT and the 80-100 kyr cycles thereafter gave rise to the so called regolith hypotheses (Clark et al., WSR 2006). This hypothesis invokes a change in the Laurentide ice sheet bed conditions that changes the ice sheet flow, hence the ice sheet cross section. Another study by Tzedakis et al. (Nature 2017) invokes a change in an energy threshold for deglaciation over the MPT caused by summer insolation. 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. 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.

b) 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.

Specific comments:

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

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

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.

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

page 2 line 6 and throughout the manuscript: Here you cite Bereiter et al. (2015) for the CO2 record, but later you cite Lüthi et al., 2008. Note that in Bereiter et al., a correction of the EPICA Dome C CO2 values by Lüthi et al. was introduced for ice older than about 600 kyr. You should use the corrected Bereiter et al. data throughout the manuscript.

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

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

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.

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.

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

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?

page 11 line 10: "... close to those..."

page 12 line 9: "d11B"

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

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

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

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.

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.

page 16 line 21: "d11B"

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.

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

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.

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