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Climate of the Past Discussions

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Interactive comment

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.

Andrey Ganopolski (Referee)

andrey@pik-potsdam.de

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The manuscript by Berends et al. presents a new step in the application of inverse modelling approach in conjunction with forward modelling to study past climate variability, the approach which the Utrecht group explores already over a long time. I believe that this is a very promising approach which will allow us to learn more about past climate dynamics and internal consistency of different paleoclimate reconstructions. Unfortunately, I have a number of problems with this manuscript which require clarifications and critical discussions. I also believe that it is absolutely crucial to properly estimate the real uncertainties of the proposed method.

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Discussion paper

General comments

1. 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”.

2. 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

CO2=175+50.2 (5.2-s),

where "s" is 5000-years averaged 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. 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.

3. 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. 4. "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.

Specific comments

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

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

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).

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.

p. 5. I am not sure I understand why the authors put “data” and “model” in quotes.

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.

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% contribution from Antarctica”.

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.

Fig. 4. It is not explained what shading shows in this figure.

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

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