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Climate of the Past Discussions

Interactive comment on "Continuous and self-consistent CO_2 and climate records over the past 20 Myrs" *by* R. S. W. van de Wal et al.

Anonymous Referee #2

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This manuscript aims to estimate atmospheric CO2 of the past 20 million years from benthic foraminiferal oxygen isotopes, where the ice volume component of d18O (i.e. sealevel) was used to force the ice-sheet volume, and temperature was subsequently estimated to match independent sealevel-observations.

This is an interesting study that should get published, however, much more information is needed for the reader to evaluate the quality of the reconstructions. For instance, on page 439 line 27 ff the favorable comparison with independent sealevel and temperature records is mentioned, however, it is not mentioned that the sealevel records are restricted to the Pleistocene only, whereas Lear et al. (2000) also estimated both sealevel and bottom water temperature from benthic foraminiferal d18O and Mg/Ca for the entire Cenozoic. I would like to see a comparison of this new reconstruction

with published estimates, a discussion why sealevel was not compared with the longer Lear et al. (2000) record, and consideration of the carbonate ion effect on benthic Mg/Ca (e.g. Yu & Elderfield 2008, EPSL, Sosdian & Rosenthal 2009). How do the estimates compare to Lear et al. 2010 (Paleoceanography), during the Miocene? A demonstration of the "favorable comparison" is necessary to evaluate this new reconstruction. In addition, how is deep-sea temperature compared to surface temperature? Please explain briefly the procedure outlined in Bintanja et al. 2005b. How has this parameterization been validated?

Page 442, line 2ff: What is the effect of a lack of the bipolar seesaw on the climate system and how could the result of this study be changed if the bipolar seesaw and Dansgaard/Oeschger events were included?

Page 444, line 3/4: is that air or surface water temperature?

Page 445, line 14ff: Temperature is used to select CO2 records that are consistent with a temperature/CO2 relationship comparable to ice cores. This procedure seems circular and dangerous, as it assumes that the CO2/T relationship in the past was comparable to the Pleistocene. In particular the B/Ca reconstruction by Tripati et al. 2009 falls in this same trap, as B/Ca varies little over their study period, and variations in their CO2 estimate are largely driven by secondary corrections such as temperature variations.

Page 445, line 21: it is argued that Pearson and Palmer used multiple species for their reconstruction, where they actually used predominantly a single species for the past 20 million years. In order to remove the species argument, only Pearson & Palmer's single species data could be compared to independent CO2 estimates. However, it should also be kept in mind that Pearson & Palmer used smaller size classes in their earlier samples, which may reflect a deeper growth habitat at lower pCO2, and that they applied a correction for the isotopic composition of seawater, and modeled alkalinity, all of which are debatable.

I find the overall treatment of proxy data in this comparison rather questionable, as proxies that extend the CO2/T relationship observed in ice cores are deemed more reliable than others. While the authors are modelers and may not fully understand the pitfalls of each proxy reconstruction, I find the conclusion that "the various CO2 proxies can be understood in the broader framework of long-term climate change" (page 451, line 22/23) rather bold and haphazard. Although this comparison may identify proxies that deviate from the average, it does not help to identify whether one proxy estimate is better than the other, or whether a proxy may find a reasonable answer for the wrong reasons. Such conclusions should be left to decide by the proxy community, not by consistency with a modeling estimate.

Page 446, line 7/8: Please provide a figure or further evidence of how the different CO2 proxies individually affect the modeled temperature and CO2 estimates.

Page 446, Line 20-25: It should be mentioned that Hönisch et al. 2009 specifically selected glacial/interglacial extremes for their reconstruction and found stable interglacial pH and pCO2 values before 1 Ma, followed by a decrease in both G/I extreme pCO2 between 0.8-0.6 Ma (comparable to ice cores). Although the average pCO2 across the MPT appeared to decrease (which would be comparable to this modeling study), the overall similarity of interglacial pCO2 was taken as an indication that Carbon was not generally removed from the active carbon reservoirs but only temporarily stored e.g. in the deep ocean. To better evaluate the quality of each proxy, it might be useful to consider the sampling strategy of different proxy reconstructions (random or G/I extremes) in comparison to their CO2 estimates.

Page 446/Line 25/26: Please make sure that the reader does not get the impression the "combined d11B and alkenone record" is a new technique. Seki et al. 2010 studied both proxies independently and compared their results. Seki et al. 2010 argued that their sampling strategy may have favored interglacials, which may explain the high CO2 values estimated for the past 1.5 Ma.

Page 447, line 4: please specify that B/Ca was measured on planktic foraminifers. It seems that the authors hint at something but do not complete the thought.

Page 447, line 11: please specify which fast and slow feedbacks have been considered.

Page 447, Line 21: "functional relationship between DeltaT and CO2" sounds like one driving the other. Please rephrase, possibly using "quantification of the covariation between Delta T and CO2"

Page 449, Line 1-3: How does the estimated NH temperature change compare to terrestrial proxy estimates?

Page 449, equation 5: Please explain the parameters used for the calculation of climate sensitivity.

Page 449, Line 19: please add references for the potential change in meridional temperature gradient

Page 449, Line 22-25: How much higher should/could CO2 have been? How do d11B and alkenones compare to that expectation?

Page 451, line 16: What is meant by "the trend in CO2 before the inception is strong"? Please rephrase.

Page 452: Please rephrase this paragraph. This reads as if proxy data cannot be trusted, when what I believe the authors would like to say is that absolute proxy data have to be considered in the framework of specific Earth system parameters at that time.

Figure 1: What determines "zero" in panel b?

Figure 2b: The individual contributions to sealevel seem to match the integrated 3D estimates at sealevel >0m but a large part of the variation is missing at sealevel <0m. What makes up this difference? Please specify that sealevel validation with independent estimates was only done for the Pleistocene. A) "thick lines in lower panel" is

confusing, please rephrase.

Figure 3: Seki et al. 2010 did not “combine” d11B with alkenone estimates, they used both techniques and compared the results.

Figure 6: This figure should be plotted larger, the turquoise symbols can barely be seen. The horizontal bar on the middle panel may be better replaced by a vertical bar laid behind the data. Caption b) should read “NH glacial inception”

In summary, this is an important contribution that may likely attract many readers. Although several explanatory studies are referred to, I think it would be useful if this manuscript could be easier understood without having to read multiple secondary papers describing the modeling methods and constraints applied. The introduction and treatment of proxy data should be somewhat revised and conclusions about their reliability toned down.

Interactive comment on Clim. Past Discuss., 7, 437, 2011.