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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.

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van de Wal present here a novel way to calculate temperature and CO2 for the last 20 Myrs and explore the implications of this association, especially with regards to climate sensitivity. This topic is of significant interest to many, and the approach taken by the authors is novel and compelling. Overall, the manuscript is in pretty good shape except for the discussion of climate sensitivity. I begin with some comments with the broadest significance, followed by a list of more detail-oriented comments.

Abstract: The abstract revolves around the second sentence ("The lack of transient climate models and in particular the lack of high-resolution proxy records of CO2, beyond the ice-core record prohibit however a full understanding of the inception of the Northern Hemisphere glaciation, as well as the mid-Pleistocene transition"). The authors

need to return to this key sentence at the end of the abstract. Do their data support the statement? Why or why not? As a whole, the abstract misses the mark. It doesn't focus on the areas that are emphasized most in the main body of the text (especially climate sensitivity). Also, on line 7, I don't think it is fair to say that the authors produced a "continuous high-resolution CO2 record". It is a modeling effort, and in essence what the authors have done is "filled in the gaps" between existing proxy records assuming an averaged transformation between temperature and CO2 (Figure 5).

p. 440: Please be clear here and throughout (including Figures 1, 4 & 5) what you mean by reconstructed temperature. Is this deep-water temperature or surface temperature? And if it's surface temperature, is it sea surface temperature or land+sea surface temperature? Is this temperature fully-integrated from equator to pole? »>Ahh, I see the answers to my questions on p. 444. This information needs to be stated back on p. 440!! And it should be given in abbreviated form in the captions for Figures 1, 4, & 5. Just saying "temperature" or "NH temperature" is not sufficient.

p. 445, lines 21-24: The two Pagani alkenone records should be combined: they come from the same author, following the same methodology, etc. Some key stomatal-based estimates are missing. They are from: Kürschner, W.M., van der Burgh, J., Visscher, H., Dilcher, D.L., 1996. Oak leaves as biosensors of late Neogene and early Pleistocene paleoatmospheric CO2 concentrations. Marine Micropaleontology 27, 299-312. Once these are added, the "stomata" regression slope should be steeper. And finally, the Pearson and Palmer (2000) data can be rejected for reasons related to diagenesis, use of incorrect fractionation factors, and poor modeling of seawater alkalinity and δ11B (Foster, G.L., Ni, Y., Haley, B., Elliott, T., 2006. Accurate and precise isotopic measurement of sub-nanogram sized samples of foraminiferal hosted boron by total evaporation NTIMS. Chemical Geology 230, 161-174. Klochko, K., Cody, G.D., Tossell, J.A., Dera, P., Kaufman, A.J., 2009. Re-evaluating boron speciation in biogenic calcite and aragonite using 11B MAS NMR. Geochimica et Cosmochimica Acta 73, 1890-1900. Klochko, K., Kaufman, A.J., Yao, W.S., Byrne, R.H., Tossell, J.A., 2006.

CPD

7, C26–C31, 2011

Experimental measurement of boron isotope fractionation in seawater. Earth and Planetary Science Letters 248, 276-285. Lemarchand, D., Gaillardet, J., Lewin, É., Allègre, C.J., 2000. The influence of rivers on marine boron isotopes and implications for reconstructing past ocean pH. Nature 408, 951-954. Pagani, M., Lemarchand, D., Spivack, A., Gaillardet, J., 2005. A critical evaluation of the boron isotope-pH proxy: The accuracy of ancient ocean pH estimates. Geochimica et Cosmochimica Acta 69, 953-961. Royer, D.L., Berner, R.A., Beerling, D.J., 2001. Phanerozoic CO2 change: evaluating geochemical and paleobiological approaches. Earth-Science Reviews 54, 349-392).

p. 450, lines 24-26: This doesn't make sense because the slope of the "ice" line in Figure 4 (red line) is steeper than the overall regression, implying a smaller change in CO2 for a given change in temperature.

Climate sensitivity section: This section is difficult to understand for several reasons. First, the authors present on p. 449 that the climate sensitivity calculated from present-day and late Pleistocene observations (i.e., "Charney" sensitivity; equations 4-5) is similar to the sensitivity calculated from paleo-CO2 and modeled temperature (Figures 4-5). Thus, it is difficult to figure out where the authors' conclusion for higher sensitivity in the ancient record comes from (see also previous comment). Second, the calculated climate sensitivity for doubled CO2 is $\sim$30K, or 12K for global surface temperature, for both the first principles calculation (equations 4-5) and the empirical calculation (Figure 5). This makes no sense because Charney sensitivity is typically around 3K. These problems require addressing. Further, the manuscript would be much clearer and compelling if the section on how paleo-sensitivity compares to Charney sensitivity was expanded. This is a topic of great interest to many, especially if the data are also presented in terms of doubled CO2 and global surface temperature. Along these lines, the papers of Hansen, Lunt, and Pagani should be discussed. They find climate sensitivities of 4-6+K per doubled CO2 for the late Cenozoic glaciation. (Hansen, J., Sato, M., Kharecha, P., Beerling, D., Berner, R., Masson-Delmotte, V., Pagani, M., Raymo, M., Royer, D.L., Zachos, J.C., 2008. Target atmospheric CO2: where should

CPD
7, C26–C31, 2011

humanity aim? Open Atmospheric Science Journal 2, 217-231. Lunt, D.J., Haywood, A.M., Schmidt, G.A., Salzmann, U., Valdes, P.J., Dowsett, H.J., 2010. Earth system sensitivity inferred from Pliocene modelling and data. Nature Geoscience 3, 60-64. Pagani, M., Liu, Z., LaRiviere, J., Ravelo, A.C., 2010. High Earth-system climate sensitivity determined from Pliocene carbon dioxide concentrations. Nature Geoscience 3, 27-30.) Third, the authors should explore the potential for variable climate sensitivity within their paleo-record. For example, if the CO2 proxies are correct, the authors overpredict CO2 from ∼7-10 Myrs, and perhaps from 2.5-10 Myrs too if the "Alk.+δ11B" estimates are incorrect. This means that climate sensitivity during this period was higher than the mean sensitivity calculated by the authors. This pattern may make physical sense because it would mean that sensitivity dropped going back into the mid-Miocene climatic optimum, when glaciers were at their minimum extent (and thus the ice-albedo feedback weakest).

Minor comments

p. 438, lines 16-17: "We find no evidence for a change in climate sensitivity other than the expected decrease following from saturation of the absorption bands for CO2." Climate sensitivity accommodates for the saturation effect (i.e., it is cast in log space), so the second half of the sentence is misleading and should be cut. The first half should be revised too, given that the authors conclude that climate sensitivity was higher than the present-day during their paleo-interval (!)

p. 438, line 20: "Finally it might be noted that we observe" This is awkward; change to something like "Finally, we note".

p. 438, lines 21-22: Why should the reader care about only minor CO2 changes during the mid-Pleistocene transition? A follow-up sentence is needed to put these data in proper context.

p. 438, line 23: This statement is somewhat misleading because it implies that the climate has steadily cooled. This of course is not true, for example during the mid-

CPD
7, C26–C31, 2011

Miocene climatic optimum.

p. 439, line 19: “we forced by then. . .” Not sure what this means.

p. 440, line 4: “It” should read “it”

p. 441, lines 22-24: Why are the authors restricting themselves to calculating Northern Hemisphere temperature if Southern Hemisphere ice sheet data are now available? This choice needs to be defended.

p. 442, line 16: Instead of saying “Miocene”, give the numerical age you are using for your point of comparison. During the mid-Miocene climatic optimum, for example, your modeled temperature change is close to 30K, much larger than the 12K figure you cite here! “Miocene” is too ambiguous.

p. 443, line 20: “gradually” should read “gradual”

p. 444, line 16: “Intriguing is the question. . .” Bad language.

p. 445, line 15: “there is a relation between CO2 and temperature”. This point should be made more nuanced, as the authors are comparing CO2 to temperatures adjacent to northern hemisphere ice sheets.

p. 447, lines 3-4: Or a problem with your model. . .

p. 449, line 7: “ïĄň”should read “ïĄğ”. Also, the authors say here f = 0.72 but on the previous page say 0.71.

p. 450, lines 12-17: This section is not highly related to the first part of the same paragraph. It would be much clearer if this section was its own paragraph.

p. 450, lines 14-16: This sentence is too opaque. The associated CO2 change is needed. Ditto for the present-day scenario.

p. 450, line 25: remove comma and “which are”

p. 451, line 21: remove comma

CPD

7, C26–C31, 2011

p. 452, line 1: The authors should clarify here that the mean changing sensitivity relative to the present-day, not changing sensitivity within their paleo-time series.

Figure 3: It's very difficult to figure out what's going on in this plot. I see several problems. First, the vertical scale is so compressed that it is difficult to see patterns in CO2. An obvious solution is to combine all subplots into a single unified plot. Second, why are there horizontal lines at 300 ppm? A more intuitive choice is to have the horizontal lines dividing each subplot. Third, it is difficult to figure out the magnitude of the minor tick marks. Yes, it is stated in the figure caption, and yes, a few of the subplots have "400" written on the right-hand side (why only a few? Why not all?), but if I am any measure of a typical reader it will take a few minutes to figure this out. This delay is not desirable at all. Again, a solution is to combine all subplots. Fourth, why are multiple ice-core studies combined into one subplot but not for the other records. For example, the authors split the two Pagani studies into two separate subplots. Why? At first pass, the figure gives the impression that alkenone records show a marked decline at 5 Myrs. Not good!! Finally, what is the star marked "100"? This is not described anywhere in the figure or figure caption.

Figure 5: The red dots are virtually impossible to see. Enlarge them.

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