Clim. Past Discuss., 9, C938–C943, 2013
www.clim-past-discuss.net/9/C938/2013/

Climate of the Past Discussions

Interactive comment on "A high resolution record of atmospheric carbon dioxide and its stable carbon isotopic composition from the penultimate glacial maximum to the glacial inception" *by* R. Schneider et al.

Anonymous Referee #1

Received and published: 28 May 2013

Review of Schnieder et al (CPD).

General Comments:

This manuscript covers an important topic, relevant to readers of Climate of the Past: the time-evolution of past atmospheric CO2 levels and the carbon cycle. The manuscript both presents and analyses new data that provides insight into the causes of past changes in atmospheric CO2. I must say that I am not qualified to comment on the details of the laboratory methodology used to collect the new data. As such, my review focuses on the way in which the data has been analysed and the conclusions

drawn. Spline work is conducted to find trends from the raw data – it seems to follow successful methods previously described in the literature. Overall, I find that the quality of the work behind the manuscript is suitable, after some revisions, for publication in the main journal.

My main concern with the manuscript at present is how some of the conclusions are stated, for example the conclusions about the past carbon cycle from the d13Catm and CO2 trends. In several places inferences about the nature of the past carbon cycle are stated as if they are known to be accurate. In reality, the inferences are known to be data-consistent, but they may or may not be accurate.

When making inferences about the nature of the past carbon cycle there can be a very large difference between an accurate past reconstruction and a data-consistent reconstruction, because many very different past carbon cycle scenarios can be simultaneously consistent with the same data. This is for two reasons [e.g. Goodwin et al, 2011]: (1) The system is unconstrained because there are fewer independent types of proxy than different past processes being reconstructed, or (2) The system is ill-conditioned, making small uncertainties in the proxies amplify into large uncertainties in the reconstructed past carbon cycle. This ill-conditioning occurs when (some of) the proxies happen to react similarly to the same carbon cycle processes.

I suggest a re-wording of some of the key conclusions of the manuscript to make clear the distinction between data-consistency and accuracy. These are outlined in Section (3) of the Specific comments. Following these re-wordings (and other suggestions raised in the specific comments section below) I would find this work suitable for publication in the main journal.

Specific Comments:

(1) Why it is difficult to reconstruct the causes of past changes in CO2.

The authors state the difficulties in disentangling the complex set of simultaneous

changes that could cause atmospheric CO2 to vary (Line 20 p 216 to line 5 p217):

"Various processes are known to influence changes in the carbon distribution and its isotopic signature between the ocean, the atmosphere, terrestrial and marine organic carbon, reactive sediments and the lithosphere. Multiple processes operate simultaneously, and interact with each other non-linearly (Ko ÌĹhler et al., 2005; Brovkin et al., 2007; Sigman et al., 2010; Fischer et al., 2010; Tschumi et al., 2011) allowing for a wide range of possible scenarios to explain observed natural changes in atmospheric CO2. Thus, an unequivocal interpretation of past variations in the global carbon cycle is difficult." I disagree with the emphasis of this paragraph being entirely on one reason: that difficulty in reconstructing the causes of past CO2 change is entirely down to multiple processes operating simultaneously and interacting non-linearly. While the simultaneous and non-linear interactions undoubtedly do not help, a recent study (Goodwin, Oliver and Lenton, 2011 in GBC) showed that the way in which the proxies combine together can be key to allowing an accurate past reconstruction. In the paragraph above, the authors should state that the way in which the available proxies combine together to constrain the system is important for determining how difficult it is to make an unequivocal interpretation of past variations in the global carbon cycle. For example, if there are adequate proxies that allow a well-conditioned reconstruction of the processes you are considering, then an unequivocal reconstruction of past carbon cycling is difficult but still possible. However, if the available proxies combine to only allow an ill-conditioned reconstruction then an unequivocal reconstruction of past carbon cycling is impossible. For these reasons I think the authors should say that the extent and nature of the proxies available helps determine how difficult it is to find an unequivocal interpretation of past variations in the global carbon cycle.

(2) Motivation for measuring d13Catm and the information it provides:

The authors state the specific motivation for measuring d13Catm: "The stable carbon isotope signal of atmospheric CO2 (δ13Catm) represents a valuable tool to constrain processes affecting the global carbon cycle. Scrutinizing the potential processes and

their contributions to the observed CO2 variations, using long-term δ13Catm data sets represents an objective way to analyse the carbon cycle of the past." I disagree with the emphasis here because d13Catm is a multi-proxy: its value is affected by multiple processes. Therefore, it is not very useful to measure in isolation (because no multi-proxy is very useful to measure in isolation). Re-stating this, it is necessary to measure many different proxies, but none are necessarily better than the others on their own. Again, it is how the proxies combine together to help disentangle the system that is important. The authors could improve their motivation for measuring d13Catm by stating that d13Catm provides a valuable tool, when used in conjunction with other proxies, to help disentangle the complex simultaneous set of processes affecting the carbon cycle in the past. Important for motivating measurements in d13Catm is it reacts differently than mean d13Cocean to ocean biology. This allows terrestrial and ocean biological signals to be disentangled if both d13Catm and d13Cocean are measured. The authors should see Table 4 in Goodwin et al (2011) – the coefficients describing the sensitivities of d13Catm and d13Cocean to terrestrial carbon perturbations are both the same sign, but the coefficients relating to ocean biological carbon perturbations are opposite signs. This means that it is very good to measure both d13Catm and d13Cocean. Since Oliver et al (2010) already provide a comprehensive look at d13Cocean, this adds to motivation for d13Catm measurements presented in this manuscript.

(2) Holocene CO2 rise

Section 5: p2041 line 22 to p 2042 line3 – When discussing the rise in CO2 during the Holocene, the authors should also see Goodwin et al, (2011) GBC which presents an assessment of all the processes that are data-consistent with the Holocene rise in CO2. It also shows how important d13Catm is for constraining the Holocene rise in CO2.

(3) Statements of accuracy rather than data-consistency:

P2033 line 11:

“In summary, taking into account CO2 and δ13Catm data, essentially the same processes have been active during Termination II as in Termination I but with different strengths, different relative timing and from different starting conditions”

This is too strongly worded, indicating that the following described sequence (lines 14 to 20) is known with greater certainty than is possible from CO2 and d13Catm alone. Firstly (and most importantly), the system is not adequately constrained from CO2 and d13Catm – because there are more than two potential processes to be analysed from only two proxies to measure (The authors later recognise this p2033 line 27, but it should also be alluded to in wording of above sentence). Secondly, I am not sure that the then described sequence is pinned down with certainty for Termination I – although it is likely as a leading contender in the literature.

I would suggest re-wording to state that the CO2 and d13C data are consistent with the same processes having been active during Termination II as Termination I, but with different strengths, timings and starting conditions. The current wording suggests that a conclusion is accurately known from the proxies, the suggested change indicates that the same conclusion is data-consistent with the proxies. I think the authors have shown the data-consistency of their conclusion, but not shown that their conclusion is necessarily accurate.

Abstract:

"Our isotopic data suggest that the carbon cycle evolution along Termination II and the subsequent interglacial was controlled by essentially the same processes as during the last 24000 yr, but with different phasing and magnitudes."

I think the isotopic data is ‘consistent with’ rather than ‘suggests’, but this is more subtle than the other examples. If in the main manuscript the issues surrounding the data-consistency versus accuracy are made clear, then ‘suggest’ would probably read fine here.

Technical Corrections:

(1) There is an o ÌĹ immediately prior to the word ‘Accumulation’ on line 26 p2026.

Interactive comment on Clim. Past Discuss., 9, 2015, 2013.