

## ***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 #2**

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Review of Schneider et al. CPD 9, 2015-2057, 2013

This paper is a mostly well-written description of an important new data set that will be of interest to a wide variety of scientists. It does not solve the problem of glacial-interglacial CO<sub>2</sub> or provide a definitive interpretation of the data, but as a first look at a new data set it is clearly useful and brings in some new ideas. I am particularly interested in the 0.4 per mil offset between the glaciations and suspect this will be an interesting point for many carbon cycle scientists.

I certainly support publication in CP but have a number of suggestions for improvement.  
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The structure and length of the paper are appropriate and the figures are for the most part clear, though I had trouble reading Figure 2 (perhaps not a problem for an online journal though). I did not see any mention of archiving the data once published, though this group has a great track record of doing that.

My comments are for the most part specific so I will list them as they arise in the manuscript.

2016, line 8. It would be helpful to say in the abstract which direction the 0.4 permil offset goes – that is, which period is heavier?

2017, line 9. It is not clear to me what “rate of damping” means.

2017, line 10-11. I agree that precision better than 0.1 per mil is needed. But, what does “significantly better” than 0.1 per mil mean? I think this is a little vague.

2018, line 25. It is not clear to me what “mean reproducibility of the respective core” means. Can you clarify?

2019, line 2. The title of the paper includes the words “high resolution” but the mean sample resolution is 600 years. Is this high resolution? There is no standard that I know of for when to use this term, but one might think it relates somehow to the ratio of the data resolution to the shortest possible variations recorded in the archive. By that standard I would not say that the data are high resolution. One might also say that its use related to how difficult the measurements are or what has been done previously. Sorry, this is a minor point, but perhaps the issue is whether more data would potentially reveal more about the system or not. The authors might want to address this, though I leave that decision to the editor.

2020, line 14-17. This sentence below does not make sense to me and does not seem to convey any information. Can you elaborate?

“The offset between the Schmitt et al. (2012) and the Lourantou et al. (2010a) data in EDC the bubble ice for this time interval was systematic and was attributed to any

method specific systematic fractionation. “

2020, line 26. One of the 13C should be a 12C.

2020, line 1-2. Delta 15N can be affected by temperature change and that, and why it is probably not important in this context, should be mentioned here.

2021, line 7. I might have just missed it but I don't think I saw a source for the Talos Dome delta 15N data.

2021, line 13. Although it is true that the uncertainty in the 15N correction does not affect the single point precision of any one delta 13C measurement it surely affects comparing the atmospheric signal of one single point to another single point unless they are from the same depth. So I am not sure why it is important to raise this issue of the precision of a single point.

2021, line 19. Why choose cutoff of 375 yr? What happens if you choose a different number?

2022, line 27. I think it would be clearer if there were a comma after “0.2.”

2023, line 5. Figure 3 is referred to here and I do not think I saw a reference to figure 2 before this. Are they out of order?

2024, line 9-12. What is the evidence for no carbonate reactions in Talos Dome? The current statement is a bit vague. Also on these lines, the statement about the gravitational correction is unclear. Does it mean that when that correction is made the difference between the cores in CO<sub>2</sub> concentration increases?

2024, line 15-21. The discussion about damping of the signal explaining part of the offset between the cores does not completely convince me. I think the mean value for both cores over the period of interest should be the same, one should just be more smoothed than another. I would like to see a model of the process if the authors believe it is the correct explanation. Otherwise I would suggest reconsidering the possibility of

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in situ production or at least giving it a little more credibility. Note that a millennial excursion is noted in the EDC record on lines 28-29, so these can be preserved.

Also in this section, the authors should refer to the CO<sub>2</sub> data from the Dome Fuji ice core from Kawamura et al. (2007, Nature, dry and wet extraction) for Termination II, both in terms of timing and absolute values.

2024, line 19-28. The change in d<sup>13</sup>C<sub>CO<sub>2</sub></sub> in interval 2 becomes important later in the paper because an analogy is drawn to the large decrease during the early part termination 1. Here though the paper is a bit vague about how much d<sup>13</sup>C of CO<sub>2</sub> really changes during that interval. A figure of about 0.2 per mil is quoted, but the data are pretty scattered. Because it becomes important later I think more attention should be given to how much the data really constrain trends during this interval.

2026, line 14. I suggest changing “we briefly report on” to something like “it is useful to consider.”

2027, lines 5-15. It would be helpful to state the isotopic composition of the carbon source in the scenarios discussed here. What about methane hydrates, could they be involved in this putative oscillation?

2027, line 19-24. It appears to me that the authors feel that the oscillation reported by Lourantou et al. are some kind of analytical artifact but the text does not quite state that clearly. The use of the terms “rather must consider” is vague. I suggest some clarification here.

2028, line 12. The word “incline” is confusing here.

2028, line 25-26. Here the decrease during interval II is described as a maximum of 0.2 per mil. Referring to my earlier comment, I am just not sure how robust this change is in the data. I am concerned that the authors are trying to tell a simple story, that the terminations I and II are similar, but it is not clear to me that the data are really good enough. As mentioned above I think this deserves some more attention.

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2029, line 25. It is not clear what “our record” refers to here. T1 data or T2?

2030, line 24-28. This paragraph needs a citation.

2033, line 14-16. As mentioned above I am concerned about how well constrained a decrease in  $\delta^{13}\text{C}$  is during the beginning of TII, which is the feature that I assume leads to the conclusion that upwelling of isotopically light water and/or decrease in iron fertilization happened at this time. At least it would be good for the authors to comment (as requested above) on how well the data really constrain the  $\delta^{13}\text{C}$  change.

2035. General comment on section 4.4 : I find the question this section addresses very interesting. I wonder if changes in the amount of carbon in the methane hydrate reservoir would have any leverage on the difference between the two glacial periods. Could this be addressed?

2035, line 7-9. Not clear what the word “favourable” refers to.

2037, line 18. I think a flux is being discussed so a time unit is needed (Gt C/yr?).

2039, line 26. “Both time intervals are about 120,000 yr apart” does not quite work – the intervals are 120,000 years apart from each other. Rewording (replace “both time” with “the”) is needed.

2040, line 8. It would be best to use a different term than “isotopic dilution process” since isotope dilution has a specific meaning in analytical geochemistry.

2040, line 16. Misspelling – “preservation” should be preservation.

Figure 1. Typo in caption (scalqe instead of scale).

Figure 3. The caption says that  $\delta^{13}\text{C}$  is plotted but only  $\text{CO}_2$  is plotted.

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