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Climate of the Past
Discussions

Interactive comment on "Antarctic temperature and CO_2: near-synchrony yet variable phasing during the last deglaciation" *by* Jai Chowdhry Beeman et al.

Anonymous Referee #2

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Overview

Chowdhry Beeman et al. investigate the time relationship between Antarctic temperature and atmospheric CO2 during the last deglaciation. The question is of importance for our understanding of climate-carbon cycle feedbacks and has been tackled in multiple previous papers, most recently Parrenin et al 2013 and Pedro et al 2012. Chowdhry-Beeman et al. is distinguished from these previous studies by its use of the high time resolution and low delta-age WDC ice core CO2 record and a new regional Antarctic temperature stack. The consensus emerging from previous studies is that there is little-to-no significant time delay between CO2 rise and Antarctic temperature rise throughout most of the deglaciation. Chowdhry-Beeman et also find synchronous

changes within uncertainties (excepting the Holocene onset), however they place more attention than others on centennial-scale signals in the temperature and CO2 series and their purported relationship. The techniques used to analyse of the phase relationship are more complex than used in previous studies. Although I have no reason to doubt the techniques, I do have some concern about their rather qualitative and selective interpretation. In particular the conclusion that there is a significant change in Antarctic temperature corresponding to the abrupt CO2 change around 16ka is not convincing. My overall impression is that the complex technique used to generate the PD histograms is out of proportion to their qualitative interpretation.

Missing is some clear hypothesis testing and a more sceptical view of whether the centennial scale signals detected in ATS and CO2 are really meaningfully related. In general the approach appears very promising but the interpretation is still requiring quite some work. I would support publication after revisions to address the concerns below.

Major Comments

Section 3.1: The technique applied to study the time relationship between ATS2 and WDC CO2 is more complex than previous studies and difficult (at least for this reviewer) to follow. My concern is that despite the complex approach the interpretation of phasing, in the end rests on a rather qualitative assessment of the change point histograms presented in Figure 3. Adding to my concern is that there is never a clear description of what makes a mode distinct and worthy of discussion or of the precise criteria for defining a significant change point. The caption of Fig 3 says that the y-axis range for the probability histograms is 0 to 0.0024. It's not clear to me precisely what is meant here. Can you show horizontal lines marking e.g. 0.05 probability cut-offs, which could then be used to judge which modes are significant? Or am I entirely missing the point? If the latter, then please work on a better explanation of how to interpret these PD histograms. Some more specific examples of my concern about this section and approach as follows:

We are told (pp7 line 18) that the degalacial CO2 rise features 'two modes' (17.63 ka and 17.30 ka) separated by a 'distinct anti-mode'. On what basis is the anti-mode distinct? Could this be over-interpretation of noisy data?

Further down (line 24) the authors describe 'a broad low probability peak' in ATS2 at 15.96 ka. It's not explained how 15.96 is selected as the centre of this peak or why this peak is considered significant, given there are similar amplitude peaks elsewhere in the deglacial CO2 record that are not discussed at all. The same can be said for the 'small upward probability peak' in CO2 at 16.15ka. This ambiguity about what is a significant feature and what is not continues throughout the section.

To give another example (line pp8 line 7), the author's describe 'two larger modes in CO2 at 11.12 and 11.01 ka.' as being 'indicative of higher-frequency variability or noise.' It's not clear on what basis these peaks are considered noise whereas the (smaller) peaks around 16 ka are considered meaningful and related to ATS2. My overall impression is that the complex technique used to generate the PDs is out of proportion to their qualitative interpretation.

I suggest the authors revise Section 3.1 to be shorter and more quantitative. I think part of the reason the some of the interpretation is unconvincing is that the authors do not appear to use their results to test any specific hypotheses. Instead we get a rather post-hoc interpretation of the leads and lags. Reframing the introduction to set up some specific hypotheses for testing could make the discussion more convincing.

Section 3.2: The methodology here appears good, however the section rests on the selection in Section 3.1 of five 'common' change points in ATS2 and CO2. As above its not clear by what criteria these 5 are selected. Please clarify.

p10 line 5. The significance of the ATS2 change point at 16k is not convincing. Please be more clear about the criteria for its selection over and above other peaks in the PDs that are not discussed at all.

P10 line 11. " during the complex, centennial-scale changes associated with the 16 ka rapid rise and the ACR onset, ATS was most likely synchronous with CO2 ". This is not convincing given the +- 340 yr uncertainty and the ambiguity of the 16ka peak in ATS2. I'd suggest a more cautious interpretation: centennial scale variability in both series (possibly physically related, possibly not) restricts ability to make any clear statement on significant leads or lags during this interval.

P10 line 22 to 29. McConnell et al suggested that accelerated warming was triggered by the Mt Takahe eruption. The finding here, that accelerated warming begins *before* the Mt. Takahe eruption, contradicts the McConnell hypothesis. The spin about "additional forcings beginning to accelerate warming before Takahe" is very unconvincing.

p11 line 1. The authors claim here to 'confirm' an imprint on Antarctic temperature of ice berg discharge to the Sth Ocn *and* Nth Atlantic around 16ka". This is not convincing at all. First, as above, the ATS2 signal around16k is questionable given other similar sized peaks in the PDs that are not discussed. Second, as the authors well know, correlation in timing does not prove of a casual relationship. Third, what is the imprint supposed to be (warming, cooling, stabilization?) and how did the icebergs drive it? Revise.

p11 line 5. The 'reversal in phasing' between T1 and the ACR end is not convincing. The phasing at T1 is 292+-343 yrs (1 sigma!), thus spanning from CO2 lag to CO2 lead. How can the phasing reverse if it is not distinct at T1? A simpler interpretation is that the ATS2 and CO2 are roughly synchronous with the exact lead-lag varying between change points due to centennial scale variability in both series.

p 11 line 9: "Centennial-scale variability may have been superimposed on coherent millenial scale trends, for example". Performing a similar analysis on band-pass filtered versions of the two series could be used to test this idea and would add a substantial new result.

p12 line 18. Comparison between east and west Antarctic temperature and CO2 could already be done by making an east Antarctic and west Antarctic stack. The authors might consider doing this in revisions, it would add a substantial new result to the lead and lag discussion.

Technical comments
p10 line 5. It should be mentioned that within uncertainties the results are consistent with Parrenin et al and Pedro et al.

Figure 4. Important typo. I think the phasing at the ACR end should read *-*250 +- 188.

p 11 line 9: "Centennial-scale variability may have been superimposed on coherent millenial scale trends, for example". Performing a similar analysis on band-pass filtered versions of the two series could be used to test this idea and would add a substantial new result.

P 11 line 11. It's very difficult to follow this sentence. Please revise.

P12 line 3. " Notably, we identify change points in ATS2 that are associated with rapid rises in CO2." Which change points exactly? The previous paragraph comments that rapid change in CO2 and ATS2 around the ACR are not clearly in common. And my concerns remain about the significance of any signal in ATS2 around the rapid 16ka signal in CO2. Without further evidence this conclusion of related abrupt changes in not convincing and not justified to include as a major conclusion here or in the abstract.

P12 line 13-15: "This variability suggests complex mechanisms of coupling. Indeed, perhaps different mechanisms of ATS2 and CO2 rises, some coupled, others decoupled, were activated and deactivated (Bauska et al., 2016) throughout the deglaciation." This statement so encompasses all possibilities that it is almost meaningless.

Please advise where the new Antarctic temperature stack will be made publicly accessible upon publication.

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