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Climate of the Past Discussions
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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 #1

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Summary:

Beeman et al. investigate the phase relationship between atmospheric CO2 concentration and Antarctic temperature during the last deglaciation. This question has been investigated various times over the last decades as it can illuminate on the role of CO2 forcing and carbon cycle changes in the deglaciation. In comparison to earlier studies, Beeman et al. use a CO2 dataset (WAIS) that is better resolved in time, and better constrained in terms of its delta age uncertainty. In addition, they include new water isotope data from the WAIS divide ice core. Furthermore, they do not just estimate a mean lag between CO2 and temperature over the entire study period (e.g., Pedro et

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al. 2012) but investigate the relative timing of change-points in both timeseries, similar to (Parrenin et al. 2013). Their results indicate that CO2 and temperature change synchronously except at the end of the Antarctic cold reversal (CO2 leads) and the onset of the Holocene (CO2 lags). The availability of the new high resolution CO2 data from WAIS and the approach to investigate change-points rather than a mean lag make this paper interesting and timely. The results will be a valuable contribution to the discussion of causes and effects of deglacial climate changes. However, I have some reservations about aspects of the manuscript and method that warrant further testing and/or justification.

General Comments:

i) The authors use a stack of five Antarctic ice core records as their temperature estimate. Their argument is that this reduces local noise. While this may be true, it also removes local differences in the temperature sensitivity to CO2 and/or sea ice changes. For recent climate change, Jones et al. (2016) showed that the local temperature trends vary strongly across Antarctica, including cooling in some regions. If we use figure 1 in Jones et al. as a potential analogue, than 4 out of the 5 cores used by Beeman et al. fall into regions where temperature hasn't warmed or is cooling since 1979. Thus, from recent observations one can expect the response of Antarctic temperature to CO2 forcing to be spatially heterogeneous. Hence, instead of stacking the isotope records, I think it is more informative to determine the phase relationship for each ice core. Because: If we want to determine cause and effect between CO2 and Antarctic climate, it is the first robust temperature rise at any ice core that is informative and not their mean. Obviously, this will increase the noise in the temperature dataset, but ideally a change-point detection method can handle noise (see also specific comments). If all ice cores lead to internally consistent estimates of temperature change-points, their likelihood estimates can be combined to reduce the errors – but this consistency would first need to be shown. And in case of inconsistencies this needs to be discussed. The stacking may also influence the detection of fast tempera-

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ture rises (such as around 16k) where already minor synchronization uncertainties and differences in resolution can lead to smoothing.

ii) The authors state throughout the manuscript that they detect abrupt temperature rises corresponding to the rapid CO2 rises discussed by Marcott et al. (2014). However, their method actually only detects the stabilization afterwards and not rises themselves (at least in temperature).

iii) In fact, this hints at another problem: The applied change-point detection method requires user input regarding how many change-points it is allowing for. The authors chose a number of 8 (and tested 7 for comparison). However, just by visual inspection of the CO2 record one can identify more change-points: i) the onset of the deglaciation, ii) the 16k rise, iii) the levelling off after 16k, iv) the rise at 14.6k, v) the levelling off after 14.3k, vi) the end of the ACR, vii) the jump into the Holocene and viii) the levelling off at the onset of the Holocene. Including the start and end point in the series which have to be marked as change points in the method, this yields 10 change-points. In fact, all of these points are discussed in the manuscript. This creates the inconsistency, that the method only allows for 8 (minus 2, i.e., start end) change points, but 10 (minus 2) are discussed – a solution that cannot be true in any of the realizations of the method: Some of these points must exclude each other if the maximum is set to 8. I think the authors should test their method allowing for more change points (e.g. 10).

iv) It is unclear, how the change-points that are actually discussed are chosen. The authors discuss the CO2 rise at 16k, which has a very low probability peak, but do not discuss the large probability peak of a CO2 increase at the onset of the ACR. How is this justified? I agree that the rise at 16k is the relevant one, but the method obviously implies a higher likelihood of a CO2 increase at 14.3k? What does this imply about the methods ability to detect the correct change points and infer their timing? The authors should discuss more clearly, how they evaluate the likelihood of a given point to be a change-point at all, before discussing its timing.

Specific Comments:

PP1,L13: “Multimodal timings” – what do you mean? Multimodal probability distributions of change point estimates? I suppose the real change cannot be multimodal.

PP2, L6: “Consistently”: Replace with “on average”, since Shakun et al. do not discuss whether this is consistent over the entire time.

PP2, L8: Also global T did not increase continuously according to Shakun et al.

PP2, L20-21: Please rephrase the colloquial “thanks to the so-called isotopic paleothermometer”. Possibly: “... due to temperature dependent fractionation of water isotopes during condensation”?

PP2, L24-25: Please replace the CO2 lock in depth with the corresponding estimate from WAIS and a reference to (Buizert et al. 2015). Generally it is not clear throughout the manuscript, whether EDC CO2 is used at all, and if so, how it is spliced together with WAIS CO2. If it is not used, please shorten the methodological discussion of EDC CO2 as it is misleading the reader to believe EDC CO2 data was used too.

PP3, L10: d15N of N2 as a proxy for DZ height: Refer to (Buizert and Severinghaus 2016) who propose some uncertainty to this assumption?

PP4, L14: “stack”: Please describe how this is generated, so that other people can reproduce this. Is this an average over all records? Are they resampled to equal resolution beforehand? Are they standardized or kept in degC? If kept in degC, which slope (per mille/degC) is used? Do all cores have similar amplitudes across the deglaciation (in degC or per mille) or can the stack be biased by single records with exceptional amplitude? Later on, the fitting procedure requires an error in K for each data-point: How is this derived? Does it include uncertainty from the isotope temperature conversion and other (e.g., circulation) influences on isotopes?

PP4, L17: “previously published ties”: The list in (Parrenin et al. 2013) includes a lot of isotopic tie-points between the ice cores as well. Are these used here too? They introduce some circularity in the approach as they will reinforce the structure and timing of isotope (temperature) changes in the ice core that is used as a target. Please clearly state, whether all tie-points are volcanic or not.

PP4, L17-18: Synchronization: Please provide a list of tie-points as well as the ECM data for both cores in the supplementary, so that people can reproduce the analysis.

PP4, L23: “1,030 measurements”: Earlier (PP3, L14) it is stated that there are only 320 points? Are these replicates? If so, please state this as it is slightly confusing.

PP4, L27-28: “At EDC. . .”: Again – is this used at all? If not, remove. If yes, elaborate how CO_2 records of such different resolution are stacked.

PP5, Figure2 caption: “Ratio of the age difference between two consecutive tie points..” . . . and what? Not clear what is plotted here.

Section 2.3 – 2.5: I encourage the authors to have a look at these sections again, and try to rewrite them more clearly. As it is now, it is near impossible to really understand what’s going on. The authors elaborate on how the MH sampler is working. This may be nice for saving computing time, but hopefully doesn’t affect the results. A reference could be enough? At the same time the authors do not discuss more relevant aspects of the method: How does the method deal with irregular sampling resolution in the records. They just say it becomes less precise (PP6, L3), but is that really so, or can it become biased? Similarly, it is not discussed, how the uncertainty for CO_2 or ATS is derived. Are the residuals/uncertainties treated as independent or correlated? Since the method only detects linear trends, any other internal climate variability would basically be a correlated uncertainty (i.e., red noise, as opposed to white measurement noise)?

PP7, L13: “. . .and corresponding rise in temperature around 16ka”: Is that true? Looking at figure 3, there is no detection of a ATS increase around 16ka. Only the stabilization. In principle I agree, that there appears to be an ATS increase. However, there is a similar increase around 17kaBP in ATS without a corresponding change in CO2, implying that this may just be internal variability/noise. And in any case: The method does not detect either of these increases in ATS.

PP7, L17: see previous comment. The ATS rise at 16k is not actually detected by the method.

PP7, L18-21: Multimodality: This paragraph is written as if the CO2/ATS increase was multimodal. However, each realization of the method probably only picks one or the other mode as a possible change point, and never both. Hence, the multimodality reflects an uncertainty in the change-point identification, not the identification of two separate change-points. Is this correct? If so, please rephrase.

PP7, L22: "a small positive probability peak". The probability of this point being a change-point is very low, much less than for example the positive peak for CO2 around 14.4ka which is not discussed. How do the authors choose which peak to discuss? How trustworthy is a change-point that is apparently only used in a small number of iterations?

PP7, L24: See comment above. The ATS probability peak is very low. How reliable is the inference of a change point there (and its timing)?

PP7, L 24-25: This section illustrates my concerns about how well the method deals with noise, and how subjectively some probability peaks are discussed while others are not. Why is the positive probability peak in CO2 at 14.64ka discussed, while the bigger positive peak at 14.42 isn't? I agree that the rise at 14.64ka and the stabilization around 14.42ka are the relevant change points, but the statistical method doesn't. To me this highlights, that the method may underestimate noise in the CO2 and ATS data, and hence, depict potentially erroneous change-points, which also have a seemingly high degree of certainty (in terms of timing). Please comment.

PP8, "leads and lags": Generally, I think the results should be more explicitly compared to (Parrenin et al. 2013), who applied a largely similar method to a different CO2 (and slightly different ATS) dataset. Their estimates could be shown in figure 4 for comparison. Are the results consistent for each change point?

PP8, L2: "...it is not obvious." Change to: "our method doesn't detect it."

PP8, L7: "either the histogram peak around 12.9 or ...". I don't think that you can interpret single peaks in the histogram like this. The timing of the rise in CO2 at the end of the ACR is detected as a broad probability distribution and not just by two minor peaks in the histogram. All values within the probability distribution are the possible "true" value with a given likelihood. Even if the peaks are the most likely single values, the cumulative probability of the true value not being these peaks is higher (cf. it is very unlikely to actually draw the exact mean value of a normal distribution).

PP8, L6: "which loses data resolution.." See earlier comments: How does the method deal with this?

PP8, L6-8: Again: Can all these modes really be interpreted in other terms than uncertainty? The authors mention "later peaks, where data resolution is lower, to be likely indicative of higher frequency variability or noise". If the method cannot handle those, how good are the timing estimates for the other change-points? Please elaborate.

PP9, L1-2: "Applying the cross correlation operator...". How is it handled when the method indicates near equal probabilities of a CO2 rise and fall like around the ACR onset?

PP9, L13-14: See earlier comments on the reliability of the method and the handling of noise and resolution.

PP9, L14: "Calculating the phasing between 12-11.5..." How is this done? Are certain values excluded from the histogram for CO2? How?

PP10, L5-6: "We identify a coherent ATS2 change-point..." See general comments. I don't think this is the case.

PP 10, L11-12: “minor modes”. See earlier comments. Can these really be interpreted?

PP10, L27-29: “Mt. Takahe” I do not understand why the cumulative probability of the ATS2 change-point is relevant here. The method does not detect whether there are multiple change points there, but only a single one with a given uncertainty in timing. Mt. Takahe falls into the uncertainty range of the detected change point at that time. Correct?

PP11, L1: “Here we confirm”. See general comments. The method does not detect ATS rises coinciding with the rapid CO_2 rises.

PP12, L 3: “we identify change points”. See earlier comments.

Technical Comments:

PP2, L14: “. . . is thought to have. . .” (not “haved”)

PP2, L19: “. . . and atmospheric composition. . .” (not “atmosphere”)

PP4, L18: “Sigl et al. 2015”: Change to 2016.

PP4, L17-18: “the offset in between ice and the air trapped much later at a given depth”: Convoluted, please rephrase. Possibly: “The age difference between trapped gas and the surrounding ice matrix, delta age, . . .”

PP7, L22: “a small positive probability peak”. Probability is always positive. Please rephrase.

PP9, L4: “2 sigma”: The numbers in the following paragraph (PP9, L7-9) match the numbers in figure 4. However, the caption of figure 4 says this was 1 sigma? Please check.

PP9, L8: “At the peak of the 16ka rise”. Since you do not detect the 16k rise in ATS a better formulation could be: “CO_2 and ATS stop rising synchronously at the onset of the ACR” or similar.

PP10, L4: replace “in AMOC” with “from AMOC”

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