Dear editor, dear reviewers,

We have made substantial revisions to our manuscript, *Antarctic temperature and CO_2: near-synchrony yet variable phasing during the last deglaciation* following the two reviews and subsequent editorial comments.

A major concern shared by the two reviewers and the editor was the treatement of uncertainty in the methodology. One reviewer expressed concern that our method might overfit correlated noise in the data series. We have therefore revised our treatment of the inverse covariance matrix that weights residuals between the two series and the piecewise linear fits used to estimate change points. This covariance matrix is designed to account for autocorrelation between residuals, but we found several weaknesses in our previous estimate, which used interpolation to calculate an empirical covariance estimate on regular intervals. Our CO_2 series is irregularly sampled in time, and as such interpolation adds spurious information to the covariance estimate. Additionally, the empirical covariance estimate is not robust to outliers. We thus implement an AR(1) model designed for autocorrelated noise in unevenly spaced time series (Mudelsee, 2002) combined with a robust fitting approach (Chang and Politis, 2016). A test of this robust procedure shows it to appropriately model the noise in the CO_2 series.

The resulting change point timing histograms are located similarly to the previous estimates, but contain considerably less noise and are often broader. The new procedure is also applied to the fits of individual isotopic temperature records from the five cores used in the study, as requested by the reviewers. These histograms contain significantly less noise as well.

We consider this methodological correction to be rather important, and have thus replaced the old results in the manuscript with the new results. This constitutes a rather major change to the manuscript. As such, we reassess our point-by-point responses to the editor's and reviewers' major comments below, which we think are better answered using the new method (all minor comments have still been accepted).

Below these point-by-point responses is a marked-up version of the manuscript. In order to maintain readability, we do not mark-up minor changes, which have all been accepted unless the relevant text has been deleted entirely, but show all major changes (in red) and all changes to the text corresponding to the methodological change (in italics).

All the best, on behalf of all co-authors,

Jai Chowdhry Beeman

Point-by-point responses

Editorial comments

The wording stabilization is misleading, essentially you mean a significant kink in the record, i.e. a change in the slope. Please change wording here and throughout, where you used stabilization.

The wording "stabilization" is removed when referring to a change in slope, and replaced with change in slope or change point.

[With respect to Dome Fuji fits] This example clearly shows the limits of your methods and may be related to the strong autocorrelation of the record in this time interval? Please choose a wording that clearly expresses these limitations.

As mentioned above, autocorrelation in the records is treated more appropriately by our new formulation of the covariance matrix. In the individual case of the Dome Fuji record, the Deglaciation mode is now centered on 18ka, in accordance with the other records, in spite of the highly correlated data variation just afterward. The very high correlation here appears to still not be completely accounted for, and is fitted by a narrow peak in probability, but large sub-millenial scale variations like these should indeed not be entirely smoothed when using our AR(1) modeled covariance matrix, which is designed to rather be robust to them. Since the Dome Fuji result is not significantly different from the others at T1 using the new fits, this discussion has been eliminated from the text.

I wonder in how far the noise level of the individual records affects the results. It is clear that due to the different accumulation rates and the different sampling strategies, the noise levels are different in all temperature records. You may want to downsample the higher records to the DF resolution to allow for a fair comparison.

Since a unique AR(1) model is used to make the covariance matrix for each record, varying noise levels should not greatly affect the results. Under the previous formulation, we could expect the error due to interpolation during the covariance matrix calculation to be larger for low-noise, low-resolution records like Dome Fuji, Talos Dome and EDC, since the true amount of information at the lowest time step would be quite low. These records indeed appear to have the noisiest histograms in the older formulation, while the smoothness of the histograms in the newer formulation is relatively similar.

A limitation remaining in the new formulation is the treatment of very highly correlated residuals, treated as outliers by the robust AR(1) model. In the case of the CO_2 series, these are the centennial-scale jumps at the ACR and Holocene onset, such a jump is also visible in the DF temperature record at the T1 onset. However, using a non-robust fitting of the AR(1) model would improperly account for the remaining correlations in the series, and thus bias the positions of the millenial scale change points.

[The significance test]...is not clear...Please explain more thouroughly. ...explain in more detail why the longer phase appears to be more correct in your opinion.

Testing the significance of probabilities calculated in an ostensibly bayesian framework is not a standardly applied procedure. We can choose two approaches: comparing the probabilities with the probabilities resulting from a null hypothesis, or comparing the probabilities among themselves.

For the first approach, we choose the null hypothesis that any point chosen randomly from a uniform distribution is equally probable to be a change point. To correctly treat this hypothesis, we calculate the directionality (concave up/down) of slope changes at the change points for 1000 fits distributed uniformly in time, with y-values chosen by interpolating between data points in the series. None of the normalized histogram bins made this way for either series surpasses 0.0002.

For the second approach, we search for a probability value below which lie approximately 95 % of histogram bins for our fits. 94.9 % of the bins for our CO_2/ATS2 fits fall below 0.0003. Since this value is higher than the value calculated using the first approach, we use it to define our probability threshold.

Note that since the histograms are normalized, and in order to treat the series consistently, we do not calulate different thresholds for the CO_2 or ATS2 fits, or according to directionality, but rather assess all histograms cumulatively.

We include the above note in the supplement; in the main text, it is treated as follows:

We implement a probability threshold to select significant change points. 94.9% of the histogram bins have (normalized) values below 0.0003—the value we select for the threshold. This threshold does not, on the other hand, evaluate significance in the sense of comparison with a null hypothesis. A simple null hypothesis would be that the series is equally well-represented by segments placed anywhere on the interval, in time, with y-axis values approximately corresponding to the data. We randomly generate 1000 points along the time intervals for both series, and calculate y-axis values for each point by linearly interpolating between data points at the respective x-value. We can thus create upward-facing and downward-facing histograms that reflect the approximate slopes of the series at any given time, but that effectively consider any change point timing to be appropriate. The bin values of the resulting normalized histograms do not surpass 0.0002. We choose the higher of these two estimates of significance.

[With respect to multimodality at the Holocene onset ...] ...does not meet the reveiwer concern. Please elaborate more carefully in the revised text.

The multimodality of this change point is reduced by the new covariance matrix formulation, but is still present. We treat it in the text as follows : A second, broad mode, representing further methodological uncertainty about the timing of the change of the long-term, millenial scale trend. Our method cannot specify which mode better represents the change, and both must be considered.

The figure with fits to the Savitsky-Golay filtered series is now included, as requested, in the main text.

Reviewer 1 Comments

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 C1 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 temperature rises (such as around 16k) where already minor synchronization uncertainties and differences in resolution can lead to smoothing.

We have calculated timings for each ice core for the revised manuscript (See figure 5). We greatly appreciate this suggestion, which we think significantly improves our study.

The timings for the temperature records are generally consistent. However, there are several differences between the temperature records, as discussed in the text in section 3.2.

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).

This comment is correct, and any phrasing which implies that the beginnings of rapid rises in temperature are detected has been removed.

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).

We may not have been clear enough about a subtle point of our method, which we think likely makes it less sensitive to the number of change points. When we analyze a time series with n points, these points may be proposed anywhere in the time interval of the series, as long as the x-values increase monotonically. Then, all the accepted points are considered in the calculation of one probability distribution (rather than one distribution per point). Because a fit need not be perfect to be accepted in the Markov Chain Monte Carlo simulation, we may estimate more peaks of high probability than the number of points used in the linear representation. Thus, 10 peaks of probability for an 8-point simulation is not inconsistent. This is emphasized in the revised manuscript in section 2.4, line 600.

We have performed a test using 10 change points, included in the supplementary materials for revised manuscript. This test shows convergence to approximately the same distribution as the 8-point test.

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.

We implement a probability threshold to justify our choice of change points, which is discussed in detail in section 2.4, lines 606-614.

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.

Omitted from the abstract.

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

Accepted.

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

This line has been changed to read: “But Antarctic temperature and CO_2 concentrations changed much more coherently as T1 progressed.”

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”?

This line has been rephrased to “...due to the temperature dependent fractionation of water isotopes...”

PP2, L24-25: Please replace the CO_2 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 CO_2 is used at all, and if so, how it is spliced together with WAIS CO_2. If it is not used, please shorten the methodological discussion of EDC CO_2 as it is misleading the reader to believe EDC CO_2 data was used too.

Replaced with: “However, the age of the air bubbles is younger than the age of the surrounding ice, since air is locked in at the base of the firn (on the order of 70 m below the surface on the West Antarctic Ice Sheet (WAIS) Divide) at the Lock-In Depth (LID) (Buizert and Severinghaus, 2016).”

EDC CO_2 data were not used at all. The later line on P4:

“At EDC, Delta-depth, the depth shift between synchronous air and ice levels, is calculated using an estimate of the LID based on nitrogen-15 data (Parrenin et al., 2013) that assumes negligible convective zone height.”

has been omitted.

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

Buizert and Severinghaus (2016), do not propose a quantitative uncertainty, we propose to include the following line. “However, the assumption that d15N reflects DZ height is imperfect, as it may underestimate the DZ height for sites with strong barometric pumping and layering (Buizert and Severinghaus, 2016), generally those closer to the coast.”

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?

We now include: “The individual isotopic records are converted to temperature (C) and are corrected for source isotopic variations (Bintanja et al., 2013), resampled to a timestep of 20 years, and averaged. The standard deviation of the records at each timestep is assumed to be representative of the uncertainty concerning the conversion from isotopes to temperature, and of the uncertainty rooted in the geographic distribution of the stack.” The spreadsheet used to calculate the stack will be made publicly available.

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.

We only use the volcanic tie points from Parrenin et al. (2013) + new EDC-DF and EDC-WD volcanic tie points. The tie-points themselves will be made available.

The line now reads

“We use previously published volcanic ties...”

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.

We will make these available in the supplementary materials and the appropriate paleoclimate databases.

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.

Rephrased to "1,030 measurements at 320 depths..."

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.

EDC was not used at all, this has been removed.

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

Will rephrase to "Ratio of the age differences between two consecutive pairs of tie points..."

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?

Only section 2.4 treats the MH sampler. Lines 15-23 are moved to the supplement.

The other two sections are important for reproducibility. Section 2.3 indicates how the goodness of individual fits to the time series is assessed, and section 2.5 details the formal calculation of leads and lags.

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)?

The residuals are corrected using the inverse of an autocorrelation matrix, the treatment of which has been considerably modified. This is now discussed in detail in section 2.3.1

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 CO_2, implying that this may just be internal variability/noise. And in any case: The method does not detect either of these increases in ATS.

Eliminated.

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

Eliminated.

PP7, L18-21: Multimodality: This paragraph is written as if the CO_2/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.

Eliminated (due to new histograms)

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 CO_2 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?

Eliminated

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 CO_2 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 CO_2 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. *A considerable amount of noise is removed from these probability spikes by the new treatment of the covariance matrix. Only two (significant) spikes remain to represent the rapid rise, and these are much lower in probability.*

However, we must still make the choice of whether to include the 'tail' of the ACR onset in CO_2 in the calculation of the uncertainty. It arguably represents uncertainty about whether the change is best represented by a short (centennial-scale) line or a longer variation, and therefore should be included.

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 CO_2 (and slightly different ATS) dataset. Their estimates could be shown in figure 4 for comparison. Are the results consistent for each change point?

A new version of figure 4 is included (now figure 6

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

Accepted.

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 CO_2 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).

Removed as a result of new fits

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.

This comment is still relevant in spite of the new fits. The multiple modes should be interpreted as methodological uncertainty.

PP9, L1-2: "Applying the cross correlation operator. . .". How is it handled when the method indicates near equal probabilities of a CO_2 rise and fall like around the ACR onset?

The cross-correlation operator is applied only in the coherent direction.

The equally large, bi-directional spikes at the ACR onset are not present in the new fits.

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 CO_2? How?

We no longer separate probabilities at the Holocene Onset, but consider the two modes together.

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

Accepted. The probability in ATS2 no longer appears.

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

Changed to:

[Changes in CO_2] are overlayed with centennial-scale substructures.

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?

The cumulative probability is important when assessing whether one event occurred before or after another. In the new fits, the Takahe eruption occurs more clearly after the temperature rise.

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

Changed to: Here, we confirm that the ends of two of these events correspond with stabilizations in the Antarctic temperature record.

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

Accepted. (Deleted)

Technical Comments: PP2, L14: ". . . is thought to have. . ." (not "haved")

Accepted.

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

Accepted.

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

Accepted.

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, . . ."

Accepted.

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

We now refer to downward-oriented and upward-oriented changes.

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.

Accepted (reworded since there is no ACR change probability at 16k in the new fits)

PP10, L4: replace "in AMOC" with "from AMOC"

Accepted

Reviewer 2 Comments

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.

We address this criticism as proposed by the reviewer, below.

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.

Probabilities are normalized, and thus the integrals of the histograms over the entire study period should sum to one.

Can you show horizontal lines marking e.g. 0.05 probability cut-offs, which could then be used to judge which modes are significant?//

For each of the histograms (concave-up and concave-down change points for ATS2 and CO2), 94.9% of the bins have (normalized) values below 0.0003—the value we select for the threshold. This is discussed further in the response to the editor and in Section 2.4, lines 10-20//

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?

The description of the antimode is removed from the text.

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.

Because of the probability threshold and the new covariance matrix, this point is no longer included

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.

With the new covariance matrix, the Holocene onset only has two significant modes in the CO_2 *series. We accept the reviewer's interpretation, and treat these modes equally.*

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.

We accept this suggestion for the revised manuscript. The main working hypothesis was that CO2 and ATS2 are synchronous and coherent. An important hypothesis that is now tested is included in the comments of reviewer 1, with respect to regional differences in temperature series.

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.

The (now four) change points are now using a probability threshold, as mentioned above.

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.

We are not convinced by this point either-it does not meet the probability threshold, particularly with the new covariance matrix. It is no longer discussed, we state rather that our method does not show coupling at 16 ka.

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.

We agree with this interpretation.

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.

We accept this clearer rephrasing of our findings. With the covariance-adjusted fits, the probability that warming began after the eruption is even lower 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.

This paragraph is removed.

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.

We accept this interpretation, though the T1 phasing is now more marked. This now reads
"Though the T1 onset and the ACR end are both thought to originate in AMOC reductions (Marcott et al., 2014), our results allow for the directionality of CO_2-ATS2 phasing to be reversed during the two events. CH_4 changes nearly synchronously with CO_2 at both points, but the phasings are opposite in direction and different in magnitude."

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.

We include a figure of Savitsky-Golay filtered data, and the corresponding fits (Figure 4), which is discussed in detail in section 3.1, lines 10-17.

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.

As suggessted by Reviewer 1, we have applied our method to the isotopic records from each ice core. These results are shown in Figure 5, and discussed extensively in section 3.2

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

With the new fits, this is not always the case. This is rephrased to "Within the range of uncertainty, our lead-lag estimates are only roughly consistent with those of Pedro et al. (2012) and Parrenin et al. (2013)"

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

Accepted, changed in the new version of this figure as well.

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

Reworded to: Bauska et al. (2016), for example, hypothesize that an earlier rise 10 in CO2 at 12.9 ka, driven by land carbon loss or SH westerly winds, might have been superimposed on the millenial-scale trend.

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.

Accepted.

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

The second sentence is omitted.

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

The stack is already available on the linked github page (https://github.com/Jai-Chowdhry/LinearFit-2.0-beta/tree/v0.0) as ATS2-new-sigma2.txt. It will be made available on Pangaea/NOAA Paleoclimate upon publication.

Antarctic temperature and CO_2: near-synchrony yet variable phasing during the last deglaciation

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Abstract. The last deglaciation, which occurred from 18,000 to 11,000 years ago, is the most recent large natural climatic variation of global extent. With accurately dated paleoclimate records, we can investigate the timings of related variables in the climate system during this major transition. Here, we use an accurate relative chronology to compare regional temperature proxy data and global atmospheric CO_2 as recorded in Antarctic ice cores. We build a stack of temperature variations by averaging the records from five ice cores distributed across Antarctica, and develop a volcanic synchronization to compare it with the high-resolution, robustly dated WAIS Divide CO_2 record. We assess the CO_2 / Antarctic temperature phase relationship using a stochastic method to accurately identify the probable timings of abrupt changes in their trends. *Four coherent changes are identified for the two series, and synchrony is within the 2σ uncertainty range for all of the changes except the Holocene onset. During the large, millenial-scale changes at the onset of the last deglaciation at 18 ka and the onset of the Holocene at 11.5 ka, Antarctic temperature most likely led CO_2 by several centuries. CO_2 and Antarctic temperature peaked nearly synchronously at 14.4 ka, the onset of the Antarctic Cold Reversal (ACR) period. And CO_2 likely led Antarctic temperature by around 250 years at the end of the ACR. However, two significant changes, one at 16 ka in the CO_2 record and one after the ACR onset in the temperature record, do not have significant counterparts in the other record. The timings of changes in trends for the individual proxy records show variations from the stack, indicating some regional differences in the pattern of temperature change, particularly between West and East Antarctica. The likely-variable phasings we identify testify to the complex nature of the mechanisms driving the carbon cycle and Antarctic temperature during the deglaciation.*

1 Introduction

Glacial-interglacial transitions, or deglaciations, mark the paleorecord approximately every 100,000 years over the past million years or so (Jouzel et al., 2007; Lisiecki and Raymo, 2005; Williams et al., 1997). The last deglaciation, often referred to as glacial termination 1 (T1), offers a case study for a large global climatic change, very likely in the 3-8°C range on the global scale (Masson-Delmotte et al., 2013), and thought to be initiated by an orbitally driven insolation forcing (Berger, 1978; Hays et al., 1976; Kawamura et al., 2007). The canonical interpretation of this apparent puzzle is that insolation acts as a pacemaker of climatic cycles and the amplitude of glacial-interglacial transitions is mainly driven by two strong climatic feedbacks: atmospheric CO_2 and continental ice surface-albedo changes. However, the mechanisms that control the CO_2 rise are still a matter of debate. Accordingly, reconstructing the phase relationship (leads and lags) between climate variables and CO_2 during the last termination has become of importance, and has a substantial history in ice core research (Barnola et al., 1991; Caillon et al., 2003; Parrenin et al., 2013; Pedro et al., 2012; Raynaud and Siegenthaler, 1993).

Global temperature has been shown to lag CO_2 on average during T1 (Shakun et al., 2012), supporting the importance of CO_2 as an amplifier of orbitally-driven global-scale warming. But Antarctic temperature and CO_2 concentrations changed much more coherently as T1 progressed. Indeed, near the end of the glacial-interglacial transition, Antarctic warming slowed and even reversed during a period of about 2000 years, coinciding with a warm period in the North called the Bølling–Allerød (B/A). This period of cooling in Antarctica is called the Antarctic Cold Reversal (ACR). A period of cooling in the Northern Hemisphere known as the Younger Dryas (YD), followed the B/A, coinciding with a period of warming in the SH.

High-latitude Southern Hemisphere paleotemperature series–including Southern Ocean temperature–varied similarly to Antarctic temperature during T1 (Shakun et al., 2012; Pedro et al., 2016). Upwelling from the Southern Ocean is thought to have played an important role in the deglacial CO_2 increases. *The Atlantic Meridional Overturning Current, or AMOC, a major conduit of heat between the Northern and Southern Hemispheres and component of the bipolar seesaw, the umbrella term encompassing the mechanisms thought to control the seemingly alternating variations of Northern and Southern Hemisphere temperature, is thought to have influenced Southern Ocean upwelling during the deglaciation (Marcott et al., 2014)*. A weakening of the oceanic biological carbon pump appears to have dominated the deglacial CO_2 increase until 15.5 ka, when rising ocean temperature likely began to play a role as well (Bauska et al., 2016).

Ice sheets are exceptional archives of past climates and atmospheric composition. Local temperature is recorded in the isotopic composition of snow/ice (Jouzel et al., 2007; NorthGRIP Project Members, 2004) due to the temperature dependent fractionation of water isotopes (Lorius and Merlivat, 1975; Johnsen et al., 1989). The concentration of continental dust in ice sheets is a proxy of continental aridity, atmospheric transport intensity and precipitation (Petit and Delmonte, 2009; Lambert et al., 2012). Finally, air bubbles enclosed in ice sheets are near-direct samples of the past atmosphere. However, the age of the air bubbles is younger than the age of the surrounding ice, since air is locked in at the base of the firn (on the order of 70 m below the surface on the West Antarctic Ice Sheet (WAIS) Divide) at the Lock-In Depth (LID) (Buizert and Severinghaus, 2016). The firn, from top to bottom, is composed of a convective zone (CZ) where the air is mixed vigorously, and a diffusive zone (DZ) where molecular diffusion dominates transport. Firn densification models can be used to estimate the LID and the corresponding age difference (Sowers et al., 1992).

Atmospheric CO_2 concentrations, recorded in the air bubbles enclosed in ice sheets, are better preserved in Antarctic ice than in Greenland ice, because the latter has much higher concentrations of organic material and carbonate dust (Raynaud et al., 1993). Measured essentially on the Vostok and EPICA Dome C ice cores, the long ice core record of CO_2 (Lüthi et al., 2008) covers the last 800 ka. This record is of global significance.

Early studies suggested that at the initiation of the termination around 18 ka B1950 (kiloyears before 1950 A.D.), just after the Last Glacial Maximum (LGM), Antarctic temperature started to warm 800 ± 600 yr before CO_2 began to increase (Monnin

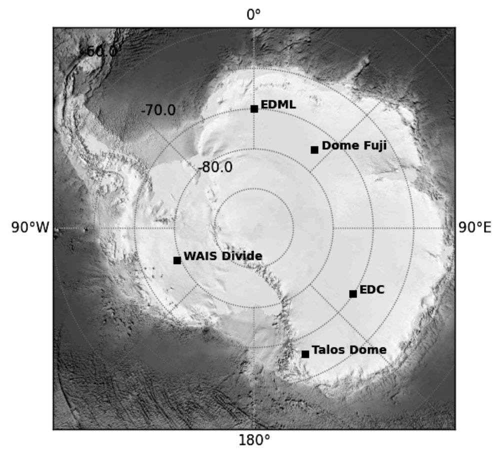

Figure 1. Drilling locations of the ice cores from which the CO_2 and isotopic paleotemperature records included in this study were measured.

et al., 2001), a result that was sometimes misinterpreted to mean that CO_2 was not an important amplification factor of the deglacial temperature increase. This study used measurements from the EPICA Dome C (EDC) ice core (Jouzel et al., 2007) and a firn densification model to determine the air chronology. However, this firn densification model was later shown to be in error by several centuries for low accumulation sites such as EDC during glacial periods (Loulergue et al., 2007; Parrenin et al., 2012).

Two more recent works (Pedro et al., 2012; Parrenin et al., 2013), used stacked temperature records and improved estimates of the age difference between ice and air records to more accurately estimate the relative timing of changes in Antarctic temperature and atmospheric CO_2 concentration. In the first of these studies, measurements from the higher accumulation ice cores at Siple Dome and Law Dome, used to decrease the uncertainty in the ice-air age shift, indicated that CO_2 lagged Antarctic temperature by 0-400 yr on average during the last deglaciation (Pedro et al., 2012). The second study (Parrenin et al., 2013) used measurements from the low accumulation EDC ice core but circumvented the use of firn densification models by using the nitrogen isotope ratio δ^{15}N of N_2 as a proxy of the DZ height, hypothesizing that the height of the CZ was negligible during the study period. CO_2 and Antarctic temperature were found to be roughly in phase at the beginning of TI and at the end of the ACR period, but CO_2 was found to lag Antarctic temperature by several centuries at the beginning of the Antarctic Cold Reversal and Holocene periods. However, the assumption that d15N reflects DZ height is imperfect, as it may underestimate the DZ height for sites with strong barometric pumping and layering (Buizert and Severinghaus, 2016).

A new CO_2 record of unprecedented high resolution (Marcott et al., 2014) from the WAIS Divide (WD) ice core merits the reopening of this investigation. The air chronology of WAIS Divide is well constrained thanks to a relatively high accumulation rate and to accurate nitrogen-15 measurements (Buizert et al., 2015). The WAIS record evidences centennial-scale changes in the global carbon cycle during the last deglaciation superimposed on more gradual, millenial-scale trends that bear resemblance to Antarctic temperature (Marcott et al., 2014).

The deglacial temperature rise seen at WD is structurally similar to that at other Antarctic sites. However, West Antarctic warming may have been greater in magnitude than East Antarctic warming by up to 3 degrees, and the rise in West Antarctic temperature shows early warming starting around 21 ka B1950, following local insolation (Cuffey et al., 2016). This early warming trend is much more gradual in records from East Antarctic ice cores. The difference between the two records may be related to sea ice conditions around East and West Antarctica, and perhaps to elevation changes (Cuffey et al., 2016; WAIS Divide Project Members and others, 2013). However, the temperature record at WAIS Divide shows an acceleration in warming around 18 ka B1950 which is also present in East Antarctic records (WAIS Divide Project Members and others, 2013).

On the much shorter timescales of the observable past, Jones et al. (2016) note differing temperature trends at the drilling sites of the five cores used in this study. On the other hand, the interpretation of individual isotopic records can prove complicated, as local effects, including those of ice sheet elevation change and sea ice extent, are difficult to correct.

In the present work we refine our knowledge of leads and lags between Antarctic temperature and CO_2. We develop a new stack of accurately synchronized Antarctic temperature records to reduce local signals, placed using volcanic matching on the WAIS Divide chronology (WD2014). We then compare the temperature stack to the high resolution WAIS Divide CO_2 record by determining the probable timings of changes in trend, and calculate probable change point timings for the five individual isotope-derived records used in our stack as well.

2 Methods and data

2.1 Temperature stack and ice chronology

We develop a stack of isotopic temperature records (Antarctic Temperature Stack 2, or ATS2) in order to remove local influences and noise in the individual records to the greatest extent possible. Our stack contains five records: EDC, Dome Fuji (DF), Talos Dome (TALDICE), EPICA Dronning Maud Land (EDML) and WAIS Divide (WD). We use previously published volcanic ties between EDC, DF, TALDICE and EDML (Parrenin et al., 2013; Fujita et al., 2015). We then develop a volcanic synchronization between the EDC and WD cores (Figure 2) to place our stack on the WD2014 chronology (Buizert et al., 2015; Sigl et al., 2016). The Vostok record, included in the stack used by Parrenin et al. (2013) is excluded: it contains additional chronological uncertainty as it is derived using records from two drilling sites.

The individual isotopic records are converted to temperature (deg *C) corrected for source isotopic variations (Bintanja et al., 2013), resampled to a timestep of 20 years, and averaged. The standard deviation of the records at each timestep is assumed to be representative of the uncertainty concerning the conversion from isotopes to temperature, and of the uncertainty rooted in the geographic distribution of the stack.*

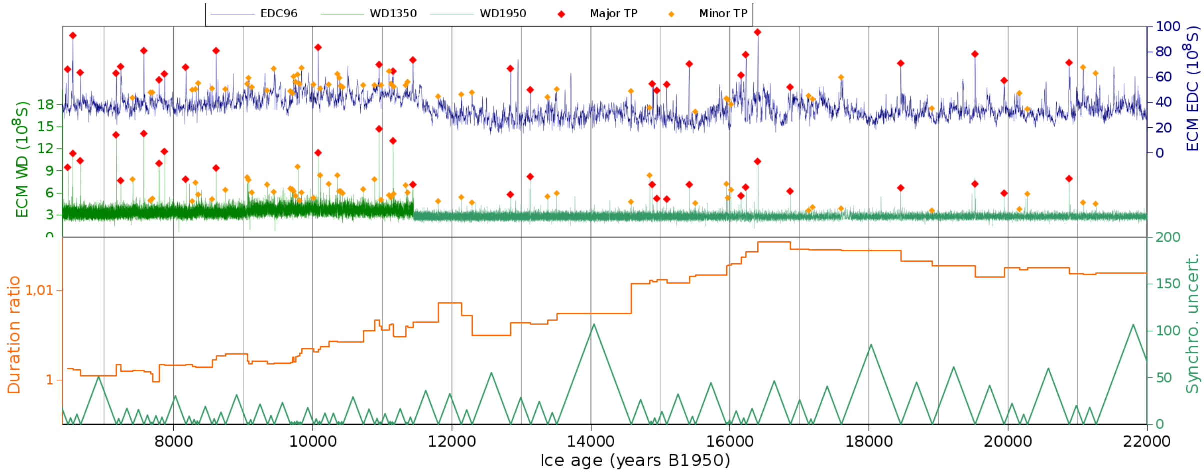

Figure 2. Volcanic synchronisation between the EDC and WD ice cores. (Top) ECM records from EDC (blue) and WD (raw data: 6.4-11.4 ka; adjusted data: 11.4-24 ka). Red diamonds show a primary set of synchronization points, selected in an initial round of visual synchronization. Orange diamonds are a secondary set of synchronization points, selected in a second round of visual synchronization. (Bottom) The ratio of the age difference between two consecutive pairs of tie points is shown in orange. The synchronization uncertainty, which is determined as 20% of the distance to the nearest tie point, is shown in green. This uncertainty is included in the calculation of leads and lags.

2.2 CO_2 and air chronology

We use atmospheric CO_2 data from the WD ice core Marcott et al. (2014) which consist of 1,030 measurements at 320 depths that correspond to ages between 23,000 and 9,000 years B1950 with a median resolution of 25 years. At WD, the age offset between the ice and air (trapped much later) at a given depth, Δage, is calculated using a firn densification model, which is constrained using nitrogen-15 data, a proxy for firn column thickness (Buizert et al., 2015). Δage ranges from 500±100 yr at the last glacial maximum, to 200±30 yr during the Holocene. Δage uncertainty is added to cumulative layer counting uncertainty to determine the total uncertainty of the air chronology.

2.3 Identifying changes in trend

We identify likely change points by taking the residuals of the linear interpolations between the change points with respect to the raw data (similarly to Parrenin et al. (2013)). At the base of our method is a parallelized Metropolis-Hastings (MH) procedure (Goodman and Weare, 2010; Foreman-Mackey et al., 2013). Therefore, we do not present a single “best fit” but rather analyze the ensemble of fits accepted by the routine. We plot two histograms: an upward-oriented histogram for concave-up change points, and a downward-oriented histogram for concave-down change points. We use these histograms as probabilistic locators of changes in slope (Figure 3).

The change point representations of the ATS2 and CO_2 time series are composed of a set of n specified change points $\{\boldsymbol{X}_i = (x_i, y_i)|\, i = 1, \ldots, n\}$. We denote the vector of m time series observations o at time t $\{\boldsymbol{O}_l = (t_l, o_l)|\, l = 1, \ldots, m\}$, and the scalar residual term J between observations and the linear interpolation between change points f_y:

$$J(\boldsymbol{X}_i) = \boldsymbol{R}^T \mathbf{C}^{-1} \boldsymbol{R};\; \boldsymbol{r}_l = (f_y(t_l) - o_l)_l \quad (1)$$

where $\boldsymbol{R}$ is the vector of residuals at each data point with components $\boldsymbol{r}_l$ and $\mathbf{C}$ is the covariance matrix of the residuals. The ATS2 series contains 700 data points, and the WD CO_2 series contains 320, each of which is considered in the residuals.

We fix $x_0 = t_0$ and $x_n = t_l$; i.e. the x-values of the first and last change points are fixed to the first and last x-values of the observation vector, with the y-values allowed to vary. The remaining points are allowed to vary freely in both dimensions.

2.3.1 Estimating the covariance matrix C: treating uncertainty and noise

Our method fits time series with piecewise linear functions, and the residual vector thus accounts for any variability that cannot be represented by these fits. Paleoclimate time series, like the CO_2 and ATS2 series used here, typically contain autocorrelated noise (see Mudelsee (2002), for example) which cannot be accurately represented by a piecewise linear function. Weighting the residuals of a cost-function based formulation by a properly estimated inverse covariance matrix ensures that this autocorrelated noise is not overfitted, and can improve the balance of precision and accuracy of the fits.

Our time series contain two potential sources of uncertainty: measurement or observational uncertainty, related with the creation of the data series, and modeling uncertainty, related to the residuals. We formulate a separate covariance matrix to account for each source of uncertainty. These matrices are then summed to form $\boldsymbol{C}$. We assume the first source of uncertainty to be uncorrelated in time (i.e. a white noise process). Thus, the associated covariance matrix $\boldsymbol{C}_{meas}$ is diagonal, and the diagonal elements $\boldsymbol{C}_{jj}$ are each equal to the variance of observation o_j, σ_j^2, as estimated during the measurement process.

The covariance matrix of the modeling uncertainty, which we denote $\boldsymbol{C}_{mod}$, is more complicated, since the residual vector contains any autocorrelated noise in the time series that is not accounted for by the piecewise linear fits. Additionally, the time series contain outliers with respect to these linear fits, and these can impact any non-robust estimate of covariance. Finally, an initial idea of the model must be used to calculate residuals, and thus estimate their covariance. These challenges can be circumvented when data resolution is low enough to assume that residuals are uncorrelated, as in Parrenin et al. (2013), however, including the covariance matrix allows us to make use of noisy, high-resolution data.

We arrive at an initial model by running a MH simulation in which $\boldsymbol{C}$ is assumed equal to the identity matrix, and select the best fit of this run. Note that $\boldsymbol{C}_{meas}$ is not taken into account at this point, since we require an independent estimate of $\boldsymbol{C}_{mod}$. At this point, covariance could be estimated directly, but tests indicated that this method was not robust, making the covariance matrix estimate sensitive to outliers and to the initial model fit. Our CO_2 data are unevenly spaced in time, and developing a covariance matrix using the traditional covariance estimator would require some form of interpolation, which can introduce substantial error.

The residuals with respect to the initial model are instead used to fit an AR(1) model (Robinson, 1977; Mudelsee, 2002) which treats the autocorrelation between a pair of residuals r_i *and* r_{i-1} *as a function of the separation between the two data points in time* $t_i - t_{i-1}$. *The Robinson (1977) / Mudelsee (2002) model is expressed:*

$$r_i = r_{i-1} \cdot a^{t_i - t_{i-1}} \tag{2}$$

where the constant a *determines the correlation between two residuals separated by* $t_i - t_{i-1}$ *units of time, and minimizing the loss function:*

$$S(a) = \sum_{i=1}^{n} \left\{ r_i - r_{i-1} \cdot a^{t_i - t_{i-1}} \right\} \tag{3}$$

allows us to estimate a. *We do so using a nonlinear least-squares estimate with L1-norm regularization to provide a robust estimate (Chang and Politis, 2016). We confirm the validity of the AR(1) hypothesis by comparing* r_i *with* $r_{i-1} \cdot a^{t_i - t_{i-1}}$ *(Supplement). Given that the AR(1) hypothesis is accurate, we can use* a *to calculate the theoretical correlation between two residuals, and construct a correlation matrix* $\boldsymbol{K}$ *and the model covariance matrix* $\boldsymbol{C}_{mod}$ *as follows:*

$$\boldsymbol{C}_{mod} = \sigma_{mod}^2 \boldsymbol{K} \; ; \; \boldsymbol{K}_{ij} = a^{t_j - t_i} \tag{4}$$

where σ_{mod}^2 *is the variance of the modeling error, assumed constant and estimated using a robust estimator as* $(IQR(\boldsymbol{R})/1.349)^2$. *Finally, the covariance matrix of the residuals* C *is calculated as:*

$$\boldsymbol{C} = \boldsymbol{C}_{mod} + \boldsymbol{C}_{meas}. \tag{5}$$

Rather than inverting the covariance matrix, we use Cholesky and LU decompositions to solve for the cost function value J, *as in Parrenin et al. (2015).*

2.4 Estimating the posterior probability density

In general, the probability density of the change points cannot be assumed to follow any particular distribution, as short-timescale variations of the time series may lead to multiple modes or heavy tails, for example. Thus, stochastic methods, which are best adapted to exploring general probability distributions (for example, Tarantola (2005)), are suited to our problem.

To tackle the large computation time required for traditional MH sampling, we apply the ensemble sampler developed by Goodman and Weare (2010) (GW) as implemented in the python emcee library (Foreman-Mackey et al., 2013). This sampler adapts the MH algorithm so that multiple model walkers can explore the probability distribution at once, making the algorithm parallelizable. It has the advantage of being affine invariant: that is, steps are adapted to the scale of the posterior distribution in a given direction.

We make histograms of the probable timings of 8 major change points for the WD CO_2 and ATS2 series. The choice of 8 points is not entirely arbitrary: it reflects our goal of investigating millenial-scale variability (8 points allows for approximately one point per two millenia over the study period). This choice is subjective, but our method is not particularly sensitive to the number of change points. Since fits need not be perfect to be accepted in the stochastic simulation, we may estimate more or less peaks of high probability than the number of points used in the linear representation, and probability distributions of simulations with 8 and 10 points are rather similar (Supplementary Materials). The results of the 8-point simulation are shown in Figure 3. The most probable timings are identified by probability peaks, or modes. We avoid comparing incoherent modes by separating changes by the sign of the second derivative of the fits. Further details of the simulations are given in the supplement.

We implement a probability threshold to select significant change points. 94.9% of the histogram bins have (normalized) values below 0.0003—the value we select for the threshold. This threshold does not, on the other hand, evaluate significance in the sense of comparison with a null hypothesis. A simple null hypothesis could be that the series are equally well-represented by segments placed anywhere on the interval, in time, with y-axis values approximately corresponding to the data. We randomly generate 1000 points along the time intervals for both series, and calculate y-axis values for each point by linearly interpolating between data points at the respective x-value. We can thus create upward-facing and downward-facing histograms that reflect the approximate slopes of the series at any given time, but that effectively consider any change point timing to be appropriate. The bin values of the resulting normalized histograms do not surpass 0.0002. We choose the higher of these two estimates of significance, and consider peaks to be significant when the threshold is approximately met (within $\pm 2.5 \cdot 10^{-5}$) or clearly passed. The choice of this threshold over the lower threshold is ultimately arbitrary, and allowing for a buffer zone around the threshold reflects both this arbitrariness and the small uncertainty resulting from the stochastic nature of the histograms.

2.5 Phasing

We estimate ρ_{lead}^{ATS2}, the probability that ATS2 leads CO_2 over a given interval, as

$$\rho_{lead}^{ATS2} = (\rho_x^{ATS2} \circ \rho_x^{CO_2}) \star \rho^{chron}, \tag{6}$$

where ρ_x^{ATS2} is the probability of a change point at time x for ATS2, $\rho_x^{CO_2}$ is the probability of a change point at time x for CO_2, $\circ$ is the cross-correlation operator, which is used to calculate the probability of the difference between two variables, and $\star$ is the convolution operator, which is used to calculate the probability of the sum of two variables. ρ^{chron} is the probability distribution of the chronological uncertainty between the two records, which we take to be Gaussian centered on 0, with standard deviation $\sigma = \sigma_{chron}$ (shown in Figure 3). The intervals associated with each change point are given in Figure 6.

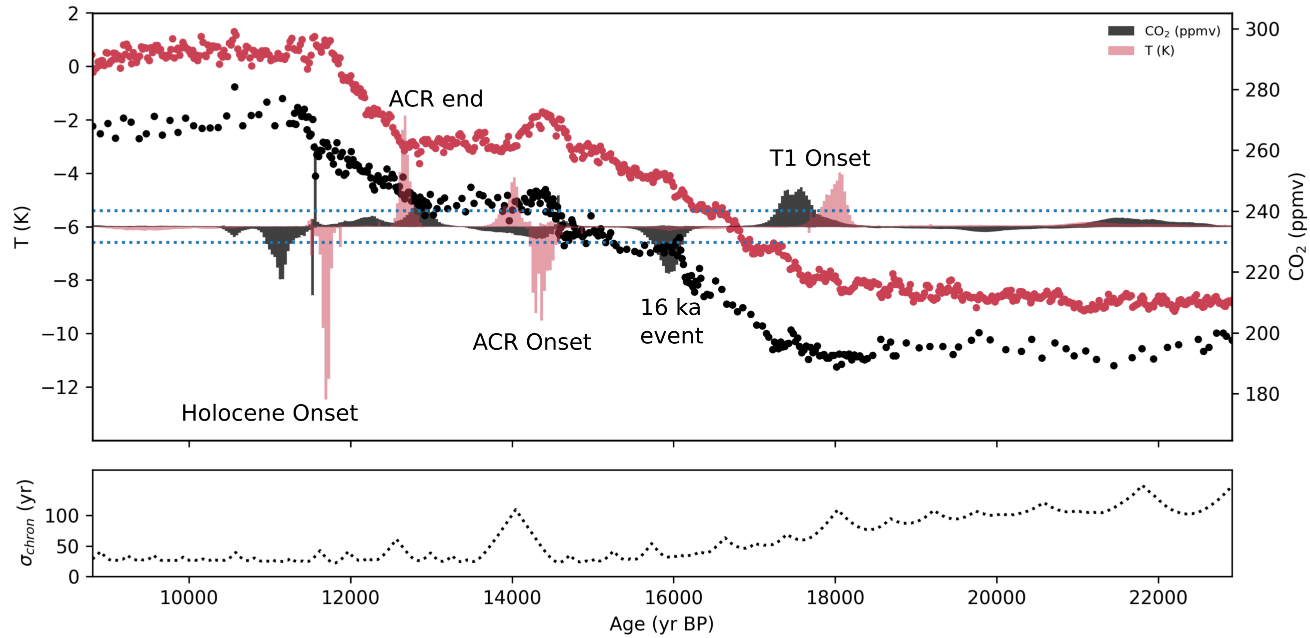

Figure 3. Upper panel: Atmospheric CO_2 (black) and ATS2 (red) placed on a common time scale, with the normalized histograms of probable change points (8 points). Histograms are plotted downward-oriented when the rate of change decreases and upward-oriented when it increases (same colors, y-axis not shown) Rrobabilities are normalized so that the integrated probability for a given histogram sums to 1, and range from 0 (center) to 0.004 (top/bottom). The 0.0003 probability threshold is marked by dotted blue lines. In four distinct time intervals, both series show concurrent probable change points. Lower panel: Chronological uncertainty, taken as the sum of the ∆age uncertainties and the uncertainty estimate for our volcanic synchronization.

3 Results and discussion

3.1 Change point timings

The change point histograms for the ATS2 and CO_2 time series in Figure 3 confirm that the millenial-scale changes in the two series were largely coherent. We identify four major changes in trend which surpass the 95% confidence interval for both series: the onset of the deglaciation from 18.2 to 17.2 ka B1950; the onset of the Antarctic Cold Reversal (ACR) at around 14.5 ka, the ACR end beween 12.9 and 12.65, and the Holocene onset, at approximately 11.5 ka. For each of these four changes, we calculate the probability of a lead or lag. Two additional change points, one for CO_2 centered at approximately 16 ka, and a second change point for the temperature series after the ACR onset, centered at 14 ka, are also significant but do not have significant counterparts in the other series. Two abrupt, centennial-scale rises in CO_2, one at the ACR onset and before the Holocene onset, are visible in the histograms as narrow peaks. These changes have been identified in the WD CO_2 record by Marcott et al. (2014), though the beginning of a third such rapid change before 16ka is not detected here.

The deglaciation onset begins with a large, postive change point mode for Antarctic temperature, centered around 18.1 ka. The corresponding change point for the CO_2 series is centered around 17.6 ka.

The CO_2 rise peaks at around 16 ka, identified by a downward-oriented probability peak, which has no significant counterpart in the temperature series. This peak is followed by a brief plateau in CO_2 concentrations, before a gradual, accelerating resumption of the increase.

An abrupt CO_2 rise preceded the Antarctic Cold Reversal. Two narrow spikes in probability, upward- and downward-facing, near 14.58 ka, appear to represent this rise, the downward peak just reaching the significance threshold. The broad tail of the downward-facing CO_2 change point peaks again at around 14.35 ka. On its own, the second mode does not reach the significance threshold, but it appears to reflect further methodological uncertainty with respect to the timing of the millenial-scale change in CO_2. An unambiguous negative temperature change also occurs at around 14.35 ka, roughly concurrent with the downward CO_2 change point. Antarctic temperature began to descend rapidly after the ACR onset, finally stabilizing at the concave-up change point identified by the mode centered on 14.02 ka. No corresponding change point is detected for CO_2.

The ACR termination is represented by significant modes in both series. An increase in CO_2 began at the peak occuring around 12.88 ka, while the ATS2 increase is centered at 12.65 ka, appproximately.

The Holocene onset is well-defined in the ATS2 series, with a large mode centered at 11.7 ka. The probability peaks in the CO_2 series are remarkably similar to those identified at the ACR onset. A rapid rise in CO_2 is represented by narrow peaks from 11.57 to 11.53 ka. A second, broad mode, representing further methodological uncertainty about the timing of the change of the long-term, millenial scale trend. Our method cannot specify which mode better represents the change, and both must be considered.

As a second test of the timings of millenial-scale events, we use our method to fit filtered versions of the ATS2 and WAIS Divide CO_2 data. A Savitsky-Golay filter, designed to have an approximate cutoff periodicity of 500 years, is applied to the two records. In the two filtered series, the sub-millenial scale AR(1) noise present in the original series should be essentially removed. As such, fitting change points to these two series, assuming the residuals to be uncorrelated, provides a second form of verification of the appropriateness of the covariance matrix we use to fit the raw data.

Figure 4 shows the Savitsky-Golay filtered CO_2 and ATS2 time series, and the corresponding change point histograms. The four major changes identified in both series, at the T1 onset, the ACR onset, the ACR end, and Holocene onset, are similar in shape and center to the fits of the raw data. However, there are two notable differences between the two fits. First, the spikes representing centennial-scale CO_2 rises before the ACR and Holocene onsets are entirely removed. This is not surprising, given that the Savitsky-Golay filters are designed to remove all variability with periodicities less than 500 years, whereas the covariance matrix applied to the fits of the raw data only treats AR(1) correlated noise. Finally, the probability of the post-ACR change in ATS2 is considerably smaller for the filtered series. Savitsky-Golay filtering has its own drawbacks–data reinterpolation is required, for example, and propagating measurement uncertainty becomes difficult. However, the similarity of the two results supports our fits of the raw data.

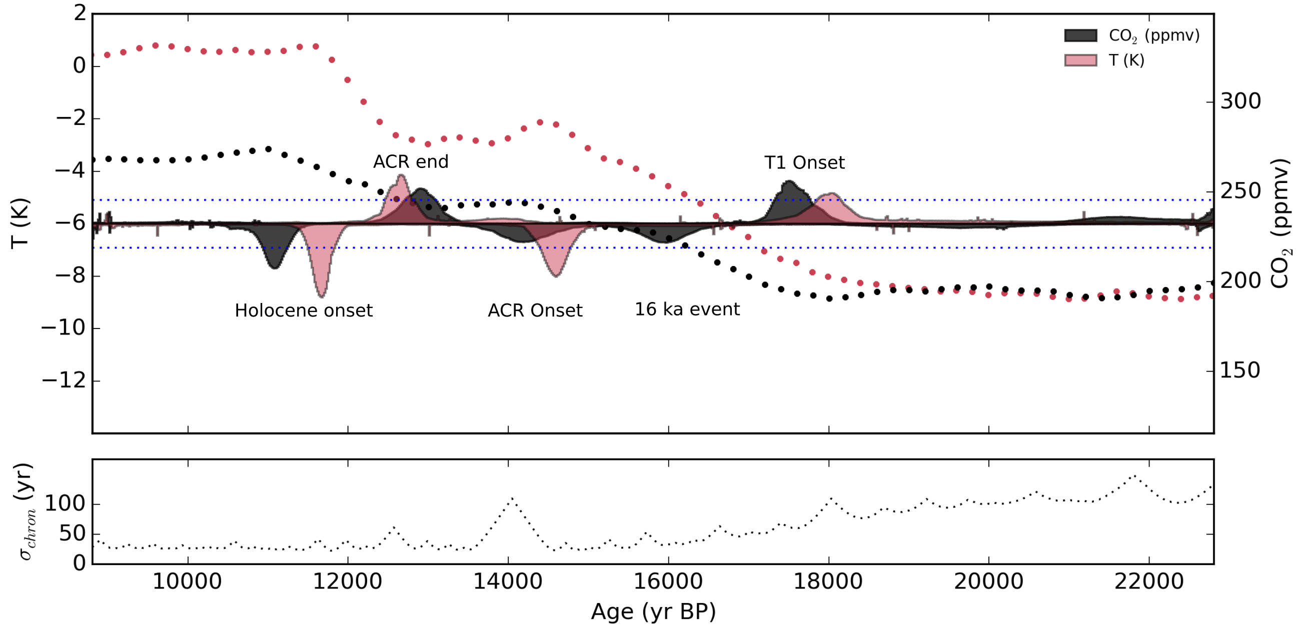

Figure 4. Upper panel: Savitsky-Golay filtered atmospheric CO_2 (black) and ATS2 (red) placed on a common time scale, with the normalized histograms of probable change points (8 points). Histograms are plotted downward-oriented when the rate of change decreases and upward-oriented when it increases (same colors, y-axis not shown, probabilities range from 0 (center) to 0.0024 (top/bottom)). The 0.0003 probability threshold is marked by dotted blue lines for reference, but is not applied here. Lower panel: Chronological uncertainty, taken as the sum of the Δage uncertainties and the uncertainty estimate for our volcanic synchronization.

3.2 Change point timings for individual temperature records

Fits of each of the regional temperature records, corrected for source isotopic variations (Bintanja et al., 2013) are shown in figure 5. These fits should still be interpreted cautiously, as additional information included in the isotopic records used to calculate temperature–the signal of ice sheet elevation change, for example–are not corrected for. The comparison of these fits provides an initial, exploratory picture of potential regional differences in climate change during the last termination.

Of the four changes identified as coherent between the temperature stack and CO_2, those at the deglaciation onset, the ACR end, and the Holocene onset are expressed as significant probability peaks in all five records. Some ambiguity appears to exist about the timing of the ACR onset in the EDML record. It is expressed by a rather broad, non-significant probability mode extending between 16ka and 14ka, though a significant spike at 14ka marks the downturn seen in the other records. The ACR onset is significant and well-defined in all of the five other records.

The WAIS Divide record is, notably, the only isotopic record in our stack from the West Antarctic ice sheet. We could thus reasonably expect it to show considerably different trends from the other records. Indeed, a significant change in the WD temperature record occurs at 22 ka. This early change in the isotopic record was identified and confirmed to indeed be a

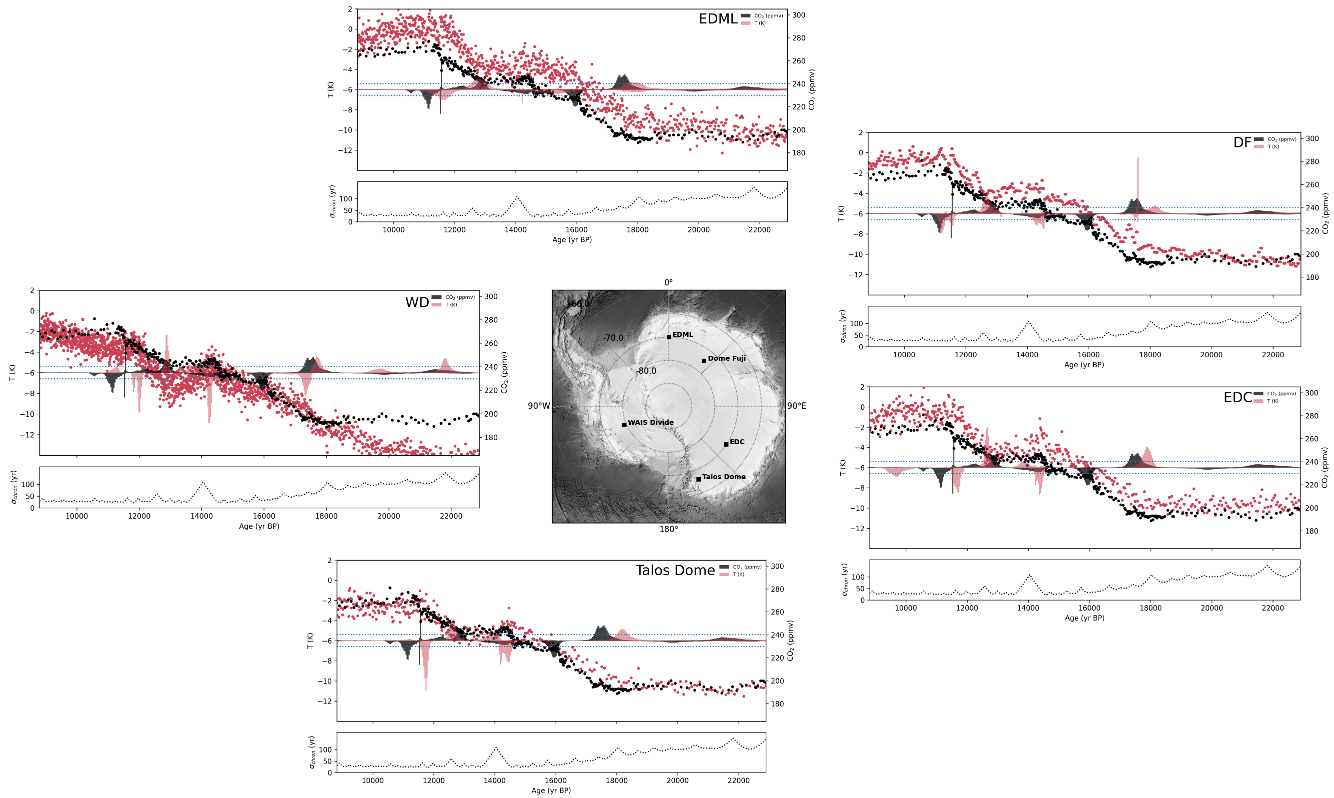

Figure 5. Atmospheric CO_2 (black) and source-corrected temperature records (red) placed on a common time scale, with the normalized histograms of probable change points (8 points) for each ice core used in the ATS2 stack; the locations of the drill sites are shown in the center. Details of the histogram plots are as in Figure 3.

temperature signal by Cuffey et al. (2016) using a borehole temperature record, though their study places the change at 21 ka. We confirm that the onset of the deglacial temperature rise in West Antarctica likely began as much as 4 ka before the onset of temperature rise in East Antarctica. Interestingly, the WD record also shows a temperature change point around 17.8 ka, expressed slightly later than in the other records and more synchronous with CO_2. This apparent acceleration of the temperature rise is followed by a significant downward-facing change point not seen in any of the other records. A difference appears to exist in timing at the Holocene onset as well, with temperature change at WD apearing to slightly precede temperature in the East Antarctic records, and the DF temperature change in particular appearing to occur more synchronously with CO_2.

3.3 Leads and lags

The probability densities of leads and lags at the coherent change points between ATS2 and CO_2 are shown in Figure 6. We then report the 1 σ standard deviation of the lead/lag, but this estimate must be applied with care where the lead probability is still multimodal, as is the case at the Holocene and ACR onsets.

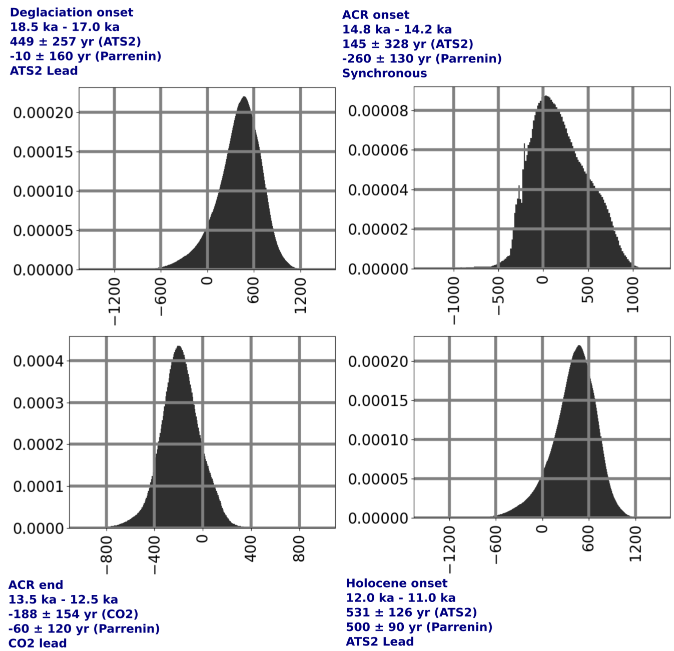

Figure 6. Probability density ρ (y-axis, normalized) of an ATS lead (x-axes, in years) at each of the selected change point intervals (noted on subfigures). Negative x-axis values indicate a CO_2 lead. In the text in each box, the name of the period, the time period in which the lead is calculated, the mean and standard deviation of the lead/lag density (μ and 1σ), and the leading variable are given.

ATS2 led CO_2 by 449 $\pm$ 257 years at the T1 onset. Given the large range of uncertainty, we cannot exclude the possibility of synchrony. At the ACR onset, the range of uncertainty (145 $\pm$ 328 years) that includes both the large relative chronological uncertainty and the ambiguity concerning the timing of the CO_2 change point, related to the large centennial scale variability near this point, does not allow us to identify a lead or lag . At the ACR end, CO_2 led ATS2, by 188 $\pm$ 154 years, but again, the possibility of synchrony cannot be excluded within 2 σ.

At the Holocene onset, a CO_2 lag is certain. Calculating the phasing between 12.0 ka and 11.0 ka, we obtain an ATS2 lead of 531 $\pm$ 126 years.

If the end of the centennial-scale change in CO_2 coincides with the true millenial scale change, which appears visually plausible, the ATS2 lead is much smaller. However, considerable uncertainty with respect to the millenial-scale change is expressed by the second mode, and the estimate of 574 $\pm$ 143 is statistically more appropriate.

3.4 Discussion

Our results refine and complicate the timings and leads and lags identified by the most recent comparable studies (Parrenin et al., 2013; Pedro et al., 2012). We identify a CO_2 change point not treated in these studies at 16 ka, associated with the end of the centennial-scale rapid rise identified by Marcott et al. (2014), and an Antarctic temperature change point at 14 ka, neither of which have a counterpart in the other series.

During the major, multi-millenial scale changes which occur at T1 and Holocene onsets, Antarctic temperature likely led CO_2 by several centuries. However, during the complex, centennial-scale change at the ACR onset, ATS was most likely synchronous with CO_2, and at the end of the ACR, CO_2 leads temperature. Further, we do not identify an analog in CO_2 of the marked temperature decrease in Antarctica after the ACR onset, or a temperature analog for the CO_2 change at 16ka, indicating at least some degree of decoupling during these changes. Additionally, the CO_2 changes at the ACR and Holocene onsets are overlayed with centennial-scale substructures. Finally, synchrony is within the 2σ uncertainty range for each of the phasings, with the exception of the Holocene onset.

The changes in CO_2 occurring at the ACR onset, ACR end and 11.6 ka and the Holocene onset have been identified to correspond with changes in CH_4 (Marcott et al., 2014), which are thought to originate in tropical wetland sources (Chappellaz et al., 1997; Fischer et al., 2008; Petrenko et al., 2009) and are indicative of Northern Hemisphere and low-latitude temperature changes during the deglaciation (Shakun et al., 2012). Indeed, the CO_2 modes appear to demarcate the rapid changes in the WD CH_4 record, shown in Figure 7.

The beginning of a gradual rise in CH_4 at around 18 ka appears to be near-synchronous with the T1 onset rise in Antarctic temperature. This rise is not seen in Greenland paleotemperature records, where it may have been masked by AMOC-driven wintertime cooling (Buizert et al., 2017) but it appears as well in proxy temperature stacks spanning both the Northern and Southern 0° to 30° latitude bands (Shakun et al., 2012).

Tephras from Mt. Takahe, a stratovolcano located in West Antarctica, have been detected in Antarctic ice cores during a 192 year interval around 17.7 ka. It has been postulated that this eruption may have provoked changes to large-scale SH circulation via ozone depletion, possibly triggering the transition between the gradual SH temperature rise beginning well before 18 ka and the more rapid rise marking the deglaciation (McConnell et al., 2017). The CO_2 mode we find at the deglaciation is coeval with this event within the range of dating uncertainty (Figure 7), and CH_4 visually appears to accelerate concurrently. However, the cumulative probability of the ATS2 change point is much greater before 17.7 ka than after.

Though the T1 onset and the ACR end are both thought to originate in AMOC reductions (Marcott et al., 2014), our results allow for the directionality of CO_2-ATS2 phasing to be reversed during the two events. CH_4 changes nearly synchronously with CO_2 at both points, but the phasings are opposite in direction and different in magnitude. This hints at a complex coupling, depending on conditions defined by multiple other variables and mechanisms, between CO_2 and Antarctic Temperature. Bauska et al. (2016), for example, hypothesize that an earlier rise of CO_2 at 12.9 ka, driven by land carbon loss or SH westerly winds, might have been superimposed on the millenial-scale trend.

The apparent decoupling between CO_2 and ATS2 at 16 ka also merits further discussion. None of the five isotopic records show significant probability in this region, but the EDML record contains extremely broad uncertainty associated with the significant ACR onset peak, stretching to 16 ka, which indicates that this portion of the EDML time series is indeed notably different in shape from the other records, even if a clear signal is not identified at 16 ka by our method. EDML is located geographically closer to the Atlantic than the other drilling sites, potentially positioning it to better record changes in AMOC or the Atlantic water cycle during the climatic reorganization that occured around 16 ka as detailed by Landais et al. (2018), who also note the difference of the EDML record during this period. CO_2 and ATS2 are similarly apparently decoupled at

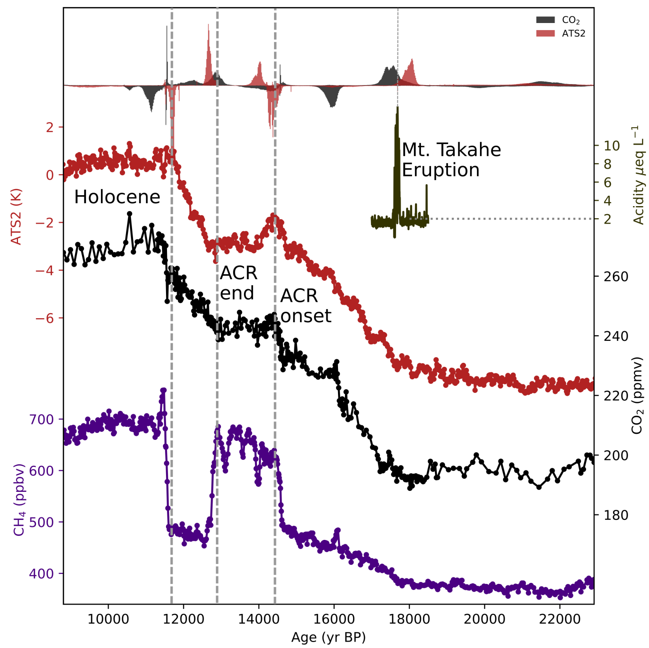

Figure 7. WD CO_2 and ATS2 change point histograms plotted with WD Acidity, ATS2, WD CO_2 and WD CH_4 series (top to bottom). Vertical lines are plotted to highlight select change point modes for the CO_2 (black) and ATS2 (red) series. CH_4 tracks changes in Northern Hemisphere climate. CO_2 modes correspond with rapid changes in CH_4 at the ACR end, ACR onset, 16 ka rise, and the rapid rise preceding the Holocene onset.

the temperature change point centered at 14 ka, and this point could be indicative of variability specific to the Pacific/Eastern Indian Ocean sectors, as it is present only in the TALOS Dome and EDC records, and slightly later, around 13.7 ka, in the WAIS Divide record, indicating a cooling trend after the ACR onset which is not clear in the DF or EDML series.

Within the range of uncertainty, our lead-lag estimates are only roughly consistent with those of Pedro et al. (2012) and Parrenin et al. (2013). The addition of the WD paleotemperature record and removal of the Vostok record from ATS2, the updated atmospheric CO_2 dataset, and our more generalized methodology are all, in part, responsible for the differences in computed time delays (SI). This testifies to the importance of data resolution, methodological development, and chronological accuracy in the determination of leads and lags.

4 Conclusions

Our study is a follow-up of the studies by Pedro et al. (2012) and Parrenin et al. (2013) on the leads and lags between atmospheric CO_2 and Antarctic temperature during the last deglacial warming. We refine the results of these studies by using the high resolution CO_2 record from WD; using the ice-air shift computed on WD; deriving a new Antarctic Temperature Stack composed of 5 volcanically synchronized ice core isotope records; and using a more precise and complete probabilistic estimate to determine change points. Our methodology detects four major common break points in both time series. The phasing between CO_2 and Antarctic climate is close but variable, with phasing ranging from a centennial-scale CO_2 lead, to synchrony, to a centennial-scale lead of Antarctic climate. This variability suggests complex mechanisms of coupling that can be modulated by external forcing.

We additionally explore the hypothesis of regional difference in temperature change in West Antarctica. Though the use of individual isotopic temperature records is complicated by the regional external influences on the isotopes, we confirm that the deglacial temperature rise did not occur homogeneously accross the Antarctic continent, with significant differences existing between the WAIS Divide and East Antarctic records at the onset of the termination, and smaller potential differences occuring between the East Antarctic records.

Hypotheses of relationships between these events should now be reinvestigated with modeling studies. The relationship between CO_2 and Antarctic temperature on longer timescales and during other periods of rapid climate change is also of interest, as is the investigation of the role CO_2 in global temperature change. Additional high-resolution West Antarctic paleotemperature records would allow for a robust investigation of regional differences between West and East Antarctica, and our analysis at the Holocene onset could be improved with continued high-resolution CO_2 measurements through the beginning of the Holocene. *Finally, the continued measurement of high-resolution ice core CO_2 records is essential to understand the relationship between CO_2 and global and regional temperature during the last 800,000 years.*

Code and data availability.

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Competing interests. The authors declare that no competing interests are present for this study.

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