Author Response (Clim. Past Discuss., 8, 621, 2012)

We are grateful to Ed Brook and the anonymous reviewer for their encouraging and constructive comments on our manuscript. We also thank Frédéric Parrennin for his extensive interactive comment. We make a point by point reply here to all of these comments.

The manuscript has been improved in four main areas:

(1) A more detailed description in the Introduction of the time intervals and also the numerical methods used in previous studies of Antarctic temperature and CO_2 phasing and associated with this a more precise comparison of our result with the previous lag estimates in the Results and Discussion.

(2) The addition of a section which demonstrates (using a jack-knife procedure) that our results are robust to the exclusion of individual records from the T_{proxy} composite.

(3) Following F. Parrennin's comments, we improve the way that the uncertainty from the sensitivity analysis is carried into our final estimate of the lag. As a result, whereas we previously concluded that the lag was likely to fall in the 0—400 year range, our main conclusion is now that "the deglacial CO_2 increase likely lagged regional Antarctic temperature by less than 400 years and that a short lead of CO_2 over temperature cannot be excluded."

All text added to the manuscript in response to the review comments copied below. Individual comments by the reviewer are labelled **EB/R2/FP** and our author response is labelled **A**.

E. Brook (Review of Clim. Past Discuss., 8, 621, 2012.)

EB: This paper further quantifies the phase relationship between Antarctic temperature and CO_2, using an approach that takes advantage of most of the relevant data that are available. It is a nice addition to the literature on this subject and seems to narrow the uncertainties. I don't find the result all that surprising, though that does not minimize its importance.

Page 624. Why not also use the EDC CO_2 record in this analysis? It can be placed on the same time scale as the others using methane and therefore the delta age problem for EDC is not relevant. The advantage of the EDC record is possibly better data quality.

I say possibly because although the EDC record is smoothly varying and therefore looks visually very reliable, it may be that smoothing in the firn has reduced shorter-term variability in the data (this may actually be a reason not to use it).

A: A number of recent studies have pointed to unresolved uncertainties in the available EDC gas timescales. Loulergue et al., (2007), concludes that there has been an "*overestimate of Δage by the firn densification model for the entire glacial period and the last deglaciation*" and that "*tests with different accumulation rates and temperature scenarios do not entirely resolve this discrepancy*". F. Parrenin makes the same point in his interactive comment stating "*previous evaluations were based on firn densification model experiments applied on the EDC or Vostok ice cores which have, in summary, been proven wrong*". Lemieux-Dudon et al. (2010) constructed a new timescale for EDC which was based on a best compromise between glaciological modelling and ice and gas stratigraphic constraints among several Antarctic and Greenland cores. However, as noted by Shakun et al., (2012), the gas timescale still appears "one to two centuries too young during parts of the deglaciation" (see their Figure S7).

E. Brook suggests that we could bypass these uncertainties by using the EDC methane record to place EDC gasses on the GICC05 timescale common with the other records. However, performing a precise methane synchronisation for EDC is made difficult by the extremely low accumulation rate which results

in substantial diffusion of gasses during bubble close; for example, Kohler et al., (2011) suggest a (climate-dependent) age distribution in the gas phase in the range 600—400 years at the site during the LGM to Bølling-Allerød (as E. Brook suggests the diffusion can also reduce short-term variability in the record). In contrast, gas records from higher accumulation sites in near-coastal Antarctica (Law Dome, Siple Dome, Byrd) are less affected by diffusion and have age distributions approximately an order of magnitude smaller. Methane synchronisation of the later cores with their sharper signals is therefore more precise than is currently possible for EDC.

The age model issues at EDC may be improved by efforts currently underway elsewhere to reassess and reduce uncertainty in Δage and Δdepth at the site; e.g. the recent submission by Parrenin et al., (Clim. Past Discuss., 8, 1089-1131, 2012) and a ongoing revision of the inverse model by Lemieux-Dudon et al. (Quaternary Science Reviews 29 (2010) 8–20). Until results from these studies are verified and made available we feel that a comparison with EDC, on the timescales currently in use would not elucidate the primary goal of determining the lag.

We add the following sentence to the revised version:

"We do not include EDC CO_2 in this analysis since there are unresolved uncertainties in the currently available gas age timescales for the core (see Loulergue et al., (2007), Lemieux-Dudon et al., (2010) and Figure S7 of Shakun et al., (2012))".

EB: Page 624, lines 0-10. A little confusing here. On the one hand the authors suggest that the Ahn et al. lag result may suffer from "from the fact that the Siple Dome deglacial isotope record contains abrupt changes not observed in other Antarctic records (Taylor et al., 2004; Brook et al., 2005), suggesting a local climate signal that would not be expected to correlate with CO_2 evolution.," and on the other hand they use the Siple Dome isotope record in the Antarctic composite, implying it is a regional climate record, I believe).

A: We are implying that the Antarctic composite better represents regional Antarctic climate than the Siple isotope record alone. We make this point with an added sentence in the revised text:

"Previous studies have demonstrated that local and/or non-climatic signals in individual ice cores are reduced in multi-core composites leading to a more robust representation of regional climate trends (e.g. Fisher et al., 1996; White et al., 1997)."

We clarify that our remark about Siple Dome applies also to other lag-assessments based on individual cores:

 "A caveat associated with the Siple Dome result and other lag assessments based on individual ice cores relates to the effects of local and/or non-climatic influences on stable isotope records (e.g. very complex ice flow, changes in surface elevation, and changes in the seasonal distribution of snow fall, Jones et al., 2009). Signals caused by such variability would not be expected to correlate with CO_2 evolution and therefore may have a confounding influence on lag assessments."

EB: Page 625, line 14-16. The authors have interpolated the CO_2 record to a very fine sample spacing then smoothed that interpolated record, and compared it to the isotope record, also smoothed, but sampled originally on this finer spacing. They explore the impact of the smoothing chosen on the results, but I would like to be sure that the interpolation does not affect the results. What happens if the isotope data are interpolated to the sample spacing of the CO_2 data? The critical issue is to understand the
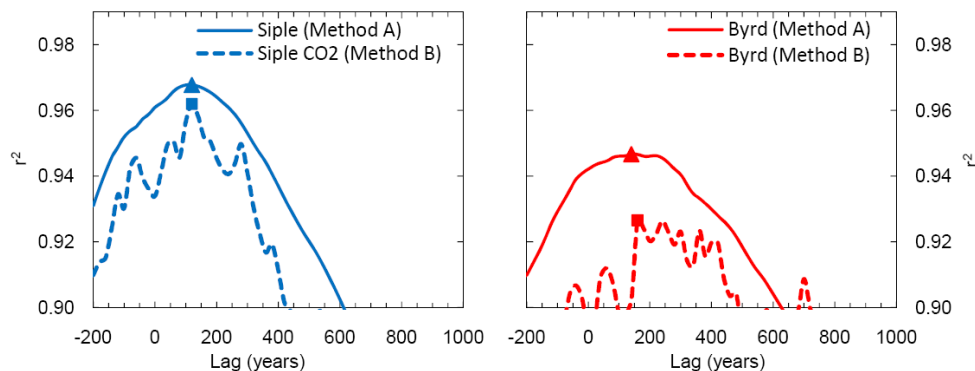
limitations placed on the result by the sampling interval for CO_2, which I do not think are necessarily addressed by the analysis done here.

A: We perform some tests to check whether the direction of interpolation affects our results. Let's call the approach we use in the manuscript (interpolation of the CO_2 data to the same fine spacing of the isotopes) 'Method A', and the approach suggested by E. Brook (interpolation of isotopes to the same spacing as CO_2) 'Method B'. Considering separately the Byrd and Siple Dome CO_2 records, as in the manuscript, we calculate the maximum of the lag-correlation for both methods

For the Siple Dome CO_2 record the maximum correlation is at lag 120 years for both Method A and B. For the Byrd CO_2 record the maximum correlation is at lag 140 years and 160 years for Method A and B, respectively. This good agreement suggests that there is not any systematic bias introduced by the interpolation direction. The lumpier shape of the Method B curves should be expected given the uneven spacing of the CO_2 series, which were not smoothed for this test.

We can see no obvious reason to prefer one method over the other. The previous Siple Dome lag assessment by Ahn et al., 2004 also used Method A. In the manuscript we apply a Monte Carlo version of Method A which takes into account the effect of varying the start and end periods for the lag calculation and varying the CO_2 measurements within their errors. We also repeat the analysis using the derivatives of the CO_2 and T_{proxy} curves and find consistent results.

In our assessment the sampling interval for CO_2 is sufficient for a robust constraint on the CO_2 temperature lag over the deglaciation. However, it is not sufficient for constraining the lag at discrete parts of the deglaciation, for example at the onset of warming or at the onset and terminations of the ACR, for this higher resolution data will be needed.

Figure 1: Trialling different directions of interpolation. Method A interpolates the CO_2 data to the same spacing as the isotopes data. Method B interpolates the isotope data to the same spacing as the CO_2 data. The curves show the correlation between the Siple (left panel) and Byrd (right panel) CO_2 records at the lags marked on the x axis. For this example, the lag correlations were calculated over the mid-range of the deglacial interval used in the manuscript, i.e. 11.8—18.0 ka.

EB: Page 626, line 14-16. The correspondence of CO_2 and Antarctic temperature may support the idea that southern ocean processes control CO_2, but it is not an ironclad fingerprint. CO_2 is globally distributed and any change in source/sink balance could cause atmospheric levels to rise. It is not inconceivable that processes outside of the southern ocean that cause CO_2 to go up could correlate with Antarctic temperature.

A: We add some text to the Results and Discussion which acknowledges this point:

"This does not imply that the Southern Ocean is the only important CO_2 source during deglaciation, in a coupled system it is plausible that other processes operating outside the region may also correlate with Antarctic temperature."

EB: Page 626, line 25. I think this should refer to Figure 1b not 2b.

A: Fixed.

EB: Finally, it would probably be appropriate to mention and address the result from the Shakun et al. paper recently published in Nature that addresses the relationship between global temperature and CO_2 during the deglaciation.

A: We agree and add a paragraph to the revised Results and Discussion:

"A brief comparison with the recent work by Shakun et al. (2012) is also warranted. Using a new multi-proxy global (rather than exclusively Antarctic) temperature reconstruction and the EDC CO_2 record, these authors argue that the deglacial CO_2 increase led global temperature by on average 460±340 years (considering the Δage issue for EDC the lead could arguably be somewhat higher). However, in constructing a global temperature curve Shakun et al. (2012) are effectively superimposing the coupled but distinct (Fig. 2) patterns of deglacial warming in the Southern and Northern Hemispheres. In our view, the remarkable similarity of the Antarctic temperature and CO_2 curves and the independent evidence (outlined below) that the high latitude Southern Ocean was a centre of action in the deglacial CO_2 release mean that the phasing determined from an Antarctic perspective is a more useful parameter for constraining the mechanisms involved in the CO_2 increase. The Shakun et al. (2012) result is nevertheless important, emphasising the role of CO_2 as both a feedback and a forcing in the deglacial warming."

Anonymous Referee #2 (Review of Clim. Past Discuss., 8, 621, 2012.)

R2: General comments:

Ice core CO_2 records show that CO_2 is strongly correlated with Antarctic temperature. However, the exact phase relationship and control mechanisms remain unclear. The authors utilise a recently developed proxy for regional Antarctic temperature in order to better compare CO_2 with Antarctic temperature during the last glacial termination. The results are similar to those from previous studies, but important to paleoclimate and carbon cycle research communities. This paper would have benefited by clarifying or better wording as suggested in the "Specific comments."

Specific comments:

Title: It is not entirely clear if the "rapid" is well supported in the paper (see comments for Page 622, Line 13-14). "during the last deglaciation" could be better words than "during deglaciation" because the authors calculated and discussed only for the last glacial termination.

A: We have revised the title in response to this concern. We think that the new title addresses R2's concern and better represents the content of the paper:

"Tightened constraints on the time-lag between Antarctic temperature and CO_2 during the last deglaciation"

R2: Page 622, Line 13-14: "faster. . . than. . . suggested by previous studies" may be misleading.

The time lag calculated by the authors is smaller than the average of the previous results, but similar to some of the individual results.

A: We rephrase this line to read:

"This result, consistent for both CO_2 records, implies a faster coupling between Antarctic temperature and CO_2 than previous estimates, which had permitted up to millennial-scale lags".

We maintain that 'faster coupling' is justified and note that E. Brook and F. Parrenin begin their reviews with comments that support our view.

R2: Page 623, Lines 14-19: the authors should be cautious in comparing the time lags because previous studies used various methods with various focuses. For example, Fischer et al. (1999) compared CO_2 with Antarctic temperature AT THE END of the glacial terminations while Monnin et al. (2001) did it AT THE ONSET of the last termination.

A: We have added a paragraph to the introduction providing precise information about the time intervals and also the numerical methods used by these studies:

"In interpreting these values it is important to be aware of the different methods and uncertainty terms that were applied. Fischer et al. (1999) used spline approximations to obtain the timing of the long-term minima and maxima in δD_{ice} and CO_2 before and after the past three deglacial transitions. The reported 400–1000 year range includes uncertainty in picking the timing of these features but excludes Δage uncertainty (estimated at between 100 and 1000 years for modern and glacial conditions at the site). The authors state that the Δage uncertainties prevent any firm conclusion about the lag at the onset of deglaciation, but that a real lag is supported at the end of deglaciation/start of interglacials. Monnin et al. (2001) focused specifically on the onset of the last deglaciation and selected the points at which CO_2 and δD_{ice} began to rise by means of the crossing points of linear fits to the respective records. The 800±600 year range includes uncertainty in picking the timing of features and also nominally in Δage. However, the age model used by Monnin et al. (2001) has since been called into question. With the aid of new dating constraints in the ice and gas phase, Loulergue et al. (2007) argue that the model substantially overestimates Δage for the last glacial period and deglaciation, implying that the lag given by Monnin et al. (2001) is also too large."

Similarly, we are more precise in our description of the methods and time intervals considered in studies of CO_2 and temperature phasing during previous deglaciations:

"A different approach to determining temperature and CO_2 phasing involves shifting the CO_2 and stable isotope records relative to one another in time until the optimum correlation between the series is reached. This method was applied to the 390–650 ka interval of the EDC core (Siegenthaler et al., 2005) and a similar method was applied to the full 420 ka of the Vostok core (Mudelsee, 2001), yielding optimum correlations at lags of 1.9 ka, and 1:3±1:0 ka, respectively. Both studies list Δage uncertainty as the major source of error in their estimates, a specific error term is not provided by Siegenthaler et al. (2005)."

R2: Page 624, Lines 3-4: It may be better to mention the age where the abrupt change was observed.

A: We now provide this information in the revised Results and Discussion:

"The abrupt increase in dD$_{ice}$ at around 15 ka in the Siple Dome record may be an example of the later (as discussed previously, Severinghaus et al., 2003; Taylor et al., 2004).

R2: Page 624, Line 26 and Page 625, Line 2: Specify "stratigraphic makers." If they represent age tie points, those ages should be provided in the paper.

A: The markers are ice-ice depth ties that carry an age as one of the cores (NGRIP) has been dated in a process independent from the tie points. Stratigraphical markers is the appropriate term as detailed in the paper we cite. We modify the text and provide a link to the data-base where they can be downloaded:

"…using stratigraphical markers (following Rasmussen et al., 2008, the markers are listed in their Table 2 and at http://www.iceandclimate.nbi.ku.dk/data/)."

R2: "Fig. 1B" instead of "Fig. 2B"?

A: Fixed.

R2: Page 627, Lines 15-17: Should be compared with Ahn et al. (2004)'s results, too. Page 627, Lines 17-20: The calculated time lags from Monnin et al. (2001) & Loulergue et al. (2007) are relevant only to the start of the last termination. Thus, the comparison with the average time lag during the entire termination should be reconsidered.

A: As mentioned earlier we now provide clear details on the different intervals considered by previous studies. In the Results and Discussion we mention again these intervals and make the comparison more cautiously.

"Direct intercomparison of our result with prior estimates is complicated by the different time intervals and different ways to estimate uncertainties applied in the respective studies. As mentioned earlier, Fischer et al. (1999) were confident in a real lag between temperature and CO_2 for Vostok only at the start of the interglacials, whereas for EDC, the 800±600 year lag was defined at the onset of deglaciation. Our result provides a tighter constraint on the lag than these studies, albeit consistent within their reported uncertainties, and it is in broad agreement with the suggested short lag at Siple Dome over the entire deglaciation. Confidence in our estimate is provided by the sensitivity analysis, consistent results for independent CO_2 records and use of two different correlation methods. The conclusion of Loulergue et al., (2007) that the Δage (and thus the CO_2 temperature lag) was significantly overestimated by Monnin et al., (2001) also lends support to our result."

R2: Page 628 Line4 _ Page 630 Line 14: It would be informative to readers if the authors shortly explain the CO_2 control mechanisms on longer (glacial-interglacial) timescales. The discussion focuses only on millennial CO_2 variations.

A: We have referenced a number of key works and review papers in this field: Lorius et al., 1990; Shackleton, 2000; Sigman et al., 2010; Skinner et al., 2010; Fischer et al., 2010. The storage and

ventilation of old CO_2 from the Southern Ocean, which is discussed in the manuscript, is important on both millennial and orbital timescales.

R2: Page 631, Line 4: "Antarctic temperature" and CO_2 lag?

A: Corrected.

R2: Page 631, Lines 5-6: "However, if the response in the Southern Hemisphere is instantaneous, then higher resolution records may not be sufficient" could be deleted.

A: Deleted.

R2: Fig 1. Locations of Vostok and Dome C sites should be marked on the Antarctic map.

A: The map now marks Vostok and Dome C.

R2: Fig 2. Gray part (no significant trend) of 14.6_14.8 ka seems to correspond to a cooling period. The boundary between gray and pink at 11.68 ka could be moved to 11.6 ka.

A: The timing of these features was determined objectively by the curvature analysis program SiZer (Chaudhuri and Marron, 1999), as described in our previous paper (Pedro et al., 2011). It is not until 14.6 ka that cooling can be declared significant at the 95% CI. As such it is not justified to alter the colour boundaries.

F. Parrenin (Interactive comment on Clim. Past Discuss., 8, 621, 2012.)

FP: In this manuscript, Pedro et al. revise the estimated phasing between Antarctic temperature (T_{proxy}) and CO2 during the last deglaciation (or TI) based on data from coastal Antarctic sites.

They find that the average lag of CO2 vs AT was only of 0 to 400 yr on average during TI, a significant revision with respect to earlier studies which suggested a larger lag.

Previous evaluations were based on firn densification models experiments applied on the EDC or Vostok ice cores which have, in summary, been proven wrong (Loulergue et al., CP, 2007). So it is certainly time to revise these previous lag evaluations which probably induce wrong interpretations of C-cycle mechanisms in our community.
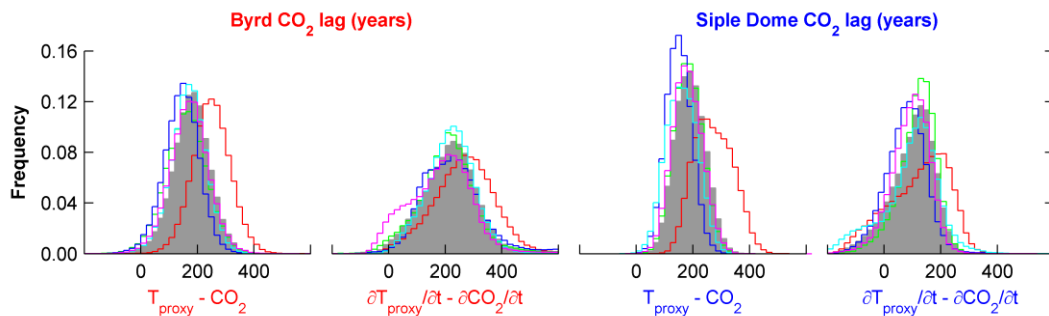
I however have technical remarks and questions concerning the method, which, I hope, will help to clarify the manuscript. I will not comment the discussion part.

A: The first part of F. Parrenin's comments concern our previous paper Pedro et al. (CP, 2011). We addressed these comments in our earlier response (http://www.clim-past-discuss.net/8/C203/2012/cpd-8-C203-2012.pdf).

We perform jack-knife tests to evaluate the sensitivity of our results to individual ice core records and describe the results in the revised manuscript:

"We conducted an additional series of jack-knife tests to assess the sensitivity of the lag estimate to the inclusion/exclusion of any of the individual stable isotope records used in T_{proxy}. By excluding in turn each

of the 5 records and using each of the two CO_2 data sets and correlation methods, 20 histograms like those in Fig. 1B are produced. The mean lag value of these 1.33 million realisations is just one year larger than the value obtained using all the records, but this mean value comprise some systematic differences. Excluding Law Dome, Byrd, Talos Dome or EDML records reduces the mean lag values by up to a couple of decades with a mean value of 12 years, while there is somewhat larger sensitivity to exclusion of the Siple Dome record, which shifts the lag higher by on average 55 years. A possible explanation for the somewhat larger sensitivity for Siple Dome may be an underestimate of Δage at that site, or the influence of local and/or non-climate signals. The abrupt increase in δD_{ice} at around 15 ka in the Siple Dome record may be an example of such a local signal (as discussed previously, Severinghaus et al., 2003; Taylor et al., 2004). Note that an error in Δage would not affect the dating of the Siple Dome CO_2 record since it is directly synchronised using methane records to Greenland."

Figure 2: Lag histograms for the two methods of determining the lag of atmospheric CO_2 after regional Antarctic temperature (direct correlation and correlation of derivatives), using each of the two CO_2 data sets. The gray background histograms are the same as in Fig. 1B based on the full T_{proxy} record. The curves on top show the corresponding lag histograms when excluding in turn each of the 5 records that comprise the T_{proxy} stack (jack-knifing): Excluding Siple (red), excluding Law Dome (green), excluding Byrd (blue), excluding EDML (cyan), and excluding Talos Dome (magenta).

FP: Now coming to the new submitted article: The manuscript refers to previous studies for the quantitative determination of the lag, so that the method is unclear at first glance. I think it is important to write in details the method in the current manuscript since it is so pivotal. In particular, it is not clear to me how the 'methodological' uncertainty for the lag is evaluated. Explicitly writing a likelihood function of the lag would help a lot the reader.

A: This is how we describe the method in the revised manuscript:

"We determine the lag quantitatively by maximising the time-lagged correlation between the deglacial temperature and CO_2 curves throughout the entire deglaciation."

"Two methods are used: first, by direct correlation between T_{proxy} and CO_2 ('direct method', similar to Siegenthaler et al., 2005); second, by correlation between the corresponding derivative curves, $dT_{proxy}=dt$ and dCO_2/dt ('derivative method', similar to Ahn et al., 2004). The derivative method has smaller sensitivity to misidentification of the pre- and post-transition levels at the expense of increased sensitivity to measurement noise, especially in the sections of sparse data."

This description is clear in our view.

The term 'methodological uncertainty' is perhaps causing confusion. What we mean here is the uncertainty captured by our Monte-Carlo sensitivity analysis (in terms of the distribution of the 66,500 optimal lag values for each CO_2 record and lag determination method). This includes (1) the effect of CO_2

data uncertainty; (2) the effect of different degrees of smoothing of the CO_2 records; and, (3) the effect of different choices of deglacial time interval. In the revised text rather than calling this "methodological uncertainty" we call it "uncertainty captured by the sensitivity analysis".

FP: - If I understand correctly, the authors use a simple linear model for the determination of the average lag over TI: $CO_2(t+lag)=T_{proxy}(t)$, where CO_2 and T_{proxy} have been normalized to unit variance and zero mean. If so, this should be explicitly stated in the manuscript.

A: It is correct that we use a simple linear model, as also used by many previous studies to evaluate temperature and CO_2 phasing (Ahn et al., 2004, Siegenthaler et al., 2005; Shakun et al., 2012).

The revised text now states "(that T_{proxy}) is normalised to unit mean and zero variance". The CO_2 data is not normalised. See also our response to the next comment.

FP: - If I understand correctly, the authors took into account the uncertainties in the CO_2 and T_{proxy} measurements, but NOT the uncertainty in their model. If so, this would be a severe flaw which would considerably underestimate the 'methodological' uncertainty of the lag evaluation. The total uncertainty (data and model) is usually evaluated from the residuals between the data and the model (in this case $CO_2(t+lag)-T_{proxy}(t)$). The total errors (data and model) are certainly not independent so that a covariance matrix should be used.

A: We have clarified above the uncertainty that is captured by the sensitivity analysis. The revised text now makes the caveat inherent in our choice of method very clear.

"A caveat associated with our lag determination method is that it implicitly assumes that the maximum of the lagged correlation does in reality provide a valid estimate of the lag. Simple linear models of this form are widely used in previous studies of temperature and CO_2 phasing (e.g. Ahn et al., 2004; Siegenthaler et al., 2005; Shakun et al., 2012)."

FP: - The advantage of using the Byrd and Siple ice cores is that they have a well constrained gas/ice offset because of their high accumulation rates. However, in the present study, the authors treat independently the gas and ice records, both being synchronized onto GICC05. It is thus not clear at first glance that the advantage of using high accumulation ice cores is preserved.

A: The Byrd and Siple Dome methane records were first synchronised to Greenland and then their ice timescales, used for $\delta^{18}O$ and δD, were derived by applying the gas/ice offset i.e. Δage. The uncertainty in Δage is carried into the uncertainty in the methane synchronised ice ages, this was done by Blunier and Brook (2001) and Brook et al., (2005), not by us. The CO_2 timescales are determined from the synchronised methane timescale, again done by Blunier and Brook (2001) and Ahn et al., (2004), not by us. The advantage is preserved since the well-constrained Δages in Byrd and Siple Dome lead to better constrained ice-age/gas-age offsets in the synchronised records.

FP:- If the CO_2 and T_{proxy} age scale are indeed treated independently, the error on the CH4 synchronisation, σ_{sync} and the error on Δ_{age}, σ_{age}, should be counted twice, since they impact both the T_{proxy} and CO_2 age scales.

A: We disagree with this comment and maintain that synchronisation error partially cancels as we also state in the manuscript.

FP: If this is not the case (as this seems to be suggested p. 627, l. 6-8), the method should be presented in a different way. A (in my opinion) better presentation would be: 1) construction of ice age scales for the Antarctic ice cores based on GICC05 and 2) construction of gas age scales from the ice age scales based on firn densification modelling. This way, we are that the ice and gas age scales are consistent.

A: With regard to both these ideas we note that the work described in the submitted manuscript Parrenin et al. (Clim. Past Discuss., 8, 1089-1131, 2012) and current work on revision of the inverse model of Lemieux-Dudon et al., 2010, in which F. Parrenin is also involved probably will represent a significant progress in this direction. However, these results are not available at this point and to carry out a similar exercise is far beyond the scope of this paper. We maintain that synchronizing the high-resolution records used by means of the methane records and using the published values for Δage is the best available option at this point.

FP: - Especially important for the onset of TI, where there are no CH4 tie points is the fact that the interpolation relies on glaciological models. Typically, if one assumes that the 1σ uncertainty on event durations from glaciological models is: 10- p. 624, l. 24-26: you are transferring gas records from GRIP to NGRIP using an ice synchronisation. Doing so, you assume that Δage as a function of the age is the same for GRIP and NGRIP. This should be explicitly stated and the error associated with this assumption should be evaluated. Same remark for the Siple CO_2 record which has been transferred from GISP2 to NGRIP via ice synchronisation.

A: We use the gas records with the ice-age gas-age relationships (i.e. Δage) published with the data as referenced. When transferring the records to GICC05 we use the published GRIP and GISP2 depths of the methane matching ties after correction for Δage and transfer to NGRIP depths using interpolation between the ice-based stratigraphic markers. No assumptions not already mentioned in the manuscript are made.

FP: - p. 625, l. 17: why the data need to be smoothed? Why not using the raw data? Specifically, the uncertainties of the measurements are defined for the raw data, not for a smoothed version.

A: The smoothing is applied to minimize spurious peaks in the lag results that occur due to the very different sampling rates of the CO_2 and temperature proxy records. The sensitivity of the results to this smoothing is duly quantified.

FP: - p. 626, l. 8: why the period younger than 11.5 kyr is excluded? This is actually where things get interesting! Indeed, if you restrict your study to one interval where both CO_2 and T_{proxy} increase linearly, the lag is not constrained anymore! (you will find a correlation coefficient of 1 whatever the lag) It is actually the break points, where the slope of T_{proxy} and CO_2 change, that allow to constrain the lag. Why do you think 'different processes are responsible'? The same processes could well act also during the remaining of TI.

A: We disagree that the lag is not constrained if we restrict to an interval where both CO_2 and T_{proxy} are increasing. In this case (which applies to some but not all of our simulations) constraint is still provided by the onset of deglaciation, the ACR and centennial scale variations in T_{proxy} and CO_2. This is particularly the case for the derivative method of lag calculation which is less sensitive to start and end points.

In the revised version we give some more information on why it is sensible to exclude the early Holocene:

"Given the close agreement between CO_2 and temperature throughout the deglacial warming and Antarctic Cold Reversal, the weaker relationship in the early Holocene may imply that different processes (e.g. processes operating outside the Southern Ocean) become more important in controlling atmospheric CO_2. Indeed, a recently produced carbon-stable isotope ($\delta^{13}C_{atm}$) record from the EDC core suggests that strong net changes in terrestrial carbon storage begin around the time that deglacial warming is complete [Schmitt et al., 2012)."

FP: - p. 627, l. 24-26: How do you combine the means and standard deviations of Fig. 2b? Do you take an average? (if so, what is the rational behind that?) Or do you assume these are independent estimates of the lag? (which is obviously not the case since they are based on the same datasets)

A: We agree that the four histograms are not fully independent. As such, this comment alerts us to the possibility that the weighted standard deviation may not properly account for the spread in the sensitivity analysis. For the revised manuscript we take a conservative approach and pool all of the results, this increases the uncertainty from the sensitivity analysis from 34 to a more conservative 89 years. The text is revised accordingly:

"Since the individual distributions are not completely independent we take a conservative approach and pool all of the results from Fig. 1B. Applying a Gaussian best-fit to the pooled results suggests a mean (±1 sigma) for the lag of 162 ± 89 years".

FP: - p. 627, l. 10-11: Where the 200 years uncertainty comes from? The evaluation of uncertainties should be as mathematical, precise and as objective as possible. By the way is it a 1σ or a 2σ uncertainty? Same question for the 0-400 yr lag estimate. In p. 626, l. 26, you give a 'best guess' value for the lag of 163 yr. Now the middle of the 0-400 interval is 200 yr. Why this difference?

A: How we arrive at the 200 year relative dating uncertainty is already explained in the manuscript. We revise the text to clarify the error propagation and take into account the revised uncertainty from the sensitivity analysis.

"We estimate an overall relative dating uncertainty (nominal 1σ) of 200 years. This term is independent of the 89-year methodological uncertainty from the sensitivity analysis. Taking the central estimate of the lag from the pooled sensitivity analysis and combining the two uncertainty terms in quadrature leads to a likely range for the CO_2 lag of -56 to 381 years. From this we arrive at our main conclusion that the deglacial CO_2 increase likely lagged regional Antarctic temperature by less than 400 years and that a short lead of CO_2 over temperature cannot be excluded."

The abstract is also changed accordingly.

Other changes to the manuscript not otherwise specified above:

Following personal communication from J. Álvarez-Solas we clarify the part of the Discussion referring to freshwater forcing and AMOC weakening during Greenland Stadial 2 and cite some recent work in this area:

"A feedback process has recently been proposed wherein the weakened overturning leads to a warming of North Atlantic subsurface waters (Marcott et al., 2011; Gutjahr and Lippold, 2011), destabilising ice shelves and in turn driving further ice and freshwater release (Alvarez-Solas et al., 2011)"

We add "ka b1950" to all GICC05 dates reported in the paper and define this nomenclature in the text:

"All dates mentioned in the text hereafter use the convention `ka b1950' referring to thousands of years before 1950 AD."

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