Answer to report 2 of reviewer 2

The colour code for changes in the new manuscript are in red for the first round of review and in blue following the comments and answers to the current report.

Review of revision of Bazin et al. “Phase relationships between orbital forcing and the composition of air trapped in Antarctic ice cores”

The authors have improved the manuscript in several regards; the most important of which are the inclusion of appendix B in which they assess the accuracy of the assigned minima and maxima (as requested by both reviewers), and the use of a recently published DF-EDC volcanic synchronization. Unfortunately, in several other cases the authors did not adequately address the reviewer concerns (outlined below).

Both reviewers noted that invoking Heinrich events to explain the lag of d18Oatm behind O2/N2 is highly speculative. To my mind, justifying such a claim would require either (a) a process-level understanding of how the d18Oatm lag is linked to H-events, or (b) very strong empirical evidence that the two are linked.

Regarding (a), the authors argue that during H-events the d18O of low-latitude meteoric waters becomes enriched, which in turn increases d18Oatm. While this is indeed a plausible explanation for the observed d18Oatm trends during H-events (Severinghaus et al. 2009), it does not explain why the lag of d18Oatm behind precession should increase. I understand that superimposing d18Oatm excursions on top of the orbitally-driven d18Oatm signal can change the perceived location of the d18Oatm maxima/minima; however, it seems to me that this process is just as likely to shorten the perceived d18Oatm lag time as to lengthen it. Furthermore, the proposed mechanism does not imply a change in the response of the Dole effect to precession (as the authors suggest), but merely a shift in the perceived location of the maxima/minima due to the superposition of a second signal.

We fully agree with this proposition of a shift in the perceived location of the maxima/minima due to the superposition of a second signal. We have thus followed this good way to explain this possible process in the new text. Consequently, we have rewritten the corresponding paragraph in the discussion.

Lines 437-456:

“Severinghaus et al. (2009) have observed a systematic increase of δ18Oatm during Heinrich events over the last glacial period, these events being imprinted both in the calcite δ18O and ice core δ18Oatm. Landais et al. (2013) also evidence that the maximum in δ18Oatm during Terminations I and II are directly related to the occurrence of large Heinrich events before the abrupt increase in North Atlantic temperature. Again the δ18Oatm signal over these two terminations parallels the calcite δ18O signals of Chinese speleothems. Following this finding, Reutenauer et al. (2015) used outputs from coupled climate model and atmospheric general circulation model equipped with water isotopes to estimate the change of δ18Oatm induced by a freshwater input. These calculations show that the increase of δ18Oatm during a Heinrich event is induced by a southward shift of the ITCZ associated with the freshwater input leading to an increase of the δ18O of the low-latitude meteoric water in the northern hemisphere. This signal is then transmitted to the δ18O of O2 through photosynthesis of the important terrestrial biosphere in low latitudes of the Northern Hemisphere during the last glacial period. The occurrence of freshwater input can thus delay the change in δ18Oatm induced by the sole insolation. This mechanism would satisfactorily explain a lag in the perceived location of the maximum in the δ18Oatm signal compared to the sole influence of precession. Our working hypothesis is thus that we have a superposition of two signals influencing δ18Oatm: (1) a direct effect of precession leading to increase of δ18Oatm for increasing precession and (2) an influence of Heinrich events, or Greenland/European ice sheet discharge events, with the associated weak monsoon intervals leading to an increase of δ18Oatm."

Regarding (b), the suggested synchroneity of Heinrich activity and increased d18Oatm lag time is simply not very convincing, as I argued before. The temporal mismatch is on the order of 10-20ka, so much larger than the dating uncertainty. The assigned uncertainty to the manual picks also seems too small (should be at least the 3-4ka from appendix B). Increased lag times all occur during glacial times, so the authors could just as well have invoked global temperature or ice volume as the culprit. The lag curve actually closely resembles the orbital eccentricity (old figure 4), with large lags during times of small eccentricity (and hence weak precession forcing).

We do not see the same temporal mismatch between the marine records and ice core records as the reviewer suggest. We agree that the respective chronology uncertainties of both archives prevent us from discussing the absolute timing of occurrence of ice sheet discharge events and the delay increases. However, when we compare roughly their stratigraphic position, ice sheet discharge events (either Hudson strait and/or Greenland/European) occur before or at the same time (cannot say due to the absolute uncertainty of timing) as the increase of the d18Oatm-dO2/N2 delay. In order to illustrate that, please see the zoom on figures R1 and R2 with tentative identifications of the same "events" in the isotopic records of EDC and marines cores. Moreover, we have corrected the text accordingly to make it clearer about the relative timing compared to the stratigraphy.

L470-476:
"The uncertainty associated with this dating method is estimated to be 4 ka for the last 1 million years. Such a large uncertainty prevents us from any comparison of the absolute timing of ice sheets discharge events with our ice core records. However, the occurrence of IRD events against the δ18O record of foraminifer gives us information about their relative timing within the stratigraphy. We thus only discuss the occurrence of Heinrich-like events and Greenland/European ice sheets discharges in regards to the variation of the δO2/N2–δ18Oatm offset."

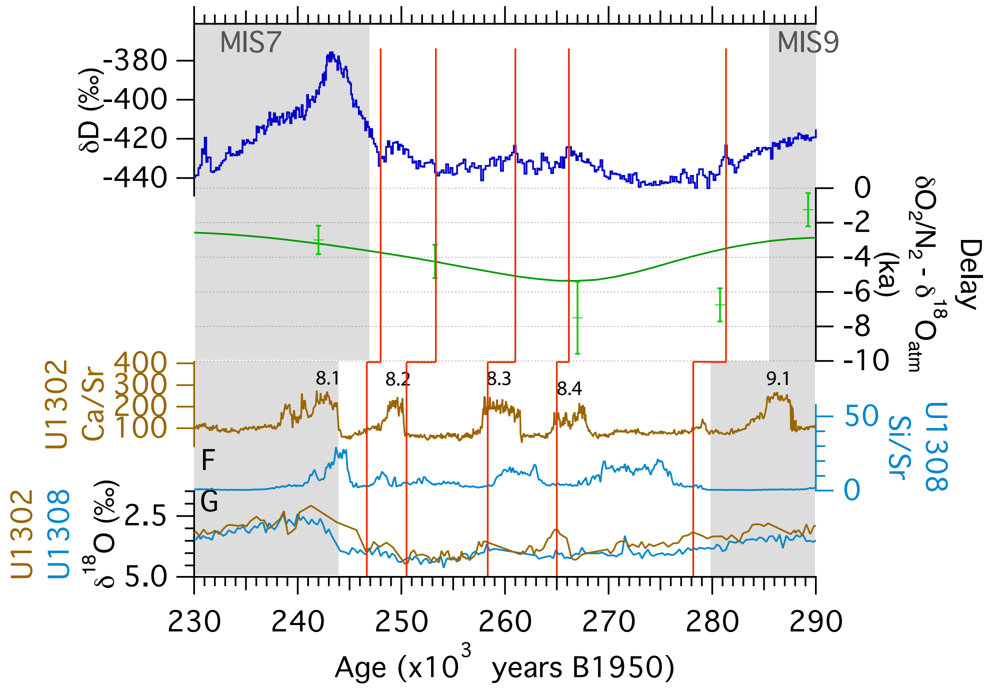


Figure R1: Water isotopic composition of EDC and delay between dO2/N2 and d18Oatm compared with the IRD and foraminifer isotopic composition of both marine cores between 230-290 ka. The red lines illustrate a tentative identification of the same "isotopic event" in both archive records.

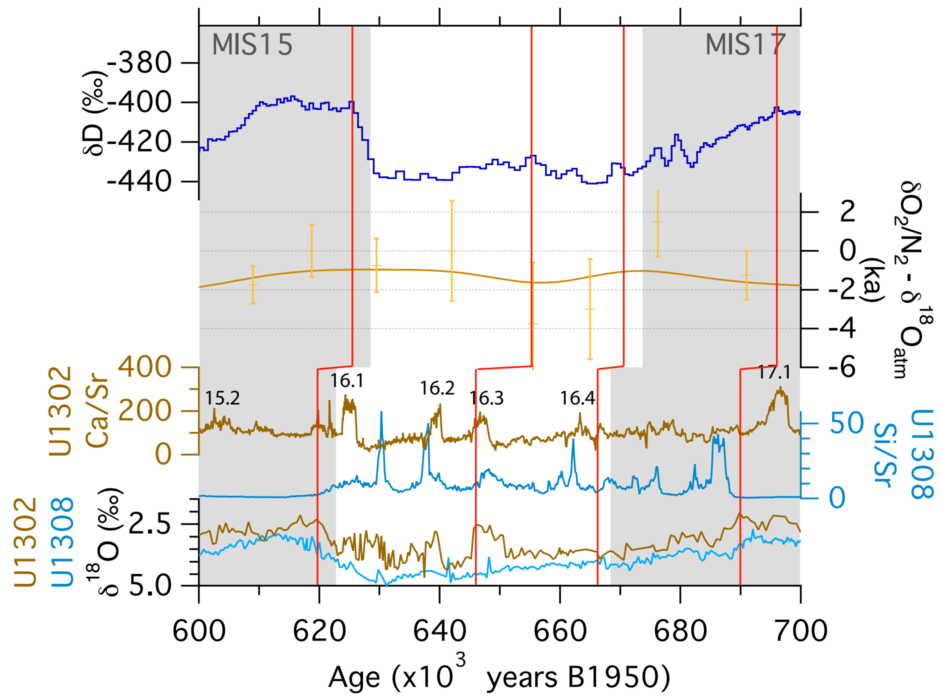


Figure R2: Water isotopic composition of EDC and delay between dO2/N2 and d18Oatm compared with the IRD and foraminifer isotopic composition of both marine cores between 600-700 ka. The red lines illustrate a tentative identification of the same "isotopic event" in both archive records.

We do not have any mechanism for invoking global temperature or ice volume as a culprit. On the contrary, over the last two terminations (i.e. where we have relatively well dated records), ice volume and global temperature changed several thousand of years before the inflexion point in d18Oatm. Temperature and global ice volume can thus not be invoked as the driver of the d18Oatm variations (Landais et al., 2013).
We made the text clearer and have added the following text:

L439-442:
"Landais et al. (2013) also evidence that the maximum in δ18Oatm during Terminations I and II are directly related to the occurrence of large Heinrich events before the abrupt increase in North Atlantic temperature."

In summary, I am not convinced by the evidence presented for the link between the dO2/N2-d18Oatm lag and Heinrich events. However, paleoclimate science has a long tradition of speculation (unfortunately), and undoubtedly other reviewers would be comfortable with the claims that the authors make. If the authors choose to persist in their claims, they should at the very least include the caveats mentioned here.

This has been done with the integration of the two paragraphs above (previous comment) and the following paragraph in the conclusion:

L524-537:
"We have calculated the phase delay between δO2/N2 and δ18Oatm over the last 800 ka by coupling Vostok and EDC data. This lag has varied from 1 to more than 6 ka with minimum values occurring during MIS 6–7, the end of MIS 9, the end of MIS 14-start of MIS 15 and the end of MIS 17, corresponding to periods of intermediate ice-sheet extent with no occurrence of strong ice sheet discharge events (Heinrich-like events and/or Greenland/European ice sheet discharge events). Based on results observed over MIS 5, we made the assumption that δO2/N2 is more or less synchronous with summer solstice insolation and that the δO2/N2–δ18Oatm varying lag is mainly induced by variations in the relationship between δ18Oatm and precession. It has been shown over Terminations I and II that the δ18Oatm response to precession peak can be delayed by Heinrich events, associated with weak monsoon intervals. We thus propose that the variations of the apparent lag between δ18Oatm and δO2/N2 is due to the superposition of two influences on the δ18Oatm signal: orbital forcing (precession) and millennial forcing (ice sheet discharge events associated with weak monsoon intervals) on the low-latitude hydrological cycle, hence on δ18O of meteoric water transmitted to δ18Oatm by photosynthesis."

Both reviewers commented on the use of imprecise and incorrect language. Unfortunately the revised manuscript has not improved in this regard. The request by reviewer 1 for less ambiguity in the sentence subjects has not been implemented – even the sentence picked by reviewer 1 as an example of this problem has not been altered. I (reviewer 2) gave a list of typos and language corrections at the end of my review. The authors comment that all of these have been corrected in revision; however, on comparing to my original list to the revised manuscript I noticed that none of them have been corrected. While this was undoubtedly due to an honest mistake (e.g. mixing up different versions of the manuscript) it will need to be addressed in a future version of the MS.

You are right that it was due to a mix up between different versions of the manuscript. We have now reformulated the ambiguous sentences and corrected the typos and languages listed in the first round of review and the ones listed in the current report. We are very sorry about this mistake and we apologize to the reviewers for the offence.

Additional comments:

1) I commented that the AICC chronology is based on orbital tuning of d18Oatm, and therefore one cannot meaningfully interpret the power spectrum of d18Oatm (Fig. 2a) because orbital frequencies are included by design. The authors concede that this is true, but make no corrections to the MS (such as e.g. remove Fig. 2a, or include this important caveat).

We have now removed figure 2a from the new manuscript and we have modified the corresponding paragraph as follow:

L194-199:
"The spectral analysis of the new δ18Oatm record on the AICC2012 chronology gives a power spectrum consistent with previous studies for EDC between 400 and 800 ka (Dreyfus et al., 2007, on EDC 2 and EDC 3 chronologies) as well as Vostok and Dome F records between 0 and 400 ka (GT4 - Petit et al. (1999) and DFO-2006 - Kawamura et al. (2007) respectively). Since the construction of the timescale AICC2012 partly rely on d18Oatm orbital tuning, no additional information from the spectral analysis over the orbital frequencies can be expected."

2) The inclusion of appendix C is an improvement, but I don't see why the authors don't simply plot DF tuned to AICC2012 (as in C1 upper plot) in Figure 3. That way all three cores are on the same chronology. This would avoid much confusion, and allows a meaningful comparison.

We did not include the tuning of DF records on AICC2012 in the main text of the original manuscript initially because the correlations between the dO2/N2 records of both sites on either tuning (AICC2012 or DFO-2006) were not improved compared to the original chronologies. Since, the volcanic synchronization between EDC and Dome F has been recently published (Fujita et al., 2015), you are right it will be more consistent to present DF data tuned on AICC2012. Consequently, we have replaced figure 2 with the following figure R3, and we have modified the related text. We have also rearranged the appendices B and C for consistency with the order of reference in the main text. Moreover, we have added the identification of extrema and mid-slopes of the dO2/N2 record of Dome F tuned to AICC2012 in appendix C (figure C1 and table 2).

L260-262:

"Figure 3 displays the δO2/N2 records from Dome F (transferred on AICC2012 using volcanic matching (Appendix B), Fujita et al., 2015; Kawamura et al., 2007), EDC and Vostok (both on their respective AICC2012 chronologies, Veres et al., 2013; Bazin et al., 2013) from 100 to 150 ka."

L276-277:
"Figure 3 presents the water isotopic composition, δO2/N2 and δ18Oatm on the AICC2012 timescale, using the volcanic synchronization proposed by Fujita et al. (2015) for Dome F data (Appendix B)."

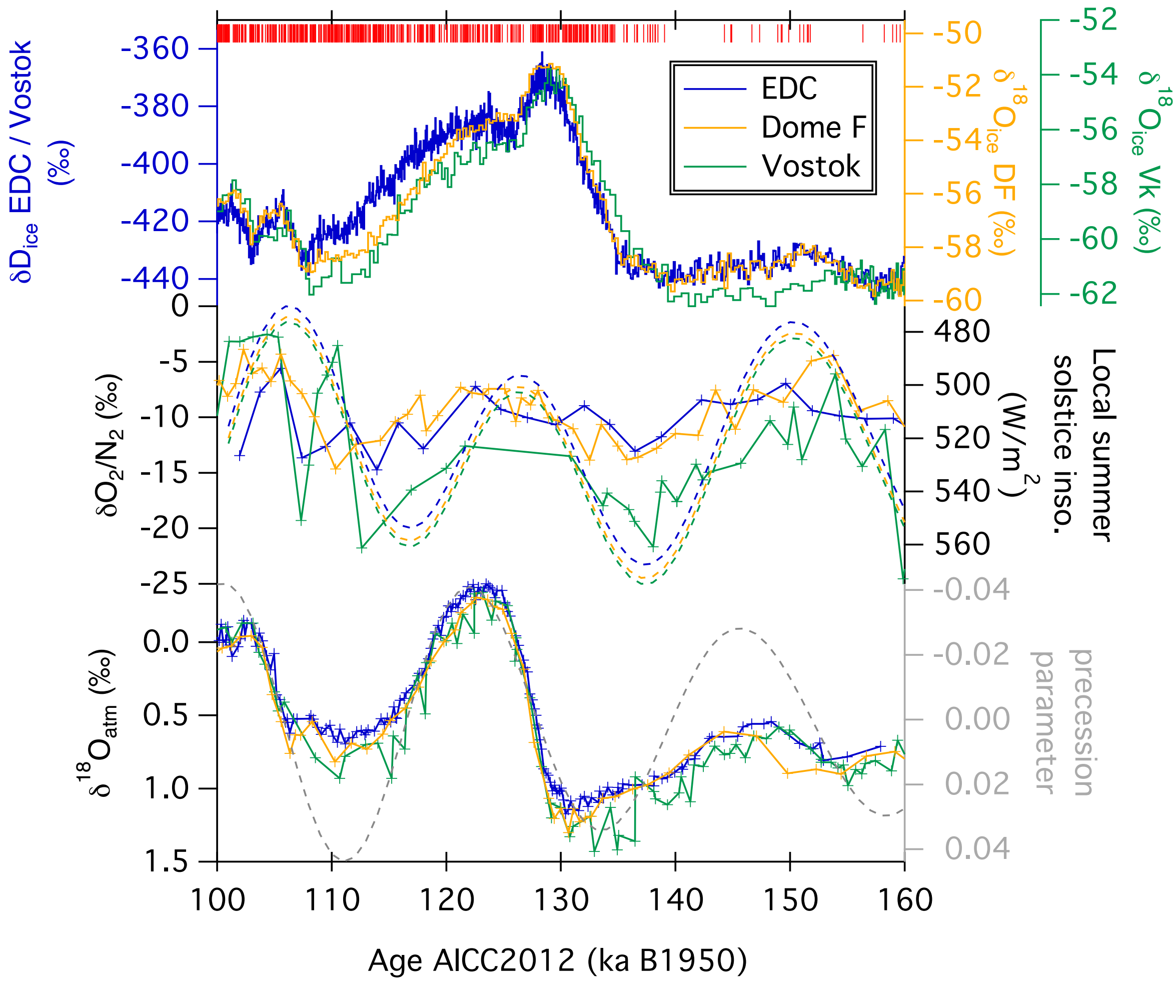


Figure R3: Inter-comparison of Vostok (green), Dome F (yellow) and EDC (blue) data covering MIS 5 presented on AICC2012 (Bazin et al., 2013; Veres et al., 2013), using the volcanic matching between Dome F and EDC published by (Fujita et al., 2015). Top: water isotopic composition (Vostok δ18Oice: Petit et al. (1999), Dome F δ18Oice: Kawamura et al. (2007), EDC δD: Jouzel et al. (2007)). Middle: δO2/N2 records and local summer solstice insolation at each site (Suwa and Bender, 2008b; Kawamura et al., 2007, this study). Bottom: δ18Oatm and precession parameter shifted by 5 ka (Suwa and Bender, 2008b; Kawamura et al., 2007; Landais et al., 2013, this study).

3) Line 11: the authors claim that at GISP2 the dO2/N2 behaves differently than

Antarctica. This actually seems opposite to what is claimed by Suwa and Bender (2008a), who write: "The stacked GISP2 dO2/N2 record shows strong spectral power at the orbital frequencies, and dO2/N2 is in antiphase with local summer insolation. This observation is consistent with the earlier findings for the Vostok and Dome Fuji ice cores from East Antarctica. ". Please explain what you mean here.

You are right this sentence (L111) needs rewording. The point we wanted to make is that the GISP2 dO2/N2 record presents millennial scale variability in addition to the orbitally-driven variations. We propose the following sentence in replacement:

L108-114:
"Finally, while Suwa and Bender (2008a) have confirmed the orbital signature in the δO2/N2 record from the Greenland core GISP2 as already observed in Antarctic records, they have also observed an additional millennial-scale variability component. The millennial variability of GISP2 δO2/N2 record is in phase with accumulation rate and temperature changes associated with the Dansgaard-Oeschger events, suggesting a non-negligible influence of local temperature or accumulation on δO2/N2 variations.

4) On lines 275-278 the authors keep arguing for a 2ka shift in dO2/N2, which was disputed by both reviewers. Appendix C shows the uncertainty to be 3-4ka, so this is not a robust result. The 2ka shift in DF d18Oice is just due to a dating problem, and this section is needlessly confusing.

On lines 275-278 we did not argue about the dO2/N2 lag, but just tried to remind the reader that the two chronologies (DFO-2006 and AICC2012) present a 2 ka shift at termination II. With the change of figure 3 that you have suggested, i.e. with volcanic tuning on AICC2012, we have now fully rewritten the paragraph (cf answer to comment 2 and following changes).

L276-282:
"Figure 3 presents the water isotopic composition, δO2/N2 and δ18Oatm on the AICC2012 timescale, using the volcanic synchronization proposed by Fujita et al. (2015) for Dome F data (Appendix B). This timescale transfer removes the original 2 ka shift for Termination II observed between the Dome F (DFO-2006 Kawamura et al., 2007) and AICC2012 chronologies (Bazin et al., 2013). Fujita et al. (2015) have proposed that the large age offset between the DFO-2006 and AICC2012 chronologies originates either from an overestimation of the surface mass balance in the glaciological approach and/or an error in one of the δO2/N2 age constraints by 3 ka."

5) The attempt to correlate dO2/N2 directly to accumulation (Lines 299-307) is not meaningful, because variability in the O2/N2 signal is dominated by the local insolation signal. Kobashi et al. could study the correlation during the last 4ka, because the insolation signature is small during such a short period. The O2/N2 signal should be corrected for the insolation signal first before attempting to find correlations with accumulation – which is not an easy task to undertake.

The way to correct the dO2/N2 data from the insolation variations is not trivial. One way to try it would be to use the Multi-Resolution Analysis (MRA) for example. However, we consider such a work to be beyond the scope of this paper. Such an in-depth investigation should definitely be studied in the near future, especially if we want to reduce the uncertainties associated with orbital tuning. For the scope of this paper we prefer to just specify that the dO2/N2 to accumulation relationship is not simply linear and open the door for future work.
Consequently we made the following changes in the text:

L304-311:
"Another approach would be to consider the accumulation rate corresponding to the gas age, following Kobashi et al. (2015) observations of a significant correlation between the δAr/N2 on the gas age and the accumulation rate for Greenland ice cores over the Holocene. However, as our δO2/N2 record spans periods with varying insolation conditions, we would first need to correct our δO2/N2 record from insolation variations. Such a correction cannot be performed in a simple way but should be tested in future. Consequently, we only state here that there is most probably a link between accumulation variations and δO2/N2, as previously suspected, but this relation is not linear and need to be further investigated."

6) Lines 346:347. I was not aware that the timing of the insolation maximum at 15 Jan differs by as much as 2 ka from that at 21Dec. This seems important to the use of O2/N2 as a dating tool. This uncertainty should be added to the 3-4ka uncertainty identified in App B for dating applications.

This is one of the reasons why we think that dO2/N2 uncertainty estimate of 2 ka is underestimated. Still, we do not think this should be systematically integrated in the uncertainty calculation yet. Indeed, in order to properly estimate the uncertainty associated with the choice of orbital tuning, i.e. the insolation curve corresponding to summer solstice or maximum of temperature, we would need longer records of temperature and surface snow conditions at the different Antarctic ice core sites. The proposed uncertainty of 3-4 ka is already more reasonable than 2 ka as proposed by other authors.

7) Lines 445-456. I really don't see how this mechanism can influence the delay behind precession – wouldn't it just shift d18Oatm in the vertical direction? This could shift the perceived location of minima/maxima both forward and backwards, depending on whether the orbital d18Oatm trend at the time is positive or negative.

You are right, the confusion was due to the imprecision in our explanations. We have now reworded the text. Hopefully, this is now clearer.

L437-456:
"Severinghaus et al. (2009) have observed a systematic increase of δ18Oatm during Heinrich events over the last glacial period, these events being imprinted both in the calcite δ18O and ice core δ18Oatm. Landais et al. (2013) also evidence that the maximum in δ18Oatm during Terminations I and II are directly related to the

occurrence of large Heinrich events before the abrupt increase in North Atlantic temperature. Again the δ18Oatm signal over these two terminations parallels the calcite δ18O signals of Chinese speleothems. Following this finding, Reutenauer et al. (2015) used outputs from coupled climate model and atmospheric general circulation model equipped with water isotopes to estimate the change of δ18Oatm induced by a freshwater input. These calculations show that the increase of δ18Oatm during a Heinrich event is induced by a southward shift of the ITCZ associated with the freshwater input leading to an increase of the δ18O of the low-latitude meteoric water in the northern hemisphere. This signal is then transmitted to the δ18O of O2 through photosynthesis of the important terrestrial biosphere in low latitudes of the Northern Hemisphere during the last glacial period. The occurrence of freshwater input can thus delay the change in δ18Oatm induced by the sole insolation. This mechanism would satisfactorily explain a lag in the perceived location of the maximum in the δ18Oatm signal compared to the sole influence of precession. Our working hypothesis is thus that we have a superposition of two signals influencing δ18Oatm: (1) a direct effect of precession leading to increase of δ18Oatm for increasing precession and (2) an influence of Heinrich events, or Greenland/European ice sheet discharge events, with the associated weak monsoon intervals leading to an increase of δ18Oatm."

L533-537:
"We thus propose that the variations of the apparent lag between δ18Oatm and δO2/N2 is due to the superposition of two influences on the δ18Oatm signal: orbital (precession) forcing and millennial scale forcing induced by ice sheet discharge events associated with weak monsoon intervals on the low-latitude hydrological cycle. This δ18O signal of meteoric water was then transmitted to δ18Oatm by photosynthesis/respiration cycles."

8) As a general comment, it may be a good idea to discuss all the implications of your results for the future dating of ice cores in one central place. This would improve the logical structure of the work.

Following this comment we have added a full paragraph in the conclusions regarding the implications of our results for the future dating of ice cores.

L513-523 for dO2/N2:
"Thanks to our comprehensive dataset, we have been able for the first time to compare the sequence of events between water stable isotopes, δO2/N2 and δ18Oatm for three Antarctic ice cores (EDC, Vostok and Dome F), over MIS 5. The combination of δO2/N2 records from the three sites has permitted us to estimate the uncertainty of the δO2/N2 orbital tuning method to be in the order of 3-4 ka. However, differences in the mean level of δO2/N2 and their high-frequency variability have been noticed. This study demonstrates the strength of a multi-proxy, multi-ice cores chronology approach for proper assessment of uncertainties of individual age markers. The mechanisms responsible for local δO2/N2 variations still remain to be understood. This is particularly important over periods of low eccentricity when the insolation variations are not well imprinted in the δO2/N2 records (350–450 ka and 700–800 ka). The δO2/N2 orbital tuning method should be used in combination with

other dating methods over these periods."

L524-537 for d18Oatm:
"We have calculated the phase delay between δO2/N2 and δ18Oatm over the last 800 ka by coupling Vostok and EDC data. This lag has varied from 1 to more than 6 ka with minimum values occurring during MIS 6–7, the end of MIS 9, the end of MIS 14-start of MIS 15 and the end of MIS 17, corresponding to periods of intermediate ice-sheet extent with no occurrence of strong ice sheet discharge events (Heinrich-like events and/or Greenland/European ice sheet discharge events). Based on results observed over MIS 5, we made the assumption that δO2/N2 is more or less synchronous with summer solstice insolation and that the δO2/N2–δ18Oatm varying lag is mainly induced by variations in the relationship between δ18Oatm and precession. It has been shown over Terminations I and II that the δ18Oatm response to precession peak can be delayed by Heinrich events, associated with weak monsoon intervals. We thus propose that the variations of the apparent lag between δ18Oatm and δO2/N2 is due to the superposition of two influences on the δ18Oatm signal: orbital (precession) forcing and millennial forcing induced by ice sheets discharge events associated with weak monsoon intervals on the low-latitude hydrological cycle. The δ18O signal of meteoric water was transmitted to δ18Oatm by photosynthesis/respiration cycles."

Some additional typos and language corrections (in addition to the previous ones that were not corrected):
Line 39: the construction of the chronology has not been confirmed, but the accuracy of the chronology.
Line 116 and 118: please avoid the "we" form when talking about published work (the author list is not the same)
Line 124: The work of Ikeda-Fukazawa seems to suggest it is diffusion through the ice crystals rather than loss through micro-cracks.
Line 186: what is meant by "this"
Line 236: what is meant by "component of the surface energy budget"
Line 284: O2/N2 age constraintS
Line 286: does not significantly improve the correlation...
Line 371: This comparison relies on...
Line 392: between 15-100ka?? Do mean you use a bandpass filter with 15-100ka period pass band?
Line 392-393: "The filter is computed using Fourier transform and convolution products". This phrase so general that it is almost meaningless.
Line 393-394: "The delay....after cross-correlation". How d oes this give you a continuously changing delay? Do you use a moving window to calculate the delay? Please give more details, such as the window size.

We only mean that the phase is calculated for each point of the wavelet transforms of the resampled dO2/N2 and d18Oatm records. In the Fourier system we can then calculate the phase between the two records. Then, the delay is deduced after changing of referential from the phase space into the time space.

Line 412: "Matlab delay" is not defined. What is it?

Line 422: the MISA 16 lag seems to be smaller than 2ka from the figure.... (not the -3ka that is claimed)
Line 461: Heinrich events consist OF
Near Line 480: MIS 7 actually shows manual lags of 7ka or so, in the absence of H-activity....

There are Heinrich-like events occurring during MIS7, corresponding to the delay increase. For clarification we have now added the numbering proposed by Channel et al., 2012 of the Heinrich-like events recorded in the Ca/Sr ratio of core U1302/03.

Line 601: Marine core data

All of the typos and languages corrections listed here and in the previous record have been corrected in the current version of the manuscript.