Answer to reviewer 1

Overview This paper presents new δO2/N2 and δ18Oatm measurements from the Dome C ice core. The ice was kept at very cold temperatures to avoid gas loss. Phase relationships with orbital parameters were investigated, confirming considerable uncertainty of these gases as dating tools. The phase relationship between δO2/N2 and δ18Oatm was also investigated with speculation that Heinrich events affect the magnitude of the lag of d18O relative to dO2/N2.

This paper has the potential to be a good discussion of the uncertainty associated with the use of ice-core gas measurements for orbital tuning. The new δO2/N2 and δ18Oatm measurements are a valuable contribution and provide sufficient resolution to assess multi-millennia phase relationships. Unfortunately, the writing and organization of the paper need considerable improvement to justify publication. The confusion surrounding the timescales of Dome Fuji and Dome C during MIS5 is highlighted by Dr. Wolff's comment; the authors' brief appendix does not sufficiently improve this section.

Overall, this paper has useful new data and the potential to contribute to ice-core dating. However, the current form of the manuscript needs substantial improvement prior to publication.

Specific scientific issues:

Uncertainty, filtering and lags – Overall, statistical quantification is lacking in the paper. As discussed more below, the timing of the MIS5 minima, on which the site-specific differences in dN2/O2 rest, is not defined objectively. Later, there is virtually no description of the filtering, other than the statement "by wavelet transform". There is no reference given. Looking at Figure 4, it seems like the wavelet filtering may be shifting the timing of minima and maxima. In addition, there is no description of how the lag values have been determined. Is it by cross-correlation of the filtered data? How is the measurement noise included? How is the timescale (gas vs. ice) uncertainty incorporated? What time windows are the lags being determined for? If there are multiple estimates of lags during a period (say between 550-650 ka) how do the estimates differ within a period? Can the measurements resolve a ~1 ka lag when the average sampling resolution is 1.1 ka for δ18Oatm and 2.35 ka for δO2/N2?

Generally we have given more precision in the new version of the manuscript. The different methods used are more detailed in the paper.
For discussion of the dO2N2 records over MIS 5, we now have treated the data using different methods (smoothing, re-interpolation, filtering) for the three sites in order to comprehensively define the minima, mid-slops and maxima of each dO2/N2 record as well as an estimation of their uncertainty. Please find below more details to answer the specific comment.

The filtering methods were performed using Analyseries software (Paillard et al., 1999), apart for the delay calculation. The data are re-interpolated evenly, with respect of their mean resolution, and filtered using a piecewise linear shape with a slope bandwidth of 10^{-9} a^{-1} and between 15-100 ka.

Lag values are obtained using wavelet transform after resampling (lowest resolution between d18Oatm and dO2/N2), filtering (15-100 ka) and cross-correlation calculation using Matlab. The delay is calculated from the reconstructed (wavelet) filtered data and is deduced from the phase calculated between the two records. The delay values are also confirmed by minima/maxima identification from the raw, smoothed, resampled and filtered data (same method as for MIS 5) with an estimation of the delay uncertainty, now added on figure 4 of the article. The combination of both estimations permits to assess the robustness of the variation of the delay value.

We have now completed the periods with missing data that were published in the CPD manuscript by performing new measurements, between 470-490 ka and 340-380 ka. We have re-done the delay calculation on our new complete record of EDC between 340-800 ka. The delay calculated for this complete dataset is unchanged compared to the previous version of the article, where the delay was calculated with a 1 ka resolution. The fact that the delay is unchanged, although the new calculation is more adapted to the data, gives us confidence in its values.

MIS 5 The different timescales for Dome Fuji and Dome C present a major difficulty in the presented comparison, as pointed out by Dr. Wolff. As discussed above, the authors' appendix is not sufficient to address the confusion in this section. The appendix shows two figures with either Dome Fuji aligned to Dome C or vice versa. However, this appears to align the warming of TII and not the glacial inception. While I understand the authors wanted to choose unambiguous markers in the isotopes, I think correlating the full d18Oice curves for the ice timescale and d18Oatm for the gas timescales would be much more useful.

Following Dr Wolff comment we have performed a new synchronisation based on the volcanic synchronisation of EDC and DF of Fujita et al., 2015, using the supplementary material of their final CP paper. Using the volcanic matching, we have now transferred (1) Dome F data (d18Oice, dO2/N2 and d18Oatm) from DFO-2006 to AICC2012 (see Figure 1 below) and (2) EDC data from AICC2012 to DFO-2006 chronology (see Figure 2 below). As you can see on Figures 1 and 2, there are numerous volcanic markers (red markers on top) between these two cores over the whole MIS5 period, compared to the 6 isotopic tie-points we have first proposed in the answer to Dr Wolff comment. This volcanic synchronization is then more robust and also independent of any climatic assumption compared to the previous tests presented in the answer to Dr Wolff comments. The volcanic matching tends to give similar results as previously. As noted by Fujita et al., 2015, this volcanic synchronization do not resolve the difference of ice isotopic composition over the glacial inception at these two sites. Potential causes for this large age difference between the DFO-2006 and AICC2012 chronologies are suggested to come from an overestimation of the surface mass balance in the glaciological approach and/or an error in one of the dO2/N2 age constraint by 3ka. We have added these figures in Appendix C and included the following paragraph in the new text page 9:

« This particular feature is persistent after volcanic synchronization between EDC and Dome F ice cores (Appendix C; Fujita et al., 2015). In Fujita et al. (2015), potential causes for this large age offset between the DFO-2006 and AICC2012 chronologies are suggested to come from an overestimation of the surface mass bal- ance in the glaciological approach and/or an error in one of the $\delta O_2/N_2$ age constraint by 3ka. In this study, as the transfer from one chronology to the other (either DFO-2006 on AICC2012 or the other way around) do not improve significantly the correlation between the $\delta O_2/N_2$ records of EDC and Dome F (Appendix C), we suggest that this behaviour over the glacial inception results from different relationships between $\delta O_2/N_2$ and the water stable isotopes at these two sites. »

We do not think it is necessary to correlate the d18Oatm curves as we can use them for the validation of the synchronisation method on the ice phase. As we do not observe any difference in timing for the d18Oatm records when EDC is tuned on DFO-2006 or Dome F is tuned on AICC2012, this indicates that either the lag in the ice isotopic composition records is real and independent of chronology construction (i.e. glaciological models), or there are also inconsistences for the delta-age estimations for both ice cores in addition to the overestimation of the surface mass balance. Further studies are needed on this aspect and this should be comprehensively studied for the next common chronology that will integrate the Dome F core as well.

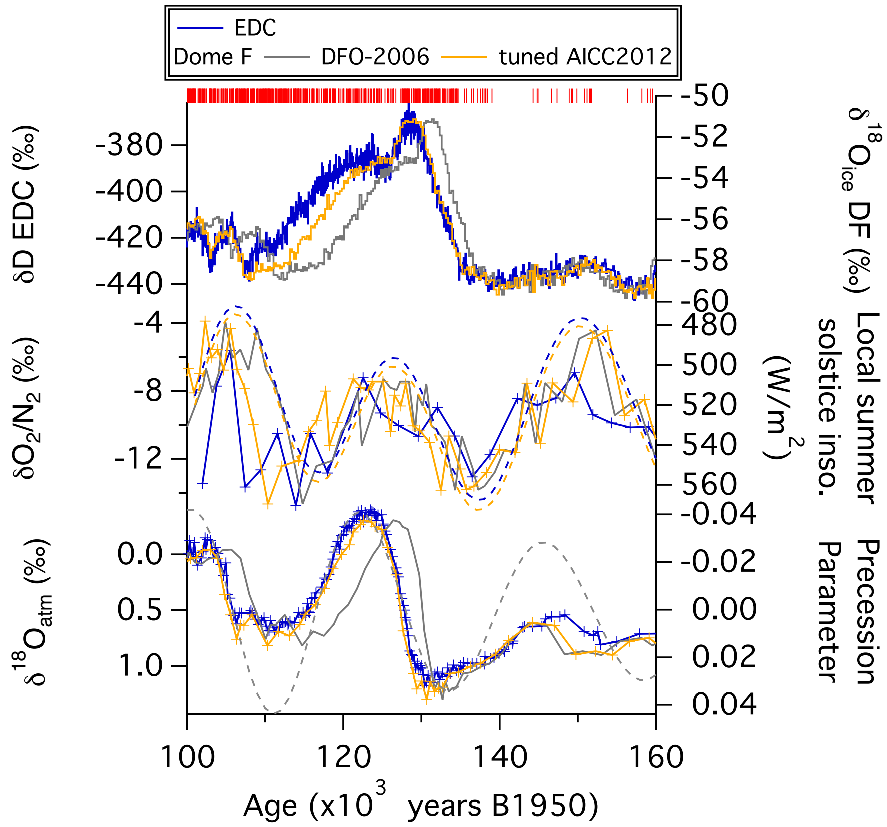

Figure 1: Transfer of Dome F records on AICC2012. The volcanic age markers used for the tuning are in red on top of the figure. EDC records are presented in blue. Dome F records are presented in grey on the DFO-2006 chronology and yellow when transferred on AICC2012.

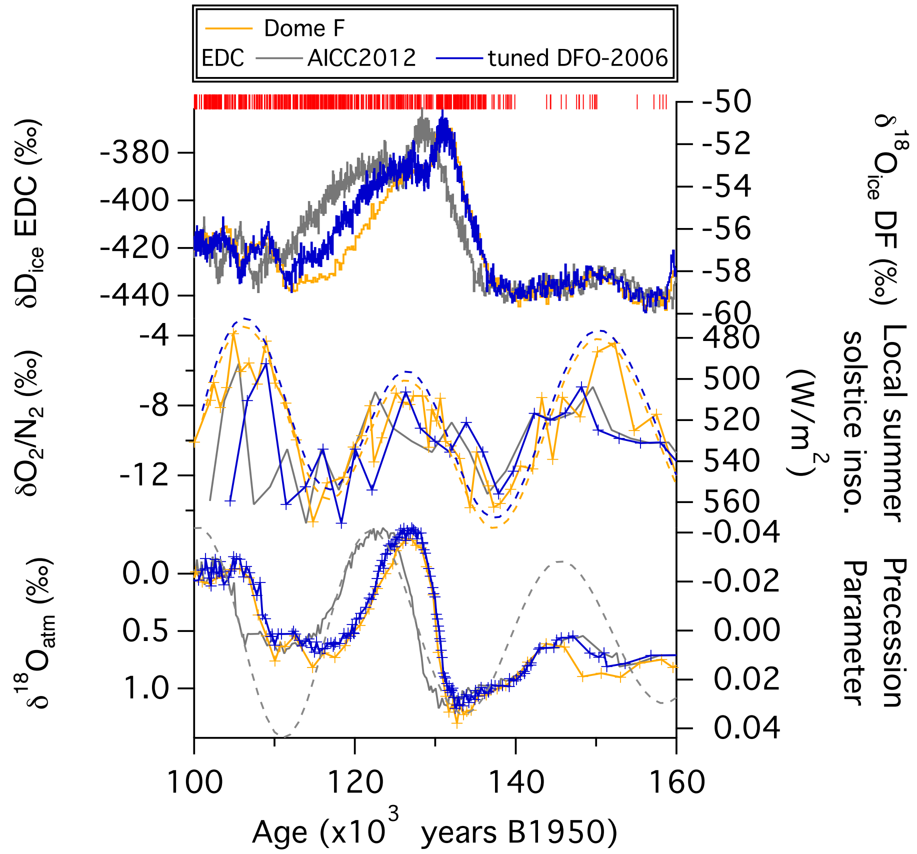

Figure 2: Transfer of EDC records on DFO-2006. The volcanic age markers used for the tuning are in red on top of the figure. EDC records are presented in grey on the AICC2012 chronology and in blue when transferred on DFO-2006. Dome F records are presented in yellow on the DFO-2006 chronology.

Regardless of the timescale issues, the minima seem very difficult to determine accurately. Dome Fuji has what looks to be its lowest value nearly 4 ka after the chosen value (Figure 3 arrows). Also, the sample spacing appears to be a couple thousand of years, so can anything really be said about the relative timing of the minima? I would like to see a statistical analysis used to define the minima and its uncertainty. My guess is that the uncertainty in the timing of the minima would be greater than the difference between sites. From what's presented, I feel like any discrepancies in the dN2/O2 relationship are most likely due solely to noisy data series.

Following this comment, we have performed a statistical analysis in order to define the minima, mid-slope and maxima in the dO2/N2 records of EDC, Vostok and Dome F as well as estimate their uncertainty around MIS5. We have used the dO2/N2 records of EDC and Vostok on the AICC2012 chronology and the dO2/N2 record of Dome F on the DFO-2006 chronology. For each sites, we have treated the data accordingly:

- smoothing using a 3-points running mean,
- re-interpolation between 101-160 ka with an even sampling corresponding to the mean resolution of the record (2.37 ka, 1.87 ka and 1.69 ka for EDC, Vostok and Dome F respectively,
- filtering of the re-interpolated data using a piecewise linear shape with a slope bandwidth of 1^-9 and between 15-100 ka.

Then, we have determined the minima, mid-slope and maxima in the (1) raw data, (2) smoothed, (3) re-interpolated and (4) filtered records for the three sites. Using these 4 age estimates for each site, we were able to determine the mean age and standard deviation for each minimum, mid-slope and maximum for the three sites (Table 1). The final uncertainty associated with the identification of the extrema and mid-slopes of the dO2/N2 records has been obtained after considering also the resolution of the records and the uncertainty of their respective chronologies (AICC2012 for EDC and Vostok, DFO-2006 for Dome F, Table 1). The results are illustrated on Figure 3 where the pink lines and shaded zones correspond to the mean age and uncertainty of minima and maxima of dO2/N2 for EDC (top), Vostok (middle) and Dome F (bottom). The grey bars indicate the position of minima and maxima in the local summer solstice insolation for comparison.

As the reviewer said, it is not possible to significantly discuss the difference in timing for the extrema of dO2/N2 records between these sites. However, it permits us to justify the uncertainty value that should be considered for this orbital tuning method for longer records. This statistical analysis of the dO2/N2 records over MIS 5 gives us support for the use of an uncertainty of 3-4 ka to be associated with the orbital tuning of dO2/N2 with local summer solstice insolation (Table 1). This kind of study should be systematically made for the determination of dO2/N2 age markers and estimation of their uncertainty in the future. We have integrated the method in Appendix B of the new manuscript.

Table 1 : Mean age and uncertainty estimates for minima, mid-slopes and maxima in the dO2/N2 records of EDC, Vostok and Dome F between 101-160 ka. The error takes into account the standard deviation of the identification of extrema and mid-slopes of the records, the resolution of the records and the uncertainty of their original chronologies.

		max 1	mid	min 1	mid	max 2	mid	min 2	mid	max 3
EDC	mean(ka)	104.7	107.7	114	119	123.8	131.8	136	141.3	148.3
	error(ka)	3.0	3.0	3.0	2.9	3.0	3.2	3.4	3.7	4.1
Vostok	mean(ka)	104.8	110.8	115	119	126.3	133.3	137	144.5	152.5
	error(ka)	2.7	2.5	2.7	2.7	2.8	2.8	3.2	3.4	3.6
Dome F	mean(ka)	106.5	112	115.8	120.3	126.5	132	138	144.5	151
	error(ka)	2.8	2.7	2.7	2.7	3.0	2.9	2.9	4.4	4.4

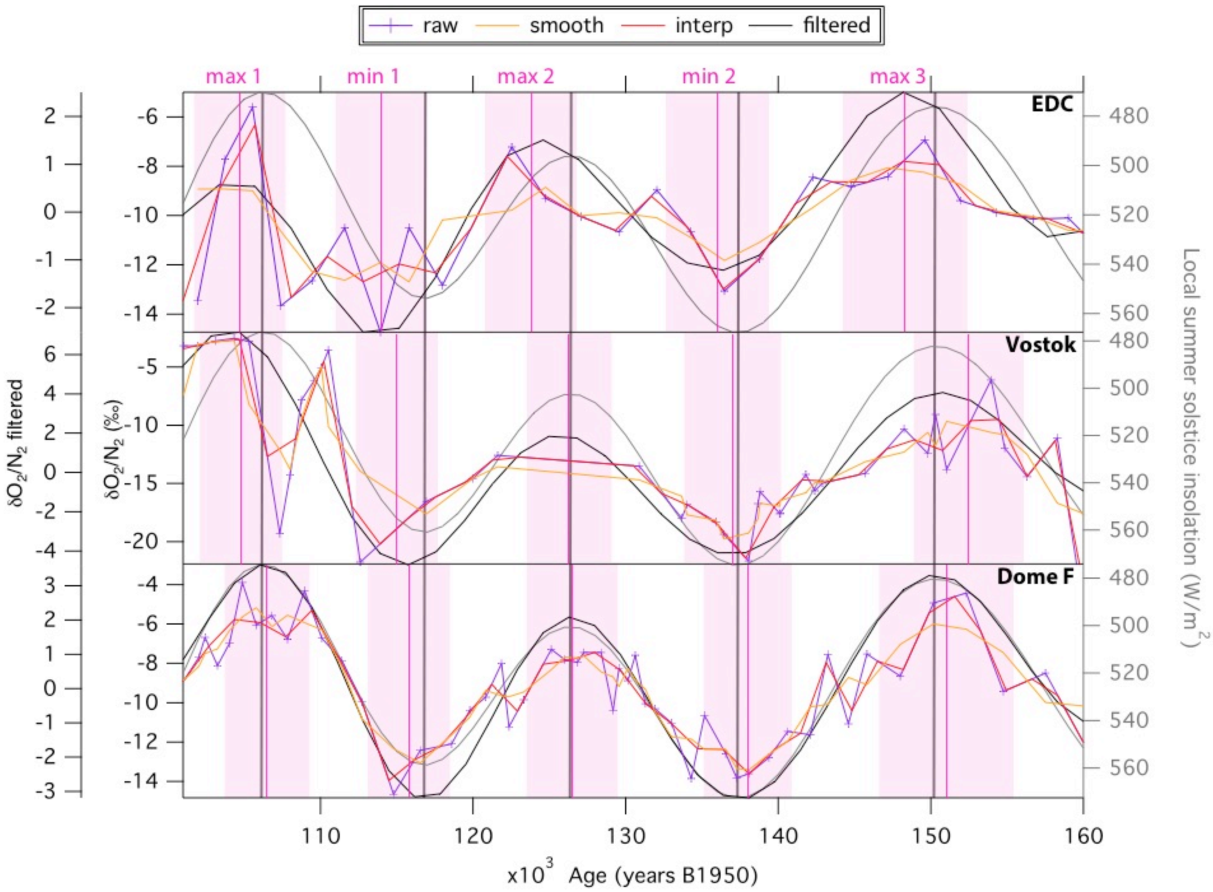

Figure 3: Determination of the minima and maxima of dO2/N2 records between 101-160 ka for EDC (top), Vostok (middle) and Dome F (bottom). For each panel the raw dO2N2 data are in purple, the 3-points running mean correspond to the orange curves, the re-interpolated data are in red and the filtered data are in black. The local summer solstice insolation of each sites are represented by the grey curves on an inverse scale. The grey bars highlight the position of minima and maxima in the insolation curves. The pink bars and shaded area represent the estimation of the age of minima/maxima in the dO2/N2 curves with their uncertainty estimates. The EDC and Vostok data are presented on the AICC2012 chronology. The Dome F data are presented on the DFO-2006 chronology.

The discussion of three possible explanations for site-dependent differences seems tangential. Or at least it came as no surprise that none of the three things investigated yielded better results. Since there are no physical models relating grain metamorphism at the surface to dO2/N2 values trapped thousands of years later at bubble close off, is investigating the timing of maximum temperature with 3-years of data set really even worth attempting? I much rather read a discussion of how the gas trapping at depth many thousands of years after the snow was deposited on the surface affects the expected relationship between dO2/N2 and insolation.

The spectral analysis of the long dO2/N2 record of EDC (340-800 ka) possesses a significant peak corresponding to a 100 ka periodicity. This characteristic suggests that the snow accumulation have an influence on the trapping process, as suggested by Hütterli et al., 2010. In the model proposed by Fujita et al., 2009, explaining the link between dO2/N2 and local insolation, the accumulation rate tends to complicate the simple link via (i) the decrease of layer surface exposure time to strong insolation, (ii) the increase of non-diffusive zone, (iii) the modification of the surface temperature gradient that also has a direct effect on snow metamorphism. Moreover, Suwa et Bender 2008 have shown that the link between dO2/N2 and local insolation is not as straightforward for the GISP 2 core, which is characterized by a high accumulation rate, contrary to sites with low accumulation rates such as Vostok. The dO2/N2 data at GISP2 and air content data at NEEM show a direct answer to DO event on a gas age scale hence demonstrating the direct influence of accumulation rate on these two parameters (Suwa and Bender, 2008; Edwards et al., Millennial Scale Climate and Total Air Content in the NEEM, GISP2 and WAIS Divide Ice cores, PIRE conference, Grenoble, September 2015).

Such a discussion has been added in the new version of the article. This is better introduced in page 3:

« Fujita et al. (2009) proposed a model to explain both total air content (effusion effect) and $\delta O_2/N_2$ (permeation effect) variations. This model is based on the different densification rates of layers affected by strong surface metamorphism and layers affected by low surface metamorphism. It is known that the snow metamorphism near the surface is the most rapid and strongest owing to the higher temperature (in summer) and high temperature gradient (Libois et al., 2014). Thus even if the residence time of the snow in the near-surface layer (e.g. 10 cm depth) is very small compared to the time required to reach the close–off depth, the metamorphism occurring during this short period results in major micro-structural changes in the snow. The near-surface metamorphism can be at least partially preserved down to the close-off depth. It is therefore expected that all factors integrated in the surface snow energy budget (air temperature, snow albedo, solar radiation penetration depth), controlling the temperature profil in snow, have an impact on snow metamorphism (Picard et al., 2012). Moreover, strong modifications of layering and microstructure are also observed at several tenths of meters be- low the surface (Hörhold et al., 2012). It is therefore expected that pore structure at close-off is also affected by changes in dust load (Freitag et al., 2013). Finally, the direct effect of accumulation rates cannot be neglected in these processes (Hutterli et al., 2010). Accumulation rate will indeed have a direct influence on the permeation mechanism proposed by Fujita et al. (2009) through the increase of the pressure difference between open and closed bubbles near the close-off and the increase of the depth of the non-diffusive zone at the bottom of the firn (Witrant et al., 2012). The direct link classically assumed between summer solstice insolation and $\delta O_2/N_2$ variations is therefore complicated by these different influences. Suwa and Bender (2008a) have observed a very different $\delta O_2/N_2$ vs summer solstice insolation relationship for the high accumulation rate site of GISP2 in Greenland compared to the low accumulation rate sites of the East Antarctic plateau. »

Orbital Tuning Uncertainties The final sentence of section 3.1 recommends an uncertainty of 3-4 ka for O2/N2. This recommendation seems to come from nowhere and is not quantified earlier in the section. Is this number (or range) from just the MIS 5 comparison at ~135 ka? The value of the uncertainty needs to be supported.

This estimation of the uncertainty is now supported by the previous discussion over the MIS5 period. This is now added in the paper as Appendix B.

Heinrich Events – invoking the presence or absence of Heinrich events in explaining the δ18Oatm lag seems unnecessarily speculative. What is the mechanism for Heinrich events affecting δ18Oatm? I think the argument the authors are making is that Heinrich events are markers of large fresh water input into the North Atlantic which suppress warming in the Northern Hemisphere. This keeps the ITCZ and southern hemisphere wind belt farther south, leading to small monsoons and less tropical vegetation. These combined effects delay the change in d18Oatm, leading to larger lags behind insolation. Regardless of what the mechanism is, it needs to be fully and succinctly stated. It is also worth noting that the Heinrich events aren't a causal part of this system – it is the fresh water input into the North Atlantic that is critical (unless the authors are further arguing for the placement of freshwater into the North Atlantic being critical, in which case they are getting even farther afield from the focus of this paper).

Our discussion on the link between d18Oatm variations and Heinrich events is based on the observation by Severinghaus et al. (2009) of a systematic increase of d18Oatm during Heinrich events over the last glacial period, these events being imprinted both in the calcite d18O and ice core d18Oatm. 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 d18Oatm induced by a freshwater input. These calculations show that the increase of d18Oatm during a Heinrich event is induced by a southward shift of the ITCZ associated with the freshwater input that leads to an increase of the d18O of the low latitude meteoric water in the northern hemisphere. This signal is then transmitted to the d18O of O2 through photosynthesis of the important terrestrial biosphere in the low latitude Northern Hemisphere during the last glacial period. The occurrence of freshwater input can thus delay the change in d18Oatm induced by the sole insolation. This mechanism would satisfactorily explain lags of d18Oatm behind insolation when Heinrich events are observed.

We have added such a discussion in the new text page 13:

"Severinghaus et al. (2009) have observed a systematic increase of $\delta^{18}O_{atm}$ during Heinrich events over the last glacial period, these events being imprinted both in the cal- cite $\delta^{18}O$ and ice core $\delta^{18}O_{atm}$. 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 $\delta^{18}O_{atm}$ induced by a freshwater input. These calculations show that the increase of $\delta^{18}O_{atm}$ during a Heinrich event is induced by a southward shift of the ITCZ associated with the freshwater input that leads to an increase of the $\delta^{18}O$ of the low latitude meteoric water in the northern hemisphere. This signal is then transmitted to the $\delta^{18}O$ of O_2 through photosynthesis of the important terrestrial biosphere in the low latitude Northern Hemisphere during the last glacial period. The occurrence of freshwater input can thus delay the change in $\delta^{18}O_{atm}$ induced by the sole insolation. This mechanism would satisfactorily explain lags of $\delta^{18}O_{atm}$ behind insolation when Heinrich events are observed. »

General Language Reading this paper was quite frustrating due to the imprecise writing. One of the most common problems is ambiguous subjects. Many sentences begin with "This" or "Such processes" and follow complicated sentences such that the reader does not know what part of the previous sentence is being referred to. One example from the conclusion: "This should motivates(sic) further study to unveil the processes at play both for long term trends and at glacial-interglacial/eccentricity timescales." I don't know what "This" refers to. Is it "spectral analysis", the subject of the previous sentence. Is it the "peak in the periodicity band"? Is it the "the processes other than local insolation"? I also don't know what further study "This" would motivate. Do you mean firnification? Measurements of better conserved ice samples? Hydrology changes during glacial-interglacial cycles?

We have now fully rewritten the manuscript and ask for an English native to correct us. We are very sorry for this.

I have copied an annotated copy of my comments since trying to put them in digital form quickly became both confusing and time-consuming. I have asked the editor to pass this along privately. I hope the comments will help the authors identify points of confusion and frustration for a reader.

Answer to reviewer 2

Bazin et al. present new d18Oatm and dO2/N2 data from the EPICA Dome C ice core, which were measured on ice samples stored and transported at -50oC to prevent gas loss. This procedural step is particularly important for the integrity of the O2/N2 signal, and to a lesser extent the d18Oatm signal. These new data have the potential to improve orbitally-tuned ice core chronologies by providing additional age constraints. In this manuscript the authors aim to better understand the phasing relationship of d18Oatm and dO2/N2 relative to orbital variations. The authors focus on two specific cases:

The first case is a comparison of the dO2/N2 minimum around 137 ka between the Vostok, Dome F and Dome C ice cores. The authors argue for a ~2 ka delay of the O2/N2 minimum at EDC relative to Vostok/Dome F. This conclusion seems untenable in the face of the scatter inherent to O2/N2 data, a data resolution of around 2-3 ka, and the obvious chronological errors exposed by the misalignment of the water isotopes. The discussion of different confounding influences on the O2/N2-orbital relationship remains inconclusive.

The second case is an analysis of the timing of d18Oatm relative to O2/N2 in the Vostok and EDC cores. No details are provided on the analytical methods of establishing the lag, nor on the uncertainty in the result. Based on wiggle-matching the authors argue that the lag of d18Oatm behind O2/N2 (or behind insolation, this is unclear) increases as a result of Heinrich events, but this is not obvious to me. Also, no dynamical pathway is provided.

Unfortunately, the overall result is that after reading a relatively long and dense paper, the reader is not much wiser as to what controls dO2/N2 and d18Oatm on these timescales, or how robust the timing relations are that the authors derive. The new data presented by Bazin et al. are obviously of great value. However, for this paper to be acceptable, I believe the analyses will need to be done in a more robust way that incorporates realistic uncertainty estimates.

Comments:

Please label subpanels (A,B,C etc) in the graphs. When referring to "Fig. 4" the reader is not sure which of the ~20 curves to look at.

Following your advice we have now labelled each panel of figure 4.

Page 1445/Fig. 2: Isn't the AICC2012 EDC chronology in this time interval largely based on the assumption that d18Oatm follows insolation? In that case discussing the power spectrum is not meaningful, given that the orbital frequencies are included by design. Has the d18Oatm data been corrected for mean ocean d18O? Ocean d18O has a lot of power in the 100ka band.

You are right the AICC2012 chronology is partly constrained by d18Oatm-precession age markers. Consequently, it is normal to find the frequencies of precession in the d18Oatm spectrum.

We did not correct for the mean d18O of ocean. If we correct the d18Oatm from the mean ocean d18O, the precession imprint is stronger and the obliquity imprint is removed (Landais et al., 2010)

Note that the d18Oatm power spectrum we obtain is nearly the same as on the one presented in Dreyfus et al., 2007 and also coherent with the d18Oatm power spectrum obtained on the Vostok ice core (Suwa et Bender 2008b).

Page 1446/Fig 2: The 100ka signal in the O2/N2 spectrum is a very nice observation. Wouldn't this argue for an influence of climate on O2/N2, for example through accumulation, dust or temperature?

This is one of the main interpretations proposed in this paper. We have rewritten the text in order to make it clearer in page 7:

« However, neither the modulation in amplitude nor the 100 ka signal are related to local summer insolation, pointing to other local parameters affecting the snow metamorphism and firnification processes. Potential candidates that may imprint on $\delta O_2/N_2$ with a 100 ka period would be changes in temperature, accumulation rate, firn dust content or component of the surface energy budget. »

The 100 ka periodicity suggests indeed that the dO2/N2 may be influenced by climatic parameters such as accumulation rate, dust or temperature. We discuss these potential influences later in the text. However, we do not observe any direct link between these climatic parameters and dO2/N2. Moreover, missing data (Dome F dust record, accumulation reconstruction on DFO-2006) and the lack of a common chronological framework between Dome F and EDC/Vostok prevent us from a clear conclusion about climatic parameters influencing dO2/N2.

We have reorder the different paragraphs on the potential influence on dO2/N2 in pages 10-11 in order to make it clearer.

Section 3.1 / Fig3: As I mentioned earlier, the conclusion that the EDC O2/N2 minimum lags by 2ka is not tenable. This analysis is done by assigning a single datapoint as the minumum, which is probably the least reliable way to do so for noisy, low-resolution records such as these. A more reliable way to assess the timing may be to perform a cross-correlation between the records, or apply filtering to the records. Any analysis regarding the timing of the O2/N2 minimum should at the very least consider the following:

- There is quite a lot of scatter inherent to O2/N2 data, both in your record and the VK/DF data. Note that this is no reflection on the quality of your data, but just a general problem with O2/N2 data. The scatter is clearly much larger than the pooled SD of replicate analyses. Due to the low resolution it is not clear whether this variability represents noise or a real ice core signal.

- data resolution; I don't think you can identify a 2ka lag in a record with 2.4ka average resolution.

- uncertainty in ice age and Delta-age; this is clearly larger than 2ka, considering the alignment of the water isotopes.

- to avoid circular reasoning the chronologies must be completely free of O2/N2 age constraints.

We have now performed a more statistically robust study of MIS5 dO2/N2 data for the three sites. We now have estimates of the extrema and mid-slopes ages of dO2/N2 with their corresponding uncertainties (see Figure 3 in the answer to the reviewer 1 and Appendix B).

We now propose an alignment of EDC and Dome F records using the volcanic match points of Fujita et al., 2015. Note that we still observe the same differences in timing for the last glacial inception as previously (see Figures 1 and 2 in the answer to the reviewer1). This is now added in Appendix C.

Over this period the AICC2012 chronology is completely free of the dO2/N2 record of EDC, as the data were not available when AICC2012 has been built. MIS 5 is mostly constrained through the numerous stratigraphic links between the Antarctic cores as well as 5 Vostok orbital markers (110.6 ka, 121.9 ka and 133.5 ka with 6 ka uncertainty based on d18Oatm, and at 121.8 ka and 132.3 ka with 4 ka uncertainty based on dO2/N2) and 1 air content marker for EDC (101 ka +/- 4 ka).

I am afraid that much longer and/or higher resolution dO2/N2 records are needed to address this question satisfactorily. At the very least the authors should provide a realistic uncertainty estimate on the phasing – my sense is that this uncertainty will be much larger than 2ka.

Compared to the records proposed in the CPD paper, we have now completed the data. The new dO2/N2 is now continuous between 340-800 ka with a mean sampling resolution of 2.07 ka. We have now performed a better estimation of the lag and its uncertainty, using different methods (delay calculation using Matlab and manual identification of extrema in the d18Oatm and dO2/N2 records). This is explained in more details in the answer to the comments of reviewer 1. We have modified the text in order to better explain why we should consider an uncertainty of 3-4 ka when using the dO2/N2 as orbital dating tool in pages 7-8:

« The identification of the $\delta O_2/N_2$ extrema and mid-slopes within the three records indicates that the $\delta O_2/N_2$ variations can be considered synchronous, within the calculated uncertainty, for the three sites over this period (Appendix B). This method of identification, taking into account the scattering of the data, the resolution and the chronology uncertainty, gives an error of 3-4 ka for this orbital tuning method for EDC, Vostok and Dome F (Appendix B). »

Page 1449- 1451: The discussion of confounding influences on the link between O2/N2 and insolation is important. Personally I think the observation of power in the 100ka band is a stronger motivation than the putative 2ka lag. For all four lines of argument the authors don't provide a clear mechanistic link to O2/N2 fractionation in the deep firn. Bender (2002), Severinghaus and Battle (2006) and Fujita (2009) provide such frameworks for understanding

O2/N2 fractionation in relation to firn processes, and these mechanisms could be briefly addressed.

We have added such a discussion in the article (pages 3-4 in the new manuscript). The corrections are added in green:

"Two other ice core parameters have been used for orbital tuning, but with a completely different underlying mechanism. The air content and $\delta O_2/N_2$ measured in the air trapped in ice cores are controlled by the enclosure process near the close-off depth (depth of closure of ice interstices and formation of air bubbles). At this depth, a depletion of the ratio O_2/N_2 compared to the atmospheric ratio is observed and attributed to the smaller size of O_2 molecules compared to N_2 ones (Battle et al., 1996, Severinghaus and Battle, 2006, Huber et al., 2006). It is expected that the entrapment process and the associated O_2 effusion or permeation effects are linked to the physical properties of snow at that depth. Because snow metamorphism is very strong at the surface of the ice sheet in summer, snow physical properties are expected to be driven by local summer insolation. Records of $\delta O_2/N_2$ and air content measured at Vostok, Dome F and EDC indeed depict variability at orbital frequencies, which appears in phase with local summer insolation (Bender et al., 2002, Kawamura et al. 2007, Landais et al., 2012, Raynaud et al., 2007, Lipenkov et al., 2011)."

"Contrary to $\delta^{18}O_{atm}$, $\delta O_2/N_2$ and air content are not influenced by remote climatic-driven signals such as low latitude hydrological cycle or northern hemisphere land ice volume. Fujita et al. (2009) proposed a model to explain both total air content (effusion effect) and δO2/N2 (permeation effect) variations. This model is based on the different densification rates of layers affected by strong surface metamorphism and layers affected by low surface metamorphism. It is known that the snow metamorphism near the surface is the most rapid and strongest owing to the higher temperature (in summer) and high temperature gradient (Libois et al., 2014). Thus even if the residence time of the snow in the near-surface layer (e.g. 10 cm depth) is very small compared to the time required to reach the close–off depth, the metamorphism occurring during this short period results in major micro-structural changes in the snow. The near-surface metamorphism can be at least partially preserved down to the close-off depth. It is therefore expected that all factors integrated in the surface snow energy budget (air temperature, snow albedo, solar radiation penetration depth), controlling the temperature profil in snow, have an impact on snow metamorphism (Picard et al., 2012). Moreover, strong modifications of layering and microstructure are also observed at several tenths of meters below the surface (Horhold et al., 2012). It is therefore expected that pore structure at close-off is also affected by changes in dust load (Freitag et al., 2013). Finally, the direct effect of accumulation rates cannot be neglected in these processes (Hütterli et al., 2010). Accumulation rate will indeed have a direct influence on the permeation mechanism proposed by Fujita et al. [2009] through the increase of the pressure difference between open and closed bubbles near the close-off and the increase of the depth of the non-diffusive zone at the bottom of the firn which is directly related to surface accumulation rate [Witrant et al., 2012]. The direct link classically assumed between summer solstice insolation and $\delta O_2/N_2$ variations is therefore complicated by these different influences and Suwa and Bender (2008) have observed a very different relationship between summer solstice insolation and dO2/N2 in the high accumulation rate site of GISP2 in Greenland than in the low accumulation rate sites of the East Antarctic plateau."

Page 1449: I don't see why a 2 week lag of maximum temperature behind maximum insolation would influence the orbital phasing. Isn't this delay mostly due to thermal inertia? I think one could reasonably argue that summer temperature scales with summer insolation, regardless of such a small time delay.

It is not obvious to scale the summer temperature with insolation at the summer solstice. Indeed, as the maximum temperature occurs around the 15th of January at EDC, we may suggest that it is better linked to the insolation of the 15th of January. The curve of summer solstice insolation and 15th of January insolation show maxima and minima that can be shifted by up to 2 ka for EDC (Figure 4 left). We wanted to explore the influence of such a phasing on our conclusions. When considering the 15th of January and 30th of December insolation, corresponding to the date of maximum temperature observed at EDC and Vostok respectively, the dO2/N2 orbital tuning tend to give younger ages than when using the summer solstice insolation as target (figure 4). With such insolation curves as target, the disagreement increases between the insolation-dated dO2/N2 records of EDC/Vostok and Dome F.

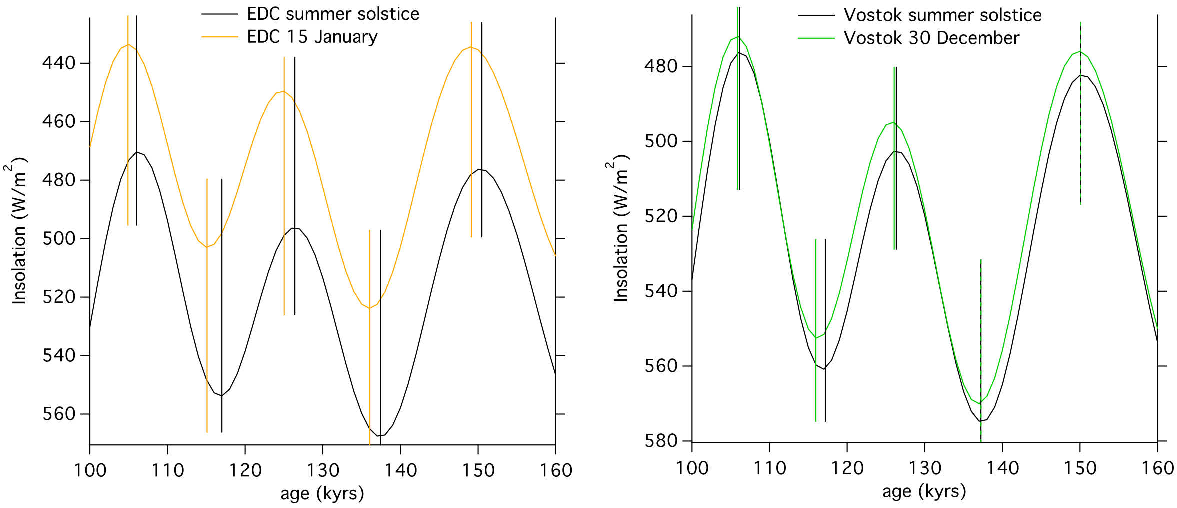

Figure 4 : comparison between summer insolation at solstice (black) and at maximum temperature for EDC (yellow, left) and Vostok (green, right). The vertical colored lines help to identify the position of minima and maxima in the corresponding insolation curves.

Page 1450-1451: regarding the accumulation, do you investigate the relationship by comparing O2/N2 and Acc on an ice age chronology? Recently Takuro Kobashi argued that accumulation can influence O2/N2 via overburden pressure (doi: 10.5194/acpd- 15-15711-2015), in which case you'd have to look at the accumulation during a period after deposition – the duration of this period would be roughly equal to the time of burial (i.e. Delta-age), which is different at each site.

The study of Kobashi et al., 2015 focus on the dAr/N2 over the Holocene for Greenland ice cores. They observed a significant correlation between their dAr/N2 records on the gas age scale with the accumulation rate. Following their observation, we have calculated the correlation between our dO2/N2 record and the accumulation rate corresponding at the age corrected of the delta-age (equivalent to the gas age). We have obtained a correlation of 0.1335. This does not mean that there is no link between dO2/N2 and accumulation, but it just suggests that this link is more complex. In particular, the relationship between dO2/N2 and accumulation rate appears to be more complicated for low accumulation rates sites at orbital timescale than in Greenland over the Holocene. Further studies are needed in order to better understand the impact of accumulation rate on dO2/N2. We have added the following paragraph in page 9 of the new manuscript:

« Kobashi et al. (2015) observe a significant correlation between the $\delta Ar/N_2$ on the gas age and the accumulation rate for Greenland ice cores over the Holocene. Following their observation, we have calculated the cor- relation between our $\delta O_2/N_2$ record and the corresponding accumulation rate at the age corrected of the delta–age (equivalent to the gas age). No significant correlation is identified (R=0.134 between 340–800 ka). The absence of significant correlation between the $\delta O_2/N_2$ record and accumulation rate probably reflects a non straightforward relationship between these two quantities. In particular, the relationship between $\delta O_2/N_2$ and accumulation rate appears to be more complicated for low accumulation rate sites at orbital timescale than in Greenland over the Holocene. »

Section 3.2: Also here the authors should provide much more detail on their methods, and assess the robustness of their result in a meaningful way. How were the records filtered? How did you determine the lag? - this is not explained at all. The elephant in the room is of course the Delta-age (which is not meaningfully investigated), but data scatter and resolution probably influence this result also.

The different methods are now fully explained and detailed in the new manuscript. The filtering methods used were always performed using Analyseries software. The data are re-interpolated evenly, in respect of their mean resolution, and filtered using a piecewise linear shape with a slope bandwidth of 10^{-9} a^{-1} and between 15-100 ka. Lag values are obtained using wavelet transform after filtering and cross-correlation using Matlab. They are also confirmed by extrema determination using the same method as in Appendix B. The two delay calculations are in good general agreement, which gives confidence in their estimation. Now the lag values are associated with an uncertainty that takes into account the noise, timescale uncertainty and resolution. More details are given in the answer to the comments of reviewer 1 and Appendix B of the new manuscript.

Please define clearly what you mean by the d18Oatm – O2/N2 phasing. Are you (1) determining the relative phasing of maxima/minima in both records directly, or (2) are you evaluating the phasing of d18Oatm relative to orbital forcing? If (1): why would you expect these to be in-phase in the first place, given that one is a local, and the other a global signal? If (2): What orbital forcing do you expect d18Oatm to follow? Throughout the paper it seems that the authors expect a direct link with precession, but why not use 30oN insolation, for example.

The delay is calculated from the cross-correlation of the records. The aim of calculating the delay between the d18Oatm and dO2/N2 records is to study the response of d18Oatm to orbital forcing. As the AICC2012 chronology is partly constrained by d18Oatm orbital tuning, it is not meaningful to discuss directly the delay between d18Oatm to precession. Consequently, because dO2/N2 can be assumed synchronous with local summer solstice insolation in the first order, and as precession and insolation are in phase (+/- 500 years), we can directly study the delay between d18Oatm and dO2/N2, then free of chronology bias, and interpret it as the delay between d18Oatm and precession. We have rephrased the text to make it clearer page 12:

« We re- interpolate the data according to the largest sampling resolution between the $\delta O_2/N_2$ and $\delta^{18}O_{atm}$ records of each sites (2.07 ka for EDC and 1.76 ka for Vostok). There is a close resemblance of the interpolated and original data. In order to calculate the relative offset between the two proxy records, we normalize the data (minus the mean, divided by the standard deviation) and filter them between 15–100 ka using wavelet transform. The filter is computed using Fourier transform and convolution products. The delay is deduced through the conversion of the phase calculated between the $\delta O_2/N_2$ and $\delta^{18}O_{atm}$ filtered records after cross-correlation. An independent estimation of the offset has been manually calculated from the identification of the timing of extrema in both records following the same methodology as in Appendix B. »

P1453, L18-20: why is there no O2/N2 signal in bubbly ice from the last 100ka? In the melt-refreeze you should get all the gas, right?

There are dO2/N2 records before 100 ka, however we cannot use them for orbital tuning because they present a too large variability. This is illustrated on Figure 5 with the data from Vostok of Suwa et Bender 2008b. We can observe positive values of dO2/N2 due to the partial dissolution of air in clathrate hydrates.

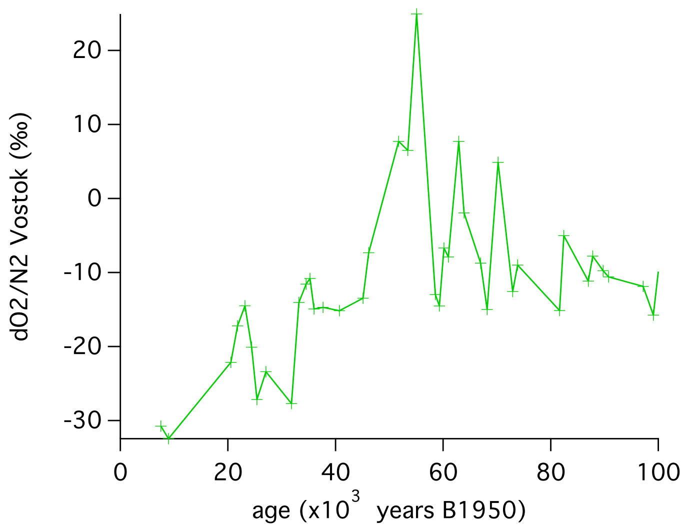

Figure 5 : dO2/N2 record of Vostok for the last 100 ka presented on the AICC2012 chronology (Suwa et Bender 2008b).

P1454, L18-22: You claim that O2/N2 is synchronous with local insolation, but in Fig 3 you just argued it is not. How does the O2/n2 lag influence your result?

We agree with you that this was not clearly explained in the paper. We have reworked the text in order to make it clearer for the reader.
The comparison of dO2/N2 records from EDC, Vostok and Dome F over MIS 5 is now written in order to justify a 3-4 ka uncertainty to be associated with the dO2/N2 orbital tuning method.
In the second part of the article we want to investigate the reasons for a varying delayed response of d18Oatm to precession variation, in order to reduce the uncertainty associated with this orbital tuning method (currently of 6 ka). To this end, we consider the dO2/N2 and insolation variations to be synchronous at first order. This hypothesis is relevant as insolation

can be considered as the main forcing parameter imprinting on dO2/N2 thought metamorphism of the surface snow. Based on this working assumption and focusing over periods where the dO2/N2 closely resemble insolation variations, we study the time difference between variations recorded in dO2/N2 and d18Oatm. As the insolation and precession present the same timing of variations, the delay between dO2/N2 and d18Oatm can be interpreted as reflecting the delayed response of d18Oatm to precession variations. This is now clearer in the new manuscript in page 12.

P1455: I am puzzled by the choice of IRD record. Why not simply use an IRD record, rather than the Ca/Sr records. The authors argue that Heinrich events must show up in BOTH records to be truly a Heinrich event. By this definition they miss many commonly recognized Heinrich events, such as e.g. H11 around termination 2, which is a very prominent event in most records, but not visible in core U1308.

We choose the Ca/Sr ratio as an IRD event because this proxy permits to highlight surging events with a Dolomite/Calcite main composition, characteristic of the Hudson strait Heinrich events (Channell et al., 2012). H11, as well as H3 and 6 are not really recorded in this proxy as they are not originating from the Hudson Strait, but mostly from the Fennoscantian/Greenland ice-sheets (Channell et al., 2012; Naafs et al., 2013).
We initially choose these records as they were the first ones proving the existence of Heinrich-like events before the last glacial period to our knowledge.
We now have reworked the text in order to make it clearer and have changed figure 4. We now combine both Ca/Sr and Si/Sr records in order to also consider large surges originating from the Greenland and Fennoscandian ice sheet. These changes can be seen in page 14 of the new manuscript.

« In order to study the possible link between variations of the $\delta O_2/N_2$–$\delta^{18}O_{atm}$ offset and the occurrence of Heinrich events, we confront our results with marine records from cores U1302/03 and U1308 located within the IRD belt of North Atlantic (Figure 4 E, F, G Hodell et al., 2008; Channell et al., 2012; Channell and Hodell, 2013). Sites U1302/03 and U1308 are located on the western and eastern borders of the IRD belt respectively. Heinrich events consist in large iceberg discharges of the Laurentide ice sheet through the Hudson Strait. These events are well recorded by spikes in the Ca/Sr ratio, which traces the abundance of carbonate grain in the sediment. On the contrary, IRD events corresponding to discharges of the Greenland and/or European ice sheets (Fennoscandian, British ice sheets mainly) are identified by large amounts of detrital quartz in the sediment, then characterized by peaks in the Si/Sr ratio. Consequently, thanks to their respective locations, the Ca/Sr record of core U1302/03 is a good proxy for the Hudson Strait iceberg events (Heinrich-like events), and the Si/Sr record of core U1308 is a good representative for the Greenland/European ice sheets destabilization events. The marine cores data on Figure 4 are presented on their original chronologies, constructed by tuning of their $\delta^{18}O$ to the LR04 benthic stack (Lisiecki and Raymo, 2005). 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 absolute timing of ice sheets discharge events with our ice core records. We thus only discuss the occurence of Heinrich- like events and Greenland/European ice sheets discharges in regards to the variation of the $\delta O_2/N_2$– $\delta^{18}O_{atm}$ offset. We can see that major spikes in Ca/Sr and Si/Sr recorded in the marine cores occur at roughly the same periods as the maximum

$\delta O_2/N_2$–$\delta^{18}O_{atm}$ offset values. The correspondance is especially well marked in the manually calculated offsets (red circles and arrows on Figure 4). The lag values over MIS 15 do not present the same variability in both offset estimates as previously noticed. In the marine records of iceberg discharge we only see small but regular peaks in the Ca/Sr record during this period. For Channell et al. (2012), these peaks do not reflect the occurence of Heinrich- like events but most probably correspond to debris flows or glacial-lake drainage events caused by changes in hydrological budget or changes in base level. Compared to our $\delta O_2/N_2$–$\delta^{18}O_{atm}$ offsets records, we suggest that the manually calculated delay may reflect these individual events while the Matlab delay may just integrate them all progressively due to the filtration of the data.Interpreting the chosen marine data as proxies of Laurentide and Greenland/European ice sheets discharges, we suggest that for Termination II, MIS 8 and MIS 16, the Heinrich-like and Green- land/European ice sheets discharge events delay the response of monsoons and thus $\delta^{18}O_{atm}$ with respect to precessional forcing. By contrast, when we detect the smallest offsets between $\delta O_2/N_2$ 490 and $\delta^{18}O_{atm}$ (Figure 4), no discharge events are observed within our marine core records. We there- fore explain the minimum lag between $\delta^{18}O_{atm}$ and precession during MIS 6–7, the end of MIS 9, the end of MIS14-start of MIS 15 and the end of MIS 17 by the combination of three factors: minimum effects of ice volume changes (due to intermediate ice sheet extent), strong impact of precession on monsoons (due to high eccentricity), and the absence of ice sheets discharge event. »

The link between H-events in core U1308 and the d18Oatm-O2/N2 delay seems completely arbitrary to me. The authors pick two maxima in the delay (marked by arrows), and argue that these coincide with increased Heinrich activity. Consider the following:

- Similar increases in the d18Oatm delay are observed around 150ka, 350 ka, 530 ka, without much increased Heinrich activity.

Around 150 ka this would correspond to Heinrich event 11. It was not hightlighted in the figure because this Heinrich event have mostly an origin from the Fennoscandian/Greenland ice sheets and not Hudson strait, as defined for a Heinrich event. The new figure with the Si/Sr record now prove the existence of the IRD events corresponding to a Fennoscandian or Greenland origin. At 350 ka, the large delay observed is most probably induced by border effect as the Vostok record end at 400 ka. Moreover, this time also coincide with a period of low excentricity that make the identification uncertain. At 530 ka, the delay was not discussed there because the dO2/N2 record do not present the same variability as insolation, then do not respect our working hypothesis. We have rewritten the text in order to make it clearer (page 12 -14).

- The prominent U1308 events around 240ka and 625ka occur during times of a small d18Oatm delay

- In both cases the d18Oatm delay starts to increase several ka BEFORE the H-events take place, making it dubious that the latter are the cause of the phasing delay.

For these two comments, we should keep in mind the dating method used to produce the marine chronologies. The chronologies of U1308 and U1302/03 are built by tuning of the d18Orecords to the LR04 benthic stack. This method results in large associated uncertainties (4ka for the last 1 Ma, Lisiecki and Raymo 2005). Consequently, it is difficult to precisely determine which Heinrich event should correspond to a larger delay in our record. We now have made this clearer in the new figure and in the text p 14.

We have now replaced the figure by a new one (Figure 4 in the new manuscript). The delay over periods were the dO2/N2 records do not present similar variations as local summer solstice insolation should not be considered in the discussion, because they would not respect our working hypothesis (synchronous variations in dO2/N2 and insolation).

We have fully rewritten the text of this section in order to make it clearer. These changes can be seen in page 14 of the new manuscript.

The authors also do not provide any mechanistic understanding to underpin their proposed Heinrich mechanism. Severinghaus (2009) show unambiguously that H-events strengthen the Dole effect, but the current study does not provide any additional insight.

See answer to the comments of reviewer 1 for a similar question. We have now added such a discussion in the new version of the article at the end of page 13.

P1456, L10: "The phase identified over T1 and T2 may not apply for earlier transitions without Heinrich events". However, according to your own preferred IRD record (Ca/Sr from U1308) there was no H-event during T2....

You are right, there is no record of H11 before T2 in the Ca/Sr ratio that we choose. The Heinrich event before T2 is mostly recorded by Si/Sr ratio as it is a large event associated with icebergs originating mostly from the fennoscandian/Greenland ice-sheet (Channell et al., 2012, Naafs et al., 2013). This has been made clearer in the text and figures. The Heinrich event 11 is now visible on the added Si/Sr record of U1308 in Figure 4 of the new manuscript.

The authors could elaborate on the potential of their data for refining ice core chronologies. Also, what are the possibilities for linking d18Oatm data to absolutely dated speleothem records?

This has been added in the text as potential further work

Typos, etc :

All typos have been corrected or are not relevant for the new version of the paper.