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The Cryosphere
Discussions

Interactive comment on "Archival of the water stable isotope signal in East Antarctic ice cores" *by* Mathieu Casado et al.

Mathieu Casado et al.

mathieu.casado@gmail.com

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Dear Editor,

Please find below our response to the different comments of the anonymous referees. We are grateful for the time they spent reviewing the paper and think the manuscript has been greatly improved thanks to their comments.

Please find attached as a pdf a coloured version highlighting the answers.

On the behalf of all the co-authors,

Mathieu Casado

âĂČ Anonymous Referee #1

It’s critical to study the formation processes of the ice core records for a sound interpretation, especially for those ice cores recovered from the low accumulation sites of the East Antarctica, where the longest ice core record is found. This paper presents observations on the isotopic composition of the vapor, the precipitation, the surface and the buried snow samples collected from the inland of the East Antarctic Plateau, with purpose to understand the post-depositional processes affecting the variability of the isotopic composition in the buried snow. In general, the dataset is invaluable, especially from the brutal remote sites of the East Antarctica, and the study is quite comprehensive. We thank the anonymous referee for the time he has spent to review of our article. However, the dataset is too limited to reach robust conclusions. For instance, the authors suggested enriched (depleted) heavy isotopes in the vapour (snow) during frost deposition events, but only a single frost event was sampled. There exists spatial variation of the stable isotopic composition, as also indicated by the authors, but it’s hard to qualify its effects on the temporal change of the stable isotopic composition of the surface and the buried snow. New precipitation events, and possible snow drifting as well, during the observation further complicate the situation. We agree with the referee and believe a large effort was made to clearly present the limits of the dataset presented here. If no precipitation event and snow drift were observed during this frost deposition, only one flat area in a relatively turbulent night was studied. Thus, we neglect the impact of the surface roughness due to sastrugi and of stratification. This has been precised in the manuscript (Page 24, line 12 to 15) “These results are preliminary as the study relies on only one event of attested frost deposition and the monitoring of more events is necessary to be able to quantitatively evaluate the fractionation processes involved. In particular, this study neglect the impact of the surface roughness as the study area was free of sastrugi and only evaluated the exchanges between the snow and the vapour for a summer, relatively turbulent night.” The authors identified a common 20 cm cycle. Given the significantly different accumulation and sampling resolution, the common 20 cm cycle may have a quite different meaning. For example, at S2 with accumulation of 6.0 cm snow equivalent and finest resolution of

3cm, the 20 cm cycle may suggest multi-year variation. To the contrary, at South Pole with accumulation of 19.7 cm snow equivalent and finest resolution of 1.1cm, the 20 cm cycle may do attribute to the seasonal variability of precipitation. There's a possibility that the common 20cm cycle is just an artificial identification by chance. Parts of the paper should be rearranged. For the S2 location, it is indeed possible that aliasing impacts our data preventing to see a seasonal cycle, as mentioned in the manuscript in section 3.5, at the page 19, line 6 to 10. If it is possible that other hypotheses explain the cycles, such as multi-year variations. Here, we believe that multi-year variations are unlikely to be the cause of the cycles because the peaks from snow pits taken the same year at the same site a few hundred meters apart show significant different in the location of maxima and minima. We included this discussion in the manuscript (Page 19, line 17): "Cycles of approximately 20 cm were also observed by Hoshina et al., (2014) at Dome F and its vicinity and were attributed to multi-year cycles. Here, when we compare several snow pits dug the same year at the same sites (for instance at Vostok and Kohnen as presented on Fig. 8 and observed on more profiles), we do not observe synchronous peaks in the profiles of isotopic compositions. This tends to indicate that non-climatic (post-deposition) processes are preponderant for the formation of these cycles as we expect that multi-year cycles in the climatic conditions and thus precipitation isotopic composition, would globally affect one site. Still, the mechanisms involved in the formation of this 20 cm cycles are not clearly identified and will be the topic of another independent study in preparation." For instance, it's very strange for Fig. 10, together with a short discussion on the distribution of isotopic composition, to be firstly appeared in Conclusions. The choice of placing the figure 10 in the conclusion has been done after a long writing process. Indeed, this figure required all the different datasets to be introduced and because we believe it is synthesising the results presented beforehand. If this choice is slightly unconventional, it is not unprecedented and seems to be justified in this case.

âĂČ Anonymous Referee #2

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Discussion paper

(General) This paper analyzes the interpretation of water stable isotope signals in the ice core from the seasonal variations of isotopic composition in the vapor, the precipitation, the surface snow and buried snow. It is a valuable study. These observations were carried out in the Antarctic inland region where almost none was observed. The publication of valuable data is very worthwhile. However, there are many qualitative discussions. Unfortunately, the quantitative conclusion has not reached yet. Each observation period and method are different. So the direct comparison can not be made, and it seems to have been summarized by qualitative discussion. We thank the anonymous referee for the time he has spent to review of our article. (comments) In PRE-REC observation, snow sampling on the plate 1 m above the ground is regarded as precipitation. About the sampling of this precipitation, it is conceivable that drift is mixed in, but how do you evaluate it? It is indeed conceivable. The plate is shielded by 8 cm high walls surrounding the plate to prevent as much as possible from blowing snow. This question was also raised by Stenni et al. 2016 (The Cryosphere). We do not have a solution yet about how to identify the impact of blowing snow in our precipitation product. A mention has been added to the manuscript (Page 6, line 7 yo 8): “As already mentioned by Stenni et al (2016), episodes of blowing snow might affect the precipitation gathered on the plate.” It is classified as surface snow on the plate of snow surface. Since water vapor does not come in and going out of buried snow, can you think the same as other surface snow observation? The samples obtained from the place in the project PRE-REC seem to behave more like the surface snow obtained in the NIVO and SUNITEDC projects than the precipitation, thus explaining the description as surface snow. It is possible that despite the isolation of the plate, the surrounding vapour at the surface is generated by the snow below through lateral transport. The meter scale transport of vapour at dome C goes beyond the framework of this article. There is no mention of the thickness of GLACIO's surface snow. A mention of the thickness of the GLACIO's surface snow was added (Page 6, line 2): In 2014/15, an additional sampling took place within the GLACIO project twice a day from December 2014 to January 2015 near the location of the inlet used for water vapour monitoring (See section 2.4 and Casado et al (2016) following the same protocol (15 mm thick samples gathered directly in the snow) In Figure 6, you can explain the change in isotopic composition of surface snow assuming that isotope fractionation of water vapor and surface frost grows by sublimation condensation. Although it is explained in the paper, evaluation of spatial variability, spatial and temporal variations of snow sampling and vapor is insufficient. In the figure 6, the spatial variability of the surface snow isotopic composition does not apply as the surface snow was sampled every hour both randomly over a field of 30m^2 and at a fixed location below the inlet. In this case, the spatial variability impact is thus described by the error bars generated by the measurement of duplicate. This was not possible for day-to-day sampling resulting in the difference of interpretation. A note has been added to the manuscript (Page 14 line 2 -5) : “The spatial variability of the surface snow isotopic composition was estimated by realising two sorts of duplicates: every hour, two samples were taken from a random location over a 30 m^2 field and one sample was taken at a fixed location. The error bar presented in Fig. 6 represents the noise on the surface snow due to the spatial variability.” It was also mentioned page 25 (line 11-12) in the discussion: “a single frost deposition event resulted in more than 2 \textperthousand \ in a 5-hour period which is significant compared to the standard deviation associated to the local spatial variability around 1.7\textperthousand .” There is no description of the broken line in Figure 9. It is described as black dots. The caption was slightly too small to see that they were dots. For the resubmission, it will be cared for. Table 5 shows the average value of precipitation over 4 years? Definition of summer and winter? It has been precised in the text. For the interpretation of isotopic fluctuations, see the paper by Hoshina et.al. Hoshina et.al., (2014): Effect of accumulation rate on water stable isotopes of near-surface snow in inland Antarctica, J. Geophys. Res. Atmos., 119,d oi:10.1002/2013JD020771. This paper has been included in the manuscript (Page 19, line 17). âĂČ Anonymous Referee #3

Summary

The manuscript of Casado et al. (tcd-2016-263) presents an analysis of the post depositional modification of the oxygen isotopic composition in East Antarctic snow using various types of isotope dataset including water vapor, precipitation, surface snow, and buried snow. The interesting point of this study is that documents seasonal and inter-annual variations in oxygen isotopic composition of precipitation and surface snow at the far inland of the Antarctic continent. The work shows that the seasonality of the surface snow does not correspond to that of the precipitation, but is linked to the seasonal change in grain index which is an indicator of coarsening of snow at surface. While more work needs to be done to quantitatively interpret the post-depositional effects, the work described here is pioneering and the implications will doubtless be fleshed out in future work. I list some minor comments in below. We thank the anonymous referee for the time he has spent to review of our article. Primary comments:

There is some disconnect between isotopic variability of surface snow and the post depositional process. The metamorphism of surface snow is not only factor controlling the isotopic variability of surface snow, but the precipitation amount also influences to the inter-annual variations of summer isotopic peaks because the depth of sampling layer is fixed. For example, Figure 4 shows that the summers with relatively higher oxygen isotopic peaks (2012 and 2014) are accompanied with relatively heavy snowfall events in summer. In contrast, the years with weak snowfall events are characterized by lower oxygen isotope peaks. These results suggest that inter-annual variations of summer oxygen isotopic values would reflect the relative contribution of summer precipitation to the samples. Hence, inter-annual variability of snowfall amount has to be considered before discussing the influence of metamorphism. This is a very important point which is difficult to assess. Indeed, the characterisation of the snowfall in Antarctica still is a problematic question for which we were not able to find relevant quantitative results and the best we could do here is to use the ERA-interim snowfall product which has been shown to underestimate the total accumulation. Also if there seems to exist a visual link with amount of summer precipitation by looking at figure 4., the comparison of the integrated amount of precipitation over the summer (testing different range of summer extents from December alone to DJFM) to the amplitude of the summer isotopic composition amplitude does not show any correlation (r^2 = 0.004 between the integrated amount of precipitation and the amplitude of the isotopic composition summer increase, n = 4). It is possibly due to the availability of only 4 summer maximum. We also tried this analysis using accumulation products from stakefarm which didn't produce any better correlation (r^2 = 0.001, n = 4). Still, Picard et al, (2012) shows that there is a link between the integrated amount of precipitation and the grain index, thus we would expect one between the summer surface snow isotopic amplitude and the precipitation amount. More years will be necessary to reach a solid conclusion, which justifies that the project NIVO continues in the future. A note has been added in the manuscript (page13, lines 4 to 9): "Summer variations of surface snow isotopic composition seem to be driven by large snowfall events as illustrated in Fig. 4. If large precipitation events seem to be a prerequisite to observe a large summer increase of surface snow isotopic composition (for instance as in 2012 and 2014), it is not necessarily sufficient as illustrated by the summer 2011. The comparison of the amplitude of summer surface snow isotopic composition increase with the integrated amount of precipitation is thus not so straightforward (not shown)." The conclusion that the sublimation/condensation cycle occurs in closed system at Dome C site seems to be exaggerated. Fig 6 shows that the increasing trend of oxygen isotopes in water vapor does not perfectly correspond to the decreasing trend of those in surface snow. When air temperature begins to rise at 18:00 UTC, the isotopic value of water vapor also starts to increase, but the decreasing trend does not. The vapour isotopic composition increase indeed starts before the decreasing trend of surface snow. As the ice integrates the deposition whereas the vapour does not, it is expected to see a delay in the modification of the snow isotopic composition compared to the vapour. This has been precised in the manuscript, first on page 16, line 9 to 11, : "We observe a small delay (2 to 3 hours) between the beginning of the vapour isotopic composition increase and the decrease of the surface snow isotopic composition, but considering the precision of the measurements, there is a strong uncertainty on the identification of this delay. " and on page 26, line 9 to 11,

: “Note that a delay is observed for the decrease of the surface snow isotopic composition compared to the vapour increase, this could be linked the integrative effect of the phase transition on the ice and not on the vapour. “ And, I have another question as for frost formation process. Generally, frost formation occurs when surface temperature becomes cooler than the atmosphere. Fig 6, however, shows that frost formation starts after the surface temperature rises above the 3-m temperature. How do you explain this contradiction? We agree with the referee that it is surprising that the frost deposition is observed after the minimum of temperature. If the relative humidity seems to be maximal over a large range of time, only for the last 4 hours of the very supersatured period do we observe on the video a frost deposition. One explanation could be that the rising temperature enhance the turbulence of the boundary layer, enable a more efficient mixing. It could be interesting to compare the vapour isotopic composition to the turbulence using the Richardson number for instance, it goes beyond the framework of this study. Another possibility is that the moisture does not necessarily originate from the atmospheric vapour, indeed, the maximum of temperature in an insulated firn is located a few cm below the surface (1 to 5), thus the moisture condensing at the surface could also be originating from the firn and not the atmosphere. This goes beyond the framework of this study, a note has been added to the manuscript about this, on Page 16, line 12: (note that the origin of the vapour can be either from the free atmosphere or from the interstitial area, we are not able to discriminate them here) A recurrent multi-year cycles, corresponding to 20cm cycles in this study, can be seen in the snow pit collected at Dome Fuji (see Fig 5 in Hoshina et al., 2014). They explained the multi-year cycles reflect the multi-year cycles of large precipitation events. Please discuss if this theory works for the 20cm cycles in this study. I think the authors seem to overestimate the role of post depositional effects. Hoshina, Y., et al. (2014) Effect of accumulation rate on water stable isotopes of near- surface snow in land Antarctica, 119, 274-283, doi:10.1002/2013JD020771. The multiyear cycles of large precipitation events described by Hoshina et al, 2014 are not supported by our pits. In particular, the comparison of different pits realised the same year at the same site, separated by few hundred meters, indicate that the stratigraphic noise and the post deposition impact on the profile is larger than the imprint of the multi-year cycles on the profile of snow isotopic composition. In order to be able to evaluate these cycles, stacking several profiles with an approach similar to Munch et al, 2016 (Climate of the past) would be necessary. Several mentions to these cycles have been added to the manuscript (page 18, line 8 to 10): "\cite{Ekaykin2002} observed 20 to 30cm cycles in the isotopic composition of the snow at Vostok and attributed them to undulation of the surface level. \cite{Hoshina2014} also describes similar cycles at Dome F, a site with one of the lowest accumulation of the East Antarctic Plateau, and instead attributed them to accumulation cycles of 3 to 5 years resulting in a multi-year cycles independent of the temperature " and page 20, line 22 to 27: "Cycles of approximately 20 cm were also observed by Hoshina et al., (2014) at Dome F and its vicinity and were attributed to multi-year cycles. Here, when we compare several snow pits dug the same year at the same sites (for instance at Vostok and Kohnen as presented on Fig. 8 and observed on more profiles), we do not observe synchronous peaks in the profiles of isotopic compositions. This tends to indicate that non-climatic (post-deposition) processes are preponderant for the formation of these cycles as we expect that multi-year cycles in the climatic conditions and thus precipitation isotopic composition, would globally affect one site. Still, the mechanisms involved in the formation of this 20 cm cycles are not clearly identified and will be the topic of another independent study in preparation."

This manuscript includes various types of oxygen isotope dataset. To distinguish them, why don't you use the symbols shown in Fig 2? For example, oxygen isotopic composition of precipitation can be represented by $deltaËĘ{18}$Op. I think that the word of "precipitation isotopic composition" is not popular. Generally, we describe as "isotopic composition of precipitation" or the delta 18O of precipitation. We will include this suggestion throughout the manuscript.

Please do not use italic letters in the text. All symbols and units should move out from math mode. For example, not $deltaËĘ{18}O$, but $deltaËĘ{18}$O.

Other minor comments: P5; L15: I can't understand this mean; "picked provided". Taken into account P8; L13: "sastruga" -> "sastrugi" From Russian, the singular is sastruga, applying the same rules that for latin words (maximum/maxima). P8; L26: "precipitation isotopic composition variations based on" -> "variations of delta18Op using" Taken into account P8; L26-P9; L2: These sentences are repeated in the following subsections. Hence, I recommend to omit them. P12; L1: Please replace "standard deviation" to "difference between two samples". At lease three more samples are necessary to calculate standard deviation. We did calculate the standard deviation (std, 1 sigma) of the half Gaussian curve obtained by fitting cups of the difference obtained between the duplicates on the NIVO samples (66 values). P12; L6: "Van Den Broeke (1998)" -> "Van den Broeke (1998)" Taken into account P13; L13: "isotopic composition seasonal variations" -> "seasonal isotopic variations" Taken into account P13; L31: "isotope exchange" -> "isotopic exchange" Taken into account P14; L1: "in parallel with : : :." Repeated sentence. Taken into account P14; L8: What is the mean of "frontal perturbation"? Do you mean the passage of frontal system? Taken into account P14; L11 – P15; L3: "In addition to the isotopic composition: : : (Goff and Gratch, 1945)" This sentence is grammatically incorrect. Corrected P15; L28: Which temperature do you use? Surface temperature or 2m-temperature measured by AWS? 2m temperature, precised in the description of the figure. P16; L8: "From the 16th of December : : :" SSA decreases do not correspond to the increases of the difference of delta 18O between surface and subsurface snow. Fig 7 shows that the large isotopic difference occurred a few days ago before SSA decreasing. We observe a first decrease of SSA around the 16th of December which lasts for a few days. What is interesting here is that we observe the increase of the difference of isotopic composition at the surface and at the sub-surface with this small, but significant decrease of SSA and no significant input of new snow to explain the creation of the signal. This has been precised in the manuscript (Page 17, line 5 to 8). "From the 16th of December, we observe large differences between the surface and the sub-surface snow isotopic composition (up to 5\textperthousand \ higher at the surface) and a first decrease of SSA indicating the metamorphism is effective. This feature is not associated with significant precipitation input, and the formation of the signal between the surface and the sub-surface isotopic composition could be linked to post-deposition processes." P17; L7: "is indicating" -> "indicates" Corrected P17; L7: "in the case of SSA" -> "in contrast, for SSA" Corrected P17; L22: "varriations" -> "variations" Corrected P19; L7: "not necessary capture" -> "not necessarily capture" Corrected P20; L8: Section 4.1 I can't understand the logics of this section. I wonder why the authors used the MCIM results to discuss the influence of the patchiness of the accumulation and precipitation intermittency. What is the physical mean of delta 18O of snow – surface temperature plots in Figure 9b and 9c? Did you pick up the samples only precipitating days? Please explain your strategy more specifically. In this section, we use the MCIM to illustrate the performances of Rayleigh type models to predict precipitation and surface snow isotopic composition. These kind of models are often used in Paleoclimate studies to evaluate the link between the isotopic signal found in deposited snow and climatic conditions. A precision of the role of the MCIM in this manuscript has been added (Page 8, line 23 to 32): "Indeed, \cite{Jouzel1984} evaluate the impact on kinetic fractionation during the snow formation is parametrised from the supersaturation, such as the effective fractionation coefficient $\alpha_{eff}ˆi$ is: α_{eff}ˆi = \frac{\alpha_{eq}ˆi S_i }{\alpha_{eq}ˆi\frac{D}{Dˆi }(S_i- 1)+ 1} where S_i, the supersaturation against ice, is tuned against temperature with a linear relationship; D and $Dˆi$ are the diffusion coefficients of water in the air and of the heavy molecule of water i, respectively; and $\alpha_{eq}ˆi$ is the equilibrium fractionation coefficient. The tuning of the supersaturation has been proven suitable to evaluate the variations of isotopic composition at Dome C \citep{Winkler2012a}. This will provide a comparison between the spatial (at the scales from 10 to $1000 \: km$) and the temporal slope of the isotopic composition of precipitation at the seasonal scale. It will also be used to highlight the post-deposition processes impact on the surface snow isotopic composition by providing a reference for the precipitation isotopic composition, for days with no precipitation." Here, we show that this kind of

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Discussion paper

model is revelant to evaluate the precipitation isotopic composition (as it is supposed to do), but not necessarily the deposited snow, as illustrated by the comparison with the surface snow. This has been mentioned in the manuscript (Page 22, lines 11-13): “Thus, for Rayleigh type models to evaluate the isotopic signal in deposited snow, we expect that it is necessary to add a component taking into account 1. the integrator effect of the snow layer accumulation and 2. the post-deposition impacts.” The patchiness of accumulation and precipitation intermittency is here studied in a second phase using the difference of signal in the isotopic composition measurements of precipitation and surface snow samples. Figure 9b and 9c are motivated by the common practice in paleoclimate reconstruction using stable water isotopes to infer a linear relationship between isotopic composition and temperature. Here, we present that the link is not so direct, we try to investigate what processes can be at the source of the intermittent imprint of the precipitation, ruling out patchiness of accumulation and precipitation intermittency alone. A better distinction between the model/data comparison and the precipitation/surface snow data comparison has been added in section 4.2. (page 22, line 16) “Second, we compare the signals obtained in the time series of precipitation and surface snow isotopic composition.” P23; L31: “surface snow isotopic composition variations” -> “variations of delta 18O of snow” Corrected P24; L10-L15; I think these sentences are meaningless. Taken into account P25; L14: “focus in particular oin” -> “focus, in particular, on” Corrected Figure 2. There is no explanation as for “phase X”. Please add the explanation for each symbol shown in Figure 2. Taken into account Figure 4. Please add surface air temperature from the ERA-Interim. This exercise has been done but do not bring any more information to the figure and makes it less readable. If needed, we can add a supplementary material comparing AWS data with ERA-interim temperature, but the description of the results has been made in section 2.4. Figure 6. Please add Local time Taken into account Figure 7. Although temperatures are expressed by degree C in previous Figures, this Figure uses Kelvin unit. Please unify the expression of the unit for temperature. Taken into account Figure 9. Did you use daily average temperature when you take surface snow samples? Are all data including sunny days plotted in Figure 9b and 9c? Yes, included in the description of the figure.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-263/tc-2016-263-AC1-supplement.pdf

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