Review of „ Archival processes of the stable isotope signal in East Antarctic ice cores “ by Casado et al.

This paper presents a synthesis of various datasets of the isotopic composition of near-surface water vapour, precipitation, surface snow, buried snow from Dome C as well as snow pits from five other Antarctic sites. The analysis of this data focuses on gaining a better understanding of the processes that govern the temporal variability of the surface and buried snow isotope signals at the synoptic to seasonal timescales. The paper remains rather qualitative in the results presented and discussed, but a good overview of the relevant processes is provided. This includes a discussion on the input of fresh snow by snowfall, the influence of deposition-sublimation cycles, metamorphism, as well as redistribution by wind.

I read this paper with great interest, it provides a valuable overview and first analysis of important post-depositional processes affecting the water isotope signals at low-accumulation sites such as Dome C. However, I have several concerns of major and minor nature, which address a few methodological aspects as well as the description of the relevant processes.

Major Comments:

1) I found the discussion of the frost deposition event particularly interesting, because, I also believe that such deposition events during warm advection might play an important role for the snow cover isotope signal, particularly at low accumulation sites (Section 3.3.1). I was however puzzled by several aspects that need clarification. **First**, it is astonishing that during the period of frost deposition the air temperature Ta is lower than the surface temperature Ts. I would expect it to be the other way around, which would hint towards an inversion layer near the ground that favours sensible and latent heat fluxes towards the surface. Is it really the case that Ta<Ts? If it is: Do the authors maybe have access to other sensors? The amplitude of Ts is as large as the one of Ta, isn't this surprising for an event that the authors argue to be a synoptic warm advection case (I would expect a larger amplitude for Ta than Ts, the snow temperature evolution being slower/dampened)? Was there any precipitation recorded during the event? The supersaturation is extremely large (Fig. 5). Is this the relative humidity with respect to a liquid or an ice surface?
   Second, I am confused with the closed box model used in Section 3.3.2. Why don't you use isotope ratios instead of changes in number of molecules? How do you represent the phase change in this model? I don't find any fractionation factor relating the isotope ratio in the vapour to the one in the ice. Actually, if I am right, you seem to use the observed changes in R18O in the vapour to infer the changes in the snow cover by mass conservation in a closed snow-vapour box. Could you make this clearer in the text? I suspect that there is a strong inversion (to be verified with the Ta-Ts observations) which prevents strong mixing between the near-surface air and the air higher above in the boundary layer. This would be consistent with an epsilon value=0. Furthermore, if the closed box model assumption is good, you should be able to predict the time evolution of the vapour and snow phase isotope composition from a simple Rayleigh model with initial conditions from your observations around 18 UTC on the 6th of January 2015. Did you try this?

2) Statistical evaluation of the predictions from the precipitation and snow isotope models: I would find it very useful, if you could quantify the goodness of your simple precipitation and snow cover isotope model simulations by comparing them with your measurement data using scatter plots, mentioning the temporal correlation and the root mean square difference. This could be shown in the appendices A and B and the summary numbers could be mentioned in the main text.
3) The relevant processes discussed here are sometimes referred to in an unprecise manner. For example, is "sublimation-condensation cycles" really what you mean? Don't you mean frost deposition-sublimation cycles. It would be of great use for the reader if the 4-5 processes discussed as important in the paper were precisely defined in the introduction (which phase changes are meant?) and then reused consistently throughout the paper.
4) Something that disturbs me: why is the vapour isotope composition not more prominently mentioned for example by including the d18Ovapour signal in Figure 3? If I understood it correctly the main message of the paper is: "snow metamorphism matters, repeated frost deposition-freezing cycles alter the isotope composition of the snow". That would imply that the vapour isotope signature is mirrored into the snow, wouldn't it? Also in the perspective of other recent publications e.g. Steen-Larsen, et al. 2014 vapour-snow interactions seem to really play an important role in the "dry" time periods between precipitation events.

Minor/technical Comments:

1) Units should be in normal style, not italics
2) Abstract: p.1 L1-2: The first two sentences mention "records" (repetition). And I would add a sentence to link the oldest ice core records with the stable water isotope composition of the ice, before mentioning them as being important climate proxies of conditions over the ice and at the moisture source.
3) P. 1, L. 4: I would add "**trajectory-based** Rayleigh distillation and isotope-enabled climate models"
4) P.1, L. 7: I suggest "of **the** isotopic composition **of the snow later forming the ice core ice**"
5) P1, L. 8: "we combine observations of **the** isotopic composition"
6) P1, L. 11: I suggest "on **the isotopic signal of** the surface snow"
7) P1, I suggest to also mention the importance of the vapour isotope signal in between precipitation events in the abstract.
8) P2, L. 3: You could add Jouzel and Masson-Delmotte, 2010, **and references therein**
9) P2, L. 16: A philosophical question: I wondered whether these phenomena really all create **non-climate** signals? Could the strength of the spatial variability over longer timescale not be a measure for the importance and typical spatial scale of these post-depositional redistribution-related processes? And is the redistribution really homogeneous in time and not dependent on the local climate? For example, in periods of more frequent warm advection events and more stable stratifications couldn't the redistribution by wind be weaker? Or in other words: are local wind turbulence conditions not also somehow related to the frequency of different weather regimes and thus dependent on the climate?

10) P2, L. 19: to open more on your work, you could begin by: "**Thus an important open question that needs to be addressed is**, whether this seasonal cycle is archived or not…"
11) P2, L. 28: "between **the** isotopic composition"
12) P.2, L. 29: What do you mean by "boundary layer processes"? At the evaporative source or at the sink over the ice?
13) P.2, L. 31: Add in the "source" evaporation conditions
14) P.3, Fig. 1: In the caption indicate what Pv and Psat is (the partial pressure and the partial pressure at saturation?)
15) P. 4, L. 4: "Condensation" seems strange at such low temperatures. Is this really what you mean? So first a phase change from the vapour to the liquid phase and then freezing into the solid phase?
16) P. 4, L. 5: Why do you expect that? Could you argue a bit more explicitly? Is it because of the long inter precipitation-event duration and thus the long exposure of the surface snow to these processes?
17) P.4, L.11: Maybe add "net sublimation occurs" or "sublimation dominates over condensation".
18) P. 4, L. 14: "more **stably** stratified". Or probably even the build up of strong inversion layers. You mention "important temperature gradients observed". Can you say more about this? I.e. Do you observe inversion layers?
19) P.-4, L. 21: "acquired during **evaporation at the moisture source** and the formation of precipitation…"
20) P. 4, L. 32 "deliver and **discuss**"
21) P. 7, Table 2: I suggest "sampling rate or frequency in days" instead of "Resolution".
22) P. 8, L. 15: could you just mention the correlation coefficient and the root mean square difference between the measurement station data and ERA Interim at the 6-hourly and seasonal time scale? I don't expect ERA Interim to be too good at Dome C and it's the best estimate that you have, but just to know how good the reanalysis data is.
23) P.8, L12-18: it was not immediately clear for me why you write this paragraph. Please explicitly say this, i.e. mention that you use ERA-Interim for the modelled delta18O precipitation.
24) P.9, L1: you thus use the grain index as a proxy for the strength of metamorphism? Can you say that explicitly?
25) P.9, L.10: I somehow missed how you computed the air mass trajectory, could you please mention this?
26) P.9, section 2.5; I like your approach and organisation of the paper of trying first to explain the snow isotope signal by precipitation isotope input, and then introducing the toy precipitation-snow cover transfer model!
27) P. 9, L. 24: don't you mean specifically the exchanges with the **vapour phase** here? Exchange with the atmosphere would for me include the input of precipitation.
28) P. 9, L.23-26: this sentence is a bit long and the second part should be something like: "by relating the surface snow and precipitation isotopic composition to the meteorological conditions and the grain index".
29) P.10, section 3.1.1 I would find it very insightful to know what the average precipitation amount per event and the inter event precipitation duration is.

30) P.10, L. 13-15: what are the possible reasons for this difference in precip isotope-temperature slopes?
31) P. 12, L. 9: "these **warms** events are particularly visible in winter due to increased storminess in the sea ice margin in this season (Papritz et al. 2014, http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-13-00409.1)
32) P. 13, L. 17: "as well as the warm anomal**ies**"
33) P. 13, L. 22: "snow **water** equivalent"
34) P. 13, L. 25-26 it would be nice to know how well the ERA-Interim snowfall matches observations at the site (see also my major comment 2)
35) P. 14, L. 5: add a space between permil and are recorded
36) P. 14, L. 17: "interaction with the surface roughness" sounds a bit awkward and unprecise, what do you mean exactly?
37) P. 15, L. 4: "turbulent and convective atmospheric boundary layer" this surprises me, I would expect a very stable or even inversion thermodynamic layering in the boundary layer during such a warm event if indeed it was one. A synoptic map with the pressure reduced to sea level and the 500 hPa temperature distribution would maybe help to assess the large-scale weather situation.
38) P. 19, L. 8: "sublimation/condensation cycles" I would say frost deposition/sublimation cycles (see my major comment 3)
39) P. 20, L. 4: I am not convinced that "globally" is a good wording here.
40) P. 20, L. 12: "However,…." Rephrase this sentence, I got lost here.
41) P. 22, L. 5: "precipitation isotopic composition" is it? Don't you mean the surface snow isotopic signal? (see again my major comment 3)
42) P. 26, L. 13-14: remove the "at least for winter conditions at the end of the sentence. You already say it at the beginning of it.
43) P. 28, L. 15: "that **the** surface snow"
44) P. 28, L. 17: " The amplitude of **the snow** isotopic composition"
45) P. 28, L. 26: Reformulate the first sentence.
46) P. 28, L. 31: "for isotopic signal**s**"

All in all, this is an interesting interdisciplinary paper with innovative ideas!