Review of "*Temperature and mineral dust variability recorded in two low accumulation Alpine ice cores over the last millennium*" by Bohleber et al., 2017

The paper presents an excellent dataset of stable water isotopes and other 'dust' proxies (i.e. insoluble particles and Ca^{2+}) from two separate ice cores drilled at Colle Gnifetti in the Pennine Alps, reaching back in time as far as a thousand year, a remarkable achievement for a European alpine ice core. This study combines a very good quality of data retrieval with a robust strategy regarding the dating, and therefore deserves to be published in Climate of the Past. The data treatment and statistical approach is also adequate and robust and only minor changes should be made. I will illustrate now few of the weaknesses that the manuscript presents and some suggestions on how to strengthen these points before the final publication. Detailed comments follow.

Firstly, the manuscript fails a bit in illustrating the reason why it is important to obtain a Ca^{2+}-derived temperature profile and what advantages/disadvantages this would have compared to a conventional $\delta^{18}O$-derived temperature profile. As mentioned in the abstract, the high and potentially non-stationary isotope/temperature sensitivity limits the quantitative use of the stable isotope ($\delta^{18}O$) variability and therefore a Ca^{2+}-derived temperature profile could provide essential information for a better constrain of temperature variability in the deepest (oldest) section of the two ice cores. This point should be highlighted more considering, however, that: i) Ca^{2+} sensitivity to temperature changes might be, and it is likely to be, non-stationary as well over the last 1000 yrs; ii) the relationship between Ca^{2+} and temperature could very well derive from post-depositional processes. This last point is particularly relevant (also considering that NH_4 show a similar temperature dependance) and the authors should elaborate more on why they think this is not the case. For example, if there is any data available of density, DEP or occurrence of melt layers, I suggest that the authors should use these data to back up some of their assumption regarding the summer-signal preservation by consolidation and its relationship with the seasonality of Ca^{2+}.

Furthermore, the assumption that the Ca^{2+} signal is almost entirely expression of a dust input from Saharan region is not enough justified in the text. The fact that the Ca^{2+} profile might derive from both wet and dry deposition and both proximal and distal sources cannot be ruled out from the data shown in the manuscript. Since the isotope/impurity co-variation on the inter-annual scale is mainly related to changes in the amount of winter precipitation contributing to annual mean values, I think is necessary to briefly consider different scenarios concerning the (although marginal) role of dry deposition in the Colle Gnifetti area and how these could change the Ca^{2+} signal in the different cases.

While provenance studies (Sr and Nd isotopes for example) go beyond the scope of the work, I think a more detailed discussion on the comparison of the insoluble dust profile vs the Ca^{2+} profile is necessary to utilize the calcium signal a proxy for Saharan dust input.

Whether Saharan_dust-Ca^{2+} data is a reliable proxy for palaeotemperature is yet again another point that needs to be better illustrated in the text. I think the authors should provide more justification regarding why the Ca^{2+} variability is mainly related to temperature changes and not, for instance, to changes at the dust source (Saharan desert).

Detailed comments:

Page 1 Line 1-2: I would update this statement in view of the recent 7000-yrs long ice core record from the Ortles (Gabrielli et al., 2017).

Page 3 Line 11-12: "which prevents any link of the climatologic precipitation rate to the net snow accumulation rate". I am not sure I understand here: Does this mean that the seasonality in the proxies is not governed by accumulation rate? Or is rather the longer-time variability? In any case I suggest changing the word "prevents" with "limits".

Page 3 Line 17: I found the wording a bit confusing. What "chemical/isotopic conditions" means? Do you mean chemical and isotopic signatures?

Page 4 Line 1-2: "the isotope/impurity co-variation on the inter-annual scale reflects to a large degree changes in the amount of winter precipitation contributing to annual mean values" I think is important here to highlight why the authors think dry deposition is playing a marginal role.

Page 4 line 10-11:"Therefore, the Ca^{2+} record of the CG ice cores is primarily related to mineral dust and dominated by Saharan dust". It's hard to tell without provenance studies. I suggest using "dominated by dust, most likely originating in the Saharan desert".

Page 7 Line 3: "Deviations from a CPP of 50% indicate higher or lower contribution of large and small particles respectively". You have to exclude local sources of dust then if you want to use the threshold to distinguish Saharan dust layers. I would add a sentence justifying this.

Page 8 Line 1: I would specify what "Ca signal" means. Is it Intensity in counts per second? Or total counts? Please add this also to the relevant figures.

Page 9 Line 8: "Below 26 m WE the identification of annual layers became ambiguous and was abandoned". Maybe I missed this information, but why then LA-ICPMS was not performed on the KCI core? Please provide justification, if it is not provided somewhere else.

Page 13 Line 12: "due to the strong effect of isotope diffusion at CG, inter-annual or even seasonal isotope variability is effectively eliminated". What about Ca^{2+} diffusion? While dust does not diffuse, the contribution of soluble particles to the Ca^{44} signal should be briefly addressed too, together with their possible diffusion.

Page 14 Line 31-32: "From a preliminary inspection of snow pit data recently obtained for the KCI-KCC flow line, there is no clear indication of a systematic trend in mean $\delta^{18}O$ levels upstream of KCC, however." It might be worthy to consider adding a plot (at least in the supplementary material) showing this.

Page 15 Line 12: "higher sensitivity values for KCI than KCC, revealing 2.3 vs. 1.4 ‰/◦C, respectively".

This discrepancy seems surprisingly high even considering the difference in accumulation rate that you correctly highlight. Could it be related also to the strong isotope diffusion at CG?

Page 20 Line 10-17: This entire section seems a bit far-fetched. As the authors said, the summer-bias signal at CG strongly advocate against a NAO imprint on the KCC and KCI temperature reconstruction. I suggest adding few more considerations to justify this link or remove the entire section.

Page 21 Line 1-20: I suggest to the authors to add a sentence outlining the feasibility of using Ca^{2+} records for temperature reconstruction in other alpine site, or generally in other low accumulation ice core site.

References

Gabrielli, P., Barbante, C., Bertagna, G., Bertó, M., Carturan, L., Dinale, R., & Seppi, R. (2017, April). 7000 year European climate record from the Ortles ice core. In EGU General Assembly Conference Abstracts (Vol. 19, p. 9932).