"Temperature and mineral dust variability recorded in two low accumulation Alpine ice cores over the last millennium" by Pascal Bohleber et al.

- Response to reviews -

Please note:

- ***All line numbers in "Changes to manuscript" refer to the new version (if not noted otherwise)***
- ***Changes in the corresponding pdf are highlighted in red***
- ***Author's responses to the referee's comments are in blue***
- ***All new references can be found in the new manuscript***

Introductory remark:

We thank both referees for their very thorough reviews and we appreciate the helpful suggestions and comments. After careful consideration, especially of points commonly raised by both reviewers, we determined the need to clarify our basic line of argument. For this purpose, we would like to emphasize the following key points:

1) We aim to distinguish throughout the paper two separate signal components of the Ca2+ record: ***1. Episodic spikes***, typically two orders of magnitude above background levels, and ***2. Long-term trends*** of the decadal-scale average Ca2+ concentration. Both components are evaluated separately. At CG, mineral background aerosol levels are generally low and the Ca2+ record is dominated by inputs of Saharan dust (e.g. Wagenbach et al. 1996). In this sense, the already established link between Ca2+ and Saharan dust concerns signal component 1. The potential new link between Ca2+ and temperature is evaluated for signal component 2.
2) Regarding 1., we do not intend to make quantitative inferences regarding mineral dust concentrations of individual events but aim to estimate their frequency of occurrence at CG. For this purpose we build on what has already been demonstrated in previous studies, namely that Ca2+ combined with an alkalinity measure is in fact a sensitive and appropriate tool to identify Saharan dust layers at CG (Wagenbach et al., 1996).
3) Regarding 2., we respond to the intriguing present situation at CG where we face i) fundamental shortcomings in making quantitative use of the stable water isotope

thermometer (Bohleber et al. 2013) and ii) the already known co-variation between trends in Ca2+ and delta O-18 (Wagenbach et al. 1996, Wagenbach and Geis, 1989) as well as delta O-18 and instrumental temperature (Bohleber et al. 2013). This raises the question to what extent a relationship exists between temperature and Ca2+ trends, and if this may serve as a potential substitute for quantitative temperature reconstruction at CG.

4) While we explore the suggested relation between Ca2+ trends and temperature, we strongly emphasize that it is not our intention to introduce a new ice core temperature proxy. We evaluate the Ca2+ trends solely regarding their *site-specific* temperature connection. This is an analogue approach as pursued for NH4+ in the Bolivian Andes (Kellerhals et al. 2010).
5) We also emphasize that we by no means disregard the influence of snow deposition and post-depositional effects. In fact, the main goal in using the semi-quantitative snow deposition model (section 2.2) is to demonstrate that post-depositional influence may not be disregarded when evaluating the temperature coupling to Ca2+-trends.

In order to eliminate the apparent ambiguities in the original version and in order to make our line of argument more clear we have made the following major changes to the manuscript. We feel that by means of these changes the most important issues raised by the reviewers have been properly addressed and the clarity of the paper has been substantially improved. Detailed responses to the referees' comments are given separately for referee #1 and #2, respectively.

Changes to manuscript:

- We have clarified the abstract and the conclusions according the above points. We now present two additional tables and two additional figures as supporting evidence in the appendix / as supplementary material.
- Page 4 Lines 29ff.: We added a clear statement regarding the separate treatment of Ca2+-spikes and long-term variability in this study.
- We have split up the previous section 2.2. as follows:
    - Page 3 Lines 16ff.: We combined with the original section 2.1 the fundamental description of snow preservation at CG and its consequences for interpreting the isotope and mineral dust proxies. This also includes the

basic reasoning for expecting a temperature-related imprint in the long-term Ca2+-variability.

    - Since we feel like it has diverted the attention from our main line of argument, we have moved the details of the semi-quantitative treatment of snow deposition to the supplementary material in the appendix.
    - Page 18, Line 4ff.: We now refer to the semi-quantitative analysis at a later point in the manuscript. The discussion of potential causes of the observed Ca2+-temperature co-variation is now presented within 5. Results and Discussion. We believe this makes it easier to follow for the reader, since the results have been presented at that point.
- Page 18, Line 21ff: We have included a clear statement regarding the site-specific nature of the observed Ca2+-temperature connection.

Response to anonymous referee #1

This paper presents an exceptional data set composed by stable isotopes and various proxies of aeolian dust deposition as measured in two Alpine cores from Colle Gnifetti (European Alps) during the last 1000 years. The quality of the data presented appears robust and, in general, the time scale adopted seems reliable. However, while this data set does deserve publication, the interpretation and use of the results obtained is often weak, speculative and potentially misleading. I believe the discussion section of the journal Climate of the Past represents a suitable venue to possibly sort out several important interpretative issues before the possible final publication.

We thank the referee for the detailed comments. As outlined in the introductory remark, we feel that there is an apparent ambiguity regarding the interpretation of the results, which we have now tried to remove and believe that by this means the main issues raised by the reviewer can be addressed properly.

Regardless the source of the dust deposited, the correlation between Ca2+ (as dust proxy) and atmospheric temperature may be really linked to a poorly known post depositional process. Dust in surface snow may indeed facilitate a metamorphic process that could consolidate and conserve the snow on site, for instance via a reduced albedo (larger amounts of solar radiation absorbed) at times with more frequent clear sky and higher atmospheric temperatures. In my view the authors should really try to better describe this possible process.

We fully agree that a post-depositional influence must be taken into account, and the mechanism described by the reviewer is entirely consistent with our general view regarding the post-depositional effect of dust-rich surface snow. We have added a more detailed description of this process. In addition we now provide supporting evidence for the influence of dust on snow consolidation. We have included a comparison between Ca^{2+} and the high-resolution density profile to show directly that in general dust-rich snow layers correspond with confined layers of enhanced density (with respect to the surrounding layers). This supports the concept of dust-rich layers featuring faster snow consolidation (Figure A2).

Changes to manuscript:

- Changes according to splitting the original section 2.2 as outlined in the initial remarks (see above).
- Page 18, Line 4ff.: We have included in this discussion our supporting evidence from the high-resolution density data. We also now include references for each potential driver for the Ca^{2+}-temperature co-variation.

Considering the poor seasonality of intense dust events of Saharan origin, at this time I do not see how their semi-quantitative model can be useful to support the possible occurrence of out of phase dust (wet) deposition and post-depositional consolidation. Most importantly, the out of phase dust/snow deposition and the possible post depositional process linked to atmospheric temperature is not sufficiently supported by data. In this respect results from past snow pit studies might be illuminating and should be extensively presented.

As outlined in the introductory remark, the main purpose of the semi-quantitative model is to demonstrate that snow deposition can have a non-negligible influence on the long-term variability of Ca^{2+} and thus must be considered as a potential contribution to the Ca^{2+}-temperature coupling. We have clarified this view accordingly and now also include additional information in the text regarding the following points:

- Snow pit data presented in earlier studies (Wagenbach and Geis, 1989) show that local maxima of stable isotope and dust-proxies coincide. This is consistent with what we find in the uppermost section of our core, providing a near-seasonal resolution (Figure A1). Analogue to the treatment of the stable isotope signal in

Wagenbach et al. (2012), it is thus justified to assume a potential phase-lag between accumulation and Ca2+ maxima. We also note that the principal function of the phase lag is to mimic the seasonally biased sub-sampling. Qualitatively similar results are found when using a different approach to model this process, e.g. by following Fisher and Koerner (1988).

Changes to manuscript: Include Figure A1 as additional supporting evidence in the appendix / supplementary material

- The connection between atmospheric temperature and snow consolidation is well established, e.g. temperature being used as parameter in various firn-densification models (also consider Figure 27 in Fauve et al. (2002)). A faster snow consolidation driven by higher summer temperature corresponds in the model to an increase in a_r and a decrease in t_phi. We have clarified this accordingly.

Changes to manuscript: Added respective references to discussion on page 18, lines 4ff. Added text and clarified the influence of temperature via snow consolidation in the semi-quantitative model (appendix A1).

- We also now explicitly point out that the seasonality in Ca2+ is not dominated by the seasonal occurrence in Saharan dust events, but rather the result of increased vertical mixing of air masses in summer vs. overall clean winter snow conditions (cf. Figure A2).

Changes to manuscript: Added text and references on page 4, lines 17ff.

While the occurrence of a Saharan dust fallout via wet deposition is very well established at Colle Gnifetti, the interannual variability of this kind of Saharan events is very high in the Alps and is mostly related to periods when atmospheric advection dominates (late fall, winter, early spring). During summer, however, atmospheric vertical convection rules in the Alps and dry transport and deposition of dust of local/Alpine origin is extremely likely and probably intense also on Colle Gnifetti. Source reconstruction of dust entrapped in ice cores is very complex even when sophisticated multiple proxies are used (e.g Sr and Nd isotopes). Ca2+, even when combined with ECM and dust size, cannot be considered a specific proxy of Saharan dust events and cannot discriminate with sufficient confidence between different kinds of sources (e.g. Saharan vs. local) and atmospheric circulation (e.g. meridional flows vs. vertical convection).

We agree that vertical atmospheric convection likely rules the seasonal cycle of Ca2+. The sophisticated isotopic fingerprinting of individual dust layers is certainly a worthwhile future target (and we appreciate the suggestion), although requiring substantial analytical capabilities as well as a significant portion of the ice core. For our main target being quantifying the frequency of occurrence of Saharan dust events, the task was to identify the specific imprint of Saharan dust in presence of events of increased impurity load by enhanced vertical mixing. Here we were able to rely on what has already been shown in the two earlier studies investigating dust-related records at CG (i.e. Wagenbach et al. (1996) and Wagenbach and Geis (1988)). These studies have clearly demonstrated that

- investigating "whether the size distribution parameters of the Saharan dust deposited differ significantly from those of dust deposits related to more regional source areas or even background aerosol" revealed that "the volume size distribution of the two Saharan dust events is indeed significantly shifted towards larger particles" (Wagenbach and Geis, 1988, and cf. Figure 7 therein). The authors also conclude that "it seems reasonable to attribute individual differences in the size distribution of the Saharan dust deposited on CG to different transport times which in turn means different transport velocities and/or different lengths of the trajectories".
- "that the Ca^{2+} spikes associated with the strongly alkaline snow layers in recent firn at Colle Gnifetti are most likely due to the mobilisation of calcerous Saharan soil material which, following long range transport to the Alps, is mainly deposited there by precipitation scavenging" (Wagenbach et al. 1996). The authors compile data from various CG snow pits and ice cores, finding that "This compilation suggests that the analyses of ionic Ca^{2+} in connection with reliable alkalinity measurements appear to be a very sensitive and specific tool to identify Saharan dust influenced snow layers in high depth resolution".

We now make additional clear reference to these specific earlier findings to point out that it is not the scope of our work to present a refined detection of Saharan dust in CG ice cores. Instead, we intend to make use of already established tools, in combination with our new ice core chronology comprising the last millennium at sufficient confidence.

Changes to manuscript: Page 4, Lines 26-28: Added text and reference accordingly.

Remarkably, if Ca^{2+} concentration is assumed to really trace dust from a specific area (e.g. Sahara desert) this would severely prevent its general use as paleothermometer as this parameter would strongly depend on environmental conditions at the source that are not directly related to atmospheric temperature. For instance a Ca2+ based paleo thermometer might be severely biased by different soil conditions at the source, due for instance to changes in atmospheric circulation and precipitation, not temperature (e.g. green Sahara during the middle-early Holocene). The use of Ca2+ to construct a paleothermometer could thus depend on very site specific post-depositional processes and on changing environmental conditions at the source of dust over time. Thus it is fundamental to greatly caution about its general use in space (drilling site dependence) and time (it may work only during certain times). In conclusion, while the relation between Ca2+ and temperature is interesting, its extended use cannot provide unambiguous novel knowledge about past atmospheric temperature.

Here we refer again to our initial comment regarding the separate treatment of Ca2+ spikes (Saharan dust proxy together with an alkalinity measure) and the overall trend component of the Ca2+ signal. Only the latter is explored with respect to a potential coupling to temperature, and we are now being more explicit regarding this distinction. We are also clearly stating that, at best, Ca2+ is a site-specific temperature proxy. Regarding its performance to reconstruct temperature in time, please see the next comment below.

Changes to manuscript:

- Page 4, Lines 29ff.: Added statement regarding distinction of signal components in Ca2+.
- Page 18, Lines 21ff.: Added statement regarding site-specific role of Ca2+.

When compared to Ca2+, the use of stable isotopes as a proxy of atmospheric temperature is better justified by the physics and is less sensitive to source effects due to moisture changes. It is remarkable that the correlation of stable isotopes and instrumental atmospheric temperature is very good during the last 150 years when the recorded instrumental temperatures are most robust. It appears that the real problem of the possible paleo-thermometer based on stable isotopes is the “excessive” and spatially variable sensitivity (1.4-2.3 per mill/C) when compared to what one would expect (0.65 per mill/C). For this reason the authors decide not to attempt a quantitative temperature reconstruction based on stable isotopes. However, while this decision may be justified, I believe that an extended discussion of the possibly biased paleo-temperatures obtained by means of this presumably too sensitive/variable paleo-thermometer would be very interesting. This could show for instance the inconsistency of the temperatures obtained, even considering physical processes such as the amplification of atmospheric temperature anomalies with the elevation.

We believe that an adequate discussion of the lags and leads of the peculiar high isotope/temperature sensitivity is beyond the scope of this work and would likely result in an entirely different paper. Ideally, such an evaluation would need to take into account regional isotope-climate modeling. We take the reviewers comment as an encouragement to pursue such a dedicated assessment in a future study. The agreement between the isotope and temperature trends within the last 150 years has been discussed in an earlier study, also showing that if calibrated with the last 100 years, isotope levels at CG suggest a warmer "early instrumental period" by about 0.4 deg C (Bohleber et al. 2013). Following this comment we now include this point in our discussion.

Remarkably, however, if considering the Ca^{2+} trends against instrumental temperature, we find an agreement of similar quality as for the isotopes, but now persistent over the full instrumental 250-year period (cf. Figure 7). Although the exact reason for a potential non-stationary isotope-temperature sensitivity remains elusive, this finding suggests that the potential Ca^{2+}-temperature relationship may not be affected to the same degree. Moreover, if using the Ca^{2+}-temperature for a tentative calibration, the reconstructed temperature variability is consistent with the latest summer temperature reconstruction based on other archives (the Luterbacher et al. (2016) record). The main point here is that given the agreement with instrumental data and other temperature reconstructions, we find no evidence of potential non-stationary behavior in the Ca^{2+}-temperature correspondence. We have added text in order to be more clear about this interpretation.

Changes to manuscript: Page 19, Lines 13ff.: Added text to clarify the interpretation of the comparison with the other proxy reconstruction.

A likely reliability of the timescale obtained for the two cores from Colle Gnifetti is suggested by the good correlation of Ca^{2+} with an independent, well dated, past summer temperature record obtained from tree rings. However, the reliability of this time scale may be just due to the use of recent absolute time horizons (during the last century) and ^{14}C measurements (although the significant reference, PhD thesis of Hoffmann 2016, cannot provide an accessible and peer reviewed methodological support). Counting of annual layers is not convincing in the deep part of core KCC. In particular the concept of “group of peaks" of Ca2+ to identify a single annual layer seems extremely arbitrary, and, at this time, not supported in the paper by any snow pit observation. In addition a possibly larger deflation of lighter snow during the colder summers of the Little Ice Age may have removed more annual layers that expected. In this way, it is very possible that the interval period between 150 and 600 BP remains unconstrained and prone to larger uncertainties.

We do not use the comparison with the summer temperature reconstruction to show the reliability of our time scale. This would require comparing an already demonstrated temperature signal reconstructed from the ice core. Our argument goes in the opposite direction: Having constructed a reliable time scale, we look for consistency between the potential Ca2+-based temperature variability and an established reconstruction from another archive.

We believe we have demonstrated the reliability of our time scale for the following reasons: Annual layer counting is a well-established and widely used tool for ice core dating. However, at CG the employment of this tool was so far limited by rapid layer thinning beyond the resolution of most melting techniques. Based on the agreement between CFA and LA-ICP-MS, we were able to make extended use of annual layer counting to build our chronology. While the identification of additional absolute dating horizons, such as volcanic eruptions, would of course be desirable, this is at present not possible for CG. However, we have taken great care to account for the resulting uncertainty in "unconstrained" annual layer counting (section 4.4). The resulting age-scale is backed by 14C markers, an approach already employed successfully at CG in previous studies (e.g. Jenk et al. 2009). The PhD thesis by Helene Hoffmann has been reviewed as part of the process in obtaining her PhD at Heidelberg University. It is fully available online and we have now included the link in the respective reference. In addition, we are now able to reference her according publication which has been accepted for publication in *Radiocarbon* in the meantime (Hoffmann et al. 2017).

We thank the referee for bringing our attention to provide additional justification for the concept of "grouped peaks" in the LA-ICP-MS Ca signal. For this purpose we have prepared a supplementary figure showing the uppermost 5 m of the delta O-18 and Ca2+ stratigraphy in our core. Here, multiple sub-seasonal peaks in Ca2+ are clearly present to back up this concept. In addition, we again refer to the fact that based on its ultra-high resolution and non-destructive technique, LA-ICP-MS can map the spatial heterogenity of impurities even within a single annual layer.

Changes to manuscript:

- Page 19, Lines 13ff.: Clarified regarding the comparison with the other proxy reconstruction not being intended for dating verification.
- Added reference for Hoffmann et al. (2017a,b)
- Added Figure A1 as supporting evidence for "grouped peak" concept

While Colle Gnifetti is a very well know ice core drilling site and many glaciological studies have been performed, this paper fails to offer the necessary comparison with existing data set available from other ice cores (and snow pit samples) obtained at the same site. In particular several studies of particulate and aerosol deposition were performed (e.g. Thevenon, JGR 2009) and should be carefully compared to check the consistency (or not) of the new findings with the previous results.

We already compare our results to the findings of Thevenon et al. (2009) on page 18 line 5 (original manuscript). However, a more detailed comparison is hampered given the difference in depth resolution and dating uncertainty between the two studies. We would certainly welcome a specific suggestion by the referee regarding the further comparison of our results.

Changes to manuscript: Page 17, Lines 7ff.: Added some more detail regarding the comparison with Thevenon et al. (2009)

Specific comments:

P1 L1: “Among ice core drilling sites in the European Alps, the Colle Gnifetti (CG) glacier saddle is the only one to offer climate records back to at least 1000 years”

There is now in the Eastern Alps a new ice core climate record that goes back almost 7000 years (Gabrielli et al. The Cryosphere 10, 2779–2797, 2016; Gabrielli et al. 19th EGU General Assembly, EGU2017, proceedings from the conference held 23-28 April, 2017 in Vienna, Austria., p.9932).

Thank you for pointing this out. We have changed our wording in order to be more specific. Colle Gnifetti is the only non-temperate site (Ortles in the Eastern Alps is partially temperate).

Changes to manuscript: Page 1, Line 1 and Page 2, Line 5: Changed wording accordingly.

P1 L8: “A high and potentially non-stationary isotope/temperature sensitivity limits the quantitative use of the stable isotope variability thus far”.

This statement is not discussed sufficiently within the text.

In this case the statement primarily serves as background and motivation (referring to what is already known at CG).

Changes to manuscript: Page 1, Lines 7ff.: We have changed the text accordingly to make this more clear.

P1 L15: “the medieval climate period around 1100–1200 AD stands out through an increased occurrence of dust events, potentially resulting from a relative increase in meridional flow and dry conditions over the Mediterranean”.

While the frequency of the dust horizons is reproducible in the two cores, they cannot be linked to individual dust events of Saharan origin that cannot be unambiguously distinguished from the occurrence of local past summer surfaces marked by dust accumulation.

As pointed out in our response above, we are making use of a tool developed in an earlier study and demonstrated to be suitable to identify Saharan dust events in CG ice cores (Wagenbach et al. 1996). Following the discussion with the referee, however, we now include a statement that more sophisticated methods based on isotopic fingerprinting exist today and may be used in the future to test and refine our findings.

Changes to manuscript: Page 20, Lines 15ff. Added text with a respective statement.

P2 L5: “Colle Gnifetti (CG) in spite of its limited glacier depth – stands out as the only site where net snow accumulation is low enough to provide records over the last millennium and potentially beyond at a reasonable time resolution”.

Again, this is not correct, please see Gabrielli et al. The Cryosphere 10, 2779–2797, 2016.

Thanks- we changed our wording accordingly (see above).

P4 L5 “A single deposition event typically lasts less than a few days (Sodemann et al., 2005; Schwikowski et al., 1995). The associated warm air temperature and the substantially lowered snow albedo both support surface snow consolidation and partly protect the dust layer from wind erosion.”

As long as air temperature is below the freezing level (as during snow events), this cannot be a factor facilitating snow consolidation.
As discussed in our response above, temperature does play an important role in snow consolidation.
Changes to manuscript: Page 18, Lines 4ff.: We have added a specific remark and an according references regarding this point.

P4-10 ”Therefore, the Ca2+ record of the CG ice cores is primarily related to mineral dust and dominated by Saharan dust related spikes”.

This conclusion is unsupported by more recent data detailing more specific proxies of Saharan dust.
As indicated in the text, this statement refers to findings already published by two previous studies (Wagenbach et al. 1996, Wagenbach and Geis 1988). If the referee would like to suggest a specific study that is providing new (in particular refuting) evidence, we would certainly consider this in our discussion.

P4-27 “For instance, warm summers feature increased vertical mixing and hence a higher atmospheric impurity load, and in addition, entail faster fresh snow consolidation. This may lead to an increased relative amount of impurity-rich summer snow deposition.”

This is a very reasonable and, unlike the meridional Saharan advection, a more regular process in the Alps. I’m not sure why the authors do not consider and discuss it further within the text.
From the discussion with the referee we learned that we have not emphasized this point strongly enough, although we believe it is a very important process in this context. In fact part of this process is what we explore with the semi-quantitative model exercise. We have

clarified this and come back to it in the respective parts of the Discussion.

Changes to manuscript:

- Page 3, Lines 18ff.: Clarified and added text.
- Page 18, Lines 4ff.: Included a detailed discussion of all involved processes.

P5-L7 "Here we follow the model of Wagenbach et al. (2012), which assumes sinusoidal cycles for the precipitation-borne signal S(t) and the surface accumulation pattern A(t), and a phase-lag t' between S(t) and A(t)."

While accumulation and delta 18O seasonal patterns are accepted at Colle Gnifetti, it is much less so for a disturbed seasonal signal like dust (Ca2+). The authors need to present data supporting how the sinusoidal assumption is justified for Ca+2 deposition.

Paragraph 2.2 In general I do not find that this paragraph well written or even necessary. It is not clearly explained what are the main motivations and conclusions of the conceptual model. At this time I'm also not sure how it supports the rational of the interpretation. Does the model show that a phase lag between deposition and consolidation explain some recent observations? If so, the phenomenology of these processes needs to be supported, displaying existing data (e.g. snow pits) that could complement the conceptual discussion performed by means of this model.

As outlined in the initial comment and after careful consideration of the referee's comment we have decided to break up the original paragraph as not to divert from our main line of argument. Being now used in the Discussion the scope of the model consideration becomes more clear, i.e. to semi-quantitatively demonstrate that the influence of snow preservation may not be disregarded when evaluating the long-term variability of Ca2+ and that it must be considered as a potential process introducing the coupling to temperature.

Albeit the sinusoidal pattern is of course an idealization, we have now included additional evidence for the seasonal pattern and link between delta O-18 and Ca2+ (Figure A1).

Changes to manuscript:

- Page 18, Lines 4ff.: Moved and rewrote this section (originally part of 2.2) to clarify
- Additional supporting figures in the appendix (Figure A1, A2)

P7-L2 "The threshold (4.0 μm) was chosen such that it corresponds to the expected median particle diameter of Saharan dust particles at CG (Wagenbach and Geis, 1989).“

Is this threshold necessary AND sufficient to discriminate between Saharan and non-Saharan dust? I do not think so. The reference reported is pretty old and in the meantime many tools have been developed to characterize dust sources. In my view this threshold is just indicative but not strictly discriminant.

Although the reference is old we are not aware that its findings have been refuted by newer studies. As said before, we certainly agree that more sophisticated tools may potentially allow for a precise fingerprinting of the individual dust sources, although requiring substantial analytical effort.

We intended to use the already established discrimination method based on the median particle diameter and set our threshold according to the previous study. That said, the results are qualitatively independent with respect to the exact choice of the threshold. Notably this includes the outstanding feature in CPP during the medieval period.

Only the sensitivity of the CPP to changes in the particle size distribution (PSD) is dependent on the threshold. Choosing the threshold to be the median value of the normal PSD means that the CPP is 50% for “regular” dust and is sensitive to changes in the PSD.

Changes to manuscript: Page 20, Line 7-8: We added a statement to point out that the outstanding feature during the medieval period is not a result of the choice of threshold.

3.2.1 Radiocarbon analysis. A table detailing all the results obtained by analyzing the 6+5 samples from KCC and KCI needs to be reported, including the linked uncertainties.

We have added a table as requested and would also like to point out that we were able to include an additional radiocarbon measurement for KCI, in clear support of the age scale.

Changes to manuscript: We included a respective table in the appendix, Table A2.

4 Ice core dating. A Table summarizing all the time horizons used in KCC and KCI needs to be reported. Another table indicating different kind of annual layer counting in different sections of the two cores would be also very useful.

Changes to manuscript: We included a respective table in the appendix, Table A1.

P9-L9 “(BP, referring to the drilling year of the respective ice core if not otherwise noted)”.

This could be very confusing. I strongly suggest to use a different notation.
We have generally changed this notation to either year AD or stating the precise year, e.g. year b2005 or year b2013 in order to be more precise.

P10-L6 “The groups of peaks are separated by a comparatively stable signal of low Ca concentrations. The latter is interpreted as resulting from the varying degree of winter snow being included in the record otherwise dominated by summer snow. “

This “group of peaks” sounds very suspicious and arbitrary. This idea needs to be supported with additional evidences from snow pit studies or from a comparison with the seasonality of stable isotopes at depths where annual layers are still distinguishable.
We thank the reviewer for bringing this to our attention. As requested we provide additional support of this concept in a supplementary Figure A1.

P10-L8 “Accordingly, the grouped peaks correspond to sub-annual snow deposition events of elevated Ca concentration during the summer period”.

How can multiple wet dust deposition events be distinguished from the formation of one or more summer surfaces of accumulated dust?
We do not see how, based on the LA-ICP-MS Ca signal alone, one could distinguish wet and dry deposition. However, the comparison with the CFA-based counting in depth intervals where CFA clearly identifies the annual layer signal (Figure 4 b)) shows that the "grouped peaks" are not a result of multiple, very closely spaced, summer surfaces. It would be hard to imagine a depositional behavior that produces such a "grouped peak" pattern at the observed regularity. Considering the additional Figure A1, it appears much more plausible to assign this to sub-seasonal structure resolved by LA-ICP-MS.

The different age depth relationships displayed in Fig. 5 for KCC and KCI between 150 years BP and 700-800 years BP needs to be carefully discussed. In fact, within this interval both cores do not depend on absolute time horizons.
As discussed above, unfortunately there is no means to identify additional horizons in the respective time period. We have accounted for the respective uncertainty in annual layer counting accordingly. Nonetheless, we have added more text to point out this circumstance.

Changes to manuscript: Page 10, Line 32-34: Added a respective statement to the text.

Caption Fig. 4b The year to year correspondence between Ca annual layers determined by LA and CFA should be indicated drawing lines connecting the star symbols. At the moment no clear one-to-one link is apparent.

Although the overall pattern between the CFA signal and the general baseline in LA-ICP-MS is highly similar, we are cautious about necessarily assigning a one-to-one correspondence between peaks. This is not least due to the possibility of a slight remaining offset between the two depth scales. We have added text to fully explain this in the caption. It was our intention to demonstrate that for a given depth interval, the number of years counted in both signals (CFA and LA-ICP-MS) is consistent within uncertainty.

Changes to manuscript: Added text to the caption of what is now Figure 3.

P12 L10 "The frequency of occurrence in these total snow loss events is, however, extremely hard to quantify. Counting annual layers in between the above mentioned (dust) horizons within the last century, reveals an offset of typically only one to two years as compared to the known age of the horizons." The last century is not very much representative of colder periods (Little Ice Age, LIA) where snow drift could have been far more important, possibly eroding more annual layers. Notably the LIA is also the time when no absolute time horizons are available for the time scale that depends entirely on counting annual layers.

Agreed, but there is probably not much that can be changed about this until additional absolute horizons are discovered. To reiterate, we have employed a dedicated approach to quantify our counting uncertainty.

P13 L13 "(e.g. note the distinct isotope minima around 1360 AD)." This is an important note when considering the companion paper by More et al. 2017 in Geohealth as this time corresponds almost exactly with the time of the Black Death. Could this isotope minima have been a large winter snow accumulation event? In this sense the implications for the interpretation of the linked Pb record could be very important.

We point out that the two papers were not designed to be companion papers, e.g. as stated in the manuscript there are minor differences in the used age scale. Although the 1360 AD isotope minima could be in principle be connected to a higher percentage of winter snow preservation, the signal in the impurity species is less outstanding. We thank the referee for noting this and take this comment as encouragement for further investigation.

P14 L14 “However, this is the first time that the correlation holds to this extent also for the comparatively old core sections”.

As far as I know, this has been also observed at least in the Ortles ice cores (Gabrielli et al. The Cryosphere 10, 2779–2797, 2016).

This statement refers primarily to the CG ice cores, and we have changed the wording accordingly. That said, to our knowledge Gabrielli et al. (2016) show a comparison of the three Ortles ice cores on a depth scale, not as time series.

Changes to manuscript: Page 12, Line 17: " However, this is the first time that the correlation holds to this extent also for the comparatively old core sections of CG cores"

P15 L5 “Stack of the two stable isotope records (calculated as their simple average)”.

Please, mention the temporal step used to calculate averages.

Changed accordingly.

Changes to manuscript: Page 13, Line 32: "Stack of the two stable isotope records (calculated as their simple average, at nominal annual resolution)**"**

P15 L15 “substantially higher sensitivity values for KCI than KCC, revealing 2.3 vs. 1.4 per mill/C, respectively”.

This is surprising considering the striking similarities of the two stable isotope profiles (Fig. 6). Could you provide an explanation?

We do not have a full explanation for this phenomenon, adding to the enigmatic nature of the isotope sensitivity at CG. However, the central difference between KCI and KCC is the especially low net accumulation at KCI. This entails an even stricter confinement to sampling mainly the summer season precipitation. This points towards the depositional bias to play a role in explaining the high sensitivity values.

P15 L16 “changes in snow preservation are expected to bias sensitivity (cf. the conceptual consideration in section 2.2).”

In this case, the conceptual model presented should be useful to quantify and perhaps correct this bias.

This statement refers to the reasoning of behind the previous comment (see above). While using the model to correct for the bias is an interesting suggestion, it would certainly require information on the past variability in snow deposition at each site, which is not available and generally very hard to quantify at CG. We decided to reword this paragraph to clarify our reasoning.

Changes to manuscript: Page 14, Line 10ff.: Reworded the paragraph accordingly.

P15 L17 “This is consistent with the sensitivity difference among KCI and KCC, since an even more strict confinement towards sampling the high summer season can be expected for the lower accumulation KCI.”

Agree, but from just a look of the two records, KCI and KCC show very similar absolute stable isotopes values during the potential calibration time (Fig. 6).

The difference is in fact small compared to the absolute values (e.g. for the last 100 years -13.48 vs -13.71 per mil for KCC and KCI, respectively). However, it also affects the magnitude of long term trends, e.g. the recent increase in isotope values over the last 100 years (e.g. based on linear regression 0.20 vs. 0.25 per mil / decade for KCC and KCI, respectively). We thank the reviewer for pointing this out and have included a respective remark in the text.

Changes to manuscript: Page 14, Line 10ff.: Reworded the statement accordingly.

P17 L3 “While the particle signal alone is not sufficient for differentiating these events, Saharan dust layers in CG ice cores can be reliably identified based on the analyses of Ca2+, supplemented by alkalinity measurements and, in principle, particle size distribution (Wagenbach et al., 1996).”

This information is not sufficient to discriminate between a Saharan dust event and a past summer surface formed by dry dust accumulation. While Saharan dust events may well have these characteristics, also summer dust layers formed during prolonged dry periods

could have the same or similar characteristics. In addition, a higher coarse particle percentage may be more indicative of local dust rather than long-range transported dust.
In order to avoid redundancies we would like to refer to our previous responses presented above, and references to the earlier studies.

P18 L5 “This is in broad agreement with periods of enhanced Saharan dust deposition reported by Thevenon et al. (2009) obtained from elemental analysis in a CG ice core.”

This presumed broad agreement should be demonstrated in detail. At this time the high frequency of dust events in the described periods is both consistent with more frequent Saharan events and the formation of dust enriched past summer surfaces.
Accepting the limitations due to the different depth resolution in both records, we made an attempt to compare our findings with the results by Thevenon et al. (2009) in a little more detail.
Changes to manuscript: Page 17, Lines 7 ff.: Added text.

P19 L25 “The connection of the distinct increase in coarse particles with enhanced dust event frequency indicates an increase in direct transport of Saharan dust the (as opposed to indirect advection with longer pathway and thus stronger decrease in coarse particles).”

This is, at best, consistent (not indicative) with an increase in direct transport of Saharan dust. In fact this observation is also compatible with other scenarios (e.g. higher occurrence of summer surfaces marked by dust, increase in the intensity of the summer vertical transport of dust of Alpine origin).
Based on our response presented above regarding the distinct difference in particle sizes of Saharan dust (Wagenbach et al. 1996, Wagenbach and Geis 1988), in our view this observation is not compatible with the mentioned scenarios. However, we have slightly changed the wording of this statement following the referees suggestion.
Changes to manuscript: Page 20, Line 11: **"**the connection of the distinct increase in coarse particles with enhanced dust event frequency rather *suggest*s an increase in direct transport of Saharan dust**"**

P19 L27 “Notably, this view is consistent with increased deuterium excess, which would be expected from warm and dry air masses collecting moisture over the Mediterranean."

This may be an important note that needs to be expanded and adequately referenced. Finding increased values of deuterium excess supports the view of a relative increase in Saharan dust advection, which we have made more clear. We have also added an according reference as suggested.
Changes to manuscript: Page 20, Line 15ff.: Changed accordingly.

P20 L12 "increased meridional transport favoring direct Saharan dust advection over the Mediterranean is consistent with a NAO+ dominated MCA, lasting 1100 to about 1300 AD, as proposed by Trouet et al. (2009). Although NAO is mainly a winter signal (thus not a direct concern to CG summer representative ice core signals)"

As mentioned, Saharan dust advection is just one of the possibilities and a NAO signal consistent with this hypothesis is really a weak reasoning, especially considering that a NAO winter signal can have little impact on the summer biased cores from Colle Gnifetti. Please, consider to entirely remove this section (lines 10-17).
Following the reviewers comment and after additional consideration, we have decided to remove this section.

Conclusions: conclusions needs to be tuned down accordingly to the main observations performed.
Changed accordingly.
Changes to manuscript: Page 20, Line 25ff.: Reworded the second part of the conclusions.

P21-L12 "The intrinsic contribution of snow preservation may bias the isotope temperature sensitivity".

This is interesting but it has not been shown and adequately discussed within the text.
We have removed this statement from the conclusions.