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Climate of the Past
Discussions

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

Anonymous Referee #1

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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.

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.

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.

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).

Remarkably, if Ca2+ 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 pale thermometer 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.

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 paleothermometer based on stable isotpes 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 paleothermometer 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.

A likely reliability of the timescale obtained for the two cores from Colle Gnifetti is suggested by the good correlation of Ca2+ 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 14C 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.

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.

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).

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.

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, thye 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.

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.

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.

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.

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.

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 $t\varphi$ 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.

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.

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.

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.

P9-L9 “(BP, referring to the10 drilling year of the respective ice core if not otherwise noted)”.

This could be very confusing. I strongly suggest to use a different notation.

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.

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?

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.

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.

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.

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.

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).

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

Please, mention the temporal step used to calculate averages.

P15 L15 “substantially higher sensitivity values for KCI than KCC, revealing 2.3 vs. 1.4 ‰âŮęC, respectively”.

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

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.

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).

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 5 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.

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.

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).

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.

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).

Conclusions: conclusions needs to be tuned down accordingly to the main observations performed.

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.

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