

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

3 - Response to reviews -
4

5 **Please note:**

- 6 • *All line numbers in "Changes to manuscript" refer to the new version (if not*
7 *noted otherwise)*
 - 8 • *Changes in the corresponding pdf are highlighted in red*
 - 9 • *Author's responses to the referee's comments are in blue*
 - 10 • *All new references can be found in the new manuscript*
- 11

12 **Introductory remark:**

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

- 17 1) We aim to distinguish throughout the paper two separate signal components of the
18 Ca²⁺ record: **1. Episodic spikes**, typically two orders of magnitude above
19 background levels, and **2. Long-term trends** of the decadal-scale average Ca²⁺
20 concentration. Both components are evaluated separately. At CG, mineral
21 background aerosol levels are generally low and the Ca²⁺ record is dominated by
22 inputs of Saharan dust (e.g. Wagenbach et al. 1996). In this sense, the already
23 established link between Ca²⁺ and Saharan dust concerns signal component 1. The
24 potential new link between Ca²⁺ and temperature is evaluated for signal
25 component 2.
- 26 2) Regarding 1., we do not intend to make quantitative inferences regarding mineral
27 dust concentrations of individual events but aim to estimate their frequency of
28 occurrence at CG. For this purpose we build on what has already been demonstrated
29 in previous studies, namely that Ca²⁺ combined with an alkalinity measure is in fact
30 a sensitive and appropriate tool to identify Saharan dust layers at CG (Wagenbach et
31 al., 1996).
- 32 3) Regarding 2., we respond to the intriguing present situation at CG where we face i)
33 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

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

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

- 76 • Page 18, Line 21ff: We have included a clear statement regarding the site-specific
77 nature of the observed Ca²⁺-temperature connection.

79 **Response to anonymous referee #2**

80 The paper presents an excellent dataset of stable water isotopes and other ‘dust’ proxies
81 (i.e. insoluble particles and Ca²⁺) from two separate ice cores drilled at Colle Gnifetti in the
82 Pennine Alps, reaching back in time as far as a thousand year, a remarkable achievement for
83 a European alpine ice core. This study combines a very good quality of data retrieval with a
84 robust strategy regarding the dating, and therefore deserves to be published in *Climate of*
85 *the Past*. The data treatment and statistical approach is also adequate and robust and only
86 minor changes should be made. I will illustrate now few of the weaknesses that the
87 manuscript presents and some suggestions on how to strengthen these points before the
88 final publication. Detailed comments follow.

89 [We thank the referee for the comments and encouragement to further strengthen the](#)
90 [manuscript.](#)

91
92 Firstly, the manuscript fails a bit in illustrating the reason why it is important to obtain a
93 Ca²⁺-derived temperature profile and what advantages/disadvantages this would have
94 compared to a conventional $\delta^{18}\text{O}$ -derived temperature profile. As mentioned in the
95 abstract, the high and potentially non-stationary isotope/temperature sensitivity limits the
96 quantitative use of the stable isotope ($\delta^{18}\text{O}$) variability and therefore a Ca²⁺-derived
97 temperature profile could provide essential information for a better constrain of
98 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 dependence) 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+} .

We thank the referee for this comment, in particular for the suggestion to include considering the density profile of the core. As discussed in the initial remarks, considering the reviews we realized that a few issues need to be clarified, and see some of these points arising here, too. In fact, we believe that post-depositional processes must be considered when explaining the apparent coupling between temperature and long-term variability of Ca^{2+} . We have clarified and extended our discussion of this point. The comparison between density and Ca^{2+} data clearly shows that dust-rich layers are coinciding with locally enhanced density, that stem from fast snow consolidation. This "self-preserving" characteristic of Ca^{2+} (and other dust-related species) against wind erosion is one of the main differences with respect to the stable isotope signal. We have also added text to discuss the fact that, while a non-stationary character of the Ca^{2+} -temperature relationship is certainly a possibility, we find no evidence for this within the instrumental period (in contrast to the stable isotopes). Following the referees comment we have also elaborated that, for this reason, fundamental shortcoming exists in quantitatively interpreting the isotope-thermometer over long time scales at CG. Although we do not intend to introduce Ca^{2+} as a new general temperature indicator, we see our findings as a strong indication of the potential for using the long-term variability of Ca^{2+} as a *site-specific* temperature proxy. We have clarified this view also in our conclusions.

Changes to manuscript:

- Page 4, Lines 4ff.: Rewrote part of this section accordingly. Specifically regarding the motivation for expecting a temperature-related imprint in Ca^{2+} .
- Page 18, Lines 4ff.: Moved and rewrote part of the paragraph (originally in section 2.2), specifically mentioning the self-preserving character of Ca^{2+} .
- Page 13, Lines 14ff: Included additional mentioning of the shortcomings of the

132 stable isotope thermometer at CG.

133 • Page 19, Lines 13ff.: Included a statement to clarify the lack of evidence for a non-
134 stationary Ca²⁺-temperature relationship.

135 • Page 18, Lines 21ff: Emphasized the site-specific role of the Ca²⁺-temperature
136 association.

137

138 Furthermore, the assumption that the Ca²⁺ signal is almost entirely expression of a dust
139 input from Saharan region is not enough justified in the text. The fact that the Ca²⁺ profile
140 might derive from both wet and dry deposition and both proximal and distal sources cannot
141 be ruled out from the data shown in the manuscript. Since the isotope/impurity co-
142 variation on the inter-annual scale is mainly related to changes in the amount of winter
143 precipitation contributing to annual mean values, I think is necessary to briefly consider
144 different scenarios concerning the (although marginal) role of dry deposition in the Colle
145 Gnifetti area and how these could change the Ca²⁺ signal in the different cases.

146 We would like to refer here to the initial comments and point out that at CG, mineral
147 background aerosol levels are generally low (including summer) and the Ca²⁺ record is
148 dominated by inputs of Saharan dust, which has been demonstrated in previous studies
149 (Wagenbach et al. 1996). Thank you also for pointing out the role of dry deposition, which
150 we have so far not explicitly mentioned in the manuscript.

151 **Changes to manuscript:** Page 4, Lines 13ff.: We have included a brief discussion of the
152 contribution made by dry deposition to the mineral dust content at CG.

153

154 While provenance studies (Sr and Nd isotopes for example) go beyond the scope of the
155 work, I think a more detailed discussion on the comparison of the insoluble dust profile vs
156 the Ca²⁺ profile is necessary to utilize the calcium signal a proxy for Saharan dust input.

157 We agree with the referee that a provenance study based on isotopic trace element analysis
158 exceeds the scope of this study. At the same time the identification of Saharan dust input
159 based on Ca²⁺ (and an alkalinity measure) has already been established in a previous study
160 (Wagenbach et al. 1996). Thus we did not intend to develop a new (and arguably more
161 precise) proxy for Saharan dust events at CG, but intended to use this already established
162 tool. We have clarified this in the respective introductory section 2.

163 **Changes to manuscript:** Page 4, Lines 26ff.: Added a clarifying statement regarding the
164 tool to identify Saharan dust events.

Whether Saharan dust-Ca²⁺ 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 Ca²⁺ variability is mainly related to temperature changes and not, for instance, to changes at the dust source (Saharan desert). As outlined above it is not our intention to directly link the Saharan-dust component of the Ca²⁺ data (spikes) to temperature, but rather investigate for this purpose the long-term variability of Ca²⁺. We find the Ca²⁺ trends in surprisingly good correlation with instrumental temperature throughout the full instrumental period, and go on to discuss how snow preservation plays a decisive role in introducing this Ca²⁺-temperature coupling. It seems likely that only large and systematic changes at the dust source would change the long-term Ca²⁺ variability, or eventually override the coupling to temperature. On the other hand, these changes (e.g. increased dust mobilization) would likely also influence the Saharan dust spikes and their frequency of occurrence. However, the only instance where we find an outstanding according feature is the increased dust occurrence in the medieval period of our record. We thank the referee for this suggestion and now consider this issue in our discussion.

Changes to manuscript: Page 19, Line 18: Added text to discuss the role of changes at the dust source.

Detailed comments:

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

Changed accordingly to clarify. In contrast to Ortles, Colle Gnifetti is a non-temperate site.

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".

What we intend to say is that due to the highly variable snow deposition at CG, it is not possible to infer precipitation changes based on e.g. annual layer thickness (e.g. as done with Greenland ice cores). We have clarified the wording accordingly.

Changes to manuscript: Page 3, Line 10-11: "limits linking the net snow accumulation rate

to the climatologic precipitation rate"

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

Yes we mean the signature of chemical and isotopic species measured in the CG ice cores.

We have clarified the wording accordingly.

Changes to manuscript: Page 3, Line 16: "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.

See our response above, we now include a short discussion of the role of dry deposition.

Changes to manuscript: Page 4, Line 14ff.: Added text regarding dry deposition.

Page 4 line 10-11:"Therefore, the Ca²⁺ 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".

Thank you, we have reworded this statement and included the respective reference.

Changes to manuscript: Page 4, Line 13-14: Reworded the previous statement.

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.

Thank you. We now point out the findings of Wagenbach and Geis (1988) in this context, who showed that Saharan dust in fact differs in volume size distribution in comparison to local and background sources.

Changes to manuscript: Page 5, Line 16: " The threshold was chosen such that it corresponds to the expected median particle diameter of Saharan dust particles at CG, which was shown to be distinguishable from background sources"

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.

Thank you for pointing this out- in this case it is in fact intensity in counts per second,
although it is possible to achieve an according calibration of the LA-ICP-MS signal (Sneed et
al. 2015).

Changes to manuscript: Added text to captions of Figures 2 and 3, respectively.

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.

There was actually a pilot study for LA-ICP-MS performed on KCI (Sneed et al. 2015),
however, not targeting yet the identification and counting of annual layers. Given the
sophisticated and time-consuming nature of LA-ICP-MS we have so far only analysed KCC in
a continuous manner. We take the comment as encouragement to further pursue the LA-
ICP-MS analysis, potentially revisiting KCI in the future. We have added text to provide this
information.

Changes to manuscript: Page 8, Lines 10ff.: Added text regarding LA-ICP-MS on KCI.

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 Ca²⁺ diffusion? While
dust does not diffuse, the contribution of soluble particles to the Ca⁴⁴ signal should be
briefly addressed too, together with their possible diffusion.

The effect of diffusion is certainly smaller for Ca²⁺ than for the stable water isotopes, as we
do not see any evidence of diffusion hampering the identification of the annual layers at the
high resolution afforded by LA-ICP-MS. However, we are now mentioning this effect, and in
particular also point out the contribution of soluble Ca to the LA-ICP-MS signal.

Changes to manuscript:

- Page 6, Line 13: "The ⁴⁴Ca signal comprises contributions of soluble and insoluble Ca"
- Page 9, Line 16-17: "The annual layer signal remains clearly identifiable for the remaining part of the depth-range investigated here (e.g. apparently not affected by diffusion of soluble Ca)"

263

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

268 As mentioned in the text the detailed investigation of the isotope-upstream effect is still
269 ongoing based on sophisticated 3D-flow modelling (PhD thesis Carlo Licciulli at Heidelberg
270 University). However, we have provided additional information regarding the preliminary
271 inspection.

272 **Changes to manuscript:** Page 13, Lines 24ff.: Added text accordingly.

273

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

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

279 The degree of isotope diffusion could certainly be another difference between KCI and KCC,
280 thank you for pointing this out. This is especially so in the firn section, and here (due to the
281 difference in accumulation rate) the age interval represented by the firn column differs for
282 the two cores. Although it is difficult at this stage to give a more quantitative evaluation
283 regarding its effect on isotope sensitivity, we now include mention isotope diffusion. We
284 will also consider this in a potential future investigation on the enhanced isotope sensitivity.

285 **Changes to manuscript:** Page 14, Line 13-14: Added text accordingly.

286

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

291 After considering the comments of both referees in this direction, we decided to remove
292 this section from the discussion.

293

Page 21 Line 1-20: I suggest to the authors to add a sentence outlining the feasibility of using Ca²⁺ records for temperature reconstruction in other alpine site, or generally in other low accumulation ice core site.

We are now generally trying to be more clear about the site-specific nature of the potential temperature significance of the Ca²⁺ long-term variability. However, it would be interesting to test if the Ca²⁺-temperature association observed at CG holds also at other alpine sites.

Changes to manuscript: Page 20, Line 28: Included a respective statement in the conclusions.

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).