The Cryosphere Discussions

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Comment on tc-2022-250

Giovanni Baccolo (Referee)

Referee comment on "Chemical and visual characterisation of EGRIP glacial ice and cloudy bands within" by Nicolas Stoll et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-250-RC1, 2023

This is an extremely interesting contribution about the analysis of cloudy bands in glacial ice found in the EGRIP ice core. The authors applied a set of cutting edge techniques, providing valuable data about the micro-structure and –composition of these well-known features that are found in polar ice from the glacial periods. Despite most ice of core scientists that have worked with polar ice cores know what a cloudy band is, most of them does not really know what is behind their formation and significance. Considering this, this work is more than welcome and will represent a benchmark for future studies dealing with the structure and description of glacial ice in polar ice cores.

The manuscript generally reads well, despite I think that most sections could be shortened. Reducing the text would surely improve the readability of the entire work. The amount of data presented is really impressive: visual stratigraphy, texture, impurity distribution and mineralogy. The integration of all these data is for me the most lacking aspect of the manuscript. Sometimes you have the impression that the authors are just presenting the data, without really discussing them in relation to ice micro-structure or to the paleoclimatic significance of the proxies they are focusing on. Also the cited literature reflects this, there are some discussions or interpretations that do not really take into account what was done in previous study and this of course weakens the discussion of your data. This is probably related to the fact that there is so much material in this manuscript that it is not easy to elaborate it, but I think that more efforts toward this direction would improve the soundness of the study and increase its future impact. My impression are also supported by the fact that the authors say many times that some of the data will not be discussed because such discussions would be out of the scope of the present manuscript. I personally don't like finding 3 or 4 times such a statement and suggest to remove them and to consider to reduce all the discussion that is not really linked to the story of this research. In the light of this, I found that after some revision the manuscript will be ready for publication in The Cryosphere. Below my more specific comment.

Line 2: “concentrations, are”; I would not say “the last glacial”, of course your are thinking of Greenland ice cores, where talking about glacial ice means talking about ice from the last glacial period, but for Antarctica this is not automatic, I would rather say “from glacial periods”.

Line11: why you say “Cloudy bands are thus clearly distinguishable in the chemical data.”? I mean, using thus it seems that in the previous passage, you provided evidences to support what you are stating here, but it does not seem the case. Maybe I have not understood, but I suggest you to rephrase this part of the abstract.

Line 15: I would add “in deep polar ice cores” at the end of the passage

Line 19-22: I suggest removing the cited drilling sites, this is because, especially for Antarctica, there are so many deep sites that it is impossible to cite all the relevant ones. For example now you are not considering Vostok that is actually the site where the deepest ice core ever has been drilled, or also Dome A, where the future longest ice core is expected. I would simply remove the brackets where you list a few sites, it is impossible to cite them all or to make a rigorous selection. You could also think of citing a review paper about ice core science at this point, for example Brook & Buizert, (2018) or Jouzel (2013) or also Langway (2008) about early polar ice cores drilled in Greenland.

Line 22-23: I suggest changing to “Considering different polar ice cores most of the physical and chemical properties of ice and of its impurities vary, depending on several parameters that are different at each drilling site. Concurrently, there are some features that seem recurrent, such as the presence of the so-called “cloudy bands” in glacial ice.”

Line 25 -26: I suggest starting this passage describing how they look visually, so that the reader can understand their name, and then you can describe more in detail their physical structure. I would not say small crystals, rather “crystals smaller (maybe add a reference value here to have an idea of what you mean with small) than the surrounding ice”

Line 31: I suggest specifying here that grain refers to insoluble particle size, while ice grain are referred across the entire manuscript as ice crystals. This would help not to confuse the reader.

Line34: “There is no typical cloudy band” I don’t understand this passage. What do you mean? That until now a unique description for this ice-structure has not yet been established? Or maybe that cloudy bands can significantly differ within each other? Please reformulate

Line 36-39: Do you mean that in some cases the bands correspond to the seasonal peaks of specific analytes? If this is the case this should be better explained.

Line 47-48: suggest to change to “and thus affect the bulk deformation rate of ice softening it”

Line 67: I suggest adding one or two references to papers prepared by the Japanese group that also developed a similar technique. For example (Ohno et al., 2005; Sakurai et al., 2009).

Line 68-82: I would change a bit the structure of the introduction here. I would start saying that this work focuses on the study and description of cloudy bands in the EGRIP ice core using the methods that you have just cited. Then I would say that you choose this specific ice core as it is one of better studied from this point of view, so you already know a lot of things about the distribution, quality and quantity of impurities present in the ice and about ice microstructure.

Line 90: it is not clear what you mean with grain size. This is probably ice-grain size, but not to confuse the reader I would be consistent throughout the manuscript in distinguishing ice crystals and insoluble impurities using different terms (see my comment at line 31).

Line 119: here you say that the bands presenting signs of folding were grouped as "unknown", but a few lines above (line 110) you said that in this study you did not consider deformed cloudy bands. I don't understand.

Line 144: I would add "to clean the surface of interest before the analysis"

Line 145: I would add "to monitor potential instrumental drifts"

Line 152-153: this passage is not clear, please rephrase

Figure1: maybe better saying in the caption that blue bands correspond to the cloudy bands you measured through raman and LA? Because now it seems that you only considered those bands while actually you classified all of them throughout the considered interval depth.

Line 160: I would remove "However, the relationship between particle size and grayscale is unexplored.", this is not so relevant for your manuscript.

Figure2: I have noticed that you never discuss probably the most evident feature of this image, that is the frequency increase of unknown cloudy band types at specific depths. Could you add some detail about this in the discussion part of the manuscript? How do you interpret this?

Line 180: be careful because you are saying that carbonaceous particles belong to a specific mineral class, but this is not the case

Line 190: this is a curiosity I have. I have seen that you identified hematite rather frequently, what about goethite? From my experience this Fe-oxide is typically more abundant than hematite in polar environments. This is because hematite requires arid environments that are more easy to find in the tropics, while goethite is more related to temperate and subpolar climate (relatively cold and wet conditions). Maybe the fact that you did not identify goethite depends on your technique that did not allow to see it?

Line227: you say that elemental ratios are highly variable in the particle clusters you identified with la-icpms, but actually you are never showing results about these ratio, is a figure missing?

Line231: from what I see in Figure 1, at 1600 m the mean grain size of ice crystals shows the minimum value in the considered depth interval, with a smoothed mean value of mean grain area (red curve) which is near to 1 mm2. This is not in accordance with what you say in the text, you claim in fact that at this depth ice crystals are larger, but I can't really see this in the graph. The depth of 1600 m corresponds to the LGM, the coldest phase of the last glacial period and also the one where the ice present the highest impurity content. It is well-known that dusty ice presents smaller crystals because the dynamic recrystallization is inhibited by the high number of defects found in the ice lattice. About this you can see for example (Durand et al., 2006). Your data well confirm previous findings, also in relation to the overall increase of grain size with depth, a classic feature observed in deep ice metamorphism. In the light of this, I would change your text, including a brief discussion of how climatic features influence ice microstructure in the EGRIP ice core.

Line 235-236: I don-t understand why pointing out that your data are unprecedented if you don't explore or discuss them, I would remove this passage.

Line 238: what you mean when you say "develop"? Do you mean increase in size? I would rephrase with something like "The increase of ice grain size with depth is faster at EGRIP

than NEEM…". I am also asking what you mean ejrm you say that ice metamorphism is faster and more intense at EGRIP because of the location and of the strong dynamic of the glacier at the site. I mean, I understand the point but I think that now a reader who is not aware about ice metamorphism would not understand. I suggest you to rephrase more clearly and add some references about the effect of strain on ice grain size and also of temperature maybe (you are saying that recrystallization is faster at EGRIP than at NEEM also because of the lower elevation and the higher temperature right?).

Figure7: would it be possible to add at panel a (I think you should always distinguish your figures into panels if they are composed by more than a part) the smoothed curves of the data, this would help appreciating the similarities and differences between the two cores. It would also be nice to include EGRIP isotopic data to be consistent with the comparison between the grain size, are these data available?

Line243-245: this passage is not really well written, I suggest to rephrase or also to delete it.

Line 249-250: are you talking about impurities in glacial ice? It is not clear, it seems that you missed to add the term of comparison.

Line250: comparable to what? I would rephrase for example as "In general, mineral variability in glacial ice is slightly lower than what is found in Holocene ice (Stoll et al., 2022). This is mostly due to a richer diversity of sulfate minerals found in interglacial ice."

Line 252-253: I don't understand this passage, in S^ you identified 69 minerals (I see from Figure 3), here you say that in this sample you could not identify 8 minerals, corresponding to about 10% of total observed minerals. Why are you saying that in this sample "the total amount of different minerals at this depth is higher than identified", I can't get the point.

Figure 8: I don't think this is the best graph o show the evolution of mineral assemblages over depth (and climatic stages). Spider plots are the best to highlight trends referred to single species (maybe to highlight this it would be nice to add a linear trend to the data), but to have an idea of how the overall mineral assemblage changes with depth, you could prepare pie charts showing mean data about each climatic period (so one chart for the Holocene, one for YD, one for BO, one for the glacial0. In this way it could be possible to really see if the climate and mineral diversity are somehow related, now this is extremely difficult to see. If I interpreted correctly the grey lines are all the minerals, while the highlighted ones refer to the specific minerals referring to single panels. Is this right? You should better explain this in the caption, however I suggest to remove grey lines, it would be clearer to have a single line for each panel, I don't see the reason to show all the minerals in each panel. I have seen that when you did not find a mineral you just skipped the point associated to the considered sample. I suggest to add 0 values in place, in this way it would be possible to better appreciate when the mineral is not present (that is an information). For example if I look at hematite it seems that in the Holocene its concentration is rather constant, but this is only because there are many samples where you did not find hematite that are not reported. I suggest to deeply revise this figure and improve its readability.

Line 254: considering the above comment, I can't really see the decreased mineral diversity in glacial ice according to the way they are now presented, I suggest you to prepare a different type of graph to better discuss this point.

Line 255: I do not agree with your interpretation. It is for sure true that during the glacial periods the atmosphere (and thus the ice) is dustier, but this is not directly related to an increased mineral diversity. From what we know, glacial dust is mostly supplied by specific

sources that activate under glacial conditions, so there is a lot of dust but its signature is rather uniform. On the contrary, during interglacials the atmosphere is cleaner because of a suppressed atmospheric dust cycle, so every small dust source can contribute to dust emission without being overwhelmed by powerful glacial dust sources. This has been noted in Antarctica (see for example Baccolo et al., 2018; Delmonte et al., 2020; Gabrielli et al., 2010) but also in Greenland (see Bory et al., 2003; Svensson et al., 2000; Újvári et al., 2022), where dust diversity in the Holocene is higher than in the LGM. I would change accordingly to this discussion

Line270-274: finding carbonates in glacial ice and not in interglacial one is something that have already been reported and discussed both for Antarctica and for Greenland. In glacial periods the atmosphere is much more dusty, mineral particles are thus less affected by acidic weathering, both during transport and once trapped into the ice. This is because the acidic species present in the aerosols are efficiently neutralized reacting with the huge amount of dust present in the atmosphere. On the contrary during the integlacials the acidic species are more abundant in relative terms, because dust concentration is much less abundant. Therefore, dust that can potentially react with acidic species (such as carbonates) are easily weathered. The final result is that you don't find much carbonates in ice from interglacial periods, while you can find some in glacial ice, as you are showing with your findings. I suggest you to cite the following papers about this and revise the discussion: Eichler et al., 2019; Iizuka et al., 2008; Ohno et al., 2006.

Line284-289: you could compare this result with Baccolo, Delmonte, Di Stefano, et al., 2021, where the only other available hematite record from a polar ice core is presented. Hematite is generally not stable at pH lower than 4 (Schwertmann & Murad, 1983; Zolotov & Mironenko, 2007), so the fact that you find it across the entire core could be used as a proxy for this information. You could also mention this. Since hematite is produced in relatively warm and arid environments (Schwertmann, 1988), its presence is also in accordance with the main source for Greenlandic dust (i.e. arid areas in central Asia). This is another thing to tell here. Again i am asking myself why you did not find any trace of goethite, that should be much more abundant in dust deposited in polar and cold regions (there is some literature about that).

Line 290: you have never introduced the concept of ice as a geochemical reactor (which actually was introduced not in the paper you are citing (Baccolo, Delmonte, Niles, et al., 2021), but in (Baccolo, Delmonte, Di Stefano, et al., 2021)), introducing this so abruptly is not very clear. You could change to something like "Nitrates and sulfates in the third row in Fig. 8 are minerals, which have been recognized as byproducts of weathering processes that involve dust trapped in deep polar ice".

Line 318-319: it seems that in this passage you are missing a word. It does not sound correct.

Paragraph 4.3 and 4.4: I found the first part (4.3) a bit wordy. I think this could be greatly shortened. At the end the main point is that combining Raman and LA-ICPMS allows to fully describe how impurities are distributed in deep polar ice, i.e. insoluble crustal elements concentrated in intra-gran inclusions, more mobile and soluble impurities at grain junctions because of ice-crystallization. What is not very clear in these two paragraphs (especially 4.4) is the link between your data (laser ablation one in particular) and cloudy bands. Looking at Figure 5 and 6, where you compare Raman spectrometry, laser ablation and visual stratigraphy, I can't really understand how you link these 3 elements with the presence or absence of cloudy bands. For example you never fully discuss the variability that is observe in results from LA-ICPS. You data clearly show some trends occurring at grain junctions, with some part of the sample that are richer in Fe and other in Ti (see for example S11 in Figure 6), in other cases you have nice accumulation of elements at junctions and suddenly ice much purer or with a higher abundance of intra-

grain inclusions (S10, Figure 5). I think that data integration here should be improved.

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