

Total Air Content measurements from the RECAP ice core
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We thank the two anonymous reviewers for their second review. Our point-by-point response to the reviewers' further comments below.

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Anonymous referee #1

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

This manuscript has changed substantially following major revision, and the authors have clearly put significant time and effort in to improving the manuscript following the reviewer comments.

The manuscript is much clearer to follow, with a logical rational and structure. This makes the scientific takeaways easier to glean and provides a more coherent story despite there being a few different approaches to the scientific interpretation within one paper. It would be nice to see this dataset published and I just have some more minor comments:

The last 2K years – the Holocene record of TAC is largely compromised by melt, but in the last 2K years there are virtually no melt layers seen. TAC variability in this section is still rather high, outside of what can be related to elevation change. Perhaps the authors could add a comment on the cause of this variability?

Some of the samples in the top region of the core have been selected since they show melt. Therefore, the variability in figure 2 does not represent randomly picked samples in that region. We added a sentence explaining this in the figure caption.

Figure 1(b): Can the red location point be made slightly larger and labelled? It is quite difficult to see.

Changed accordingly.

Line 96-97: Given that it is stated at this early stage in the manuscript that TAC measurements are only useful for elevation estimates in climatically stable periods,

could the authors add comment on whether they expected this to be different for their measurements at Renland?

Sentence has been added as requested.

2 Measurements: How have the two systems been quantified for measurement uncertainty, and can these values be presented here to compare? I.e. Error of the analytical system itself, as opposed to pooled St.Dev of the TAC measurements. I feel I am missing a quantitative comparison of the two different systems. Also see related comment for supplement.

We are not able to provide that information. However, the pooled standard deviation is a measure integrating all uncertainties of the system including also the natural variability of TAC. Therefore, the similarity of the pooled standard deviations of the three measurement series shows that they are equivalent.

Figure 4: I was confused by the negative melt percentages at first. Reading the supplementary methodology, I'm guessing this comes from the wide range of TAC values spanning 0% melt in the calibration such that many values in this range sit above the final calibration? Perhaps this could be explained somewhere just for clarity, supplement would be fine.

We added an explanatory sentence to the figure caption.

Line 236: 'Comparable to today 120 kry ago': suggest rephrasing, 'comparable between 120 kry ago and today'.

Done

Line 260: 'And comparison to' should be 'and compare to'.

Done

Line 264-265: 'The time period considered, corresponding to the time it takes for surface snow to arrive at close off is delta age'.

The sentence seems identical to the one in the manuscript. We unfortunately do not understand what the reviewer would like us to change.

Line 272-273: repeats closely to lines 263-264, rephrase to avoid repetition.

We rephrased lines 272-273.

Line 280: 'when D-O manifest as drop': 'when the D-O event manifests as a drop'?

Done

Line 297: 'the correlation of (the?) spline of ISI...'

Done

Line 303: 'allowing (exclusion of) data affected...'

Done

Section 5.5: I would like to see a figure of the last glacial maximum data included in this section since it is directly working with it for the elevation reconstruction.

We moved figure S12 from the supplements into the main text. Accordingly, we deleted section S9 from the supplements. For readability we increased the font size in figure 7 (formally S12).

Line 337: 'For example' would be preferable to just 'E.g' for starting a sentence.

Done

Supplement

PICE TAC system – How accurate is the cutting of the samples, stated to be of 22 x 25 x 25 mm? Are they calliper measured and to what accuracy? Perhaps this fits in with the previous comment on measurement uncertainty being quantified for the two systems. Are potential errors such as this one included in quantification of measurement uncertainty?

Sample size is measured to 0.5mm precision. A sentence has been added to the supplementals. Such statistical errors are not included in quantification of the individual measurement uncertainty.

S2 final sentence: 'Given the relative to the sample...'. This sentence is not very clear. Please rephrase.

Reformulated

S3 first sentence: ...'calculates from the'... remove 'from'.

The cut bubble effect (CBE) is indeed calculated **from** the average bubble diameter. Removing "from" would change the meaning of that sentence.

Figure S4: It is really quite difficult to see the bubbles in the ice that are being referred to, is there a clearer image available?

The provided picture is representative of the images that were used in the analysis.

Submitted on 22 Oct 2024

Anonymous referee #2

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

I have read the revised version of the manuscript by Vudayagiri and colleagues. I thank them for considering my comments and overall making their paper easier to follow and to understand. However, I still think that further few improvements are needed before it can be published. I list here additional comments I would ask the authors to consider.

I'm still a little puzzled by the beginning of the paper with the first introductory section (lines 25-36); it is more of a second abstract than a paragraph to introduce the subject of the manuscript. It is fine to keep this first paragraph short but the authors should consider providing more simply some introductory sentences about the use of TAC historically as an elevation change proxy (referencing studies from Raynaud and colleagues) and keeping their text of the motivation to use it for the RECAP core considering the previous studies by Vinther et al. and the previous Renland ice core. This is fine to then list the different topics tackled in the paper but I would suggest it to present it as an outline of the rest of the manuscript e.g. in the following Section X will tackle Y. In section 3, we will investigate etc.

We thank reviewer 2 for the feedback. We appreciate the suggestions regarding the introductory section. However, we prefer to keep the current structure and content of the introduction as it is. We believe it effectively sets the stage for the manuscript by summarizing the motivation and scope without delving too deeply into historical details.

In general, I still find that there are still left some imprecise statements and title sections in several places. I would urge the authors to go through their manuscript to fix that. I'm not listing them all here. But for instance, they should be careful when referring at ice core names, it should be specified when it is about the ice core, or when it is about the TAC data, examples are at lines 26 (it should be Renland ice core), line 183 (it should be the Renland ice core site) or line 264 (it should be the NGRIP and RECAP TAC records).

We changed accordingly for the first and last example. The statement on line 183 is true for entire Renland so not only the coring site. Therefore, we would like to leave it as is.

-Line 10: to reformulate such as: "The RECAP TAC data shows incoherently low values during time intervals corresponding to the Holocene climatic optimum (6 to 9 kyr b2k) and part of the last interglacial (119 to 121 kyr b2k) originating from melt layers which renders the TAC data unfit for paleo elevation interpolation.

Done

-Line 20: to reformulation such as: “Within uncertainty, the elevation of the RECAP site during the last glacial maximum was similar to today.

Done

-Line 26: to reformulate to : “...used the first Renland ice core (+ add references)”.

Done

-Line 33: to remove the term Eemian in brackets and elsewhere in the manuscript. I read the answers from the authors however 1/ I don't agree that it is customary in the ice core community to use that term (it was just initiated through one prominent publication presenting the NEEM ice core) and hence 2/ I still see no added value to use the term “Eemian” to refer to the Last interglacial when investigating the Last interglacial period in ice core records. I would again refer the authors to the paper Govin et al. QSR 2015, a community-led effort to try and avoid, amongst other things, confusions to be brought with the misuse of certain terms defined in specific archives to designate time intervals such as the Last interglacial.

I, Thomas Blunier, do not agree. Throughout my career, the term "Eemian" has been consistently used to refer to the previous interglacial in Greenland. While the added value may be debatable, we will remove the term from the manuscript for good measure.

-Line 36: Define the LGM acronym here and use it in the rest of the manuscript.

Done

-Line 78: to reformulate the sub-section title to “TAC variations at orbital –scale”

Done

-Line 93: to reformulate the sub-section title to “TAC variations at millennial-scale”. I agree that rapid TAC variations are not fully understood however the papers cited by the authors (Eicher et al. 2016 and Epifanio et al. 2023) do propose some possible hypotheses related to changes in the firm structure. This should be formulated here with a couple of additional sentences.

We believe the title is adequate since the changes seem to occur also on shorter than “millennial scale” and prefer to leave it as is.

We have shortened this introductory section on the reviewers' previous requests. We believe that as an introduction stating “lacking understanding” is a fair statement.

-Line 164: I would re-iterate my comment to avoid a section title that is just a time interval (here “holocene” but also later with “the last interglacial”), the authors should propose more specific titles.

Changed to “The RECAP TAC Holocene record”.

-Line 209: In their answer to the review, the authors mention that in Langen et al. (2017), modelled melt amounts and melt extents have been evaluated against in-situ and satellite-based observations. I believe that this would be a valuable information to add here. Also, line 210: rephrase “extrapolated temperatures” into “modelled temperature”

Added in appendix A.

-Line 249: I think I now understand better what is the purpose of this section and it is helpful to have the figures shown bigger to look at the results. However, I think that the authors could still improve its presentation by formulating properly the objectives of the approach developed. “To create a general picture of what is happening in the firn column” imprecise (and also redundant statement as it is in line 263 and then line 272.

Also, they formulate the following results:” For both cores, on average, the TAC values start to decrease around the depth (time) when CH₄ starts to increase at the beginning of a D-O event. However, the minimum TAC is found before the depth (time) when D-O manifest as drop in dust or increase in d18O. For NGRIP this minimum is reached some 600 years before the snow associated with the D-O event reaches close off while for RECAP it is about 150 years.” But they don’t propose an interpretation nor discuss potential ways to investigate this further (for instance using specific experiments with firn densification models). I find that it is missing.

The second statement has been adapted on the request of reviewer 1.

Firn air modelling, if it is to be helpful, requires a hypothesis that we currently are not able to offer. If reviewer 2 has suggestions on the processes that might be at work, we are happy to hear them.

-Line 292: Reformulate the title to “RECAP TAC and local summer insolation”. Also, regarding the link between orbital-scale TAC changes and local summer insolation: I understand that changing the ISI target to ISI390 or some other type of local summer insolation curve will not change the result that orbital-scale TAC changes are not significantly correlated but the authors should still at least acknowledge in the text that there are some open questions related to the most appropriate choice of orbital curve to use for comparison with TAC variations. The authors could refer to the recent work from Raynaud et al. CP 2024 which is discussing this in details.

The title has been updated as requested. However, we do not see a compelling reason to include a reference to the latest paper by Raynaud et al., as we are simply describing our own approach in this context.

-Line 304: Reformulate the title to “Elevation change reconstructions from RECAP TAC during the Last Glacial Maximum”

Done

-Line 345: to reformulate the sentence as such: “...and toward the end of the last interglacial (119 kyr to 121 kyr b2k)”

Done