Climate of the Past Discussions

Clim. Past Discuss., referee comment RC1
https://doi.org/10.5194/cp-2021-155-RC1, 2021

Comment on cp-2021-155

Anonymous Referee #1

Referee comment on "A multi-ice-core, annual-layer-counted Greenland ice-core chronology for the last 3800 years: GICC21" by Giulia Sinnl et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-155-RC1, 2021

General comments:

In this study a new common time scale for the Greenland ice cores called GICC21 for the last ~ 3800 years is presented. In the first part of the paper, the authors discuss their improvements of annual layer counted dating and synchronization of six Greenland ice cores and the uncertainties of the method. The new timescale is subsequently compared to the previous GICC05. In the second part of the paper, the authors investigate impacts of volcanic eruptions on the isotope signal and annual layer thickness and discuss the potential presence of two mediterranean eruptions in the Greenland ice core records.

The development of a new common Greenland ice-core chronology including the two most recent cores NEEM and EastGRIP is an important step forward to better compare ice core records with other climate archives. It is a valuable contribution to Climate of the Past and should be published.

The manuscript is very well written and clearly structured. The argumentation is stringent and easy to follow in most sections. The first part of the manuscript is very detailed and discusses in depth the development and the uncertainty assessment of the new chronology. The authors put a lot of effort into the assessment of the chronology and its uncertainties, which I appreciate. However, the second part of the manuscript, starting with the discussion of the ice core records around volcanic eruptions, is lacking a bit of depth and critical evaluation in some parts. Regarding the overall length of the paper, I would like the authors to consider splitting the manuscript into two: Keeping the first part as a technical and methodological assessment of the new GICC21 chronology and dedicating the second part to the scientific investigations of the volcanic eruptions and the IntCal / ^{10}Be discussions. I think the second part of the manuscript could benefit from some more in-depth analysis.

I have some specific comments and questions especially about sections 3.4, 3.5, 5.2 and 5.3

Specific comments:

L18: Radiocarbon dated evidence. please clarify. What kind of evidence? Evidence for what?

L24: Late Holocene – please give time frame

L33: a short description of the general differences in dating methods, stratigraphic and relative vs. absolute age markers and their pros and cons in terms of error would be helpful.

L47: That is quite a bold statement. We know that isotopes can be used as temperature proxies but only to a very limited extend and under certain circumstances, also keeping in mind all the post-depositional effects. Please weaken this statement and also cite some more recent studies, for example: Laepple, T. et al. On the similarity and apparent cycles of isotopic variations in East Antarctic snow pits, The Cryosphere, 12, 169–187, 2018.

L52: Reference for high and stable snow deposition?

L108: why do nitrate peaks coincide with ammonium peaks? Please explain, give reference.

L110ff: please add the tritium peak as a radiometric marker

L157: what bias do you refer to? Overcounting, undercounting or something else? Please clarify.

L175: Please define WDC

Table 1: Accumulation given in m total or m.w.e.?

L252ff: can you please give depth resolutions for the NEEM and NEEM-2011-S1 data?

L265ff: Depth resolution for GRIP data?

L275ff: Depth resolution for DYE-3 data?

L283-290: Nice overview of the data processing steps

L306f: Please clarify that sentence. What do you mean exactly by pre-processing? What was the control data?

337-340: I don't really understand the knowledge gain by inverting and log transforming the ECM into pseudo NH_4^+. In my opinion this does not give any additional information and rather creates apparent alignments which are completely artificial. Either clarify the benefits of the pseudo-ammonium or take it out (also in Fig. 2), because I think it could be misleading.

L341: Clarify reference datum

L345-355: Using both times scales (b2k and CE) can be confusing for the reader. I would recommend to use b2k throughout the manuscript and only additionally refer to CE for some of the historical events where the CE age is well known.

Fig.2: would be good to either use more distinct colors for pseudo NH_4^+ and NH_4^+ or take out the pseudo signal completely (see comment above). Please add NH_4^+ on the y-axis, not just ppb. Why was the big ammonium peak between 214 and 216 m in North GRIP not used for matching? Seems like an unusual gap and as if you could find matching features at least in GRIP and DYE. Contrarily some matchpoints are not very convincing, e.g. nr. 5 in EastGRIP and GRIP.

Table 2: I'm not sure if it is helpful to calculate the total score, because for example for Laki the total score is two, but four out of six cores undercounted it. Seems to be misleading and not necessary for the fine tuning procedure in my opinion.

L424: I can't find the 17% improvement in Table 3

L420-425 and Table 3: I would recommend to shorted or take out the comparison between DYE-3 and the SWG Greenland temperatures as well as the correlation study between GRIP and DYE-3 isotopes. I would not call the improvement of correlation "substantially", even if it is 17% and for the SWG temperatures the authors themselves state that there is no significant change. As it is not relevant for the further discussion I would remove this paragraph.

L455: What exactly do you mean by historical evidence? Is that referring to volcanic eruptions?

L461: what kind of local effects do you refer to? Please clarify.

L467-486: That section needs some clarification in my point of view. First, I think it should be emphasized that for the youngest part the uncertainty is ~2 years, that important statement is somehow lost in the text in line 471. Then follows the calculation of the SC uncertainty compared to the fine tuned record using convolution of the individual probability distributions of the cores. Looking at Fig. S3 it seems as if there are systematic offsets towards over- or undercounting for certain cores and the differences can be quite significant (e.g. panel d or k in Fig. S3). I am therefore wondering if the convolution that weights each core equally is the best method to assess this uncertainty or if there should be a more individual assessment for each core similar to section 3.3

L493: This number is the key finding of the section if I understand it correctly and should be emphasized.

L495-508, Fig. 4 and Table 5: I think this section could be shortened and needs some clarification. It is currently a little confusing. I don't think the fine tuning of the obtained prob-SC and the bias correction are necessary, or maybe the conclusion of that correction should be clarified. Also I think Fig. 4 and Table 5 could be moved to the supplement if not discarded. The consequences of the outcome of 0.24 years of uncertainty after bias correction of the SC counting is left uncommented to some extent. It is also somehow unrelated to the final statement in L505ff where the uncertainty of the fine tuning process is very vaguely defined as "smaller than the bias correction itself" and an overall uncertainty of 1 year every 100 layers is deduced. Please clarify and shorten that paragraph with respect to these issues.

Fig.5: What caused the outlier at about 3500 years?

L535 and equation 1: Please explain a little more how equation 1 was constructed. I assume the 2 denotes the error for ages younger than Samalas? Does absolute uncertainty mean the result of eq. 1 is the +/- range of the referred age?

L556f and Fig.: 6: What causes the common uncertainty excursion in DYE-3 and EastGRIP around 600 b2k? Is that just coincidence? Could be addressed a little more in the main text and not just in the caption of Fig. 7

L607-641 and Fig. 9a: It would be interesting in this discussion to see a direct comparison between GICC21 and IntCal20. The advantages of GICC21 over GICC05 have been discussed in the section before. Why not show something like GICC21-IntCal20 in Fig. 9a? that would help to understand the interpretation of Fig. 9b

L641: Why do you still refer the uncertainty to IntCal 13 here and not IntCal 20? Please clarify.

L651: That is quite a bold statement. I would recommend to weaken it. As was stated, there are some differences in the production signals and the wiggle matching is not always entirely convincing (e.g. at about 3650 b2k). I think the underlying processes and uncertainties of IntCal 20 as as well as the ^{10}Be record are rather complicated, so I would be careful with the conclusion you draw.

L658: I wonder if stacking all of the core signals and trying to find common features of volcanic eruptions is the best way to approach this. Given the geographical and meteorological differences of the core locations, it might be more meaningful to look at the cores separately on the new common time scale. This could also be done additionally to the investigation of the common features. It would be interesting to see to what level the single cores contribute to the stack. This is my main point of criticism for this section.

L678: The finding that NorthGRIP, GRIP and DYE-3 return a minimum in isotope but not in layer thickness, but the full stack (basically only adding NEEM and EastGRIP) does, could hint to a more local feature? A little more discussion of this result would be good.

L681: Again, it would be interesting to see the single core reactions compared to the stack, if only in the supplement. Can you put the retrieved numbers into context? What does a 0.12‰ drop in isotopes compare to? e.g. the last deglaciation or changes in the Holocene? I'm missing a quantitative evaluation here. Same for the layer thickness result.

L683: a change in isotope signal could also mean a change in source or transport, please don't only refer this to temperature. The change in accumulation rate underpins that there must be more complicated things going on than just a drop in temperature. Please discuss a little more.

L685-689: Can you assume that changes in the NAO will affect all core location in the

same way? Is there a difference between the more southern and the more northern ice cores? Once again it would be great not to only look at the stack results but at the single cores separately. I think that would help to interpret the isotope and layer thinning results.

L692-693. Please clarify that sentence. What do you mean exactly by wet/dry depositional effect on the measurement?

Fig. 10: Looks like there are two dips in the stacked ECM data at ~-8 and ~-16 years. Are these significant?

L707: please clarify why a thinning should be a result of cooling. See comment above.

L716: Reference for Vesuvius magnitude?

L725: does ECM refer to the stacked signal here? Please clarify.

Fig. S6: I think there are some minima in the 1700-1900 b2k window relating to the ECM peaks, just with a little delay. Is there a reason why they should be directly on top of each other? These are two completely different processes.

L731-732: Did you just investigate the ECM stack or also look at other indicators for volcanic eruptions (e.g. sulfate data where available, pH and other potential parameters).

L737: also sulfur isotopes could be an option.

L737-741: I think this paragraph is not necessary and could be deleted. The exact date of the Vesuvius eruption is known, so why refer to the less precise radiocarbon dating?

Fig. 11: what about marker nr. 4? Is that way too far out of uncertainty to be considered? Or is that the one that was thought to be Vesuvius in GICC05? Please make a short comment.

L775-785: Seems like a promising candidate for the Thera eruption to me. I would also

fall into the wiggle matched age range by Van der Plicht. And also keep in mind the IntCal 20 uncertainty and the GICC21 to IntCal 20 uncertainty. It is briefly addressed in the caption of Fig. 12 but not in the main text. I would not discard it for the lack of layer thinning, without further investigations about the reasons for this thinning in other case (see comments above). Why do you not investigate matchpoint 3 any further? It would also fit into the proposed age range. This section needs a deeper discussion.

Fig. 12: I wonder why nr 1 was picked as a matchpoint and the peak at 3610b2 was not? Seems a little inconsistent to me. Please comment on your choice.

L787: I would not rule it out completely.

L790 I would highly recommend to also investigate the sections around 3610 b2k, not only for tephra, but also for example for sulfate and sulfur isotopes in more cores.

L830ff: That sounds good, I would like to see it!

L843-844: 3300b2k is younger than Thera. I would be careful to still call the offset negligible in that time frame. Please comment.

Technical corrections:

L45: rather “contains” than “is enriched” considering the overall amounts.

L69: Please reformulate. The counting is not done “on the isotopes” but on the signal or the record

L133: Add “ice cores” after Greenland

L357: There are no top and bottom axis, only left and right. Please clarify

L363: change “must be” to “expected to be” or “should be”

L417: change “site” to “vicinity”

Fig. 7: Hard to see the records, maybe broaden the x-axis to improve readability

Fig. 8: Use a different color for Na hat I more distinct from the isotopes

L585: “reason for” not “reason of”

L677: Figure S5, not S4

Fig. 10: The direction of time in plots a, b1 and c1 is counter intuitive. Could you change negative and positive signs of age?

L711: Figure S5, not S4

L713: “of” not “on”

L727: Figure S6, not S5

L728: “reconstruction” not “reconstructions” and “reaches”, “not reach”

L746: L727: Figure S6, not S5

L732-734: “as an additional… “ that sentence does not make any sense in this context.