Climate of the Past Discussions

Clim. Past Discuss., author comment AC2
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Reply on RC1

Giulia Sinnl et al.

Author 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-AC2, 2022

Dear Anonymous Referee #1,

Thank you for a detailed review of our manuscript. Your comments have been very helpful and we hope to have improved our manuscript by addressing your concerns.

The construction of the GICC21 timescale has been a long work and we are glad to see our efforts recognized. We are satisfied to see that all Referees agreed on our uncertainty estimation to be valid.

We agree that the manuscript ended up being quite long, however GICC21 needs thorough explanation and documentation. In response to your comment and to those by An.Ref.#2 and An.Ref.#3, we have concluded that the manuscript will gain more readability by shortening some parts, in order to refocus the manuscript around the GICC21 timescale, i.e. we intend to cut out the discussion of volcanic cooling and Mediterranean volcanic events (sections 5.2 and 5.3, Appendix A and B, and the relevant parts of abstract and conclusion). We will continue the investigation on these topics in our future work. We thank you for the comments about these sections, which we will consider when working onward on these topics.

All ‘technical corrections’ have been included into the revised manuscript.

Our replies to your specific comments:

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

Reply: The evidence from eruption sites such as the Olive branch from Thera. This sentence as well as most of the last paragraph of the abstract will be removed as a consequence of removing sec 5.2 and 5.3.

>>L24: Late Holocene – please give time frame

Reply: From 4.2 ka b2k to today (Walker et al., 2012).

>>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.

Reply: We agree. We have added a few sentences on the matter in the revised manuscript (MS).

>>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.

Reply: We will weaken our statement and include the proposed citation.

>>L52: Reference for high and stable snow deposition?

Reply: The stability of Greenland Holocene climate is demonstrated by the quite constant isotope values (with the exception of the cooling events older than 4.2 ka b2k) (Vinther et al., 2009). The description of "high" accumulation is subjective and we will rephrase it to be about the relative change of layer thickness compared to glacial values (Cuffey & Clow, 1997; Rasmussen et al., 2006).

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

Reply: We apologize that this sentence was not substantiated with proof and references. This observation was made during the dating work, although it did not provide an additional base for the matching. We will add a figure in the Supplement to illustrate the coincidence of the peaks in the two species. Nitrate is described in Legrand et al. (2016) as a potential proxy for bio-mass burning and the frequent co-occurrence of nitrate and ammonium is documented in this publication, hence we will cite their study in this context. However, the use of NO^-_3 as a proxy for bio-mass burning is questioned in that same study because of the influence of other sources, so we refrained from using NO^-_3 peaks as tie-points. We remark, however, that in the absence of NH^+_4 in one ice core because of a data gap, one could use NO^-_3 tentatively, if the other cores showed co-registration of NO^-_3 and NH^+_4 peaks.

(In Supplement: Figure 1 Example of the co-occurrence of NH^+_4 and NO3 peaks in the NEEM ice cores.)

>>L110ff: please add the tritium peak as a radiometric marker

Reply: We will add it.

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

Reply: As you say, a dating bias can be caused by several aspects of the dating process. However, it is hardly ever caused by the counting in itself but rather by mistakes in the layer-identification rules. As stated in Vinther06 and in Rasmussen at al., 2006, the MCE accounts for uncertainties because of layers that were hard to interpret, but it does not account for the bias related to the layer-identification criteria used by each observer. Hence, no account of this type of bias is used to quantify the MCE. In our error estimation, on the other hand, we are somewhat trying to estimate the bias the MCE did not account for, by looking at the systematic undercounts/overcounts produced by StratiCounter. We will expand on this in the revised MS.

>>L175: Please define WDC

Reply: Will be added in the revised MS: West Antarctic Ice Sheet (WAIS) Divide Ice Core (WDC).

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

Reply: The accumulation is given in m ice equivalent.

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

Reply: The resolution of NEEM is 1 mm, and that of NEEM-2011-S1 is 1 cm.

>>L265ff: Depth resolution for GRIP data?

Reply: For the isotopes it’s 2 cm , for ECM 1 cm

>>L275ff: Depth resolution for DYE-3 data?

Reply: For the isotopes it’s 1 cm , for ECM 1 cm

>>L283-290: Nice overview of the data processing steps

Reply: Thank you.

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

Reply: We will explain this better in the revised MS: StratiCounter has an inbuilt pre-processing option that allows the user to select the pre-processing that SC should apply to the data in order to count the layers at the best of its possibilities (e.g. log-transformation of Na and Ca data). By control data we mean the ice-core data between the Laki and Samalas eruptions. The way we selected the appropriate pre-processing steps was by studying which one produced the layer count closest to the true number of years between the two eruptions.

>>L337-340: I don’t really understand the knowledge gain by inverting and log transforming the ECM into pseudo NH4+. 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.

Reply: We observed in our work that log-transformed and inverted ECM aligns well with NH^+_4 peaks, which is a consequence of NH^+_4 neutralizing the acidity of the ice core. The alignments were seen within the same ice core, i.e. between the log-inverted-ECM and NH^+_4 of EastGRIP, NEEM, and NorthGRIP. The use of log-inverted-ECM is hence extended to those cores for which NH^+_4 wasn’t measured.

In the revised MS we will rectify that not all ECM dips are caused by NH^+_4 peaks, since there are other positive ions in ice cores. To explain better, we intend to add an illustrative figure in the supplement, and we will perform a statistical test of correlation.

(In Supplement: Figure 2 Alignment of log-inverted ECM (pseudo-NH^+_4) peaks and true NH^+_4 peaks in the NEEM ice core.)

>>L341: Clarify reference datum

Reply: We will explain this more in the revised MS as follows. Dating the ice cores down from the surface is often not practical or not feasible because the very top layers are made of loose firn and have very variable width. Moreover, most deep-ice-core data start after some depth, e.g. the main EastGRIP core was only drilled from 14 m down. Therefore, we chose a reference datum that is both well established and not too far from the surface, but deep enough for the data quality to be sufficient. The Laki eruption of 1783 CE is well documented in all ice cores and was used as reference datum, by which we mean the absolute date to which the rest of the timescale is anchored to.

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

Reply: We agree and we will make the changes.

>>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 match points are not very convincing, e.g. nr. 5 in EastGRIP and GRIP.

Reply: Because the peak doesn't appear in all cores, not even in years close by. Hence, we did not use it as a tie-point. A gap of 20 years between tie-points such as the one between n. 10 and 11 is not unusual in the timescale. The patterns formed by nr 8,9,10 and 11,12,13 provided a match in which we are more confident. We will remove some tie points such as nr.5 for the sake of clarity of our message: it is patterns of ammonium peaks that are matched, not just single isolated peaks.

>>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.

Reply: We agree and we will be removing the total column.

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

Reply: 0.27/ 0.23 = 1.17. We will make an addition to the table to highlight this percentage.

>>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.

Reply: We disagree with removing this section as we think it provides additional information to the reader about the timescale. By keeping a high correlation to the SWG temperatures while increasing the correlation between GRIP and DYE-3, we demonstrated that the timescale has improved, since a correlation measurement of a long time-series is unlikely to improve by chance. The SWG temperatures (recorded from 1784 CE) should correlate with the isotope signal, according to the finding by Vinther et al. (2010). Since we are exceeding the uncertainty of GICC05 (MCE~1 year), we have to verify if the correlation with SWG is still valid. Moreover, the correlation between DYE-3 and GRIP could have changed in GICC21, so that this is an independent and objective test of the quality of the timescale.

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

Reply: Yes, we are referring to historically dated volcanic eruptions.

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

Reply: We will explain this more clearly in the revised MS. By local effects we mean the effects that influence the layer identification in one core within neighboring tie points: accumulation fluctuations, uncertain layers across all ice cores, local tie-point uncertainty (e.g. a volcanic marker placed differently across cores), measurement gaps.

>>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

Reply: We agree with your evaluation of the uncertainty section being very technical and insufficiently explained. Emphasizing the 2-years lower boundary is important and we will do it.

If we didn't fine-tune the timescale at all, then a convolution of the ice-cores' StratiCounter probability distributions would have been the natural choice of timescale uncertainty. However, systematic offsets of the SC-count were observed and quantified in Table 2: under-count in the "CFA-cores" and overcount in the "isotope cores". That is why we do the fine-tuning in the first place. The SC bias is quantified by how much the probability convolution is off with respect to the fine-tuned result: we quantify this as a systematic error of σ_{SC} = 0.95 years per century, on average. We could have done a separate treatment of the ice-cores, but the average result would still be close or equal to 0.95.

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

Reply: Yes, we stand by this value. We will give more weight to it in the revised text.

>>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.

Reply: We will try to explain this better in the revised MS. In general, we believe there are two main sources to our local error: a StratiCounter error and a random error given by the statistical fluctuations of the ice core data. These should be uncorrelated sources of error and we assume they can be added in quadrature ($\sigma^2_{tot}=\sigma^2_{SC}+\sigma^2_{rand}$). We empirically deduce σ_{SC} from the SC bias evaluation and we find 0.95 years/century. Having performed the fine-tuning, the ice cores' distributions of the likely layer count become centered around the fine-tuned value; we then presume that they can be multiplied together to derive σ_{rand} , which we find to be 0.24 years/century. The fine-tuning we perform resolves issues that could not be solved in a single-core analysis. Therefore, we ask ourselves how much uncertainty there is in our own fine-tuning, which, by account of the high volume of ice-core data compared and the numerous iterations of fine-tuning applied, we speculate is negligible compared to the σ_{SC}. What we mean by this is that if we repeated the whole process of fine-tuning again in a year from now, having forgotten our interpretation of the data, we would very likely reach the same timescale because we would again aim to make the number of annual layers agree in the different data sets. So, we argue that there is low uncertainty in the fine-tuning (which in itself tells us little about the accuracy of the timescale, as there is still a risk that GICC21 is wrong). In conclusion, σ_{tot} = 0.96 years per century, which we round to 1 year.

In conclusion, the entire section L467-508 (i.e. sec 3.5) will be rephrased to make our reasoning clearer to the reader. Following comments by An.Ref. #2, we will also perform a more continuous evaluation of the uncertainty (instead of using only 4 test sections). As a consequence, the exact values presented until now might change slightly.

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

We will consider this datapoint during the re-writing of this section. However, we are not sure we can classify this datapoint as an outlier, since it lies less than 3σ away from the average.

>>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?

Reply: We will expand on this in the revised MS. When considering the time-dependent uncertainty of the timescale, we linearly add the time-dependent part to the (conservative) 2-year contribution given by the delay of volcanic signals and possible mistakes in tie-point placing. The time-dependent contribution is derived from the 1-year per century estimation we have stated previously. Since we've argued that the century-errors are uncorrelated, we apply a quadrature sum: . For convenience, we then hypothesize that the formula can be made continuous, i.e. .

>>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

Reply: The main excursion is at 400 b2k, and we assume the comment is about this excursion. We think it's a coincidence: EastGRIP has a tie-point on a broad-shaped eruption that was mismatched previously (Mojtabavi et al., 2020), and since layers of EastGRIP are relatively thin, already 4 layers had been misplaced. For DYE-3 we observe a layer thickness fluctuation that was already seen in Vinther06. The revision of the tie-points in this region caused the excursion of the timescale offset at 400 b2k.

>>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

Reply: Repeating the study by Adolphi et al. (2016) was considered beyond the scope of this work . To do that we would have to repeat the wiggle-matching of GRIP ^{10}Be data, dated on GICC21, to the IntCal20 ^{14}C production rate. In alternative, we produced Fig 9b, where we show that the datasets for the wiggle-matching are promising in reducing the offset. The results are very likely to be similar to what we already know, i.e. that the wiggle-matched and our independent timescale compare well within uncertainties. The offsets visible in fig. 9b would probably be less than what Adolphi predicted, thanks to a better alignment between the purple and the green curve.

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

Reply: Because it's essentially the same, but we will rectify this in the revised MS. The datasets underlying IntCal20 in this timeframe have not produced substantial changes in the Late Holocene (Muscheler et al., 2020).

>>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 well as the 10Be record are rather complicated, so I would be careful with the conclusion you draw.

Reply: We agree and we will weaken our statement.

(Comments about sections 5.2 and 5.3 have not been answered but will be considered for future work)

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

Reply: This sentence will be removed.

Kind regards,

Sinnl et al.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-155/cp-2021-155-AC2-supplement.pdf