Climate of the Past Discussions

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Reply on CC1

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-AC5, 2022

Dear Prof. Michael Sigl,

Thank you very much for a helpful and detailed comment to our manuscript. We are very pleased to see our work recognized by you and by the Anonymous Referees. Your Nature paper of 2015 has been of great inspiration throughout our work and we are thankful for the comments about the sections of our manuscript that report on your results. We are sorry for any misinterpretations and will make modifications where you have found inaccuracies. We wanted to keep the GICC21 timescale as unbiased as possible, and having the NS1-2011 timescale as an independent reference has been a key element in our work.

We hope that our revised manuscript and the following comments address your concerns satisfactorily.

>>The authors provide a revision of the previous GICC05 age-scale for Greenland ice cores for the past 3,800 years. They combine automated layer recognition approaches with synchronization techniques to develop a dating framework which is both consistent between ice cores and annually dated. This tedious work is of great importance not only for ice-core science but also welcomed in paleoclimatology. Overall, I think the GICC21 timescale is a great improvement of the GICC05 and the previous long standing dating bias has been corrected for this time period. The stacked records of ECM, layer thickness and d18O are very useful paleo-climate proxies with much increased signal-to-noise properties as accessible from individual ice cores. They are used to discuss two time windows suspected to contain the volcanic eruptions of Vesuvius 79 CE and the Minoan eruption of Thera.

Reply: Thank you for recognizing all the key aspects of this work. As we have replied to the Anonymous Referees, we will cut the sections about the stacks and the Mediterranean eruptions for the manuscript to gain more readability and more focus on GICC21. We are grateful for the comments on these sections and we will certainly remember them for the upcoming work that is going to be necessary.

>> Below I provide a list of detailed comments to clarify some uncertainties which are aimed to improve the quality of the presentation of the results. It has been 16 years after GICC05 was constructed for this revision so I assume no additional revision is planned anytime soon for this time period. Thus the documentation of this timescale should be as comprehensive, clear and accurate as possible.

Reply: We agree and we will make sure we do our best at providing all the necessary information for the reproducibility of this work.

>>Before that I would like to summarize four more general points:

A) Input data for annual-layer counting

Ideally, such an effort would include all available suitable ice cores. As presented, it appears to me that you have incorporated only the CFA data from NEEM analyses done in the field, but you have not incorporated the CFA-ICPMS data from the same core analyzed in the trace chemistry lab at the Desert Research Institute on the NEEM Steering Committee piece (abbreviated with NEEM-SC in the SOM file of Sigl et al., 2015). This dataset is published for the time period 500 BCE to 146 CE. Since you here use SC as an abbreviation for StratiCounter I wonder if you may have assumed that the file NEEM-SC describes the StratiCounter results of the traditional NEEM-CFA analyses. It is instead a completely independent analysis on a parallel NEEM ice-core section with state-of-the-art analytical instrumentation. The NS1- 2011 timescale is solely based on manual layer counting using the two NEEM aerosol datasets (see Figure 2 for a comparison of the calcium data) as reported by Sigl et al. (2015), though the co-author M. Winstrup may have done some StratiCounter analyses on the data. Clarification is thus needed what the NEEM-SC COUNT (Figure 6) actually represents. It is probably too late now to include the NEEM data from DRI into an already existing dating framework, but please point out that you haven’t considered all available ice-core analyses, and that there are differences in the input data used between GICC21 and NS1-2011.

Reply: We have aimed at gathering data that is representative for Greenland and suited for layer counting. It will be necessary to weaken all statements that imply that “all” data was used, because we recognized it is not so. We did in fact not use the NEEM-SC data from 500 BCE to 146 CE as input for StratiCounter. We will make the necessary remark in the revised manuscript and underline the differences in input data for NEEM. We will review the fine-tuning of NEEM in the section of NEEM-SC data and keep track of any differences we will find in the process.

B) Absolute age markers

Absolute age markers -- when correctly identified and well dated -- have the potential to improve the accuracy and reduce the uncertainty of an annual-layer dated chronology.

However, if they are ambiguous and in cases erroneous, they may also contribute to manifest in a dating bias (e.g., as was demonstrated on the examples of Vesuvius 79 and Hekla 1104; or the infamous 1453 Kuwae eruption to which ice-core chronologies from Antarctica had been tuned for many years). Finding the best balance is difficult and can be subjective. For NS1-2011, I have been trying to use minimal age constraints (1258, 939, 775, 626, 536) and let StratiCounter define the annual layers in between. I noted that key volcanic age markers discussed in the literature are exactly reproduced by GICC21, such as 536, 626 and 939 CE (Sigl et al., 2015); Okmok II in 43 BCE (McConnell et al., 2020) and Aniakchak II in 1629 BCE (Cole-Dai et

al., 2021). Since overall, there are quite some age differences between the different ice cores, I wonder if these ages have been prescribed when constructing the chronology?

On the other side, it appears that the ^{10}Be anomalies identified by Sigl et al., (2015) which anchor the NS1-2011 chronology (see Figure 1) have not been used to constrain GICC21 for those ice cores for which this data exist (NEEM-2011-S1, NGRIP). Specifically, if I transfer the GICC21 ages to the only available annually dated ^{10}Be data (i.e. cutting the samples along the annual-layer boundaries) from NEEM-2011- S1, it appears that the ^{10}Be rise had started in 773 (see Figure 1), a year before the SPE event was supposed to have occurred in boreal summer 774 (Buntgen et al., 2018). A shift of GICC21 +1 year (consistent with NS1-2011) would also bring inline a sharp and short-lived sulfate spike in 800 CE with a short-lived cooling recorded by tree-rings in 800 CE (Sigl et al., 2015).

Another potential and frequently used age marker is radioactive fallout from nuclear weapon testing (peaking 1954-1963) which has been identified for some of the ice cores used in GICC21 (Arienzo et al., 2016). Have such age markers been considered for GICC21? This might also be used to validate the potential to use NH4 for age synchronization, because there are prominent biomass burning signals in Greenland ice cores in 1961 and 1964 (Legrand et al., 2016; McConnell et al., 2007). A precise and accurate chronology is in particular valuable for the most recent period due to the overlap with observations, reanalysis and remote sensing.

Reply: We did not prescribe the age of any age markers during the construction of our chronology, except for the Laki eruption, 1783 CE, and the Öræfajökull eruption, 1362 CE (but only for DYE-3, since the other cores have a very low ECM signal at this age). StratiCounter and the fine-tuning were operated as to independently assess the age of each and every layer, disregarding any information about the historical age of the markers. We thereby prove that a close comparison of a high volume of ice core data contains all the necessary information to date the tie-points, but that single ice cores sometimes fail to do so. This is true for Greenland but might not be relevant for Antarctica. As for ^{10}Be tie-points, we aim at keeping the Greenland ice core timescale as independent as possible from tree rings. We argue that 1 year inaccuracy can be explained by our uncertainty. Of course, we will again verify if NGRIP and NEEM-2011-S1 are well matched with regards to the ^{10}Be tie point, which we believe is already the case. But, similarly for what we do for the 2610 years BP ^{10}Be peak (O'Hare et al., 2019), we consider these tie points to be an independent evaluation of the ice-core timescale agreement to tree-rings.

C) Relative age markers: volcanoes versus biomass burning

For the alignment of the ice-cores in between the constraints of the 105 major volcanic eruptions you make extensive use of NH4 (some 240 major tie-points according to the SOM) which is among other sources often emitted during biomass burning. The low number of widely accepted volcanic tie-points and the high number of NH4 tie-points, which to my knowledge hasn't been used before to align ice cores across the Greenland ice sheet, are the biggest surprises in the manuscript. I wouldn't go so far to reject the idea that many or even the majority of the NH4 tie-points are correctly tying together the corresponding biomass burning episodes, at least for ice cores located closely to each other, e.g. NGRIP and EastGRIP. But I don't find the rational and supporting data convincingly laid out in this manuscript. If biomass burning events indeed left a unique fingerprint in the NH4 throughout Antarctica, you should be

able to validate this using independent biomass burning tracers (such as black carbon) not used for synchronization. There is a continuous 2,500 year record of black carbon available from the NEEM-2011- S1 and NEEM ice cores (Sigl et al., 2013; Sigl et al., 2015) cited by (Zennaro et al., 2014). Can you confirm in the NEEM ice cores the biomass burning source for your major NH4 tie-points by co-registration of BC peaks, considering that black carbon is in the pre-industrial atmosphere a unique fire tracer?

Whereas black carbon records are numerous in Antarctica they have never been used for chronological synchronization purposes owing to the large spatial and temporal variability of black carbon over Antarctica (Liu et al., 2021; McConnell et al., 2021). This doesn't mean per se that it couldn't be possible in Greenland but the burden of proof lies on the side of the authors.

Reply: We agree with you and the Anonymous Referees when you say our proof and reference list about NH^+_4 is incomplete and that we need to expand more on the topic. We remark that the method of NH^+_4 -matching has been used before (by Rasmussen et al., 2008 and by Legrand et al., 2016). In the revised manuscript we will provide a more exhaustive description of why we believe NH^+_4 peaks to be a supportive matching tool for Greenlandic ice cores, secondary to volcanic tie points but still better than basic interpolation across eruption signals. The black carbon record of NEEM-2011-S1 confirms our take about NH^+_4 peaks in Greenland ice being closely related to burning events, as can be seen in the figure below, but we cannot exclude some NH^+_4 peaks to come from other sources, i.e. there are some NH^+_4 peaks that don't correspond to BC peaks. We will aim to review our NH^+_4 tie-points to verify if the co-occurrence with other species such as BC can better validate them as a tie-point between distant ice cores. As a preliminary test, we saw that most of our NH^+_4 tie points also correspond to BC peaks in NEEM-2011-S1.

(In Supplement: Figure 1 NEEM-2011-S1 Black Carbon and NH^+_4 comparison. The blue bars are not GICC21 tie-points but merely indicate the co-occurrence of NH^+_4 peaks with BC peaks.)

D) Thera eruption

In advent of ongoing developments in the radiocarbon community and with the revised dating and stacking of relevant proxy records at hand, new insights may indeed be forthcoming; however, we need to be careful to not over-interpret the results. A minimum in stable isotope values in Greenland may have many explanations, including that of a major volcanic eruption. But in the absence of evidence of a clear stratospheric eruption signal in the ice cores (i.e. increased sulfuric acidity deposition over several years uniformly over Greenland) a volcanic source is the least likely explanation for the assumed cooling, let alone that is was specifically the Thera eruption. The Thera eruption has already previously been tied to a major cooling event in 1627 BCE, which subsequently was revised. This period interesting and worth reporting but I would try to emphasize the speculative nature of the current appraisal of the evidence.

Reply: If we were to include this paragraph in the revision, we would consider a rephrasing of our claim. Our effort in proposing a valid alternative to the 1627 BCE will be continued in our future work, unless of course someone finds Thera in an ice core before we do so. We will investigate more if the cooling event at 3600 b2k (1600 BCE) can be explained by other means.

Our replies to your Specific comments:

Abstract:

>>L. 4 & l.10: Please be specific which ice-core chronology was revised (i.e. GICC05); there are several annual-layer counted chronologies for ice cores from Greenland notably from GISP2 (Meese-Sowers), NEEM-2011-S1 (NS1-2011) and NGRIP2 (DRI_NGRIP2). The latter two do not have a dating bias, and the one from GISP2 has not been assessed in your paper.

Reply: Thank you, we will edit that.

>>L. 16: The statement that "cooling lasted for up to a decade, longer than reported in previous studies of volcanic forcing" is incorrect. There is a large body of literature reporting that volcanic cooling may have lasted for up to a decade (Büntgen et al., 2020; Büntgen et al., 2016; Sigl et al., 2015; Tejedor et al., 2021), some of which you are citing in l.37-38.

Reply: Thank you for the information, our statement would have to be limited to Greenlandic ice cores.

>>L. 20-21: This is speculation. There is no evidence of a major stratospheric volcanic eruption at the time in any ice core so this stable isotope anomaly is most likely not related to volcanic activity. If at all this speculation belongs into the discussion rather than in the abstract.

Reply: We agree.

Main text:

>>L. 25-26: I would believe this statement is controversially discussed in the field of human history and difficult to prove either wrong or possible.

Reply: We will consider rephrasing our opening statement.

>>L. 59: Na^+ instead of Na^{2+} (happened a few times throughout the paper)

Reply: Thank you, it will be corrected.

>>L. 59-63: How are the seasons of the impurity peaks inferred?

Reply: In our description, they are inferred relatively to the Na^+ peak.

>>L. 68-68: Counting can also be done on insoluble parameters.

Reply: That is noted and will be added to the introduction.

>>L. 80-81: Whereas the use of sulfate for age synchronization is common in the field, the use of biomass burning events has to my knowledge never been used for this purpose, unless for ice cores drilled a few meters apart. Maybe this could be better highlighted by citing relevant literature.

Reply: We will report our method in more detail.

>>L. 80-87: the wording is a bit imprecise in this section: with sulfate you can't "locate individual eruptions" but rather identify eruption signals in the ice strata; the duration of volcanic sulfate deposition is highly variable and depends on many factors, there are numerous sulfate depositions lasting less than a year, though these signals are usually not used for volcanic synchronization purposes, because they are not equally widespread and uniformly deposited over the Greenland ice sheet. I would suggest replacing "event" with "deposition signal" to make clear that you don't refer to the start and end of the eruptions which you also called events in l.80.

Reply: Thank you for your correction, we will edit that.

>>L. 89-90: Consider adding the relevant tephra papers which overlap with the age range of GICC21, the

last 3,800 years. I haven't found any mention of tephra in the methods and results sections, except a link to the Laki 1783 eruption. Have you been using the available tephra evidence to constrain the absolute dating (e.g. Veiðivötn Feb. 1477; Tianchi, Nov. 946) or relative dating (e.g., Churchill 853; Okmok II 43 BCE) of the ice cores in GICC21? If none of the tephra has been used to constrain or evaluate the GICC21 timescale, why mention it in the introduction of the paper?

Reply: Except for a few deposits that constitute the reference datum of the chronology (Laki and Öræfajökull), we did not make use of any other tephra tie-points with a constrained age. However, tephra knowledge has been useful in recognizing some tie-points that were sure to be the same across Greenlandic ice cores and as a validation of the timescale at a later stage. Hence, we will make this remark in the paper and reference the tephra-data that were most useful for establishing the ice-core match.

>>L. 105-107: the literature research on the ability of ammonium or other biomass burning proxies in ice core for synchronizing ice cores across the Greenland ice sheet is incomprehensive. There is a vast body of literature analyzing the extent to which biomass burning left unambiguous, reproducible signals between different proxies (e.g. ammonium, black carbon, vanillic acid) and/or between different ice core locations in Greenland or Antarctica (Keegan et al., 2014; Legrand et al., 2016; Liu et al., 2021; McConnell et al., 2007; Zdanowicz et al., 2018). Legrand et al. (2016) for example only identified 9 biomass burning events with synchronous NH4 deposition in a 200 year-window using three precisely dated high- accumulation ice cores.

Reply: Thank you for the information, we will deepen the description of this part of our methodology. We are aware of the review by Legrand et al., and we will add more proof that NH^+_4 can be used for ice-core matching. As we replied to An.Ref.#2, we by no means think that NH^+_4 is better than volcanic matching, but we think it is a valid candidate for a secondary matching tool. We will add a figure in the Supplement to show more examples of the NH^+_4 peak patterns we have used in our work.

>>L. 110-115: Have these events been used for the GICC21 chronology? If so, it would be valuable to provide a table with the ages and corresponding depths for each of the ice cores in which 10Be anomalies have been identified. I have been plotting the only available annual 10Be data from the NEEM-2011-S1 ice core (Sigl et al., 2015) on the GICC21 chronology and note that the 10Be rise occurred in 773 CE a year before the solar proton event has occurred.

Reply: Thank you for verifying the 774 CE event. We will investigate why the ^{10}Be in GICC21 has occurred 1 year before the tree-ring expected date, even though this can be explained by uncertainties. We will not make any changes to the timescale unless we find a very clear layer incongruence that caused 775 CE to be anticipated in our chronology. Again, we would like to be as independent from tree-rings as possible.

>>L. 131: Incorrect ice core stated here: "we identify tephra particles and determine that volcanic shards extracted from a depth of 429.3m in the GRIP ice core are likely due to the 79AD Vesuvius eruption". (Barbante et al., 2013)

Reply: Thank you, we will edit that.

>>L. 132: Very vague statement. Recent geochemical analyses of tephra shards from the NEEM-2011-S1 directly associated with the large acidity peak demonstrate that the peak is not linked to the Vesuvius eruption but points to other potential sources including from Alaska. The tephra from NEEM-2011-S1 is geochemically distinct from the shards described by Barbante at al. (2013) which have no direct stratigraphic context with the acid peak, but appear a year earlier. Note that the cited Discussions paper is now in press.

Reply: We will add more details to our statement.

>>L. 133: I think the synchronization has never been an issue here. The sulfate peak is large and clear. It is the association of the peak with a historic event and the subsequent transfer of its supposed age and uncertainty into the GICC05 framework which were causing an issue.

Reply: Thank you, we will clarify that there is no issue of synchronizing ice cores, but the attribution of Vesuvius is what caused the problem.

>>L. 134: The issue of a potential age bias in the GICC05 chronology has also been raised by e.g. (Baillie, 2008, 2010; Lohne et al., 2014, 2013; Torbenson et al., 2015).

Reply: Thank you for the information, we will add the suggested references.

>>L. 137-138: This is incorrect and needs to be corrected**.** Coulter et al. (2012) was targeting the supposed Hekla 1104 period in three ice cores (Dye-3, GRIP, NGRIP). No tephra was found in Dye-3, and GRIP but four shards were identified in NGRIP (QUB-1186). However, these are situated c. four years before the onset of the massive sulfate peak which had been previously attributed to Hekla 1104 eruption in GICC05. We have also targeted this sulfur peak in the NEEM-2011-S1 and TUNU2013 ice cores (using large cross sections) and have not found any tephra, as summarized by Plunkett et al., (in press). Consequently, the NS1-2011 (Sigl et al., 2015) and DRI_NGRIP2 (McConnell et al., 2018) annual-layer counted chronologies have not been constrained by this erroneous match to the historic Hekla 1104 eruption. Moreover, no sulfur isotope measurements have been reported from any Greenland ice core in the literature. A recent study focusing on this eruption signals in ice cores has suggested that multiple eruptions, including an eruption of Asama in 1108 may have contributed to the distinctive signal in the Greenland ice cores (Guillet et al., 2020).

Reply: Thank you for the clarification, we will edit our statement to include this information.

>>L. 161-163: To clarify, the new NS1-2011 chronology (Sigl et al., 2015) constructed for the NEEM-2011-S1 ice core is between 1258 CE and 2013 CE identical with the previous age model for this ice core which was constrained by volcanic eruption dates taken from GICC05 and annual-layer counting in between these marker years. Before 1258 CE, the NS1-2011 chronology was no longer constrained by GICC05 including the previous matches to Vesuvius 79 CE and Hekla 1104 CE, but was instead constrained by three well-dated observations of volcanic dust veils from documentary sources (i.e. 536, 626 and 939 CE) and the solar proton events of 774 and 993 CE. Between these marker events, StratiCounter was used to identify annual-layer boundaries in the multi-parameter impurity records. Other than Samalas, no bipolar tie-point is employed to constrain NS1-2011 before 1258 CE.

Reply: We will rephrase the introduction to this paragraph to be more precise.

>>L. 161-163: Sigl et al., (2013) manually interpreted annual layers in NEEM-2011-S1 in between the prescribed GICC05 dated volcanic markers. Sigl et al. (2015) used StratiCounter on the same NEEM-2011- S1 dataset after replacing the GICC05 dated volcanic markers with the new tie-points as described above.

Reply: Thank you for the information.

>>L. 164-165: To clarify, the 15 volcanic match-points between NGRIP and NEEM referred to here are only for the time period 88 CE until 500 BCE. For the time period for which NGRIP sulfate data was available (190-1999 CE) we synchronized the NGRIP sulfate data to the NS1-2011 chronology using 123 volcanic tie-points identified in NGRIP and NEEM-2011-S1.

Reply: We will report this in the revised manuscript.

>>L. 166: The number of years counted in the aerosol records from the two independent NEEM core analyses was c. 590 years. How about “were conducted until 500 BCE”?

Reply: Thank you, we will rephrase our sentence.

>>L. 182/83: Are you sure you used all available data from Greenland deep ice cores? Based on section 2.3, you used only the measurements done on the main NEEM core in the field by traditional CFA, but you omitted the data from the trace element analyses done on NEEM between 500 BCE and 146 CE at the Desert Research Institute, which is the data mainly used for the NS1-2011 chronology before 88 CE. Analyzed with two high-res. mass spectrometers in a class 100 clean room, this data is ideally suited to resolve intra-annual variations in multiple impurities records (e.g. Sigl et al., 2016; McConnell et al., 2018) and less likely to be contaminated during data acquisition compared to the CFA field analyses (see

Figure 2, which I want to emphasize is not representative for the entire NEEM CFA analyses). Also the

deep GISP2 ice core has been dated by annual-layer counting.

Reply: We did not, and we will weaken our statement. We will verify the agreement of the NEEM-SC data with the field-CFA data, in order to check once more that the fine tuning in the relevant timeframe is consistent, and include this in our revised manuscript. We also intend to compare the GISP2 with GICC21, but we did not include it in our timescale because of the proximity to GRIP and the lower quality of the ECM after 300 m.

>>L. 214-215: In which ice cores was tephra found? The Coulter et al. (2012) reference is missing here describing the tephra results from NGRIP. Trace elements confirming the link to Thera are presented in Plunkett et al. (2017).

Reply: This sentence is going to be removed.

>>L. 233-251: Your description of the datasets is a bit imbalanced. You could elaborate a little on the NorthGRIP2 dataset, which just like EastGRIP include a vast range of species measured continuously at high resolution, but at a site with almost twice the accumulation rate of EastGRIP. Since GICC21 closely follows the independent annual-layer counted DRI_NGRIP2 chronology (McConnell et al., 2018) for most part, it seems reasonable to assume that this new record is the cornerstone of the new chronology.

Reply: Yes, this dataset was important to have a continuous dataset of the NorthGRIP site. We describe NGRIP2 a few lines on, we will make sure to highlight its importance.

>>L. 333-340: I am still missing when you lay out your arguments (ideally backed up with data and/or relevant publications) why you assume that NH4 peaks are synchronous across Greenland with a frequency of 0.1 years^{-1}. Are these NH4 peaks thought to be from individual biomass burning plumes? Is there observational evidence for such widespread deposition? For volcanic eruptions the long atmospheric lifetime (especially following stratospheric eruptions) provides very distinctive sulfate signals regarding duration, magnitude and shape. Therefore, their use is well established in the ice-core dating community. For NH4, a similar framework is still missing, and the data as presented in the paper doesn't yet convince me that the additional use of NH4 will be better than a tight volcanic alignment with annual-layer counting in between. In the past 2,500 years, the frequency of volcanic eruptions detectable in Greenland ice cores is 0.09 years^{-1} (Sigl et al., 2015).

Reply: We don't go as far as speculating on the origin of the NH^+_4 spikes, for which we will gather more literature in the revised manuscript. We simply noted that between volcanic tie-points that are widely spaced (there are some parts of the timescale where the frequency of adequate volcanic tie-points drops to around 1 in every 100 years) one could clearly see distinctive NH^+_4 patterns that were used as supportive evidence for the layer fine-tuning. We agree with you that sufficient proof of this observation was lacking and we will prepare example figures.

>>L. 380-381: Necessarily, if you aim for a unified, synchronized chronology for several cores, subjective decisions need to be made. And these subjective decisions can become influenced by prior knowledge that we have. For example, on what we know about the dating bias towards too old ages in GICC05 throughout the Holocene. The age offsets between ice-core 10Be and tree-ring ^{14}C are well established. Frost-ring events potentially linked to volcanic eruptions have been proposed in the last years. Can you comment on steps taken to ensure such prior information has not influenced the manual fine tuning?

Reply: Yes, subjective bias is indeed an issue and we have taken multiple precautions to minimize the problem, which will be reported in more detail in the revised manuscript.

Firstly, we think one important aspect has been that we completed multiple iterations of our fine-tuning in separate sessions, often targeting non-subsequent sections of the timescale. Only some of the co-authors were directly involved in the fine-tuning process. Giulia was the one keeping the overview, and did one whole fine-tuning iteration of the timescale by herself. Then Sune and Mai became observers and they repeated the fine-tuning together with Giulia in many meetings, either with one or both of them. We (as the fine-tuning team) often went over the same depth intervals multiple times, especially when having doubts. But we always started from the original StratiCounter layer count, recording the results of our fine-tuning iterations in a logbook, from which we could see that the same fine-tuning results were being reported over and over, thus reducing the uncertainty of the fine-tuning in itself. The two additional observers, Sune and Mai, would be exposed to a quite random problem of ice-core fine tuning every time. By this we tried to minimize the bias of the two more experienced observers, who might have had some personal bias about the topic.

Secondly, for ages older than Samalas, we only checked on the expected or reported ages of e.g. volcanic eruptions in the very last stages of the project.

Thirdly, we did not constrain any of the ages of the timescale except for the reference datum. We were only influenced by some very known eruptions younger than Samalas (e.g. we knew Katmai could only be in 1912 and not in, say, 1915 CE), but otherwise all ages older than Samalas were estimated as independently as possible from known ages. That is why the uncertainty estimation is so important to us, because as much as we would like our timescale to be accurate, we cannot be sure we did not misinterpret a layer or a volcanic tie-point somewhere in the timescale.

>>L. 383-84: I would have limited this exercise to volcanic eruptions for which tephra was clearly identified in ice cores, therefore to my knowledge excluding Hekla 1510. Typo for the 1362 eruption. How did you identify the Oræfajökull 1362 eruption in the EastGRIP, NorthGRIP, NEEM and NEEM-2011-S1 ice cores? There is hardly any sulfate peak in many ice cores, nor was the tephra identified in the GRIP ice core (Coulter et al. 2012) associated with an ECM peak.

Reply: Öræfajökull is not clearly identifiable in many ice cores, as you say, but it was nonetheless an important eruption for the timescale because of the presence of tephra in DYE-3. We will consider editing the table to include better resolved eruptions.

>>L. 471: Do you argue that the uncertainty in GICC21 is never below 2 years?

Reply: Yes, since depositional dynamics might delay volcanic signals we need to accommodate for some minor stretching of the timescale. We state 2 years as a conservative general rule, even though one could argue for a smaller uncertainty close to a small number of eruptions characterized by sharp ECM peaks e.g. from Iceland. Similarly, one could use solar events to constrain the timescale down to one year or less, assuming that the tree-ring chronologies are accurate. We have chosen the conservative 2-year estimate.

>>L. 519: Why is your frequency of volcanic tie-points so low compared to the frequency of eruptions in Sigl et al. (2015), 1 every 11 years?

Reply: Because not all eruptions make good inter-ice-core tie-points (e.g. Öræfajökull). In this paper we don't attempt to make an exhaustive record of all eruptions ever recorded in ice cores, we focus on the synchronization problem.

>>L. 544: Figure 6: It seems that the comparison of GICC05 and NS1-2011 has been omitted between 87 CE and 187 CE creating an apparent gap in the data, which doesn't exist. Is this correct? GICC05 ages of NEEM-2011-S1 are available by Sigl et al., (2013) and the corresponding NS1-2011 ages are published in Sigl et al., 2015 together with a transfer function between NS1-2011 and GICC05.
https://www.nature.com/articles/nature14565#Sec24

Reply: Thank you for pointing this out, we agree with you that the gap of the NS1-2011 chronology is apparent. We have figured out a way to solve the issue, which depends on

NEEM-2011-S1 not being plotted in this figure. In the supplement to Sigl15, we found the NorthGRIP 1 match to NEEM (down to 190 CE), and the new NEEM data (from 146 CE to -505 BCE). The transfer function to GICC05 is reported until 190 CE, and a set of tie-points between NorthGRIP and NEEM is reported from 146 CE to -505 BCE. Hence the gap of 50 years.

We will use the NEEM-2011-S1 dated on GICC05 (from the Sigl et al., 2013, supplement) and compare it to NS1-2011 and GICC21 ages. This should close the gap in the figure.

>>L. 548: Please add reference for the NS1-2011 chronology (Sigl et al., 2015).

Reply: Yes.

>>L. 553: Wouldn't a potentially wrong NH4 tie-point also produce a drift between the ice cores?

Reply: Yes, we will rephrase 'volcanic match is different' to 'ice-core match is different'.

>>L. 591: There are no 130-year long periods without any volcanic eruption. To clarify: Do you mean you don't see a volcanic eruption in any of the cores or a volcanic eruption signal which can be detected in all of the cores? Using established methods and the NGRIP2 chemistry data (McConnell et al., 2018), volcanic eruptions can be detected on average every 13 years, with the longest repose period lasting less

<60 years.

Reply: Thank you we will rephrase this sentence. We meant the spacing between adequate multi-core tie-points, not a lack of eruptions.

>>L. 598-99: This may be true, but it doesn't mean per se that several ice cores are always better than a single ice core (as the previous age bias in the GICC05 ice cores has shown). Note, there is no offset between the GICC21 and DRI_NGRIP2 chronology for almost 1,500 years (i.e. between 200 BCE and 1270 CE, Figure 3) despite the latter being based on a single ice core (NGRIP2), and only four of the annual- layer decisions in GICC21 deviate from those from the previous annual-layer-counting age model (McConnell et al., 2018). The observed offset is more likely attributable to the reduced core quality of NEEM-2011-S1 is some sections around 300 CE.

Reply: Thank you for this remark, the reduced core quality is also a valid explanation and we will add it to our sentence.

>>L. 661: better to write following all selected (or all 105) eruptions. The number of eruptions detectable in Greenland is much larger (i.e. Sigl et al., 2015 detected 221 eruptions in Greenland in only 2,500 years).

Reply: Yes.

>>L. 674: Have you accounted in your stacked analyses for the fact that volcanic eruptions often formed temporal clusters (e.g. 536/540, 1453/1459, 1809/1815), and that an apparent persistency (i.e. your secondary dip) may be an artifact caused by secondary major eruptions? In tree-ring research on the volcanic climate response, this has been taken into account (Büntgen et al., 2020). In Plunkett et al., (in press) we demonstrate that a decadal scale reduction of d18O from Dye-3, GRIP and NGRIP is explained by the cooling response following a cluster of eruptions dated 536, 540 and 547 CE.

Reply: We have checked that close eruptions did not affect our results significantly, by separating clustered eruptions and only averaging the signal of the latest one in the cluster. In the end we chose to compute all eruptions in the stack, but this topic will be of course be addressed in our future work on this matter.

>>L. 717: I am not sure one can cite a personal communication from a co-author. The most comprehensive and recent review article of tephra deposits in the Holocene in Greenland is (Plunkett and Pilcher, 2018) which also addresses the situation for tephra from the Mediterranean.

Reply: We will refrain from citing pers. comm. from co-authors in the future.

>>L. 724: In 2015, no annually dated Northern Hemispheric temperature reconstruction was available, thus we created the N-Tree composite. In the meantime, more comprehensive reconstructions have become available and have already been used to demonstrate the accuracy of the NS1-2011 chronology in the Common Era (Büntgen et al., 2020). Since NS1-2011 and GICC21 are close to each other, I would assume that these results would look the same for GICC21.

>>L. 726: eruptions identified in Greenland ice cores.

Reply: We will edit this.

>>L. 731: Have you analyzed relevant sections from any of the ice cores encompassing 79 CE for the presence of tephra?

Reply: No, we did not.

>>L. 737: I miss the relevance of the radiocarbon dating of material in context with the exactly dated 79 CE Vesuvius eruption for the problem of identifying volcanic fallout from this eruption in Greenland. I suggest that the point would be better removed, to bring the focus more back to the ice core chronology.

Reply: We agree that this paragraph can be removed. The intention was to compare Greenland with evidence close to the eruption site, as a proof of concept for the following Thera paragraph.

>>L. 762: Please provide relevant references here.

>>L. 767: Here and again later (e.g. l. 854) you refer to the 3560 year b2k ice-core anomaly. In the supplementary data the rise of the ECM signal is in 3562 year b2k, and the peak in 3561 year b2k. Can you please clarify the age of the eruption signal in GICC21?

Reply: The exact age we found for the rise is 3561.4 years b2k (-1562.4 BCE). We think that for most cores the signal is contained within one year, but it is appears that EastGRIP has a smeared peak that lasts into the next year.

>>L. 773: I think it would be appropriate to cite in this section the pioneering work of (Baillie, 2010) who noted a number of the mismatches between tree-ring anomalies and ice-core acidity peaks which now appear to have been corrected.

Reply: We will keep this in mind.

>>L. 785: If the assumed 10-year cooling would have been caused by a volcanic eruption it would have been through the stratospheric sulfate aerosols. The sharp and short-lived (1 year only) ECM spike characterized by strong spatial variations in Greenland is more likely the result of a tropospheric eruption rather than a major stratospheric eruption. There is no credible evidence in the ECM data and available sulfate records from Greenland (GISP2, (Zielinski et al., 1996)) around 1610 BCE of any significant stratospheric sulfur injection. Linking the drop in d18O to a volcanic eruption is thus only speculation. Internal variability, changes in atmospheric circulation and moisture sources and/or solar activity are just as likely candidates to explain the d18O feature.

Reply: Thank you for checking this, we will investigate further on the isotope drop in the future.

>>L. 788: It would be helpful for future research if you provided (if possible) metadata on which ice cores and which cross sections (cm^2) have been sampled continuously in this time window. To my understanding, NGRIP ice core has been fairly extensively studied by Coulter et al., (2012); GRIP was studied by Hammer et al (1987). NEEM, EastGRIP may have been studied by Eliza Cook, cited before as

personal communications and this paper would greatly benefit from more specific detail about the nature of any tephra work contributing to GICC21. The new timescale revision will certainly stimulate new studies around this time period, and it would be helpful to know the state-of-the-art regarding tephra investigations in this critical period.

Reply: We will consider making such a file for future studies on the matter.

>>L. 818: This additional ±2 year uncertainty could be reduced for a number of events for which (1) atmospheric transport times are short (<weeks), (2) the timing of tephra arrival is well documented by high-time resolution particle records and (3) independent age constraints (e.g. dendrochronological, documentary or tree-ring evidence) can provide sub-seasonal information. I would argue that Veiðivötn (Feb. 1477 (Abbott et al., 2021)), Tianchi (November 946 (Oppenheimer et al., 2017) whose tephra in Greenland is reported by Sun et al. 2014) and the solar proton events in 993 CE, 774 CE and 660 BCE can all be dated with uncertainties less than ±2 years, and the list could be expanded including Eldgjá 939, unidentified volcanic eruptions in 626, 536 CE (Sigl et al., 2015) and the

eruption of Okmok II in winter 44/43 BCE (McConnell et al., 2020). I have, however, no objections if you are leaning towards a more conservative approach regarding the uncertainties for these events.

Reply: Thank you for this comment, we will consider this in our revision.

>>L. 823: A multi-ice core comparison is favorable rather than essential (see Figure 3). I would further argue that the single ice-core WD2014 chronology is more accurate than the multi ice-core GICC05 chronology over the Holocene (Sigl et al., 2016). It is the quality of the expression of the intra-annual variations that matters most, not the number of ice cores.

Reply: Yes, the quality of the annual signal is a key ingredient for the ice core timescale. We will rephrase ourselves to include your point. We produced our timescale despite ice core data quality of single ice-cores preventing us to make a single-ice core timescale such as WDC2014. Hence, the number of ice cores is an important feature for the Greenlandic timescale, especially because we continued beyond the 2500 b2k age limit of the NS1-2011 chronology and therefore had much less historical evidence to rely on. For the future of the GICC21 revision, we need to use as much as possible of the existing data.

>>L. 853: Typo: Droughts.

Reply: Yes.

>>L. 911: All datasets used for the timescale should be made publicly available at high depth resolution to allow for independent validation of the timescale. This was not possible for GICC05, but is now mandatory under FAIR open data principles.

Reply: We understand your concern and we will make sure that in the revised manuscript we explain more clearly where data can be found. We emphasize that all data will be available to the reader on Pangaea, but as Pangaea currently has a rather long processing time, not all data sets may get a doi in time for inclusion in this manuscript. For NEEM and NorthGRIP CFA data, the Pangaea repository listed (https://doi.pangaea.de/10.1594/PANGAEA.935838) already contains data of all the species we have used in our work (ECM, Na, Ca, NH+4, NO3), and the moratorium will be lifted as soon as the corresponding ESSD paper is published.

As for EastGRIP data, the ECM has since been made available at https://www.iceandclimate.nbi.ku.dk/data/. For the EastGRIP CFA, Erhardt will make a Pangaea repository, but preliminary data can be obtained by Erhardt.

The two other ice-cores (DYE-3 and GRIP), for which we only used ECM and isotopes, already have published ECM and decadal-resolution isotopes, and we will make sure to refer to those publications in the revised manuscript. As for the full-resolution isotopes and ECM data from GRIP and DYE-3, they will be published separately by Rasmussen in a paper that makes data from GICC05 and GICC21 available. The files have been submitted to Pangaea.

Authors who want to access data sets which are not published when this manuscript is published, will be able to get the datafiles from the author team (with preliminary metadata, as the metadata is subject to review by Pangaea editors).

Kind regards,

Sinnl et al.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-155/cp-2021-155-AC5-supplement.pdf