

Clim. Past Discuss., author comment AC2
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Reply on RC2

Julien Westhoff et al.

Author comment on "Melt in the Greenland EastGRIP ice core reveals Holocene warm events" by Julien Westhoff et al., Clim. Past Discuss.,
<https://doi.org/10.5194/cp-2021-89-AC2>, 2021

REPLY: Dear reviewer,

Thank you for a detailed review of our manuscript. Your comments will greatly help improve our work and the impact on the readers.

The reply to each comment will follow below.

If you agree to these changes, then we will upload the revised version of our manuscript.

Best regards,

Westhoff et al.

COMMENT: Westhoff et al. present a Holocene melt record from the new Greenland EGRIP ice core. Melt features are identified visually, and divided into melt layers and melt lenses. The authors discuss the recurrence intervals and thickness record of past melt. They compare the melt record to NH tree ring records. They further speculate that a particularly warm Greenland summer of 986 CE may have triggered Norse colonization of Greenland.

COMMENT: The paper represents a lot of detailed analysis, and a very valuable dataset. The work is certainly suitable for Climate of the Past. However, I think that both the writing and analyses can be improved prior to publication.

COMMENT: Throughout the distinction between melt layers and melt lenses seems somewhat arbitrary. Is a melt layer not simply a melt lens that is larger than the core diameter? Also as the coffee experiments clearly demonstrate, the refreezing of melt water is highly irregular, and so a melt layer does not necessarily reflect a larger melt event than a lens does.

REPLY: Thank you for the suggestion. We have combined the melt layers and lenses into melt events, after the first figure in the results.

COMMENT: Therefore, it is unclear to me why the authors analyze these type of melt indicators separately. For example, in Fig 9 where they show the interval between layers,

and that between lenses. To me, the most (only?) interesting statistic would be the interval between melt events (either lens or layer).

REPLY: Thank you for pointing this out. We agree and the melt layer and lens spacing plot will be moved to the supplements. We will include a combined plot (layers and lenses) into the main text.

COMMENT: The difference between certain and uncertain layers is made, but not used meaningfully in the analysis. The authors report Holocene trends, but these trends are only visible when including the uncertain layers – if not, the trends are actually opposite (increasing melt through the Holocene, instead of decreasing). Given that these layers are labeled “uncertain”, it seems to me we should be very cautious in interpreting these. Could you show a number of pictures of layers/lenses that are “uncertain”?

REPLY: We added examples of uncertain melt layers to the supplement and addressed more caution when drawing interpretations from these uncertain events.

COMMENT: The site elevation history seems key to understanding the EGRIP melt record, given that it has dropped so much in elevation. Given important this elevation signal is relative to the HTM signal, both an increasing or a decreasing Holocene melt trend could be expected. This is not meaningfully discussed or evaluated.

REPLY: To better discuss the effect of site elevation change, we have moved the second half of the “The highly dynamic EastGRIP site” section from the introduction to the beginning of the discussion. We also include an overview of drill site elevations, including latitude and 10 m borehole temperatures to better discuss the effect of elevation on temperature.

COMMENT: Throughout the authors can make better use of statistics in making various claims about periods with strong or weak melt. Based on a simple Poisson distribution, can you make claims about the expected variability in melt? Does it violate the assumption of a single Poisson distribution, for example?

REPLY: We have included a Poisson distribution to analyze melt in warm and colder periods (discussion). We have also checked that the spacing between events is exponentially distributed to verify the naturally occurring homogeneous Poisson distribution (supplements).

COMMENT: Regarding the coffee experiment: Does the thin wall alter the liquid movement? One can imagine that normally melt lenses would spread further in the lateral dimension – this water movement is blocked because of the finite wall thickness. This means the coffee may penetrate deeper than regular melt would.

REPLY: Added to discussion: A limitation to this experiment, is that we are not able to reconstruct the lateral extension of the coffee layers. Due to the thin wall between the pits the coffee leaved the wall and cannot extend in the vertical. The vertical percolation is also limited by the thin walls, as vertical pathways must be found within this wall. Within the unconfined snow pack, the vertical percolation could thus reach greater depths.

COMMENT: The authors make a very speculative claim about Norse settlement of Greenland being triggered by a warm summer in 986. This seems too speculative for print. There are no historians on the author list (only ice core researchers) – which makes me doubt the authors have a thorough historical understanding of the time period needed to support such a claim. I don't have such knowledge either, but both the Britannica and Wikipedia encyclopedias note that Erik the Red first traveled to Greenland in 982, which would pre-date the suggested “trigger” by 4 years.

REPLY: The section about the Viking voyages “triggered” by a warm event has been reconsidered, and written in a more cautious form, removing all causalities and using the suggestions you provide further below.

COMMENT: Overall the paper is excessively verbose and too long. I think the same science could be described after cutting the length by ~40% or more. I strongly advised the authors to reduce the length, as this will make the paper much more useful to the community. Few people have the time to read a paper of this length in detail.

REPLY: In a revised version, we have reduced the length of the manuscript from almost 600 lines to around 450 lines. We have greatly shortened the results section, removed repetitions across sections, and have revised the verbose language. Sections not needed for the main document have been moved into the supplementary material. We have decided to keep the rather long introduction to our work, to cover as many aspects of melt layers as possible, but shortened all other sections. We have furthermore combined figures that go together well.

Detailed comments:

COMMENT: Title: shouldn't it be “Holocene warm events”, rather than “Holocene warming events”?

REPLY: Yes, we agree and will change it.

COMMENT: Line 25: Alley and Anandakrishna is not a “full Holocene” melt record as claimed, because visual identification is problematic in clathrate ice.

REPLY: Changed to “A 10,000 year...”

COMMENT: Line 41: also include citation of NEEM community members 2013 for first use of noble gases

REPLY: Thank you for the information. We have included the reference.

COMMENT: Line 44: that is not the official NEEM acronym (should be The North Greenland Eemian Ice Drilling)

REPLY: Corrected

COMMENT: Line 60-68: the discussion starting here is poorly worded, and should be clarified. It is well known and documented that the polar ice sheets are colder under clear-sky conditions, as snow absorbs and radiates effectively in the longwave but reflects in the shortwave.

REPLY: We will improve the discussion.

COMMENT: Line 76: The NEEM noble gas analyses were first presented by the NEEM community members in 2013 (online supplement), and not by Orsi et al..

REPLY: Corrected, as stated above.

COMMENT: Line 79: It is possible also for meltwater to refreeze homogeneously within the

firm without leaving a clear lens. This is observed on the Antarctic peninsula. Lenses only form if there is a low-permeability layer in the firn for the water to pool onto. The visual melt record may give a lower bound on the full melt intensity.

REPLY: We will add this information.

COMMENT: Line 94: it is not clear how fig 2c was measured. Did you dig a new pit every day?

REPLY: Clarified: "To acquire undisturbed data, the trench was widened by approximately 0.5 m every measurement day."

COMMENT: Line 115: as shown by ice cores: you mean $\delta^{18}O$ -ice in ice cores?

REPLY: Yes, added.

COMMENT: Line 118: At this point you should probably also cite Badgeley et al. 2020, who get a late HTM, and Lecavalier et al. 2017 who get a very early HTM.

REPLY: Thank you for the advice. We will cite the two articles.

COMMENT: Line 134: Note that the lapse rate may vary between seasons. For melt records, summer lapse rates matter more presumably.

REPLY: Will be added to discussion.

COMMENT: Line 142: note that b2k is not the preferred reference frame in the paleoclimate literature, which is BP with present at 1950.

REPLY: This is very valid advice. We prefer to use the, equally accepted, years b2k for our work and hope you can agree to this convention.

COMMENT: Line 151: "This bubble-clathrate transformation is not a gradual process" What does this mean? What is the timescale you consider "gradual" here?

REPLY: We will add: "This bubble-clathrate transformation is not a gradual process over depth, but has variable rates within different layers..."

COMMENT: Line 154: Do you think the partial clathrate nature of the deepest ice you analyze has an impact on the reliability of the melt detection?

REPLY: Yes, it does. Is discussed in the discussion section.

COMMENT: Line 157: What is the "optical dark-field method"

REPLY: Reference added for explanation.

COMMENT: Line 158: Every drilling? Do you mean every Danish deep drilling?

REPLY: Changed to: Every European deep drilling.

COMMENT: Line 181: "it should always be greater than one to two millimeter". Which is it, 1 mm or 2 mm? or is it 1.5mm. And is this at its narrowest point?

REPLY: Changed to one millimeter for consistency. Also adjusted for comment in line 288.

COMMENT: Line 185: Isn't a layer just a lens with a diameter >10cm? The distinction seems irrelevant to me. Why not just call them both melt features, or refrozen meltwater?

REPLY: As we can distinguish them, we do so, to give an idea of their extent in the visual stratigraphy.

COMMENT: Line 191: - In Fig. 6 you identify crusts throughout the core down to 6000 years. Isn't that in conflict with your statement here that you cannot identify them any longer below 250 m?

REPLY: Yes, that is correct. The confusion occurs as the thin melt layers we initially classified as crusts. We discuss this in the relevant section.

COMMENT: This assumption (all crusts are melt features) may or may not have a bit impact on the melt frequency reconstruction. Can you give an estimate of how frequently wind crusts occur relative to melt layers in the upper 250 m?

REPLY: Crusts removed from main document. In the upper 250 m we find that melt events are approximately ten times more frequent than crusts (17 crusts and 195 melt events). Thus they do not influence the frequency distribution much.

COMMENT: Line 195: don't you also have additional core breaks from cutting the cores into 55 cm bags? Or do these not show up as ambiguous layers in the linescan images?

REPLY: Added: Cuts from sectioning the ice core into 165 cm pieces occur at the top and bottom end of the sample and are therefore easy to discard. Cutting the sample into 55 cm pieces is only done after line scanning.

COMMENT: Line 207: "such great quality has not been achieved". Isn't this somewhat of a subjective statement? Core quality is notoriously hard to quantify. 6 breaks per m (fig. 10a) certainly does not seem like great quality ice.

REPLY: Statement removed

COMMENT: Section 2.5: do you only record the depth of the melt feature, or also its thickness and/or extent?

REPLY: We state in the text: "For melt layers we record the upper and lower boundary, for all other features we only record the center value for depth." Extent is not noted, as melt layers cover the full width. We did not include a detailed study on the melt lenses, i.e. only their depth is recorded.

COMMENT: Line 238: I am unclear why lenses vs. layers can tell us something about how "local" the melt was. In my mind, percolating melt water refreezes in somewhat random ways, and the size of any individual melt feature (and its thickness) does not necessarily bear any relationship to the spatial extent of the melt event that caused it.

REPLY: We will remove the word "local" and rephrased the sentence. The statement aimed to describe the intensity of a melt event.

COMMENT: Line 242: The increase toward the early Holocene is only there for the uncertain layers and lenses, not for the certain ones. I think this distinction is important.

REPLY: Yes, thank you for the comment. We have revised the paragraph.

COMMENT: Line 244: There appear to be plenty of lenses around 2000 b2k. In general,

these statements about changes in melt frequency appear somewhat subjective. Could you for example exclude the null hypothesis that the melt frequency in any interval simply follows a Poisson distribution with constant parameters? Consider also that as single melt event percolating down will sometimes leave multiple, and sometimes only one (or zero?) melt features.

REPLY: Thank you for the suggestion. We will include it into the tree rings section.

COMMENT: Line 248: even before 9ka the ratio of uncertain to certain layers greatly increases. Isn't that also indication that the layers become difficult to detect?

REPLY: Yes, we will add it to the text.

COMMENT: Line 250: what are "indications of melt events"? Why is this different from the melt features shown in the panels 6a-6c (curves look different to the eye)? And why are these not listed as "types of events"?

REPLY: Thank you for pointing this out. We have moved to the section to the supplements, and included an example of an "indication of melt event". We have also clarified the difference in the text, as "indications of melt" are the same as "very uncertain melt events".

COMMENT: Line 269: Is it physically possible for the melt layers to thin less than the directly ice below it without invoking extrusion flow?

REPLY: Yes, as thinning is first driven by rearrangement of grains in the firn, where thick ice layers can clearly be excluded. Then recrystallization and deformation processes (sintering) take over, where layers can be thinned differently, depending on crystal structures. Later thinning is driven by bubble compression, also here ice layers can behave differently. Furthermore, pure shear can thin layers above and below and leave these ice lenses and layers, which are just big lenses, in place, without deforming them equally.

COMMENT: Figure 7: the events appear to often cluster very close together. The coffee experiment clearly shows that a single melt event can leave multiple layers. Please comment.

REPLY: We will add an indication the figure where events cluster (multiple layers within five years), and discuss them in the text.

COMMENT: Line 282: The first reference for the 8.2 ka event is Alley et al. 1997

REPLY: Thank you for mentioning this. We have included Alley et al. 1997

COMMENT: Line 282: I don't fully understand this argument. Are you suggesting 8.2ka event cooling reduced the melt? These events are short-lived, and should show up as short-term reductions and not long-term trends.

REPLY: We suggest, that in the period of the 8.2 ka cooling event, we find less melt layers.

COMMENT: This seems like something you could test by isolating the actual 8.2 ka event depths, and comparing melt during this event to adjacent ice.

REPLY: Removed here, and adjusted in the discussions section.

COMMENT: Line 288: Why make definitions when you then choose to disregard them? Why not define melt layers as being thicker than 1 mm?

REPLY: See comment from line 181. We will correct it for consistency.

COMMENT: Line 289: "Correcting for thinning removes the layers between zero and one millimeter and provides three categories of similar sizes, with 54, 43, and 40 events per group." What does this mean? I don't understand.

REPLY: Removed from text, as not necessary.

COMMENT: Figure 8: these appear to be log-normally distributed?

REPLY: Yes, we will include this in the caption.

COMMENT: Figure 9: Does this figure use the uncertain features or not? Would it not make more sense to combine both types of melt features? Now there are periods with a red bar in one plot, and a blue in the other. Climatically it makes little sense to separate these, in my view.

REPLY: We agree and will move this figure and the corresponding text into the supplements. A figure combining both melt features will be included.

COMMENT: Some of the melt lens spacings are close (<5 yrs). Are these really separate events, or multiple lenses formed in a single event?

REPLY: We agree and will clarify this (see comment to fig. 7). We will discuss this in melt layer thickness section.

COMMENT: Line 313: Like before, how meaningful are these periods that are subjectively assigned? Does the distribution differ statistically from a Poisson distribution?

REPLY: We agree and it will be added.

COMMENT: Section 3.5: this is an odd title. Why not something like: time average total melt, or similar.

REPLY: We agree and will change it.

COMMENT: Line 335: why are melt lenses excluded from this estimate?

REPLY: As the dimensions of lenses were not measured, they cannot be included.

COMMENT: Figure 10: how about a correction for surface elevation?

REPLY: Thank you for the very valid point. A feasible method to correlate the number of melt events to surface elevation (or temperature) in a plot is difficult. We will discuss the effects in the discussions section.

COMMENT: Figure 11c: why is the 1ka peak removed by the running-mean averaging? I think the black line can be omitted. Millennial averaging is already enough smoothing in my view.

REPLY: We agree and will remove the black line.

COMMENT: Figure 12 caption: how did you define "colder" and "warmer" periods? Was this

done by eye, or were there criteria?

REPLY: We will clarify this in the text.

COMMENT: Line 360: The overall picture is not necessarily one of steady decrease – prior to 6ka there is no cooling trend.

REPLY: We agree and will change it.

COMMENT: Line 363: typo: stalbe should be stable

REPLY: Corrected.

COMMENT: Line 364: Do you mean the interpretation is consistent with Bova 2021?

REPLY: We will clarify this in the text.

COMMENT: Line 366: Our data suggest that the warmest SUMMER periods....

REPLY: We agree and will correct this.

COMMENT: Line 367: The MWP: earlier you state that there are just a couple of hot summers around the year 986 (L348: "Here, it is important to note, that this is an event confined to a short period over one or a few summers, and not a signal representative for the entire century or millennium."). Please be consistent! Is it just an isolated spike, or an elongated warm period like the MWP?

REPLY: We will write this more specifically and make it consistent.

COMMENT: Line 373: I am not too convinced by the 8.2 ka event arguments. Most of the reduction in the # of events there are in the uncertain category, the number of certain events is quite stable. The 6000-7000 ka interval, consistently marked in red, has melt event frequencies quite comparable to the 8.2 ka event slice. Can you do some significance tests on such statements?

REPLY: We will work on a more careful phrasing of such statement.

COMMENT: Line 380: microwave observations?

REPLY: Yes, thank you for the suggestion, we will include it.

COMMENT: Line 389: very differently

REPLY: Corrected.

COMMENT: Line 390-391: I don't understand the argument that this must be a rain event? Meltwater can pool at the surface (between sastrugi etc), and run down at the lowest point to generate vertical pipes. I don't understand why the coffee experiment suggests that only rain can cause vertical pipes and multiple melt lenses at depth.

REPLY: Thank you for the hint. We will clarify this in the text. We meant to refer to a large amount of melt, which could be from a rain event.

COMMENT: Line 412: "not there in the first place" – do you mean that melt happened at that time, but that the meltwater simply percolated elsewhere and not into the exact narrow 10cm spot represented by the EGRIP core?

REPLY: Yes, that is what we meant, and we will include your suggestion.

COMMENT: Line 412: by adding uncertain layers, don't you also increase the change of identifying melt layers that are not real (false positives)?

REPLY: Yes, that is true. We will clarify this in the text.

COMMENT: Line 428: parasitic? Maybe diagenetic?

REPLY: Thank you for the suggestion, we will change it to diagenetic.

COMMENT: Line 462: would the lower elevation of EGRIP (at all times) not play an important role also? How do the results compare if we consider only melt layers (and not lenses) at EGRIP?

REPLY: We will include a discussion about this.

COMMENT: Line 482: why global climate? Wouldn't regional climate be the only thing that matters here?

REPLY: We agree and will change it.

COMMENT: Line 506, 519: Remove "in prep" references

REPLY: Will be removed.

COMMENT: Line 513: Is the observation of 2.5 yrs older consistent with the idea that meltwater percolates and refreezes at depth? That would be ~1m of percolation, right?

REPLY: Yes, it is consistent.

COMMENT: Line 514: Another metric to consider, would be average (and std) of the decadal temperature of the identified melt features. How does this compare to the mean and std dev of the tree-ring record? Can you use a t-test or similar to prove they are different?

REPLY: We will include your suggestions into the section.

COMMENT: Line 524: My understanding is that the 2012 melt event (and others like it) were driven by local blocking ridge. That is a very local event, and may be quite decoupled from seasonal meant temperatures, so it may not be surprising that there is not a perfect correlation.

REPLY: Yes, that is true. Interestingly we do find this partial correlation.

COMMENT: Line 530: this statement is questioned 5 lines down. Maybe state: Sometime around 983-986 CE

COMMENT: Line 534: America had already been "discovered" 21,000 years earlier from Asia. He "arrived" in America, likely as the first person from the west.

REPLY: Thank you for the correction. We will include it.

COMMENT: Line 540: Can you find a reference from the original tree ring literature for this claim? Sigl et al. is not the original reference for this observation.

REPLY: No reference needed, as it is a claim we make.

COMMENT: Line 540-543: I think it would be wise to remove this speculation. The Vikings hardly knew it was going to be a warm summer, these journeys took a long time to plan and execute, and it is unclear whether it would be obvious from being in Iceland that it was warm/raining in Greenland. Also, historical developments in Norse society may have played a much bigger role. I am not an historical expert, but just looking up online information on Erik the Red suggests he first visited Greenland in 982 during his exile. That does not line up with your claim that the warm summer of 986 "triggered" Norse exploration of Greenland. Do you have better sources that dispute that earlier timeline? The cited reference (Brooks 1986 is from a newsletter to teachers, not a scientific monologue).

It would be ok to note the coincidence, and note that it may have contributed to the success of the passage, but I think any hint of causality should be removed. We simply cannot know these things.

REPLY: We agree and have rephrased every part concerning the Viking voyages. We will adapt to your suggestion and note the coincidence.

COMMENT: Line 545: Here you

REPLY: Corrected.

COMMENT: Line 550: This is not discussed meaningfully in the text.

REPLY: We agree and will include a more meaningful discussion.

COMMENT: Line 568: parasitic? Seems like the wrong word. Does it need an adjective?

REPLY: Changed to diagenetic, as you suggested above.

COMMENT: Line 572: some events align, but not all.

REPLY: Corrected and clarified.

COMMENT: Line 575: Not sure this is supportable. I don't think there is evidence to support this was a causal link. From your data, it seems like a one-off event, rather than a long-term warm period.

REPLY: We agree (see comment from line 540).

COMMENT: Line 581: aren't there a series of melt layers?

REPLY: Yes, correct. We meant to refer to the thickest one of the melt layer series.