

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-89

Anonymous Referee #2

Referee 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-RC2>, 2021

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.

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.

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.

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).

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"?

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.

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?

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.

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.

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.

Detailed comments:

Title: shouldn't it be "Holocene warm events", rather than "Holocene warming events"?

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

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

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

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.

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..

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

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

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

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.

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

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

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

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?

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

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

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?

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?

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?

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?

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?

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.

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

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.

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.

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.

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?

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"?

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

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.

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

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.

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.

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

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.

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

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.

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

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

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

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

Figure 10: how about a correction for surface elevation?

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.

Figure 12 caption: how did you define "colder" and "warmer" periods? Was this done by eye, or were there criteria?

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

Line 363: typo: stalbe should be stable

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

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

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?

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?

Line 380: microwave observations?

Line 389: very differentLY

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.

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?

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

Line 428: parasitic? Maybe diagenetic?

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?

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

Line 506, 519: Remove "in prep" references

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?

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?

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.

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

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.

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.

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 to 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.

Line 545: Here you

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

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

Line 572: some events align, but not all.

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.

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