We appreciate the constructive comments by Anders Carlson and three anonymous referees. We have provided detailed responses to each of their comments and explain how we have modified the manuscript.

**<u>Response to Anonymous Referee #1</u>**

General Comments

I have two main substantive general comments and a few others:

1. My first main comment relates to the author's explanation of the basis for their interpretation of the XRF PC1 record. While the low-resolution (millennial) record presented in the paper is based on multiple proxies (visual stratigraphy, magnetic susceptibility, per- cent organic matter, and scanning X-ray fluorescence [XRF PC1]), the high-resolution record, which is the central focus of the paper, is based almost exclusively on the XRF PC1 record. While the other methods are well established, and their relationship to up- valley glaciation also fairly well established (%OM reflecting dilution of autochthonous and allocthonous biological productivity by glacigenic clastic-sediment input; MS re- flecting the ratio of non-weathered [glacially eroded] vs. weathered [eroded by other watershed processes] materials), the X-ray fluorescence (XRF PC1) is newer and its interpretation probably needs a fuller justification than is given in the paper. In section 4.2 the paper states that the elements analyzed by XRF "K, Ca, Ti, Mn, Fe, Zn, Rb, Sr . . . are common in silicate sediments" and that "Changes in concentration of these elements reflect changes in the contribution of minerogenc material eroded from catch- ment bedrock and delivered to the lake." What is "going up", when these elements "go down"? Organic matter? Some other type of minerogenic material that is not a product of glacial erosion? That the overall pattern of the XRF PC1 record is very similar to the patterns of MS and %OM is clear. Since the latter have been shown to be related to (driven by?) glaciation in the catchment, the authors make a plausible assumption that XRF PC1 is also driven by glaciation. Plausible . . . but I would like to see the reason- ing more fully explained – especially since record of centennial-scale glacier variation proposed in the paper is based almost exclusively on the XRF PC1 record.

-The reviewer makes a reasonable point. We overlooked explaining this because it seems quite obvious, but in fact, a line of explanation might be warranted due to the fact that XRF data are "new on the scene." The explanation for the response in sedimentary elemental composition to changes in glacier size is quite simply that the major elements we have listed and included in the PC1 are components of the bedrock. Indeed, the increase in the elemental abundances during times of larger glacier size is because there is a greater proportion of minerogenic input relative to organic input to the lake at these times. Conversely, when the glaciers are smaller the relative contribution of minerogenic (eroded bedrock) material to the lake decreases relative to organic matter. This happens due to smaller glacier size yielding less minerogenic material and also because warm intervals that result in glacier recession also result in an increase in primary productivity in the lake, and therefore increases organic matter input to the sediments. We have added the explanation of this interpretation to section 5.1, second paragraph.

2. My second main comment (which is discussed in more detail in my comment below on page 2020 – lines 3 & 4) concerns what can really be inferred from existing geochronology about synchrony/asynchrony. This is perhaps a question of predilection. I don't doubt that most of the glacial chronology developed from Kulusuk core could be synchronous with the other records cited. If one is inclined to believe that things should be synchronous, this might be interpreted as sufficient evidence to say things are synchronous. If, on the other had, one begins either without that predilection, or with a feeling that synchrony is the exception rather than the rule, I am not sure how compelling some aspects of the correlation argument would be. Why, for example, should a reader accept a suggestion that centennial-scale advances in one area dated at 2.8 ka and 2.1 ka in one area are really synchronous with those in another area dated at 2.6 and 1.9 ka, simply because they overlap "within chronological uncertainty"? All that really indicates is that it is possible that they are synchronous, not that they actually are. I am not arguing that the authors should abandon their model of synchrony, but perhaps that they should phrase it a little more carefully to suggest that the new data are permissive/suggestive of synchrony.

-We have modified the language in several places, as described in the Detailed Comments below, and also on Lines 311-314, 332-333, 343-344, 364-365, and 388-391 to state that the evidence we have compiled 'suggests' or 'appears to show synchrony' rather than definitively shows regional synchrony in glacier behavior.

I would feel a bit more confident in interpreting the short duration variability of the Kulusuk core as a clear indication of tributary glacier activity (rather than some sort of non-climatic event) if there were multiple cores from the lake in which the events appeared. This is particularly true of short-lived XRF PC1 that don't show up in other, lower-resolution, proxies as long as the controls on XRF PC1 aren't completely clear.

-We are comfortable with our interpretation concerning the controls on XRF-PC1 and the individual elemental abundances derived from XRF. We have also explained our interpretation of these records in the text.

Figures 2 and 3 might be combined, as there is some redundancy and a reader is left jumping back and forth from one to the other while reading the paper. If the authors do leave them as two separate figures, they might consider rearranging the axes on one or the other so they would be easier for a reader to relate one to the other.

I think the authors should introduce their approach to interpretation of the core earlier in the paper – as is, it is left to two sections on 2016 lines 20-27 and 2017 lines 14-18. I would move some of this to page 11 – at least by referring to how previous studies have interpreted specific aspects of core sedimentology as indicators of upvalley glacial activity.

-We now provide more information on our approach, including the techniques that we use to interpret minerogenic input in the last paragraph of Section 1.

Detailed Comments (note: the page numbers referred to here are off by one)

Page 2009 – line 10 – Shouldn't centennial-scale be hyphenated?
-Modified as suggested.

Page 2011 – lines 10-12 – Yes, but probably worth mentioning paraglacial effects, etc. Glaciers may not produce the highest sediment yield when they are at their max- imum extents, but rather highest sedimentation rates are commonly associated with rapid recession. Timescale is critical here. At centennial or shorter timescales, such paraglacial effects may be significant and might be expected to differ glacier-to-glacier.
-Potential influence of paraglacial effects added to this sentence.

Page 2014 – lines 11 – data show (not shows)
-Modified as suggested.

Page 2014 – line 25 – 2.5-1.8 m is not really the base of the record. The actual base (3.5 – 3.0 – shown in figure 2) shows strong variability.
-Text clarified.

Page 2015 – line 4 – MAR is shown in figure 2, not figure 3.
-Figure number changed.

Page 2015 – lines 10-12 – This is not a very clear explanation of origin of variations in the elements – when they are in low concentrations, what is replacing them, and what does that indicate about glaciation?
-We would like to leave the discussion of how we interpret minerogenic changes in the Discussion section. In the following paragraph, we do discuss in detail what drives variations in minerogenic input related to glacier activity. We have also added text to section 5.1 (2nd paragraph) to explicitly state the reasoning as to why elemental abundances reflect minerogenic input and thereby glacier size.

Page 2016 – lines 20-26 – should this explanation be placed somewhere earlier in the paper? As is, a reader not versed in these techniques would have little idea why you are measuring these characteristics and what they can tell you.
-Text has been added to the Introduction to state this information earlier in the manuscript:
"We characterize changes in sedimentation using measurements of physical sediment properties, including: bulk density, organic matter content, magnetic susceptibility, and accumulation rates. We also measured the relative elemental compositions of the sediment using scanning X-ray fluorescence (XRF) to characterize minerogenic changes at higher resolution and with greater sensitivity. These data provide detailed information on sedimentation in Kulusuk Lake related to glacier input."

Page 2017 – line 6 – Mass-wasting events are not always so easily identifiable in glacial lake sediment as this section suggests.
-Text was removed that suggested mass wasting events could be easily identified, however their presence in our record is unlikely because of the catchment characteristics, as stated in this section.

Page 2018 – line 3 – As I interpret figure 3 and the calibrated radiocarbon age, the "8.2 ka" advance was well underway by 8539-8359 BP. Is that consistent with other records of the timing of this advance. More generally – how close in time do your centennial and sub-centennial events need to be for you to consider them synchronous?

-In the previous sentence, we do acknowledge the earlier advance and provide references to other sites in the region that document cooling c. 8.5 ka prior to the 8.2 ka event. We also state clearly that the age control in this section of our record is not sufficient to exactly constrain the timing of these events, but our age model indicates they are similar in timing at centennial scale.

Page 2018 – line 11-13 – How much ELA rise would be necessary to deglaciate the drainage entirely?

-If the catchment was completely deglaciated, our data indicate that the regional equilibrium-line altitude would have been greater than ~676 m, which is the elevation of the mountain peaks above the lake. We added this information to the second paragraph of Section 5.2.

Page 2018 – line 19 – Are these two peaks based on a single measurement each? If so, how much confidence do you have that they are real?

-There are at least two data points supporting each of the peaks and therefore we do not want to discount their potential for being 'real' indicators of changes in sediment characteristics, even though they are indeed very minor sedimentary changes.

Page 2019 – line 7 – In figure 3 (or figure 5) the evidence for any sort of trend (slow or fast) after 1.3 ka is not at all clear.

-There is evidence in that data that after 0.7 ka (AD 1250) there is a very gradual increase in XRF PC1 and MS data. This is visible in Figure 3 and highlighted in Figure 5 where values are consistently above (albeit only slightly above) the average over the last 1.3 ka. We did however clarify the text where the reviewer suggested to specify that the slight increasing trend we are referring to is after 0.7 ka.

Page 2019 – line 12 – Well, since the error is the pooled sum of errors from each age and from the interpolation, I think it would be greater than this. Whatever the error on each individual age is, the error on interpolated ages would be the square root of the sum of the squares of the errors on each age and the square of the error on the interpolation. I'm not sure what that would be, but it would be greater than the 2-σ uncertainty on each age.

-We have applied a conservative estimate of uncertainty (2-σ) to the chronology. Without generating a large set of simulations for the chronology that could yield an estimate of additional uncertainty downcore, we have no way of estimating the uncertainty of error on any interpolated depth. We argue that applying the uncertainty of the radiocarbon dates obtained at intervals is appropriately (and sufficiently) cautious; it is very unlikely that the statistical exercise called for by the reviewer would change the broad picture that we observe, linking glacier changes across the region.

Page 2020 – lines 3-4 – OK, here is the crux of one of my concerns. Yes, you might be able to argue that within error 2.6 and 1.9 ka advances of the Bregne Ice Cap are synchronous with the 2.8 and 2.1 ka advances at Kulusuk Lake. (Although, if I accept your argument on the previous page – 2019 – line 12 – that the interpolated ages are accurate to better than 100 years [2σ] you probably could not make this argument on a statistically valid basis). However – really all you would be saying is that it is statistically possible that they were synchronous. It is also statistically possible that they are asynchronous. Given the mean spacing of 500 years between dated Kulusuk advances in the 4.3 – 1.9 ka interval and you willingness to accept a 200 year apparent age difference and still correlate events ("within chronological uncertainty"), it will be fairly difficult to find any dates of advances within that interval that could not be correlated within uncertainty – even if none of them were in fact synchronous.
My concern here is that while the records you cite, and with their uncertainties, allow the possibility of correlation, I am uncomfortable saying that they prove the correla- tion. If you are inclined to believe that such events are in fact correlated, you can find in these data evidence to support that belief. On the other hand, I do not think that the chronologies are really well enough constrained that they preclude the possibility that for whatever reason (regional differences in climate forcing, differences in sys- tem response times, paraglacial sedimentation effects) that the events recorded are in fact out of sync by a century or more – a significant interval when one is considering centennial-scale climate.

-We stand by this observation and we are comfortable offering it as a discussion point in this manuscript. We cannot "close the case" on this and say with 100% certainty that all centennial-scale changes in glaciers around the North Atlantic have been synchronous, and we have not done this. However, we are comfortable proposing that there is evidence for synchronicity and that the Kulusuk record adds to this evidence.

Page 2020 – line 15 – I don't really see evidence for this "slow and very gradual expan- sion after 1.3 ka". Perhaps from 1.3 to about 0.75 ka, but there really does not seem to be any trend after about 0.75 ka. If anything, there might have been an overall step change at about 0.75 ka.

-We have added text to clarify here (as we did earlier in the manuscript, above) that there is evidence after 0.7 ka indicated by a gradual increase in XRF PC1 and MS data. This is visible in Figure 3 and highlighted in Figure 5 where values are consistently greater than the average over the last 1.3 ka.

Page 2020 – line 21 – "Precisely"? Looking in detail at figure 5 – your blue lines ("periods of increased glacier size" – at Kulusuk? or generalized for all areas?) seem to bracket periods of highest glacigenic sedimentation (highest XRF PC1) at Kulusuk, but commonly seem to end just as glacigenic sedimentation rates are increasing at Big Round Lake on Baffin and especially at Langjökull in Iceland. Perhaps we are looking at a paraglacial effect in the latter two areas and not at Kulusuk – but in any case, the records do not seem "precisely" the same.
-Text was modified to indicate the timing is "similar"

Page 2020 – line 22 – I think you mean figure 5 here for Baffin Island at least.
-Text modified as suggested

Page 2020 – line 22 – What evidence for post-1450 expansion?
-We do have language in the figure caption indicating that we are interpreting periods of advance as, "sustained above average PC1 values." We have also added a similar statement at the beginning of Section 5.3 so this is clearer.

Figure 1 – Moraines seem to extend beyond the red line on the northern glacier.
-These lines define the primary ridge crests mapped in the field. Text was added to the caption to clarify.

Figure 3 – I don't see the dashed line on the PC1 plot.
-Text was removed.

**Response to Anonymous Referee #2**

My only suggestion here would be to throw in a sentence or two in the final conclusions section that again highlights the unique lake setting and why the authors were able to generate such a clean glacier signal from proglacial sediments. I think it is that important.

-A sentence was added to the conclusions highlighting the geomorphic setting, as suggested.

Comparison to Greenland Ice Sheet fluctuations: The authors have noticeably stayed away from comparing their record of cirque glacier fluctuations to recorded fluctuations of the Greenland Ice Sheet margin. I'm guessing that the authors wanted to compare "apples to apples" and just stick to other cirque/mountain glacier records. Rather, you can back out a climate record from cirque glacier fluctuations, but not really from ice sheet fluctuations. I think that approach is fine, but a brief paragraph that makes a few links to the GIS would make this paper stronger and likely garner more citations, while at the same time it would remain clear that this paper's focus is on climate records that can be deduced from cirque glaciers. It looks like Carlson eludes to this very same point with his posted comment on the interactive discussion page that accompanies this manuscript. Again, a paragraph making the link between key advances seen in the Kulusuk record and key advances of the GIS margin would make for an important paragraph. Luckily for the authors, but unfortunately for the scientific community, the list is going to be short as the detailed Kulusuk record spans an interval where detailed GIS margin records are lacking. The authors could mention the ~1.5 ka advance seen in both the Kulusuk and GIS records that Carlson suggests, and also mention the clear 8.2 ka advance seen at in the Kulusuk record and the glacier margin record at Jakobshavn Isbræ. For the 1.5 ka advance from the southern GIS per Carlson's suggestion, I would add the caveat that this is the only place along the GIS margin where this advance is seen, and unlike the 8.2 ka event for example, there is not a clear and well-established cooling event at 1.5 ka that can easily explain the synchronous advance of both types of ice margins. The 1.5 ka advance could indeed have been driven by cooling, but it could just as easily been driven by ice dynamical processes and the timing is just pure coincidence. Again, I would add this record to the text, but just include the aforementioned caveat. For the 8.2 ka records, the authors could even mention that the coeval response of the small and responsive Kulusuk glacier and Jakobshavn Isbræ speaks to the sensitivity of GIS outlet glaciers. Rather, here is direct evidence that at least a portion of the GIS is able to respond just as quickly to a climate perturbation as a small 'responsive' cirque glacier. This would be an interesting and important point because the authors use the small and responsive cirque glacier argument as part of their initial motivation for this study. The appropriate references for the 1.5 ka advance of the GIS are Bennike and Sparrenbom, 2007; The Holocene, v 17 and Winsor et al., 2014, QSR, v. 98. For the 8.2 ka event related GIS papers, the authors could consult Young et al., 2011, Geophysical Research Letters, v. 38 and Young et al., 2013, QSR v. 60.

-For the reasons that the reviewer states, we had avoided comparisons to fluctuations of the ice sheet margin, but we have now included some references to provide readers with information about the Greenland ice sheet margin during the Holocene. We added a sentence stating that that [at least] one area of the Greenland ice margin did respond to the 8.2 ka event (Young et al., 2011, 2013) (lines 279-281) and a paragraph discussing the late Holocene advance of the southeast sector of the ice sheet based on Winsor et al. (2014) and Bennike and Sparrenbom, 2007) (lines 346-352).

Comparison to other regional records of glacier variability over the last ~1200 years: The authors try to make the case that the Kulusuk record is coeval with the Baffin Island record of ice cap expansion. The authors state "Kulusuk glaciers increased in size ca. AD 1250–1300 and again ca. AD 1350 and AD 1450, precisely when ice caps on Baffin Island (Miller et al., 2012) and Iceland were expanding." I agree that there is synchronous ice-cap expansion at ~ AD 1250-1300. This is the first sharp peak in the Baffin Island probability plot and also coincident with a period of extreme volcanism (cited cooling mechanism in Miller et al., 2012). However, I think the authors here are misinterpreting the Baffin probability plot a bit, maybe in a bit of an effort to argue for more synchroneity that there actually is. Mainly, a period of synchronous glacier expansion at ~1350 AD is a bit of a stretch. I see this pulse in the Kulusuk record, but this coincides with a period of ice growth and melt in the Baffin probability plot, not just ice expansion. Rather, the entire Baffin probability hump is not one large period of ice-cap expansion, nor can you pick out pulses of ice-cap expansion beyond the 1275 and 1450 AD peaks; those are the only clear pulses of ice-cap expansion (both peaks linked to volcanism). To make a claim about synchronous glacier growth at 1350 AD is not supported. Moreover, the overall comparison between the Kulusuk record and the Baffin Island record is a bit tenuous because while I agree there are similarities between the two beginning ~1275 AD, this relationship breaks down back in time. In fact, the Baffin Island record depicts ice cap expansion between ~AD 875- 975 whereas the Kulusuk record depicts the exact opposite – a significant period of glacier recession at the exact same time. I think at best the authors can claim there appears to be a synchronous advance at ~1250-1300 AD, and that glaciers remain extended after AD 1450, which is also seen in the Iceland and Baffin lake records. I would modify this text accordingly and make note that prior to ~AD 1275 there does not appear to be much similarity. This does not include the mention of glacier expansion coincident with Bond events seen in the Kulusuk and Iceland lake records, this is all fine and good.

-We agree with the reviewer and have modified the language in this section (Lines 365-367). We back off the strength of our language and use of the word, "precisely" to describe the similarities among records. We now state that the advances of the Kulusuk glaciers and ice caps on Baffin Island and Iceland are 'similar' after AD 1250 and correlate during the intervals AD 1250-1300 and AD 1450.

Minor comments:
Page 2011, line 15: maybe add "geomorphic" before "evidence"
-Change made as suggested.

Page 2020, line 21: "possibly" instead of "possible"
-Change made as suggested.

Page 2021, line 13: "Likely" seems a bit strong here. This paper presents a valid hypothesis, but "likely" makes it sounds as if this hypothesis is set in stone.
-Change made as suggested using 'possibly' instead of 'likely'

Figure 3 caption: Does there need to be letters in the figure that correspond to the a,b.c in the caption? Also, I see no dashed line on the PC1 plot. I see the dashed line down in Figure 4, but not Figure 3.
-Figure captions updated

**Response to Anonymous Referee #3**

Holocene Thermal Maximum:
p. 2019, lines 22-24: this study ". . .refines previous estimates for [the HTM] onset and termination", but does not clarify in what way these estimates are refined. Does the 7.8 to 4.1 ka HTM in Kulusuk align with estimates of HTM, as described in Kaufman et al. (2004)?

-The Kaufman et al. (2004) estimates are based on extrapolated estimates from sites around this region and roughly place the HTM in the early-to-mid Holocene (roughly 9-4 ka). Text has been added to the sentence to clearly state this, Lines 301-303.

It would be informative to address whether the Kulusuk data align with the body of work examining North Atlantic glacier and Greenland Ice Sheet response to early Holocene warmth (e.g., Briner et al., 2014; Funder et al., 2011; Larsen et al., 2015; Lecavalier et al., 2014; Solomina et al., 2015; Tarasov, 2003).

-We agree that some mention of Greenland Ice Sheet margin reconstructions are necessary, as also suggested by Reviewer #2. However, we do not agree that all of the references listed by Reviewer #3 are comparable. We have primarily focused on comparisons to high-resolution and continuous records. Moreover, we want to compare our site to small glaciers and ice caps that likely respond rapidly to climate changes, whereas the response of the Greenland Ice Sheet is more complicated due to large-scale ice dynamical processes and many of the studies cited only broadly define trends in the extent of the ice sheet. We have added references to Young et al. (2011, 2013), Winsor et al. (2014), Bennike and Sparrenbom (2007), and Larsen et al. (2014), which now provide readers with some context to the response of the ice sheet during the period we examine of the Kulusuk record.

There is some signal and variation in the XRF PC1 data from 7.8 to 4.1 ka, so there must be some source of allochthonous minerogenic material, even though the glaciers were small or absent. What is the source of this material? Could the source be per- mafrost and periglacial processes? Or snowmelt and rainwater runoff? Or something else?

-This minor signal in the XRF data likely indicates some input from runoff or paraglacial processes. We added text to state what the processes that could be responsible for any minerogenic input during this interval.

<u>Interpreting centennial-scale variability after 1.3 ka:</u>

Because small-scale variability exists in the XRF PC1 from 7.8 to 4.1 ka, when the authors assume that the Kulusuk glaciers were small or nonexistent, it is unclear to me how the small-scale PC1 variability after 1.3 ka, which is of a similar magnitude to the PC1 variability from 7.8 to 4.1 ka, can be interpreted to represent changes in glacier size. Could this variability be related to other sources of allochthonous minerogenic material?

-We agree that the centennial-scale trends after 1.3 ka are minor, but it is not appropriate to directly compare it to the interval from 7.8-4.1 ka. The mid-Holocene interval is marked by the extremely low and sustained magnetic susceptibility values, high organic matter content, and low sedimentation rate. After 1.3 ka, MS values are very high, the sediment is laminated, with strong variability in the XRF data and the sedimentation rates are higher. Therefore the processes driving minerorgenic input are quite different.

<u>Synchrony of glacier & climate response</u>:

I agree with Reviewer 1 about interpreting synchrony. It could be helpful to use prob- abilistic methods (an example is Anchukaitis and Tierney (2012)) to determine the likelihood of synchrony between the different glacier and climate records.

-A statistical analysis similar to the one suggested by the reviewer is beyond the scope of this paper. It would require significant additional effort to assemble chronological data from sites that we have not worked on. We encourage the reviewer to carry out such a regional analysis, to assess whether the conclusions that we have drawn stand up to further scrutiny.

<u>Minor comments:</u>

p. 2010, lines 6-9: Perhaps “continuous records of variations in glacier size” is more appropriate than “higher frequency variations in glacier size”: sites in the Arctic that do have early Holocene moraines (e.g., Alaska) don’t necessarily have centennial resolu- tion.

-Text modified as suggested.

Geochronological data:

How do the authors deal with terrestrial vs. aquatic 14C ages? There is often a reser- voir effect in arctic terrestrial 14C ages, due to storage in permafrost.

-We have dated macrofossils from terrestrial and aquatic sources and there doesn't appear to be a reservoir effect in this system, which would likely show-up as large age offsets between samples that we do not observe.

Some geochron. information that is important to provide for recalculation if necessary in the future: Raw 210Pb activity data used to model the age of surface sediments. Fraction Modern for 14C measurements

p. 2020 line 19-p. 2021, line 7: It seems to me as if there are two separate mechanisms being called upon here as the main driver of glacier change: insolation and North Atlantic cooling. Right now the following two statements seem rather disparate:
p. 2020 line 20-23: "each episode of glacier advance was followed by a period of retreat. . .possible suggesting that the glaciers repeatedly grew out of equilibrium with external insolation forcing and then retreated back toward an equilibrium state"
p. 2021 line 5-7 (following discussion of synchronous ice rafting/cooling events in the North Atlantic and ice cap advances in Iceland and Greenland) "continuous records of glacier activity. . .reveal synchronous glacier response to abrupt episodes of climate change".
These two statements are not necessarily independent from each other, but it would be helpful to clarify which mechanism is most likely causing the observed changes. Or are both mechanisms at play? This would be good to clarify.

-Text was added to the first paragraph of Section 5.3 (lines 321-326) to more clearly explain that: On millennial time-scales the glaciers are responding to insolation changes. So the gradual decline in northern hemisphere summer insolation is driving the progressive growth of glaciers from the mid- to late Holocene. Superimposed on that long-term trend is centennial-scale variability likely driven by dynamics internal to the climate system.

Fig. 1: Would it be possible to add bathymetry of Kulusuk Lake? This would help clarify if there are bathymetric highs related to glacier deposition (e.g. moraines) that may have been deposited during the period studied and therefore influence the "glacial" signal in the lake sediments.

-Unfortunately, detailed bathymetric data is not available that would resolve the features the reviewer is interested in.

Fig. 2. What does percent sand indicate? Could it be a signal of IRD? Why is Holocene maximum percent sand not at maximum glacial extent inferred from other proxies (ap- prox. 10 cm)?

-The grain size measurements were made at much lower resolution (every 10 cm) so can't be compared directly to the other proxies. We use it to show how overall the amount of coarse sediment changes across each interval.

Fig. 3: Add a, b, c labels and dashed line on PC1 indicating absence of ice.

-Labels added and the reference to the dashed line was removed.

Fig. 4: What data are yellow and blue shading based on? The Kulusuk record, or previous publications?

-More detail was added to the figure caption to explain that the yellow shading is based on Kulusuk data, and the blue bars indicated cooling events that are comparable among the records.

Figs. 4 and 5: Why use different data in Fig. 4 and 5 to represent Langjökull? Do the C/N and sedimentation rate data in Fig. 4 reveal the same patterns as varve thickness in Fig. 5?

-C/N and sedimentation rate do generally resemble trends in varve thickness over this time interval. We choose to present the data this way because the varve data is the highest resolution proxy at that site and we are comparing high resolution data over the last 1200 years.

It would be informative to show the full Holocene magnetic susceptibility record from Big Round Lake. The timing of minimum Holocene glacier extent from the Big Round Lake record is different than at Langjökull and Kulusuk glaciers, but that is interest- ing information, which perhaps tells us something about regional climate and glacier variability.

-We don't agree that the full comparison would be worth adding to the manuscript.

It seems important to mention the different glacier-lake systems shown in Figs. 4 and 5. The transport path between Kulusuk glaciers and Kulusuk Lake and Langjökull and Hvítárvatn is much shorter than the transport path between the glacier and Big Round Lake, so there could be more sediment storage and other related processes influencing the Big Round Lake record. For example, MS seems to reflect thickness of sand layers deposited in late summer, so higher MS at 1250-1300 AD is perhaps not solely due to glacier activity. Big Round Lake varve thickness was originally interpreted to represent temperature, the opposite interpretation is used here. It seems important to at least mention the differences between these sites, and to mention the difference in interpretation from the original publication.

-We agree that we didn't provide enough context when including the Big Round record and its interpretation. We have now added a more detailed explanation (Lines 372-). On longer timescales, the magnetic susceptibility data from Big Round have been interpreted as indicating glacier size changes (Thomas et al., 2010) and it is worth showing that there are interesting similarities in trends among the records we present in Figure 5. We also include the varve thickness data because at times it also resembles MS, although not at all intervals, even though there is a significant correlation between varve thickness and summer temperature that cannot be ignored (Thomas and Briner, 2009). We suggest that both interpretations can still be valid and that the discrepancy in the proxies could be related to the timescales on which they affect sedimentation and/or the geomorphic characteristics of the proglacial system, which as the Reviewer points out, is different than Kulusuk and Langjökull.

**Response to A. Carlson**

Dear Balascio et al., I am intrigued by your study and would like to point you to another record of late Holocene glacier change (and summary of southern Greenland records) that was presented in Winsor et al. (2014, QSR). This study found a glacier advance in southern Greenland ending at ~1.6 ka, similar in timing to your advance documented in Kulusuk.

-We appreciate the comment and reference to a related study from the Greenland Ice Sheet margin. As mentioned above in responses to suggestions by Reviewer #2 and #3, we were originally avoiding comparisons to the ice sheet margin because of the potential differences in response time and influence of large-scale ice dynamical processes that might complicate movement of the ice sheet margin. However, it does seem appropriate to reference ice sheet margin advances to provide a more complete context for our study. In reference to your specific comment, we have added a citation to this paper and a paragraph discussing the late Holocene advance of the southeast sector of the ice sheet (Section 5.3, 2nd paragraph).