Clim. Past Discuss., 11, C808–C813, 2015
www.clim-past-discuss.net/11/C808/2015/

Climate of the Past Discussions

Interactive comment on "Glacier response to North Atlantic climate variability during the Holocene" *by* N. L. Balasacio et al.

Anonymous Referee #1

Received and published: 25 June 2015

This paper addresses issues of broad interest to the Quaternary research community – both by presenting a new high-resolution approach to reconstructing centennial- and sub-centennial-scale Holocene glacier variations and by providing evidence of synchrony of such variations across a fairly broad region of the Arctic. The paper certainly addresses questions within the scope of Climates of the Past. The scientific approach, methods, and assumptions are generally well explained – although in one instance (discussed below) I think that a fuller explanation of the basis of interpretation of one critical line of evidence could be more fully presented. The conclusions of the study are clearly laid out and explained. Overall, the interpretations and conclusions reach are supported by the data presented, although to some extent I think there is enough latitude in the data that it might be interpreted somewhat differently by someone who brings a different predisposition to questions of synchrony or non-synchrony of glacier

behavior. This is also something I will discuss below. Methods presentation, title, abstract, overall structure and presentation of the paper, and writing are fine; references are appropriate and adequate. Overall, tables and figures are good, although I have a few figure suggestions I will make below.

General Comments

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

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, percent 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 reflecting 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 catchment 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 reasoning more fully explained – especially since record of centennial-scale glacier variation proposed in the paper is based almost exclusively on the XRF PC1 record.

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.

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.

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.

Detailed Comments

Page 2009 – line 10 – Shouldn't centennial-scale be hyphenated?

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 maximum 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.

Page 2014 – lines 11 – data show (not shows)

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.

Page 2015 – line 4 – MAR is shown in figure 2, not figure 3.

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?

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.

Page 2017 – line 6 – Mass-wasting events are not always so easily identifiable in glacial lake sediment as this section suggests.

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?

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

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?

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.

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.

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 correlation. 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 system 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.

Page 2020 – line 15 – I don't really see evidence for this "slow and very gradual expansion 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.

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.

Page 2020 – line 22 – I think you mean figure 5 here for Baffin Island at least.

Page 2020 – line 22 – What evidence for post-1450 expansion?

Figure 1 – Moraines seem to extend beyond the red line on the northern glacier.

Figure 3 – I don't see the dashed line on the PC1 plot.

Interactive comment on Clim. Past Discuss., 11, 2009, 2015.