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Climate of the Past Discussions

Interactive comment on "The onset of Neoglaciation in Iceland and the 4.2 ka event" *by* Áslaug Geirsdóttir et al.

Anonymous Referee #2

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Summary comments:

This paper focuses on the paleolimnological evidence for episodic cooling on Iceland ca. 4,200 yrs ago, a time associated with numerous other climate changes around the world, and particularly in the North Atlantic. The manuscript synthesizes previously published data from the same group of authors using factor analysis to examine commonality in the signals among the various records, across lake types and proxy types. The paper is an extension of a previous analysis by Geirsdottir and others, but with additional sediment records included. The study is inherently valuable because it brings together a large body of work produced by a single research group during the past decade or so, the records are of high quality, and the age models are good. I think the results will be eventually suitable for publication in CoP, but I do have some

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questions and comments that I would like the authors to address. Many of my comments seek clarification of points that are being made in the discussion that I think are too vague and more transparency concerning the basis for determining ages of glacial inception. In a bigger sense, though, I think the manuscript would be improved if it had a more thorough discussion of all the stepwise cooling events that are revealed by the sediment record synthesis, not only the cooling event around 4.2ka.

Reviewer comments:

1. The title of the paper includes the phrase "onset of neoglaciation," and I appreciate that the authors include a discussion of the origin of this term, but when do they believe this onset began in Iceland? It seems this should be a fundamental conclusion of the paper if it is so prominent in the title. The discussion leads one to understand that neoglacial inception was catchment-specific across Iceland, and this is to be expected. Yet, this seems at odds with the conclusion that the neoglacial began at ∼5ka (which really comes from Larsen et al., 2012). I don't follow the logic used to determine this timing. Why are the BSi records shown in Fig 6 and 7 used to justify this timing for glacial inception? Do the blue bars on this figure denote onset of minreogenic input to the lakes from glacial erosion? I have either missed it in the discussion, or it could use further explanation. As it stands, I do not find that a neoglacial onset of 5.5ka can be concluded from the records shown. What provides the basis for this?

2. Section 5.2 is called: The onset of neoglaciation. Section 5.3 is called: The onset of neoglaciation in the circum North Atlantic and the 4.2 ka event. I think the authors intended to have a greater discussion of the North Atlantic patterns of / evidence of neoglacial inception in different regions in section 5.3, but this never materializes. It probably should, because there is ample evidence from many parts of the North Atlantic. This discussion should either be expanded or the section title changed to reflect the content. The discussion of neoglaciation in the circum North Atlantic seems too brief. It basically stops at noting that records appear to be driven by monotonic insolation forcing. This is a shame, because there are other studies that previously showed

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stepwise cooling in the Holocene (including at 4.2ka) and that provide support for the authors' interpretations. These should be discussed/cited.

3. I understand that this is for a special issue of CoP concerning climate changes that took place around 4.2ka and that this motivation has steered the direction of the discussion of the datasets being synthesized in this manuscript. However, it seems like a missed opportunity to focus only on this single event. Doing so implies to the reader that the changes in the records observed at 4.2ka are somehow bigger, more abrupt, or different in some way from the many other abrupt changes seen in these records. What about the 6.5ka, 5.5ka, 3.0ka, and 1.5ka Events? These all stand out as equally important and noteworthy in the 7-lake all proxies record. The fact that the high latitude North Atlantic cooled through the mid-late Holocene in a stepwise manner is very important. I think a section that discusses the other abrupt cooling steps and their relationships to potential forcing factors, oceanographic changes, etc, as has been done for the changes between 4-4.5ka, would be very valuable.

4. There is an implication running through his manuscript that the climate perturbations seen in these records are due to climate cooling episodes caused by volcanism. This is possibly true. But I think I think this hypothesis should be given it's own discussion rather than being inserted here and there throughout the manuscript. For example, the first line in the method section presupposes that volcanoes are the primary climate forcing responsible for the signals in the proxies – I don't really think this is an appropriate place in the manuscript to insert this concept. Something that strikes me as particularly confusing is that one of the conclusions of the paper is that the Hekla4 eruption, although coincident with the 4.2 event, could not be responsible for the climate cooling because of low SO2 in the eruption. Yet, there is wording throughout the manuscript that leads a reader to believe that volcanism was responsible for climate cooling events observed in the record. I think that a section that specifically focuses on he role of volcanoes on climate and on landscape dynamics would be very useful.

5. The authors interpret C/N and TOC as both directly related to catchment erosion

(pg5 lines 1,2). This may be the case. If so, it can be easily tested by comparing the two data sets. Do TOC and C/N carry the same signal within each lake? This would support the interpretation. How do they correlate within each record? I ask because it seems these signals could be much more complicated. It isn't clear to me how colder conditions lead to greater soil erosion. Is it because there is less vegetation during cold times? Or is it because there is greater glacial erosion? That shouldn't matter in the non-glaciated catchments. Moreover, when BSi decreases shouldn't the in-lake organic productivity also go down, leading to higher C/N even in the absence of changes in terrestrial input? When BSi increases due to increased productivity, won't TOC go down due to dilution of the sediments by BSi, even in the absence of changes in soil erosion? It seems like mass accumulation rates are needed to consider these proxies independently, particularly in the glaciated catchments where changes in glacially derived material is likely the primary control on all of these other measured proxies (as %).

6. Ideally, a synthesis of various records would include error bounds that propagate the uncertainty in the age models of each sediment core. I know the age models are quite good in these records, due to the abundant tephra layers, but correlating the records of lakes to within a couple hundred years is still quite challenging. The correlation uncertainties change as a function of distance from age control points, and the authors have already calculated the age uncertainties for each sediment record using BACON. These should really now be used to propagate these uncertainties into the "all proxy" composites. The abrupt changes are very evident and I do not doubt them and I think they will remain a robust feature, but this uncertainty analysis would be useful.

7. One thing that is unclear to me in the manuscript at times is what is meant by "volcanic impact" on a catchment. It seems as though this is sometimes referring to the indirect impact of volcanism via its impact on climate, and at other times is referring to the actual physical impact on a specific catchment stemming from local volcanism. One example of this is line 22 on page 10, where the authors refer to the greater impact

on HVT and ARN (relative to the other lakes) from catchment-specific processes, including volcanism. I can't tell whether this is implying that volcanism leads to changes in catchment erosion independent of climate that then obscures the climate records, or if the point is that these catchments respond differently to external climate forcing because they are more continental than the coastal sites and a given volcanic eruption impacts them more. I believe that this could use clarification. A dedicated section about the impacts of volcanism on lake records would probably be very useful in clarifying the authors' meaning.

8. Page 7, line 26: "Sedimentological analyses of HVT. . .toward cooler conditions. . ." Fig. 3a is referenced here, but there is no BSi record on that figure and the "all proxy record" from HVT shown on Fig 3 actually shows a first cooling step at 6.5ka, not 5.5ka. Is there some other information being used to assign the 5.5ka step as the neoglacial inception? Perhaps mag susceptibility, grain size, or minerogenic content? Especially because "neoglacial onset" is in the title of the paper, it seems that being as clear as possible about the underlying evidence is important. Also, is this change in BSi actually diminished biological productivity, or driven more by the dilution impact of renewed input of glacially derived sediments to the lake? Seems the latter is a better indication of glacier inception.

9. Page 8, line 2-4: "The impact of the tephra on the landscape in either case is unambiguous. . ." What is the "unambiguous impact" of the tephra on the landscape? There is an implication here that the tephra somehow impacts the catchment response to climate forcing, or maybe confounds the proxies in the lakes of those catchments such that they do not represent climate when there is tephra in the catchment - but exactly how this works and the impact on the proxy interpretations is never discussed. I'd like a more detailed discussion of these impacts.

10. Line 24 page 11: The final words here are "supporting our conclusions," but it is not clear what is meant. I think the authors are saying that other studies that have either documented or speculated about abrupt changes in ocean currents during the

past 2ka support their interpretation that the lake sediments document abrupt cooling events. This isn't really true, first of all; but secondly the wording is too vague and I don't understand what exactly is being linked between these cited papers that have interpretations about the internal feedbacks of the North Atlantic and the conclusions of this paper, which at this point in the paper have not yet been reached, or at least are still a bit vague.

11. Conclusions: There are 6 conclusions of the paper provided as a bulleted list. Some of these are not actually conclusions that can be drawn from the results presented here, and are better described as discussion points than conclusions. Some are not necessarily supported by the data presented. I would ask the authors to be more specific about their conclusions, remove those that aren't really conclusions from this study, and to provide them as a narrative rather than a list, so they can explain/summarize. 1. ELA intercepted Langjokull at 5ka. There isn't evidence for this presented in this paper. 2. This conclusion is saying that the Holocene cooling on Iceland happened in a stepwise manner, which I think is a reasonable conclusion. However, based on the records presented, the first cooling event happened well before 5ka. 3. Stepwise cooling requires internal feedbacks, which possibly involve ocean dynamics. Reasonable conclusion. 4. I think this conclusion is that Hekla 4 eruption did not cause the cooling associated with the 4.2 event, even though they are contemporaneous. Does this conclusion really stem from the results presented in this study? 5. This is about sea ice expansion during neoglaciation, but this isn't a conclusion from this Iceland lake synthesis. 6. Ocean circulation influenced climate on Iceland. This can be a conclusion based on the comparison of the Iceland records with some marine records, but the conclusion should be much more specific than this. Clearly, ocean dynamics impact Iceland's temperature – isn't there more than can be concluded about how and when?

12. Tables: Data tables are incomplete. Table 1 – Can easily delineate and measure the catchment area of the two lakes that aren't included using readily available maps.

Table 1 – title says core description, but the table contains only lake descriptions.

13. There are some typos throughout that the author's should look out for. But one in particular that spellcheck won't pick up on is in the Abstract, where is says decent instead of descent.

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