

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

Áslaug Geirsdóttir et al.

age@hi.is

Received and published: 16 December 2018

Reviewer's 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

C1

in CoP, but I do have some 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.

Response: We thank the reviewer for the thorough comments and suggestions that help clarify and improve the paper. Firstly, we kindly remind the reviewer that this paper is being submitted to a special issue of CoP that is focusing on the 4.2 ka event - see below:

A special issue on the 4.2ka event: The ~4.2–3.9 ka BP abrupt aridification and cooling event (Zanchetta et al., 2015; Weiss, 2016) is recognized in many locations across the globe, but its causes, precise timing, characteristics and quantification remain enigmatic. A 3-day international workshop on this topic was held at the Dipartimento di Scienze della Terra (Università di Pisa, Italy), 10–12 January 2018, and attended by ~60 people. This special issue will include individual papers presented at the meeting and regional syntheses subsequently developed by those in attendance.

Although we do agree that our complete Holocene record deserves a special paper, we would like to see such a paper equally discuss all the different perturbations that have taken place during the last 10 ka. Such a paper would be more suited as a review paper and requires a different focus than the one for this special issue, which requires some focus on the 4.2 ka event.

Below we address all of this reviewer's comments and concerns to the best of our ability. Corresponding revisions will be made to the manuscript.

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

C2

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 minerogenic 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?

Response: We refer to page 9, line 33 and page 10, line 1 to 3 and again page 10 lines 7-19. Regarding the onset of Neoglaciation/first glacier inception we do point out that Icelandic glaciers are most sensitive to temperature and that we expect the timing of glacial inception to be controlled by the rate at which Holocene temperature declines. BSi currently reflects best the temperature change in our records. We also say on page 9, line 33: Following Porter's (2000) definition, the onset of Neoglaciation in Iceland based on our lake records occurred before 5.0 ka for Langjökull, although the initial growth of Drangajökull occurred much later, ~2.3 ka. This indicates that the spatio-temporal nucleation of glaciers in Iceland was indeed asynchronous and likely reflects the relation between the regional ELA and topography, but does not change our interpretation of when the Neoglacial onset began (i.e. ~5.5 ka). The nature of the topography (i.e., the hypsometry) controls how quickly the glacier will expand after the ELA intersects the topography. This is an important result as defining the onset of the Neoglaciation in Iceland would depend on which ice cap one studied. If the study solely relied on the datasets from Drangajökull, the conclusions would be very different compared to the conclusions drawn when two additional (and high elevation) ice caps are included.

To clarify this and to improve the flow of the paper we have rearranged the sections

C3

within the Discussion so that we now discuss the 4.2 ka event first with focus on the BSi record as a spring/summer temperature indicator. Here we also extend the description of the sedimentological parameters that do support glacier activity in HVT's catchment such as the MS record and the sediment accumulation rate. We then discuss the demise and growth of the Icelandic glaciers during the Holocene, and finally we discuss the onset of Neoglaciation.

The conclusion is as pointed out in the manuscript that the onset of Neoglaciation in Iceland did indeed occur with the inception of Langjökull although other glaciers didn't start to nucleate until around 4.5 ka or later, which emphasizes the importance of the 4.5-4.0 ka temperature decline.

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

Response: We acknowledge this comment and refer to our response to comment 1 above. We have now changed the title of the section. The focus here is on the stepwise temperature decline as seen in the BSi record, the 4.2 ka event and relation of the Neoglaciation of Iceland. We cite similar studies from the circum-North Atlantic area as supportive research for similar ELA lowering, but have decided that it is not necessary to review studies on Holocene stepwise climate changes in the circum-North Atlantic area in depth in this paper.

C4

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.

Response: We refer the reviewer to Geirsdóttir et al. (2013), which discusses the overall stepwise changes during the Holocene. Because we point out in the text (and Figure 5) the same stepwise pattern is very apparent in the all lake composites, we do not feel like that it is necessary to repeat the Geirsdóttir et al. (2013) conclusions. Instead, we place the focus here on the appearance of the 4.2 ka temperature decline and its relation to the nucleation of current glaciers in Iceland and the global 4.2 ka event. Our record from HVT (both physical and biological) shows the first major and apparent temperature change between 5.5 and 5.0 ka, which is further supported by glacier modeling indicating glacier inception at this time. Hence, 5.5 ka is an important point in time in regard to the focus of the paper. All the other lakes show the first synchronized/correlative big change between 4.5- 4.0 ka, which allows us to tease apart its potential relationship to the global 4.2 ka event. Hence 4.2 ka is another important step to focus on in this paper, especially given the focus of the CoP special issue.

We have added a paragraph at the end of the Introduction as followed to clarify the aim of the paper: In order to understand the non-linear pattern and stepped changes in

C5

Iceland after the HTM (Geirsdóttir et al., 2013), and how regional temperatures evolved in terms of timing, magnitude and glacier inception, we focus specifically on the climate steps between 6.0 and 3.0 ka. This time interval includes the 4.2 ka aridification and cooling event recognized at many global locations across latitudes and longitudes. We place the 4.2 ka event in the context of our Icelandic Holocene climate reconstruction and knowledge of large Icelandic volcanic eruptions as a way of judging if it is indeed a major climate event.

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 the role of volcanoes on climate and on landscape dynamics would be very useful.

Response: We have amended the text to reflect two different effects of volcanism where appropriate (i.e., tephra deposition and aerosol production), which were likely not clear to the reviewer beforehand. One effect is the local tephra deposition that results in vegetation destruction and soil erosion. Because this is manifested as increased C/N in the lake sediment records, which we typically interpret as cool and windier winters, it potentially obscures the temperature records we are aiming for. The other effect is emission of aerosols and gasses from volcanic eruptions, which would impart a climatic effect. We have changed our wording throughout the text to reflect

C6

our intending meaning of the two possibilities.

To prevent any confusion, we have now removed the first sentence in the Method chapter, “that volcanoes are the primary climate forcing responsible for the signals in the proxies.” We have also extended our discussion on the impact of explosive volcanism and tephra fallout on the catchments and lake proxies in section 2 and 3.1, but do not feel an entirely separate section is needed at this point.

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 %).

Response We have now made corresponding revision to the manuscript where we have added a brief discussion of the reasoning for using these proxies and their interpretation. For a detailed explanation, we direct the reader to the 2013 paper, which indeed assesses the relationship between TOC and C/N. To specifically address both reviewers' question about TOC, we suggest that TOC in the sediment is a product of both production and transport terms (among other factors). The production term increases during warm periods due to increased plant growth, but transport from land is

C7

reduced as vegetation growth stabilizes slopes and carbon accumulates (and remains sequestered) in soils. During cold periods, even though the autochthonous production term is minimized, catchment vegetation is reduced allowing increased transport of previously accumulated organic matter from eroding soils resulting in a large influx of terrestrial OC to the lake sediment. This more than compensates for any decrease in productivity due to shorter growing seasons and leads to a net increase of lake sediment TOC during cold periods. The soils of Iceland lack cohesion and are susceptible to erosion, both through eolian processes and overland flow (Arnalds, 2004). Of these processes, wind transport of soils is widespread and significant in Iceland, as displayed by characteristic 'rofabard' features (Arnalds, 2000). A comparison of modern winter wind speed and lake sediment shows good correspondence during the instrumental record in northwest Iceland (Geirsdóttir et al., 2009). We do not discount that soil erosion happens due to overland flow or glacier erosion, but conclude that wind is the dominant driver, particularly since most of our lakes are non-glacial. This part of the manuscript has been expanded as suggested.

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.

Response: we note the comment and will in future papers work to propagate uncertainties more rigorously. However, The tephra-based chronologies for each lake sediment sequence and correlation between lakes based on the same tephra layers together with synchronization of paleomagnetic secular variation between four of the lakes (HVT,

C8

HAK, ARN, TORF) and a very well dated marine sediment core off the coast of northern Iceland, provide robust chronologic control and minimal age uncertainties over the Holocene (e.g., Jóhannsdóttir, 2007; Stoner et al. 2007; Ólafsdóttir et al 2013; Harning et al., 2018a). The age model for each lake was constructed by fitting control points with a smoothed spline using the CLAM code (Blaauw, 2010). Analyseries software (Paillard et al., 1996) was used to resample each proxy time series to the same 20-year increments before making the composites.

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.

Response: See response to comment 4.

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

C9

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.

Response: see our response to comments 1 and 2 - We have now changed the figure reference to Fig. 3a,b – as described in the text the all composites includes both temperature change and catchment reactions whereas the BSi (Fig. 3b) is more indicative of the temperature decline. . .

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.

Response: see response to comments 4 and the amended text.

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.

Response: The text has been revised as follows: Although the gradual decline in summer insolation progressively lowered the ELA, the significant stepwise trend in the Icelandic records suggests that strong local to regional feedbacks modulated the primary

C10

insolation forcing. The rate of cryosphere expansion at 4.5-4.0 ka and particularly after 1.5 ka suggests contemporaneous shifts in the northern North Atlantic region. Such episodic ice expansion cannot be explained by the summer insolation forcing alone and requires additional forcing or changes in North Atlantic circulation.. Variations in the strength of the thermohaline circulation, weakening of the northward heat transport of the AMOC and/or increasing influence of the Arctic waters influence all these locations. Changes in the strength of AMOC and/or the subpolar gyre and changes in the Arctic sea ice extent with the associated meridional heat transport into the Arctic have been related to past cooling events, particularly during the last 2 ka (Trouet et al., 2009, 2012; Lehner et al., 2013; Moreno-Chamarro et al., 2017; Zhong et al., 2018).

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

C11

how and when?

Response: We have now edited the conclusions to reflect what we are deriving from the dataset presented.

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.

Response: Acknowledged. Has been corrected.

12. 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 it says decent instead of descent.

Response: Acknowledged. Has been corrected.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-130>, 2018.

C12