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Climate of the Past Discussions

Interactive comment on "Freshening of the Labrador Sea as a trigger for Little Ice Age development" *by* Montserrat Alonso-Garcia et al.

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Response to Reviewer#3 on "Freshening of the Labrador Sea as a trigger for Little Ice Age development" by Montserrat Alonso-Garcia et al.

We kindly thank Reviewer#3 for his very valuable comments. We really appreciate the feedback provided by this reviewer regarding to atmospheric processes, and ice sheet-ocean interactions.

In order to provide context to our replies, the referee's comments have been copied below preceded by "RC" and our replies are preceded by "REPLY". We agree with most of the suggestions to reformulate the text, so below we only included the discussion comments for which we can give an answer.

RC

Alonso-Garcia and coauthors present high-resolution sediment data from Eirik Drift off the southern coast of Greenland. They analyze the amount and mineralogy of icerafted debris (IRD), covering approximately the last 2000 years. The primary results are several episodes of increased deposition of IRD and an increase in hematite stained grains after about 1400 AD. This leads the authors to suggest that Arctic sea ice export strengthened and the Atlantic subpolar gyre (SPG) weakened. As a consequence, less Atlantic waters were present in Greenland fjords, changing the stability of tidewater glaciers, their calving rates and thus the amount of IRD transported to the core site. The original data is compared with several older records from the same region in the subpolar North Atlantic. I think this study contains very valuable new data and that it is a good addition to the existing literature (Copard et al., 2012; Moffa-Sanchez et al. 2014a, Moreno-Chamarro et al., 2016). Text and figures are mostly clear. Previous works are referenced adequately. I do not fully agree with the interpretation of the data, which is rather speculative on several occasions and likely incorrect in some aspects. I will summarize my criticism in 4 major points: 1) Several of the claims made in the discussion are not well supported by the data. In my opinion, the sediment record does not warrant statements about the stability of the calving front of Greenland glaciers. Fjord environments are extremely complex and heterogeneous in their dynamics so that no robust conclusions can be drawn from a single sediment core several hundreds or thousands of miles away. Ice-rafting occurs both during 'cold' and 'warm' episodes, for which two different and only weakly supported explanations are given. Furthermore, whether the MCA and the LIA periods really had a temperature signal in the relevant regions has not been shown in the manuscript. I think they are better characterized as high and low sea ice periods.

REPLY

We agree that finding an explanation for ice-rafting during both "warm" and "cold"periods is challenging, particularly if you see higher IRD deposition during the

MCA. Here the reviewer suggests variations in sea ice as a better explanation for the IRD input. I guess the reviewer means higher/lower sea ice export from the Arctic rather than in situ sea ice conditions, since our site is quite far from the continent. However, we find it difficult to argue in favor high sea ice export during the MCA based on the available proxy records. Instead, we find evidences for increases in sea ice export after ∼1300 yr AD (e.g. (Andrews et al., 2009; Massé et al., 2008), in agreement with the increase in our HSG record. Moreover, our hypothesis of intense calving in SE Greenland is based on records from the SE Greenland coast (Jennings and Weiner, 1996; Miettinen et al., 2015), which show rather high SST, low sea ice coverage and Atlantic water presence in the fjord of this region from 1000 to 1200 yr AD. This interval coincides with the interval of high IRD deposition in our record, therefore, we argued that intense calving occurred in SE Greenland fjords during the late MCA due to warm temperatures and the presence of Atlantic waters.

RC

2) Throughout the paper, references are made to an outdated reconstruction of the North Atlantic Oscillation (NAO) (Trouet et al., 2009). It has been shown that the method of this reconstruction is flawed and that its results are not trustworthy (Lehner et al., 2012). A new and more advanced method shows very different results (Ortega et al., 2015). It is clear that "some" change in the atmospheric circulation took place at 1400 AD that led to changes in the proxy records (Trouet et al., 2009; Olsen et al., 2012), but that change is probably unrelated with the NAO. I really do not see the necessity to relate the original data of this manuscript with the NAO and thus recommend to remove this link altogether.

REPLY

This was also commented by Reviewer#1 and we are eliminating all the discussion related to NAO, as well as the NAO references in figures 3 and 5 (see response to Reviewer#1).

RC

3) The delay between the onset of the stronger freshwater forcing at _1000 AD and the weakening of the SPG _1250 AD, as well as the further lag of 200 years before the cooling of Atlantic waters in the fjords (page 12) are not reasonable and not well supported by evidence. Present-day observations and modeling show that a slowdown of the SPG after weakening the convection in the Labrador Sea takes place within a single season or at most some few years. There is no physical reason to assume a delay of multiple centuries. I believe the same is true for the cooling of Greenland fjords, although I do not know that for certain. I suggest the authors estimate the energy balance and fluxes to support their claim or remove this part. On a more general note, the authors seem to expect a strict determinism underlying their data, which is probably not correct. In the highly variable North Atlantic and due to the strong positive feedbacks associated with the SPG, abrupt changes can happen suddenly and in response to rather minor forcing pulses (Moreno-Chamarro et al., 2016). Not every wiggle will have an easily identifiable cause.

REPLY

We agree with the reviewer that not finding an explanation for every is difficult. This part of the discussion is being rephrased eliminating the interpretations about lags in the forcings.

RC

4) While a weakening of the SPG at about 1400 AD is consistent with findings by Copard et al. (2012), Moffa-Sanchez et al. (2014a) report more frequent changes in the strength of the gyre. How can these views be reconciled? This should be included in the discussion.

REPLY

The subpolar gyre entered in a weaker mode at ∼1300 AD, according to Moffa-

Sanchez et al. (2014a), and after that they registered some SST and salinity oscillations in the record south of Iceland, which indicate oscillations in the subpolar gyre. However, their G. bulloides record from the Labrador Sea (see supplementary data from the same article, Moffa-Sanchez et al. (2014b), and fig. 4 of this manuscript) indicates the Labrador Sea remained rather fresh and cold after 1300 AD. Indeed the Labrador Sea started to get colder at ∼1200 AD. This discussion will be added to the manuscript

RC

minor comments: l 95: Say when this shift occurred.

REPLY

At ∼1350 AD. This will be added in the final version.

RC

l 137: Add a reference.

REPLY

(Born and Stocker, 2014)

RC

l 146: How can robustness be claimed when the key publication by one of the coauthors has not even been submitted?

REPLY

The age model for this record has been included and will be published in this paper (see response to reviewer#2).

RC

l 215: This argument is not convincing. If the westerly winds prevented dust from

being transported upwind to the sedimentation site, they would also have avoided dust from reaching Greenland glaciers as well. However, the climatological wind direction is irrelevant here, because volcanic eruptions are usually short-lived events and dust is transported in the direction of wind at that time. It can not be ruled out that a volcano erupted during a week of predominantly easterly winds, even though that may be the less common situation.

REPLY

With this statement, we just wanted to say that atmospheric conditions do not favor the deposition the volcanic shards in the study area and maybe this is why we can't find any specific layer of volcanic ash. This is going to be rephrased in order to clarify our statement.

RC

l 246: This argument shows that while the small recent variations in Arctic sea ice export correlated with the NAO, large anomalies in export probably have a different cause, like the GSA. Maybe sea ice during the LIA was thicker in the Arctic and so more ice (volume) was exported without a faster flow? This view would be consistent with findings from Miller et al. (2012) and Lehner et al. (2013). Also, I am unsure whether the manuscript discusses the impact of freshwater forcing from sea ice.

REPLY

NAO discussion has been removed.

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