Clim. Past Discuss.,
doi:10.5194/cp-2016-80-RC2, 2016

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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Received and published: 23 August 2016

I read the paper with considerable interest. I would make two comments to start: 1) I have worked on the "upstream" issues of ice-rafting and sediment provenance for nearly 3 decades hence feel reasonable confident to comment on the paper (e.g. Andrews and Jennings, 2014), and 2) I was a co-author on the 2013 Alonso-Garcia et al. paper.

The basic premise behind the paper is that changes in the amount and source of ice-rafted material (IRD) explicitly contain information about changes in the flux of freshwater, hence can potentially provide information on deep water formation. This premise requires that the proxy provides an unambiguous signal linked to freshwater exports, and of course the link is that the IRD is exported to the Erik Drift either in icebergs or

in sea ice.

The paper provides no information on the chronology other than to say it is discussed in a paper that is listed as "in press"âĂŤbut it is not in the reference list. It is also important, in my view, to state what has been used for the ocean reservoir correction and was an error attached to the value? This issue limits how well the chronology can be defined, hence the reliability of correlations with other records. It is a difficult issue that bedevils all of us (see ref. to Sjerup et al. 2010, their ref list). The authors note that Jennings et al (2014) were not able to identify a specific Icelandic tephra in the last 1 cal ka or so, hence it is difficult to constrain the possible ΔR.

I feel quite strongly that there needed to be more discussion on rationale for choosing the > 63μm fraction as an IRD signal (Andrews, 2000). I think the only really unambiguous IRD grain-size signal are clasts > 2 mm (Grobe, 1987), although a solid case can be made for a $\geq$ 250 μm. When the fine sand and greater fractions are being identified, especially on a Drift, then I think an initial analysis should include the entire grain-size spectra (Prins et al., 2002) as this, typically, indicates IRD as a distinct hump at the coarse end of the grain-size spectra. I also note that there is no discussion on iceberg history (e.g. (Bigg, 1999; Bigg et al., 2014; Bigg and Wilton, 2014)) or on sea ice, especially the export of the "storis" (Schmith and Hanssen, 2003).

Finally, the discussion of the provenance of the > 63μm fraction might have usefully identified (on their Fig. 1?) the major tidewater ice streams/glaciers of SE/E/NE Greenland and have referenced the likely annual flux (km3/yr) versus that of sea ice, this would help in trying to establish provenance. For example, coal outcrops in the area of Nansen Fjord, East Greenland, and it has been recorded in sediments on the inner shelf (Jennings, person. Commun. 2010) but I am not sure if this was stated in any of her publications. The issue of the source(s) HSG is an important one given the attention it achieved through Gerard Bond's work. The most probable source is the Devonian outcrop ca 73°N, NE Greenland (Larsen et al., 2008) in the area of Kasjer Franz Joseph Fjord. Several cores were taken from this area during a Polarstern cruise

(Evans et al., 2002; Hubberten and al., 1995; Stein, 2008) although evidence for significant IRD output over the last millennium is muted and the number of tidewater glaciers on the outcrop is limited.

Thus although I have some concerns about the paper I also believe that it represents an important contribution to our understanding of climate change in an area that is critical to our understanding of the Earth's Climate System.

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Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-80, 2016.