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Climate of the Past Discussions

Interactive comment on "Multi-decadal to-century NAO-like marine climate oscillations across the Denmark Strait ($\sim 66^\circ$ N) over the last 2000 cal yr BP" *by* J. T. Andrews and A. E. Jennings

Anonymous Referee #2

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This manuscript presents a synthesis of marine sediment core data across the Denmark Strait elucidating the past 2000 years of oceanographic variability. The proxies include quartz (sea ice) and calcite (surface water stratification and nutrient supply) as well as mineralogical composition of IRD. In analogy with present day conditions in the Denmark Strait, it is suggested that the observed variability over longer time-scales reflects low-frequency NAO-like variability. Major findings include AMO-like changes in water mass stratification in the Kangerdluggsuaq Trough and similarity with the general Arctic summer temperature reconstruction. Maximum sea ice occurrence is observed

around 1350AD and strong water mass stratification is observed 1550 AD. Overall the conclusions are justified, but the manuscript would improve greatly from a more focused section 1-3. A paper like this should gain attention from a broad range of polar climate researchers. A quite high number of cores are included in the synthesis and this really is a major strength of the manuscript, in that many similar studies tend to focus on a few cores and leave out others. However complicated inclusion of a high number of cores may get, it is important in terms of making convincing interpretations in terms of paleoceanographic variability, and clearly the authors master this. More regional synthesis studies of this kind are generally needed around Greenland. Clearly there are chronological issues in this region, particularly the Greenlandic side, however, it is in this regard that the inclusion of a high number of studies helps making a more convincing report on this. Section 1-3 – introducing study area and background The manuscript really would improve from a rewriting of the introduction and the physical setting. A lot of the climate stuff mentioned in the introduction makes more sense after having read the physical background. I suggest cutting down greatly on the introduction and moving some of the climate text here to the physical background section. In the physical background section it would be helpful with a basic outline of climate conditions (atmospheric and oceanographic), followed by an outline of climate variability on different timescales (NAO and AMO), including the inter-relationship with sea ice. This will provide a good foundation for introducing the GSA paradox, which is a quite important conundrum to keep in mind in studies discussing NAO and sea ice variability. Here I would like to draw the attention to Scmith and Hansen's (2003) very fine work presenting an 1840-2000 AD Fram Strait sea ice flux. This work is already mentioned in the manuscript, but it is also worth mentioning their finding of a positive correlation between increased sea ice and NAO+ during the positive AMO phase, breaking down during GSA's occurring at the timing of negative AMO episodes (see also Dima and Lohmann, 2007). Following this a more focused section on interpretation of proxies would come natural – as it is now, the proxy interpretation is mixed into both the physical background section (2) and the working model section (3). A

straightforward and focused section also provides opportunity to keep non-geologists interested in the paper. Page 3874 line 29: 'drift ice can impinge on the shelf during NAO negative circulation'. I am not sure I understand the meaning of this. Generally increased sea ice is linked with a positive NAO index (except during the GSA, when sea ice occurrence was marked and NAO negative). So be careful when linking sea ice with the NAO. Results and interpretation On page 3880 line 6: The inflow of Atlantic waters to the East Greenland fjords are associated with a negative NAO, not a positive NAO. Same page line 8-10 it is stated that the increased rate of iceberg melting would limit the area over which IRD is delivered. May sound plausible, but Mugford's (2010) study of Kangerdluggsuag glacier-fjord actually concludes that the main control on IRD deposition is the calving rate and not the water temperature. So this statement could do with some elaboration and referencing. The main author of the paper has worked a lot on this issue and it is a question often arising in studies of IRD so I think it deserves some attention here. A geological map (highlightning important information) would be very helpful in studies of the provenance of sediment – I advice making the effort of constructing an appropriate map. Page 3883 line 24: 22 % of the sediment is sourced from Kangerdluggsuaq glacier. I just wonder how so relatively much (even if the overall sediment entrainment in icebergs from Blosseville is relatively smaller due to a larger travel distance) sediment can come from Kangerdlugssuaq. The core site is 100 km down the trough – I would think that the absolute majority of icebergs from Kangerdlugssuaq glacier (which are rarely more than 200 m deep according to Syvitski, I think) is diverged directly southward with the EGC/EGCC upon leaving the fjord and thus only very rarely passes the core spot.

3884 line 3-4: 'Over the last 100 years the relative contribution from Geikie plateau has increased'. It should be over the last 100 years of the record – that is 1800-1900 AD. There is a big difference. Same page line 23-26 – the comment on the Lowell paper: Either remove this comment or – if needed for this paper – elaborate. 3885 line 1: 'the older interval' – write 'the older calcite minimum interval' Section 4.3. Heading of section: just keep it to 'N/NW Iceland' and remove the conclusion (none to variable

sea ice, variable IRD) to the end of the text in this section. The same for section 4.4. Discussion The first 12 lines in the discussion would fit in better in the physical setting. With a focused physical setting highlighting modern conditions and climate variability, this provides a good foundation for discussing the analog scenario between variability for the past 2000 years and modern conditions. This is really the heart of the paper and could be more focused in the discussion. As it is now it mostly focuses on the statistics (although these are of course essential) I find that discussion of the causes of the LIA is a bit scattered around in the paper (Lamb's GSA analog, Miller's volcanic forcing). The paper would become more interesting with a short focused section outlining the generally believed forcing mechanisms behind the LIA cooling. I also think that the climatostratigraphical units should be referred to as either their classic denominations (DACP, MWP, LIA) or to the Wanner episodes – not both – that may confuse some. Also highlight these episodes in all figures. Nansen fjord: Why was biogenic production so high (stratification so low) during the supposedly cold DACP (sea ice was also high)? It was low as expected in Kanger Trough. Page 3887 line 20-22: I advice to put in geographical names after mentioning core numbers in the text. Then the reader doesn't have to go back and forth between text and figure to keep track. Figure 8: try to recompose. It contains the calcite wt% twice. I suggest to put geographical names as headers onto figures 8not only in figure text) to make overview easier. Fig. 11C: Can we assume then that the blue line is calcite? Page 3879 line 4. Jennings et al. 2013, not listed in reference list. Page 3874 line 24: should be Andresen et al. 2012

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