

## ***Interactive comment on “Reconstruction of Atlantic Water variability during the Holocene in the western Barents Sea” by D. E. Groot et al.***

**Anonymous Referee #1**

Received and published: 13 September 2013

General comments: The paper is clearly written and well presented, and the authors respond to the given purpose of the paper. The authors presents new high-resolution records of Holocene bottom water conditions from the western Barents Sea, and discuss them in relation to other studies from the area. The paper does not present any major new concepts.

The authors could elaborate on and discuss more potential problems related to their reconstructions (e.g. calculated temperature and salinity) and how these relates to their other proxy records (e.g. when the  $\delta^{18}\text{O}$  results indicating opposite responses than seen in the foraminifera based temperature and salinity responses). It would also have been interesting to see a more in depth discussion on implications of their results, and on what their results add to the knowledge of Holocene climate development in the

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region, beyond statements like “this has also been seen in other records”.

Furthermore, the carbon isotope data is included as results but almost not mentioned and not really included in the discussion; these data should be better implemented in the story told. There is also a misunderstanding some places regarding the relationship between forcings, responses and mechanisms that has to be cleared out.

Specific comments:

1. Abstract, line 8-9: The term “increased influence of Atlantic Water” should be clarified – what do you really mean? Stronger current? Warmer current? ...? Check through the paper how this and similar terms/sayings and used and make sure that it is clear what it meant. Presence of sea ice does not necessarily contradict increased temperatures of the bottom water, and does not necessarily imply larger heat transport into the area. Can the increased temperatures potentially be a response to reduced loss of heat if the sea ice reduces the loss of heat from the ocean (Gerdes 2003)?
2. Introduction, line 26, page 4294: “even small variations”. Be more specific; how small is a small change? Is the small change small also compared to the variability seen in instrumental records, where the response is known, or is it small compared to e.g. interglacial-glacial scale changes?
3. Introduction, line 7, page 4295: Distribution of polar water will be a response to other forcing factors, not a factor in its self. And atmospheric forcing (winds) is important not only at local scales, but also for large scale ocean circulation.
4. Introduction, line 9-10, page 4296: “reduced temperature and salinities” – relative to what? Specify or rewrite.
5. Chronology: The same dates and age model is used in Berben et al. submitted to the same issue as this paper. The dates not previously published are in Berben et al. given as new in that study. In Groot et al. nothing is said regarding the new dates, hence, implying that they are originally published in Groot et al. It should be clear

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which paper presents the age model for the first time, and the other should present the chronology but refer to the paper presenting the dates and the established chronology.

6. Results, 4.2 vs. 4.3 vs. 3 material and method: Based on the given resolution of the records, it is not clear how your records relate to each other. Does your records have overlapping results from the same samples? For TOC/TC you say you have measurements every 4 cm, foraminifera counts every 143 yr and isotopes every 82 year. Please clarify the given information. From the given info it e.g seems impossible that you have counts and isotopes in the same samples. For long term trends this may be of less importance, however for direct comparison between events in the records it is important to know the basis for comparison.

7. Section 5: Specify ages within the different periods discussed, e.g. line 28 page 4302 "The subsequent decrease of . . ." When did that happen? Information is given for some of the discussed transitions, but it would be helpful if you would consistently provide such information.

8. Page 4303, line 19: Specify that you mean bottom temperature as calculated from the benthic foraminiferal fauna.

9. Page 4303, line 18 - page 4304, line 6: You argue that the fauna data indicate an increase in Atlantic water species. The fauna data is the basis for the calculated bottom water temperatures that declines. It is not clear how you will explain this intuitively contradicting information. In addition the  $\delta^{18}\text{O}$  data is stable. You try to explain this discrepancy, however, try to clarify this paragraph by first giving your explanation to the fauns/bottom water temperature discrepancy (influence of species with a strong response to food supply in addition to temperature). Thereafter present the  $\delta^{18}\text{O}$  and the contradiction between  $\delta^{18}\text{O}$  and foraminifera based bottom water temperatures, followed by a discussion on potential explanations for this discrepancy. I am not convinced by your seasonality explanation. Following your argumentation on page 4305 (ref to Risebrobakken et al., 2011) there is no big seasonal changes at the water depths

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that your measurements and fauna represents?

10. You do have independent bottom water temperature and salinity; have you tried to calculate  $\delta^{18}\text{O}$  based on this information, to see how they would compare?

11. Page 4304, line 23: What is the argument for lowered influence of Arctic water? Please specify. No distinct change is seen in temperature, salinity,  $\delta^{18}\text{O}$  or in relative abundance of any of the foraminifera species.

12. Page 4304-4305: Is there consistence between the timing of when the different records record warming?

13. Page 4305-4306, line 26-29 + the rest of the paragraph: Rewrite. Awkwardly written, and too simplistic to say that insolation decrease so therefore the  $\delta^{18}\text{O}$  record cools. This is not consistent with your own fauna based temperatures, or with several studies indicating that insolation don't have a strong direct effect at temperatures of the depth of your core (Jansen et al., 2007; Andersson et al., 2010; Risebrobakken et al., 2011). Comparing your fauna temperatures and the  $\delta^{18}\text{O}$  records, it is not just the long term trends that are different, but also in details of variability. You should include a discussion on potential explanations for this. What are the differences? Which explanations can be suggested? What are your preferred explanation, and why? After establishing what you believe from your own data you can compare to other records. Delete line 9-12 page 4306 – or you need to include a discussion on to why these records should be relevant for your bottom water temperature development. There is just as much literature out there telling you that you should not expect these records to behave in the same manner as yours.

14. Page 4306, line 19-21: What are the arguments for a stronger inflow of Atlantic Water? This is also observed. . . Specify. What is "this" – coarser grains? Stronger current?

15. Page 4307, line 3-4: This is the only time you mention the  $\delta^{13}\text{C}$  record. Either

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delete it out and don't use it, or include/incorporat the record in your full story.

16. Page 4307, line 16: The changing bottom water conditions. . . Specify what you mean.

17. Page 4307, line 20: Elaborate on different timing between different cores. Is it a dating issue, or is it related to real differences, and if so, what may that imply?

18. Page 4307 - 4308: How do you link your bottom water temperatures to insolation changes? – see comments above as well. Insolation should not have a direct effect on the bottom water temperatures at your site. Why do you thin insolation explains your signal, and what is the physics behind it?

Except for the age model, no references is made for the Berben et al., paper submitted to the same special issue, a paper that discusses the same core. This is fine as long as you keep focus on bottom water conditions, but as soon as you involve other surface data in the discussion (and you do) it seems strange that you don't refer to the Berben et al., study as well. Is there consistence between the interpretations, implications of the interpretations and stories told between the two papers?

Figure 5. I recommend you to reverse the axis direction of the salinity plot so that the changes read in the same direction as the potential change in  $\delta^{18}O$ .

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Interactive comment on Clim. Past Discuss., 9, 4293, 2013.