

In the following we respond to the suggestions and recommendations by the reviewers, D. R  ther and the editor. The original comments are followed by our reply.

## 1 Comments from Reviewer 1 (Anonymous)

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 are 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)?

In the abstract we refer to an enhanced influence of Atlantic Water and thus a decreased influence of the colder Arctic Water masses.

We have clarified the terms influence and inflow throughout the paper; see also the answer to Reviewer 2 on this point.

The increased temperature in the Early Holocene could also potentially be a response to the reduced heat loss to the atmosphere due to shielding by the ice cover as suggested. We included this in the paper. However, we still infer an increased influence of Atlantic Water based on the benthic foraminiferal assemblages which show an increase of Atlantic Water associated species such as *C. neoteretis*.

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?

We have rewritten this section to clarify that the small changes are small compared to glacial-interglacial 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.

We deleted distribution of polar waters as a forcing since this indeed is a response.

4. Introduction, line 9-10, page 4296: “reduced temperature and salinities” – relative to what? Specify or rewrite.

We have rephrased this sentence.

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 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.

We feel it necessary for both papers to present and discuss the new age model with the data presented in the respective papers. Therefore Groot et al. and Berben et al. will both present it as new.

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.

Samples for TOC were taken every 4 cm in 1 cm thick slices. Samples for isotopes and counts were taken from the 0,5 cm thick slices. Therefore, isotopes and counts were done on the same samples, yet TOC on different ones. Sample that are counted are also analyzed for isotopes. Some samples for isotope analyses had to be combined (10 500 cal yr BP and older) due to the low amount of *C. neoteretis* in this part of the core. For these combined samples we get a slightly different age compared to the counted samples.

In the manuscript we have included slice thickness for TOC/TC to document that these analyses were done on different samples, we included sample resolution for counts and we highlight that isotopes were analyzed on higher resolution.

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.

We added the age when this was unclear.

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

We introduced the abbreviations  $BWT_{TF}$  (bottom water temperatures calculated by transfer functions) and  $BWS_{TF}$  (bottom water salinities calculated by transfer functions) and use these abbreviations throughout the paper.

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  $\delta^{18}O$  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  $\delta^{18}O$  and the contradiction between  $\delta^{18}O$  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 that your measurements and fauna represents?

We have rewritten this paragraph and restructured it. We do not expect big seasonal changes; therefore we included the possible influence of salinity on the  $\delta^{18}O$  signal.

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

We refer to our answer to Reviewer 2, first comment.

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,  $\delta^{18}O$  or in relative abundance of any of the foraminifera species.

We indeed see no distinct change in temperature, salinity,  $\delta^{18}O$  or in relative abundance. The lithological properties however clearly indicate a shift in current regime. Berben et al. report presence of (seasonal) sea ice until 9500 yr BP, and infers that the subsurface waters are changing

from Arctic to Atlantic at this time. We therefore speculate that there was transport of Arctic Water, winnowing the shallow banks enclosing Kveithola, to Kveithola and depositing the sediments at the core location. In this situation only the (sub) surface waters were influenced by Arctic Water up to 9500 cal yr BP and the deeper waters in the trough were occupied by Atlantic Water before and after the transition in the surface waters.

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

There is consistency between the timing. For Kveithola and the western Svalbard shelf age of ca. 11.5 are reported. The northern Svalbard margin is slightly delayed at a reported age of 10.9. We included this in the paper.

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.

Delete lines as suggested (9-12 page 4306). We calculated  $\delta^{18}\text{O}_{\text{sw}}$  to determine the influence of salinity. We also compare our measured  $\delta^{18}\text{O}$  values to  $\delta^{18}\text{O}$  calculated from independent temperature and salinity data as suggested in point 10. Salinity changes only explain a small part of the  $\delta^{18}\text{O}$  trend, which therefore must reflect decreasing temperatures. We discuss the offset between  $\delta^{18}\text{O}$  and  $\text{BWT}_{\text{TF}}$ .

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?

Arguments for a stronger inflow is the coarser grain-size. We have rewritten this part.

15. Page 4307, line 3-4: This is the only time you mention the  $\delta^{13}\text{C}$  record. Either delete it out and don't use it, or include/incorporate the record in your full story.

We deleted the  $\delta^{13}\text{C}$  record since this study focuses more on temperature and salinity changes.

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

This sentence is rephrased.

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?

We included a short discussion on the implications of timing differences.

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 think insolation explains your

signal, and what is the physics behind it?

For the Late Holocene we do not observe a change in bottom water temperatures. The environmental conditions during the late Holocene are a result of a stronger current regime and a change in surface water conditions. We refer to Berben et al. for a more detailed discussion on the surface water characteristics.

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?

Since the paper is now available we included the findings from Berben et al. in our discussion.

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  $\delta^{18}\text{O}$ .

Followed recommendations and reversed axis.

## 2 Comments from Reviewer 2 (Anonymous)

The authors have the unique advantage of paired samples of  $\delta^{18}\text{O}$  and estimated transfer function bottom water temperatures. Thus, offering the opportunity to calculate the ambient  $\delta^{18}\text{O}$  of seawater ( $\delta^{18}\text{O}_{\text{sw}}$ ). I therefore ask if the authors have considered calculating the  $\delta^{18}\text{O}_{\text{sw}}$  and compare for example to the transfer function estimated salinity. According to the core location, in close proximity to the Arctic front, water mass properties such as salinity is expected to vary during the Holocene. Could the Holocene  $\delta^{18}\text{O}$  signal be more influenced by salinity changes than previously believed?

In any case, temperature reconstruction using benthic foraminifera transfer functions in conjunction with  $\delta^{18}\text{O}$  calcite - enabling the calculation of  $\delta^{18}\text{O}_{\text{sw}}$  - would be a step forward.

We calculated  $\delta^{18}\text{O}_{\text{sw}}$  and  $\delta^{18}\text{O}_{\text{calcite}}$  and compared these to the measured  $\delta^{18}\text{O}$  and results from the transfer functions (Fig. 5). We included the results in paragraph 5.2 (Early Holocene) and 5.3 (Mid Holocene).

Did the authors consider the use of statistical programs (e.g. MultiVariate Statistical Package or Regime Shift Detection) to confirm your division of Holocene into different sections? With for example cluster analyses the down-core data can be divided into foraminiferal assemblage zones. This would help writing up the results and help the reader to follow up on your data description.

Confirming our division of the Holocene by the use of a statistical programme is a good suggestion. However, our current division is based on some distinct shifts in benthic foraminiferal assemblages and sediment properties which in our opinion justify the division made.

The present day bottom water temperature and salinity at the core site is nicely shown on figure 5. Consider also including it in the text, especially the difference in present day salinity and estimated values for the site today. Does it fall within the error bars of the transfer function reconstruction?

We added the error bars on both reconstructions in Fig. 5. From this it shows that present day values fall within the error bars.

The authors refer to other studies from the region and even from the same core. Could the most important comparison data be plotted along with the results? That would

help the reader to follow the interpretation, which often turns out to be conformation of previous findings (which at times made me wonder: What is the step forward with this paper?).

We made a compilation figure (Fig. 6) with data from the SW Barents Sea (Risebrobakken et al., 2010) and western Svalbard margin (Skirbekk et al., 2010).

The retreat of the Arctic front is quite important. What is the timing of the marginal retreat? That is an important conclusion also in comparison to other records in the region.

Based on the benthic foraminiferal assemblages the retreat of the Arctic Front is placed slightly earlier than what is reported for the surface waters (Berben et al.). We include the precise timing in the paper.

In section 5.2 it is a bit hard for the reader to follow, it might help if presented in chronological order (the authors take the reader a bit back and forth in time). I thought it was confusing that the different proxy results (interpreted in section 5.2, 5.3, 5.4 - see below) were all explained with stronger inflow of Atlantic Water (consider other explanations or differentiate from faster flow of the current and warmer temperatures in the current, does that always hold hands - intensification with or without getting warmer):

We re-arranged paragraph 5.2 in chronological order as suggested.

5.2 "A stronger inflow of Atlantic Water in the Early Holocene..."

Has been rewritten to enhanced influence, e.g. influence of Arctic Water diminished. There has been a temperature change due to change in water masses and indeed no change in current speed.

5.3 "... due to the strengthened and constant inflow of Atlantic Water" (Can it be both strengthened and constant? Do you mean constantly strengthening inflow?)

Rewritten to strengthened inflow. The inflow has increased compared to the Early Holocene and does not change during the Mid Holocene.

5.4 "... due to a stronger inflow of Atlantic Water at the western Barents Sea margin."

In the Late Holocene we infer an increase in current speed, hence the term stronger inflow.

Different timing of the onset of the late Holocene cooling in the region is introduced in the 3rd paragraph in section 5.4. Could the authors please discuss the reason for that?

This was done in order to show the variability of the Late Holocene as recorded by different records from the Barents Sea and Svalbard region. We discussed this section more thoroughly as suggested by Reviewer 1 (point 17).

In conclusions the terms polar and subpolar fauna are introduced for the first time. Consider giving an example earlier in the manuscript of polar and subpolar fauna.

We introduced the terms polar and subpolar in the discussion of the Early Holocene.

In the manuscript the authors talk about "distribution patterns of benthic foraminifera."

My understanding of distribution pattern is when referred to a study of how the foraminiferal species are distributed in a specific area (e.g. comparing surface cores).

Please consider to add "down-core" distribution patterns or use "the diversity of benthic foraminifera."

We follow the recommendations and added the term "down-core" in order to avoid confusion.

The paper reads better if the verbs are always in the same tense. Please be consistent throughout the manuscript, for example in results; the sedimentological section is in

present but the foraminiferal section is in past and then again the stable isotope section is in present.

We corrected the tense.

The English in the manuscript could be improved, several minor grammar and spelling errors occur.

We checked the English.

Figure 3: It is difficult for the reader to see which curve is the flux and which is the concentration, please clarify. The authors could clarify figure text by identify panels with A, B, C. Number of species/samples is introduced in a wrong order.

We used lighter grey for concentration, added explanation in figure caption and identified panels.

Figure 4: Please consider using transparent color (or lighter tone of gray color), because data is lost behind the black curves.

We used a lighter color as suggested.

Figure 5: It could be helpful to zoom into periods of interest, for example zoom into the last 10.000 yrs BP to better outline the variability in the dataset.

We increased the length of the Y-axis to better outline variability instead of presenting the same data twice.

Figure 5: A 5 point mean of the data set is plotted (does the dataset need to be presented with lower resolution?). What does the 5 point mean add to the interpretation/conclusion (is the transfer function estimated values and the  $\delta^{18}\text{O}$  values not with the same sample resolution?).

We choose to add a 5 point mean in order to show the long term trend more clearly.

The transfer functions estimated values are on a lower resolution than the  $\delta^{18}\text{O}$  values, but for every faunal count we have a  $\delta^{18}\text{O}$  measurement (see also answer to Reviewer 1, point 6).

Please consider including error bars on the transfer function estimates in figure 5.

Error bars for the transfer functions are added.

In figure 5  $\delta^{13}\text{C}$  is plotted. If it is presented please include in results. Beside from the method section it is only mentioned once in the manuscript (one sentence in second paragraph in section 5.4). Consider what it adds to the presented study.

We deleted the  $\delta^{13}\text{C}$  record, see our answer to Reviewer 1, point 15.

### 3 Comments from D. R  ther

As the authors point out under 3. Material and methods, the lithology of the studied core has previously been described in R  ther et al. 2012. Unfortunately, the authors did not deem it necessary to comment on and discuss observations and ideas presented in R  ther et al. 2012 which in my opinion are rather central to major conclusions in the paper at hand. It is pointed out in R  ther et al. 2012 that a major erosional boundary is present at 85 cm core depth which with the presented age model in Groot et al. would correspond to roughly 9400 cal ky. This statement was based on the observation of a sharp, undulating transition from clay into bioclast-rich sandy mud as well as the occurrence of distinct sand lenses below that boundary. The authors may disagree that these observations have any significance to their study. The sharp transition from mud to bio-clast rich sandy mud may well be explained by local shifts in current regime as suggested here, but I would

nevertheless encourage the authors to discuss the possibility of the presence of an erosional boundary.

The authors agree that an erosional boundary is present in the studied core at 84/85 cm depth. In the age model presented by R  ther et al. the erosional event dates to 8.5-8.2 cal yr BP. In the present study, Groot et al. present a new date from 80-81 cm depth, just above the erosional event. The authors excluded the dates performed on mollusks (see section 3. Material and Methods). Since we do not have a date from directly underneath the erosional contact and there are different lithological units from 85 to 110 cm depth (the next dating) we can only speculate on the sedimentation rates and therefore range of the hiatus. It could be possible that the data in this depth range might be older than presented in the paper (based on extrapolating the oldest sed. rate towards present). But again, this would be speculation.

Based on the fact that the changes we observe in our record are comparable in timing to other studies in the region, we infer that the hiatus in the record is of limited time range.

Further, R  ther et al. argue for a discontinuous deposition in core KA11 with a very low sedimentation rate or a hiatus between 8 and 1.5 cal kyr BP. Additional dating performed by Groot et al. for the Mid Holocene section shows that there is continuous deposition at the core location, thereby ruling out more erosional events or discontinuous deposition.

#### 4 Comments from Editor J. Giraudeau

- Calculations of  $\delta^{18}\text{O}$  water and  $\delta^{18}\text{O}$  equilibrium calcite ( $\delta^{18}\text{O}_{\text{ec}}$ ), based on your independent bottom water temperature and salinity estimates, would expand the discussion on the question of  $\delta^{18}\text{O}_{\text{w}}$ /salinity mixing line, isotopic disequilibrium (benthic foram  $\delta^{18}\text{O}$  vs  $\delta^{18}\text{O}_{\text{ec}}$ ), and might help explaining some of the inconsistencies pointed out by the referees between foram  $\delta^{18}\text{O}$  and bottom temperatures for instance.

We followed the recommendations made by Reviewers 1 and 2. We refer to our answer to Reviewer 2, point 1.

- The term "influence of Atlantic water" should be more clearly defined throughout the manuscript : volume transport/speed or temperature (or both) signatures?

We have clarified the terms throughout the manuscript.

- Also, on various occasions in the manuscript, it is not clear whether "bottom temperatures" refer to values estimated from transfer functions or derived from  $\delta^{18}\text{O}$ .

When referring to bottom water temperatures derived from transfer functions we used the abbreviation  $\text{BWT}_{\text{TF}}$ .

- A restructuring of section 5.2 (see comments by referee #2), as well as a more thorough interpretation of the mid-Holocene interval (section 5.3, referee #1) are needed.

We restructured paragraph 5.2 and discussed paragraph 5.3 more thoroughly.

- Please discuss the  $\delta^{13}\text{C}$  record (bottom water ventilation, organic matter flux, comparison with other existing record from nearby locations (eg. Rasmussen and Thomsen (Geology, 2009)) or remove any references to this dataset in the text and figures.

Deleted, see our answer to Reviewer 1, point 15.

- Brine water convection is a very hot topic in the studied area; your dataset (benthic  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$ ) might help investigating the presence (or not) and impact of such a phenomenon in the western Barents Sea throughout the investigated time period (see above reference, as well as the Holocene record of Sarnthein et al., Boreas, 2003). Though I am aware that such a topic would deserve a manuscript on its own.



Brine water convection is indeed reported to take place in the western Barents Sea. A well studied area is Storfjorden, from this location  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  records are available (Rasmussen and Thomsen 2009), both from the brine basin and from brine overflows. From this location brines are characterized by high  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  values and low temperatures. Salinities of brines from Storfjorden can vary between 34.8 and 35.1 (Schauer 1995). Midttun (1985) reports that brines formed in the mid Barents Sea are characterized by temperatures around  $-1^{\circ}\text{C}$ .

Sea ice, a pre-condition for brine convection, was present at Kveithola during the transition period and the Early Holocene (Berben et al., 2013). During the transition period and the Early Holocene the  $\delta^{18}\text{O}$  values of ca. 3.4‰ were lighter than the values of 4 to 4.4‰ observed for brines by Rasmussen and Thomsen (2009)(see Fig. 1). Also,  $\text{BWT}_{\text{TF}} > 0^{\circ}\text{C}$  are too high for brines.

It is difficult to compare our  $\delta^{13}\text{C}$  values to the records from Storfjorden since we measured  $\delta^{13}\text{C}$  on an infaunal species (*C. neoteretis*) whereas Rasmussen and Thomsen measured on the epifaunal species *C. lobatulus*. Infaunal species tend to have a lighter  $\delta^{13}\text{C}$  due to oxidation of organic matter which causes depletion in the  $\delta^{13}\text{C}$  of sediment pore water (Bauch et al., 2004). Klitgaard Kristensen et al. (2013) also showed there is a substantial difference between  $\delta^{13}\text{C}$  values of *C. neoteretis* and *C. lobatulus*. When we compare the  $\delta^{13}\text{C}$  values from Storfjorden and Kveithola (both corrected for vital effects), values in Kveithola are lower than in Storfjorden and indicate a poorly ventilated water mass.

Based on the available data, we cannot show that brine water convection took place in Kveithola Trough.

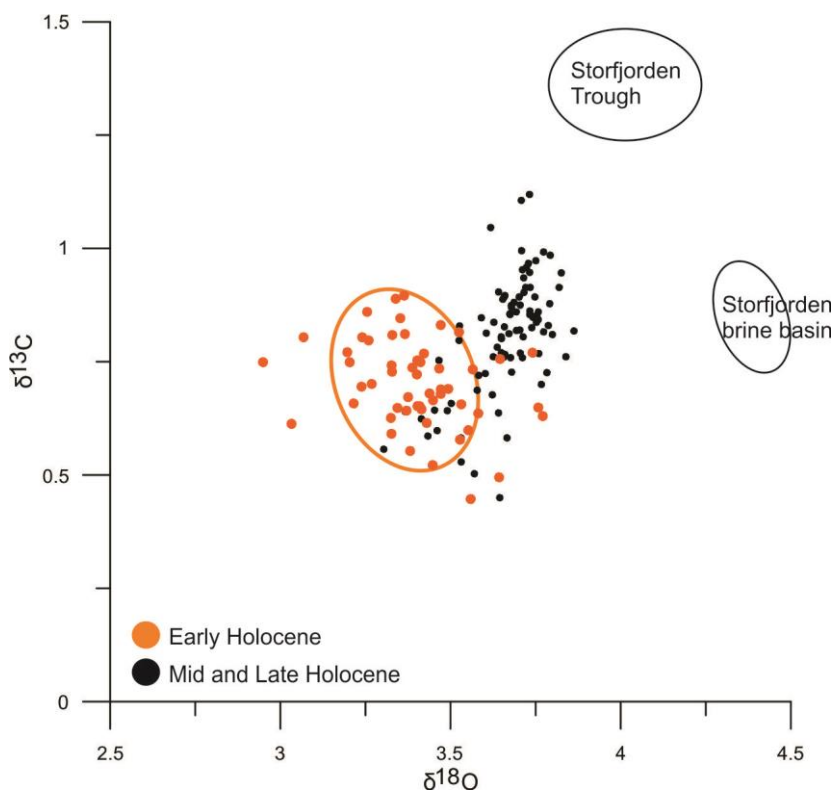


Fig. 1 Comparison between  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  values from Kveithola Trough with brine values from Storfjorden (Rasmussen and Thomsen, 2009).

- Finally, your final response should include a comment to the point raised by R  ther about the erosional feature within the early Holocene part of the sediment core.

See our answer to D. R  ther.



## **5 Additional comment from the Authors:**

We have discovered a consistent mistake made in the age model. It is necessary to correct any mistakes made and therefore we have updated our age model and figures accordingly.

### **References:**

Bauch, H.A., Erlenkeuser, H.B.D., Mueller-Lupp, T., Bauch, T., teldenkova, E.: Stable oxygen and carbon isotopes in modern benthic foraminifera from the Laptev Sea shelf: implications for reconstructing proglacial and profluvial environments in the Arctic, *Marine Micropaleontology*, 51, 258-300, 2004.

Midttun, L.: Formation of dense bottom water in the Barents Sea, *Deep-Sea Research*, Vol. 32, No. 10, pp. 1233 to 1241, 1985.

Rasmussen, T.L., Thomsen, E.: Stable isotope signals from brines in the Barents Sea: Implications for brine formation during the last glaciation, *Geology*, 37, 903-906, 2009.

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