Dear Reviewers,

On behalf of myself and my co-authors, I would like to thank you all for your supportive comments and generous suggestions to assist us with improving our manuscript.

Please see our responses typed in italics.

Response to Referee#1:

The main point is moderate revision is needed in which a convincing case is made for the significance of the paper for deglacial & Holocene paleoceanography not only for the Barents Sea Svalbard region, but for the eastern Arctic where AW flows into.

We agree, and the following additional sentence has been added to the introduction: "Since the studied sediment core was retrieved from an oceanographic frontal zone, sensitive to larger-scale changes, we believe that the presented data show the general climatic/oceanographic trends in the eastern Arctic."

Wollenburg did excellent paleoceanographic and paleobiological studies of the last 15 ka, in the Barents Sea region, using forams and other proxies. Her records must be cited and I think plotted against the new records.

We could not find articles by Wollenburg containing paleoceanographical and paleobiological studies of the last 15 ka, in the Barents Sea region, using foraminifera. We have checked her available articles from the Arctic region:

Wollenburg, JE; Kuhnt, W (2000) The response of benthic foraminifers to carbon flux and primary production in the Arctic Ocean., 40(3), 189-231

Wollenburg, JE; Kuhnt, W; Mackensen, A (2001) Changes in Arctic Ocean paleoproductivity and hydrography during the last 145 kyr: the benthic foraminiferal record., 16(1), 65-77

Wollenburg, JE; Knies, J; Mackensen, A (2004) High-resolution palaeoproductivity fluctuations during the past 24 kyr as indicated by benthic foraminifera in the marginal Arctic Ocean., 204(3-4), 209-238

Wollenburg, JE; Mackensen, A; Kuhnt, W (2007) Benthic foraminiferal biodiversity response to a changing Arctic palaeoclimate in the last 24.000 years., 255(3-4), 195-222

The only core discussed by Wollenburg located in the Barents Sea region (2138; Wollenburg et al., 2004) lies at a great distance from our coring site and spans the last 24 ka.

There is no reason given why the O^{18} is not corrected for sea level ice volume of vital effects. But in fact Fig 8 does correct.

Data on Fig. 6 have been corrected for the sea level ice volume. However, there is no reliable vital effect correction created for E.excavatum f.clavata. Ślubowska et al. (2007) noted that the vital effect of E. excavatum is not consistent, but seems to be large and variable as confirmed by several other studies. Poole et al. (1994) and Polyak et al. (2003) recorded negative values, whereas Bauch et al. (2004) observed both positive and negative values. Due to these uncertainties, they followed Knudsen et al. (2004) and adjusted the $\delta^{18}O$ values of E. excavatum to the corrected values of M. barleeanum, resulting in a correction factor of 1.4‰. Rasmussen et al. (2007) used the similar method, while they have corrected the values by correction of +0.7‰. Therefore we have decided not to correct the values for the vital effect, particularly that the paleotemperature was not calculated from the $\delta^{18}O$.

The sections of the paper are fairly straightforward and descriptive, and perhaps redundant.

In our view, a detailed description is needed for further discussion. However, minor changes have been made in this section.

What is missing is a critical discussion of why this new core is so important given all the previous studies. For example, warm productive periods during H-events have been proposed – what about these?

Our study concerns the Younger Dryas, which sometimes is seen as the H0. Earlier H-events remain beyond the reach of our sediment core.

Mid depth warming in the Arctic during glacial periods is also proposed due to Halocline changes. Does this deglacial record support this?

Our data from the glacial period concerns subglacial unit of the sediment (no foraminiferal data).

How fast are ocean temperature changes compared to let's say the Greenland ice core records of the YD? Decadal?

The resolution of our sediment core (Ice Proximal Unit) varies from 40 to 100 years. In our opinion, inclusion of such a comparison would be speculative.

How do deglacial ocean changes compare to those Spielhagen and others have shown for the last century in Fram Strait?

Though this question is very interesting, it is beyond the scope of this study as we are unable to compare the range of changes Spielhagen et al. (2011) have shown for the last century, as we didn't calculate the paleotemperatures from stable isotopes signal. Moreover, in our record there is a limited amount of data regarding surface water condition (only few planktic foraminifera specimens were found).

Does sea level rise affect the regional oceanography?

Yes, this problem is discussed in our manuscript in paragraphs: 5.3 Glaciomarine unit I; 5.5 Glaciomarine unit III mainly in relation to near bottom sea currents.

Figure 7 compares IRD to GISP ice core. What about other sediment proxies? What about other papers on the YD from the Barents Sea-Svalbard region? The current paper would be an ideal place to review the paleoceanography of the deglacial Holocene from this region, especially the YD, which is complex and the subject of many papers.

Additional information has been added to the revised version of this manuscript.

Figure 8. What are the key messages to derived from this isotope figure?

The key messages of Fig. 8 are showing that all the records are shifted towards lighter values in the early Holocene however, the record from our core remain mostly depleted. This proves that the records located on the western and northern shelf of Svalbard directly mirror the effect of warmer Atlantic water inflow, while record from Storfjordrenna shows the influence of isotopically lighter Arctic Water from the Barents Sea (Duplessy et al., 2005).

The early-Holocene period is often described as being influenced by an intensified inflow of Atlantic water to the Nordic Seas. Although we have observed in our record signs of warming (e.g. increasing foraminiferal biodiversity), the glacier head still remained in the proximity of the coring site. Therefore, the process of transition from Arctic water domain to the Atlantic water domain occurred later than further to the west.

I have trouble seeing significance in the low IRD measurements in Fig. 9 and the huge *norcrossi* increase without comparable IRD. Perhaps Polyak and Solheim overestimated *norcorssi* as a direct sea ice proxy. Much more sophisticated sea ice proxies have been used, some including other foram species [ie not *norcrossi*]. Moreover there is a large literature on the Neoglacial in these high latitudes, I would expect this Fig 9 to address neoglacial climate and ice activity from the study region. So I think this part of the study on IRD & sea ice is in need of revision or omission, it is simply not that strong an argument.

We agree. Fig. 9 and the related discussion has been removed from the manuscript.

Page 4 Svalbard/Barents Sea Ice Sheet – there must be a slash or hyphen after Svalbard

It has been corrected.

Page 4-5 AW = Atlantic Water, this is convention. ArW = Arctic water is too similar and other terms are used/preferred PW=polar water, surface etc. Also page 6 "Surface water" is used, but it is confusing because it does not designate an origin of the water mass. Plus SW is abbreviated in caption but not here in text.

We disagree. The abbreviation "ArW" is convention too, often used in oceanographic and paleoceanographic literature (e.g. Loeng, 1991; Cottier et al., 2005; Skogseth et al., 2005; Majewski et al., 2009; Jernas et al., 2012; Rasmussen and Thomsen, in press). Arctic water is connected with East Spitsbergen Current and its salinity is between 34.3 and 34.8 (Loeng, 1991)

Polar water (PW) is a mixture of Atlantic and Arctic waters and it is found in the western Barents Sea (Loeng et al., 1991). The salinity of PW is higher and varies between 34.8 and 35.0, while its temperature oscillates between -0.5 and 2.0 (Loeng et al., 1991; Haarpaintner et al., 2001).

Surface water (SW) is abbreviated in the text of water masses (section 2.1.). SW is formed locally and is described as *the upper 50 m; cold and fresher during the autumn and warm and fresher due to ice melting, during the summer (Skogseth et al., 2005).*

Page 6. Brine-enriched [lwer case 'b'], Page 7 Percentages: delete extra "p"

It has been corrected.

Page 10 The lithological description is important but perhaps too long for the main text, can it go in appendix/supplement? Also it is really a geochronological section , not simply litho description.

In our opinion this section is important as a base for further interpretation and discussion. We would prefer to leave it as it is. The title of this section has been changed into "Sedimentological and geochemical parameters".

Page 11 line 10. THE benthic: : : – add "The" , Page 12. Isn't *Buccella frigida* spelled with two "c's" ?

It has been corrected.

Page 12 general. This foram sequences seems really common and important in post glacial deglacial deposits of the N hemisphere. Not just northern Europe. Perhaps point this out with references.

We have increased the number of references: Vilks, 1981 from Scotian and Labrador shelves (Canada); Osterman and Nelson, 1989 from eastern Baffin Island continental shelf (Canada) and Polyak and Mikhailov, 1996 from the southeastern Barents Sea.

Page 15 references to the Agassiz out flow need updating. Rayburn et al. 2012, Cronin et al. 2012 in St Lawrence Valley, Murton & Tarasov & Peltier, in MacKenzie, Spielhagen in the Arctic, etc.

It has been updated. Two recent references by Murton et al., 2010 and Cronin et al., 2012 have been added.

Page 16 line 5 , first part of sentence is incorrect grammar,

It has been corrected.

line 6 glaciomarine spelling,

It has been corrected.

Page 17 line 12 should it be Mid-Holocene ??

It has been corrected.

Page 20 line 13 Rasmussen WHO noted[note which noted], line 20 until today [not until present days], 21 sea-ice hyphenate when used as adjective

It has been corrected.

Page 20 and elsewhere. Will the reader be confused if several geographic terms are used to refer to the study region: ie., Edgøya area – which is not identified in Figure 1 map, Storfjordrenna. Please label all place names and ocean currents and water masses that are mentioned in the text in the figures.

It has been corrected. All of the places described in the text have been label in Figure 1.

Page 35 caption, What about the core NP94-51 located in the inset map?

It has been described in the caption and then discussed in the text.

Page 43 caption. English is awkward, rewrite, "may indicate seasonal sea-ice cover" [delete "the"].

This figure has been removed.

Response to Referee#2

The objectives of the study are not well presented. The introduction is unclear and includes too many details that are not relevant for the study.

We have added some additional information to the introduction: "In this paper we present results from multi-proxy analyses of a sediment core retrieved 100 km east of the mouth of Storfjordrenna. We provide a new age for the retreat of the last Svalbard-Barents Sea Ice Sheet from Storfjordrenna and discuss the interaction of oceanography and deglaciation, as well as the postglacial history of Atlantic Water inflow onto the shelf off southern Svalbard. Since the studied sediment core was retrieved from an oceanographic frontal zone, sensitive to larger-scale changes, we believe that the presented data reflects the general climatic/oceanographic trends in the eastern Arctic."

The language is fluent, nonetheless it is clear that neither of the authors are native English speakers and expressions as "evanescent delivery of IRD" or the use of "hydrology" where "oceanography" is more appropriate needs to be corrected.

It has been corrected.

The last figure (Fig. 9) also appear not to be relevant and should be removed.

Figure 9 has been removed.

Abstract ". . .the beginning of late Holocene. . ." – when? Please indicate ca. calendar years BP.

The date 3600 yr BP has been added.

Introduction Page 3055, line 10: Which fjords? Be more specific.

It has been corrected, "Svalbard fjords" has been used instead "fjords".

Page 3055, lines 18 – 29: Too detailed – include only main point.

We respectfully disagree. As the colonization of the eastern coast of Storfjorden by Mytilus edulis was one of the drivers behind the presented study, it remains an important indicator of Atlantic water inflow to Storfjorden during the middle Holocene. The presented study explains the sea environmental condition of Storfjorden which allowed to develop species, characteristic for temperate fjords.

Page 3056, lines 1 – 11: The development of the knowledge and understanding of water masses in Storfjord seems to be beyond the scope of the current paper. I suggest removing all. Some details may be relevant in chapter 2 on study area.

We do not agree. In our opinion the knowledge and understanding of modern oceanography of Storfjorden is the basis for understanding the paleoceanography of eastern Svalbard.

Page 3056, lines 14 – 16. What time? Be more specific and add more on this. The information is very relevant for the scope of the paper. Refer to Rasmussen et al. (2007) and the recent paper by Rasmussen et al. (2014) (Quaternary Science Reviews 92, 280-291. Online November 2013)

We have added additional information to this paragraph and the relevant reference: Storfjordrenna is a sensitive area (Fig. 1) where two contrasting water masses form an oceanic polar front, separating colder, less saline and isotopically lighter ArW from warmer, high saline and $\delta^{18}O$ heavier AW. An abrupt cooling (e.g. Younger Dryas, Little Ice Age) and warming (e.g. early Holocene warming) of the European Arctic might be linked to relatively

small displacements of this front (Sarnthein et al., 2003; Hald et al., 2004; Rasmussen et al., 2014).

Page 3056, lines 17 – 19. Introduction lacks a coherent part on previous paleo-records from the region and Storfjordrenna. These sentences should be combined with the previous making a proper introduction to previous paleo-records from the region and Storfjordrenna.

The additional references have been added: Two sediment cores taken at the mouth of Storfjordrenna, reveal a continuous inflow of AW to the south western Svalbard shelf since the deglaciation of Svalbard-Barents Ice Sheet (Rasmussen et al,. 2007), while inner Storfjorden basins undergo a shift from being occupied by continental ice to ice proximal condition (Rasmussen and Thomsen, in press). Nevertheless a limited amount of paleoceanographical data is available from this region, thus the reconstruction of Svalbard-Barents Ice Sheet retreat and further development of Storfjordrenna oceanography is often speculative.

Introduction to the deglaciation of the Barents Sea Ice Sheet is also lacking.

The main focus of our manuscript is the paleoceanography of Storfjordrenna after our coring site was deglaciated. However, we discuss the timing of deglaciation of Storfjorden in the section 5.1 Sub-glacial unit (>13,450 cal yr BP).

Page 3056, lines 20 – 24. Unclear where the authors wish to take this?

We agree that this sentence was unclear and we have removed it from the Introduction.

Page 3056, lines 25 – 29. Clear presentation of study, but the introduction should contain information leading up to this and make objectives more clear.

We agree, additional sentences have been added to the introduction.

Page 3057, lines 10 – 13. This belongs to "Introduction", where more details on retreat are necessary making it clear why a new age on the retreat is relevant.

This sentence has been moved to the discussion. Please, see our response to Page 3056, lines 17-19.

Page 3057, lines 16. “(Table 1 after Skogseth et al., 2005)” should be corrected to “(Skogseth et al., 2005; Table 1)”.

It has been corrected.

Page 3058, lines 9 – 11. Reduce the numbers of references.

The number of references has been reduced.

Page 3058, lines 24 – 25. Simply refer to figure 2 without the details.

It has been corrected.

Page 3059, line 29. This reference does not deal with stable isotope measurements of *E. excavatum* f. *clavata* nor stable isotope measurements as a method. Please find relevant references on this and/or discuss accordingly.

The reference has been removed.

Page 3060, lines 9 – 11. "Bivalve. . ." this belongs - with thanks - in Acknowledgement

It has been corrected.

Page 3060, line 16. Why not Marine13? This paper was submitted July 15 2014.

It has been corrected.

Page 3062, lines 2 – 4 a. This is the first place where the boundaries of the lithological units are presented, use and list both core depth in cm and ages in cal yr BP.

It has been corrected.

Page 3062, lines 2 – 4 b. In figure 4 of the lithology the boundaries between the units are sharp. Hence, the use of ""/"ca." is not quite right, and all "" should be deleted throughout this chapter.

It has been corrected.

Page 3062, line 9. Delete "Munsell code". Page 3062, lines 13 – 14. >63 um AND between 7-10 um? This must be a mistake; please correct.

It has been corrected.

Page 3062, lines 24 – 25. Is there a color change bewteen L3 and L4? If yes, please describe this.

There is no color change between these two units. This information has been added to the text.

Page 3062, lines 26 – 27. Again >63 AND up to 15?

It has been corrected.

Page 3063, line 6. Make it clear this is the relative abundance.

It has been corrected.

Page 3063, lines 7 – 8. ". . .with 25% of agglutinated foraminiferal fauna. . ." – what do the authors mean here? Have the agglutinated foraminifera been included in the calculations of relative abundance or does the calcareous species constitute 100% in the calculations?

Yes, agglutinated foraminifera were included in the calculations.

Page 3063, lines 8 – 9. What is low? Please state the value of this sample.

The number of specimens (13 specimens g^{-1} of sediment) has been added to text.

Page 3064, lines 1 – 28. Throughout the description, mention numbers in addition to "high" and "lower".

It has been corrected.

Page 3064, line 1. Which species is the most frequent?

It has been corrected: Zone F1 is dominated by the opportunistic E. excavatum f. clavata and C. reniforme. The latter one dominates over E.excavatum f.clavata between 12,450 cal yr BP and 11,950 cal yr BP.

Page 3064, line 5. Low biodiversity? Please state the value of this sample and what it is compared to.

It has been corrected: Species richness and Shannon-Wiener index show low biodiversity (mean values of 8 and 1.26, respectively).

Page 3064, lines 22 – 25. Which species are the most frequent?

It has been corrected: The percentage of E. excavatum f. clavata increases slightly while C. reniforme decreases. The fluxes of Islandiella spp. and Buccella spp. increase significantly,

and from 2850 cal yr BP Islandiella spp. and E.excavatum f.clavata dominate the foraminiferal assemblage.

Page 3065, lines 18 – 19. Please make it clear that this is a minimum estimate.

It has been corrected.

Page 3065, lines 19-25. It is emphasized in the introduction that a new age on the deglaciation of BSIS is presented. This new date should be discussed in light of the latest results from the region, please refer to e.g. Andreassen et al (2014) and Rüther et al (2012). REFS: Andreassen, K., Winsborrow, M.C.M., Bjamadottir, L.R., Ruther, D.C., 2014. Ice stream retreat dynamics inferred from an assemblage of landforms in the northern Barents Sea. Quaternary Science Reviews 92, 246-257 (online November 2013). Rüther, D.C., Bjarnadóttir, L.R., Junttila, J., Husum, K., Rasmussen, T.L., Lucchi, R.G., Andreassen, K., 2012. Pattern and timing of the northwestern Barents Sea Ice Sheet deglaciation and indications of episodic Holocene deposition. Boreas 41, 494-512

Appropriate sentence and references have been added to discussion: "Our data stays in agreement with ice stream retreat dynamics presented by Rüther et al. (2012) and refines the recent models of the Barents Sea deglaciation (e.g. Winsborrow et al., 2010; Hormes et al., 2013; Andreassen et al., 2014)."

Page 3066, lines 7 – 9. A reference on this?

It has been corrected. This assumption has been based on the literature after Lucchi et al., 2013 and Witus et al., 2014.

Page 3066, line 11. Indicate the lithological units on the figure with foraminiferal data. It is not possible to assess this statement/correlation easily.

The lithological units have been added to the Figure 5 and Figure 6.

Page 3066, line 12. *A. gallowayi* is not epibenthic, but infaunal? In the Barents Sea, it is often found in areas with coarse sediments and stronger bottom currents together with *C. lobatulus*. NB I cannot find any mentioning on *A. gallowayi* in the cited reference: Hald & Steinsund 1996.

We agree, this sentence has been elaborated.

Page 3066, lines 17-18. This needs to be elaborated.

This sentence has been elaborated. Please, see the response to Page 3066, line 12.

Page 3066, lines 22 – 24. A reference on propagules is needed.

The description of the transport of propagules by sea currents and their subsequent settlement on the seafloor is congruent with a study by Alve and Goldstein (2003), and the reference has been added to the text.

Page 3066, lines 27. What kind of ice? Please discuss and make it clearer.

The term "glacier proximal" has been used instead of "ice proximal".

Page 3067, line 1. No, *Elphidium excavatum* f. *clavata* may also be the most frequent species in other environments than glacier proximal environments, e.g. Saher et al, 2009. In order to make this interpretation it is necessary to refer to studies of the position and distribution of the BSIS at this point.

Saher et al. 2009 showed the distribution (abundance) of E. excavatum f. clavata in the central Barents Sea. Therefore, their conclusions regard relation between abundance of foraminifera and inflowing Atlantic water/polar front position. However, clear correlation between distance from the glacier fronts and opportunistic species (E. excavatum f. clavata and C. reniforme) occurrence in the fjords was described in numerous papers (Steinsund, 1994; Hald and Korsun 1997, 1998; Majewski and Zajączkowski, 2007; Włodarska-Kowalczuk et al., 2013).The species assemblage is found in numerous deglacial records from Svalbard shelf areas (e.g. Ślubowska et al., 2005, 2007; Skirbekk et al., 2010; Rüther et al., 2012)

Page 3067, lines 5 – 6. Which areas are these records coming from? There are additional records that should be included in the comparison.

The areas where the records are coming from have been added to the text.

Page 3067, lines 13 – 16. It must be emphasized in the text what the temporal resolution is, and that there are not that many data points in this interval...

The points showing the resolution of our data (40-100 years) has been added to Figure 7.

Page 3067, lines 16 – 17. A more specific describing is needed.

This sentence has been elaborated.

Page 3067, lines 18 – 19. It is necessary to refer to a study of this mechanism.

Proper reference has been added to the text.

Page 3067, lines 20 – 27. This need to be presented and discussed in much more detail, also refer to Bakke et al 2009 (Nature Geoscience, DOI: 10.1038/NGEO439)

This part of discussion has been complemented.

Page 3068, lines 1 – 3. It is not clear what the authors mean with this? Heavier $\delta^{18}O$ values = warmer water? Explain in more detail and be specific.

Additional explanation has been added to this part of discussion

Page 3068, lines 8 – 9. It is necessary to include other type of data/references on the position of BSIS. It cannot be included solely on the foraminiferal content. This interpretation has to be supported by other findings.

We have rearranged this section, now the lithological parameters follow the interpretation based on foraminiferal content.

Page 3068, lines 14 – 19 a. The values of the current study are lighter from xxxxx cal yr BP and not just from 11500 cal yr BP. It must be addressed. Figure 8: Plot all $\delta^{18}O$ data from the current study, i.e. begin y-axis at 14.500 cal yr as figures 5-6. Discuss the lighter values in more details in the paper.

It has been improved.

Page 3068, lines 14 – 19 b. Are these values robust? It should be discussed if there are any dissolution effects on the isotope record.

As Storfjorden is known for its brine-enriched water production, we put focused attention on picking the foraminifera for stable isotopes analyses. Chosen tests had no dissolution signs, thus we assume that the isotopes records are robust.

Page 3068, lines 14 – 19 c. Any references confirming Arctic Water are isotopically lighter than AW?

The reference Duplessy et al., 2005 has been added.

Page 3069, line 3. Correct the spelling of "mollusk".

It has been corrected.

Page 3069, lines 5 – 7. This ratio has not been described or addressed before? That is necessary or this should be left out altogether.

This ratio has been described widely in the revised version of this manuscript.

Page 3069, line 22. Erbs-Hansen et al (2013) is about a paleo-record. The current ecological interpretations must be based on modern analogues/ studies of recent fauna.

The reference has been changed.

Page 3069, lines 19 – 20. Khusid and Polayk (1988) is a study from Arctic Ocean? Argue why it is relevant for this study area. It is recommended to use Saher et al 2009 which is a study of modern foraminifera in the current study area. REF: Saher, M., Kristensen, D.K., Hald, M., Korsun, S., Jorgensen, L.L., 2009. Benthic foraminifera assemblages in the Central Barents Sea: an evaluation of the effect of combining live and total fauna studies in tracking environmental change. Norwegian Journal of Geology 89, 149-161.

We have added reference to two works: Sejrup et al., 2004 and Saher et al., 2009 as the first one describes in more detail the environmental preferences of E.excavatum f.clavata. Moreover, we have rewritten the sentence and skip the reference to Khusid and Polyak (1989) as their study site is located too far away from our coring site.

Page 3069, line 29 - page 3070, line 3. It is not quite right to assume. *M. barleanus* cover a wide temperature range. Please refer to for example Hald & Steinsund 1996 (Berichte zur Polarforschung 212, ISSN 01765027) which the authors have used previously in this paper.

We do not agree. Hald and Steinsund (1996) point only that this species follow the troughs of W Barents Sea with soft and organic- rich bottom sediments. These troughs are the main gateway for Atlantic water influx to the Barents Sea.

Page 3070, lines 4 – 5. Explain how/why?

The reference to the paper describing this relationship in the Arctic fjord has been added.

Page 3070, lines 5 – 8. This need to be seen in the light of the unusual light values prior to this time (cf figure 8). What happens here? Could these values influenced by dissolution?

As Storfjorden is fairly known for its brine-enriched water production, we put a lot of attention during analyzing the foraminiferal samples to look for the dissolution effects on foraminiferal tests.

Page 3070, lines 9 – 11. This needs to be discussed in much more detail before this may be suggested. Other paleoceanographic records from the Barents Sea and Svalbard region must be taken into account as well, e.g. Risebrobakken et al, 2010. The Holocene, 20, 609-621.

Additional references regarding the AW inflow to W and NE Svalbard including Kveithola Trough has been included. However, we do not agree that data presented by Risebrobakken et al. (2010) supports our discussion as they focus on the southern Barents Sea influenced by the continental discharge. Moreover, an extra branch of AW adds warm and saline water to WSC in Bjørnøya area (Walczowski and Piechura 2006, 2007).

Page 3070, lines 18 – 21. Before comparing to terrestrial records compare to paleoceanographic records from the region including new work by Groot et al 2014; Berben et al, 2014, then compare to other records as Forwick et al (2010). REFS: Berben, S.M.P., Husum, K., Cabedo-Sanz, P., Belt, S.T., 2014. Holocene subcentennial evolution of Atlantic water inflow and sea ice distribution in the western Barents Sea. Clim. Past 10, 181-198. Groot, D.E., Aagaard-Sørensen, S., Husum, K., 2014. Reconstruction of Atlantic water variability during the Holocene in the western Barents Sea. Clim. Past 10, 51-62.

It has been corrected.

Page 3070, lines 21 – 25. Please summarize and present this with less detail.

It has been corrected.

Page 3070, lines 25 – 29 a. Explain why *M. barleanus* indicate an increased influence of AW + cite a references on its modern ecological preferences.

The additional information has been added.

Page 3070, lines 25 – 29 b. Explain/discuss the apparent contradicting signals of more AW and more IRD

In our opinion in a small extent these two signals are not contradictory since snow accumulation on land and slight glaciers advance depend on humid air transport from the

ocean. Thus slight change in atmospheric frontal zone could cause fluctuation of the glaciers range.

Page 3071, lines 14 – 15. This must be explained and argued in more detail. Please refer to later comments on figure 5.

This sentence has been rewritten.

Page 3071, lines 16 – 18. This is difficult to understand? On figure 6 it very much looks like the values become slightly heavier (higher values) for the period 3600-1200 cal yr BP indicating minor cooling and/or more salty water masses?

Indeed, the values become slightly heavier for the period 3600-1200 cal yr BP. It has been corrected in the revised version of the manuscript.

Page 3072, lines 1 – 3. Shortly state what happens with the flux shown in figure 4 and refer to figure 4 alone instead of referring not quite rightly to figure.

This sentence has been rewritten.

Page 3072, lines 3 – 4. References on the modern distribution and ecological controls of these species must be stated. It is necessary to discuss the interpretations stepwise. *N. labradorica* and *Islandiella* spp. are abundant in areas with a high biological productivity in the upper surface waters. High biological productivity occur near oceanic fronts and/or near ice margins, hence the current foraminiferal fauna indicate that the core site is near an oceanic front or near an ice margin. In the Barents Sea *Islandiella* spp has been found near marginal ice-zones, hence here it is interpreted that the core site is near a marginal ice-zone.

It has been corrected.

In figure 9 Islandiella spp. Is plotted together with IRD, this is not discussed further in the paper as it should. It appears that they only correlate in Late Holocene, hence *Islandiella* spp linked to sea ice using this correlation.

This paragraph has been removed.

Page 3072, lines 6 – 7. A reference on this?

The reference has been added.

Page 3072, lines 13 – 17.The correct reference is Rasmussen et al. (2014). Rasmussen et al (2014) find that "During the last 2000 years conditions have been increasingly unstable although with slightly increasing subsurface temperatures" which is also shown by recent studies by Groot et al (2014) and Berben et al (2014), both Climate of the Past, vol 10. The latter reference also include reconstructions of sea ice using sea ice biomarkers.

It has been corrected.

Figure 5 a: Indicate the unit of the fluxes; I assume it is specimens/g dry sediment/year?

It has been corrected. The description is in the caption - no. cm^{-2} ka^{-1}.

Figure 5 b: Some of the ecological tolerances is over simplified and/or not quite right and must be corrected: *C. reniforme* is not opportunistic.

We do not agree with this suggestion, there might be several levels of opportunism. C.reniforme is broadly and commonly described in the literature as an opportunistic species together with E.excavatum f.clavata (e.g. Ivanova, 2009; Korsun et al., 1995). C.reniforme is the second most important species in glacimarine environments (Osterman, 1982; Vilks et al., 1989; Hald et al., 1992; Jennings et al., 2000).

The term “high sea productivity”? – be more specific *N. labradorica* and *M. barleanus* are correctly connected to high biological productivity. This can occur in several settings. This also apply to *Islandiella* spp and *Bucella* spp, so it is not possible to divide them into these two groups of “ocean front” and “ice margin”. They should all be labelled as “high biological productivity”. The interpretation of ocean front vs ice margin must be done using other data and proxies form current study or existing knowledge.

We agree that N.labradorica, M.barleeanum, Islandiella spp. and Buccella spp. are indicators of high biological productivity. However, there is a wide range of available literature dividing those species into two groups: sea ice cover/sea ice margin species (Polyak and Solheim, 1994; Steinsund et al., 1994; Hald and Steinsund, 1996) and frontal zone indicators (Hald and Korsun, 1997; Korsun and Hald, 1998; 2000; Rytter et al., 2002; Jennings et al., 2004; Lloyd 2006; Knudsen et al., 2012; Jernas 2012).

We have updated the revised version of this manuscript of existing knowledge of species ecological preferences.

Figure 6: Data are displayed poorly mixing parameters that does not belong together. Move species richness, diversity and planktonic flux to figure 5. Keep these parameters together with the relevant benthic foraminiferal data and plot stable isotopes separately.

Such change will significantly enlarge Figure 5 and decrease Figure 6. We recommend to ask the editor if it is possible.

NB add the flux of *T. quinqueloba* to the planktonic data. The number may be low, but it may elucidate the influence of TAW.

The flux of T. quinqueloba has been added to Figure 6 and widely described in the text.

Figure 7: show data points of the IRD flux and d18O making the temporal resolution of the current study clear.

The data points have been added to the Figure 7.

Consider to make a plot of $\delta^{13}C$ vs $\delta^{18}O$ values from the current study evaluating the water masses (cf. Rasmussen, T.L., Thomsen, E., 2009. Stable isotope signals from brines in the Barents Sea: Implications for brine formation during the last glaciation. Geology 37, 903-906).

The scatter plot has been added to the Figure 8.

Figure 9: This comparison of *Islandiella* spp. and IRD are not properly discussed in the paper. As mentioned previously it appears that *Islandiella* spp cannot be linked to sea ice using this correlation.

The figure has been removed.

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