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Interactive comment on "Stratification of surface waters during the last glacial millennial climatic events: a key factor in subsurface and deep water mass dynamics" *by* M. Wary et al.

M. Wary et al.

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Reply to the Interactive comment on "Stratification of surface waters during the last glacial millennial climatic events: a key factor in subsurface and deep water mass dynamics" by M. Wary et al., by Anonymous Referee #1, Received and published: 25 June 2015

REV#1: Wary and co-authors present a study combining various proxy records to understand hydrographic changes during the millennial-scale climate oscillations of the last glacial period in the northern North Atlantic. The paper shows interesting new data and concepts that are interesting for the paleoclimate community and merit publication.

However, the paper needs major revisions before it could be accepted for publication in Climate of the Past. (. . .)

We are grateful to reviewer 1 for his / her careful and constructive review of our paper which will contribute to consequently improve the manuscript. Below are our reply regarding the most straightforward and unequivocal comments. For the ones which need deeper investment we will for sure take them into account and integrate them in the revised manuscript but will first wait for REV#2's additional comments before any large modification.

(. . .) First of all, a manuscript should focus on the new data produced and added to the already published data. Thus –as far as I can tell– the methods and results sections should purely focus on the planktonic foraminifer derived records and on the grain size data. The dinocyst reconstructions have been published previously and should therefore only be integrated into the discussion. Deleting the dinocyst-relevant text from the methods and results section will also shorten the manuscript. (. . .)

Reply: We totally agree and will delete parts related to dinocysts from the methods and results sections, even if it could probably be a lack for some readers who do not know the Zumaque et al. and Caulle et al. papers.

(. . .) One of the major problems of the paper is that it is treating the planktonic foraminifer reconstruction data as indicator for subsurface water conditions. Currently, I, as a reader, can support this assumption only for the δ18O and δ18Ow data because those two values are truly related to the living/ calcification depth of N. pachyderma (s), although the authors never mention which depth range they assume the N. pachyderma (s)-derived data reflect [the authors could include the following reference to strengthen their interpretation on a regional level: Peck et al. 2008. Millennial-scale surface and subsurface paleothermometry from the North East Atlantic, 55-8 kyr BP. Paleoceanography 23, PA3221, doi:10.1029/2008PA001631]. (. . .)

Reply: We will add the suggested reference in the text a well as the following ones to strengthen the discussion.

Jonkers, L., Brummer, G. J. A., Peeters, F. J. C., Van Aken, H. M., and De Jong, M. F.: Seasonal stratification, shell flux, and oxygen isotope dynamics of leftcoiling N. pachyderma and T. quinqueloba in the western subpolar North Atlantic, Paleoceanography, 25, 2010.

Simstich, J., M. Sarnthein, and H. Erlenkeuser (2003), Paired d18O signals of Neogloboquadrina pachyderma (s) and Turborotalita quinqueloba show thermal stratification structure in Nordic Seas, Mar. Micropaleontol., 48, 107–125, doi:10.1016/S0377-8398(02)00165-2.

(. . .) For the temperature reconstruction the authors are referring to a publication by Eynaud et al. (2013) that for most readers (including me) is impossible to access. (. . .)

Reply: We are really sorry for this situation, but as many of Book chapters, this article is not freely accessible. However some academic links (University) give access to Nova Science Publishers. Nonetheless, this is a very technical paper and the method is also described in several publications: see for instance supplementary information of the Sanchez-Goni et al., 2012 Geology paper (http://geology.gsapubs.org/content/40/7/627.abstract) and/or the Method section of the Sanchez-Goni et al., 2014 paper in Nature Geoscience (http://www.nature.com/ngeo/journal/v6/n10/full/ngeo1924.html). For more details, see also Matzusaki et al. (2011, doi:10.1016/j.marmicro.2011.01.004) and Penaud et al. (2011, www.biogeosciences.net/8/2295/2011/). All these references will be added in the text.

(. . .) Because the manuscript does not mention from which water depth the modern analog data for the Eynaud transfer function is, I can currently not support the statement of the authors that the planktic foraminifer derived temperatures are subsurface temperatures. Most modern analog files for planktic foraminifer reconstructions are for a water depth of 10 m – the same depth as used for the dinocysts transfer function.

Even if the planktonic foraminifers themselves might live over a wider depth range, by using the transfer function you relate all the information to the water depth provided by your modern analog data, i.e. 10 m???. In case the Eynaud et al. (2013) modern analog database is for a water depth deeper than 10 m not only the water depth chosen needs to be clearly stated but there should also be some additional information provided on why that exact water depth was chosen and how reliable reconstructions for the chosen water depth are in the region of the study area. See for example: Telford, R.J., Li, C., Kucera, M., 2013. Mismatch between the depth habitat of planktonic foraminifera and the calibration depth of SST transfer functions may bias reconstructions. Clim. Past 9, 859-870. (. . .)

Reply: As stated in all the cited papers before, our approach strictly conforms to the MARGO procedure regarding the hydrographical parameters of the modern dataset. The database merges the ones developed separately for the North Atlantic Ocean and the Mediterranean Sea during the MARGO project (Kucera et al., 2005; Hayes et al., 2005), thus compiling 1007 surface sediment samples. Modern sea-surface parameters were extracted with the sample tool (see http://www.geo.uni-bremen.de/geomod/Sonst/Staff/csn/woasample.html) developed by Schäfer-Neth & Manschke (2002) in the frame of the MARGO program. This tool interpolates the 10 m World Ocean Atlas WOA -1998 mean seasonal and mean annual temperatures over the four existing data point surrounding the sample location thus providing spatio-temporal averaged values of SST. The extracted information were obtained as annual and seasonal means (i.e. averaged winter - January/February/March months; spring - April/May/June months, summer - July/August/September months and fall - October/November/December months).

For transfer functions sensu lato, the reference living-depth of foraminifera to consider in modern SST training sets is really problematic and its definition not trivial. SSTs are actually classically extracted at 10 m and numerous tests were previously done on this question (see the list below). Pflaumann et al. 1996 (Paleoceanography 11) have demonstrated that such a consideration does not provide significant differences but conversely underlines the complexity in planktonic foraminifera (PF) assemblages in relation to seasonality which control their living depth: "The best results both with the highest correlation coefficients and relatively low standard deviations were achieved using the mean of 0- to 75-m water depth for spring temperatures, using the mean of 0- to 50-m water depths for summer and fall temperatures, and using the mean of 0- to 30-m water depth for caloric winter temperatures. These trends in the correlation coefficients and in the standard deviations may indicate that during winter season the temperature relevant species cover a smaller depth range ocean wide than during the other seasons. This can be due either to a weaker temperature sensitivity of the species living in deeper habitats and/or to a stronger one of the shallow water communities during caloric oceanographic winter. However during winter the mixed layer is relatively thick [Ravelo et al., 1990], while the production is strongly reduced [Bé et al., 1985; Thunell et al., 1983; Reynolds and Thunell, 1985]."

Since transfer functions have been developed, authors have several times mentioned the fact that PF are not representative of the very surface: see the list below + the very nice synthetic introduction (section 2.1.) published within the MARGO QSR special issue by Kucera et al. 2005b). Identical questionings were recently raised by Telford at al. (2013). This paper was not cited in our previous version but we were aware of their strong recommendations which support our choice. For instance, as stated by these authors, for sites in the North Atlantic drift (including the nearby site NA87-22; cf. page 862) paleoreconstructions at all depth are statistically significant, and "the most ecologically relevant depth varies in space and time, and the assemblages will probably integrate the communities from several depths and seasons, so selecting a more appropriate fixed depth for temperature reconstructions for each location is probably not trivial and does not completely circumvent the problem." This latter statement is also supported by Adloff et al. (2011): "The currently used technique to reconstruct temperature from planktonic foraminifera [by extracting SST averaged over a depth interval depending on the living depth of the foraminifera species identified] is

likely inadequate for time periods when the vertical temperature gradient was different from today". Hence, it appears that there is no valuable reason to define a depth or a depth section as more appropriate than another one, particularly because it must have evolved through time. Moreover, as also stated by Telford et al; (2013), "For cores north of 25°N, the [paleo]reconstructions from different depths and seasons resemble one another, with an offset", implying that even if we have chosen another extraction depth for SST, the relative variations of our signals would be very similar.

This is precisely the same argument that we develop, furthermore supported by the observed discrepancies between the two tested communities (phyto. strictly confined to the eutrophic zone vs zoo.). Indeed, the reconstructed hydrographic values integrate the diversity and the structure of the whole assemblages as well as their specific related ecology, which are fairly different even in the same community (what if we consider cryptic species. . .): as the bases of transfer function primarily rely on the assemblages and on the similarity that modern and past samples share, calculations are first representative of a specific community, whatever the depth it integrates! As we determined very distinct signatures quantitatively but also qualitatively (i.e. communities which ecologically do not sound as belonging to a common environment/ water masse), the only way to reconcile the proxies was to argue for signals (qualitative as well as quantitative signals) representative of different sections of the water column: dinocyst signals representative of the very surface (0-50 m, their depth habitat), and PF representative of an integrated signal over the 0-300 meters water-depth section (the section where species present in our assemblages live and migrate in). The subsurface argument is thus developed in order to reconcile the data and to provide a constructive view of what can be interpreted from the strong difference calculated. Moreover, the PF MAT derived SST (F-Temp. in the manuscript) values obtained here have never been discussed in our paper as subsurface absolute estimations, but in terms of relative variations.

References based on transfer functions derived from foraminifera where flexible interpretations of the SST have been evocated:

Adloff, F., Mikolajewicz, U., Kučera, M., Grimm, R., Maier-Reimer, E., Schmiedl, G., and Emeis, K. C.: Upper ocean climate of the Eastern Mediterranean Sea during the Holocene Insolation Maximum - A model study, Climate of the Past, 7, 1103-1122, 2011.

Barrows, T.T., Juggins, S., De Deckker, P., Thiede, J., Martinez, J.I., 2000. Sea-surface temperatures of the southwest Pacific Ocean during the Last Glacial Maximum. Paleoceanography 15, 95–109.

Gersonde, R., Abelmann, A., Brathauer, U., Becquey, S., Bianchi, C., Cortese, G., Grobe, H., Kuhn, G., Niebler, H.-S., Segl, M., Sieger, R., Zielinski, U., Futterer, D. K., 2003. Last glacial sea surface temperatures and sea-ice extent in the Southern Ocean (Atlantic- Indian sector): a multiproxy approach. Paleoceanography 18, doi:10.1029/2002PA000809.

Kucera, M., Rosell-Melé, A., Schneider, R., Waelbroeck, C., Weinelt, M., 2005a. Multiproxy approach for the reconstruction of the glacial ocean surface (MARGO). Quaternary Science Reviews 24, 813–819.

Kucera, M., Weinelt, M., Kiefer, T., Pflaumann, U., Hayes, A., Chen, M.T., Mix, A.C., Barrows, T.T., Cortijo, E., Duprat, J., Juggins, S., Waelbroeck, C., 2005b. Reconstruction of sea-surface temperatures from assemblages of planktonic foraminifera: Multi-technique approach based on geographically constrained calibration data sets and its application to glacial Atlantic and Pacific Oceans. Quaternary Science Reviews 24, 951–998.

Malmgren, B.A., Kucera, M., Nyberg, J., Waelbroeck, C., 2001. Comparison of statisticaland artificial neural network techniques for estimating past sea-surface temperatures from planktonic foraminifer census data. Paleoceanography 16, 520–530.

McIntyre, A., Ruddiman, W.F., Karlin, K., Mix, A.C., 1989. Surface water response of the equatorialAtl antic Ocean to orbitalforcing.Paleoceanography 4, 19–55.

Mix, A.C., Morey, A., Pisias, N.G., 1999. Foraminiferal faunal estimates of paleotemperature: circumventing the no-analog problem yields cool ice age tropics. Paleoceanography 14, 350–359.

Niebler, H.S., Mulitza, S., Donner, B., Arz„ H., PaÂÍ tzold, J., Wefer, G., 2003. Sea-surface temperatures in the equatorialand South Atlantic Ocean during the Last Glacial Maximum (23–19 ka). Paleoceanography 18, doi:10.1029/2003PA000902.

Sarnthein, M., Gersonde, R., Niebler, S., Pflaumann, U., Spielhagen, R., Thiede, Wefer, G., Weinelt, M., 2003a. Preface: Glacial Atlantic Ocean Mapping (GLAMAP-2000). Paleoceanography 18, doi:10/1029/2002PA000770.

Sarnthein, M., Pflaumann, U., Vogelsang, E., Weinelt, M., 2003b. Past extent of sea-ice in the northern North Atlantic inferred from foraminiferal paleotemperature estimates. Paleoceanography 18, doi:10/1029/2002PA000771.

Pflaumann, U., Duprat, J., Pujol, C., Labeyrie, L., 1996. SIMMAX: A modern analog technique to deduce Atlantic sea surface temperatures from planktonic foraminifera in deep-sea sediments. Paleoceanography 11, 15–35.

Pflaumann, U., Sarnthein, M., Chapman, M., Duprat, J., Huels, M., Kiefer, T., Maslin, M., Schulz, H., van Kreveld, S., Vogelsang, E., Weinelt, M., 2003. North Atlantic: sea-surface conditions reconstructed by GLAMAP-2000. Paleoceanography 18, doi:10/1029/2002PA000774.

Telford, R.J., Li, C., Kucera, M., 2013. Mismatch between the depth habitat of planktonic foraminifera and the calibration depth of SST transfer functions may bias reconstructions. Climate of the Past 9, 859–870. doi:10.5194/cp-9-859-2013

Trend-Staid, M., Prell,W.L., 2002. Sea surface temperature at the Last Glacial Maximum: a reconstruction using the modern analog technique. Paleoceanography 17, doi:10.1029/2000PA000506.

Waelbroeck, C., Labeyrie, L., Duplessy, J.-C., Guiot, J., Labracherie, M., Leclaire, H.,

Duprat, J., 1998. Improving past sea surface temperature estimates based on planktonic fossil faunas. Paleoceanography 13, 272–283.

Watkins, J.M., Mix, A.C., 1998. Testing the effects of tropical temperature, productivity, and mixed-layer depth on foraminiferal transfer function. Paleoceanography 13, 96–105.

(. . .) In the methods and results, the authors list biodiversity indices/data. What is importance of this data is for the current study? The data are mentioned nowhere in the discussion and thus treated by the authors themselves as not relevant. I therefore recommend deleting this data from the paper. (. . .)

Reply: Actually, diversity and dominance indices are quickly mentioned (maybe too quickly regarding their usefulness?) in the first section of the discussion (Sect. 5.1), to attest that fauna were not reworked and therefore that derived reconstructions reflect allochthonous signals.

(. . .) Issues related to "stratigraphy": 1) I recommend using GI as abbreviation for Greenland Interstadials because for the scientists from the ice-core community GIS stands for Greenland ice sheet. GI is also used within the INTIMATE community and the nomenclature of GI/GS (e.g., Rasmussen et al., 2014 in QSR "A stratigraphic framework for abrupt climatic changes during the Last Glacial period. . . ."). (. . .)

Reply: We used the original GIS abbreviation (North GRIP Members, 2004) but do agree with GIS being confusing, and will instead use GI.

(. . .) 2) The interval marked for Heinrich event 3 is too broad. Normally, only the last cold phase is linked to H 3 (e.g., Hall et al., 2011 for a nearby record). With the broad interval used by the authors they are including a Greenland Interstadial = GI 5.1 (e.g., Rasmussen et al., 2014) into H3 and it is therefore not astonishing that they see a three-phased pattern in their paleoclimatic records. (. . .)

Reply: The interval concerned does not mark Heinrich event 3 but Heinrich stadial 3,

as stated in the text and figure captions. Current HS3 age limits are defined following Wolff et al. (2010), i.e. end of GI5 to beginning of GI4. The three-phased pattern thus concerns the whole HS3 and not HE3, and is surely related to GI 5.1. Furthermore, in the North Atlantic, the first cold phase is also associated to HS3 (cf. Sanchez-Goñi and Harrison, 2010). We can therefore modify our terminology, by dividing our current HS3 in three parts: HS3a, GI 5.1, HS3b, and relabeling GI 5.2 our current GI 5.

(. . .) 3) H 2 timing: the general view is that Heinrich events precede a GI (e.g., Bond et al., 1993; van Kreveld et al., 2000; Hall et al., 2011). So H 2 should directly precede GI 2 and not fall into the middle of GS 3 as marked by the authors in their figures and previously by Caulle et al. (2013). Grousset et al. (2000) showed that deep-sea cores from the European margin recorded an earlier event with European-sourced IRD (and thus in the strictest sense is not a Heinrich event) that sometimes is referred to as H 2.2. So from the timing in relation to the NGRIP record the authors seem to have marked and are discussing this older H 2.2. event and not H 2 per se. However, looking at the % G. bulloides record shown in Fig. 2 I am not sure if there is not a problem in the core's age model. Normally, I would contribute the % G. bulloides peak following the marked H 2 interval to GI 2. Thus in the paleoclimate records the H 2 level might be correct; it is just too old in relation to the NGRIP chronology. To clarify this issue the authors might try to align their % N. pachyderma (s) record with those shown Austin, W.E.N., Hibbert, F.D., Rasmussen, S.O., Peters, C., Abbott, P.M., Bryant, C.L., 2012, The synchronization of palaeoclimatic events in the North Atlantic region during Greenland Stadial 3 (ca. 27.5 to 23.3 kyr b2k). Quaternary Science Reviews 36, 154-163. (. . .)

Reply: For consistency, we used the same age limits for HS2 as Caulle et al. 2013, which are by the way very close to HS2 definition given by Sanchez-Goñi and Harrison (2010; i.e. 26.5-24.3 ka cal BP, GICC05 chronology). Given that the age model of the upper section of the core is only based on 14C dates, we are aware that there might be such a kind of problem. However, Sanchez-Goñi and Harrison (2010)'s HS2 definition, based on GICC05 age scale and defined as the "maximum length of the interval of IRD deposition based on a composite of marine records from the North Atlantic" does not directly precede GI 2. We could precisely extend our grey band to these limits, but this will include the % G. bulloides peak, and as REV1, we do think that this peak should not be included within HS 2, even if this event is also known to be multiphased.

(. . .) Issues related to the modern (past) oceanography: 1) I recommend using the term of ISOW = Iceland Scotland Overflow Water instead of NSOW. ISOW is the term used by oceanographers and includes contributions by both the Norwegian Sea Deep Water and the intermediate/deep waters formed north of Iceland. (. . .)

Reply: ISOW will be used instead of NSOW.

(. . .) I would like to see a reference to a modern oceanography study included in the reference list on page 2082 line 5. (. . .)

Reply: Reference to Hansen and Osterhus, 2000 will be added.

(. . .) It would also be important to mention that only a minor part of the ISOW exiting through the Faeroe-Shetland Channel crosses the Wyville-Thompson ridge (see Hansen and Østerhus 2000) and therefore affects the core site. This aspect is highly important for the past records when convection in the Nordic Seas was reduced or shallower and thus the overflow potentially weaker. (. . .)

Reply: It is mentioned that a part of the ISOW crosses the Wyville-Thompson Ridge, and that the core site is currently affected by an intermittent flow of this southern ISOW branch, which indeed means that the modern ISOW flow affecting the site is minor. We can reformulate it to make it clearer.

(. . .) 2) Terminology: the authors use several time the phrase "Atlantic inflow" in relation to the NAD but this is not correct for the location of their core site. Inflow refers to the waters entering the Nordic/Norwegian Sea and thus to the Atlantic water current north of the Faeroer islands. (. . .)

Reply: We used the term "Atlantic inflow" for clarity, to integrate our interpretations in a more regional view. But we do agree that the use of the term "inflow" is somewhat abusive, and will use the term "flow" or "northward flow".

(. . .) 3) Convection: for the discussion of the past hydrographic conditions the authors are also mixing up regions or convection depths. Modern and likely GI deep convection took place in the Nordic Seas and thus way north of the studied site and not above/close to the site as implied by the text and the schemes in Fig. 6. Nowadays, the site is, however, located at the northern edge of the area where subpolar mode (central) water is formed/convected (see for example Brambilla, E., Talley, L.D., Robbins, P.E., 2008. Subpolar Mode Water in the northeastern Atlantic: 2. Origin and transformation. Journal of Geophysical Research 113, doi:10.1029/2006jc004063), but this an subsurface/intermediate depth water mass. On the other hand, the Rockall Plateau south of the core site is the area indicated where deep convection might have taken place during the last glacial maximum (e.g., Sarnthein et al., 1994, Changes in east Atlantic deepwater circulation over the last 30,000 years: Eight time slices reconstructions. Paleoceanography 9, 209-267. or Gherardi, J., Labeyrie, L., Nave, S., Francois, R., McManus, J.F., Cortijo, E., 2009. Glacial - interglacial circulation changes inferred from 231Pa/230Th sedimentary record in the North Atlantic region. Paleoceanography 24, doi:10.1029/2008PA001696.) So overall, the authors need to be more precise in indication how deep and where the convection took place they are mentioning in the discussion. (. . .)

Reply: We do agree that the convection site was certainly located way north of our study area during GI, and will replace in Fig. 6 the confusing term "Faeroes" by "Nordic Seas". However, we deeply think that we cannot be more precise about the depth of convection considering our set of proxies, nor about its location (except convection occurred north of our study site). Furthermore, we would like to highlight the fact that the aim of Fig. 6 (conceptual scheme) is purely to illustrate the sequential evolution of hydrological processes occurring throughout a typical DO cycle within the region around the study area (because these hydrological processes might have played a key role in DO events).

(. . .) 4) you cannot per se assume that the NAD was also present/flowing over/near your core site, in particular during Heinrich events and may be some of the GS. The NAD as a surface current might have been diverted to the south, i.e. towards the area(s) where deep convection took place, by the expanded subpolar gyre. Thus I would be very careful to use the term intensity in relation to a paleo-NAD. It might also be good to give evidence from cores along the NAD flow path or along the British margin (such as Hall or Peck papers) to support a NAD presence throughout the intervals discussed. (. . .)

Reply: Unfortunately, such evidences have been very rare up to now. We propose that the NAD might have been present close to our study area during some very occasional and atypical stadials such as HS2, because our data as well as previous studies suggest the possibility of such a pattern (Elliot et al., 2002; Voelker et al., 2006). Furthermore, recently, Dokken et al. (2013) evidenced the presence of a subsurface glacial Atlantic inflow during all GS at site MD99-2284 (southern Norwegian Sea, cf. Fig. 1); this inflow might be directly related to the warm and salty waters carried by the Continental Slope Current (flowing northward from the continental slope west of Great Britain to the Norwegian slope; Kenyon, 1986), but the latter is at present tightly linked to the NAD (Hansen and Osterhus, 2000), thus, and as discussed in page 2099, the glacial Atlantic inflow evidenced by Dokken et al. (2013) might represent a minor NAD inflow flattened against the shelf edge. Such a flow in core MD99-2284 indicates that the NAD was at least not totally diverted to the south during GS and that at least a part of it kept on flowing northward. Concerning the use of the term "intensity", we consider that a greater influence of the NAD over the study area might be related to a stronger intensity of the NAD, but it is possible that this relation might have not been constant throughout the last glacial period.

(. . .) The subsurface Atlantic inflow seen by Rasmussen and co-workers does not nec-

essarily have to be a subducted NAD but could also be in the form of a mode water –although we currently cannot distinguish this in the past. (. . .)

Reply: We totally agree with that and also suggest (page 2099) that the Atlantic inflow seen by Rasmussen et al. might represent a continuous stadial flow of Atlantic intermediate waters unrelated to the NAD.

(. . .) So I am currently not sure if the water mass signals (NAD vs/plus meltwater) as outlined in the top paragraph of page 2094 are fully correct or not –but this also goes back to the water depth reconstructed with the planktic foraminifer transfer function. Based on the modern oceanographic conditions around Greenland I would associate iceberg-calving with fresher surface waters and thus the existence of a halocline. (. . .)

Reply: The assumption taken for foraminifera transfer function is the most reasonable one (see above). Iceberg calving are for sure related to the existence of the near-surface halocline, but as soon as the halocline disappear the very-surface meltwater propagates within the subsurface layer, where the δ18O NPS capture the meltwater signal.

(. . .) Additional comments in order of page numbers: 1) in the abstract and following pages it should be "the Faeroers/ Faeroer Islands" (. . .)

Reply: We do not think this is a correct spelling. . . Actually, English dictionaries do not repertory any "Faeroers/ Faeroer Islands", but they do for "Faeroes/Faeroe Islands". . . Is "Faeroers/ Faeroer" not the German term?

(. . .) 2) page 2080 top: the base of the Holocene was defined as 11.65 ka BP or 11.7 b2k in Walker at al. (2009; Formal definition and dating of the GSSP (Global Stratotype Section and Point) for the base of the Holocene using the Greenland NGRIP ice core, and selected auxiliary records. Journal of Quaternary Science 24, 3-17.) so the top age for the glacial period should be adjusted to 11 ka BP at minimum. 3) page 2080, 2082 and 2091 the word "preconized" does not exist in English; at the bottom of page

2082 is should be “combined with” (. . .)

Reply: OK for both remarks.

(. . .) 4) page 2085 paragraph on δ18Ow calculation: the correction of -2.5 °C was introduced to adjust for the 10 m depth estimate for the planktic foraminifer transfer function based temperatures. So if the modern analog temperatures used by Eynaud et al. (2013) would be from a deeper water depth, this correction might no longer be valid. (. . .)

Reply: As mentioned above, modern analog temperatures are taken at 10 m depth.

(. . .) 5) page 2088: following the genotype analyses N. pachyderma (d) is now referred to as N. incompta. 6) page 2090: header should be changed to Deep-water proxies 7) page 2093 line 8: substitute “comforts” with “supports” (. . .)

Reply: Above remarks will be integrated in the revised manuscript.

(...) 8) page 2096 line 23: I assume you mean “shallower” instead of “deeper” AMOC (. . .)

Reply: Yes, for sure!

(. . .) 9) page 2098 a) line 13: NAD re-intensification or being present again? (. . .)

Reply: Extending northward again. We will clarify it.

(. . .) b) line 14: you don’t have a surface halocline; do you mean “near surface”? (. . .)

Reply: By surface halocline, we mean the halocline present within the surface layer, in-between the very-surface layer and the near-surface/subsurface layer. We can of course call it “near-surface” halocline for clarity purposes.

(. . .) c) line 15 ff: what depth of convection do you mean? Mode water or deep water??? You get density increases with colder temperatures and higher salinity; so even if your water might be “too warm” they could have a higher salinity that could allow convection.

Anyhow for the GI this whole process takes place in the Nordic Seas and you can therefore not use evidence –but there is data out there from the Nordic Seas that you can refer to. (. . .)

Reply: We talk about convection of deep water, as we have deep-water proxies and no proxies indicative of convection of intermediate waters.

(. . .) In line 19: heat exchange between what? Surface and subsurface? Surface and atmosphere (thus enabling atmospheric cooling)? (. . .)

Reply: Both, subsurface and surface, and surface and atmosphere. We will clarify it.

(. . .) For the GS/HS did you think about that some of the bottom current evidence might be related to an Atlantic-sourced water mass? Hansen and Østerhus mention on page 167 that in your study region a large Atlantic water component was observed. So if you would have deep convection over the Rockall Plateau, could that water not influence your site as well? In particular, if convection in the Nordic Seas was diminished and thus the overflow? (. . .)

Reply: This is a very interesting comment. We guess it is not impossible, but we think, at first sight, that it is very unlikely. A true deep current sufficiently efficient/with sufficiently high velocities is needed to induce such a reorganization of the sediment composition (in terms of magnetic particles inputs as well as in size sorting). We have never read any paper (paleoceanographic nor modern oceanography studies) indicating that such a deep current might be present upstream a convection center, but we will look further into this issue.

(. . .) 10) page 2100 line 13: add “water” after “fresh” (. . .)

Reply: OK

(. . .) 11) page 2102 line 4 ff: is there evidence for this from any of the cores along the British ice sheet margin? E.g. papers by Peck; Hall; Knutz; Austin etc. (. . .)

Reply: Scourse et al. (2009; cf. their Figure 8) recorded very high IRD fluxes during HS2 (compared to other HS and at least to H1 recorded in each of their studied cores) in cores located west and north off Great Britain.

(...) 12) figures 3 and 5: add arrows indicating direction of salinity change for δ18Ow data and increase in LLG. Mention in legends when data is shown on reversed scale (such as δ18O N. pachyderma (s)). (...)

Reply: OK

(...) There are additional minor language mistakes throughout the text that a spell/grammar check of the manuscript should pick up.

Reply: We will check again.

References cited in the reply (except for the ones previously detailed):

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