

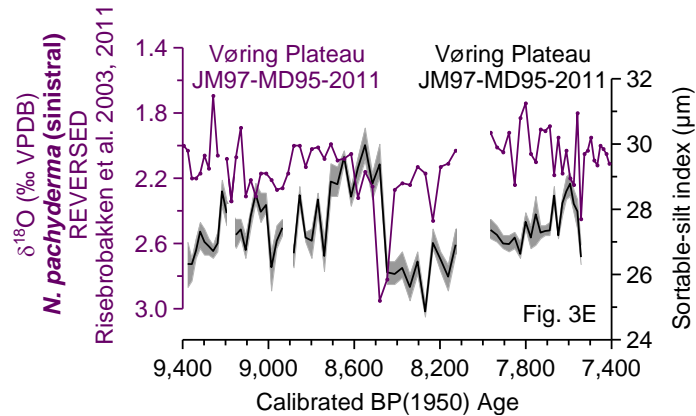
<p>Reviewer Comment</p>	<p>They need to convince us that the process of sedimentation is controlled by the current they purport to study because the basis for grain size measures of flow vigour is that sorting is produced by the flow being discussed that has a benthic boundary layer. See p. 668 line 15/16: Although the current has most probably not been in direct contact with the (high accumulation area) HA. Does this statement mean that the current (NwAC) does not extend to the bed at the core locations? If so it rather seriously undermines subsequent assertions. We need some oceanography here; some indication of the modern current structure and speed. That would belong in a section separate from 'Methods'. If the current the authors claim to be studying does not touch the bottom, to what flow do their results relate? A counter current may be possible because the deepest current meter of Orvik and Skagseth (2003) at 880m depth in 1001m of water (the authors' core is at 1048 m) has flow to 229° - 213°</p>
<p>Author Response</p>	<p>As indicated in the paper a detailed analysis of the applicability of the sortable-silt method in reconstructing past changes in the strength of the NwASC at this particular site can be found in:</p> <p style="padding-left: 40px;">Tegzes, A. D.: The Main Branch of the Atlantic Inflow into the Nordic Seas over the Holocene: An Analysis of Its Longer-Term Behaviour and Its Potential Impact on Climate, Doctoral dissertation, University of Bergen, Bergen, 67 pp., 2013.</p> <p style="padding-left: 40px;">and</p> <p style="padding-left: 40px;">Tegzes, A. D., Jansen, E., and Telford, R. J.: Reconstructing variations in the strength of the main branch of the Norwegian Atlantic Current over the Late Holocene using grain-size parameters, <i>Paleoceanography</i>, resubmitted, 2014a.</p> <p>The oceanography of the site is very complex. Therefore, an adequate treatment of the subject takes more than a section in a paper. In order to establish the applicability of the sortable-silt method at this particular location we systematically reviewed all relevant instrumental records, palaeoceanographic reconstructions and model results, regarding bathymetric control, water-mass structure, flow patterns, current strength and sedimentary processes both locally and with respect to the wider region. Tegzes et al. (2014a) give a detailed description of the HA and why they think that the coarseness of the 10-63µm fraction of the sediments there likely represents past changes in the strength of the NwASC.</p> <p>Please note that we are currently extending the above paper (Tegzes et al., 2014a) in scope as requested by the editors of <i>Paleoceanography</i>. This, however, DOES NOT affect the discussion of the applicability of the sortable-silt method, which is available to reviewers. Since Tegzes et al., 2014a has not yet been published our hands are tied as to how much of that discussion we can include in our paper on the 8.2ka Event. However, we may add a short paragraph to clarify the link between the NwASC and the HA.</p>
<p>Reviewer Comment</p>	<p>Here the authors do not state whether the data were in classes and if so whether an arithmetic or logarithmic progression was adopted.</p>
<p>Author Response</p>	<p>The target size range (10-63µm) was divided into 256 equal size bins. The size bins were defined in micrometers. This information will be inserted into the revised manuscript.</p>
<p>Reviewer Comment</p>	<p>Although a statistic based on particle number (n_i) data may be closer to the original measurement by this method than one on particle volume ($\propto n_i d_i^3$), such data are only available from particle counters of which there are very few types. Sedimentologists have always used weight/volume measures as these were classically obtained from sieve/pipette measurement (modern equivalent laser diffraction and Sedigraph). The fact remains that almost all sedimentological data is based on volume/weight statistics and use of number-based statistics because of supposed greater "truth" provides non-comparable data.</p>

<p>Author Response</p>	<p>Here we have to separate three issues:</p> <ol style="list-style-type: none"> 1. understanding real-life processes (the physics behind the numbers) and describing them to the best of our knowledge (choosing optimal statistical procedures and indices), 2. the technical feasibility of certain types of measurements/analyses, and 3. weighing traditional approaches versus new ones. <p>In our opinion we need to strive for improving our methods and instruments to get an optimal representation of the processes under scrutiny. If there are better ways, new approaches need to be put forward in the literature. We should not stick to a procedure just because an analysis has “always” been done in a certain way.</p> <p>Having said that the comparability of datasets is a real issue. This is part of the reason why we have included both the sortable-silt index time series and the true-mean-grain-size record in our paper. Thus while proposing an alternative method, we also show the data the conventional way to ensure that our results remain comparable with other datasets.</p> <p>In addition, we wanted to give a balanced, objective view of our results. Therefore, we take into consideration both datasets throughout the paper. As discussed in</p> <p style="padding-left: 40px;">Tegzes, A. D.: The Main Branch of the Atlantic Inflow into the Nordic Seas over the Holocene: An Analysis of Its Longer-Term Behaviour and Its Potential Impact on Climate, Doctoral dissertation, University of Bergen, Bergen, 67 pp., 2013.</p> <p>our conclusion that true mean grain size (i.e. sortable-silt mean grain diameter, see discussion later) may be a better proxy for current-strength than the traditionally used sortable-silt index is based on a single (and unorthodox) site. Therefore, we cannot state with scientific rigour that this result reflects “THE TRUTH.” We pointed out some POTENTIAL problems with the volume-weighted sortable-silt index to stimulate further discussion. We strongly encourage colleagues to carry out similarly detailed analyses with respect to sortable silt on samples from different locations and time intervals to test both methods. If future research supports our findings then we should redefine the “sortable-silt index,” regardless of its traditional use.</p>
<p>Reviewer Comment</p>	<p>“Sortable silt mean size” (\overline{SS}) cuts out the $<10 \mu\text{m}$ part of the size spectrum where settling velocity is dominated by aggregates and the material by a lot of clay which may control the sample mean size (McCave et al., 1995).</p>
<p>Author Response</p>	<p>As it is discussed by McCave and Hall (2006) there is no point in including the fine-silt and clay fraction (i.e. grains with $d < 10\mu\text{m}$) in our analysis because these grains behave cohesively (i.e. they tend to stick together, so when the current is mobilising, transporting or depositing them it is acting on aggregates (i.e. groups of particles), rather than individual grains. However, we can analyse sediments with scientific rigour only in their disaggregated state in the laboratory. So these two size categories are not suitable for our purposes.</p> <p>Coarse (or sortable) silt ($10\mu\text{m} < d < 63\mu\text{m}$) behaves non-cohesively (i.e. these particles tend not to stick together), hence they are current-sorted by their primary particle size and are therefore suitable for making inferences about the strength of the current that has deposited them.</p> <p>As clearly stated in the paper only grains with $10\mu\text{m} < d < 63\mu\text{m}$ (meaning the sortable-silt fraction) were included in our analysis. Sample preparation and measurements were all done in accordance with McCave and Hall 2006. It is only the calculation of “sortable-silt MEAN SIZE,” which is different. Our aim was NOT to question the theory summarised by McCave and Hall (2006), only to better approximate the average physical size of grains WITHIN the sortable-silt fraction. HAD we included the fine-silt and clay fraction (i.e. grains with $d < 10\mu\text{m}$) when calculating true mean grain size (i.e. sortable-silt mean grain diameter), it would not be directly comparable with the “sortable-silt index”</p>

	and our logic would be flawed.
Reviewer Comment	Normalisation may be necessary to compare number-based data with other data, but the actual size is important.
Author Response	<p>As our paper pre-dates the publication of Thornalley et al., 2013 at the time of writing we were working on the assumption that no absolute scale had been assigned to the sortable-silt index. Therefore, we were focusing on the relative magnitude of changes through time in the case of each of the two datasets, using the Late Holocene segments of the respective records as reference. In addition, we wanted to emphasise the differences in the pattern of change through time between the sortable-silt-index time series and the true-mean-grain-size record. Hence, we normalised both datasets for easier comparison.</p> <p>The whole idea of using true mean grain size (i.e. sortable-silt mean grain diameter) as a current-strength proxy stems from our belief that in mathematical-statistical descriptions we should approximate the physical processes as closely as possible.</p> <p>As the Reviewer states, it is the actual (physical) size of the grains that ultimately matters. The entire theory summarised by McCave and Hall (2006) revolves around the forces acting on/sorting <i>primary</i> particles. To make inferences about the magnitude of these forces (i.e. the strength of the current) we need a number, which best describes the characteristic size of the grains the current was acting upon within each sample (from each time interval). This is part of the reason why we would prefer the number-percent-weighted true mean grain size (i.e. sortable-silt mean grain diameter) over the volume-percent-weighted sortable-silt index.</p> <p>As described by Tegzes (2013):</p> <p>“... according to the typical differential number distribution of grains for the Early-Holocene part of the record 92-94% of grains fall into the 10-25µm size range. The mean grain size varies between 15.02-15.42µm, while the sortable-silt index between 25.7-30.0µm.”</p>
Reviewer Comment	Thornalley et al, (2013) give a calibration based on surface sediments from current meter sites yielding flow speeds that are in broad agreement with other data, so it is not correct to say “...sortable silt offers only a qualitative record of the depositing current, but it does not tell us anything about the magnitude of the changes in absolute terms.” (p. 668, line 7-9).
Author Response	<p>We were aware of the ongoing programme of I. N. McCave to put an absolute scale on sortable-silt records. However, as mentioned above, our paper pre-dates the publication of Thornalley et al., 2013.</p> <p>The manuscript will be corrected accordingly in revision.</p>
Reviewer Comment	The volume-based sortable silt mean size on Fig 3 is thus most useful for comparison.
Author Response	<p>While the calibration data in Thornalley et al., 2013 looks promising, the results need to be tested at other locations to ensure their robustness across regions and depositional settings.</p> <p>The authors of that paper themselves note that “it should be stressed that, from early indications, it is by no means evident that the calibration data shown ... for the northern Iceland Basin can be applied to other ocean settings.”</p> <p>When comparing our volume-percent-weighted sortable-silt index record with the number-percent-weighted true mean grain size (i.e. sortable-silt mean grain diameter) time series it is obvious that during most intervals the general trends in the two datasets are in good agreement (Fig. 2 in our paper). Nevertheless, there are time periods, some of which are of particular interest, like the second half of the 9th millennium BP, when there are significant differences between the trends</p>

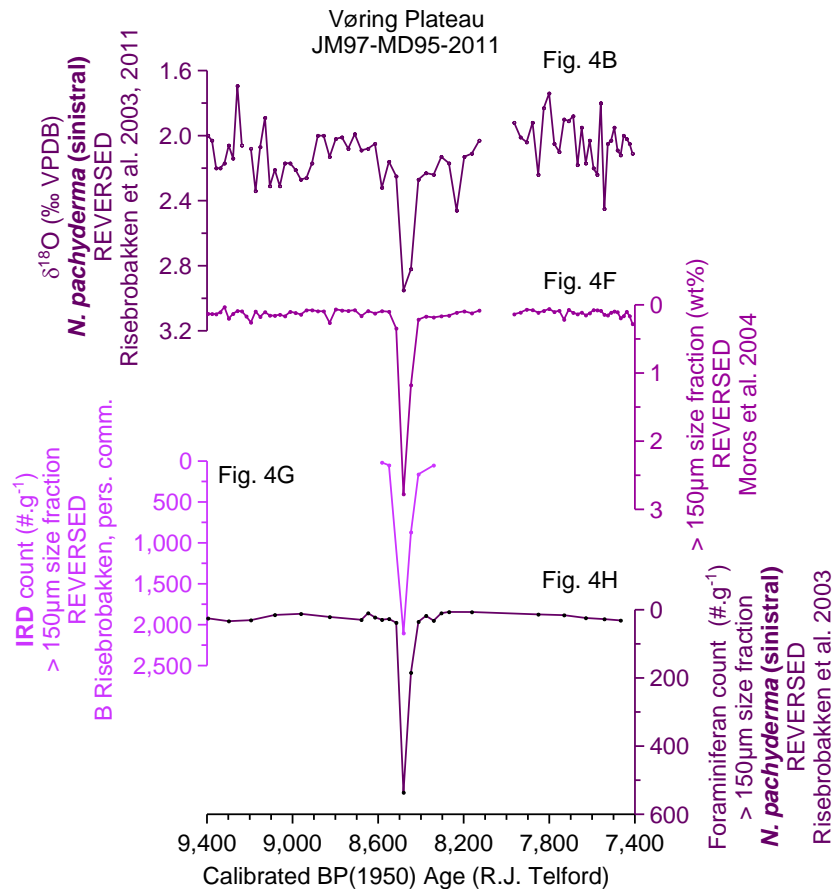
	reflected by the sortable-silt index and how the coarseness of the 10-63 μ m fraction changes along the core in “reality.” However, we would not be aware of this, had we not calculated “true mean grain size” as well. So we have to be very careful in making assentation as to which proxy is most “useful.”
Reviewer Comment	It is surprising therefore that on Fig. 3 where the authors compare a number of isotopic records, including that of Ellison (2006) that they do not also include Ellison’s detailed grain size record across this 8.2 – event interval. Had they done so it would have been seen that on Gardar Drift the deep return flow of ISOW (resulting from convection of the water supplied by NwAC) has a slow decline in speed between about 8500 and 8200 BP but a sharp increase between 8050 and 7900 BP. This contrasts with the sharp fall/slow rise seen at the JM/MD Voring site where the flow speed resumption lasts until at least 7500 BP.
Author Response	<p>A more detailed treatment of the 8.2ka Event based on all relevant data can be found in Tegzes 2013 (doctoral dissertation), which discusses in detail the palaeo-records representing the three major limbs of the overturning loop:</p> <ol style="list-style-type: none"> 1. the NwASC – sortable-silt datasets from Tegzes 2013 2. the ISOW – sortable-silt index time series from Ellison et al., 2006 and Hoogakker et al., 2011 3. the NEADW (basically DSOW) – $\delta^{13}\text{C}$ record from Kleiven et al., 2008 <p>It also examines the possible mechanism behind the potentially asynchronous response of these currents to the floodwater and iceberg discharge from Hudson Bay.</p> <p>Because of previous comments that the paper was too complex, we decided to cut certain sections from the original version and chose to focus on proxies related to the surface properties of the ocean in the study area. However, the cut sections can easily be re-inserted into the discussion, should the editorial review propose such a change in the manuscript.</p> <p>In a simple two-dimensional model where a single inflow and a single overflow form a closed overturning loop, fueled only by thermohaline convection it would be “surprising,” if the inflow and overflow did not respond in unison.</p> <p>In the real world the NwASC is driven both by thermohaline convection AND wind forcing. While it is considered to be the main branch of the Atlantic Inflow, the Norwegian Atlantic Current (NwAC) has another branch too: the NwAFC (see e.g. Mork and Skagseth, 2010; Fig. 1A in our paper). While the NwASC is regarded to be the main conduit for warm waters into the Arctic, the NwAFC tends to feed the interior of the Nordic Seas, at least in the modern ocean. The overflows have two major branches, the ISOW and the DSOW, and there are outflows as well which contribute to the mass balance of waters flowing into and out of the Nordic Seas. In this context it is not necessarily surprising that the flow-strength reconstructions of the NwASC and the ISOW do not mirror one another.</p>
Reviewer Comment	The authors need to be cautious about saying “... we cannot invoke age-model uncertainties ...” (p. 671, line 6-8) because the $\delta^{18}\text{O}$ drop is recorded just a cm or so below the SS drop, but the abundance of <i>N. pachyderma</i> increases by ~20% up-core at this point, and several authors have pointed out that bioturbation across such sharp gradients can result in them being displaced downwards; i.e. in the sense that would make the $\delta^{18}\text{O}$ shift appear earlier (e.g. Trauth, 2013 and refs therein). So the conclusion regarding relative timing of cooling and flow speed decrease is not robust. Flow slowdown might lead temperature decline.
Author Response	We are aware that bioturbation always has to be taken into consideration when analysing palaeorecords and that this possibility can rarely be totally eliminated. However, see the section on resedimentation.

The $\delta^{18}\text{O}_{\text{NPS}}$ drop IS recorded a centimetre below the drop in the sortable-silt index (Fig. 3E):

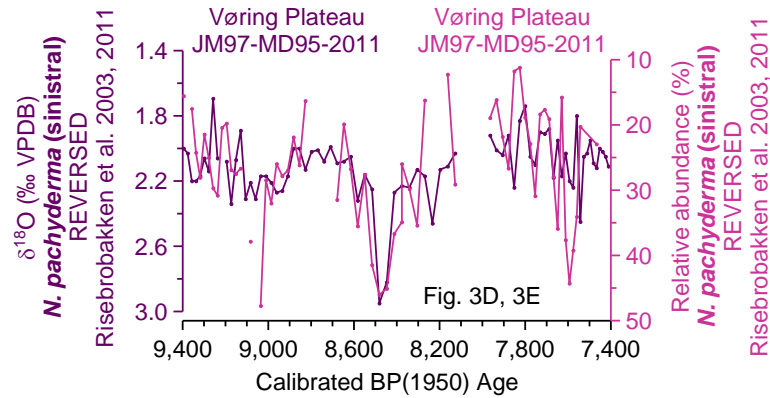


Looking at these two datasets only (i.e. $\delta^{18}\text{O}_{\text{NPS}}$ versus sortable-silt index, Fig. 3E) it could be said that those few *N. pachyderma* shells, which Risebrobakken et al. (2011) analysed for oxygen isotopes had moved downcore relative to their original position due to bioturbation.

However, when analysing the coarse fraction of the sediment (here meaning grains $>150\mu\text{m}$, Fig. 4F) several parameters show a large maximum in perfect synchronicity with the maximum in $\delta^{18}\text{O}_{\text{NPS}}$ (Fig. 4B). All of these parameters reflect grain-size distributions and some of these grains have very different physical properties: consider for example the lighter, more spherical foraminiferal shells (Fig. 4H) versus IRD fragments (Fig. 4G). We would expect if bioturbation had affected our site, the maxima in these parameters would more likely be “smeared”/shifted, not only relative to the drop in sortable-silt values, but also relative to one another.



As to the relationship between $\delta^{18}\text{O}_{\text{NPS}}$ and *N. pachyderma* (sinistral) abundance here are the two records from Fig. 3 superimposed on one another:



The peak in $\delta^{18}\text{O}_{\text{NPS}}$ coincides with the maximum in the relative abundance of *N. pachyderma* (sinistral).

Note that the value of the RELATIVE abundance of *N. pachyderma* (sinistral) also depends on the productivity of other species in the planktonic foraminiferal assemblage, while $\delta^{18}\text{O}_{\text{NPS}}$ is a signature recorded by *N. pachyderma* (sinistral) only. Consider the figure below:

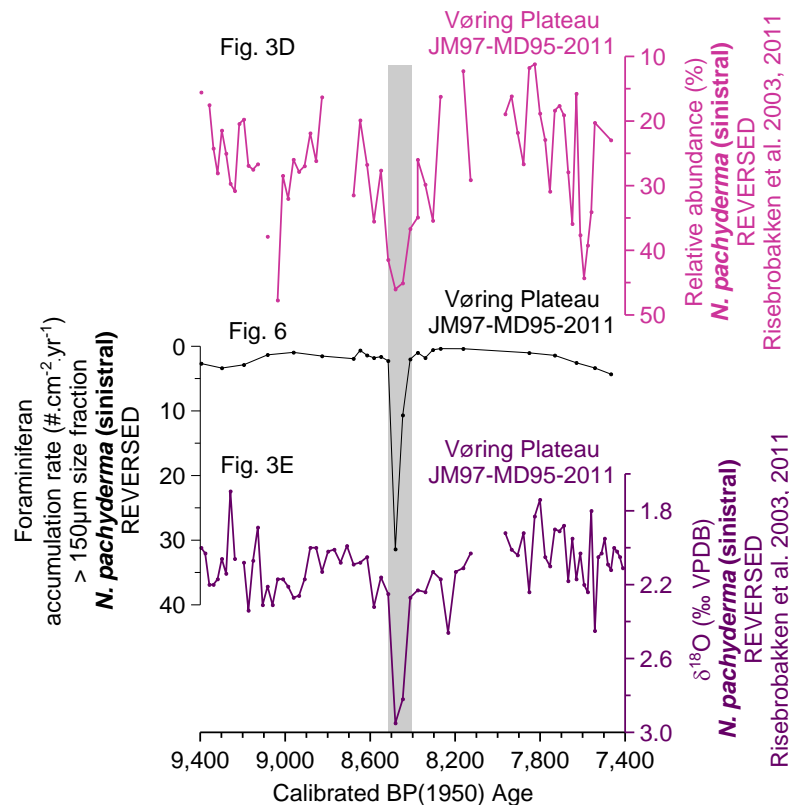


Fig. 6 above has been calculated from the foraminiferan count data of Risebrobakken et al. (2003) (Fig. 4H above), but here we have also taken into account changes in reconstructed sedimentation rates at our site. We used a 2g.cm^{-3} average sediment density to calculate values in $\#\text{.cm}^{-2}\text{.yr}^{-1}$, but this obviously does not affect the pattern of relative variability through time (Kučera, 1998).

The difference between the relative abundance (Fig. 4D) and accumulation rate of *N. pachyderma* (sinistral) is obvious (Fig. 6). The maximum in the latter is in perfect synchronicity with the peak in $\delta^{18}\text{O}_{\text{NPS}}$ (Fig. 3E).

	Also note that in the section on re-sedimentation we used the sortable-silt index to represent the coarseness of the fine fraction (here meaning grains 10-63µm in size), because its calculation relies on the volume-/weight-percent distribution of grains, similarly to the coarse-fraction time series of Moros et al. (2004).
LINE COMMENTS	
Reviewer Comment	p666/3: not 'over' but 'in'
Author Response	We will make the necessary correction.
Reviewer Comment	p666/11: should read not site but "... cores JM97-248/2A and MD95-2011 on ..."
Author Response	Here we are intentionally referring to the specific location ON the mid-Norwegian Margin, where these cores have been extracted from. If we were referring to the cores themselves, we would have written: "Here we present proxy records from sediment cores JM97-248/2A and MD95-2011 extracted FROM a high-accumulation area on the mid-Norwegian Margin."
Reviewer Comment	p666/11-12: how do we reconcile "records ...indicating a (sharp) decline in strength ..." with "... do not evidence an exceptionally strong reduction ..."? Is 'eastern' versus 'main' the key?
Author Response	"(sharp decline)" refers to both datasets: <ul style="list-style-type: none"> • the DECLINE in true mean grain size • the SHARP DECLINE in the sortable-silt index Hence the use of the bracket. Look at Fig. 2: "SHARP DECLINE" refers to the abruptness of the drop in the sortable-silt index record from 8,481 years BP to 8,447 years BP. However, if one compares this with the Late Holocene segment of the same record (i.e. the sortable-silt index time series), similarly large changes are evident there as well. Also note that the resolution of the cores increases as we move towards the most recent part of the record. If you compare the change in true mean grain size from 8,481 years BP to 8,269 years BP with fluctuations in the Late Holocene segment of the same record (i.e. the true-mean-grain-size time series) you can see that the changes are much more dramatic over the past 4,200 years.
Reviewer Comment	p667/2: also Vinther et al., 2006 (JGR, 111) for Holocene ice age.
Author Response	We will include Vinther et al., 2006.
Reviewer Comment	p668/8-9: See preliminary calibration in Thornalley et al. (2013) which allows an estimate of flow speed: An estimate of the magnitude of changes is permitted.
Author Response	We will make the necessary corrections here.
Reviewer Comment	p669/12: The term "sortable silt index" is not used (not in WOS database). "Sortable silt mean size" (SS) and sortable silt percentage (SS%) are used as indices of current strength in the present authors' and other labs (Kleiven et al., 2011).
Author Response	We wanted to avoid misleading terminology. We wanted to make a clear distinction between the traditionally used volume-percent-weighted arithmetic average (sortable-silt index, i.e. sortable-silt

	mean size) and the number-percent-weighted arithmetic average (true mean grain size, i.e. sortable-silt mean grain diameter).
Reviewer Comment	p669/12-13: The calculation of the mean is based on volumes in bins of logarithmically increasing size. Rather than claiming ‘truth’ to be on their side wouldn’t it be better to refer to a volume-mean and a number-mean size?
Author Response	<p>The word “true” in the expression “true mean grain size” is used as a synonym for “actual” or “real.” It refers to the fact that this number approximates the characteristic physical size (i.e. the mean or average size) of the grains within the sample. It reflects a concept/an approach to describing the coarseness of these sediments. It is not there to signify that this average is better than the sortable-silt index.</p> <p>To cite Tegzes (2013):</p> <p>“... according to the typical differential number distribution of grains for the Early-Holocene part of the record 92-94% of grains fall into the 10-25µm size range. ... [True] mean grain size varies between 15.02-15.42µm, while the sortable-silt index between 25.7-30.0µm.”</p> <p>From this it is obvious that there is a two-fold difference between true mean grain size and the sortable-silt index in the case of the above samples. It is also obvious that the sortable-silt index (or as the Reviewer referred to it before “sortable-silt mean size”) with values ranging between 25.7-30.0µm cannot be a measure of the average or characteristic size of grains in Early Holocene samples, when 92-94% of these grains are only 10-25µm in diameter.</p> <p>We are aware that the Coulter Counter gives only an estimate of grain diameters, using spherical approximation, but that does not change the logic of the above argument.</p> <p>While “true mean grain size” better reflects the concept behind our calculations, we will use “sortable-silt mean grain diameter” in the revised manuscript instead to avoid any misinterpretation in the future.</p>
Reviewer Comment	p669/16: “Random effects” is rather cryptic; explain.
Author Response	<p>Tegzes (2013) gives a detailed discussion. E.g.:</p> <ul style="list-style-type: none"> • “Ideally, the index should be such that it reflects the typical behaviour of the current during the time period over which it is calculated. If brief infrequent instances of exceptionally strong flow [i.e. “freak events”] are able to “overprint” that signal then this will give a false impression of the past strength of the current ...” • “... coincidence reduces the observed number of particles and increases the observed average particle size (Wynn and Hounslow, 1997) ... [when running samples on the Coulter Counter].”
Reviewer Comment	p669/23: Why, for compression, have the authors amalgamated just the first element of JM97 with all of MD95-2011 ...? Could be shorter as JM97/MD95 or 97/95 or JM/MD etc?
Author Response	JM97-948/2A is a box core and covers only the past approximately 582 years. The rest of the Holocene, including the interval bracketing the 8.2ka Event, is covered by the gravity core MD95-2011. MD95-2011 is a well-known core, which has a long history in a large number of publications (i.e. it has been thoroughly analysed), so we wanted to retain that core number.
Reviewer Comment	p669/25: Better if the ‘replace all’ command were used to put ‘number’ instead of ‘true’, [and ‘volume-mean’ for ‘index’].
Author Response	See response above.
Reviewer	p670/3: The fluctuations in SS volume mean do not appear much larger than those in number mean

Comment	(normalised). Perhaps a Std. Dev is needed to demonstrate this assertion. The volume mean (SS) at MD2251 on Gardar Drift shows a similar slower decline to that shown by the number-mean here. To avoid confusion, insert ‘... than around the 8.2 ka event’ before ‘Fig. 2’).
Author Response	We will paraphrase this sentence.
Reviewer Comment	p670/9 Data (volume mean) do not suggest “much larger slowdowns” in the late Holocene than that at 8481-8447 years BP.
Author Response	We will paraphrase this sentence.
Reviewer Comment	It is no longer necessary to refer to a sinistral morphotype since Darling et al. (2006) demonstrated that <i>N. pachyderma</i> is sinistrally coiling and that the supposed dextral morphotype is in fact a different species (<i>N. incompta</i>).
Author Response	These datasets have previously been published by Risebrobakken et al. (2011) and Risebrobakken et al. (2003). Therefore, we kept the name “ <i>N. pachyderma</i> (sinistral)” for consistency with the cited papers. However, we will include a remark on the subject.
Reviewer Comment	p671/6-8: The authors need to be cautious about “... we cannot invoke age-model uncertainties ...” because the $\delta^{18}\text{O}$ drop is recorded just a cm or so below the $\overline{\text{SS}}$ drop, but the abundance of <i>N. pachyderma</i> increases by ~20% up-core at this point, and several authors have pointed out that bioturbation across such sharp gradients can result in them being displaced downwards; i.e. in the sense that would make the $\delta^{18}\text{O}$ shift appear earlier. So the conclusion regarding relative timing of cooling and flow speed decrease is not robust.
Author Response	See detailed response above.
Reviewer Comment	p674/21-22: See p. 671/lines 6-8 re. problem with asserting non-synchronicity of flow and $\delta^{18}\text{O}$.
Author Response	See detailed response above.
Reviewer Comment	p675/6-16 This para is relatively unconstrained speculation.
Author Response	Yes, this is only a possible explanation or theory, put forward to explain the data we have. See detailed response to Reviewer 2 below.
Reviewer Comment	p676/1: -paced or –spaced?
Author Response	“paced”
Reviewer Comment	p677/20-21: The cores discussed are under the eastern branch, not the main branch (NwAFC) of the inflow.
Author Response	The Atlantic Inflow into the Nordic Seas has two main branches: <ol style="list-style-type: none"> 1. The western branch or Norwegian Atlantic Front Current (NwAFC) 2. The eastern branch or Norwegian Atlantic Slope Current (NwASC), which is considered to be the main conduit for advected heat towards the Arctic. <p>See e.g. Orvik and Skagseth, 2003; Orvik and Skagseth, 2005.</p>

Reviewer Comment	p679/12-13: Not necessary to list all 12 cities in which Elsevier has an office: one will do.
Author Response	We will use one city if that suffices.
FIGURES	
Reviewer Comment	Fig. 1 A hand lens is needed to read this. Why is the sea grey? Why not white with black lines (instead of white) plus red arrows. In the caption EGC not ECG.
Author Response	<p>Both Fig. 1A and Fig. 1B is a 112x94mm TIFF image (PackBits compression) with a resolution of 300dpi. They were designed with the dimensions of published CP papers in mind, not the discussion paper slide show. We went through a couple of iterations during type-setting and this was the best arrangement we could come up with due to technical constraints. Since this is primarily an electronic (not a print) version the size of the map depends on the size of your screen and your zoom settings. The images can be enlarged to 1600% before you start noticing the individual pixels.</p> <p>The sea is grey so that the lines representing the (sub)surface-current system can be white. The arrows representing the (sub)surface-current system are white so that when the core sites are superimposed in bold black font it is still pleasant for the eyes to look at. We tried to avoid chaotic, criss-crossing lines.</p> <p>“ECG” is a typo, which we have not noticed before, but we will correct.</p>
Reviewer Comment	Fig. 2. How was normalisation performed – just by the mean or with Std Deviation?
Author Response	Using the arithmetic mean and maximum absolute deviation from the mean. This is how we got values between (-1) and (+1).
Reviewer Comment	The y-axis in not the appropriate place to list the data source, that should be in the caption. In E and F change ‘index’ and ‘true’.
Author Response	<p>We labelled the graphs in a way that the reader gets all relevant information at a glance without having to repeatedly consult the caption. The aim was to make a clear distinction between our records and already published data. This can easily be changed if so desired.</p> <p>As mentioned earlier we wanted to avoid misleading terminology. Hence the use of “sortable-silt index” and “true mean grain size.”</p>
Reviewer Comment	Fig. 4 As every single panel A-J is cluttered up with the same “Voring Plateau JM97-MD95-2011” it would be far better to remove all and insert the information in the caption. Why does one of the co-authors get a special note on the x-axis that he did the age model? Again, wrong place for the info. There is no line along zero for panel G (assuming it should be zero except for at 8,500). In I the species should be <i>N. incompta</i> .
Author Response	<p>Fig. 4: Several age models have been published for JM97-948/2A and MD95-2011 throughout the years. The one which we used is R.J. Telford’s most complete to date. This age-depth model was constructed using mixed-effect regression, while, for example, Risebrobakken et al. (2011) used linear interpolation between dated horizons. The information may of course be located elsewhere, e.g. in the caption, if so desired. A detailed description of our age-depth model can be found in Tegzes 2013.</p> <p>Panel G: IRD count data exists only for that short time interval.</p> <p>Panel I: These datasets have previously been published by Risebrobakken et al. (2011) and Risebrobakken et al. (2003). Therefore, we keep the name “<i>N. pachyderma</i> (sinistral)” for consistency with the cited papers. However, we will insert a note on the subject.</p>

<p>Reviewer Comment</p>	<p>In Page 668 Line 15 the authors state that the current has not been in direct contact with the sediment. How has it influenced the sedimentation processes by sorting the sediment at the core site? This statement forms the basis of the paper. It is essential that the mechanism by which the sediment at the site is sorted by the NAWC strength is explained in-detail. Information on the present oceanographic setting at the site should also be included at this point in order to convince the reader that the grain size measurements from this site are indeed indicative of changes in the strength of the NAWC.</p>
<p>Author Response</p>	<p>This is a similar comment as that made by Reviewer 1 (please see above). The details of the oceanographic setting and the utility of the proxy can be found in a separate paper submitted to <i>Paleoceanography</i>. This comprises a detailed and lengthy section, which would appear somewhat out of scope for a paper that focusses on the 8.2ka Event. Our approach is to refer to this paper (the relevant sections can be sent to the reviewers in support of the review). Alternatively, the current paper would need to be considerably lengthened, and similar sections would appear in two separate papers, which would not be an optimal solution.</p>
<p>Reviewer Comment</p>	<p>The authors explain (Page 669, Line 11) that they have used the true mean grain size, which is calculated from the average of the differential number distribution of grains within a sample as opposed to the volume (sortable silt). Naming this the ‘true’ mean grain size is misleading since the coulter counter measures the volume and not the size and this parameter is calculated by using the number of grains as opposed to the volume. It may also be useful to present the formulas used to calculate the SS and ‘true mean grain size’ so it is clearer to the reader what these two parameters are. The discussion that the authors present regarding the robustness of the grain size parameters is interesting. However, why the ‘true mean grain size’ is better than the volume is not fully justified.</p>
<p>Author Response</p>	<p>In current-sorted and -deposited muds the sortable-silt signal (i.e. changes in the COARSENESS of the 10-63µm terrigenous silt fraction of the sediment) is thought to arise mainly from selective deposition as some grains are trapped in the viscous sublayer of the turbulent boundary layer just above the sea floor, while others of smaller settling velocity are kept in turbulent suspension and transported further downcurrent (McCave and Hall, 2006).</p> <p>The above theory is referring to the forces acting on INDIVIDUAL grains. The actual size of the grains (e.g. the physical diameter that you could measure with a ruler under a microscope in micrometers, assuming that these grains tend to be approximately spherical) that get deposited at a given location (i.e. our core site) at a given point in time reflects the magnitude of the forces acting close to the sea floor at that moment, which is dependent upon the strength of the current.</p> <p>When we are analysing sediment samples, we are working e.g. with a 1cm thick slice of the sediment core at a time. That centimetre-thick layer of sediment may have been deposited over e.g. 15 years on the sea floor at our site. Obviously, during those 15 years the strength of the current varied greatly. If we want to find out the average strength of the current during those 15 years, a logical approach is to find out the characteristic (average) size of the grains, which the current deposited during those 15 years, that is, the average or mean size of the grains in our sample representing those 15 years.</p> <p>The word “true” in “true mean grain size” refers to the ACTUAL, PHYSICAL size of the grains in the sample, which can be represented by e.g. the diameter of individual grains that you can measure with a ruler under a microscope (i.e. in µm), but you could equally use the volume of INDIVIDUAL grains (i.e. in µm³) as calculated from the INDIVIDUAL voltage pulses registered by the Coulter Counter. This is beside our point. In both cases we are trying to approximate the ACTUAL size of the grains in the sample.</p> <p>If we want to calculate how large the AVERAGE grain is in our sample (using grain diameter and</p>

assuming that the grains in our sample tend to be approximately spherical) normally we would measure the diameter of each and every grain, add these values up and divide that sum with the number of grains in the sample. This mean grain diameter (\bar{d}) would characterise the size of the average grain in our sample.

When running samples on the Coulter Counter we measure on the order of 70,000 grains per sample. We get the results grouped into 256 equal size bins. The size bins are defined in grain diameter (in μm). The Coulter Counter output tells us how many of the grains out of the 70,000 counted fall into the individual size bins. Following the logic of the previous paragraph we can calculate the mean diameter of the grains within our sample (\bar{d}):

$$\bar{d} = \frac{1}{N} \sum_{i=1}^{256} (N_i \cdot d_i)$$

where

N is the total number of grains measured (i.e. approx. 70,000);

N_i is the number of grains that fall into the i^{th} size bin ($i = 1, 2, 3, \dots, 256$);

d_i is the mid-point value of the i^{th} size bin ($i = 1, 2, 3, \dots, 256$).

In our paper we referred to \bar{d} as “true mean grain size” to clearly distinguish it from the traditionally used sortable-silt index and to emphasise our aim to approximate the ACTUAL size of the grains in our samples.

The traditionally used sortable-silt index (or as Reviewer 1 referred to it “sortable-silt mean size,” i.e. \overline{SS}) is calculated differently:

$$\overline{SS} = \frac{1}{V} \sum_{i=1}^{256} (V_i \cdot d_i)$$

where

V is the total volume of all (i.e. approx. 70,000) grains measured per sample;

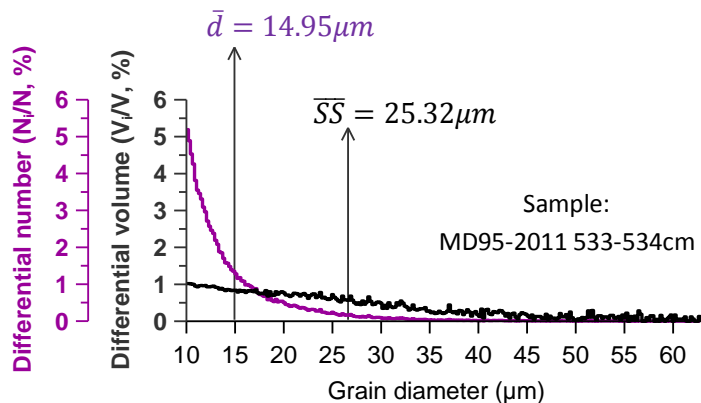
V_i is the TOTAL volume of all the grains that fall into the i^{th} size bin ($i = 1, 2, 3, \dots, 256$);

d_i is the mid-point value of the i^{th} size bin ($i = 1, 2, 3, \dots, 256$).

So here the grain diameters (representing the different size bins) are weighted by the respective volume-percents of the grains within those size bins relative to the total volume of all the grains within our sample. \overline{SS} may be considered a measure of the coarseness of the sample, but will NOT represent the ACTUAL size of the AVERAGE grain in the sample, which may be more closely related to the physical processes that produced the grain-size distribution of the sampled sediment horizon.

Consider the following example:

The figure below is based on a single run of sample MD95-2011 533-534cm. Using the saved pulse data (i.e. the “raw” Coulter Counter output) we can generate both the differential number distribution of the grains within this particular sample (lilac graph) and the differential volume distribution of the same sample (black graph). The former represents the actual, physical grain-size distribution and is used to calculate the “true” mean grain diameter or “true mean grain size.” The latter is used to calculate the sortable-silt index.



This is not to say that the traditionally used sortable-silt index is wrong. For one, the sortable-silt signal is probably produced by more complex processes than what is suggested by the core idea of the theory as explained in the first paragraph. However, the significant difference between the sortable-silt index record and the true mean grain size time series (Fig. 2 in our paper) may indicate that the issue raised here may merit further research. For a more detailed treatment of the subject see Tegzes 2013.

The problem of spherical approximation has relevance to the entire sortable-silt theory (and other measurement techniques, e.g. settling tubes, Sedigraph) as grain shape can greatly influence settling velocities, and does not specifically apply to this paper.

Reviewer Comment

The references used to back-up this argument in line 15 Page 269 are not accessible.

Author Response

Tegzes et al., 2014b has not yet been published, and is in revision. However, we can send the Reviewer the relevant section from Tegzes 2013.

Reviewer Comment

... the interpretation and discussion from the grain size data presented in this study is very limited and fairly inconclusive. I wonder if perhaps the discussion can be extended to try and incorporate the new findings and explain the grain size record in the context of changes in the AMOC instead of the exhaustive review of previous literature on this topic.

Author Response

Although the Atlantic Inflow into the Nordic Seas and the overflows are interconnected (e.g. Eldevik et al., 2009), one cannot assume that variations in the strength of the eastern branch of the Atlantic Inflow along the mid-Norwegian Margin directly translate into changes in the AMOC. The NwASC is driven both by thermohaline convection AND wind forcing. While it is considered to be the main branch of the Atlantic Inflow, the Norwegian Atlantic Current (NwAC) has another branch too: the NwAFC (see e.g. Mork and Skagseth, 2010; Fig. 1A in our paper). The overflows have two major branches, the ISOW and the DSOW, and there are outflows as well, which contribute to the mass balance of waters flowing into and out of the Nordic Seas.

It is also important to note that we did not simply summarise the already published literature (it would not have made much sense), but moulded these datasets into a “new” theory. For example:

- Bond et al. (2001) used their North Atlantic IRD records to support the hypothesis that a solar forcing mechanism may underlie the 1,500-year cycles in surface hydrography variations (i.e. changes in the amounts and trajectories of glacial ice/drift ice) in the northern North Atlantic. We noticed that the uniquely large spike in the detrital carbonate record at around 8,500 years BP does not fit into this pattern.
- The uniquely large positive excursion in the $\delta^{18}\text{O}_{\text{NPS}}$ record of Risebrobakken et al. (2003) has been cited by several authors as evidence for the 8.2ka cooling over the northern North Atlantic sector, but because the drop in (sub)surface ocean temperatures at the mid-Norwegian Margin likely occurred earlier than the cooling over Greenland and it probably

also preceded the slowing down of the NwASC, it made us think that the cooling represented by the $\delta^{18}\text{O}_{\text{NPS}}$ spike may have been specific to the (sub)surface ocean. Working on the assumption that this was still somehow linked to the collapse of the ice dam over Hudson Bay and taking into consideration the IRD data of Bond et al. (2001), we put forward the idea that only a major iceberg discharge as opposed to floodwaters could cool the ocean that far north preceding the consequent general deterioration in climate conditions (including the cooling over Greenland).

A more detailed treatment of the 8.2ka Event can be found in Tegzes 2013 (doctoral dissertation), which also discusses the palaeo-records representing the three major limbs of the North Atlantic overturning loop of the AMOC:

1. the NwASC – sortable-silt datasets from Tegzes 2013
2. the ISOW – sortable-silt index time series from Ellison et al., 2006 and Hoogakker et al., 2011
3. the NEADW (basically DSOW) – $\delta^{13}\text{C}$ record from Kleiven et al., 2008

It also examines a possible mechanism behind the potentially asynchronous response of these currents to the floodwater and iceberg discharge from Hudson Bay.

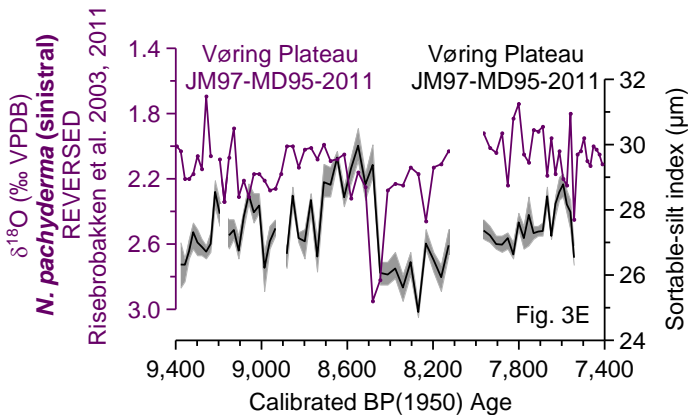
Because of previous comments that the paper was too complex, we decided to cut certain sections from the original version and chose to focus on proxies related to the surface properties of the ocean in the study area. However, the cut sections can easily be re-inserted into the discussion, should the editorial review propose such a change in the manuscript.

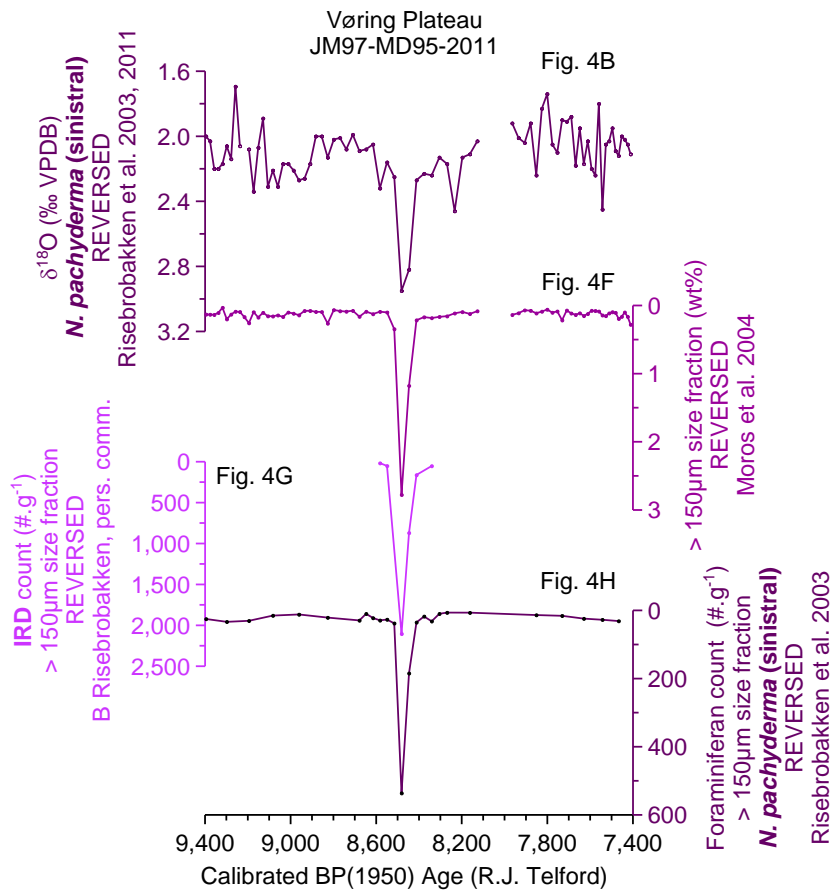
As to the “inconclusiveness” of our results: our objective here was to draw attention to a number of issues, which may have particular significance to modeling studies that aim to further our understanding regarding the sensitivity of the overturning circulation to freshwater forcing. The value added of these studies depends not only on the ability of models to capture the physics that shapes climate, but also on how closely palaeo-reconstructions, which often serve both as constraints and as validation data in these experiments, approximate the events that happened in the distant past.

If our theory is correct:

1. The initial freshwater forcing was significantly larger than the total volume of meltwaters contained by Lake Agassiz-Ojibway (Barber et al., 1999).
2. The temporal evolution and nature of the forcing was different from that suggested by the lake-outburst theory. Consider the significance of the latent heat of melting (see e.g. Wiersma and Jongma, 2010) and reduced mixing with ambient waters at lower latitudes in the case of a major iceberg discharge (compare with Condron and Winsor, 2011).
3. Even such a significant freshwater input could not bring about a uniquely large slowdown (shutdown?) of the overturning circulation, at least in the northward-directed warm (sub)surface current, the NwASC (Fig. 2 in our paper), as it has been shown in many modeling experiments (e.g. LeGrande et al., 2006; Renssen et al., 2001).
4. If that is true the main role of the NwASC (of the Atlantic Inflow) may have been the rapid advection of fresh and cold waters to high northern latitudes initiating rapid sea-ice growth and an increase in surface albedo.

We could have presented our data differently, but we were very conscious about emphasising both the errors in the different datasets/age models, where these were available to us, and that the theories put forward in our paper are only plausible possibilities, which in our view merit further research.

<p>Reviewer Comment</p>	<p>For instance: How does this data compare to the hydrographic changes of the Atlantic inflow from subpolar North Atlantic records or modelling studies [e.g. Thornalley et al., 2009; Bamberg et al., 2010; Born and Levermann, 2010]. Or whether the changes observed in the Voring Plateau are concomitant with changes in the overflow waters (as increased/decreased in the overflows would lead to increased/decreased Atlantic inflow reaching the Nordic Seas – e.g. comparison with [Ellison et al., 2006; Kleiven et al., 2008; Kissel et al., 2013]).</p>
<p>Author Response</p>	<p>While doing research for this paper we systematically reviewed all published datasets with a reasonable Holocene resolution from the North Atlantic region (including data from marine sediments and ice cores, other terrestrial records and model results). Kissel et al. 2013 was published after we had submitted our paper to CP.</p> <p>Please see our detailed response above.</p>
<p>Reviewer Comment</p>	<p>The apparent difference in the relative timing between the SS and 18O from <i>N. pachyderma</i> (s) (Nps) (Page 671 line 6) could be due to differential size mixing via bioturbation processes of each of the signal carriers (SS in the <63m and 18O Nps in the >63m fraction) this would lead to decoupling of these two paleoceanographic records obtained from the same core and an offset of 1cm wouldn't need such a large mixing layer [Bard, 2001; Weedon, 2003].</p>
<p>Author Response</p>	<p>We are aware that bioturbation always has to be taken into consideration when analysing palaeorecords and that this possibility can rarely be totally eliminated.</p> <p>Comparing only the $\delta^{18}\text{O}_{\text{NPS}}$ time series with the sortable-silt index record (Fig. 3E) it could be said that those few <i>N. pachyderma</i> shells, which Risebrobakken et al. (2011) analysed for oxygen isotopes had moved downcore relative to their original position due to bioturbation.</p>  <p>However, when analysing the coarse fraction of the sediment (here meaning grains >150µm, Fig. 4F) several parameters show a large maximum in perfect synchronicity with the maximum in $\delta^{18}\text{O}_{\text{NPS}}$ (Fig. 4B). All of these parameters reflect grain-size distributions and some of these grains have very different physical properties: consider for example the lighter, more spherical foraminiferal shells (Fig. 4H) versus IRD fragments (Fig. 4G). We would expect if bioturbation had affected our site, the maxima in these parameters would more likely be “smeared”/shifted, not only relative to the drop in sortable-silt values, but also relative to one another, which is clearly not the case.</p>



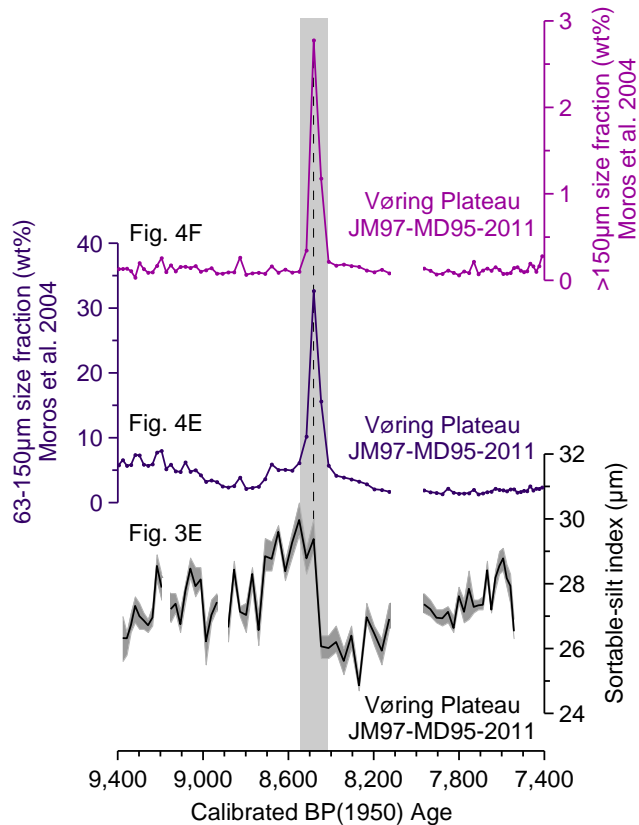
Because of the above, we were more concerned about the possibility that glacial sediments were “dumped” to our site at approximately 8,500 years ago e.g. as a result of a (smaller) slope failure, which are not uncommon in the region. This prompted us to thoroughly investigate all possible parameters of these sediments. Hence, the section on “resedimentation.”

We are not claiming that we have found irrefutable evidence (this is virtually non-existent in palaeoscience) for an iceberg discharge from Hudson Bay at around 8,500 years BP, but that it is a possibility, which is worth further investigating.

Reviewer Comment Section 5 is rather confusing. It firstly explains that the foraminifera from which the 18O from Nps was obtained at this time-interval [Risebrobakken et al., 2003, 2011] could have been reworked. On the basis that the change in the SS and the 18O from Nps does not happen at the same time (1cm off) the authors conclude that these signals must have been influenced by different processes and are therefore a climate signal. However, that 1cm difference between the SS drop and the 18O increase could be accounted for by bioturbation as mentioned earlier so this reworked material may have also affected the SS. This section needs rewriting as it is not clear and it makes the reader doubt if indeed the core is intact or contains reworked material.

Author Response As discussed above we think that it is not likely that bioturbation affected the phasing of the signals at this level.

Our main concern was that the sediment layer dated to be around 8,500 years old is (partly) made up of allochthonous grains. Based on their signature, i.e. high concentration of polar foraminiferal shells and ice-rafted debris, these could be glacial sediments dumped to our site e.g. as a result of a (smaller) slope failure. However, in that case we would expect to see a general coarsening of the sediments, INCLUDING the 10-63µm fraction, within the affected layer, which is not the case (see Figs. 3E, 4E, 4F below).



We used the sortable-silt index to represent the coarseness of the fine fraction (here meaning grains 10-63µm in size, Fig. 3E), because its calculation relies on the volume-/weight-percent distribution of grains within the samples, similarly to the coarse-fraction time series of Moros et al. (2004) (Figs. 4E, 4F).

We will revise Section 5 in line with our discussion here.

Reviewer Comment

Page 675 Line 4, there is a series of hypothetical feedback mechanisms explained in this paragraph, but there is a lack of references backing these up. Please add these.

Author Response

The paragraph starting with Line 4 on p675 is part of the theory continued in Line 19 on p676. As the Reviewer rightly says this comprises a number of “hypothetical feedback mechanisms,” which can POTENTIALLY translate the various records from the northern North Atlantic and Nordic Seas into a plausible sequence of events. This, however, still needs to be tested in high-resolution modelling experiments.

The logic behind the theory:

1. We identified two approximately coeval uniquely large IRD signals at two very different sites within the North Atlantic sector:
 - the detrital carbonate maximum off Newfoundland (Fig. 5; Bond et al., 2001);
 - the IRD peak and the large positive excursion in the weight percent of the >150µm fraction at the Vøring Plateau (Figs. 4G, 4H; Moros et al., 2004; B. Risebrobakken, personal communication) (Figs. 4G, 4H).

Both far exceed background “post-deglacial” variability at the respective locations.

2. Following a thorough analysis of sediment cores JM97-948/2A and MD95-2011 we concluded that the approximately 8,500-year-old deposits containing polar foraminiferal shells and IRDs in high concentration are likely not re-deposited glacial sediments and that this layer possibly

represents a time of colder (sub)surface-ocean conditions and a period of intense ice-rafting at the Vøring Plateau at around 8,500 years BP.

3. Provenance studies are not available from our site. However, there are only a limit number of options regarding the possible source of IRDs found in the circa 8,500-year-old sediments at the Vøring Plateau:
 - The Scandinavian Ice Sheet had completely melted by this time (A. Nesje, personal communication).
 - Accepting the hypothesis that the detrital-carbonate peak identified in the core extracted off Newfoundland (Bond et al., 2001) indicates a major ice-rafting event originating from Hudson Bay, it is unlikely that these icebergs survived (in large numbers) as far north as the Vøring Plateau.
 - Eliminating the previous two possibilities the only other source of icebergs/drift ice we could think of was the Arctic Ocean and the western Nordic Seas.
4. We needed a plausible mechanism, which could deflect icebergs/drift ice, leaving the Arctic Ocean through the Fram Strait, from their “normal” course towards the eastern Nordic Seas.
5. A number of recent papers discuss drift-ice export from the Arctic Ocean through the Fram Strait. E.g.:
 - Based on high-resolution radar satellite data from 2004-2010 Smedsrud et al. (2011) show that drift-ice export across 79°N corresponds well with variability in the local geostrophic wind. The East Greenland Current (EGC) contributes with a constant southward speed close to $5\text{cm}\cdot\text{s}^{-1}$ and drives around a third of the ice export.
 - Based on a modeling study van Angelen et al. (2011) conclude that the Greenland Sea Jet over the Fram Strait and the Greenland Sea (70-85°N) results from horizontal temperature gradients in the atmospheric boundary layer (ABL) set up between the cold ABL air over the sea-ice-covered western Greenland Sea and the relatively warmer ABL over the ice-free eastern Greenland Sea.
6. By changing the east-west temperature gradients across the Nordic Seas a major iceberg (and floodwater) discharge from Hudson Bay, as described in our paper, may have interfered with atmospheric and ocean circulation patterns in the Greenland and Norwegian Seas and could potentially repeatedly change the southward pathways of icebergs/drift ice in the Nordic Seas in the second half of the 9th millennium BP.
 - The ice-rafting event at the Vøring Plateau may be the consequence of a colder Atlantic Inflow (remember that the western branch of the Atlantic Inflow, the NwAFC feeds the interior of the Nordic Seas, at least in the modern ocean), a reduced temperature gradient across the Greenland Sea, a weaker Greenland Sea Jet, a weaker and less directional flow of waters transported in the East Greenland Current.
 - The freshening of waters in the subpolar North Atlantic coincident with the cooling over Greenland (Ellison et al., 2006) could be a consequence of increased iceberg/drift-ice export from the Arctic Ocean and Nordic Seas, which in turn could be (partly) due to a stronger Greenland Sea Jet, resulting from an increase in the temperature gradient across the Greenland Sea concomitant with a general decrease in surface temperatures and sea-ice expansion in the western Nordic Seas. This phenomenon is discussed by Bond et al. (2001), while Kleiven et al. (2008) found potential evidence for a stronger East Greenland Current (EGC) concomitant with the cooling over Greenland.

	We will include the above-mentioned references in this section.
SPECIFIC COMMENTS	
Reviewer Comment	Page 668 Line 28. As a side note, 5-10% concentration is a large concentration to be running on the coulter counter. Ideally it should be below 5% to avoid coincidence of particles.
Author Response	These inflow deposits are generally quite coarse in comparison with the sediments sorted by overflows. There is a trade-off between sample concentration and measurement time when running samples on the Coulter Counter. Heavier grains are more difficult to keep in suspension and there is almost always some degree of sedimentation during measurement, even if it is not strikingly obvious to the naked eye. You cannot increase stirrer speed beyond a certain value, because it starts generating a strong vortex and small bubbles in the beaker, which corrupts the data. Consequently, you need to reduce the measurement time. One way to do this is to increase sample concentration. We devoted a lot of time to finding the best settings. Initially, we carried out a “speed test” on a subset of samples to see how stirrer speed influenced our results. Almost all samples were run on the Coulter Counter at different concentrations to see whether coincidence can be a significant problem at high concentrations. For consistency all samples were run several times, each time in a different random order with our “best settings” to get the final results. The shaded envelopes in the sortable-silt graphs represent the scatter of values resulting from repeated measurements; they show you the actual spread of values for each sample, NOT the $\pm 1\sigma$ error.
Reviewer Comment	Page 670 line 1. Depending on how confident the authors are about the mechanisms by which the sediment sorting occurs at the site, it may be, that this current has not always affected the sediment deposition at this core location in the same way, perhaps due to E-W or vertical migrations of the current when it was weaker/stronger.
Author Response	The oceanography of the site is very complex and we devoted a separate paper to investigating all mechanisms that may influence sedimentation at our core location. While the paper (Tegzes et al., 2014a) is not yet published, we can send the relevant section from Tegzes 2013 to the Reviewer. Please also see our response to Reviewer 1.
Reviewer Comment	Page 670 line 10. At this point the authors mention that they will use the traditional SS proxy. After having argued in the methodology section that the ‘true mean grain size’ is better, using SS here is a bit confusing.
Author Response	We refer to both the sortable-silt index and true mean grain size time series throughout the paper: <ol style="list-style-type: none"> 1. We used the sortable-silt index dataset because to date this is the generally accepted current-strength proxy and to allow direct comparison with other current-strength datasets and time series based on the volume-/weight-percent distributions of sediment grains (see e.g. the section on re-sedimentation). 2. True mean grain size approximates the actual physical size of grains in individual samples, thus to a first approximation it better translates the physics behind the sortable-silt theory into numbers. 3. We also think that true mean grain size is potentially a more robust measure of mean current strength and less prone to biases due to analytical errors than the sortable-silt index. However, this conclusion is based on an analysis of sediments from our site only. Therefore, there exists the possibility that these results are site-specific. 4. Since we wanted to give a balanced view of our findings, we first presented an analysis based on the traditionally used sortable-silt index and then showed how replacing the sortable-silt index with true mean grain size changed our conclusions.

We will improve the wording of this paragraph.

Reviewer Comment

Page 671 line 15. The %Nps coincides with the heavier 18O event. Sentence starting in line 16 is very vague, and also the follow-up sentence.

Author Response

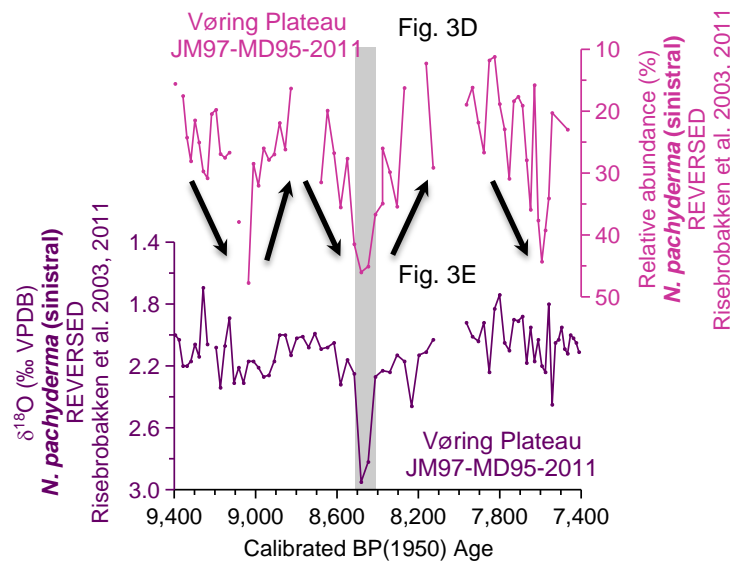
Quote from the paper:

“... the $\delta^{18}\text{O}_{\text{NPS}}$ spike coincides with a somewhat broader maximum in the relative abundance of *N. pachyderma* sinistral (Fig. 3d and e). However, this is not unique with respect to the Holocene (Fig. 4a).”

Explanation:

Our starting point in researching the 8.2ka Event was that it was a uniquely large climate perturbation in the “post-deglacial” Holocene North Atlantic sector. Therefore, we were looking for uniquely large climate signals in the records representing Holocene climate variability in the region. While the Holocene *N. pachyderma* (sinistral) abundance record of Ellison et al. (2006) (see Fig. 1 in that paper) shows a very clear signal in the mid-/late-9th millennium BP, which significantly exceeds background “post-deglacial” variability at their site, the *N. pachyderma* (sinistral) abundance record of Risebrobakken et al. (2011) from our location is not conclusive in that respect: i.e. it shows a maximum at around the time of the collapse of the ice dam over Hudson Bay, but that maximum is not uniquely large in comparison to background “post-deglacial” variability in that time series.

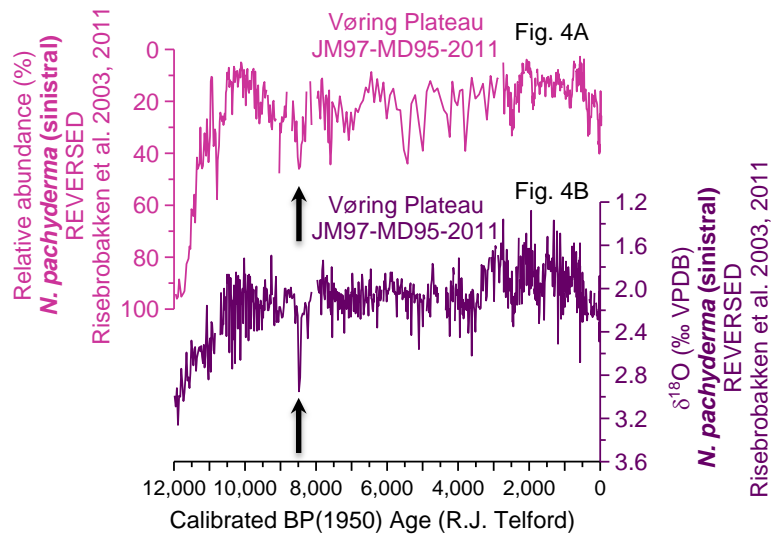
As highlighted in grey shading below the prominent maximum in the $\delta^{18}\text{O}_{\text{NPS}}$ record (Fig. 3E) coincides with the “apex” of the broader “V-shaped” maximum in the relative abundance of *N. pachyderma* (sinistral) (Fig. 3D).



It is also obvious from Fig. 3D that UNLIKE the unique peak in the $\delta^{18}\text{O}_{\text{NPS}}$ record, the broader “V-shaped” maximum in the relative abundance of *N. pachyderma* (sinistral) is part of a “repetitive” pattern as indicated by the bold black arrows. It is also evident that the *N. pachyderma* (sinistral) abundance signal carries quite a lot of relatively high-amplitude high-frequency noise.

If we zoom out and take a look at the entire Holocene the difference between the maxima in the two records at around 8,500 years BP (indicated by the black arrows in the figure below, Fig. 4A and 4B) is even more striking. The peak at around 8.5ka BP in the relative abundance of *N. pachyderma* (sinistral) “blends into” the background variability, i.e. you can see similarly large changes later on in the Holocene (Fig. 4A), while the 8.5ka BP maximum clearly stands out in the $\delta^{18}\text{O}_{\text{NPS}}$ time series (Fig. 4B). In other words the *N. pachyderma* (sinistral) abundance record does not support the idea

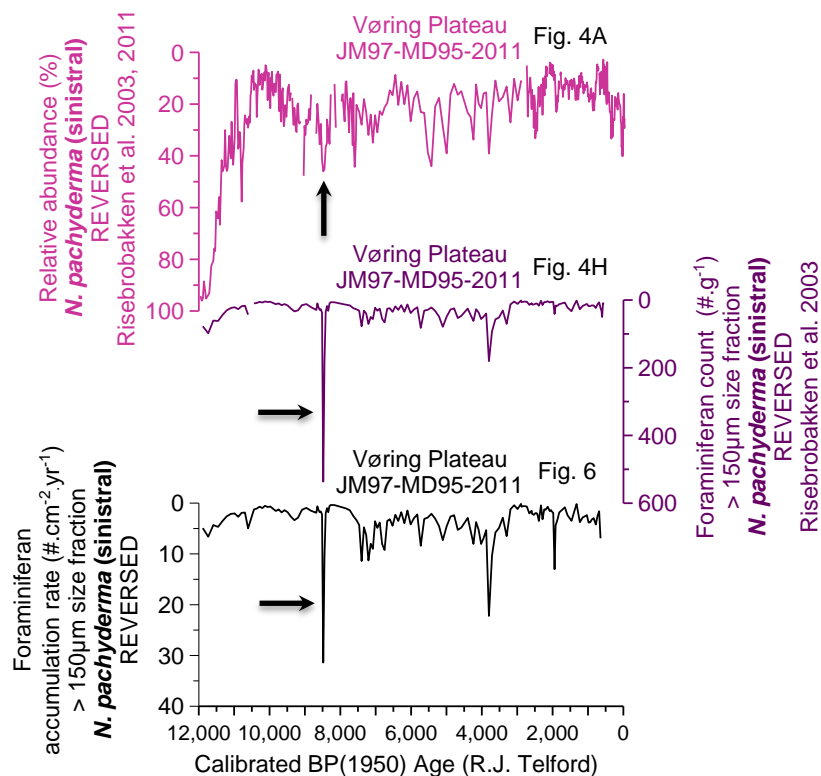
that a UNIQUELY SEVERE cold event affected the mid-Norwegian Margin at around 8,500 years ago.



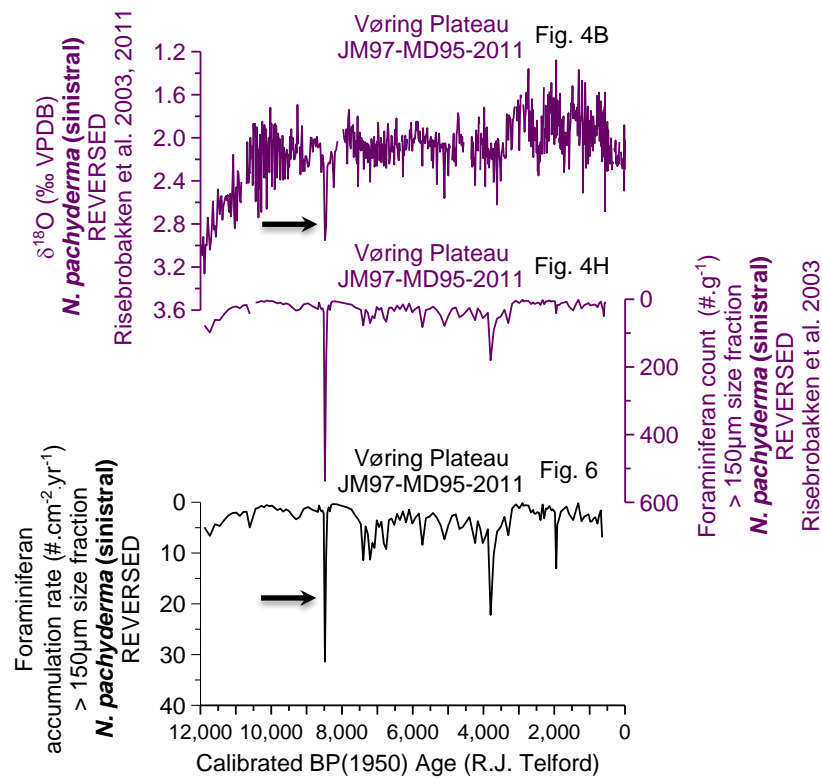
It must be noted that the proxy in Fig. 3D and Fig. 4A is called the RELATIVE abundance of *N. pachyderma* (sinistral), which means that the signal is modulated by changes in the productivity of other species present in the planktonic foraminiferal assemblage. It is also influenced by the species composition of the assemblage, which may be different in the subpolar North Atlantic (i.e. that serves as a basis for the time series in Ellison et al., 2006) and along the mid-Norwegian Margin in the Norwegian Sea (i.e. at our site). These species may have different depth habitats, reproductive season, adaptiveness, etc. and this may affect the sensitivity of the *N. pachyderma* (sinistral) abundance proxy to changes in temperature.

This is not to say that the differences between the two *N. pachyderma* sinistral abundance time series from MD99-2251 (Ellison et al., 2006) and JM97-MD95-2011 (discussed in our paper) cannot reflect concurrent conditions at around the 8.2ka Event at these two locations.

However, consider the example below (now focusing on our site only):



When looking at the absolute concentrations of the POLAR species *N. pachyderma* (sinistral) in the >150µm fraction of the sediment these values show a uniquely large maximum at approximately 8,500 years BP (Fig. 4H). When taking sedimentation rates at our site into consideration the 8.5ka peak becomes somewhat muted, but remains a prominent feature of the signal. This 8.5ka maximum is in perfect synchronicity with the large positive excursion in $\delta^{18}\text{O}_{\text{NPS}}$ values in Fig. 4B:



We will improve the relevant section in our paper.

Reviewer Comment Page 674. This difference between coarse and fine fraction relative timing could be accounted for by bioturbation.

Author Response Please see our detailed response earlier.

Reviewer Comment Page 677 line 4. Check salinity subpolar gyre reconstructions from [Thornalley et al., 2009]. Also check [Bamberg et al., 2010].

Author Response We are familiar with both papers and the related datasets.

Regarding Bamberg et al., 2010:

As described earlier our strategy in reviewing the published literature was to look for uniquely large signals in the second half of the 9th millennium BP with respect to background “post-deglacial” variability at the respective sites. Therefore, we primarily worked with datasets, which covered the entire Holocene with reasonable resolution:

1. Where a time series indicates a change in local conditions coincident with the collapse of the ice dam over Hudson Bay and/or the cooling over Greenland, but the same record also suggests similarly large variations later on in the Holocene at that particular location (see, for example, the relative abundance of *N. pachyderma* sinistral time series from our site) it is very difficult to argue convincingly that the climate perturbation around 8,500 years BP at that location has special significance, even if it could theoretically be linked to the lake outbursts and iceberg discharge from Hudson Bay.

- Where only the period bracketing the 8.2ka Event is covered and we do not know anything about how the rest of that record would look like it is very difficult to interpret the data we do have (even if we translate these proxies into absolute values of e.g. temperature or salinity, because the errors in these estimates are normally fairly large).

The data of Bamberg et al. (2010) together with the $\delta^{18}\text{O}_{\text{NPS}}$ record from our site (Risebrobakken et al., 2011) are plotted below (Fig. 7). Column A shows the entire Holocene [i.e. the relative shortness of the period covered by the data of Bamberg et al. (2010)], while Column B zooms in on the interval bracketing the 8.2ka Event. In Column B the two intermediate-water cooling and freshening events off Morocco are highlighted in grey. We also indicated the age-model uncertainties in both JM97-MD95-2011 and GeoB6007-2.

Subpolar Mode Waters (SPMW), the precursors of Eastern North Atlantic Central Waters (ENACW), form south of Iceland and east of 25°W. Since this is an area influenced by both the AMOC and the SPG our theory can provide a plausible alternative to the one put forward by Bamberg et al. (2010) to explain the two ocean cooling and freshening events (at 8.54 ± 0.2 ka BP and 8.24 ± 0.1 ka BP) at intermediate depths off the coast of Morocco. However, especially the second intermediate-water freshening event would need to be tested in the context of our theory in high-resolution modelling studies: i.e. whether a higher export of drift ice into the northern North Atlantic, along with concomitant changes in atmospheric forcing and the strength and position of the SPG and the Subpolar Front could have a detectable impact off the coast of Morocco in agreement with the reconstructions of Bamberg et al. (2010).

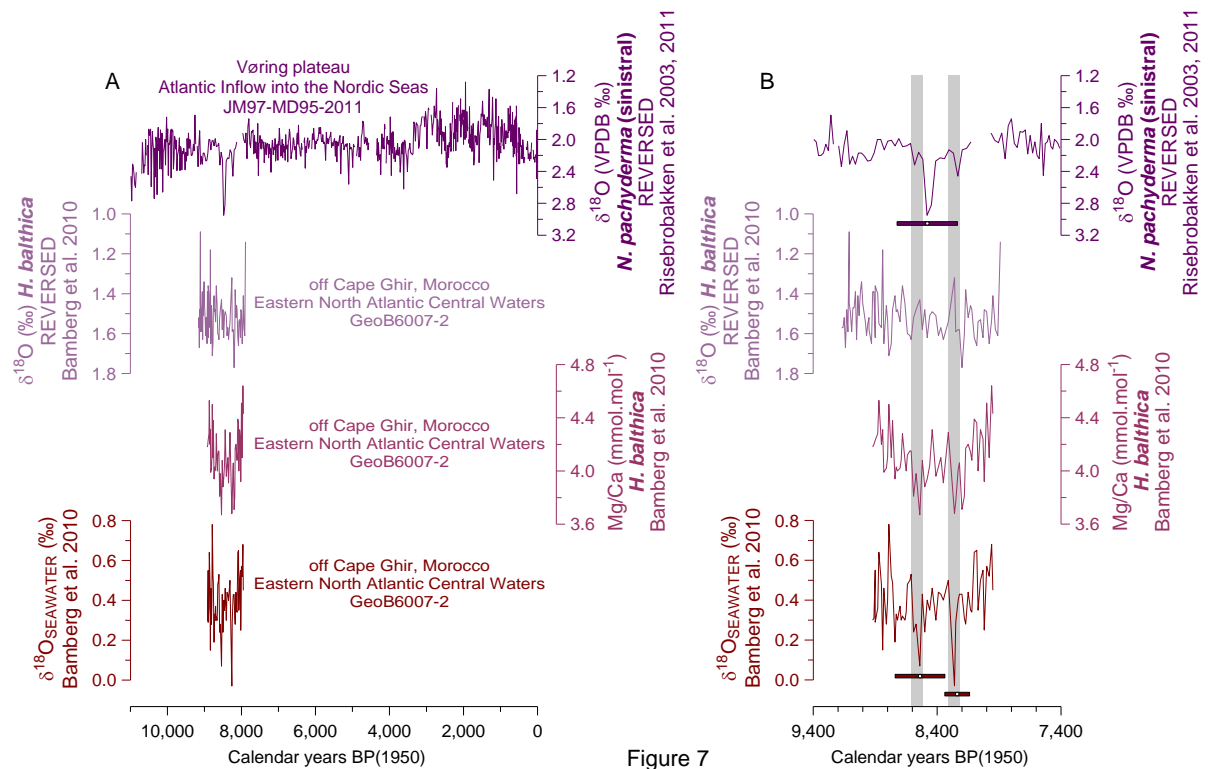


Figure 7

As to Thornalley et al., 2009:

The only relatively prominent feature of the records of Thornalley et al. (2009) with respect to the 8.2ka Event may be the decrease in $\delta^{18}\text{O}_{\text{G. INFLATA}}$ values between 8,722 years BP (?) and 8,216 years BP, which seems to have resulted from a reduction in sub-thermocline ocean salinities (indicated by the black arrows in Fig. 8 below). However, this is not mirrored by reconstructed near-surface salinities. Also, because this relatively “abrupt” change is sitting on the back of a longer-term trend (indicated by the lilac arrows) it is difficult to assess its true magnitude.

The authors note that “in response to surface freshening, it is possible that *G. bulloides* migrated to a deeper, more saline environment and that the freshening [in near-surface waters] is underestimated.”

The inconclusiveness of the data with respect to the aftermath of the collapse of the ice dam over Hudson Bay at around 8,500 years ago (e.g. the lack of abrupt cold events in these records, see the Mg/Ca ratios in Fig. 8) may also be due to the peculiar location of this core site. As the authors explain it “lies under the path of the [North Atlantic Current] where it bifurcates to form the Irminger and Faroe currents, although instrumental and historical records also document episodes of cold ice-bearing subpolar waters from the north reaching the site.” Therefore, this location may be sensitive to shifts in the oceanic Subpolar Front. If and when it is affected by the North Atlantic Drift it lies underneath its western branch, which continues in the NwAFC (the western branch of the Atlantic Inflow) north of the Greenland-Scotland Ridge, where there may be more mixing between warmer and saline Atlantic waters and colder and fresher subpolar waters in comparison with the eastern North Atlantic Drift, which continues in the NwASC north of the Greenland-Scotland Ridge.

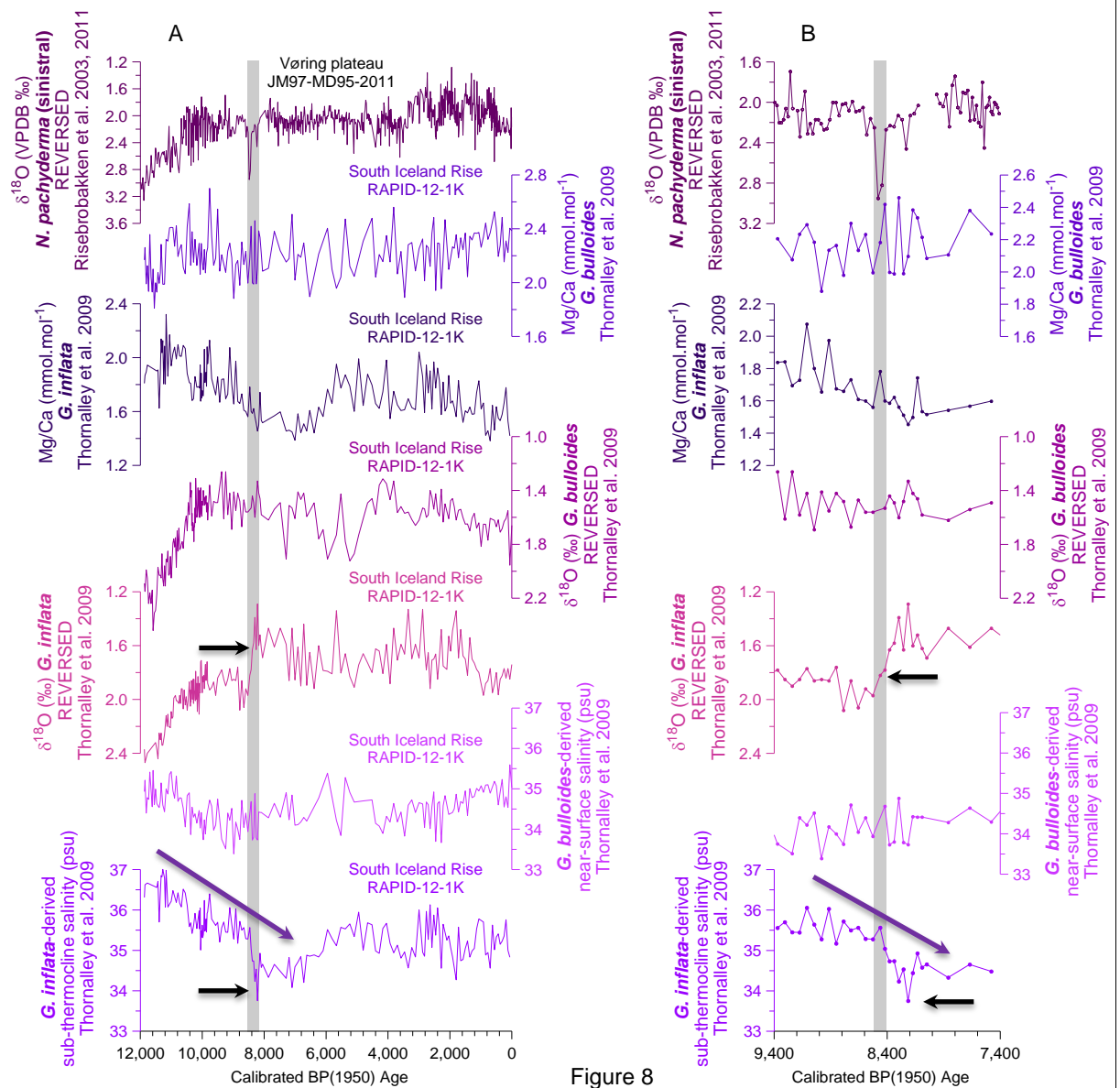


Figure 8

Also note that the time series of Thornalley et al. (2009) are somewhat lower resolution than the $\delta^{18}\text{O}_{\text{NPS}}$ record of Risebrobakken et al. (2011).

FIGURES	
Reviewer Comment	Fig1. Figure1A and 1B could be merged into one figure.
Author Response	Fig. 1A describes the (sub)surface current system, while Fig. 1B gives you the core sites. It would have been very difficult to cram all this information onto a single map.
Reviewer Comment	Fig 2. It would help if the raw data was presented here as opposed to the normalised records. The raw SS values are also useful as the larger the grain sizes and their larger magnitude of variability the measurements will be less prone to error.
Author Response	<p>The second sentence of this comment is not entirely clear.</p> <p>Graphically, the “raw” sortable-silt data looks identical to the one presented in the paper. That is why we considered the former redundant. They are, however, included in Tegzes 2013 and we can send them to the Reviewer.</p> <p>The error in the sortable-silt data is a complex issue and is one of the main reasons why we suggested using “true mean grain size” as opposed to the sortable-silt index in the first place. This is discussed in detail in Tegzes 2013 (and Tegzes et al., 2014b). However, the latter has not yet been published.</p> <p>As to the error in the measurements: each sample has been run several times on the Coulter Counter. We calculated an average value for each sample. The solid lines connect these averages. The shaded envelopes around the solid lines indicate the actual spread of values from repeated measurements. These give you an idea of the reproducibility of the individual values and also of the robustness of the trend along the core (i.e. through time).</p>
Reviewer Comment	Fig.3 and 4 are very complex and small. The colour scheme and the reduced size of the graphs makes it difficult to read them. Would it be possible to enlarge these figures and make the plots in black and white? Are all of the plots needed?
Author Response	<p>We are not happy with the way these graphs came out in the discussion slideshow either.</p> <p>Both figures were submitted to CP as high-resolution (300dpi) TIFF images. Their dimensions had been set to match a standard print journal (Fig. 3: 76-230mm, Fig. 4: 104-230mm). When printed in their original size on e.g. an A4 paper they are perfectly legible. Clearly, the slideshow layout cannot accommodate more complex figures (the navigational buttons on the right hand side also take up a lot of space).</p> <p>We suggested a number of layouts during typesetting, which would have improved the appearance of these figures, but we were told that for some reason these were technically not possible. Since on-screen these images can be readily enlarged and we did not want to waste more time on technicalities, we eventually nodded to the version, which you can now view online. We will see how these graphs look like in the final publication and will make the necessary changes, including a new colour scheme.</p> <p>Fig. 3 summarises all records discussed in the text. Here our intention was to show the relative timing of events, dating uncertainties included.</p> <p>In Fig. 4 we wanted to show the uniqueness of the event at around 8,500 years BP as represented by the various parameters measured on sediment cores JM97-948/2A and MD95-2011. We also wanted to present all records at our disposal to the reader to help judge the nature of this unique sedimentary horizon: i.e. whether this could have been produced by anything other than a climate event. This graph may be simplified.</p>

To use space efficiently we had to “pack” the graphs as closely as possible. Thus using a colour scheme (as opposed using black only) to match the graphs with their respective axes was inevitable. Because of the large number of graphs it is very difficult to come up with an acceptable colour scheme, but we will try to improve the figures in this respect too.

REFERENCES

- Bamberg, A., Rosenthal, Y., Paul, A., Heslop, D., Mulitza, S., Rühlemann, C., and Schulz, M.: Reduced North Atlantic Central Water formation in response to early Holocene ice-sheet melting, *Geophysical Research Letters*, 37, L17705, 2010.
- Barber, D. C., Dyke, A., Hillaire-Marcel, C., Jennings, A. E., Andrews, J. T., Kerwin, M. W., Bilodeau, G., McNeely, R., Southon, J., Morehead, M. D., and Gagnon, J. M.: Forcing of the cold event of 8,200 years ago by catastrophic drainage of Laurentide lakes, *Nature*, 400, 344-348, 1999.
- Bond, G., Kromer, B., Beer, J., Muscheler, R., Evans, M. N., Showers, W., Hoffmann, S., Lotti-Bond, R., Hajdas, I., and Bonani, G.: Persistent Solar Influence on North Atlantic Climate During the Holocene, *Science*, 294, 2130-2136, 2001.
- Condron, A. and Winsor, P.: A subtropical fate awaited freshwater discharged from glacial Lake Agassiz, *Geophys. Res. Lett.*, 38, L03705, 2011.
- Eldevik, T., Nilsen, J. E. O., Iovino, D., Anders Olsson, K., Sando, A. B., and Drange, H.: Observed sources and variability of Nordic seas overflow, *Nature Geosci*, 2, 406-410, 2009.
- Ellison, C. R. W., Chapman, M. R., and Hall, I. R.: Surface and Deep Ocean Interactions During the Cold Climate Event 8200 Years Ago, *Science*, 312, 1929-1932, 2006.
- Hoogakker, B. A. A., Chapman, M. R., McCave, I. N., Hillaire-Marcel, C., Ellison, C. R. W., Hall, I. R., and Telford, R. J.: Dynamics of North Atlantic Deep Water masses during the Holocene, *Paleoceanography*, 26, PA4214, 2011.
- Kleiven, H. F., Kissel, C., Laj, C., Ninnemann, U. S., Richter, T. O., and Cortijo, E.: Reduced North Atlantic Deep Water Coeval with the Glacial Lake Agassiz Freshwater Outburst, *Science*, 319, 60-64, 2008.
- Kučera, M.: Biochronology of the mid-Pliocene *Sphaeroidinella* event, *Marine Micropaleontology*, 35, 1-16, 1998.
- LeGrande, A. N., Schmidt, G. A., Shindell, D. T., Field, C. V., Miller, R. L., Koch, D. M., Faluvegi, G., and Hoffmann, G.: Consistent simulations of multiple proxy responses to an abrupt climate change event, *Proceedings of the National Academy of Sciences of the United States of America*, 103, 837-842, 2006.
- McCave, I. N. and Hall, I. R.: Size sorting in marine muds: Processes, pitfalls, and prospects for paleoflow-speed proxies, *Geochem. Geophys. Geosyst.*, 7, Q10N05, 2006.
- Mork, K. A. and Skagseth, Ø.: A quantitative description of the Norwegian Atlantic Current by combining altimetry and hydrography, *Ocean Sci.*, 6, 901-911, 2010.
- Moros, M., Emeis, K., Risebrobakken, B. r., Snowball, I., Kuijpers, A., McManus, J., and Jansen, E.: Sea surface temperatures and ice rafting in the Holocene North Atlantic: climate influences on northern Europe and Greenland, *Quaternary Science Reviews*, 23, 2113-2126, 2004.
- Orvik, K. A. and Skagseth, Ø.: Heat flux variations in the eastern Norwegian Atlantic Current toward the Arctic from moored instruments, 1995-2005, *Geophys. Res. Lett.*, 32, L14610, 2005.
- Orvik, K. A. and Skagseth, Ø.: The impact of the wind stress curl in the North Atlantic on the Atlantic inflow to the Norwegian Sea toward the Arctic, *Geophys. Res. Lett.*, 30, 1884, 2003.
- Renssen, H., Goosse, H., Fichfet, T., and Campin, J.-M.: The 8.2 kyr BP event simulated by a Global Atmosphere–Sea-Ice–Ocean Model, *Geophys. Res. Lett.*, 28, 1567-1570, 2001.
- Risebrobakken, B., Dokken, T., Smedsrud, L. H., Andersson, C., Jansen, E., Moros, M., and Ivanova, E. V.: Early Holocene temperature variability in the Nordic Seas: The role of oceanic heat advection versus changes in orbital forcing, *Paleoceanography*, 26, PA4206, 2011.
- Risebrobakken, B., Jansen, E., Andersson, C., Mjelde, E., and Hevrøy, K.: A high-resolution study of Holocene paleoclimatic and paleoceanographic changes in the Nordic Seas, *Paleoceanography*, 18, 1017, 2003.
- Smedsrud, L. H., Sirevaag, A., Kloster, K., Sorteberg, A., and Sandven, S.: Recent wind driven high sea ice area export in the Fram Strait contributes to Arctic sea ice decline, *The Cryosphere*, 5, 821-829, 2011.

- Tegzes, A. D.: The main branch of the Atlantic Inflow into the Nordic Seas over the Holocene: an analysis of its longer-term behaviour and its potential impact on climate, 2013. Doctoral dissertation, University of Bergen, Bergen, 67 pp., 2013.
- Tegzes, A. D., Jansen, E., and Telford, R. J.: Reconstructing variations in the strength of the main branch of the Norwegian Atlantic Current over the Late Holocene using grain-size parameters, *Paleoceanography*, resubmitted, 2014a.
- Tegzes, A. D., Jansen, E., and Telford, R. J.: The traditionally used sortable-silt index or true mean grain size is the better proxy for palaeo-current strength?, in revision, 2014b.
- Thornalley, D. J. R., Blaschek, M., Davies, F. J., Praetorius, S., Oppo, D. W., McManus, J. F., Hall, I. R., Kleiven, H., Renssen, H., and McCave, I. N.: Long-term variations in Iceland–Scotland overflow strength during the Holocene, *Clim. Past*, 9, 2073-2084, 2013.
- Thornalley, D. J. R., Elderfield, H., and McCave, I. N.: Holocene oscillations in temperature and salinity of the surface subpolar North Atlantic, *Nature*, 457, 711-714, 2009.
- van Angelen, J. H., van den Broeke, M. R., and Kwok, R.: The Greenland Sea Jet: A mechanism for wind-driven sea ice export through Fram Strait, *Geophysical Research Letters*, 38, L12805, 2011.
- Wiersma, A. P. and Jongma, J. I.: A role for icebergs in the 8.2 ka climate event, *Climate Dynamics*, 35, 535-549, 2010.
- Wynn, E. J. W. and Hounslow, M. J.: Coincidence correction for electrical-zone (Coulter-counter) particle size analysers, *Powder Technology*, 93, 163-175, 1997.