Editor Comment #1	The question of whether the sedimentation at the study location has actually been affected by the hydrographic current under study (the manuscript states that it likely was not, and your response seems to suggest that this subject is beyond the scope of the current study, having been dealt with in a doctoral thesis);
Author Response	We have summarised our findings regarding the oceanography of and sedimentary processes at our site in AC C486 "Response to Reviewers – Updated" (p1) posted on 15-05-2014. We have also already included this brief summary into the revised manuscript. The original version of this discussion can be found in a doctoral dissertation (Tegzes, 2013), published by the University of Bergen. However, we have also submitted a paper, referred to as Tegzes et al., 2014a in the CP discussion, to Paleoceanography, which contains the complete detailed analysis of the oceanography of and the sedimentary processes at our site. Since we need to cite Tegzes et al., 2014a in our paper on the 8.2ka Event, we will have to delay the re-submission of the latter until after Tegzes et al., 2014a has been accepted for publication.
Editor Comment #2	The questionable applicability of the term 'true mean grain size' and the proposed use of 'number mean' and 'volume mean' as a clearer means of distinction, as well as a full discussion of the proposed new grain-size index and its proposed superiority (e.g. "less subject to random effects"; but what random effects?);
Author Response	As we have emphasised in our response to reviewers our intention with the term "true mean grain size" was to convey a concept, an approach to describing the coarseness of the 10-63μm terrigenous silt fraction. In response to reviewer comments we have already noted that we will use "sortable-silt mean grain diameter" in the revised manuscript. Please see the relevant section in our response to reviewers: therein you can find a detailed discussion complete with a mathematical description. The full discussion of "sortable-silt mean grain diameter ($\bar{d}_{SS}$)" versus "sortable-silt mean size ($\overline{SS}$)" far exceeds the scope of our paper on the 8.2ka Event. Therefore, similarly to the oceanography of our site, the only possible solution is to include a brief summary of the nature of $\bar{d}_{SS}$ versus $\overline{SS}$ in our paper on the 8.2ka Event and cite our paper on the sortable-silt proxy (Tegzes et al., 2014b). As Tegzes et al., 2014b is currently in revision, we will have to delay the re-submission of our paper on the 8.2ka Event until after Tegzes et al., 2014b has also been accepted for publication. We are aware that we have touched upon a very sensitive issue: if our findings from this specific site have general validity, this would question the standard way of documenting grain-size changes, applied in a large number of already published sortable-silt time series. That is the main reason why we decided to publish our findings as a "case study," i.e. before conducting an extensive survey of sediment cores from different geographical settings and time intervals. The raw Coulter Counter output (i.e. the pulse data) is often discarded straight after measurement because it takes up a lot of disc space. Without the pulse data it is no longer possible to check whether $\bar{d}_{SS}$ values would reflect the same pattern of change in the coarseness of the sortable-silt fraction as the already published $\overline{SS}$ time series. However, by drawing attention to the possible discrepancy between $\bar{d}_{SS}$ and $\overline{SS}$ colleagues who conduct such analyses in the future will be aware of this and they may either simply save the pulse data for future use or calculate $\bar{d}_{SS}$ themselves and see how the two records compare. Such research is very costly and time consuming (not to mention that the availability of suitable samples may be very limited). Therefore, even if we do not have the definitive answer, we consider it important to make the scientific community aware of these results.

<table>
<tr><td></td><td>As to the remark '"less subject to random effects"; but what random effects?' …

We have already responded to this reviewer comment, citing two examples. Please see the relevant section in

AC C486 "Response to Reviewers – Updated" (p9) posted on 15-05-2014.

Please also see our response to "Editor Comment #3" below.</td></tr>
<tr><td>Editor Comment #3</td><td>The arguable robustness of the proposed lag between only one of the grain size indices (the one that is proposed in the manuscript to be more subject to 'random effects') versus the N.pachyderma(s) counts/isotopes and the IRD, which occurs over only 1 cm (i.e. one measurement interval) and therefore is subject to the reproducibility (i.e. by replicate raw measurements) of a single grain-size measurement (cf. the N.pachyderma isotope/counts and IRD data, which are clearly argued in your response to constrain and replicate the timing of surface hydrography changes in the core);</td></tr>
<tr><td>Author Response</td><td>The above comment introduces a new angle into the discussion. So here is some clarification:

The original reviewer comments concerned bioturbation.

Reviewer #1 noted that

"… 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 $\delta^{18}O$ shift appear earlier (e.g. Trauth, 2013 and refs therein)."

Reviewer #2 noted that

"The apparent difference in the relative timing between the SS and $\delta^{18}O$ from N. pachyderma (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 <63µm and $\delta^{18}O$ Nps in the >63µm 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)."

This issue we have dealt with in detail in

AC C486 "Response to Reviewers – Updated" (p9) posted on 15-05-2014.

We had originally discussed the problem of "random effects" in relation to the robustness of "sortable-silt mean grain diameter ($\bar{d}_{SS}$)" versus "sortable-silt mean size ($\overline{SS}$)."

We must emphasise again that the comparison of these proxies ($\bar{d}_{SS}$ and $\overline{SS}$) are based on samples from our site only. Therefore, the results should be considered as those of a "case study." However, they are fully valid for answering the Editor's comment.

The sortable-silt theory states that ocean currents can rarely move grains larger than 63µm in diameter.

Grains smaller than 10µm (i.e. fine silt and clay) are cohesive, they tend to stick together, therefore on the ocean floor the current moves them in groups. Since we can analyse samples with scientific rigour only in their disaggregated state in the laboratory the inclusion of the < 10µm fraction would only distort our results.

Thus it is the coarseness of the 10-63µm terrigenous silt fraction, which can give us information about the past strength of the depositing current. The question is how can we capture an entire distribution using a single number? How can we best approximate the</td></tr>
</table>

coarseness of this size fraction?

Fig. A and Fig. B below show the characteristic differential NUMBER distribution of grains within the 10-63µm terrigenous silt fraction in samples from the 9,500-7,500 years BP time interval. The step plots in the two figures are identical. They have been simplified for the sake of illustration: we used only 11 size bins instead of the original 256 (see "Response to Reviewers – Updated"). The vertical bars show the differential NUMBER $\left(\frac{N_i}{N}\right)$ of grains that fall into the individual size bins. The dark grey "caps" on top of the bars indicate the range of values for each size bin considering all samples covering the 9,500-7,500 years BP time interval.

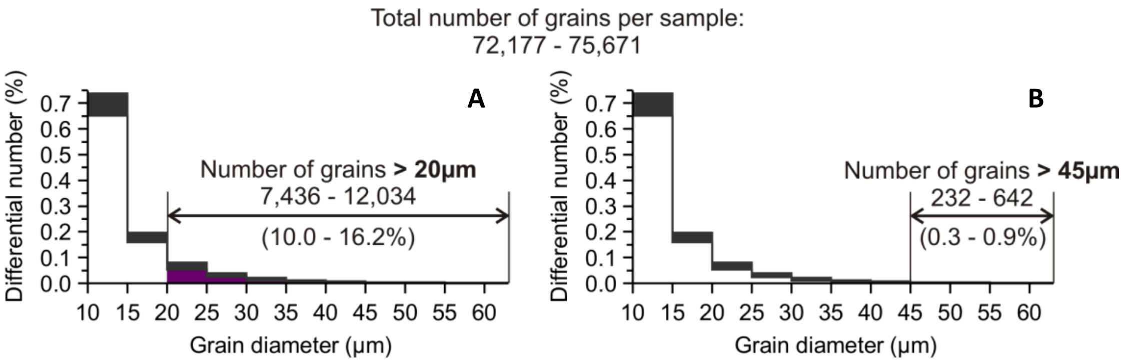
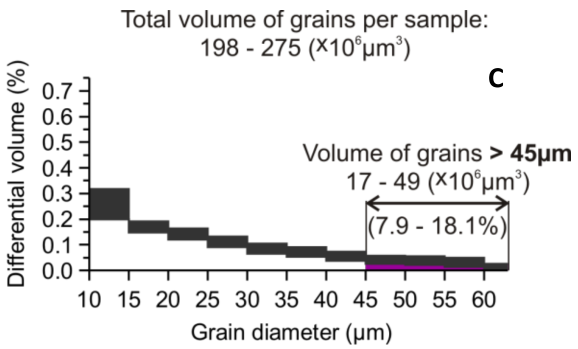


Fig. A shows that the relative number of grains with d > 20µm varies between 10-16.2% of the 10-63µm terrigenous silt fraction over the interval of interest.

Fig. B shows that the relative number of grains with d > 45µm varies between 0.3-0.9% of the 10-63µm terrigenous silt fraction over the interval of interest.



$\overline{SS}$ most strongly correlates (r = 0.98) with the number-percent-based enrichment of grains > 40-45µm (representing < 1.5% of the sortable-silt fraction, i.e. a couple of hundred grains out of the more than 70,000 measured per sample), while $\bar{d}_{SS}$ most strongly correlates (r = 0.99) with the number-percent-based enrichment of grains > 20µm (representing 10-16.2% of the sortable-silt fraction) (Tegzes, 2013; Tegzes et al., 2014b).

Even intuitively it is easy to see why: those 232-642 grains (see Fig. B above) that are larger than > 45µm volumetrically account for 7.9-18.1% of the sortable-silt fraction in samples covering the 9,500-7,500 years BP interval (see Fig. C). Thus even small changes in their number can lead to large changes in $\overline{SS}$.

In Fig. C we used the same 11 size bins as we used for Fig. A and B. However, here we plotted the total VOLUME of the grains that fall into the individual size bins instead of their number.

In general, the finer the sortable-silt fraction, the larger the impact of small changes in the relative number of the largest grains (those with d > 45µm) on $\overline{SS}$. Therefore, while the discrepancy is more subtle between the $\bar{d}_{SS}$ and $\overline{SS}$ record over the Late Holocene (see Fig. 2 in our paper), the difference between the two time series is more pronounced over the Early Holocene.

When we referred to "random effects" we had two issues in mind.

1) The current is not a steady, uniform flow. Its velocity (i.e. both the flow direction and

speed) varies greatly through time on all time scales (from hours to millennia).

When we are reconstructing the past strength of a current one sampling interval normally represents a decade or century. We think that the flow-strength proxy should be such that it approximates the mean current (the “characteristic” strength of the current) over the sampling time interval. With $\overline{SS}$ being so sensitive to the relative number of a “few” large grains within the sample (at least in our case), even a single brief interval of very strong flow (which can transport larger grains to our site) within the decade or century in question may significantly increase the corresponding value of $\overline{SS}$, which would thus give a false impression of the overall strength of the current during that time interval.

Even though $\bar{d}_{SS}$ is a number with a physical meaning (it approximates the actual physical size of the grains within the 10-63µm terrigenous silt fraction), due to the logarithmic nature of the grain-size distribution (see Fig. A and B above), it is also biased towards the coarse tail. However, because it relies on a larger percentage of the grains within the sample, we think that it better approximates the mean current than $\overline{SS}$.

2) The other issue concerns laboratory work: sample preparation and measurements.

Since these samples are not processed in a clean laboratory sample contamination can be a potential problem.

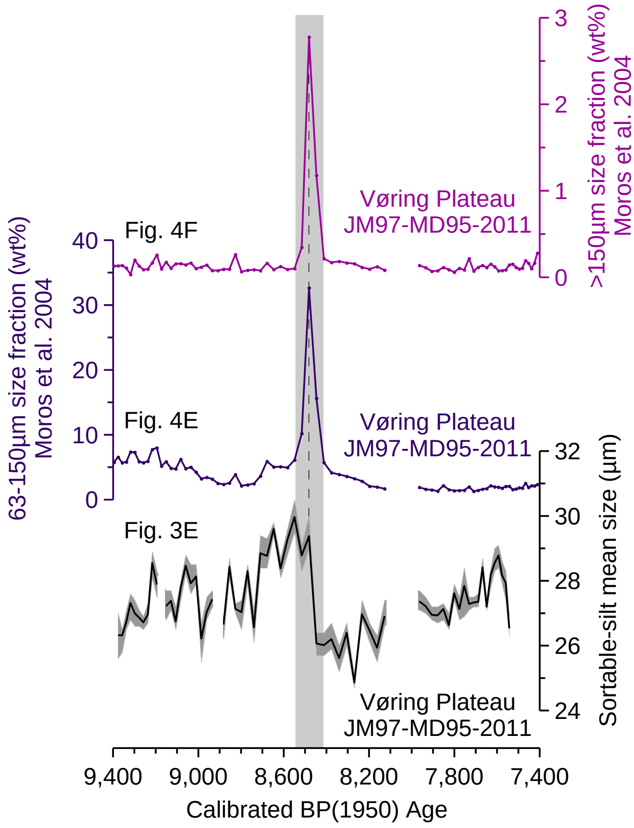
Coincidence (the erroneous detection of larger grains by the Coulter Counter) may also affect $\overline{SS}$ more than $\bar{d}_{SS}$. Although, we tried to minimise the influence of this phenomenon by rerunning samples several times and at different concentrations on the Coulter Counter. For a detailed discussion of what coincidence is see e.g. Wynn and Hounslow, 1997.

Returning to the yet undiscussed issue raised by the Editor:

As far as the coarseness of the sediment is concerned, we can only compare those proxies directly, which are mathematically calculated the same way, i.e.

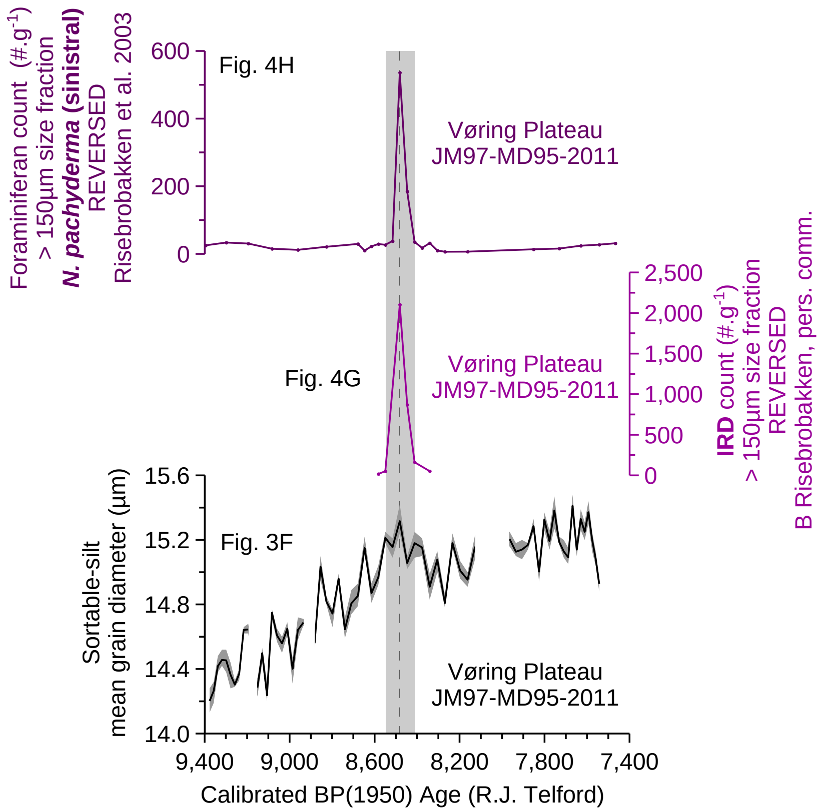
We can compare
- (a) $\overline{SS}$, representing the coarseness of the 10-63µm terrigenous silt fraction (Fig. 3E), with
- (b) the weight-percent of the 63-150µm size fraction (Fig. 4E), and with
- (c) the weight-percent of the >150µm size fraction (Fig. 4F).

(a), (b) and (c) are all based on the differential volume/weight distribution of grains within the sample.



We can compare
- (e) $\bar{d}_{SS}$, representing the coarseness of the 10-63µm terrigenous silt fraction (Fig. 3F), with
- (f) IRD counts in the >150µm size fraction (in #.g^{-1}) (Fig. 4G), and with
- (g) foraminiferan counts in the >150µm size fraction (in #.g^{-1}) (Fig. 4H).

(e), (f) and (g) are all based on the number count of grains within the sample.



Considering the logarithmic nature of the differential number distribution of grains within our samples and the increasingly disproportionate weight (in mathematical terms) of the size bins towards the coarse tail of the differential volume distribution, our choice of statistics can greatly influence:

- the bias of the apparent coarseness of the sediment from its "actual" coarseness,
- the impact of small changes in the number of the largest grains on the amplitude of variability in the apparent coarseness of the sediment along the core.

Therefore, the apparent relative timing of events in palaeoreconstructions does not only depend on past climate, bioturbation or the re-deposition of allochthonous sediments, but also on how we calculate our proxies, the equations and statistics we use. Note that Fig. 3E and 3F are based on the same set of measurements.

Our view is that the differential-number-weighted $\bar{d}_{SS}$ record better approximates the actual changes in the coarseness of the 10-63µm terrigenous silt fraction than $\overline{SS}$. Thus the relationship between the >150µm size fraction (Fig. 4G and 4H) and the current-sorted 10-63µm terrigenous silt fraction ($\bar{d}_{SS}$) (Fig. 3F) does not depend on a single sampling interval. The pattern of change is totally different in the two size fractions. There is no abrupt drop in Fig. 3F, only a gradual decline across six sampling intervals, where $\bar{d}_{SS}$ reaches a minimum.

The reproducibility of individual values is indicated by the grey envelope around both the $\overline{SS}$ (Fig. 3E) and $\bar{d}_{SS}$ (Fig. 3F) records. These indicate the actual spread of values resulting from repeated measurements.

In addition, the more gradual slowing down of the current, as suggested by $\bar{d}_{SS}$, is more in line with our theory, then the abrupt drop in the $\overline{SS}$ time series:

Taking a simplistic approach (and focusing only on the thermohaline driving mechanism) the weakening of the NwASC could ultimately be the consequence of the expansion (and

	thickening) of sea ice over a longer period of time in the wake of the collapse of the ice dam over Hudson Bay. While initially brine rejection during sea-ice formation at high-latitudes could even compensate for the freshening of the NwASC, as sea ice increasingly insulated the ocean from the atmosphere it could hinder deep convection in the Arctic (Zhong et al., 2011).
Editor Comment #4	The clarity (legibility) of the figures (regardless of their digital resolution, these will typically be printed on A4 paper and not blown up to larger sizes).
	As we have explained before all figures submitted to CP had been designed to fit the size of standard print journals and they fit perfectly on an A4 page. High resolution only means that in their original size (i.e. on an A4 page) the maps and the graphs all have very sharp contours. It is just an aside that the resolution of the images is much higher than it would be necessary for an A4 print. This is why we said that these images can be readily blown up to much larger sizes (than A4) on screen. However, all of our figures were “squeezed” onto the CP discussion slides (which have different dimensions from an A4 page). When one prints a CP discussion slideshow two slides are printed onto an A4 page where the margins are very wide. This makes the figures even smaller. In hindsight we should have edited our figures ourselves and submitted them in their original size and layout to CP as a supplement. We have already acknowledged in our response to reviewers that the colour scheme of our graphs could be improved. We will also try to simplify the figures or where possible present them in a different layout in the revised manuscript.
Editor Comment	… in my view [comments 1-4] remain important issues that must be addressed (i.e. incorporated) in a revised manuscript, rather than being dismissed summarily.
Author Response	We think that our detailed response to reviewers (as the Editor himself noted) is evidence enough that we do not want to “summarily dismiss” the above issues. The whole objective of the peer-review process is to improve the scientific content of the paper and the presentation thereof, so that the final version addresses all relevant issues in a way that is clear and understandable to the scientific community. Naturally, as indicated in our response to reviewers, our intention is to incorporate the conclusions of the peer-review process into the revised manuscript.
Editor Comment	Where supporting discussion is required (e.g. on methodologies, hydrographic interpretations etc.), and especially where raw data are concerned, these must either be presented fully in the manuscript or else summarised briefly and in reference to an already published peer-reviewed source. If there is information that does not need to be presented in the current manuscript (e.g. a full analysis of a new methodology), because it is too lengthy and is to be published elsewhere, then it will be necessary to await publication of that material prior to re-submission of your manuscript.
Author Response	As we have indicated before we can only include a brief summary of the relevant sections of Tegzes et al., 2014a and Tegzes et al., 2014b in our paper on the 8.2ka Event. Therefore, we will have to wait until these other two papers are accepted for publication, before we can submit a revised manuscript to CP.

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