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Interactive comment on "The role of the northward-directed (sub)surface limb of the Atlantic Meridional Overturning Circulation during the 8.2 ka Event" *by* A. D. Tegzes et al.

Anonymous Referee #1

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A. D. Tegzes, E. Jansen, and R. J. Telford; The role of the northward-directed (sub)surface limb of the Atlantic Meridional Overturning Circulation during the 8.2 ka Event. Climate of the Past Discussions, 10, 665–687, (2014). The authors have generated a data set of interest on an important flow component of the Atlantic MOC. They need to convince us that the process of sedimentation is controlled by the current they purport to study because the basis for grainsize 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 (highaccumulation 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 subse-

quent 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 880 m depth in 1001 m of water (the authors' core is at 1048 m) has flow to 229o - 213o . The business of whether number or weight (volume) frequency is more significant is very old (Krumbein & Pettijohn, 1938, p. 227). Prior to particle counters the question arose in relating size distribution from thin section counts to those from sieving modern sands (Sahu, 1964). In none of these cases are the data 'pure' in the sense of using the raw size data because they are normally grouped into classes of (usually logarithmically) increasing size. The size classes are set up in a logarithmic progression with di the log mid-point diameter of the ith bin by most Coulter Counter operators because frequency distributions are not linear. Here the authors do not state whether the data were in classes and if so whether an arithmetic or logarithmic progression was adopted. In the Coulter counter the particle size is not measured but is inferred as volume from the amplitudes of the voltage pulses measured by the instrument. These result from impedance changes due to volume displacement of electrolyte by particles. The sizes are thus equivalent spherical diameters back-calculated from volume. Although a statistic based on particle number (ni) data may be closer to the original measurement by this method than one on particle volume (ïĆ ţ nidi3), 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. "Sortable Silt mean size" ( ) cuts out the <10 μm 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). Normalisation may be necessary to compare number–based data with other data, but the actual size is important. 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). The volume–based sortable silt mean size on Fig 3 is thus most useful for comparison. 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. This is worth note and analysis of possible causes. The authors need to be cautious about saying "… we cannot invoke age-model uncertainties …" (p. 671, line 6-8) because the δ18O drop is recorded just a cm or so below the drop, but the abundance of N. pachyderma increases by $\sim$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 δ18O 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.

Line Comments Line p. 666 3 not 'over' but 'in' 11 should read not site but "… cores JM97-248/2A and MD95-2011 on …" 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? p. 667 2 also Vinther et al., 2006 (JGR, 111) for Holocene ice age. p. 668 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. 15/16 p. 669 12 The term "sortable silt index" is not used (not

in WOS database). "Sortable silt mean size" ( ) and sortable silt percentage (SS%) are used as indices of current strength in the present authors' and other labs (Kleiven et al., 2011). 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? 16 "Random effects" is rather cryptic; explain. 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? 25 Better if the 'replace all' command were used to put 'number' instead of 'true', [and 'volume-mean' for 'index']. p. 670 3 The fluctuations in SS volume mean do not appear much larger than those in number mean (normalised). Perhaps a Std. Dev is needed to demonstrate this assertion. The volume mean ( ) 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'). 9 Data (volume mean) do not suggest "much larger slowdowns" in the late Holocene than that at 8481-8447 years BP. 15-18 It is no longer necessary to refer to a sinistral morphotype since Darling et al. (2006) demonstrated that N. pachyderma is sinistrally coiling and that the supposed dextral morphotype is in fact a different species (N. incompta). p. 671 6-8 The authors need to be cautious about "… we cannot invoke age-model uncertainties …" because the δ18O drop is recorded just a cm or so below the drop, but the abundance of N. pachyderma increases by $\sim$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 δ18O shift appear earlier. So the conclusion regarding relative timing of cooling and flow speed decrease is not robust. p. 674 21-2 See p. 671/lines 6-8 re problem with asserting non-synchroneity of flow and δ18O. p. 675 6-16 This para is relatively unconstrained speculation. p. 676 1 -paced or –spaced? p. 677 220-21 The cores discussed are under the eastern branch, not the main branch (NwAFC) of the inflow. p. 679 12-13 Not necessary to list all 12 cities in which Elsevier has an office: one will do. Figures 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. Fig. 2. How was normalisation performed – just by the mean or with Std Deviation? Fig. 3 & 5 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'. 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 N. incompta.

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Please also note the supplement to this comment:
http://www.clim-past-discuss.net/10/C83/2014/cpd-10-C83-2014-supplement.pdf

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