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Interactive comment on "Observed decline of the Atlantic Meridional Overturning Circulation 2004 to 2012" *by* D. A. Smeed et al.

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The authors study the temporal variability of the Atlantic Meridional Overturning Circulation (AMOC) as observed at 24.5°N between 2004 and 2012, using Gulf Stream transport estimates through Florida Strait, satellite derived Ekman transport, and mid- ocean geostrophic transport derived from the RAPID mooring array. The authors report a statistically significant negative trend in the AMOC magnitude from their 8.5 yrs time series. This trend is shown to result from an intensified subtropical gyre circulation in the upper layers and a compensating decreasing transport of Lower North Atlantic Deep Water (LNADW) at depth. The relationship with atmospheric forcing and deep-water formation in the North Atlantic sector is discussed.

> By providing a robust quantification of recent AMOC trend from observations, the paper makes a useful contribution to knowledge of climate natural variability on relatively short timescales and stand as a solid benchmark for improving climate predictability. The paper is well-written and easy to read. I do not have major issues with the scientific results although I think presentation could be slightly improved at some places in the paper. I therefore recommend publication in Ocean Sciences with minor revisions. These are listed below.

Thank you for your comments. We have revised the manuscript in the light of the reviews and the comments have helped us improve the paper. In additional to the changes to text indicate in the responses to the referee's questions we have made the following changes:

- Revised Table 1 to include annual average values for UNADW and LNADW.

- Revised Figure 4 to show the contribution of western and eastern boundary to the transport per depth profile.

- Revised Figure 6 to show changes in density on the eastern boundary

- Revised to figure 7 to adjust the (arbitrary) offset in the accumulated NAO. Comments 1, 2 and 3 raise some interesting questions about the relationship between the AMOC at 26°N and observations elsewhere in the Atlantic. For a full understanding of North Atlantic climate it is important that we form a coherent picture from the diverse observations. However, hydrographic observation referred to in comment 1) and the altimeter data analysed by Hakkinen et al 2013 referred to in comments 2) and 3) focus primarily on the changes that occurred since the early 1990's. The period of the RAPID observations from 2004 to 2012 is not necessarily representative of this longer period. In fact evidence (including our observations) suggests that the climate of the

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North Atlantic is no longer changing in the same way. In particular following a long period of increasing heat content, the total heat content of the North Atlantic has shown a significant decrease since 2004. Consequently we feel it is better not to speculate too much on the relationship of our observations to those earlier measurements and have made only minor changes in response to the reviewer's suggestions.

> Some general comments (which the authors are free to address):
>
> 1) As the authors discussed the relationship between the observed AMOC decline at 26âŮęN and the rate of deep water formation at higher latitude, a link could be made with recent estimates of the MOC variability in the subpolar gyre, as presented in Mercier et al, (2013) (see reference below). Using repeat hydrography and ARGO data, the authors report a decline of the MOC of about 2.5 Sv across the A25-Ovide section be- tween the early 1993 and 2010. Although the RAPID time series is shorter, this might support a meridional coherence of AMOC changes between the subtropical and sub- polar gyre. For information, an observational study recently submitted (Desbruyères et al, 2013) report a relatively weak impact of deep convection variability in the Labrador Sea (1990's - 2000's) to the basin scale magnitude of the AMOC in the subpolar gyre - in line with the relatively constant transport of UNADW reported here ? A weak impact of LSW formation rate on the MOC strength at 26âŮęN was also reported in model simulation (see for instance Marsh et al. 2005).

We have added some additional sentences to the discussion concerning the meridional coherence of the AMOC that cite Mercier et al., (2013). However, we do not think a direct comparison can be made. Looking at Figure 5 of that paper there is no obvious trend during the period of the RAPID observations. Comments citing Marsh et al., (2005) have also been added.

> 2) The AMOC decline reported here is shown to represent a strengthening

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of the sub-tropical gyre above the main thermocline, and a compensation within the LNADW depth range. The authors may want to relate this to a basin-scale view of changes in the gyre circulation in the 2000's, revealed for instance by the extended time series of the altimetry-derived gyre index (Hakkinen et al, 2013). Observations and models suggest a weakening of the North Atlantic gyre circulation accompanied by an increasing penetration of subtropical waters towards the northeastern Atlantic and a warming of the subpolar gyre from the 1990s to the 2010s. How does this reconcile with the change in gyre circulation presented here, that is an intensification of the southward return flow of subtropical waters during the 2000's?

Can the strengthening of the subtropical gyre observed across 26âŮęN be related to the windstress "gyre mode" (second wind stress curl EOF mode) that is shown to dominates variability in the upper circulation? The authors provides an interesting discussion on how AMOC may drive SST and NAO variability, but somewhat neglect the actual regional atmospheric forcing of AMOC variability at 26âŮęN. I think a short discussion on this may be useful.

The weakening of the gyre circulation since c. 1990 described by Hakkinen et al., 2013 was accompanied by a warming of the North Atlantic and in particular Hakkinen et al., 2013 focus on the warming of the sub-polar Atlantic. The strengthening of the southward flow in the subtropical gyre observed in the RAPID 26°N data might at first seem to contradict Hakkinen et al 2013, however, our measurements cover only a part of the period analysed by Hakkinen et al 2013 and the overlap is not sufficient to make any confident comparisons. The principal component (PC) of the EOF that Hakkinen et al 2013 use as an indicator of the gyre circulation increased (but not monotonically) since the start of the RAPID measurements (Hakkinen et al., 2013 Figure 5a) but the heat content of the sub-polar gyre has decreased during this period (Hakkinen et al., 2013 Figure 5b). In fact as noted above the North Atlantic as a whole has been cooling

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since 2004. Without further analysis it is difficult to understand the relationship between our results and those of Hakkinen et al., 2013.

> 3) An approximation of heat transport trend induced by the AMOC trend might appear useful in the discussion : dHT/dt = rhoCp dAMOC/dt *ΔT where ΔT could stand for a time-mean temperature difference between the upper and lower AMOC limbs, as deduced from the six hydrographic repeat at 26âŮęN. How does dHT/dt compare with the rate of change of heat content north of 26âŮęN (which has been mostly positive in recent years - see for instance Hakkinen et al. 2013)?

This is an interesting question but it is beyond the scope of this paper to answer it fully. Johns et al., 2011 found a strong correlation between the strength of the geostrophic part of the AMOC and the meridional heat transport (MHT) with MHT(PW) = 0.16(PW) + 0.064(PW/Sv)*MOC(Sv). However, as shown in figure 7 of our paper, the error bars on the estimates of the AMOC from the hydrographic sections are large and so it is not practical to make a sufficiently accurate estimate of the change in MHT to compare with the change in heat content. Hakkinen et al., (2013) showed an increase in ocean heat content of the subpolar gyre from 1995 to 2005 (their Figure 5b) and there was relatively little change in the following years. But the OHC change north of 26°N is not shown in Hakkinen et al 2013. It would also be necessary to consider the air-sea flux of heat to address this fully.

Cunningham et al., 2013 have analysed the budget of OHC between 26°N and 41°N and found that the changes are consistent with the RAPID time series during the period form 2004 to 2010 and in particular found a significant reduction in heat content associated with the downturn of the AMOC in 2009-2010.

We have added a few sentences to the discussion to highlight the magnitude of the change in the MHT that is expected form the observed decline of the MOC. From April 2008 to March 2012 the MOC was 2.7 Sv weaker than in the previous 4 years, using the correlation of Johns et al., (2011) this corresponds to a reduction in MHT of 0.17PW. Integrated over a four-year period this represents a total integrated heat transport deficit of 21.4 e21 J of this 12.1 e21 J of the deficit occurred in the year starting April 2009.

> Some minor comments: P1620, l.7 : add (1 Sv = 106 m3s-1).

Done.

> P1620, l16 - l17: As long as you consider the AMOC in the depth framework, this statement is mostly true in subtropical region. At higher latitude about half of the poleward heat transport is carried out by the horizontal circulation.

We have clarified that this statement refers to the subtropical gyre.

> P1620, l18 : add (PW = 1.1015 W)

Done.

> P1621, l.6 - l10: This sentence seems a bit long to me. Maybe split the ideas in two separate phrases.

We have split that sentence into two.

> P1622, l.15-l20 : How the applied external transport is involved in the UMO trends described latter in the text ? Surface geostrophic velocities from satellite altimetry can sometimes be an efficient way to reference the relative velocity field. Has this method been tested at 26âŮęN ?

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This is a complex issue and we prefer to cite other previous papers rather than repeat the details of the calculation here. We are preparing another paper that discusses the method of calculation in some depth but it is not yet submitted.

> P1623, l23: Is there a typo here? You first mention 90

There is no typo here. The 90

> P1624, l.22-l.23 : Again, you mention 90

See reply to previous comment.

> P1624, l25: What "those" refer to is unclear.

We now refer to table 2

> P1624, l.27 : could the authors add the UMO trend value in the text ?

Done.

> P1625, l3 - l18: Should the description of the uncertainty arrive before in the section? Confidence intervals appear several times in section 3.1 and 3.2 before the reader is informed on how they are actually calculated. I suggest the authors to move the whole paragraph at the beginning of the section.

Done.

> P1626, l.7 : can you provide a reference for the UNADW/LNADW distinction ?

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We have added a citation to Atkinson et al 2012.

> P1626, l.11-l.13 : how much greater ? What point the authors want to make with this quantification ? Please clarify.

This is perhaps not a particularly important point but we thought it was worthy of note as the rate is large. The corresponding rate of increase of UMO is 3

> P1626, l.16-l.17 : where exactly are the density profiles taken from ? A particular mooring ? An average for the whole western array ? This should be precised.

The profiles use different moorings for different depth ranges. However, the profiles are primarily from one mooring on the steep western boundary. We have added a citation to a previously published description.

> P1626, l.24 – l.26 : I found this paragraph a bit confusing, maybe because the link between Fig.6 (right) and Fig.4 (right) is not straitghforward as the authors only show the western density profile.

We now show the contributions of the eastern and western boundary to the transport change in Figure 4. This confirms that the changes on the west are most significant. The density change on the east is also shown in Figure 6.

> - If a similar increase in density than the one observed on the western margin above 500m is observed on the eastern margin (inducing a zero UMO transport change for that layer), then is the statement " changes on the west are much greater than those on the east. . ." (l.16-l.17) true ?

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We have added a qualification to this statement and included a profile of density change on the eastern boundary.

> - If, as stated, density variations are not responsible for the decreasing transport observed above 500m, I guess it is solely due to a weaker (Gulf Stream + Ekman) transport ? This is not obvious from the value of 0.7 Sv (0.5+0.2) reported in table 2 and the transport change profile in Figure 4. Is the applied external transport involved?

It is correct that changes in the Ekman and Gulf Stream transports are included in the profile of transport shown in Figure 4. Figure 4 has been changed to show the contributions form eastern and western boundaries separately and it can be seen that only a small part (mainly near the surface) is due to Ekman and Gulf Stream.

> - The authors could add the TINT/TEXT/GS/EK contributions to the transport changes per depth profile in figure 4 – as well as a third subfigure in Figure 6 showing the density profiles on the eastern boundary. Apologies if I missed something here.

See response to previous comment. We have added an additional line to Figure 6a (previously 6b) that shows the density changes on the eastern boundary. The purpose of this is to show that they are small compared to the west and so we prefer not to clutter the figure by showing lines for the contribution from temperature and salinity for the east.

> P1627, l.4-l.5 : Again, you mention 90

Please see answer to previous comments about the confidence interval.

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P1630, l6-l11: I do not find the "uplift" argument very convincing and the reference to Cunningham and Alderson should be in my view further developed/discussed.

We have added a few details about the calculation of Cunningham Alderson. It is true that without further evidence the uplift is a hypothesis that has not been proved.

Figure 4-6 : depth should be noted with positive values.

Done.

Figure 6 : (black between) → (black) between

Done.

Interactive comment on Ocean Sci. Discuss., 10, 1619, 2013.

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