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Interactive comment on "Compensation between meridional flow components of the AMOC at 26 N" *by* E. Frajka-Williams et al.

E. Frajka-Williams et al.

e.frajka-williams@noc.soton.ac.uk

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We thank the editor and reviewers for their time and attention to our manuscript. Reviewer 1 highlighted the strengths of the manuscript, identifying it as "an exciting and clearly written paper on the RAPID data," for which we offer our thanks. Reviewer 2 raised some concerns that the paper might be written a little too much for a RAPID-centric audience, so we have added some additional details in both the methods and conclusions to help identify the main findings of the paper. Please find below our detailed responses to the reviews interspersed between the original comments of the reviewers. Our replies are in italics. We also include a revised draft of the manuscript with the changes highlighted.

1 Response to Reviewer 1

Major comment A: "The authors used definitions of the UNADW and LNADW based on depth rather than density. The depth-based definitions make it almost impossible to compare the transport estimates reported in this paper against the other transport estimates of UN- ADW and LNADW, which are almost always based on density, e.g. the estimates at the Line-W (Toole et al. 2011). I guess that the decision to use the depth-based definition may be due to lack of measurement for the density structure in the interior. However, the geostrophic transport of a density layer is technically only based on only the end points, same as the geostrophic transport of a depth-based layer. In addition, the isopycnal surfaces are mostly flat in the interior of the basin anyway. So I think the authors should at the very least provide the density-based transports along with the depth-based estimates to allow a comparison with the other studies."

Unfortunately, translating from boundary-profiles of dynamic height to transports in density space (which require interior information) is not straightforward to do well as the relationship is not one-to-one. In the interior of the basin, the mooring array has quite coarse horizontal resolution, which makes translation into accurate density co-ordinates challenging. In particular, from hydrographic sections, the Labrador Sea Water layer spans the Mid-Atlantic Ridge, while the Denmark Strait Overflow Waters are concentrated in the western basin. While we agree that transport in density space is important, further study is required to determine whether transports in density space can be resolved well enough to compare to other latitudes. The reviewer mentions the transports at line W which uses a different array design (current meters to resolve the deep western boundary current, DWBC), but it is not obvious that the width of the DWBC captured there could be easily translated to a DWBC at $26°$N. We believe that such an extensive analysis would be beyond the scope of the present paper.

Major comment B: "One of the most interesting findings in this study is that the strong compensation between the sub-annual variability of UMO and FC observed in the full

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10-year record is absent in the first 3-years, as previously reported by Kanzow et al. (2010). However, the authors only briefly discuss about this apparent non-stationarity with the Figure 10. I think a further examination of this non-stationarity may provide some hints about the mechanism involved in the compensation. How unusual was the first 3-years statis- tically? Perhaps the authors could examine the 3-year running window correlation to quantify how unusual the first 3-years was. Furthermore, the authors suspected the eddies to be responsible for the compensation. If so, does an independent measure- ment of eddies in this region, e.g. EKE from the satellite altimeter, exhibit a reduced activity during the first 3-years? Please examine this interesting aspect a bit further."

*We have added additional discussion to the text, referring to the findings of an earlier GRL paper led by the lead-author here, titled "Eddy Impacts on the Florida Current". In that paper, we showed that there was a strong **correlation** between the Antilles Current (east of the Bahamas) and the Florida Current (west of the Bahamas), during the period 2009-2011. Concurrently, the rms-anomaly of SSH east of the Bahamas (over the Antilles Current) was larger amplitude subsequent to 2009. A detailed study of the EKE, while interesting, would be beyond the scope of this paper - however that is an area for future research within the group.*

New paragraph in manuscript: *The compensation between the FC and UMO is non-stationary. A windowed correlation between the FC and UMO prior to 2007 shows no significant correlation, either between the full time series or the high-pass filtered time series (not shown). From* Frajka-Williams et al. *[2013], the FC and Antilles Current (AC) were correlated between 2009 and 2011. The AC is a narrow, northward boundary current east of the Bahamas. Its transport is measured by direct current meter measurements in the RAPID array, and is included in the UMO transport. The mean transport of the AC is relatively small (3.6 Sv, compared to 31 Sv in the FC), so that the UMO transport is still net southward. However, since the AC is part of the UMO transport, a correlation between the FC and AC should weaken an anti-correlation be-*

tween the FC and UMO. The isopycnal displacements associated with UMO and FC variability (Fig. 7) also influence transport in the AC, as long as the AC is geostrophic. The mooring used to estimate isopycnal displacements is at the western edge of the UMO transport, but is east of the core of the northward AC velocities. This means that a downward displacement of isopycnals is associated with an increase in the tilt of the thermocline between Africa and the mooring, but will have an opposing effect on geostrophic transports west of the mooring, in the Antilles. Frajka-Williams et al. *[2013] identify stronger eddy activity east of the Bahamas during the period of FC and AC correlation, which is consistent with a eddy activity playing a role in the anti-correlation between the FC and UMO on subannual timescales.*

Major comment C: "Another aspect that may deserve a further investigation is the role of the wind stress curl forcing in the reported compensation, which may shed a light on the respective roles of the barotropic and baroclinic components in the observed compensation. The 0.5 - 1 day lag (Fig. 6e) may indicate a dominant role for the barotropic component. If so, does the wind stress curl averaged along the 26degN exhibit significant correla- tions with UMO, FC, and Ekman transports? Or does the power spectrum of the wind stress curl averaged along the 26degN exhibit a similar structure as the coherences shown in the Figs. 9-10? On the other hand, the concentration of the compensation near the western boundary may suggest a role for either the baroclinic response to the local wind stress curl or the barotropic response along the western boundary topo- graphic wave guide. These aspects could be investigated based on the correlation of the transport components with the wind stress curl in different locations."

Indeed, we are presently investigating the role of wind-stress curl forcing on transports at 26°N. Given the short duration of the record, however, it has proven difficult to say with certainty that the relationships we are finding, in spite of significant correlations. Over the 10-year record, these transport time series have 2 peaks and 2 troughs, with similar timescales of variability as that of the winds, so any conclusions must be

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considered cautiously, in terms of their statistical significance. Again, we would argue that this is beyond the scope of the present paper.

Detail comments

1. Please add the horizontal and vertical grid lines in all the time series

   Some grid lines have been added, but we find that too much clutter in a figure makes it harder to see the data.

2. P.2711, L.8-11: Please quantitatively compare the first and last 5 years in terms of the number of days with flow reversal.

   There were 10 days in the latter 5 years (zero days in the early 5 years). Days with a reversal included 19-24 Dec 2009 and 9-13 Mar 2013. Details have been added to the first paragraph of section "Ten years of MOC and mid-ocean variability" (L141-43).

3. P.2711, L.15-17 and Table 1: Are the mean transports still statistically significantly different if the two periods are divided as 2004-2010 and 2010-2014? This could be one way to assess the robustness of the long-term transport reduction in the record.

   2009 was an anomalous year with the lowest transport observed during the observations and so separating the time series in 2010 instead of 2009 does change the results. However, it is perhaps better to exclude this one year when looking at the long term trend as was down in Smeed et al 2014 (tables 2 and 3 in that paper), which reduced the change/trend but did not eliminate it. Here, separating into a 6-year and a 4-year mean as suggested by the reviewer, the mean transports are statistically significantly different for all transports except UMO. Separating the two periods in 2010 suggests that the UMO transport difference is only significant at the 13% level.

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4. Figure 4 caption: It seems the data plotted in the panels c-d are not de-trended, unlike the panels a-b. However, this is unclear in the caption.

   The data in panel c and d are low-pass filtered, while panels a and b are de-trended. This has been clarified in the caption.

5. Figure 4a and the related text: The UMO has the sum of three components, ie Twbw, Tint, and Text (Eq.1). What are the correlations of each component against the FC?

   The correlation between the FC and WBW, which is dominated by the Antilles Current, is addressed in Frajka-Williams et al. *[2013]. It is significantly and positively correlated over the period after April 2009. The Text has the same time variabilty as the LNADW only, and is not correlated with the FC. The remaining transport is the Tint, which is governed by isopycnal displacements and is responsible for the observed signal. Due to the limited physical significance of the individual components (see comment below), the text has not be updated.*

6. P.2716, L.9-20: The argument in this paragraph could be more straightforward if the correlation of the Twbw, Tint, and Text against the Ekman transport is explicitly provided.

   The separation of Tint and Text is somewhat arbitrary (due to the choice of the reference level), and the Twbw is the Antilles Current but also includes some of the DWBC, so the separations, while originating from the observations and methodology are not necessarily useful physically.

   We show below the time series separated (and related to the FC and Ekman), as suggested by the reviewer. As it turns out, the FC is better correlated with the sum of Twbw, Tint and Text than any individual component. It is about equally correlated with the Tint+Text transport. Ekman transport is not correlated with either deep Twbw (which is mostly above 2000 m), or deep Tint (which is very

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small due to the lack of shear below 3000m). It is correlated only with the external component of the MO transports.

P.2717, L.11: "(1500-bottom)" -> "(1500 m – bottom)"

Thank you, this has been corrected.

7. Figure 7b and the related text: As 3000 m is about the height of the Mid-Atlantic Ridge (MAR), shouldn't the MAR sub-arrays also be considered?

   The MAR sub-arrays do not show the variability we've found in the transbasin transports, in that isopycnal displacements from the MARwest and MAReast moorings are not correlated with transport time series. This has been added to the text (as "not shown").

9. Figure 8a and c: The colors for the curves indicated in the legends and the y-axes do not match. For example, the y-axes for the depth are in black, while the curves for the isopycnal displacement are not in black.

   *Thank you, the legends and curves have been corrected."

10. Figure 9 and P.2718, L.15-19: The coherences between FC and UMO as well as that for LNADW and Ekman transport are not significant for the periods shorter than 60 days. What is the explanation for this high frequency cut-off?

    While the FC and UMO have no coherence below 50 days (and marginally significant coherence from 50-70 days), the LNADW and Ekman do have some coherence below 50 days. Our focus in this study is primarily at longer time scales, which is why we binned our data into a bi-monthly time scale. We plan future analyses that will focus more on the shorter periods.

11. Figure 10 caption: Are the arrows shown only when the co-spectra are statistically significant? Please clarify in the caption.

    Thank you, this has been clarified.

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12. Figure 11 caption: “Eq. (2)” -> “Eq. (3)”?

    Thank you, this has been corrected.

2 Response to Reviewer 2

To make more clear which results discussed in this paper are new as the reviewer suggested, we have added three sentences to the Conclusions. (Page XX line YY, page xx line yy, and page xx, line yy.)

- In this paper, we have provided a brief overview of the latest 18-months of observations.
- The main result of this paper has been detailing newly-identified compensations between MOC components (UMO and FC, and LNADW and Ekman).
- Finally, investigating longer-term variations of the MOC, we can localize the origin of the intensifying trend in the MOC to isopycnal displacements on the western boundary.}

Here we list our replies to the detailed comments the reviewer inserted into the highlighted manuscript they submitted as a supplemental addition to their review.

1. Seven years long -> Seven years length

   This has been changed.

2. “These low frequency changes have consequences for the heat content across the subtropical and tropical North Atlantic”. I don’t understand this. Is it a reference to heat flow across a line or heat content in an area?

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This sentence has been revised to read "These low frequency transport changes have been shown to be responsible for changes in ocean heat content in the subtropical and tropical North Atlantic."

3. "Observed trend of -0.5 Sv" should be Sv/yr?

   Thank you, this has been updated.

4. P2708, L2: "The depth structure and time scales of these variations are explored, illustrating an important role for the western boundary below 1000 m." Is it transport along the western boundary?

   This has been revised to read: "illustrating an important role for the western boundary below 1000 m in setting transport variability."

5. P2708 L17. "The exact number of moorings and instruments has varied over the past decade and over 20 deployment and recovery cruises." Verb?

   This has been changed to: The exact number of moorings and instruments has varied over the past decade during which there have been over 20 deployment and recovery cruises.

6. "near the surface to 500m at the bottom" –> Suggestions: close to the bottom.

   This has been updated

7. Pg 2708. Map of array?

   We have added a parenthetical note to suggest that readers see Figure 1.1 of McCarthy et al. (2015) for a detailed map of the array.

8. Pg 2709. The transport is a function of z. Barotropic is normally understood as depth independent. As far as I can see, McCarthy et al. 2015 does not claim that this term is barotropic.

Here, the velocities are barotropic (depth-independent), but due to the width of the basin, the transports are not barotropic. This has been clarified as "Due to changes in the width of the basin as a function of depth, though the applied flow is barotropic (depth-independent), the transport-per-unit-depth has decreasing magnitude with increasing depth."

9. Pg. 2710. "It is a rather *complicated* filtering of data. Rationale? Reference? Details of filtering is given here, but in figure 1, a month low-pass filter has been applied. What about this filter?"

   *We agree that the filtering applied sounds complicated. The text has been clarified to indicate that the first step of the filtering is applied during the standard RAPID processing (which corresponds to the publicly available dataset). From this starting point, we have applied additional filtering. We have tweaked the pertinent paragraph in the Methods section to make this more clear (L105-112).

   The time series in Figure 1 are our starting point - the publicly available data set. We have included in the caption the basic processing information (i.e. filtering) done on that data set; our more detailed processing discussed in the Methods section applies to the remainder of the analysis in the paper.

10. L2710. "For the purpose of calculating isopycnal displacements, in this study, absolute salinities and conservative temperatures on the twice-monthly time grid are used. For reference pressures from the surface to bottom, a mean density profile is calculated within 400m of the reference pressure, and isopycnal displacements from the mean depth are calculated. Displacements are then mapped back onto the mean depth of each density surface using the time mean profile." I don't understand what is done with the data. Time mean? Lateral mean? Please rewrite.

    These have been modified and clarified with additional detail in the text, L117-128.

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11. L2713 & L2714. “sign (note that the negative of UMO is plotted in Fig. 4a),” I suggest to introduce this note at the in the second paragraph of this section. UMO plotted in both b.

    This has been removed, with the plotting details noted in the caption.

12. Fig. 9 caption, purple –> green

    This has been updated

References:

Frajka-Williams, E., W. E. Johns, C. S. Meinen, L. M. Beal, and S. A. Cunningham (2013), Eddy impacts on the Florida Current, *Geophys. Res. Lett.*, *40*, 349–353, doi:10.1002/grl.50115.

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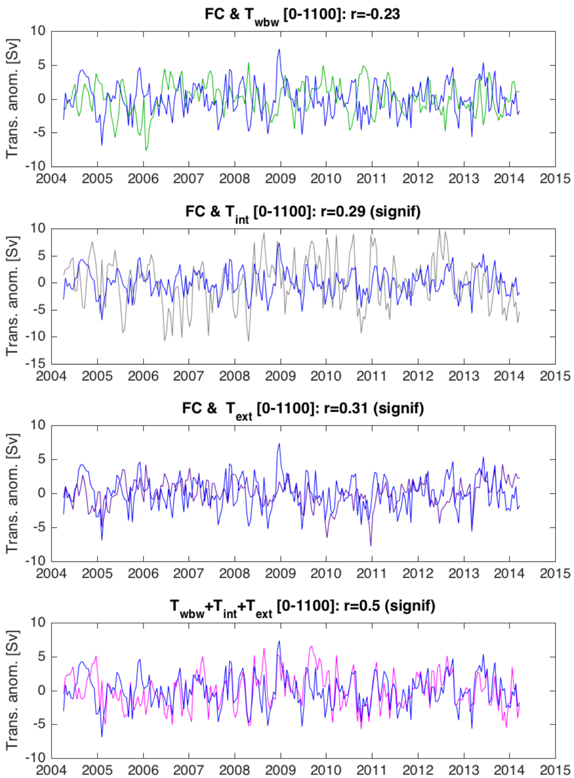

Fig. 1. Correlation between the FC and three components of the Mid-Ocean transport (a) Twbw, (b) Tint, (c) Text, and (d) the sum, over the depth range 0-1100 m.

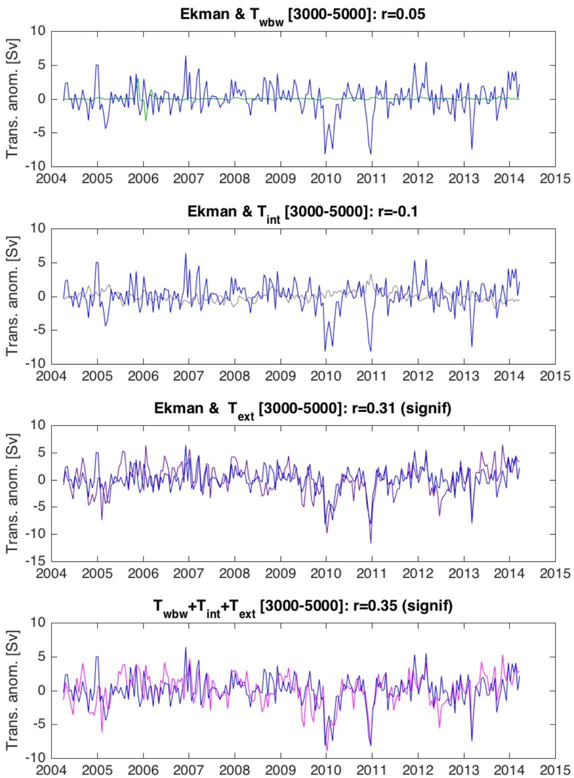

Fig. 2. Correlation between the Ekman and three components of the Mid-Ocean transport (a) Twbw, (b) Tint, (c) Text, and (d) the sum, over the depth range 3000-5000 m.

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